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## ESSAY

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### IS THE STAGGERED BOARD DEBATE REALLY SETTLED?

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*The debate over staggered boards is heating up, largely because of the appearance of novel studies—including our own prior research—that challenge the results of earlier works documenting a negative impact of staggered boards on firm value. Meanwhile, a third way has appeared in this debate. In a recent article in the University of Pennsylvania Law Review, Settling the Staggered Board Debate, Professors Amihud, Schmid, and Davidoff Solomon (ASDS) purport to settle this debate, arguing that neither the position in favor or against staggered boards “has empirical support and, on average, a staggered board has no significant effect on firm value.”*

*This Essay addresses the ASDS study and shows that the staggered board debate is very much alive rather than settled. It does so in two ways. First, it shows that our prior result that the adoption of a staggered board is associated with a positive increase in firm value is robust to the criticism in ASDS. Second, this Essay shows that ASDS’s conclusion that staggered boards have no significant association with firm value is based on statistical tests that have “poor power,” that is, tests that are unlikely to find a robust association even if such association is actually supported by the data. In contrast, the tests that indicate that our earlier results are robust have both much better statistical power and good “size,” making it unlikely that we can find a positive association between staggered boards and firm value if no such association exists in the data.*

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INTRODUCTION.....	10
I. THE DEBATE ON STAGGERED BOARDS .....	13
A. <i>Earlier Studies</i> .....	14
B. <i>Recent Studies</i> .....	15
C. <i>Settling the Debate?</i> .....	18
II. ON THE ROBUSTNESS OF RECENT STUDIES.....	20
A. <i>Preliminary Observations</i> .....	21
B. <i>Data and Subsamples</i> .....	22
C. <i>Firm Fixed Effects Regressions and Change in Q Regressions</i> .....	23
1. Firm Fixed Effects Regressions .....	24
2. Change-in-Q Regressions.....	27
III. THE PURPORTED INSIGNIFICANT ASSOCIATION OF STAGGERED BOARDS AND FIRM VALUE.....	29
A. <i>Bootstrapping and Statistical Inference</i> .....	30
B. <i>Size Test</i> .....	33
C. <i>Power Test</i> .....	36
IV. ADDITIONAL ROBUSTNESS.....	40
CONCLUSION .....	43
APPENDIX.....	44

*A lawyer must be told things frankly; then it's up to us to muddle them up.*

ALESSANDRO MANZONI, *THE BETROTHED—A TALE OF XVII CENTURY MILAN* (Archibald Colquhoun ed., 1951).

#### INTRODUCTION

The debate over staggered boards is heating up, largely because of the appearance of novel studies that challenge the results of earlier works documenting a negative impact of staggered boards on firm value.<sup>1</sup> This debate relates not just to the value of the staggered board itself, but pertains more broadly to the central debate over the appropriate division of authority between a corporation's board and its shareholders.<sup>2</sup> Critics of the staggered board argue that it entrenches directors and managers, depriving shareholders of the ability to discipline incumbents and hence promoting moral hazard.<sup>3</sup> Scholars on the opposite side claim that the protection provided by the staggered board against

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<sup>1</sup> See *infra* Section I.B.

<sup>2</sup> For a full account of the staggered board debate, see K.J. Martijn Cremers & Simone M. Sepe, *The Shareholder Value of Empowered Boards*, 68 STAN. L. REV. 67, 79-84 (2016).

<sup>3</sup> See *id.* at 82-84.

shareholder intervention is only temporary, generally occurs with the approval of shareholders, and thus constitutes a constructive, bilateral commitment device towards the creation of long-term firm value.<sup>4</sup> We belong to the latter group of scholars, as we have documented evidence that the adoption of a staggered board is associated with a positive increase in firm value (and, correspondingly, the removal of a staggered board with a decline in firm value).<sup>5</sup>

Meanwhile, a “third way” has appeared in the staggered board debate. In a recent article in the *University of Pennsylvania Law Review*, Professors Amihud, Schmid, and Davidoff Solomon (ASDS)<sup>6</sup> purport to settle this debate, arguing that both the theoretical position in favor of and against staggered boards lack empirical support and on average, “a staggered board has no significant effect on firm value.”<sup>7</sup>

More particularly, ASDS argue that, on the one hand, earlier empirical studies finding a negative association between having a staggered board and firm value “do not include important explanatory variables.”<sup>8</sup> On the other hand, our more recent research documenting a positive association between staggered boards and firm value—and, in particular, the result of the article two of us (together with Lubomir Litov) published in the *Journal of Financial Economics* in 2017, *Staggered Board and Long-Term Firm Value, Revisited* (CLS)<sup>9</sup>—would not “account for the changing nature of the firm over time.”<sup>10</sup> ASDS then add that once these issues are corrected, the adoption of a staggered board becomes statistically insignificant.<sup>11</sup>

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<sup>4</sup> See *id.* at 123-26.

