Board declassification and firm value: Have shareholders and boards really destroyed billions in value?

by

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Abstract

This paper analyzes the wave of board destaggering that has occurred over the past fifteen years. Other studies have concluded that the result of this phenomenon has been a substantial destruction of firm value, purportedly caused by re-orienting management from a long-term to short-term focus. We conclude that these results reflect a spurious correlation. We find, first, that board destaggering has occurred disproportionately among firms of very large market capitalization and, second, that firms with very large market capitalization also experienced disproportionate and unrelated relative drops in Tobin's Q over the period in which destaggering has occurred. The association between destaggering and the drop in Tobin's Q becomes statistically insignificant once one compares destaggering firms with other firms of similar market capitalization. We analyze the claim that board destaggering is especially costly for firms with high R&D, and similarly find that once one takes account of unrelated differential fluctuations in *Q* among high- and low-R&D firms, there is no evidence that destaggering a board reduces the value of high-R&D firms. From a methodological perspective, our analysis suggests that corporate governance studies using difference-in-differences or within-firm designs should take account of the possibility that differential secular trends in asset prices may confound their results.

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Board declassification, firm value, and long-term focus: Have shareholders and boards really destroyed billions in value?

1. Introduction

This paper analyzes the board declassification wave that has occurred over the past 15 years, during which over four hundred publicly traded firms have destaggered (or "declassified") their boards—many in response to demands of shareholders, who have historically seen staggered boards as mechanisms by which management can resist attractive hostile takeover bids. Some commentators have decried this wave of destaggering as a senseless destruction of value that promotes "short-termism" and leads to "underinvestment in long-term growth . . . in diametrical opposition to the interests of institutional shareholders" (Lipton and Shaffer 2015).

The empirical literature beginning in the late 1980s and extending until the mid-2000s did not support the views of these commentators, finding that staggered boards had a negative correlation with firm value. Over the past few years, however, studies have revisited the issue by analyzing the recent declassification wave. The large number of firms that have changed their board's structure has created the possibility of studying the impact of classified and annually elected boards in a time series framework rather than in the cross-sectional framework that characterized most prior studies. The results of these more recent studies are intriguing. They find that the board declassification wave is associated with a substantial *reduction* in firm value as measured by Tobin's Q—and that a staggered board therefore has a *positive* association with firm value. These studies further offer suggestive evidence that this reduction in value may be attributable at least in part to a loss in long-term focus that classified boards purportedly provide by insulating management from the threat of a hostile takeover.

These results are paradoxical with respect to both shareholder and board behavior: Why would shareholders compel a reduction in firm value? Why would boards destroy value by exposing themselves to value-decreasing hostile takeovers? Moreover, these results conflict with two decades of research finding that classified boards are detrimental to firm value.

These recent studies raise fundamental questions. If in fact declassifying a board destroys firm value, basic questions of corporate governance would have to be reconsidered. Institutional investors that support shareholder proposals to declassify boards, and that later vote to approve charter amendments that implement the declassification, would need to ask serious questions about their ability to play a responsible role in corporate governance. Boards would similarly have to ask themselves whether they are performing the leadership function expected of them rather than blindly following the whims of ill-informed shareholders. Destaggering a board, after all, generally requires the approval of both shareholders and boards. Finally, if fifteen years of destaggering indeed caused firm values to drop between 5 and 10 percent on average, as one study found, shareholders and boards could recoup the hundreds of billions of dollars in value lost by simply restaggering their boards.

In this paper, we take a closer look at the board declassification wave. We conclude there is no evidence that institutional shareholders and boards have behaved as irrationally and irresponsibly as might appear. We find that the econometric results indicating that board declassification destroys value do not hold up to scrutiny.

We analyze a sample of over 2,200 firms over the period 1996 to 2015, among which over 400 declassified their boards. In a model with firm and year fixed effects, we initially reach results consistent with prior studies' findings that the declassification of a board is associated with a 6.5 percent drop in firm value, measured by Tobin's Q. This finding raises the question why either shareholders or directors would take an action that destroys so much value. On closer analysis, however, we make two findings that explain the puzzle. First, the average drop in value associated with declassification is attributable to firms with large market capitalization, which declassified their boards at a much higher rate, and earlier in the declassification wave, than firms with lower market capitalization. This of course simply shifts the question to why large firms would destroy value by destaggering their boards. Our second finding, however, is there is no evidence that board declassification among these large firms actually destroyed value. The drop in Tobin's Q following declassification among these firms reflects a secular drop in Tobin's Q for firms of high market capitalization relative to other firms irrespective of whether they declassified their boards. This general decline in Tobin's Q among the largest firms, which began in 1999, followed a steep four-year increase relative to other firms. As a result, during a four-year period prior to the declassification wave, Tobin's Q for these firms was high irrespective of board structure, and as these firms declassified from 2003 onward Tobin's Q dropped relative to firms in the other deciles. As a consequence, the apparently causal relationship between declassification and a drop in Tobin's Q is spurious.

We remedy this problem in a firm fixed effects model by replacing the year fixed effects by slightly more granular market-capitalization-group-by-year fixed effects, thereby comparing firms with other firms of similar market capitalization within each year. This allows us to account for the differential secular trends in *Q* experienced by the largest firms. In this specification, the negative association between declassification and Tobin's Q vanishes.

We then look at firms with high research and development (R&D) to see whether, as some have found, these firms lose systematically more value as a result of declassifying their boards. In the recent literature on staggered boards, and elsewhere, high R&D is used as a proxy for firms that focus on the long run, and hence identifies the firms that are most vulnerable to lost value if indeed destaggering a board causes a sacrifice of long-term focus. Once again, we initially find that indeed these firms did lose value when they destaggered their boards. But on closer inspection, this result also vanishes. Here too, we find that the source of the spurious result is differential trends of *Q* among high- and low-R&D firms. We take account of these differential trends by controlling for R&D intensity-by-year fixed effects, thereby comparing firms with other firms of similar R&D intensity. When we do this, the results no longer show that R&D-intensive firms experienced systematically higher drops in value after declassifying their board.

We conclude that the recent literature apparently finding that the declassification wave has destroyed massive amounts of value is flawed. We find no evidence that such destruction has occurred, among declassifying firms generally or among high-R&D firms.

Our analysis also highlights a general methodological issue that should be addressed in any corporate governance study that uses a difference-in-differences or within-firm framework. Such studies should take account of the possibility that differential secular trends in stock prices may confound results. Those trends could be related specifically to well-established results in the empirical asset pricing literature (for example, the fact that firms of different size, or different book-to-market ratio, tend to experience systematically different average stock returns), or they could reflect other, less well-documented, temporary pricing anomalies.

This paper will proceed as follows: Part 2 provides a literature review. Part 3 provides a description of the institutional background relevant to understanding the declassification wave. Part 4 describes our dataset and presents the empirical findings. Part 5 reports the results of a series of robustness tests for the results of Part 4. Part 6 concludes.

2. Literature Review

A series of cross-sectional studies, beginning in the late 1980s with Jarrell and Poulsen (1987) attempted to analyze the impact of a staggered board on firm value. These studies had a variety of methodological problems (see Coates (2000) and Klausner (2013)), but provided some evidence that staggered boards were negatively correlated with firm value. See Mahoney and Mahoney (1993), Garvey and Hanka (1999), Johnson and Rao (1997), Borokhovich Brunarski and Parrino (1997).

Bebchuk, Coates and Subramanian (2002) found that staggered boards had a negative impact on the value of firms that were targets of hostile bids between 1996 and 2000. Consistent with those results, Bebchuk and Cohen (2005) found that, in the cross-section, staggered boards were negatively correlated with Tobin's Q. Bates, Becher and Lemmon (2008) also found that firm value, measured by Tobin's Q, is negatively associated in the cross-section with the presence of a staggered board. They also found that firms with staggered boards received fewer hostile bids than did firms with annually elected board, but this impact did not account for the difference in firm values. Moreover, they found that a staggered board had no impact on the likelihood or consummating a deal once a bid is made, and no impact on other potential disadvantages that target shareholders might experience in the takeover process as a result of a staggered board.

Three additional papers analyzed more specific impacts of staggered boards. Faleye (2007) analyzed the relationship between staggered boards and CEO turnover. He found that firms with staggered boards were less likely to replace the CEO of a poorly performing company than are firms with annually elected boards. He also found a negative correlation between Tobin's Q and a staggered board. Masulis, Wang and Xie (2007) found that among firms that made acquisitions, those with staggered boards tended to experience lower abnormal returns upon announcement than did firms with annually elected boards.

Summarizing the literature as of 2010, Adams, Hermalin and Weisbach (2010) concluded that the weight of the evidence indicated that staggered boards are negatively correlated with firm value. There were plausible bases for inferring that the relationship was causal as well. One limitation of these studies, however, was that all were cross sectional. There were no time series analyses of the impact of staggered boards.

The opportunity for time series analysis as of the mid-2000s was limited for two reasons. First, although there was a wave of board classification in the 1980s, which in principle could have presented an opportunity for time series analysis, there was no readily available source of data available on board structure for that time period, nor were firm charters available online. It was not until Cremers and Ferrell hand-collected these data that these data became available. (Cremers and Ferrell (2014)). Second, from 1990 on, shareholder resistance to staggered boards meant that with few exceptions no more staggered boards would be adopted for over a decade. Beginning in 2003, shareholders affirmatively pressured boards to destagger and boards acquiesced. A few studies have now begun to exploit this opportunity for within-firm time series analysis. We continue that line of research here.

Cremers and Sepe (2016) analyzed a sample period from 1978 to 2011, and Cremers, Litov and Sepe (2016) extended that sample to 2015.[1] These sample periods included both a wave of board classification that took place during the 1980s and the declassification wave that began in 2003. Using a within-firm framework, they reach the surprising conclusion that staggered boards actually *enhance* firm value. This conclusion is not without theoretical plausibility. Legal practitioners that have advocated staggered boards have long argued that the takeover protection that staggered boards provide to management promotes long-term decision-making (Lipton and Schaffer 2015, Koppes, Ganske and Haag 1999). Cremers and Sepe's primary finding is that destaggering had a *negative* impact on Tobin's Q for the period 1996 to 2011—a period that begins with very few changes in board structure, but that includes the declassification wave that began in 2003. Cremers et al. (2016) further report that this negative impact on firm value occurs gradually over at least three years, commenting that "market participants need some time to fully learn and process the changed prospects of the firm that occur after the change in board structure."[2] These results are surprising for at least a few reasons. First, they are the opposite of what all prior studies have found. Second, the decline in *Q* as firms destaggered in the 2000s is not matched by an increase in *Q* during the period in the 1980s, when firms staggered their boards. Finally, Cremers and Sepe's finding regarding the

[1] Because both of these papers report similar results pertaining to the same data, we will not always refer to both papers where they make the same point. Cremers and Sepe emphasize the positive association they find between staggered boards and firm value. Cremers, Litov and Sepe, by contrast, seem to be more circumspect about interpreting the positive within-firm association between staggered boards and firm value. They instead highlight the results of sensitivity analyses that suggest that the negative association between destaggering and *Q* is attributable to a reduction in long-term focus and a reduction in commitment to other stakeholders.

[2] Cremers et al., p. 12.

declassification wave of the 2000s means that shareholders and boards have voluntarily destroyed—and still continue to destroy—massive amounts of firm value.

Ge, Tanlu and Zhang (2016) also analyze the declassification wave of the 2000s. Using a matched sample of firms that declassified and firms that did not, they analyze the impact of declassification in a difference-in-differences framework. Like Cremers et al. and Cremers and Sepe, they conclude that board declassification destroyed firm value. Indeed, depending on the specification, their findings suggest that destaggering was associated with a 13% to 21% loss in value—a finding that in our view should have raised a red flag. Also consistent with Cremers et al., they attribute at least part of this loss in value to a reduction in long-term investment.

As explained below, we find multiple shortcomings in the analysis of Cremers and Sepe, Cremers et al., and Ge et al. Those shortcomings relate to a failure to observe that the parallel trends identification assumption underlying their baseline design is unlikely to be supported in the data, and a failure take account of the fact that firms of different market capitalization and different levels of R&D intensity experienced secularly different stock returns for reasons that had nothing to do with changes in board structure.

In this paper, we call into question the conclusion that firms that have destaggered their boards over the course of the declassification wave have destroyed firm value on a massive scale. We look more closely at whether destaggering is in fact responsible for losses in firm value, and we conclude that there is no evidence for that claim. We infer that Cremers and Sepe, Cremers et al and Ge et al. probably suffer from the same problems: a failure to take account of differential trends in stock prices. In addition, we study whether there was indeed a reduction in long-term focus among firms that destaggered their boards during the 2000s, as this recent literature has suggested. We conclude that there is no evidence that they did.

Before moving on to our analysis, we note a few more papers that have looked at the impact of staggered boards on value. Amihud, Schmid and Solomon (2017) argue that the entire literature suffers from omitted variable bias and endogeneity. In a cross-sectional framework with instrumental variables and many financial and governance variables included in their regressions, they conclude that staggered boards have no impact on value. Cohen and Wang (2012) use two court decisions involving the use of a staggered board as natural experiments, and conclude that staggered boards reduce firm value. Amihud and Stoyanov (2016) find, however, that their results are sensitive to sample selection—for example, the inclusion of very small firms, penny stock firms and one firm that had a large drop in value unrelated to the legal decisions that provided the treatment in Cohen and Wang's analysis. Daines, Li and Wang (2016) analyzed firms whose boards were staggered as a result of a change in Massachusetts law in 1990. Treating the change in law as an exogenous shock, they find that affected firms experienced an increase in value over a fifteen-year period and conclude that a staggered board can be beneficial for small, innovative firms of the sort incorporated in Massachusetts. Finally, a recent working paper by Cremers and Sepe (Cremers & Sepe (2017)) studies declassifications adopted between 2011 and 2015. They conclude that, while in general declassifications adopted during that period were not associated with drops in Tobin's Q, the

declassifications that were adopted by firms that had received precatory declassification proposals filed with the assistance of the Harvard-based Shareholder Rights Project were followed by drops in Tobin's Q. On the other hand, however, they found that the latter set of firms did not experience significant risk-adjusted abnormal returns in connection with their declassifications.

3. Institutional Background

At the risk of sacrificing detail, this section provides a brief review of the institutional background necessary to understand the declassification wave that we analyze. This will include the nature of a staggered board, how it came to be a potent defense against hostile takeovers, and how the declassification wave has evolved since 2003.

With a typical staggered board, each director serves a three-year term, and one-third of the board is up for election every three years. Directors can be removed only for cause, and they may be elected only at annual meetings. Consequently, it takes two annual shareholder meetings to replace a majority of directors on a staggered board.

Prior to 1982, the corporate law statutes of Delaware and other states had already allowed firms to "classify" their boards—that is, divide them into three "classes"—but without the poison pill, the staggered board played little (if any) role as a takeover defense. With the advent and legal validation of the poison pill in 1982 and 1985, respectively, the staggered board took center stage as a defense against hostile takeover attempts. The poison pill gave the staggered board its central role because the pill is under the control of a firm's board of directors; boards have the power to adopt a pill and to withdraw a pill with no shareholder involvement. A poison pill poses an absolute bar to a hostile takeover so long as it is in place. Consequently, the way a hostile bidder works around a pill is to mount a proxy contest to replace a target's board with a board that is more amenable to the takeover and therefore willing to withdraw a pill so that a takeover can move forward to shareholder approval, either via a tender offer or a merger. If a target has a staggered board, a bidder will have to mount two proxy contests at two consecutive annual meetings in order to replace a majority of target directors. Bebchuk, Coates and Subramanian (2002) found that a staggered board provides a substantial deterrent to a hostile bid.

Simplifying somewhat, a staggered board is generally provided for in a firm's charter.[3] In Delaware and nearly all other states, a charter amendment can be initiated only by a firm's board and it must then be approved by a vote of the shareholders. Consequently, for an annually elected board to be converted into a staggered board, or for a staggered board to be destaggered, the approval of the board and the shareholders is required.

[3] Staggered boards can be adopted in bylaws as well, but shareholders can amend bylaws without board involvement. Staggered boards provided for in bylaws are generally ineffective unless they are also coupled with high supermajority vote requirements for amendment. For a detailed discussion of "effective" staggered boards in charters and bylaws, see Bebchuk, Coates and Subramanian (2002).

Cremers et al. have shown that between 1982 and 1990, firms adopted staggered boards at a rapid rate. By 1990, shareholders recognized that staggered boards were barriers to lucrative takeover premia and, in general, would no longer approve charter amendments that adopted staggered boards. From 1990 to 2003, few firms staggered their boards and few destaggered. In many of those that staggered, management held large blocks of shares, and in others the board destaggering was bundled with a merger or other transaction that shareholders favored.[4]

Beginning in 2003, however, a massive wave of board destaggering has occurred and continues today. As we report below, in the S&P 1500, over 400 firms have destaggered their boards. This wave of destaggering began with pressure from precatory proposals supported by institutional shareholders. As indicated in Figure B4 in Appendix B, among the sample of firms followed by the SharkRepellent database (which essentially spans the universe of publicly traded companies in the United States), there were approximately 50 nonbinding shareholder proposals per year requesting firms to destagger their boards in the years 2001 to 2005. Since 2005, shareholder proposals peaked at 79 and then declined gradually to only 8 in 2016. In general, these proposals received substantial shareholder support: for the median proposal, almost two thirds of the votes cast supported the precatory resolution.

Although pressure from shareholders was surely a factor in getting the declassification wave started, it was not the only factor. Boards of directors themselves turned out to be willing to destagger. As documented by Figures B5 and B6 in Appendix B, in 2001, only 8 boards in the SharkRepellent database offered shareholders an opportunity to declassify—and the average shareholder vote in response was 98% in favor. By 2005, 71 boards initiated declassification, and from 2005 through 2016, there were an average of 73 cases of board-initiated declassification per year—a *larger* number than the number of shareholder proposals over the same period of time. Management thus seems to have embraced declassification even without the direct pressure of a shareholder proposal. This has been particularly evident over the past few years. Although shareholder pressure in the form of precatory shareholder resolutions appears to have subsided, and despite publicity given to research purporting to show that destaggering boards impairs firm value[5], a significant number of firms have nevertheless continued to put forward proposals to destagger their boards. Moreover, despite this research, shareholders have approved these proposals with votes in the high-90% range—a level of support that has remained completely stable over the past 15 years—, as Figure B6 documents.

There is no definitive explanation for why management and boards have come to accept and even promote board declassification. One reason may be that hostile takeover activity has declined dramatically in the past two decades. A related and more self-interested explanation for management's willingness to destagger their boards is that over the past 15 years, their compensation has become increasingly equity-based. Managers often hold a highly undiversified interest in the firms they manage. Managers, therefore, may not only be willing to

[4] See Klausner (2003) and Bebchuk and Kamar (2010).

[5] See, e.g., https://dealbook.nytimes.com/2015/01/05/an-unusual-boardroom-battle-in-academia/

sell into a takeover at a premium price, they may actually be too willing to do so compared to their diversified shareholders. Boards generally hold less equity, but they may have a self-interest as well. They face the possibility of a withhold vote campaign if they fail to follow the direction of a precatory shareholder proposal that has received sufficient support. At least in those cases in which there has been a shareholder proposal, directors may therefore have a personal interest in destaggering irrespective of whether they believe a staggered board is good for shareholders. It is thus possible that management and boards favor destaggering despite a belief that destaggering is not in shareholder interests.

In sum, once the declassification wave got started in 2003, it received the support of management and boards, along with the support of an increasingly institutionalized shareholder base. The research we have reviewed, however, concludes that the declassification wave has caused a massive destruction of firm value. Perhaps all parties involved—shareholders, boards, and management—are failing in their governance roles. One cannot exclude this possibility. But if it is true, it would represent a severe blow to our system of corporate governance. Therefore, the claim in the recent empirical literature that this widespread and widely supported development in corporate governance has destroyed hundreds of billions of dollars warrants a closer look, to say the least.

4 Empirical Analysis

4.1 Data description

The data employed in this paper consists of a hand-collected dataset of board-related information about all publicly traded firms that were part of the Standard & Poors (S&P) 1500 Index between 1996 and 2015, excluding financials, utilities, firms that had a dual-class share structure, and firms for which we could not find a corresponding match in the CRSP-Compustat database. We added accounting data from CRSP-Compustat. For each of the firms in our sample, and every year between 1996 and 2015, we searched a variety of sources (SharkRepellent, the IRRC database, and the Securities and Exchange Commission's EDGAR website) to determine whether the firm had a staggered or annually elected board in each year of our sample period. From these data, we then identified firms that destaggered their boards during this time period. Since our focus is on the effect of board destaggering, we drop from the sample 56 firms that staggered *up* their boards during the sample period. In addition to being irrelevant to our study of the declassification wave, these instances of firms adopting staggered boards generally occurred in contexts that would confound interpretation of the impact of the change in board structure. In many cases, the declassification occurred in the context of a transaction such as a merger, restructuring or reincorporation in which shareholders were required to cast a bundled vote on the transaction and the staggered board together. In such a situation, the impact of declassification on value cannot be isolated.[6] The

[6] In other cases, management owned a high percentage of the firm's shares—the median was 17.5%—and therefore did not need a high percentage of minority shareholders to vote with them. In those situations, while the immediate impact of the staggered board would have been low, if the management sold down its holdings later on, the staggered board could assume greater importance as a takeover

sample comprises a total of over 2200 unique firms and over 28,000 firm-year observations.

Our main outcome variable of interest is firm value. Following a long strand of literature that analyzes the effects of corporate governance on firm value, our measure of firm value is Tobin's Q. Using data from the CRSP-Compustat merged Fundamentals Annual database we construct Tobin's Q as the ratio of the market value of the firm to the book value of its assets, where market value is defined as the sum of the book value of assets and the market value of equity minus the book value of equity. As is standard, in order to prevent our results from being driven by outliers, we winsorize Tobin's Q and other financial ratios at the 2.5 and 97.5 percent tails of the respective variable's distribution. Table 1 contains the descriptive statistics for the entire sample.

Table 1: Descriptive Statistics

Table 1 presents the descriptive statistics for the main variables in our sample. All financial ratios are winsorized at the 2.5% in both tails.

VARIABLES	N	Mean	Std. Dev.
Staggered Board	28,393	0.514	0.500
Tobin's Q	28,393	2.109	1.333
Years since Public	28,393	19.498	13.970
R&D/Sales	28,393	0.052	0.091
Missing R&D	28,393	0.342	0.474

Figure 1 describes the evolution in the incidence of staggered boards among firms in the sample. As one can see, the fraction of firms with staggered board peaked in 2002, and then dropped monotonically. From 2002 to 2015, the fraction of firms in the sample with staggered boards went from almost 60 percent to approximately 35 percent. Most of the drop in the incidence of staggered boards is driven by the fact that, as reflected in Figure B1 in Appendix B, over 400 firms amended their governing documents (their charters and, less commonly, their bylaws) to declassify their boards. As we will see, however, the aggregate time series described in Figure 1 hides a significant degree of cross-sectional variation: firms with larger market capitalization as of 2002—the year when the declassification wave got started—were much

defense. In yet other cases, boards adopted staggered boards unilaterally without shareholder approval. This was permissible because they were incorporated in states that allowed boards to adopt staggered boards by bylaw amendment or board resolution or because the firm's charter already gave the board the authority to stagger up. In these cases, it is understandable both that a firm would be able to stagger its board and that share value could well decline. The anomaly of shareholders both approving declassification and bidding down share prices does not exist.

more likely to declassify their boards and to do so earlier in the sample period than firms with smaller market capitalization.

4.2 Declassification and Firm Value

As a first step in our empirical analysis of the association between staggered boards and firm value, we estimate a regression that replicates the within-firm analysis of the previous literature finding that staggered boards are positively associated with firm value—or, equivalently, that destaggering is associated with a reduction in firm value. We thus estimate the following linear specification:

$$Tobin'sQ_{it} = \alpha_i + \theta Staggered\ Board_{it} + \gamma_t + \eta_{it} + \varepsilon_{it}, \qquad (1)$$

where *Tobin's* Q_{it} is the outcome variable of interest for firm *i* at time *t*; *Staggered* $Board_{it}$ is an indicator that describes whether firm *i* had a staggered board in place as of time *t*; α_i is a vector of firm fixed effects; γ_t is a vector of year fixed effects; and η_{it} is a vector of "years since the firm became public" fixed effects.[7]

[7] Prior studies have attempted to control for the cross-sectional differences in value due to the fact that different firms may be in different points in their life cycle by including "firm age"–or some functional transformation of it–as a linear control. We adopt a more robust approach by simply including fixed effects for the number of years elapsed since the firm became public. Prior studies have also systematically included as controls measures of book value of assets, return on assets, capital expenditures, and investments in research and development. Those variables are more properly regarded as outcome variables. Thus, it is not appropriate to include them as controls in panel regressions, even if one were to use one-year lags of the variables of interest (see, e.g., Angrist & Pischke 2015, pp. 214-217). Hence, we refrain from doing so. Nonetheless, in unreported analyses we reestimate all the specifications in this paper including one year lags of (i) Return on Assets, (ii) the logarithm of the book value of assets, (iii) capital expenditures, (iv) research and development, (v) leverage, and (vi) a dummy indicating whether research and development expenditures were missing in Compustat-CRSP as controls. All the results we report remain qualitatively the same even after including those controls.

Figure 1: Fraction of firms with staggered boards

Figure 1 shows, for each year between 1996 and 2015, the fraction of firms in our sample that had a classified board in place as of the end of the fiscal year.

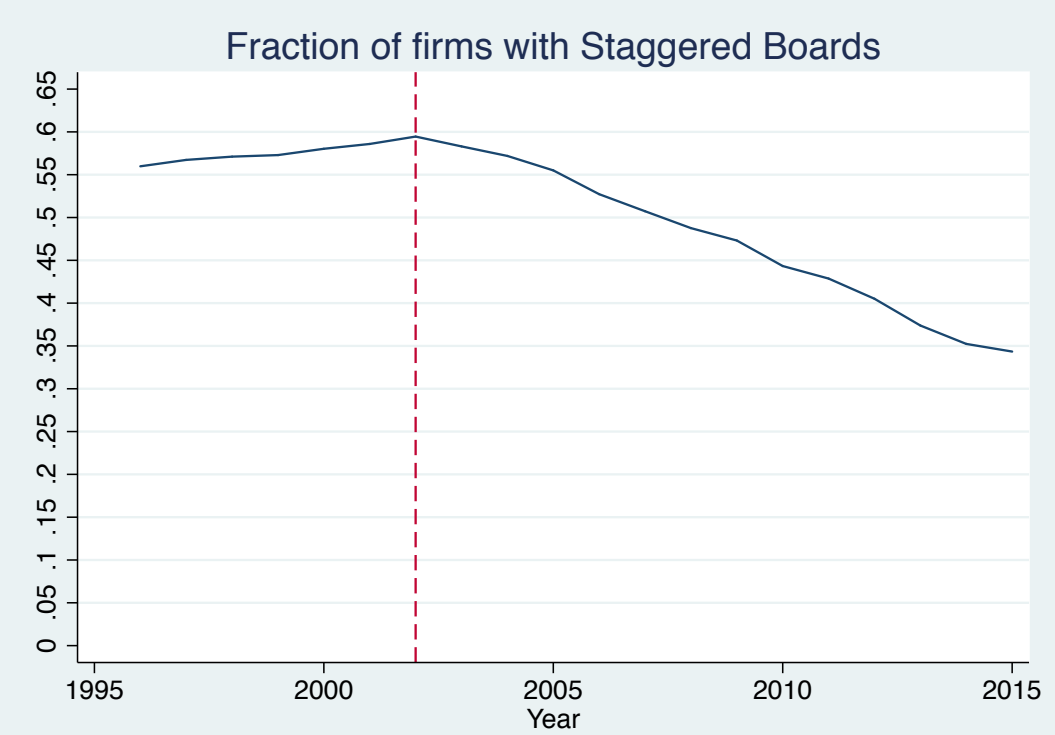


Figure 1

Column 1 of Table 2 reports the results of this estimation.[8] The results reflect a positive association between the presence of a staggered board and firm value that is both statistically and economically significant. The point estimate in column 1 can be interpreted as follows: on average across firms, for each firm, the firm's value while the firm had a staggered board in place is approximately 6.5% higher than that same firm's value after the firm has destaggered its board of directors.[9] This point estimate is comparable in sign, order of magnitude, and statistical significance with those reported by earlier analyses of the effect of board declassification. It amounts to an aggregate destruction of value during the declassification wave of $345 billion![10]

[8] These and all other regressions in this paper are estimated using the *REGHDFE* Stata command (Correia (2015)). Throughout the paper, standard errors are robust and clustered by firm.

[9] Throughout the paper, when we describe the economic significance of a point estimate, we do so by comparing it with the average value of the underlying outcome variable in the sample. Thus, for instance, the 6.5% is calculated by dividing the .138 estimate for the coefficient of *Staggered Board* with the average of Tobin's Q, which is 2.109.

[10] This figure is based on the following back-of-the-envelope calculation: the aggregate market capitalization of the 314 declassifying firms in our database that were still publicly traded as of the end

Table 2: Classified boards and firm value

Table 2 presents annual pooled panel regressions of *Q* against *Staggered Board* (in Column 1); *Staggered Board* and the interaction between *Staggered Board* and *large or very large* (in Column 2); and *Staggered Board*, the interaction between *Staggered Board* and *large*, and the interaction between *Staggered Board* and *very large* (in Column 3). All specifications control for firm, year, and years-since-public fixed effects. *Staggered Board* is defined in Table A1. *Very large* (respectively, *large*) is a dummy equal to 1 if the relevant firm was part of the 10th decile (respectively, the 8th or 9th deciles) of market capitalization as of 2002 or the closest year for which market capitalization was available for that firm (and zero otherwise). *Large or very large* is the sum of *large* and *very large*. Standard errors (in parentheses) are robust and clustered by firm. Statistical significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

VARIABLES	(1) Tobin's Q	(2) Tobin's Q	(3) Tobin's Q
Staggered Board	0.138**	-0.116*	-0.118*
	(0.0560)	(0.0640)	(0.0640)
Staggered*Large or VeryLarge		0.469***	
		(0.0991)	
Staggered*Large			0.279***
			(0.0981)
Staggered*VeryLarge			0.738***
			(0.160)
Observations	28,290	28,290	28,290
R-squared	0.583	0.585	0.585
Year FE	Yes	Yes	Yes
Years since Public FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

As highlighted above, the point estimate for *Staggered Board* reflects an average across all destaggering firms of the drop in value seemingly experienced in connection with the destaggering. However, destaggering firms were highly heterogeneous in terms of their market capitalization. Moreover, as we document in Figure 2, there was significant cross-sectional variability in the frequency and the timing of declassifications for firms of different market capitalization. To construct Figure 2, we sort firms by decile of market capitalization as of year 2002—the year before the destaggering wave started—or the year closest to 2002 for which data is available for the relevant firm,[11] and divide firms into three groups: the "very large"

of fiscal year 2015 was approximately $5.33 trillion. If by re-staggering their boards those firms could recoup 6.5% of value, the cost of the declassification wave was approximately $345 billion.

[11] For the 13% of the observations that correspond to firms that were not part of the sample as of 2002 (either because the firms left the sample before 2002, or because they only entered the sample after 2002), the decile of market capitalization as of 2002 is not defined. For those firms, we instead use their decile of market capitalization during the year closest to 2002 during which market capitalization is available (e.g., if a firm left the sample in 2001, we use the firm's decile of market capitalization as of 2001).

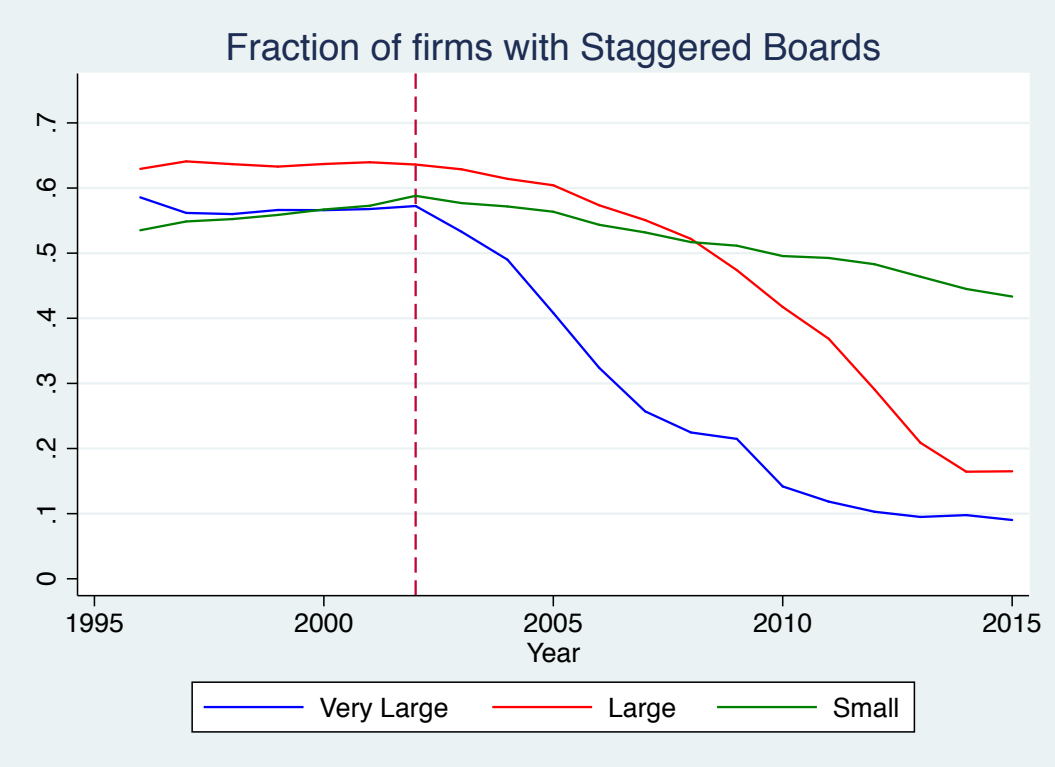
firms (i.e., firms with market capitalization in the top decile of the distribution as of that year); the "large"—but not "very large"—firms (i.e., firms belonging to the 8th and 9th deciles of market capitalization as of that year); and the rest—which we will refer to as "small firms".[12] In what follows, we will refer to each of these three groups as a "size group".

As reported in Figure 2, very large firms destaggered their boards earlier in the declassification wave and at a higher rate than did the rest of the firms, and large firms did so earlier and at a faster rate than did small firms. This is not surprising, since it is well known that very large firms tend to attract the vast majority of shareholder precatory proposals. In addition, one might expect that the very largest firms acceded more easily to shareholder demands because the likelihood was relatively low that financing would be available to mount a hostile takeover of them.