<sup>5</sup> See K.J. Martijn Cremers, Lubomir P. Litov & Simone M. Sepe, *Staggered Boards and Long-Term Firm Value, Revisited*, 126 J. FIN. ECON. 422 (2017) (providing evidence that staggered boards promote value creation for some firms by committing the firm to undertaking long-term projects and bonding it to the relationship-specific investments of its stakeholders); K.J. Martijn Cremers, Saura Masconale & Simone M. Sepe, *CEO Pay Redux*, 96 TEX. L. REV. 205 (2017) (providing evidence that the adoption of a staggered board does not produce entrenchment in the form of excessive CEO pay); K.J. Martijn Cremers, Saura Masconale & Simone M. Sepe, *Commitment and Entrenchment in Corporate Governance*, 110 NW. U. L. REV. 727, 732, 761-71 (2016) (revisiting the evidence obtained on the E-Index and showing that defensive measures benefit shareholders as long as such measures require shareholder approval); Cremers & Sepe, *supra* note 2, at 100-03 (showing that in the time-series firm value increases (decreases) after the adoption (removal) of a staggered board). While the ASDS criticism focuses on prior studies by Cremers and Sepe only, for simplicity in the rest of this Article we will use the collective form “we” when referring to the authorship of those studies.

<sup>6</sup> Yakov Amihud, Markus Schmid & Steven Davidoff Solomon, *Settling the Staggered Board Debate*, 166 U. PA. L. REV. 1475 (2018).

<sup>7</sup> *Id.* at 1477.

<sup>8</sup> *Id.* at 1478.

<sup>9</sup> Cremers et al., *Staggered Boards*, *supra* note 5.

<sup>10</sup> Amihud et al., *supra* note 6, at 1475.

<sup>11</sup> *Id.* at 1479.

This Essay addresses the ASDS study and further contributes to the staggered board debate in two central ways. First, it shows that our prior results on how firm value changes around changes in board structure (and, in particular, the result in CLS) are robust to the criticism in ASDS.

Second, this Essay shows that ASDS's conclusion that staggered boards have no significant association with firm value is based on statistical tests that have "poor power." That is, we show that ASDS's tests are unlikely to find a robust association even in self-generated samples that are constructed such that they are very similar to the actual data but where we know for sure that such association is actually present. This means that the methodology in ASDS is poorly suited to finding any association between firm value and board structure, as we show that this methodology is biased against finding *any* statistically significant association. In contrast, the tests that indicate that our earlier results are robust have much better statistical power as well as good "size," which means that they are unlikely to find a statistically significant association if such an association was not actually supported by the data.

Thus, the ASDS study *does not* settle the staggered board debate. Nonetheless, it contributes, in part, to advance this debate. Indeed, ASDS find that

prior studies, including the study by Professors Bebchuk and Cohen, do not include important explanatory variables that affect firm value and are correlated with the presence or absence of a staggered board. The result is that these studies have inappropriately attributed a lower firm value to the presence of the staggered board instead of to these omitted variables.<sup>12</sup>

This result is fully consistent with the result we obtained in our prior staggered board studies. In these works, we revisited the 2005 cross-sectional study by Professors Bebchuk and Cohen (BC)<sup>13</sup> by employing the use of a time-series panel (i.e., a pooled panel model with firm fixed effects)—a methodology that allows us to compare the average firm value before versus after a change in board structure.<sup>14</sup> Through this methodology, we find that reverse causality explains the cross-sectional finding in BC of a negative association between staggered boards and firm value.<sup>15</sup> This means that *ex ante* less valuable firms are more likely to seek increased board protection through adopting a staggered board (and that firm value tends to go up, not down, with the adoption of a staggered board), rather than increased board protection causing firms to become less valuable. Consistent with this result,

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<sup>12</sup> *Id.* at 1478 (footnote omitted).

<sup>13</sup> Lucian A. Bebchuk & Alma Cohen, *The Costs of Entrenched Boards*, 78 J. FIN. ECON. 409 (2005).

<sup>14</sup> See Cremers et al., *Staggered Boards*, *supra* note 5, at 425-30; Cremers & Sepe, *supra* note 2, at 100-03. See also *infra* Section I.B. (explaining the difference between a cross-sectional analysis and a time-series analysis).

<sup>15</sup> See Cremers et al., *Staggered Boards*, *supra* note 5, at 433-34.

ASDS also find that “past performance variables have a negative effect on whether the firm has a staggered board.”<sup>16</sup>

This is the beauty of *ongoing* debates. These debates promote further study, the deployment of novel analytical tools, and, hopefully, advances in research. In the staggered board debate, we think that one such advance has been made: the long-prevailing view that staggered boards are detrimental to long-term shareholder value is now rejected as unsupported by the data.