Figure 2: Incidence of staggered boards for three size groups

Figure [INSERT CROSS REFERENCE] reports, for each year between 1996 and 2015, and each of the three size groups of firms in our sample, the fraction of firms with staggered boards. The *very large* (respectively, *large*) group consist of firms that were in the 10th decile (respectively, the 8th or 9th ceciles) of market capitalization as of 2002 (or as of the year closest to 2002 for which market capitalization information was available for the firm). The *small* group consists of firms that are not *very large* or *large*.

[12] We somewhat arbitrarily draw the line for deeming a firm "large" at the 8th decile because firms in deciles 8 to 10 adopted roughly half of all the destaggerings adopted by all the firms in the sample. We separately focus on very large firms because earlier research (Kahan & Rock (2010)) had reported that the incidence of destaggering was particularly high among the largest firms.



Although the fact that larger firms declassified at a much higher rate than smaller firms has already been noticed (e.g., Kahan & Rock (2010)), the implications of that fact for the analysis of the effects of declassification on firm value have not. The specification estimated in Column 1 of Table 2 generates the counterfactual for a firm that declassified its board between two points in time by using the outcome for all other firms that did not change their board structure during that period. That latter group of firms is disproportionately comprised of smaller firms, since smaller firms were more numerous and much less prone to destaggering their boards. If smaller firms systematically outperformed larger firms (regardless of board structure) during the sample period in terms of stock returns, that could lead to biased estimates for the association between staggered boards and firm value. This is far from being merely speculative. Asset pricing scholars have repeatedly documented that over long periods of time stock returns for firms of high market capitalization have been systematically lower than the corresponding returns for firms with lower market capitalization (see, e.g., Fama and French (1993), Fama and French (2015)). Since the market value of equity makes up a substantial fraction of the numerator in the calculation Tobin's Q, this suggests that, over time, Tobin's Q for smaller firms may not offer a good counterfactual for larger firms' Tobin's Q.

This prompts the question: Were the (ostensible) effects of declassification on firm value comparable across firms of different sizes, or where there instead meaningful cross sectional differences in those effects? If there were cross sectional differences, then the differences in the rate and timing of declassification across size groups would become important in interpreting the coefficient in Column 1 of Table 2. As a first step toward understanding the

impact of firm size on the association between declassification and Q, we estimate the following augmented versions of Specification (1) using the three size groups described above:

$$Tobin'sQ_{it} = \alpha_i + \theta Staggered\ Board_{it} + \mu Staggered\ Board_{it} * Large\ or\ VeryLarge_i + \gamma_t + \eta_{it} + \varepsilon_{it}, \quad (2a)$$

$$Tobin'sQ_{it} = \alpha_i + \theta Staggered\ Board_{it} + \mu Staggered\ Board_{it} * Large_i + \delta Staggered\ Board_{it} * VeryLarge_i + \gamma_t + \eta_{it} + \varepsilon_{it}, \quad (2b)$$

where $Large_i$ is a dummy equal to one if firm *i* was part of deciles 8 or 9 of market capitalization as of 2002 or the closest available year (and zero otherwise); $VeryLarge_i$ is a dummy equal to one if firm *i* was part of decile 10 of market capitalization as of 2002 or the closest available year (and zero otherwise);[13] $Large\ or\ VeryLarge_i$ equals $Large_i + VeryLarge_i$; and all other variables have been defined previously.

The results of these estimations are striking. Column 2 indicates that the association between the presence of a staggered board and *Q* is negative for low market-capitalization firms (for which the presence of a staggered board is associated with an average drop in firm value of over 5% that is statistically significant the 10 percent level), while it is positive—and economically much stronger—for large and very large firms. For those groups of firms, the presence of a staggered board is associated with an average increase in value of almost 17%.[14] By further partitioning the set of large and very large firms into those that are merely large and those that are very large, the results of Column 3 provide additional granularity on this last estimate. They indicate that the presence of a staggered board is associated with an 8% average increase in value for large firms, while it is associated with a whopping 29% ((-.118+.738)/2.11) increase in value for very large firms.[15]

These results warrant further scrutiny. To be sure, the presence of a staggered board could have systematically different causal effects on firms of different sizes. Whereas a staggered board could insulate smaller firms from the threat of a hostile takeover—hence reducing their value—it would probably have a minimal effect or no effect on hostile takeovers in the case of very large firms. For very large firms, the importance of a staggered board might be to provide directors with a measure of independence from the CEO—due to their three-year term. This could in theory explain why the presence of a staggered board appears to be systematically more beneficial for firm value in the case of very large firms—or conversely why those firms

[13] In Section 5.2 we confirm that the results are robust to alternative methodologies for sorting firms into different size groups.

[14] The 17% figure is simply the sum of (-0.116+0.469) divided by 2.11. An F-statistic rejects at the 1% level the null hypothesis that the sum of θ and μ equals zero.

[15] With the estimates of Column 3, an F-statistic rejects at the 5% level the null hypothesis that the sum of θ and μ equals zero. The null hypothesis that $\theta + \delta$ equals zero is rejected at the 1% level.

lost systematically more value in connection with their declassifications. On the other hand, the magnitude of the estimates seems way too high to simply reflect heterogeneous causal effects of board structures.[16] Something else must be going on.

A straightforward way to get a better grasp of what that "something else" may be is to study the timing of the purported causal mechanism (as in Catan (2016)). Did the observed drop in value associated with large and very large firms destaggering their boards occur after the board was destaggered or was there an ongoing decline in value that began well before declassification and persisted thereafter? In a within-firm framework, either pattern would result in a negative correlation between board declassification and *Q*. (And conversely, with respect to an observed increase in *Q*, as in the case of smaller firms.) We therefore examine the dynamic evolution of firm value in the years surrounding declassification. That is, we look at whether the firm value of firms that destaggered their boards tended to rise or fall—relative to those that did not destagger—during the years prior to and following the year in which a firm destaggered its board. We do so separately for each of the three size groups of firms separately analyzed in Column 3 of Table 2, estimating the following specification:

$$Tobin'sQ_{it} = \alpha_i + \sum_{\substack{\tau=-8 \\ \tau\neq-1}}^{\tau=+8} \lambda_\tau Declassified\_Small_{\tau it} + \sum_{\substack{\tau=-8 \\ \tau\neq-1}}^{\tau=+8} \kappa_\tau Declassified\_Large_{\tau it} + \sum_{\substack{\tau=-8 \\ \tau\neq-1}}^{\tau=+8} \beta_\tau Declassified\_VeryLarge_{\tau it} + \gamma_t + \eta_{it} + \varepsilon_{it}, \quad (3)$$

where for $\tau < 0$, $Declassified * VeryLarge_{\tau it}$equals 1 for a very large firm that will declassify its board τ years in the future, and zero otherwise; for τ =0, $Declassified\_VeryLarge_{\tau it}$ equals 1 for a very large firm at the end of the fiscal year during which the board was declassified, and zero otherwise; and for $\tau > 0$, $Declassified\_VeryLarge_{\tau it}$ equals 1 for a top-decile firm that has declassified its board τ years in the past, and zero otherwise. $Declassified\_Large_{\tau it}$ is defined analogously for large firms, while $Declassified\_Small_{\tau it}$ is defined analogously for firms that were not large or very large. γ_t and η_{it} are defined as in Equation 1.[17]

The estimation sample consists of the following observations: (1) for all firms that declassified their board during the sample period, all firm-year observations in the 8 years leading to the board declassification, and the 8 years following the board declassification; (2) for all firms that never switched their board structure, all firm-year observations. The latter set of observations

[16] To put the 29% figure into perspective, a back-of-the-envelope calculation like the one described in footnote 10 would suggest that if the 66 very large firms that declassified their boards during our sample period and remained publicly traded at the end of 2015 re-staggered their boards, that would create approximately $1 trillion (or over 5% of the United States GDP as of 2015) in value.

[17] In this specification, the excluded category for eventual board-declassifiers among very large firms is $Declassified\_VeryLarge_{-1it}$. The excluded categories for large and small firms are defined analogously.

make up the control group; for them, by construction, the value of $Declassified\_Other_{\tau it}$, $Declassified\_Large_{\tau it}$, and $Declassified\_VeryLarge_{\tau it}$ is equal to zero for all τ and t.

For ease of interpretation, the results of this estimation are summarized in the three panels of Figure 3, which displays the evolution of relative firm value for very large firms in the left panel, the evolution of relative firm value for large firms in the center panel, and that of the other firms in the right panel.[18]

Figure 3: Tobin's Q - Within-firm dynamics around declassification

Figure 3 presents the relative evolution in Tobin's Q experienced by destaggering firms in each of the three size groups—*very large* firms, the *large* firms, and *small* firms—in the years leading to and following declassification. *Very large* (respectively, *large*) firms are defined as falling within the 10th decile (respectively, the 8th or 9th deciles) of market capitalization as of 2002 (or as of the year closest to 2002 for which market capitalization information was available for the firm). *small* firms are defined as firms that are not *very large* or *large*. The dynamics are recovered by estimating Specification 3. Additional details are available in Table B1 in Appendix B.

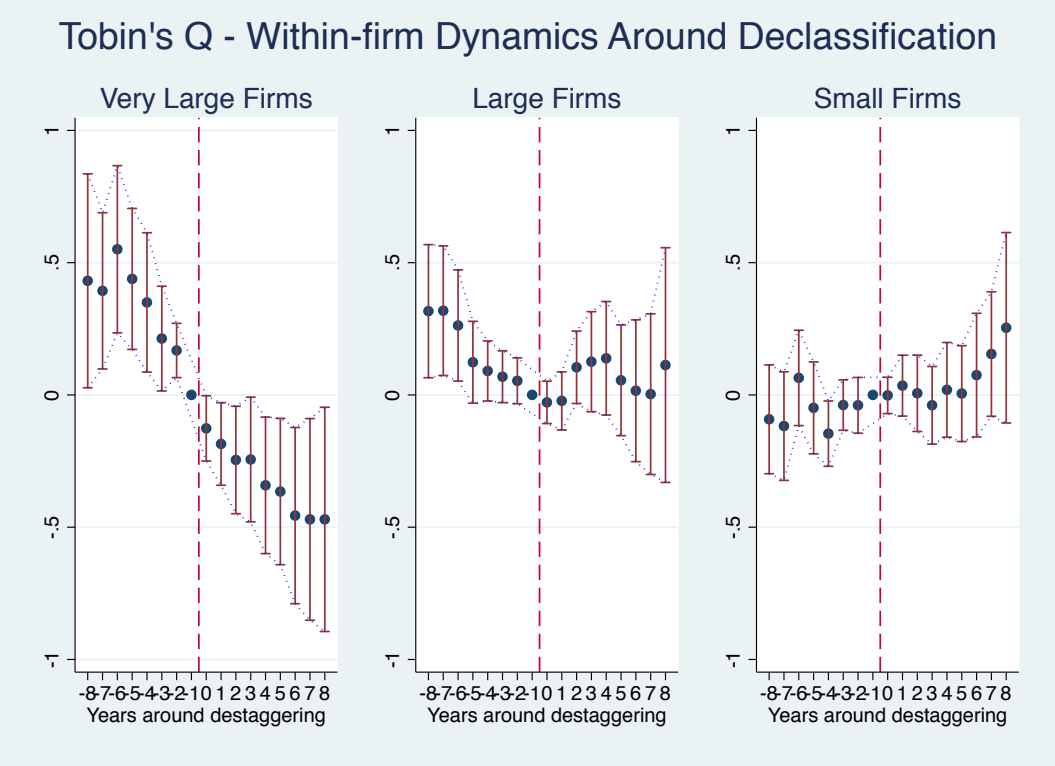


Figure 2

Each of the point estimates can now be interpreted as the differential evolution in firm value experienced by the (eventual) board declassifiers in the relevant size group between year τ around the year of board declassification and end of the fiscal year immediately preceding the

[18] The dashed vertical line indicates that the board was declassified at some point between years "minus 1" and "zero". The vertical bars centered around each point estimate describe 95% confidence bands. The detailed estimates are available in Table B1 of the Appendix.

board declassification, relative to the change experienced in the same period by equally-aged firms that never switched board structure. Thus, the point estimate of -0.126 for the coefficient of $Declassified\_VeryLarge_{0it}$ (which can be observed as the marker corresponding to year "0" in the left panel) indicates that, relative to their peers, declassifying firms in the top market-cap decile experienced a drop in firm value of about 6% between the year immediately preceding the board declassification and the end of the fiscal year during which the board was declassified (a drop that is statistically significant at the 5 percent level).

Figure 3 summarizes a wealth of information. First of all, consistent with the less granular estimates of Column 2 of Table 2, very large firms that declassified their board saw their value drop dramatically and monotonically following the declassification. For those firms, between the year immediately preceding the declassification and year 5 after the declassification, *Q* dropped—on average, relative to firms that did not declassify—by approximately 0.365 units, a drop that is statistically significant at the 1 percent level. This represents a loss in value of over 17%, percent, which if taken at face value has an enormous economic significance. By contrast, on average, firms in the other groups of market capitalization that declassified did not see their value drop in the years that followed. For those firms, the point estimates that capture the changes in value post-destaggering are not statistically significant at conventional levels. For very large declassifying firms, however, the evolution in value after the declassification seems to continue a path that had begun approximately 5 years before declassification. For that group of firms, relative value dropped steadily by about 25% between six years prior to declassification and one year prior (a drop that is statistically significant at the 1% level). By comparison, for lower market-capitalization firms that (eventually) declassified, the evolution of value leading to declassification was essentially flat. Finally, for firms in our large size group, the center panel of Figure 3 indicates that the positive association between the presence of a staggered board and firm value that had been reported for large firms in the third column of Table 3 is simply an artifact driven by the coarse nature of the estimated specification. The relative drop in average value that those firms experienced between the years when they had a staggered board and the years when they had an annual board is due to the fact that value systematically dropped in the years leading to the declassification, and it did not fully recover in the years that followed the declassification.

These results have important implications. First of all, even if one were to stipulate that the post-destaggering relative drop in value documented for very large firms indeed reflects a causal effect of the declassification, the center and right panels of Figure 3 provide no support for the proposition that declassification was systematically linked with drops in value for about 80 percent of firms that destaggered their board. However, that would be little cause for relief from the concern that wholesale value destruction occurred during the declassification wave. Given how right-skewed market capitalization is, very large firms hold the overwhelming majority of the aggregate market capitalization across all publicly traded companies. As a consequence, even if the value-destroying effects of the declassification wave had only existed among very large firms, and even if we only regarded the drop in value experienced by those firms after the declassification as being driven by the declassification, the loss of value would still be massive.

On the other hand, given the evolution of firm value for very large firms surrounding the declassification, one cannot plausibly ascribe the apparent post-declassification drop in value associated with board declassification to the fact of declassification. To put this in technical terminology: This (generalized) difference-in-differences design fails a standard test for the plausibility of the "parallel trends" identification condition.[19] As a consequence, the case for a causal interpretation of the results so far—along with the causal claims of the recent literature— is dubious.[20]

At the same time, however, the large drop in value that very large firms experienced prior to and following their board declassification raises a puzzle: Why was their value dropping at such a steep rate relative to that of their peers? Did they destagger in response to a drop in value? Did they do so because they expected things to get worse? To shed light on these questions, we compare the evolution of average *Q* over time for small, large, and very large firms.

[19] In order for the within-firm estimates of Table 2 to be unconfounded, one needs to assume that, if the declassifying firms had not declassified their boards, the value of *Q* for those firms would have moved in lockstep—hence the "parallel trends" name—with the value of *Q* for the firms in the control group (which in fact did not change their board structure). By construction, for the firms that eventually declassified their board, the board structure was constant in the years leading to declassification. Hence, one can ask whether, during that period, the value of *Q* for the eventual board-declassifiers was moving in lockstep with that of firms that never changed their board structure. It is useful to see this exercise as a "placebo test." Under the null hypothesis of "parallel trends" over the pre-destaggering period, there should not be a differential evolution in firm value among the two groups of firms (the eventual declassifiers and the control firms) over that period. Evidence of such differential evolution makes the assumption of unconfoundedness much less plausible.

[20] In section 5 we consider—and reject—the possibility that the pre-treatment trends experienced by very large firms may simply be driven by the expectation that those firms would declassify their boards.

Figure 4: Time series in *Q* for three size groups

Figure 4 reports, for each year between 1996 and 2015, and each of three size groups in our sample, the average value of Tobin's Q at the group-year level. The three groups consist of: (a) very large firms, defined as falling within the 10th decile of market capitalization, (b) large firms, defined as falling within the 8th and 9th deciles, and (c) small firms, defined as falling within the 1st through 7th decile, all as of 2002 (or as of the year closest to 2002 for which market capitalization information was available for the firm).