The remainder of this reply is organized as follows. Part I briefly overviews the current status of the empirical debate on the financial value of staggered boards. Part II shows that the main result in CLS—that the adoption or removal of a staggered board is associated with a respective increase (decrease) in long-term firm value—is robust to the ASDS criticism. Part III employs the bootstrapping methodology<sup>17</sup> to further show that the ASDS claim that staggered boards have no effect on firm value is only apparent because it reflects the use of a statistical methodology (i.e., pooled panel firm value regressions with firm fixed effects) with poor statistical power. Part II also shows that, in contrast, the positive association between staggered boards and firm value is robust when using an alternative methodology (i.e., change in firm value regressions) with stronger power. Last, Part III shows that both tests are not more likely to find evidence for any association if no such association is actually present in the bootstrapped samples, that is, that both these tests have good statistical size. Part IV discusses some of the additional robustness tests we performed in our prior staggered board studies, which ASDS ignore. Section V provides our brief conclusion.

## I. THE DEBATE ON STAGGERED BOARDS

In order to make our discussion of *Settling the Staggered Board Debate* more accessible, this Part provides the background necessary for understanding the context and importance of the empirical debate on staggered boards, which ASDS purports to settle. While a large body of studies have examined the effect of staggered boards on firm value and corporate governance more generally, we will provide only a general description of these studies and focus in more detail on the two studies that ASDS attempt to replicate: the 2005 study by BC<sup>18</sup> and the 2017 study coauthored by CLS.<sup>19</sup> After that, we will also briefly describe the main findings in ASDS.

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<sup>16</sup> Amihud et al., *supra* note 6, at 1497.

<sup>17</sup> The “bootstrapping” methodology uses random sampling techniques to estimate the statistical properties of an estimator (i.e., the methodology that we use to estimate the association of staggered boards with firm value).

<sup>18</sup> Bebchuk & Cohen, *supra* note 13.

<sup>19</sup> Cremers et al., *Staggered Boards*, *supra* note 5.

### A. Earlier Studies

Earlier empirical studies exploring the wealth effects of staggered boards include studies employing governance indices and short-term event studies examining stock price reactions to the adoption/elimination of a staggered board.<sup>20</sup> These studies have generally found that staggered boards are associated with lower firm value.

However, as we have discussed in detail elsewhere,<sup>21</sup> a general problem affecting such studies is that they exhibit significant methodological limitations. For example, some of the governance provisions included in a governance index “may matter more than others, some may have an impact only in specific circumstances, and others may have no impact at all.”<sup>22</sup> And a general problem with short-term event studies of staggered boards is that short-term stock returns may bundle the market’s assessment of staggered boards with the market’s inferences of other firm news that might explain both the adoption of a staggered board and the change in firm value.<sup>23</sup>

A different approach to evaluating the wealth effects of staggered boards is studying their cross-sectional association with long-term firm value.<sup>24</sup> The 2005 study by Bebchuk and Cohen<sup>25</sup> is arguably the best known among the studies adopting this methodology. The BC study examines an eight-year span, from 1995 to 2002.<sup>26</sup> Using Tobin’s Q as a proxy for firm value,<sup>27</sup> BC conclude that having a staggered board is associated with statistically and economically significant lower firm value,<sup>28</sup> which has been interpreted to support the view that staggered boards hurt shareholders.<sup>29</sup>

Cross-sectional studies of staggered boards, however, are particularly hard to interpret given the endogeneity of board structures. Indeed, the adoption or removal of a staggered board is an endogenous choice made by firms given particular circumstances at the time of adoption.<sup>30</sup> Under this constraint, a cross-

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<sup>20</sup> See Cremers & Sepe, *supra* note 2, at 88-92 (discussing earlier staggered board studies in details).

<sup>21</sup> *Id.*

<sup>22</sup> *Id.* at 89.

<sup>23</sup> *Id.* at 89-90.

<sup>24</sup> *Id.* at 90.

<sup>25</sup> Bebchuk & Cohen, *supra* note 13.

<sup>26</sup> *Id.* at 410.

<sup>27</sup> Tobin’s Q has become the standard proxy for the financial value of the corporation in corporate finance studies. See *id.* at 419-20. Tobin’s Q is, roughly, the ratio of the market value of assets to the book value of assets. See Eugene F. Fama & Kenneth R. French, *Testing Trade-Off and Pecking Order Predictions About Dividends and Debt*, 15 REV. FIN. STUD. 1, 8 (2002).

<sup>28</sup> Bebchuk & Cohen, *supra* note 13, at 410.

<sup>29</sup> E.g., Lucian A. Bebchuk, *The Myth That Insulating Boards Serves Long-Term Value*, 113 COLUM. L. REV. 1637, 1681-86 (2013).