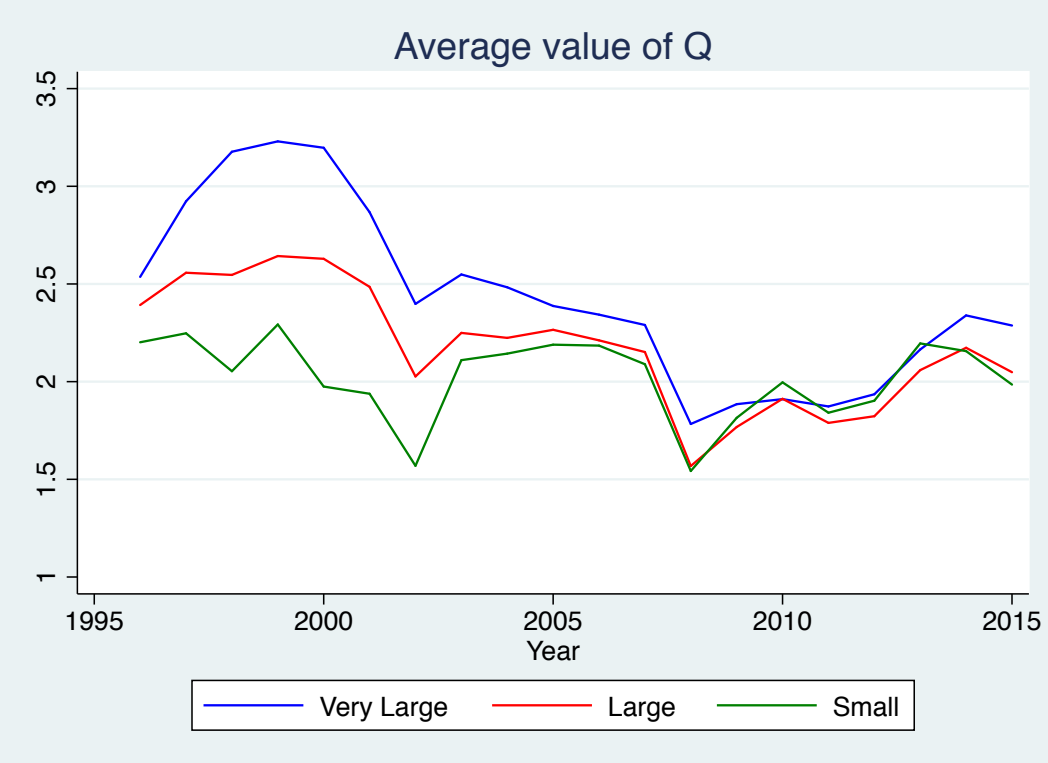


Figure 3

Figure 4 plots the average *Q* for firms in the top decile of market cap and firms in the other deciles from 1996 to 2015. As shown there, after 1999, there was a precipitous drop in *Q* for very large firms, and to a lesser extent for large ones. The drop experienced very large firms reflects a dramatic reversal of fortune. Tobin's Q for very large firms had skyrocketed in the late 1990s and then abruptly dropped during the early 2000s. Most importantly for our analysis, since 1999, the differential in average *Q* that very large firms displayed relative to all other firms increasingly shrank, and by 2011 it had all but vanished. This convergence in *Q* took place at the same time as the destaggering wave.

As Figure 2 documents, very large firms destaggered much more and earlier than the rest. Could the relative drop in value experienced by very large firms be due to the fact that those firms declassified their boards at a much higher pace than the remaining firms? That interpretation would at least make sense of our findings so far and would be consistent with prior analyses attributing a causal relationship between staggered boards and firm value.

On the other hand, the apparent magnitude of the drop still strains credulity. Moreover, it is well known that firms of different market capitalization experience systematically different

stock returns over long periods of time. Consequently, the market value and hence Tobin's Q of firms of different sizes could converge and diverge over long periods of time. If during the sample period the trend of stock prices for very large firms was systematically different from the trend of stock prices for smaller firms, for reasons that had nothing to do with changes in board structure, then specifications—as those estimated in Table 2—that implicitly compare firms that are not very large firms with firms that are very large would lead to biased estimates for the coefficient of *Staggered Board*.[21]

A look at some anecdotal evidence suggests that this turns out to be more than just a theoretical concern. Figure B2 in Appendix B reports the cumulative returns experienced by a zero investment diversified portfolio that is long in firms with high market capitalization and short in firms with low market capitalization.[22] Between January, 1996 and mid-1999, that portfolio experienced positive returns. Thereafter, the portfolio experienced a dramatic reversal of fortune. Figure B3 in Appendix B reinforces the evidence of the size-based portfolio analysis. That Figure describes the evolution of four subcomponents of the S&P 1500 index during the sample period, relative to the value that each of the subindexes had as of January 1996. Two interesting patterns emerge from that figure. Early in the sample period, larger firms systematically outperformed smaller firms.[23] Since January, 2000, however, the tide changed dramatically, and smaller firms systematically outperformed larger firms.[24] To be sure, neither of these two figures is constructed exclusively on the basis of the firms in our sample. In addition, in principle some of the systematic differences in stock performance among firms of different sizes may actually be driven by the fact that differently-sized firms were destaggering

[21] To see intuitively why the counterfactuals are being drawn overwhelmingly from the set of firms that are not very large, it suffices to see that 90% (respectively, 70%) of the observations in the sample, and probably an even higher fraction among those that correspond to firms that did not change their board structure, correspond to firms that are not very large (respectively, large). To the extent that very large firms experience systematically different shocks to their value relative to all others (as Figure 4 suggests), that will lead to *apples-to-oranges* comparisons. Interestingly, the likely upward-bias of the estimate for the impact of staggered boards in the case of very large firms is only one of the consequences of the inaccurate construction of the counterfactual. The specifications estimated in Table 2 also use the evolution of *Q* for *very large* firms that did not switch their board structure to construct the counterfactual *Q* for firms without large or very large market capitalization that declassified their board. That is likely to lead to a downward-biased estimate for the coefficient of the *Staggered* Board dummy (although given that only a small fraction of the counterfactual is likely to be based off very large firms, the magnitude of the bias is likely to be smaller).

[22] The figure is constructed on the basis of the Fama/French 5 Factors (2x3) [Daily] available at Kenneth French's website. Starting on January 1, 1996, we accumulate the daily returns of the "Small minus Big" portfolio (and then multiply them by minus one, since our focus is on a portfolio that is long on the large firms and short on the small ones).

[23] By January, 2000, the S&P 100 (which is comprised of "megacap" firms) was worth 2.47 times its January, 1996 value. By contrast, the S&P 500 (which subsumes the S&P 100, together with other 400 "large cap" firms) had seen its value increase by a factor of 2.19. The corresponding multiples for the S&P 400 Midcaps index and the S&P 600 Smallcaps index were 1.96 and 1.59, respectively.

[24] Between January, 2000, and January, 2015, the S&P 100 index only increased by a multiple of 1.16. The respective figures for the S&P 500, the S&P 400, and the S&P 600 are 1.43, 3.32, and 3.56.

at different rates. However, the anecdotal evidence in the two figures heightens the concern that our estimation strategy so far—and that of the existing literature—may suffer from omitted variables bias. Specifically, in a within-firm or difference-in-differences design that does not control for the possibility that groups of firms of different sizes may experience differential trends in their value, part of those differential trends would be spuriously picked up by the estimates of the coefficients for other time-varying regressors, like the $Staggered\ Board$ dummy, or the interaction of that dummy with dummies identifying subsets of firms of a given size, that are systematically correlated with the differential trends in the time series. The flip-side of this analysis is the testable hypothesis that the estimates for the coefficients for those time-varying regressors should change dramatically if one replaced the coarse specification by a more granular specification that explicitly accounts for those differential trends in value.

As a first step to tackling this concern more effectively, we revisit Figure 4, and take advantage of the fact that, for each of the three size groups that we analyze separately, approximately 40% of firms had an annually elected board throughout the sample period. That enables us to break down each of the time series in Figure 4 into two: the time series of *Q* among those firms in the relevant market-capitalization group that had an annual board throughout the sample period; and the corresponding time series among firms that started the sample period with a staggered board of directors (regardless of whether they then destaggered their boards). The corresponding time series are described in Figure 5. The solid blue line plots the average value of Tobin's *Q* for very large firms that had a staggered board as of their first year in the sample. The dashed blue line plots *Q* for very large firms that had an annually elected board throughout the sample period. The solid red line plots *Q* for large firms that had a staggered board as of their first year in the sample. The dashed red line plots *Q* for large firms that had an annually elected board throughout the sample period. The solid (respectively, dashed) green line plots *Q* for small firms that had a staggered board (respectively, annually elected board) as of their first year in the sample.

The trends shown in Figure 5 suggest that the convergence of value among very large firms and the rest of the firms is unrelated to the disproportionate incidence of board declassification among very large firms. First, the Tobin's Q of very large and the rest of the firms that had annually elected boards throughout the sample period initially diverged and then converged between 1999 and 2010 (as one can see by comparing the blue dashed line with the other two dashed lines). This suggests that, throughout the sample period, the stock prices of very large firms were subject to secular trends that did not similarly influence the stock prices of the rest of the firms, and that this difference was unrelated to board declassification. In addition, during 1996-2002 (i.e., before the declassification wave began) the Tobin's Q of very large firms with annually elected boards follows a path that is more similar to the path followed by that of very large firms with staggered boards than to the paths followed by that of any of the remaining groups of firms. This indicates that very large firms with annually elected boards provide a better counterfactual for very large firms that began the sample period with a staggered board

than do smaller firms.[25] Finally, the fact that after 2003 Tobin's Q for very large firms with and without staggered boards move in lockstep toward convergence with the rest of the firms suggests that the convergence is not attributable to the fact that very large firms destaggered at a higher rate than did the rest of the firms.

Figure 5: Time series in *Q* for six subsets of firms

Figure 5 reports, for each year between 1996 and 2015, and each of six groups of firms in our sample, the fraction of firms with staggered boards at the group-year level. The four groups consist of (1) very large firms that had an annual board of directors throughout the entire 1996-2015 period; (2) very large firms that had a staggered board of directors as of their first year in the sample; (3) large firms that had an annual board of directors throughout the entire 1996-2015 period; (4) large firms had a staggered board of as of their first year in the sample; (5) small firms that had an annual board of directors throughout the entire 1996-2015 period; (4) small firms had a staggered board of as of their first year in the sample.

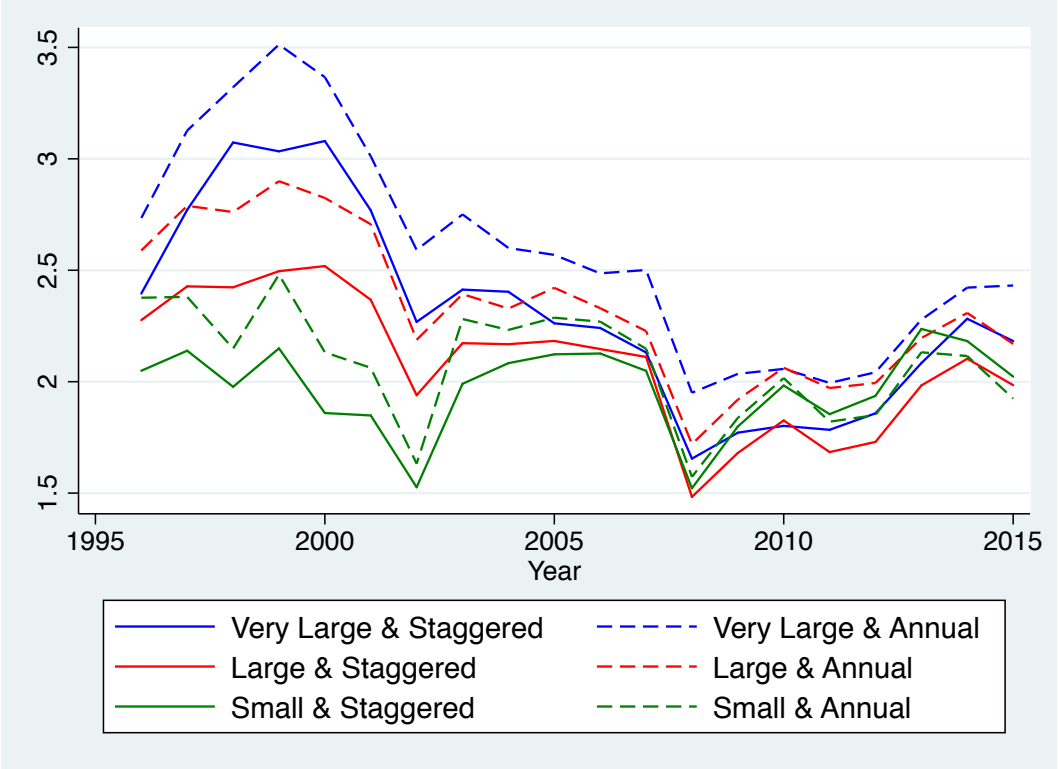


Figure 4

Figure 5 also suggests a reason why the estimates of Table 2 and the trends shown in Figure 2 indicate that very large declassifying firms experienced such a precipitous drop in value, relative to other firms, both leading to and following their declassification. Those firms declassified their

[25] By the same token, the lockstep evolution of Tobin's Q during 1996-2002 for firms of different board structures within each of the remaining size groups suggests that large (respectively, small) firms with annually elected boards throughout make a better counterfactual for large (respectively, small) firms that began the sample period with a staggered board than do firms in other size groups.

boards earlier and in greater volume, and they did so during a period in which the relative premium in *Q* that very large firms enjoyed—regardless of their board structure—was dropping. To phrase this somewhat differently, the analysis so far suggests that at least part of the reason for the statistically significant point estimates for $Staggered\ Board_{it}$, $Staggered\ Board_{it} * Large\ or\ VeryLarge_i$ and $Staggered\ Board_{it} * VeryLarge_i$ in Columns 1, 2, and 3 of Table 2, respectively, and the post-declassification drop in value documented for very large firms in the left panel of Figure 3 is that those specifications do not define a "peer" in a precise enough way: Neither year nor age fixed effects adequately tackle the concern that firms in different size groups may have experienced differential time varying shocks to *Q*.

Although time-series analyses like those in Figures 4 and 5 have the advantage of being easy to interpret, some of the dynamics reported in those analyses may be driven by changes in the firms that comprise the sample (as opposed to being driven by within-firm changes in the outcome variable over time). Moreover, even if Figures 4 and 5 strongly suggest that the results of Table 2 are substantially driven by the differential secular trends in value experienced by larger firms, one cannot conclusively answer by simply looking at those Figures whether those differential trends account for all of the results of Table 2. To answer that question, we revisit the results of Table 2**.** To mitigate potential omitted variables bias driven by the differential evolution of *Q* experienced by very large firms regardless of whether they destaggered, we augment equations (1), (2a) and (2b) by replacing the year fixed effects with a set of size-group-by-year fixed effects.[26] Table 3 reports the results of that estimation.

Table 3: Classified boards and firm value — more granular specification

Table 3 presents annual pooled panel regressions of *Q* against *Staggered Board* (in Column 1); *Staggered Board* and the interaction between *Staggered Board* and *large or very large* (in Column 2); and *Staggered Board*, the interaction between *Staggered Board* and *large*, and the interaction between *Staggered Board* and *very large* (in Column 3). All specifications control for firm, size-group – by – year, and years-since-public fixed effects. *Staggered Board* is defined in Table A1. *Very large* (respectively, *large*) is a dummy equal to 1 if the relevant firm was part of the 10th decile (respectively, the 8th or 9th deciles) of market capitalization as of 2002 or the closest year for which market capitalization was available for that firm (and zero otherwise). *Large or very large* is the sum of *large* and *very large*. Standard errors (in parentheses) are robust and clustered by firm. Statistical significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

VARIABLES	(1) Tobin's Q	(2) Tobin's Q	(3) Tobin's Q
Staggered Board	0.0126	-0.00388	-0.00402
	(0.0520)	(0.0657)	(0.0657)
Staggered*Large_or_VeryLarge		0.0340	
		(0.104)	

[26] We recover qualitatively identical results if instead of interacting the year fixed effects with a vector of size group fixed effects we interact them with a vector of market capitalization decile (also defined as of 2002, or the closest available year) fixed effects.

Staggered*Large			-0.0119
			(0.106)
Staggered*VeryLarge			0.0998
			(0.169)
Observations	28,290	28,290	28,290
R-squared	0.595	0.595	0.595
Year FE	No	No	No
Years since Public FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Size-Group x Year	Yes	Yes	Yes

As suggested in light of the coarser analysis of Figure 5, the estimate of the coefficient for $Staggered\ Board_{it}$ in Column 1 of Table 3 is now much smaller than its analogue from Column 1 of Table 2, and it is no longer statistically significant. By the same token, focusing on Column 2 of Table 3, the point estimate of the coefficient for $Staggered\ Board_{it} * Large\ or\ VeryLarge$ attenuates by over 95 percent. Finally, the estimates for $Staggered\ Board_{it} * Large_i$ and $Staggered\ Board_{it} * VeryLarge_i$ also become drastically smaller, and neither remains statistically significant at conventional levels (even though the standard errors associated with the estimates only increase marginally as one goes from Table 2 to Table 3).[27] Finally, and also in line with our earlier hypothesis in footnote 21, the point estimates for the coefficient of $Staggered\ Board_{it}$, which were significantly negative in Columns 2 and 3 of Table 2, also attenuate substantially, and become statistically insignificant.

Our decision to augment the specifications estimated in Table 2 by including size-group - by - year fixed effects is not only intuitively sensible: it is also consistent with current best practices. The conventional way to augment a difference-in-differences or within-firm design to test whether a particular subset of treated subjects was differentially affected by the treatment is to use a "difference-in-difference-in-differences" (or "triple-differences") design. Textbook expositions of the triple-differences methodology suggest that one should control for the possibility that the group that was hypothetically differentially affected by treatment may have experienced a differential outcome across the board (i.e., independently of treatment) during the "post-treatment" period.[28]

The recent studies by Cremers and Sepe (2016), Cremers, Litov and Sepe (2016) and Ge, Tanlu and Zhang (2016) concluding that the destaggering wave destroyed value are problematic

[27] An F-statistic for the null hypothesis that the sum of the coefficients for *Staggered* and *Staggered*Large or VeryLarge* equals zero fail to reject the null (*p-value*: .71). Similarly, an F-statistic for the null hypothesis that the sum of the coefficients for *Staggered* and *Staggered*VeryLarge* equals zero fail to reject the null (*p-value*: .54). In unreported analyses, we reestimate the specification of Column 1 in Table 3 using only the sample of very large firms. Consistent with the results of Column 3 of Table 3, the estimated coefficient for *Staggered Board* is .097 (standard error: .157, p-value: .54).

[28] See, e.g., Wooldridge (2010, p. 150-1), Angrist and Pischke (2009, p. 241-2).

in a few ways. First, none of these papers adequately take account of the differential trends in *Q* experienced by the largest firms relative to all others. Cremers et al.'s matched sample regressions could in principle have mitigated the impact of the differential trends experienced by very large firms and others. But their various matching approaches failed to adequately address this problem. Their nearest-neighbor approach did not include any proxy for firm size as part of the vector of covariates used to define a match, and their other approaches that attempted to take account of size used the log of book value of assets as a proxy. Ge et al. also used the log of book value in their propensity score matching. This would not address the distinctive trend in *Q* experienced by very large firms. One of the reasons why the largest firms experienced a relative drop in *Q* both leading to and following their decision to destagger is precisely that their market capitalization was disproportionately large relative to their book value of assets leading to the destaggering wave. Finally, even if one attempted to explicitly include market capitalization as an argument of the propensity score function, doing so would probably be challenging, and require a very fine-grained analysis that goes beyond including a transformation of market-cap as a linear argument of that function. This is particularly the case for the earliest years of the declassifying wave, when declassifying firms tended to systematically include the very largest firms by market capitalization that still had a staggered board in place (which implies that any match for such firms would typically involve a firm with systematically lower market capitalization).[29]

Second, none of these papers tests for the existence of pre-treatment trends. As a result, they failed to see that, for firms that declassified their boards, *Q* had been declining for several years (as a result of the unrelated secular trend for very large firms documented above) and that this downward trend continued following declassification.[30] That kind of pre-treatment differential trend would not only raise substantial doubts about the validity of the parallel trends identification assumption. It would also suggest that the difference-in-differences and within firm point estimates are downward biased.[31]

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[29] Relatedly, both Cremers et al. (2016) and Ge et al. (2016) construct their propensity-score based matched samples relying on a global estimation of the propensity score function (as opposed to running a separate estimation of the propensity score each year). To the extent that market capitalization became a less relevant determinant of the propensity to declassify as the declassification wave elapsed, that is likely to lead to imprecise estimates of the largest firms' propensity to declassify their boards during the earlier years of the declassification wave.

[30] This is actually reflected in Ge et al.'s results. In reporting the results of their covariate balance test, in Table 2, Panel C Ge et al. indicate that one and two years before the destaggering, declassifying firms had an average *Q* that was 0.051 units and 0.120 units, respectively, higher than the matched control firms. In addition, the results of the difference-in-difference estimation of Column 1 of Table 3 indicate that, during the entire pre-treatment period, average *Q* for the treatment firms was 0.307 units higher than for the control firms (an estimate that is statistically significant at the 5 percent level). This suggests that, consistent with the evidence we report in our paper, treatment firms were experiencing a relative drop in *Q prior to* declassification.

[31] In addition, since Ge et al.'s difference-in-differences design does not control for firm fixed effects, it is unclear whether the estimates are driven by within-firm changes in the outcome variable or whether, instead, they are simply driven by changes in the composition of the samples as time evolves.

Finally, and somewhat puzzlingly, the results of Cremers et al.'s long-run event studies intended to estimate the risk-adjusted long-run price effects of board declassifications fail to yield any evidence of a negative impact. Their portfolios that took short positions in companies before they destaggered reaped no abnormal returns.

Before closing this part of our discussion, it bears noting what our results did *not* show. We did not find that the board declassification wave *increased* firm value on average, as one would expect based on the literature prior to the current decade. As described above, that literature, based primarily on the adoption of staggered boards during the 1980s and 1990s, found that a staggered board was associated with lower firm value. The fact that our results based on a period that began in 2002 raise the possibility that as of this time, managements and boards faced pressures made them more open to attractive takeover bids than they had been in the past.

The recent literature finding that board declassification destroys value attributes the purported destruction of value at least in part to a loss of long-term focus. That literature, for the most part, uses high R&D expenditures to identify firms with long-term focus. Although we have found no evidence that declassification destroyed value across the board, we nonetheless proceed to analyze more specifically whether firms with high R&D experienced drops in value when they destaggered their boards.

4.3 Board Declassification and Long-Term Investment

In addition to documenting the (purported) negative effect of board declassification on firm value, Cremers and Sepe (2016), Cremers et al. (2016) and Ge et al. (2016) attribute at least part of the drop in value ostensibly suffered by firms that destaggered their boards to a decline in long-term focus. They base this conclusion on sensitivity analyses suggesting that firms with high R&D and other measures of long-term investment were systematically more harmed by declassifications. As explained in Section 3, others have argued as well that a staggered board promotes long-term investment (Lipton and Shaffer (2015)). Relatedly, some have argued that a staggered board promotes long-term relationships with suppliers, customers, employees and others. (Knoeber (1986), Shleifer and Summers (1988), Johnson, Karpoff and Yi (2015)).

Even if the recent studies of board declassification do not support the conclusion that massive losses in firm value have occurred across the board, it is still possible that declassification impairs management's long-term focus and thereby reduces firm value specifically for firms that have maintained a long-term focus prior to declassification. In this section, following Cremers and Sepe and Cremers et al., we investigate whether R&D-intensive firms suffered particularly large drops in Q as a result of declassifying.

We begin this analysis by continuing the fixed effects framework employed above. To determine whether declassification had a negative impact on firms that invest intensively in R&D, we augment Specification 1 by interacting the $Staggered\ Board_{it}$ dummy with the

$RD\ intensive_i$ dummy. The latter dummy is constructed as follows: for firms that were part of the sample as of year 2002, the dummy is simply equal to 1 (for every year) if the firm reported R&D expenditures as a fraction of sales in the top 25th percentile of the distribution for fiscal year 2002, and zero otherwise.[32] For the firms that were not part of the sample as of 2002, we instead set the dummy equal to 1 (for every year) if the firm reported R&D expenditures as a fraction of sales in the top 25th percentile of the distribution as of the year closest to 2002 for which the firm appears in the sample, and zero otherwise.[33]

Table 4: Classified boards and firm value: focusing on R&D-intensive firms

Table 4 presents annual pooled panel regressions of *Q* against *Staggered Board* (in Column 1), and *Staggered Board* and the interaction between *Staggered Board* and *R&D Intensive* (in Columns 2 and 3). All specifications control for firm and years-since-public fixed effects. Column 1 additionally controls for year fixed effects. Columns 2 and 3 control for size-group – by – year fixed effects. Column 3 controls for *R&D Intensive*-by-year fixed effects. *Staggered Board* is defined in Table A1. *R&D Intensive* is a dummy equal to 1 if the relevant firm was part of the top quartile of R&D (as a fraction of sales) as of 2002 or the year closest to 2002 for which the relevant firm appeared in the sample (and zero otherwise). Standard errors (in parentheses) are robust and clustered by firm. Statistical significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	(1)	(2)	(3)
VARIABLES	Tobin's Q	Tobin's Q	Tobin's Q
Staggered Board	0.0162	-0.106**	0.00170
	(0.0540)	(0.0523)	(0.0501)
Staggered * R&D Intensive	0.735***	0.714***	0.239
	(0.186)	(0.164)	(0.169)
Observations	28,290	28,290	28,290
R-squared	0.585	0.597	0.615
Year FE	Yes	No	No
Years since Public FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Size-Group x Year FE	No	Yes	Yes
R&D Intensive x Year FE	No	No	Yes

Table 4 reports the results of the estimation. The economically small and statistically insignificant estimate for the coefficient $Staggered\ Board_{it}$ in Column 1 indicates that, in the

[32] In Table 7, we corroborate that the conclusions we report in this section are qualitatively the same if we instead draw the line for "R&D Intensive" firms at the top 50th or top 10th percentile, as opposed to the top 25th percentile.

[33] As in the case of $Large_i$ and $VeryLarge_i$ above, we construct the $RD\ intensive_i$ dummy in this time-invariant manner—as opposed to using a more straightforward measure of R&D intensity that asks for each year whether a given firm was above the relevant percentile of the R&D distribution—because firms could alter their R&D strategies in response to declassification. In that case, using the time-variant measure would amount to including a "bad control" on the right-hand side (Angrist & Pischke, 2010).

sample at large, for firms for which the *R&D intensive* dummy equals zero—which we will refer to with the shorthand "low-R&D firms"— there is no within-firm association between the presence of a staggered board and firm value. By contrast, the estimate of the coefficient of $Staggered\ Board * RD\ intensive_{it}$ is statistically significant at the 1 percent level. Additionally, the sum of the coefficients of $Staggered\ Board_{it}$ and $Staggered\ Board * Positive\ RD_{it}$ equals 0.737. That indicates that, for R&D intensive firms the presence of a staggered board is associated with a massive 35% increase in Tobin's Q that is statistically significant at conventional levels.[34] This can be seen as broadly consistent with some of the findings that Cremers et al. report in Table 10 of their paper.[35] But it is also implausible, in light of the sheer magnitude of the point estimate.

In Column 2, following our methodology in Section 4.2, we replace the year fixed effects by the more granular size-group-by-year fixed effects. Except for the fact that the point estimate for the coefficient of the stand-alone Staggered Board dummy becomes negative and statistically significant, the results remain qualitatively the same as in column 1. One could take the fact that the estimates did not vary substantially from Column 1 to Column 2 as evidence bolstering a causal interpretation of the estimate of the coefficient for the interaction between *Staggered* and *R&D Intensive*.

However, we still must approach these results with caution. Perhaps, just as *Q* behaved differently for firms with different capitalization, *Q* may have behaved differently over our sample period among high-R&D firms compared to low-R&D firms for reasons unrelated to changes in board structure. We now investigate that possibility. Parallel to our analysis in Section 4.2, we start by examining, in Figure B7 of Appendix B, the evolution of average *Q* over time for R&D intensive and low-R&D firms.

As Figure B7 documents, during the sample period, the time series of average *Q* for R&D intensive firms behaved dramatically differently from the corresponding time series for low-R&D firms: average *Q* behaved much more turbulently for R&D-intensive firms than for low-R&D ones, especially in the early part of the sample period, when *Q* initially skyrocketed and then plummeted for R&D-intensive firms, while it remained much more stable for low-R&D firms.[36] These differential patterns of behavior were not only present in the sample at large: they were also present *within each of the size groups*, as documented in Figure B 8 of Appendix

[34] The p-value associated with the F-statistic for the null hypothesis that the sum of the coefficients for *Staggered Board* and *Staggered * R&D Intensive* is zero equals 0.0000.

[35] We use the expression "broadly consistent" for two reasons. First, the analysis in Cremers et al.'s paper does not focus exclusively on board destaggerings as the driver of the within-firm estimate they report. Additionally, the specification estimated in Cremers et al. interacts the staggered board dummy with a variable that measures the one-year lag of R&D expenditures (as a fraction of the firm's sales). As noted above, using such a time-varying variable to construct the interaction amounts to including a "bad control" as an explanatory variable.

[36] The turbulent trend experienced by R&D-intensive firms is likely to be at least partially driven by the dot-com bubble. See, e.g. https://www.economist.com/news/finance-and-economics/21725600-sector-has-changed-lot-last-peak-tech-stocks-have-regained

B.[37] Figure B 8 strongly indicates that one should not take too much comfort from the fact that the estimate of $Staggered\ Board * RD\ intensive_{it}$ in Table 4 was robust to the inclusion of size-group - by - year fixed effects. In particular, the fact that the time series behaved in such a discordant way during 1996-2002—when the destaggering wave had still not begun—suggests that attempting to use non-R&D intensive firms to construct a counterfactual for R&D intensive ones—even if one is focusing on firms in the same market-cap decile and year, as in Column 2 of Table 4—is unlikely to yield unconfounded estimates, since R&D intensive and non-R&D intensive firms clearly experienced systematically different trends in *Q* during the period of analysis.

Figure B9 in Appendix B, however, suggests a different, and arguably more plausible identification strategy. That figure decomposes each of the two time series of Figure B into two: the time series for firms in the relevant R&D category that started the sample period with a staggered board and the time series for firms in the relevant R&D category that started the sample period with an annually elected board. The fact that over 1996-2002 the two blue lines moved in lockstep and the two red lines moved in lockstep indicates that, for R&D intensive firms, other R&D intensive firms make up a much more plausible counterfactual than non-R&D intensive firms (and vice versa). As a consequence, a better way to draw *apples-to-apples* comparisons is to compare R&D intensive firms that declassified with *other R&D intensive* firms that did not switch board structure in the relevant year, and the same with low-R&D firms. That can be implemented in a straightforward manner in a regression framework, by simply controlling for R&D intensive - by - year fixed effects, which effectively compare firms in a given year with other firms of the same R&D intensity in the same year.

The results are reported in column 3 of Table 4. Augmenting the specification by adding R&D intensive - by - year fixed effects leads to a dramatic attenuation of the estimate for the coefficient for $Staggered\ Board_{it} * RD\ intensive_i$. Once one compares firms with other similarly sized firms of similar R&D intensity, that point estimate becomes statistically insignificant at conventional levels (and so does the sum of the estimates for the coefficients of $Staggered\ Board * RD\ Intensive$ and $Staggered\ Board$).[38] As noted in our discussion of the results reported in Table 3, our decision to augment the specification estimated in Column 2 by including R&D intensive - by - year fixed effects is in keeping with best empirical practices. It follows the conventional approach to augment a difference-in-differences or within-firm design to test whether a particular subset of treated subjects (in this case, R&D-intensive firms) was differentially affected by the treatment.[39]

[37] In fact, in unreported results, we find that the result is even more general: it holds within each market capitalization decile.

[38] Importantly, the drop in statistical significance is driven purely by the attenuation of the point estimate, since the standard errors do not change meaningfully as one moves from Column 2 to Column 3 of Table 4.

[39] The criticism we level in this subsection at the R&D-related sensitivity analyses in Cremers et al. (2016) and Cremers and Sepe (2016) also extends to all the other sensitivity analyses reported in those two papers. Indeed, none of those analyses control for the possibility that the type of firms that are

We thus conclude that the apparent association between board declassification and *Q* reported in Columns 1 and 2 of Table 4 is spurious, and that once one takes a more careful look at the data, one cannot infer any statistically significant relationship between board declassification and *Q* for R&D-intensive firms. Thus, like the results of the recent literature regarding the effect of board declassification generally, the results with respect to R&D intensive firms seem doubtful.

5. Robustness

The results of Section 4 prompt several questions. First, could the negative pre-treatment trends identified for very large firms in the left panel of Figure 3 be driven by the expectation that those firms would destagger their boards? Second, to what extent do the results hinge on the fact that we sorted firms into different groups (very large, large, or small; R&D intensive or low-R&D) on the basis of firm characteristics defined as of 2002 or the closest year for which data was available? Third, would the results about the differential effects of board destaggerings on R&D-intensive firms be stronger if we defined "R&D-intensive" more narrowly (or more broadly)? We tackle each of those concerns in this section.

5.1 **Pre-Treatment Trends or Expectations?**

In our analysis of the dynamic evolution of firm value for very large firms, we interpreted the stark negative pre-treatment trend in *Q* experienced by those firms as strong evidence that all large firms were experiencing differential secular trends for reasons that had nothing to do with the declassification wave. If declassifications were completely unforeseeable, that would be the natural inference to draw. However, one could argue that the market might have foreseen some of the declassifications before they were formally adopted, and hence it might have driven down the value of those firms in advance of their declassification. One could therefore challenge our interpretation by arguing that the pre-treatment trend we document simply indicates that the markets were already impounding the effects of the expected declassifications before firms became formally "treated."

We think that challenge would be invalid for multiple reasons. First, it sounds far-fetched that markets could have foreseen declassifications five years before they were adopted. As shown in Figure 3, *Q* started to trend downward for very large firms between five and six years prior to destaggering. Second, one must assume irrationality—and not mere ignorance—on the part of shareholders to interpret the data as showing that the very same shareholders that bid down a firm's shares because they expected the firm would declassify down the road would then overwhelmingly vote in favor of destaggering. Third, if expectations of destaggering drove

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hypothesized to benefit systematically more from the presence of a staggered board may have experienced differential time trends across the board (i.e., regardless of whether they switched their board structure).

down share prices, why did it take an average of five or six years after declassification for prices to fully adjust to the news, as shown in the left panel of Figure 3? Fourth, as shown in Figure 5, firms in the same size group with and without staggered boards followed the same trends in *Q*. Among very large firms, those that had annually elected boards during the entire sample period experienced the same drop in *Q* as did those that destaggered their boards during the sample period. Relatedly, if the pre-treatment trends are driven by expectations, one would expect those trends to persist even after one reestimates an augmented version of Specification 3 that controls for size-group – by – year fixed effects. However, when we perform that exercise in unreported analyses, the pre-treatment trends documented in the top panel of Figure 2 vanish—and so the estimates for the post-treatment effects, in line with the results reported in Table 3. Finally, not all declassifications were equally predictable. It was quite plausible that one would have predicted in, say, 2010 that essentially all very large firms that still had a staggered board would soon declassify. It is far less plausible to argue that someone could have forecast the destaggering wave before it began. Under these assumptions, one would expect the results reported in Table 2 to remain stable if one re-estimates the specifications underlying those results using data from 2002 to 2015. However, that is not the case.[40]

5.2 **Firm Sorting**

The analyses in Section 4 sorted firms into size and R&D-intensity groups on the basis of the firms' characteristics as of 2002—the year before the destaggering wave began—or as of the closest year when the firm appeared in the sample. For firms that declassified their boards relatively early during the declassification wave, that sorting is quite likely to lead to *apples-to-apples* comparisons. For example, assume that firm *i* declassified its board during 2003, and that that firm had a very large market capitalization as of 2002. Then it makes sense to use the evolution of Tobin's Q leading to and after 2003 for other firms with a similar market capitalization as of 2002 to generate a counterfactual for how *i*'s Tobin's Q would have evolved

[40] As reported in Table B2 of Appendix B, when we limit the sample period to years 2002-2015, the estimate for the coefficient for *Staggered Board* in Column 1 drops from 0.138 in Table 2 to an economically and statistically insignificant 0.013 in Table B2. By the same token, shrinking the sample period to 2002-2015 leads the sum of the estimates for the coefficients for *Staggered Board* and *Staggered Board * Large or Very Large* to drop from 0.353 in Column 2 of Table 2 to 0.149 in Table B2. Finally, shrinking the sample period to 2002-2015 leads the sum of the estimates for the coefficients for *Staggered Board* and *Staggered Board * Very Large* to drop from 0.620 in Column 3 of Table 2 to 0.295 in the corresponding column of Table B2. Importantly, since the declassification wave only began in 2003, the dramatic changes in the point estimates we recover as we move from Table 2 to Table B2 is not due to the fact that reducing the sample period meaningfully reduces the number of declassification events used to identify the estimates. The natural explanation, in our opinion, is simply that a dramatic fraction of the estimates in Table 2 was driven by pre-treatment trends during a turbulent period in which those trends could not plausibly have been driven by the expectation that firms would declassify their boards. In the same vein, when—in unreported results— we perform a similar exercise and reestimate the results of Table 4 using only observations from years 2002-2015, we also recover highly attenuated estimates for the coefficient of $Staggered\ Board * RD\ Intensive$.

but for the fact that *i* destaggered its board. By contrast, assume that firm *j* declassified its board in 2011. In that case, comparing the evolution of Tobin's Q experienced by *j* in the years surrounding its declassification with the evolution of Tobin's Q experienced during that period by other firms that had a market capitalization similar to that of *j during 2002* would probably generate less precise counterfactuals. Ideally, we would like to compare *j* with other firms of similar market capitalization *as of 2010*. At the same time, however, we want to avoid sorting firms on the basis of any post-treatment outcome. Additionally, if some other firm *k* declassified its board in some later year—say, 2012—, we should also be careful not to use post-declassification outcomes for firm *k* to construct the counterfactual for firms *i* and *j*.

In order to draw these kinds of comparisons, we take advantage of the methodology developed by Gormley and Matsa (2011). We illustrate the intuition behind that methodology by revisiting Specification (2a). Start by focusing on the set of firms that declassified their boards during year y_0. As suggested above, we would like to compare (a) the evolution of *Q* for those firms around the time of destaggering with (b) the evolution of *Q* experienced during the same period by non-destaggering firms that had a market capitalization similar to that of the declassifying firms as of year y_0-*1*. We could do that by sorting firms into size groups on the basis of their market capitalization as of y_0-*1*,[41] and excluding from the sample the observations that correspond to (i) post-destaggering years for firms that declassified their board in years after y_0; (ii) all years for firms that declassified their board on years before y_0.[42] With these exclusions, firms that destaggered at y_0 are compared with firm-years that are unaffected by destaggering. With a sample thus constructed—define this sample as the "y_0 cohort"—, one could then estimate the following Specification:

$$Tobin'sQ_{it} = \alpha_{ic} + \theta_c Staggered\ Board_{it} + \mu_c Staggered\ Board_{it} * Large\ or\ VeryLarge_{ic} + \gamma_{ct} + \eta_{ict} + \varepsilon_{ict}, \quad (4)$$

where α_{ic} is a vector of firm-by-cohort fixed effects; $Large\ or\ VeryLarge_{ic}$ is a dummy equal to 1 if firm *i* is large or very large in cohort *c* (which, in our example, depends on the firm's market capitalization as of year y_0-1); γ_{ct} is a vector of cohort-by-year fixed effect; and η_{ict} is a vector of "years since public"-by-cohort fixed effects.[43] $Staggered\ Board_{it}$ is defined as in Specification (2a). Estimating Specification (4) using a sample that only consists of cohort y_0 would yield estimates of the effect of destaggering, and the differential effect of destaggering for large or very large firms, for firms that destaggered their board during year y_0. In principle,

[41] Note that the requirement that firms market capitalization be defined as of year y_0-1 implies that firms that were not part of the main sample as of that year will be excluded from the comparison group.

[42] The estimation sample would thus consist of the following observations: all years for firms that never switched their board structure and all firms that declassified their board during year y_0; and only the observations that correspond to the years before destaggering for firms that declassified their board at years after y_0. Moreover, as stated in the previous footnote, the estimation sample would exclude all firm-year observations for firms that were not part of the sample as of year y_0-1.

[43] Since "cohort" equals y_0 for all observations in the y_0 cohort, the fact that we are interacting all the fixed effects with "cohort" is inconsequential at this point.

one could perform the same exercise multiple times, separately estimating Specification 4 with one cohort at a time. However, that would yield multiple different estimates of the effect of declassification, and multiple estimates of the differential impact of declassification for large or very large firms. Instead, a simple way to recover the average effect of the declassifications adopted across all years consists of constructing a sample that stacks all cohorts as previously defined into a single sample and estimating the following specification (we refer to this exercise as estimating the "cohort-based analogue" to Specification (2a)):

$$Tobin'sQ_{it} = \alpha_{ic} + \theta Staggered\ Board_{it} + \mu Staggered\ Board_{it} * Large\ or\ VeryLarge_{ic} + \gamma_{ct} + \eta_{ict} + \varepsilon_{ict}.$$ [44]

For each of the Specifications (1), (2a) and (2b) in Section 4.2, Table 5 reports the estimates of the corresponding cohort-based analogue. Although as a result of the stacking of cohorts the number of observations used to estimate the cohort-based analogue of Specification 1 is much larger than the number of observations used to estimate Specification 1 in Column 1 of Table 2, the point estimate for the coefficient of $Staggered\ Board$, its standard error, and the R-squared of the specification are all very similar to those reported in Column 1 of Table 2. Comparing Columns 2 and 3 of Table 5 with the corresponding Columns of Table 2 suggests that this alternative methodology yields qualitatively similar results to those recovered in Table 2.

Table 5: Staggered boards and firm value – cohort-based estimation

Table 5 presents regressions of *Q* against *Staggered Board* (in Column 1); *Staggered Board* and the interaction between *Staggered Board* and *large or very large* (in Column 2); and *Staggered Board*, the interaction between *Staggered Board* and *large*, and the interaction between *Staggered Board* and *very large* (in Column 3). All specifications control for cohort-by-firm, cohort-by-year, and cohort-by-years-since-public fixed effects. *Staggered Board* is defined in Table A1. For a firm in cohort *c*, Very *large* (respectively, *large*) is a dummy equal to 1 if the relevant firm was part of the 10th decile (respectively, the 8th or 9th deciles) of market capitalization as of year *c-1* (and zero otherwise). *Large or very large* is the sum of *large* and *very large*. The sample consists of cohorts 1996 to 2015. Cohort *c* consists of (i) all firm-year observations for firms that were part of the main sample as of year *c-1* and did not switch their board structure between 1996 and 2015 and (ii) all firm-year observations for firms that declassified their board during year *c*; and (ii) all firm-year observations dated before year c' for firms that were part of the main sample as of year *c-1* and declassified their board during year *c'>c*. Standard errors (in parentheses) are robust and clustered by firm. Statistical significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

VARIABLES	(1) Tobin's Q	(2) Tobin's Q	(3) Tobin's Q
Staggered Board	0.125**	-0.0812	-0.0812
	(0.0583)	(0.0681)	(0.0681)
Staggered*Large or VeryLarge		0.357***	
		(0.0996)	
Staggered*Large			0.125
			(0.100)

[44] Note that, under this cohort-based approach, some firms may be part of the treatment group in one cohort and part of the control group in others.

Staggered*VeryLarge			0.583***
			(0.140)
Observations	368,296	368,296	368,296
R-squared	0.574	0.574	0.574
Cohort x Year FE	Yes	Yes	Yes
Cohort x Years since Public FE	Yes	Yes	Yes
Cohort x Firm FE	Yes	Yes	Yes

Table 6 reports the cohort-based analogue of the specifications estimated in Table 3. Recall that in the transition from Table 2 to Table 3, as one replaced the year fixed effects by the slightly more granular size-group-by-year fixed effects, the apparent association between staggered board and firm value vanished, both across the board and within each size group. In the same vein, replacing the cohort-by-year fixed effects used in Table 5 by cohort-by-size-group-by-year fixed effects in Table 6 leads to dramatically attenuated estimates for the coefficient of $Staggered\ Board$ in Column 1, the coefficient of $Staggered\ Board_{it} * Large\ or\ VeryLarge_{ic}$ in Column 2, and the coefficient for $Staggered\ Board_{it} * VeryLarge_{ic}$ in Column 3.[45] In unreported results, we estimate the cohort-based analogue of Specification (3), and recover results that are qualitatively the same as those reported in Figure 3.

Table 6: Staggered boards and firm value – cohort-based estimation — more granular specification

Table 6 presents regressions of *Q* against *Staggered Board* (in Column 1); *Staggered Board* and the interaction between *Staggered Board* and *large or very large* (in Column 2); and *Staggered Board*, the interaction between *Staggered Board* and *large*, and the interaction between *Staggered Board* and *very large* (in Column 3). All specifications control for cohort-by-firm, cohort-by-size-group-by-year, and cohort-by-years-since-public fixed effects. *Staggered Board* is defined in Table A1. For a firm in cohort *c*, very *large* (respectively, *large*) is a dummy equal to 1 if the relevant firm was part of the 10th decile (respectively, the 8th or 9th deciles) of market capitalization as of year *c-1* (and zero otherwise). *Large or very large* is the sum of *large* and *very large*. The sample consists of cohorts 1996 to 2015. Cohort *c* consists of (i) all firm-year observations for firms that were part of the main sample as of years *c* and *c-1* and did not switch their board structure between 1996 and 2015 and (ii) all firm-year observations for firms that declassified their board during year *c*; and (ii) all firm-year observations dated before year c' for firms that were part of the main sample as of years *c* and *c-1* and declassified their board during year *c'>c*. Standard errors (in parentheses) are robust and clustered by firm. Statistical significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

VARIABLES	(1) Tobin's Q	(2) Tobin's Q	(3) Tobin's Q
Staggered Board	0.0259	-0.0152	-0.0152
	(0.0581)	(0.0689)	(0.0689)
Staggered*Large_or_VeryLarge		0.0729	
		(0.108)	

[45] As in the transition from Table 2 to Table 3, the attenuation is driven by a drastic reduction in the magnitude of the point estimates, while the standard errors remain stable.

Staggered*Large			0.124
			(0.108)
Staggered*VeryLarge			0.0214
			(0.160)
Observations	368,296	368,296	368,296
R-squared	0.585	0.585	0.585
Cohort x Year FE	No	No	No
Cohort x Years since Public FE	Yes	Yes	Yes
Cohort x Firm FE	Yes	Yes	Yes
Cohort x Size-Group x Year	Yes	Yes	Yes

In unreported results we follow the same steps as those used in Tables 5 and 6 to estimate the cohort-based analogue of the result of Table 4. We recover results that are qualitatively similar to the results of Table 4. Additionally, in unreported results we estimate variants of Tables 5, 6, and the cohort-based analogue the specifications estimated in Table 4 in which, for each cohort, we sort firms on the basis of their characteristics—market capitalization and R&D intensity—two (instead of one) years before the year in which firms in the relevant cohort declassified their board, and recover qualitatively similar results.

5.3 **Different Cutoffs for Identifying R&D-Intensive Firms**

The analysis in Section 4.3 classified firms as "R&D intensive" if their research and development expenditures, as a fraction of sales, were in the top 25 percent of the distribution for 2002 (or the closest year for which the firm appeared in the sample). In Table 7 we report the results of estimating variants of the specification estimated in Table 4 where we alternatively define the cutoff for R&D intensive firms at the top 10 percent of the distribution (in Columns 1 to 3) or the top 50 percent of the distribution (in Columns 4 to 6). The results are qualitatively similar to those reported in Table 4: the estimate of the coefficient for $Staggered\ Board_{it}$ is statistically insignificant in all specifications. The estimate of the coefficient for $Staggered\ Board_{it} * R\&D\ Intensive$ is positive and significant in the specifications that do not control for R&D-intensive-by-year fixed effects, but vanishes once one compares declassifying R&D-intensive firms with other R&D-intensive firms that did not declassify their boards.

Table 7: Classified boards and firm value: focusing on R&D-intensive firms
Alternative definitions for R&D-intensive

Table 7 presents annual pooled panel regressions of *Q* against *Staggered Board* (in Column 1), and *Staggered Board* and the interaction between *Staggered Board* and *R&D Intensive* (in Columns 2 and 3). All specifications control for firm and years-since-public fixed effects. Column 1 additionally controls for year fixed effects. Columns 2 and 3 control for size-group – by – year fixed effects. Column 3 controls for *R&D Intensive*-by-year fixed effects. *Staggered Board* is defined in Table A1. In columns 1-3 (respectively, 4-6) *R&D Intensive* is a dummy equal to 1 if the relevant firm was part of the top decile (respectively, top half) of R&D (as a fraction of sales) as of 2002 or the year closest to 2002 for which the relevant firm appeared in the sample (and zero otherwise). Standard errors (in parentheses) are robust and clustered by firm. Statistical significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

VARIABLES	(1) Tobin's Q	(2) Tobin's Q	(3) Tobin's Q	(4) Tobin's Q	(5) Tobin's Q	(6) Tobin's Q
Staggered Board	0.0362	-0.0823	0.0116	0.0160	-0.105	0.00367
	(0.0670)	(0.0654)	(0.0652)	(0.0648)	(0.0650)	(0.0642)
Staggered*R&D Intensive	0.205*	0.191*	0.0253	0.271**	0.261***	0.0482
	(0.106)	(0.0999)	(0.106)	(0.108)	(0.101)	(0.108)
Observations	28,290	28,290	28,290	28,290	28,290	28,290
R-squared	0.584	0.595	0.600	0.584	0.595	0.602
Cutoff R&D-Intensive	Top 10 percent	Top 10 percent	Top 10 percent	Top 50 percent	Top 50 percent	Top 50 percent
Year FE	Yes	No	No	Yes	No	No
Years since Public FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Size-Group x Year FE	No	Yes	Yes	No	Yes	Yes
R&D-Intensive x Year FE	No	No	Yes	No	No	Yes

In sum, these robustness checks support the analysis of Section 4.

6. Conclusion

Over the past 15 years, the boards and shareholders of several hundred publicly traded firms eliminated their staggered boards in favor of annually elected boards. Recent analyses purport to show that this wave of declassification led to significant average drops in firm value as measured by Tobin's Q, and they attributed part of that decline in value to a reduction in long-term focus. If these results were valid, they would raise serious concerns about the ability of boards of directors to perform the leadership role expected of them, and about institutional shareholders' ability to play a responsible role in corporate governance. They would also suggest that, by restaggering their boards, shareholders could recoup hundreds of billions of dollars in value.

We take a closer look at the board declassification wave, and conclude that those concerns are unwarranted. By studying a sample of more than 2200 firms over 1996 to 2015, including over 400 firms that declassified their boards, we find that the apparent negative relationship was driven by the fact that very large firms declassified earlier in the wave and at a higher rate, and that very large firms also experienced a decline in *Q* over that period that was unrelated to declassification. The recent studies were flawed in that they failed to adequately control for firms' market capitalization. We correct this error by estimating a within-firm specification that that compares firms that destaggered in a particular year with firms of similar market

capitalization that did not destagger during that year. With this approach, we find no association between destaggering and *Q*.

We also analyze whether firms with high R&D expenditures were especially harmed as a result of destaggering their boards, as the recent literature concludes. We find that, once again, differential trends in *Q* are responsible for those results. R&D intensive firms experienced dramatically different patterns of *Q*, unrelated to declassification, during the period under analysis from those experienced by firms with low R&D. When high-and low-R&D firms are analyzed together, the results appear to suggest that high-R&D firms experienced an enormous decline in Q as a result of declassifying. But when we separate the two groups of firms and compare high-R&D firms that declassified with high-R&D firms that did not declassify, and do the same with low-R&D firms, there is no evidence that declassification impairs firm value as measured by Tobin's Q.

Our findings indicate that a healthy dose of skepticism is warranted with respect to recent claims that the declassification wave caused massive drops in shareholder value and that one of the channels through which that value destruction occurred was through a reduction in long-term focus. There is no basis for the concern that boards of directors and institutional shareholders that approved board declassification failed in their responsibilities. Hence there is no need to consider the policy recommendations of Cremers and Sepe (2016)—among others, adopting rules that would make classified boards quasi-mandatory, or eliminating the shareholders' right to submit precatory proposals under Rule 14a-8 to request that the board be declassified.

On the other hand, while it does not appear that boards and shareholders have destroyed value during the declassification wave, it also does not appear that they created value, as the earlier literature found. We found no evidence that staggered boards had an impact on average value one way or another.

The methodology we employ provides two general lessons for scholars employing difference-in-differences and within-firm designs to analyze the impact of a change in corporate governance. First, scholars testing causal hypotheses regarding the impact of governance changes should granularly analyze the evolution of the dependent variable in the period surrounding the governance change to assess whether that evolution is consistent with a causal explanation.[46] Second, our analysis suggests that scholars studying the effect of corporate governance on Tobin's Q or other outcome variables that are a function of stock prices should consider the possibility that firms with different characteristics may experience systematically

[46] Evidence that the purported effects take place too early to be justified by the expectation of the change in governance would suggest that the parallel-trends assumption is likely to be invalid; evidence that those effects took place too long after the change in governance under analysis would suggest that the baseline estimates are picking up something other than the causal effect of that change.

different secular trends in stock prices over particular periods of time.[47] This is especially true of market capitalization and other factors that have been shown in the asset pricing literature to be associated with differential trends in stock returns. Following these suggestions would not only be simple—since they would require collecting little to no additional data—it would also substantially enhance the credibility of empirical findings.

[47] The decision about the proper way to partition the sample should be informed not only by results in the asset pricing literature, but also by institutional knowledge about the channels driving the changes in governance that motivate the analysis. For example, in the case of board declassifications, the fact that high market-cap firms declassified earlier and much more often than their lower-market-cap peers would suggest that market capitalization is a natural dimension along which to partition the sample.

Appendix A: Dataset Description

The central dataset employed in this paper is a hand-collected database of board-related information about all publicly traded firms that were part of the Standard & Poors (S&P) 1500 Index between 1996 and 2015. We identify the components of the S&P index at each point in time by using the *spmim* variable in the Compustat – Securities Monthly database available through WRDS. I excluded from the sample financials (firms with SIC codes between 6000 and 6999), utilities (firms with SIC codes between 4900 and 4999) (Daines (2001)), firms that had a dual-class share structure (Gompers, Ishii, and Metrick (2010)), and firms for which we could not find a corresponding match in the CRSP-Compustat database. For each of the firms in my sample, and every year between 1996 and 2015,[A48] We searched a variety of sources (SharkRepellent, the IRRC database and the Securities and Exchange Commission's EDGAR website) for information about the board structure and the presence of multiple classes of shares.

The following table describes the construction and source of each of the variables used in regression analyses.

Variable Name	Definition	Source
Staggered Board$_t$	Indicator variable. Equal to 1 if and only if the firm had a staggered board as of the relevant point in time. (For firms that switched their board structure, our coding reflects the switch as taking place on the moment when the relevant governing documents were amended –e.g. if a firm amended its charter so as to adopt a phased-in board declassification, we code the board as being annually elected from the date when the charter was amended-)	EDGAR, SharkRepellent, IRRC.
Years since Public$_t$	Years elapsed since the first year in which the firm appeared in with non-empty price in CRSP-Compustat	CRSP-Compustat annual
Tobin's Q$_t$	Market Value/Book Value of assets, where Market Value is constructed as $prcc\_f_t * csho_t + at_t - ceq_t$, and Book Value of assets equals at_t	CRSP-Compustat annual
R&D$_t$	xrd_t/Sales$_t$ (xrd_t is assumed to be equal to zero if its value is empty)	CRSP-Compustat annual
Missing R&D$_t$	Indicator variable equal to 1 if and only if xrd_t is empty	CRSP-Compustat annual

[A48] Once a firm becomes a component of the S&P index at any point during 1996-2015, that firm is part of the sample for every year in 1996-2015 in which the firm was publicly traded (including any years before the firm became part of the S&P index, and any years after the firm stopped being a part of the S&P index).

Appendix B

Figure B1

Figure B1 reports, for each year, the number of firms in our sample that declassified their boards during the relevant year. We code declassification as taking place during the year in which the firm amended its governing documents to implement the declassification (regardless of whether the entire board became up for election in the relevant year or whether that happened in a later year).

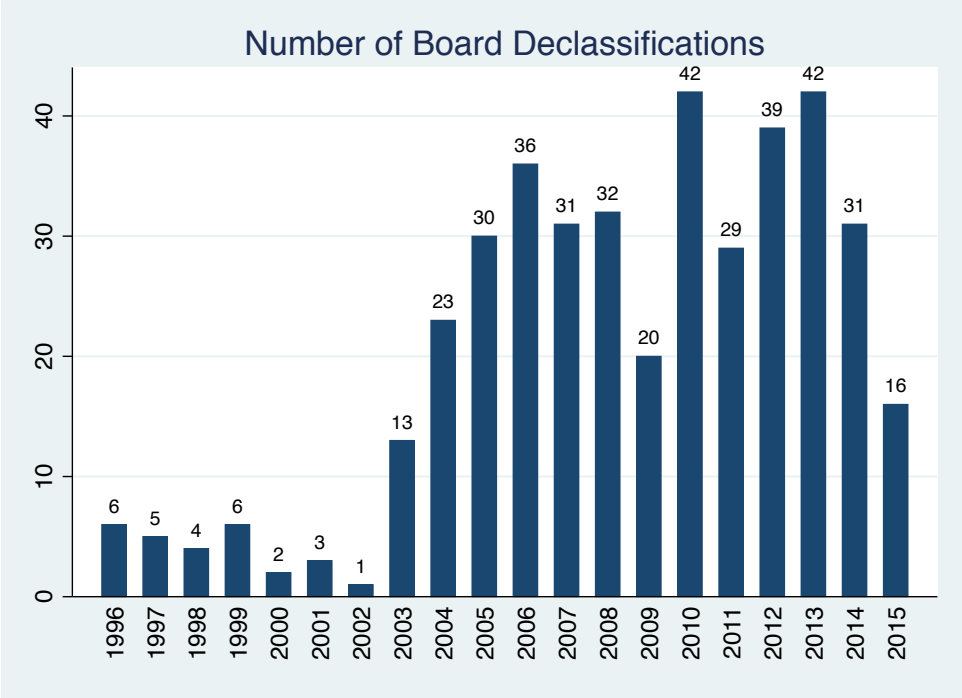

Figure B2: Returns of a "big minus small" zero investment portfolio

Figure B2 shows the cumulative returns experienced by a zero investment portfolio that is long in large companies and short in a portfolio of small companies. Daily returns on a "Small minus Big" portfolio are obtained from Kenneth French's website. Those are then accumulated over time, and then multiplied by minus one to obtain the returns of a "Big minus Small" portfolio.

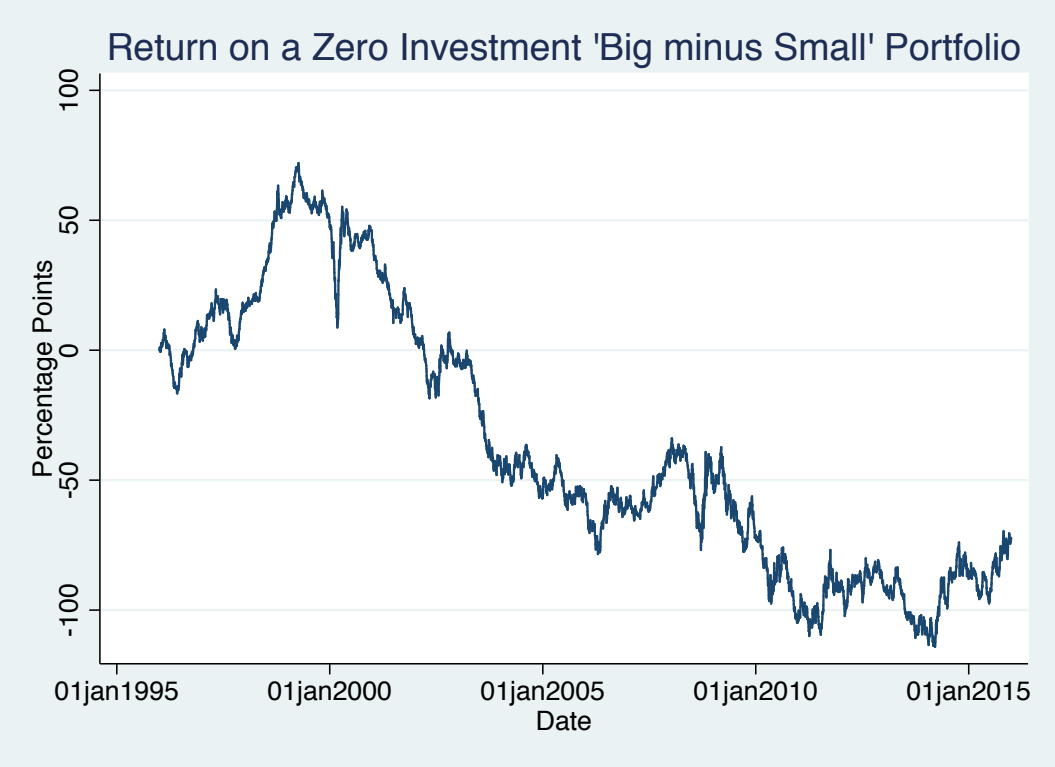

Figure B3: Evolution of the S&P Subindexes

Figure B3 reports the value of the S&P 100, S&P 500, S&P 400, and S&P 600 indexes between 1996 and 2015, divided by the value of the respective index as of January 1st, 1996.

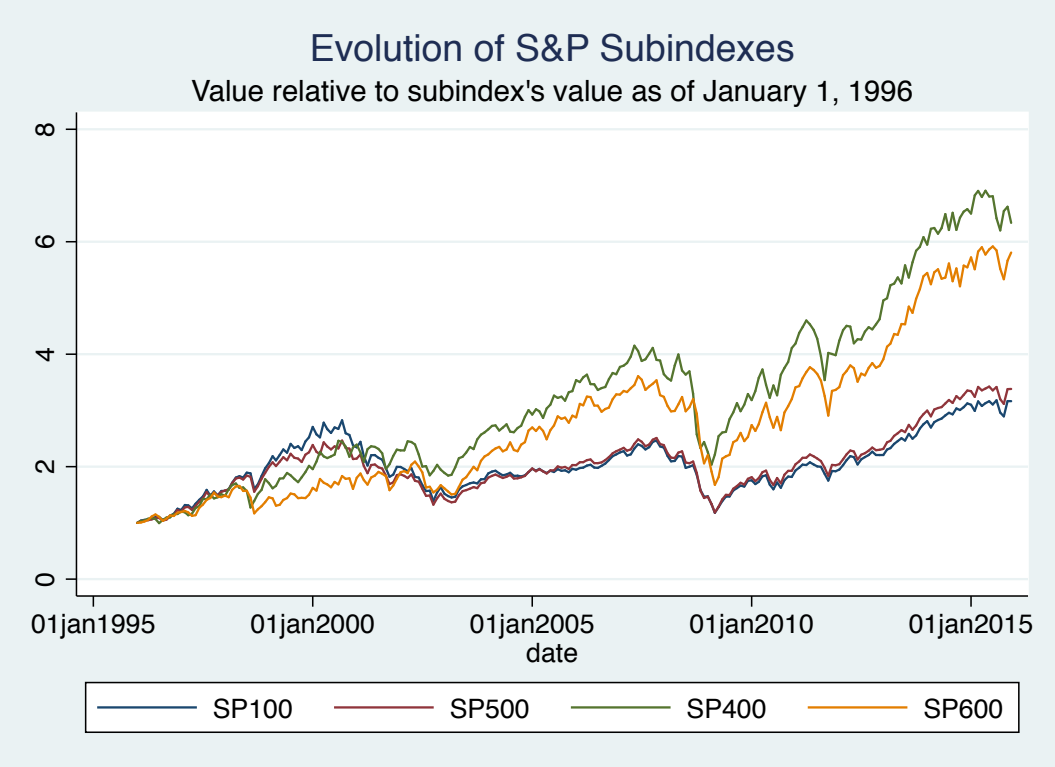

Figure [INSERT CROSS REFERENCE]: Shareholder declassification proposals

Figure [INSERT CROSS REFERENCE] reports, for each year, the number of shareholder resolutions requesting management to declassify the firm's board of directors. The underlying group of firms is the universe of publicly traded companies tracked by the SharkRepellent database.

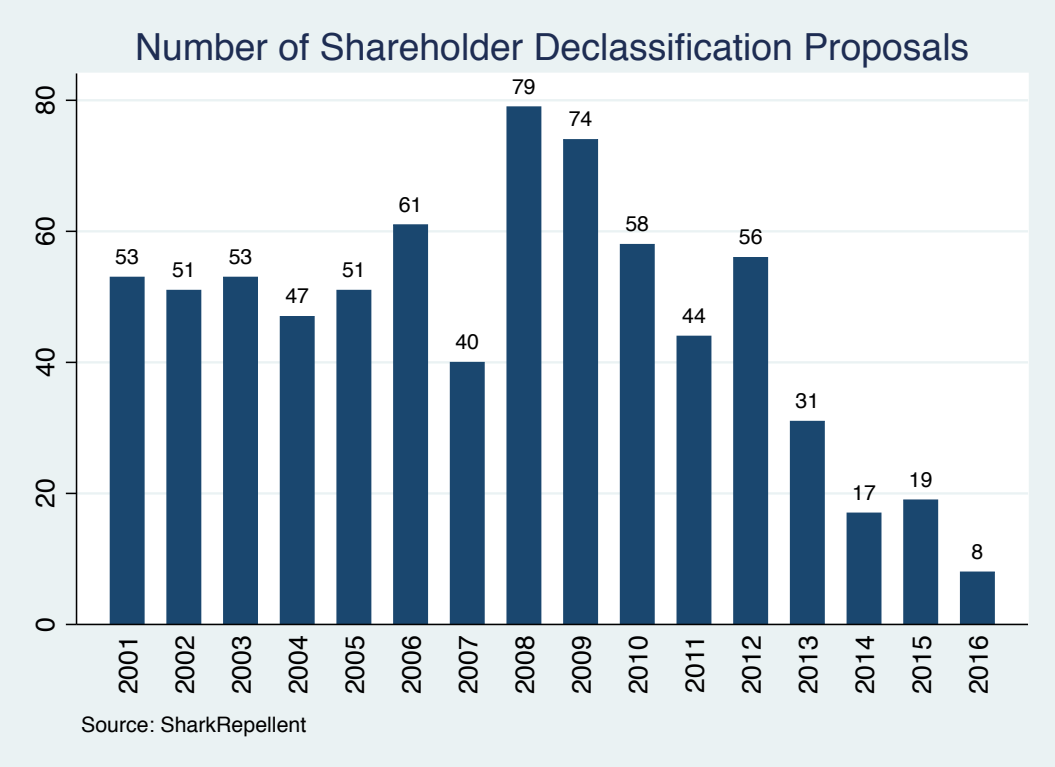

Figure B5: Management declassification proposals

Figure B5 reports, for each year, the number of management resolutions requesting shareholder approval to amend a firm's governing documents (charters or bylaws) to declassify the firm's board of directors. The underlying group of firms is the universe of publicly traded companies tracked by the SharkRepellent database.

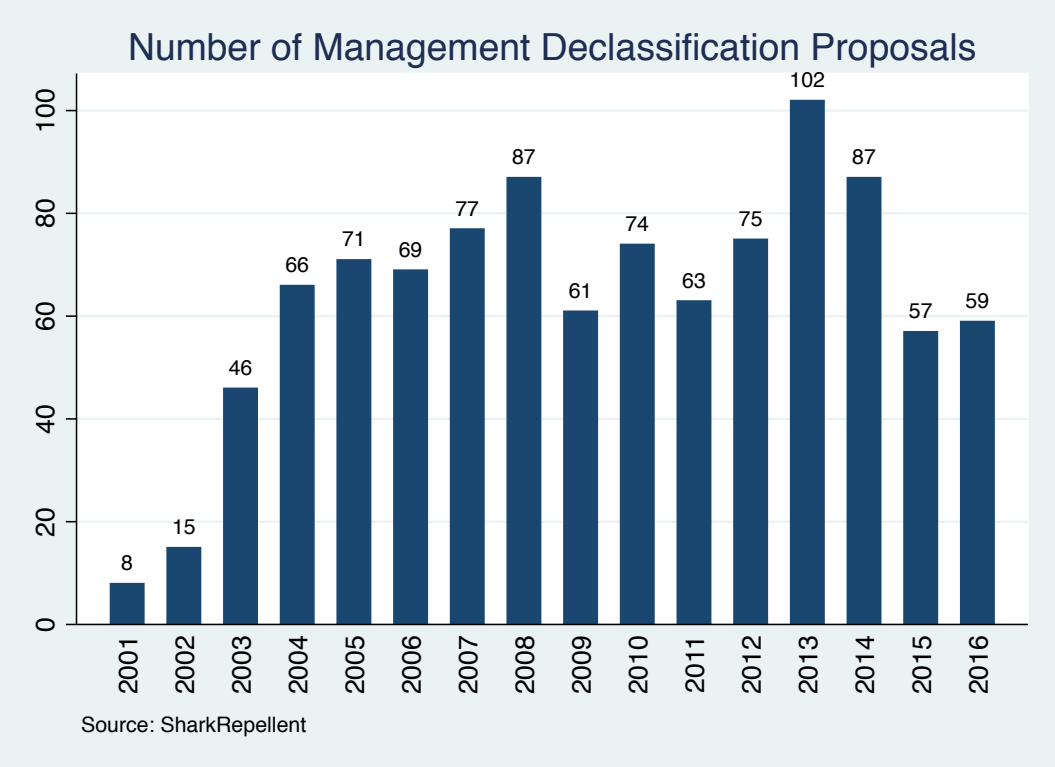

Figure B6: Shareholder support for management declassification proposals

Figure B6 reports, for each year, the average number of votes votes cast in favor of management resolutions requesting shareholder approval to declassify the firm's board of directors (as a fraction of all votes cast for, against, or abstaining with respect to the proposal). The underlying group of firms is the universe of publicly traded companies tracked by the SharkRepellent database.

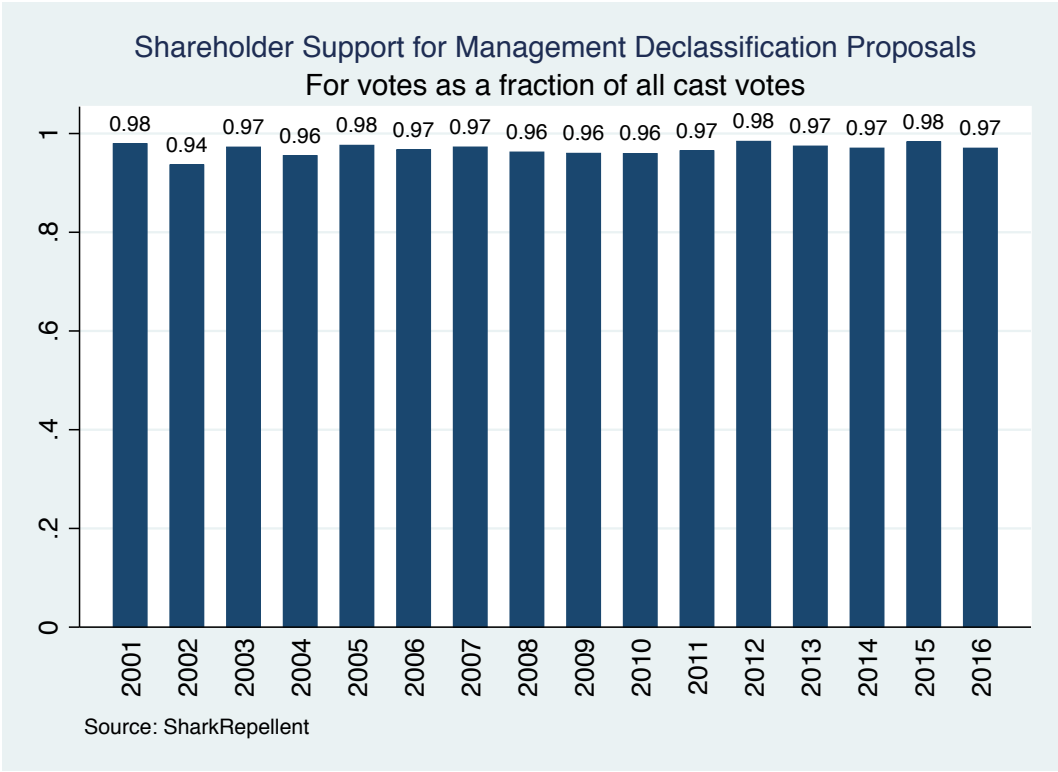

Figure B7: Time series in *Q* for two subsets of firms

Figure B7 reports, for each year between 1996 and 2015, and each of two groups of firms in our sample, the average value of Tobin's Q at the group-year level. The two groups consist of R&D intensive firms and low-R&D firms.

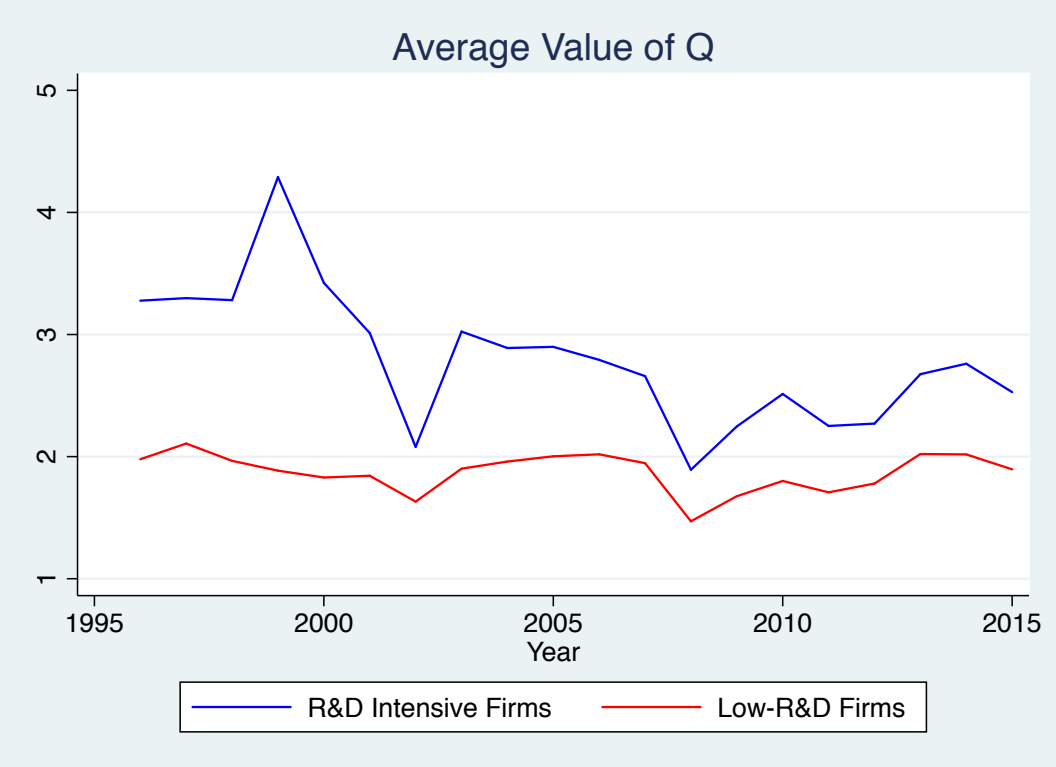

Figure B8: Time series in *Q* for six subsets of firms

Each subfigure of Figure B8 reports, for each year between 1996 and 2015, each market-capitalization decile (from the fifth to the tenth), and each of two groups of firms in our sample, the average value of Tobin's Q at the decile-group-year level. The two groups consist of R&D intensive firms and low-R&D firms. For each firm, the corresponding market-capitalization decile and R&D intensity group is defined as of 2002 (or the closest year for which the firm appears in the sample).

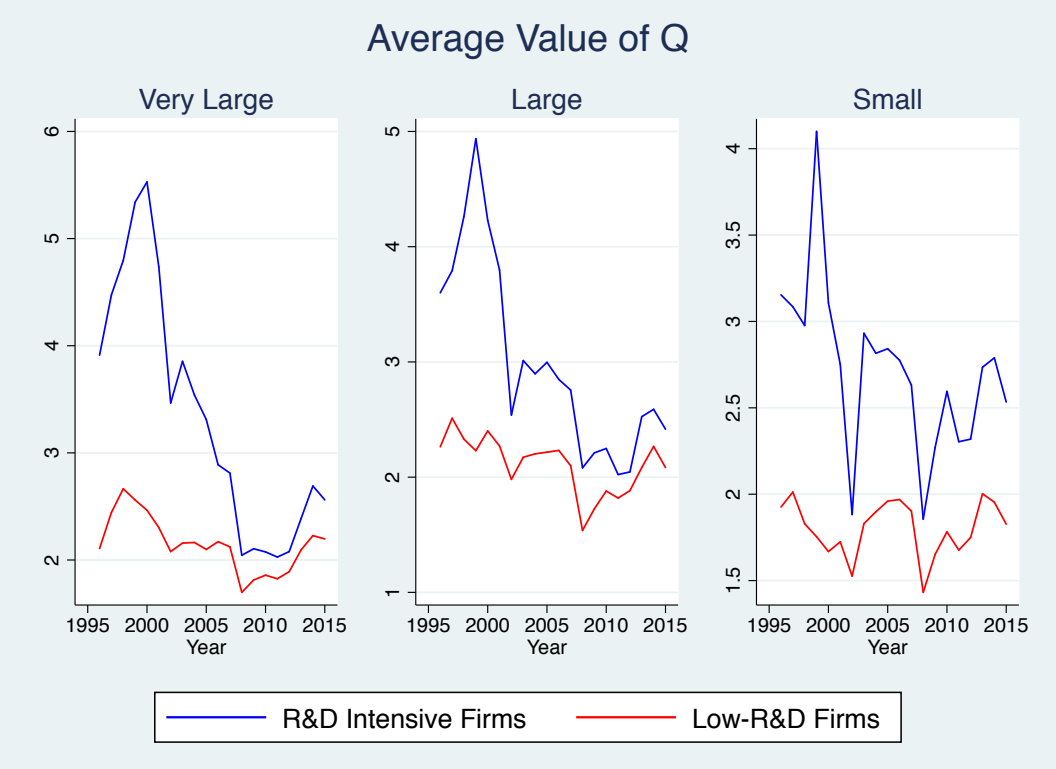

Figure B9: Time series in *Q* for four subsets of firms

Figure B9 reports, for each year between 1996 and 2015, and each of four groups of firms in our sample, average value of *Q* at the group-year level. The four groups consist of (1) R&D intensive firms that had an annual board of directors throughout the entire 1996-2015 period; (2) R&D intensive firms had a staggered board of directors during their first year in the sample; (3) low-R&D firms that had an annual board of directors throughout the entire 1996-2015 period; and (4) low-R&D firms that had a staggered board of directors during their first year in the sample.

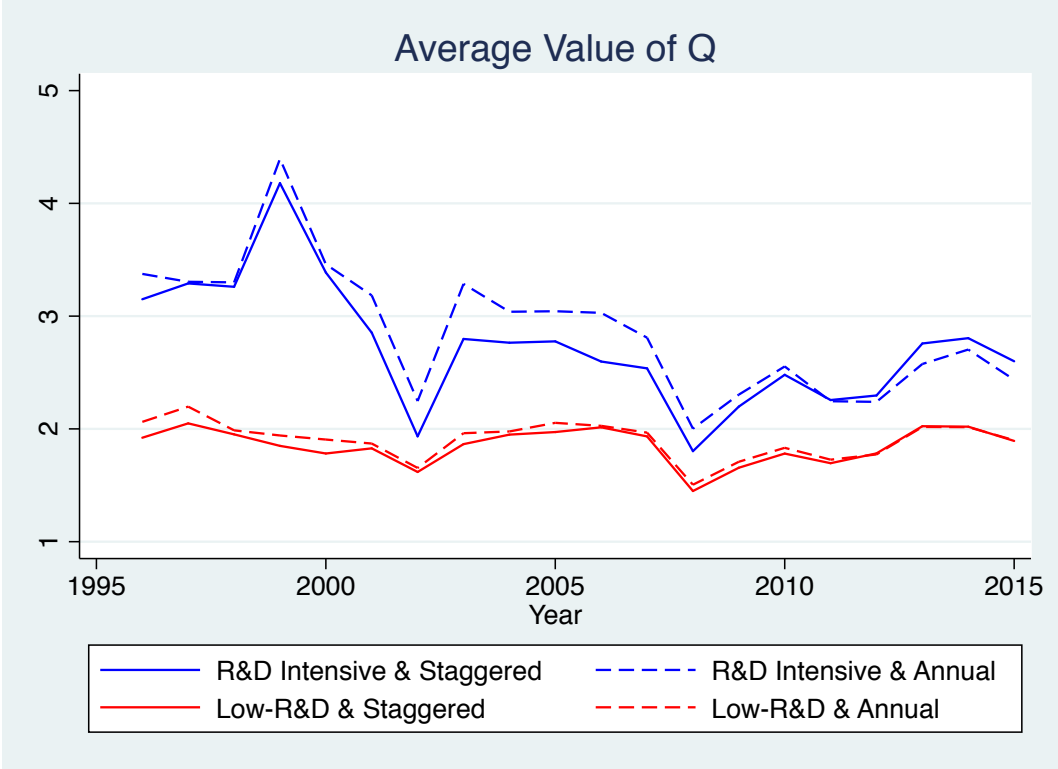

Table B1

Table B1 presents the results of estimating $Tobin'sQ_{it} = \alpha_i + \sum_{\substack{\tau=-8 \\ \tau\neq-1}}^{\tau=+8} \lambda_\tau Declassified\_Small_{\tau it} + \sum_{\substack{\tau=-8 \\ \tau\neq-1}}^{\tau=+8} \kappa_\tau Declassified\_Large_{\tau it} + \sum_{\substack{\tau=-8 \\ \tau\neq-1}}^{\tau=+8} \beta_\tau Declassified\_VeryLarge_{\tau it} + \gamma_t + \eta_{it} + \varepsilon_{it}$. For each $\tau < 0$, $Declassified\_VeryLarge_{\tau it}$ is equal to 1 if firm *i* was *very large* and firm *i* declassifies its board τ years after year *t* (and zero otherwise). For each $\tau \geq 0$, $Declassified\_VeryLarge_{\tau it}$ is equal to 1 if firm *i* is *very large* and firm *i* declassified its board τ years before year *t* (and zero otherwise). $Declassified\_Large_{\tau it}$ and $Declassified\_Small_{\tau it}$ are defined analogously for the case of *large* and *small* firms, respectively. Firm *i* is *very large* (respectively, *large*) if its market capitalization was in the 10th decile (respectively, the 8th or 9th deciles) of market capitalization as of 2002 (or the closest year for which market capitalization was available for firm *i*). Firm *i* is *small* if it was not *very large* or *large*. The estimation sample consists of (a) all firm-year observations in our sample for firms that never switched their board structure during our sample period and (b) all firm-year observations between the 8th year before the board declassification and the 8th year after the board declassification for the case of firms that declassified their board during our sample period. The specification controls for firm, year, and years-since-public fixed effects. Standard errors (unreported) are robust and clustered by firm. Statistical significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

VARIABLES	(1) Tobin's Q
destaggered_minus8_small	-0.0920
destaggered_minus7_small	-0.118
destaggered_minus6_small	0.0644
destaggered_minus5_small	-0.0489
destaggered_minus4_small	-0.146**
destaggered_minus3_small	-0.0383
destaggered_minus2_small	-0.0392
destaggered_plus0_small	-0.00185
destaggered_plus1_small	0.0352
destaggered_plus2_small	0.00619
destaggered_plus3_small	-0.0391
destaggered_plus4_small	0.0192
destaggered_plus5_small	0.00508
destaggered_plus6_small	0.0749
destaggered_plus7_small	0.155
destaggered_plus8_small	0.254
destaggered_minus8_large	0.317**
destaggered_minus7_large	0.318**
destaggered_minus6_large	0.263**
destaggered_minus5_large	0.124
destaggered_minus4_large	0.0907
destaggered_minus3_large	0.0689
destaggered_minus2_large	0.0534
destaggered_plus0_large	-0.0281
destaggered_plus1_large	-0.0224
destaggered_plus2_large	0.105
destaggered_plus3_large	0.126
destaggered_plus4_large	0.139
destaggered_plus5_large	0.0553
destaggered_plus6_large	0.0159

destaggered_plus7_large	0.00295
destaggered_plus8_large	0.113
destaggered_minus8_verylarge	0.431**
destaggered_minus7_verylarge	0.393***
destaggered_minus6_verylarge	0.551***
destaggered_minus5_verylarge	0.439***
destaggered_minus4_verylarge	0.350***
destaggered_minus3_verylarge	0.213**
destaggered_minus2_verylarge	0.168***
destaggered_plus0_verylarge	-0.126**
destaggered_plus1_verylarge	-0.185**
destaggered_plus2_verylarge	-0.246**
destaggered_plus3_verylarge	-0.244**
destaggered_plus4_verylarge	-0.342***
destaggered_plus5_verylarge	-0.365***
destaggered_plus6_verylarge	-0.456***
destaggered_plus7_verylarge	-0.470**
destaggered_plus8_verylarge	-0.470**
Observations	26,098
R-squared	0.593
Year FE	Yes
Years since Public FE	Yes
Firm FE	Yes
Size-Group x Year FE	No

Table B2: Classified boards and firm value – reduced sample period

Table 2 presents annual pooled panel regressions of *Q* against *Staggered Board* (in Column 1); *Staggered Board* and the interaction between *Staggered Board* and *Large or Very Large* (in Column 2); and *Staggered Board,* the interaction between *Staggered Board* and *Large*, and the interaction between *Stagered Board* and *Very Large* (in Column 3). All specifications control for firm, year, and year-since-public fixed effects. *Staggered Board* is defined in Table A1. *Large* is a dummy equal to 1 if the relevant firm was part of the 8th or 9th deciles of market capitalization as of 2002, or the closest year for which market capitalization data is available for the firm (and zero otherwise). *Very Large* is a dummy equal to 1 if the relevant firm was part of the 10th decile of market capitalization as of 2002, or the closest year for which market capitalization data is available for the firm (and zero otherwise). *Large or Very Large* is the sum of *Large* and *Very Large.* The sample period is 2002-2015. Standard errors (in parentheses) are robust and clustered by firm. Statistical significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

VARIABLES	(1) Tobin's Q	(2) Tobin's Q	(3) Tobin's Q
Staggered Board	0.0130	-0.143***	-0.144***
	(0.0428)	(0.0540)	(0.0540)
Staggered_large_or_verylarge		0.292***	
		(0.0781)	
Staggered_large			0.212**
			(0.0870)
Staggered_verylarge			0.439***
			(0.112)
Observations	18,494	18,494	18,494
R-squared	0.675	0.676	0.676
Year FE	Yes	Yes	Yes
Years since Public FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Size-Group x Year	No	No	No
Sample Period	2002-2015	2002-2015	2002-2015