<sup>30</sup> See generally JEFFREY M. WOOLRIDGE, *INTRODUCTORY ECONOMETRICS: A MODERN APPROACH* 444-45 (4th ed. 2009) (describing cross-sectional analysis).

sectional analysis can provide useful snapshots of the association between the level of staggered boards and firm value over different years, identifying to what extent firms with a staggered board in place tend to have different financial valuations at particular points in time from other firms without a staggered board at that time.

However, a cross-sectional analysis is ill-suited to capture temporal variations in board structure within the same set of firms, even if such analysis combines multiple years in a pooled panel. This is because changes in board structure tend to be relatively rare. As a result, the impact of any “within-firm” changes in board structure on firm value in a cross-sectional study is generally overwhelmed by differences in firm value “across firms.” In other words, cross-sectional studies only tend to capture differences between different sets of firms, namely firms with and without a staggered board, and not how changes in board structure relate to changes in firm value within one firm.

This explains why a cross-sectional analysis is especially subject to endogeneity concerns where differences in firm value might be attributable to unobservable firm characteristics (an “omitted variable” problem), or where low firm value might motivate, rather than result from, the adoption of a staggered board (a particular form of a “selection problem,” generally known as the “reverse causality” problem).<sup>31</sup>

In CLS we show that one important variable that the BC study omits is the ex ante value of the firms that adopt a staggered board.<sup>32</sup> Controlling for the relatively low value of firms prior to the adoption of a staggered board shows that the negative cross-sectional association between having a staggered board and firm value can indeed be attributed to a reverse causality problem. This means that firms with lower ex ante value are more likely to adopt a staggered board, rather than the adoption of a staggered board causing firms to have a lower value.

### B. *Recent Studies*

In recent years, a surge of new studies have examined the wealth effects of staggered boards.<sup>33</sup> Unlike earlier studies, several of these studies find that staggered boards serve a positive governance function for different subsets of firms. For example, Professors Johnson, Karpoff, and Yi document that staggered boards have a positive impact on firm value in both IPO and young publicly traded firms (i.e., in the years immediately following the IPO), especially for firms with

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<sup>31</sup> See JEFFREY M. WOOLDRIDGE, *ECONOMETRIC ANALYSIS OF CROSS SECTION AND PANEL DATA* 50-51 (2002) (providing a general discussion of the specificity and simultaneity problems).

<sup>32</sup> Cremers et al., *Staggered Boards*, *supra* note 5, at 433-34.

<sup>33</sup> For a description of these studies, see Daniel M. Gallagher & Joseph A. Grundfest, *Did Harvard Violate Federal Securities Law?: The Campaign Against Classified Boards of Directors*, 33-41 (Rock Ctr. for Corp. Governance at Stanford Univ., Working Paper No. 199, 2014), [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2536586](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2536586).

stronger stakeholder relationships.<sup>34</sup> Professors Cen, Dasgupta, and Sen similarly find that staggered boards serve a positive governance function in firms where stakeholder relationships are important.<sup>35</sup>

Our 2017 study (CLS) focuses on more mature firms and examines over thirty-five years of staggering and destaggering decisions (from 1978 to 2015), documenting that firm value tends to increase (decrease) after firms adopt (remove) a staggered board, especially in firms more engaged in research and innovation or where stakeholder relationships matter more.<sup>36</sup> As the primary identification strategy to address the endogenous choice of a staggered board, CLS employs a time-series analysis that considers how changes in board structure predict changes in firm value.<sup>37</sup> More particularly, CLS employs two different time-series methodologies: (1) pooled panel regressions of firm value on board structure with firm fixed effects and (2) regressions of changes in firm value on changes in board structure.

The first methodology—pooled panel regressions of firm value on board structure with firm fixed effects—allows us to estimate for each firm included in a panel dataset, the coefficient of a separate time-invariant dummy variable that captures the average value of each particular firm in the sample. Thus, once firm fixed effects are included, the coefficient on the staggered board is only identified through changes in board structure, and indicates the difference in average firm value before versus after a change in board structure. That is, the coefficient indicates how the average firm value changes after the adoption/removal of a staggered board within the same firm, rather than across firms. Because this methodology significantly mitigates both the omitted variable problem and reverse causality concerns, a time-series analysis with firm fixed effects is generally regarded as a more reliable method of identifying empirical relationships in econometrics than a cross-sectional analysis. More particularly, a time-series analysis is regarded as especially good at preventing spurious associations that are not really there but are only apparent because some important controlling variable is not included.<sup>38</sup>

However, because changes in board structure are relatively rare, only when data are available for considerable lengths of time can researchers count on having sufficient within firm changes in board structure to perform a time-series analysis.

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<sup>34</sup> William C. Johnson, Jonathan M. Karpoff & Sangho Yi, *The Bonding Hypothesis of Takeover Defenses: Evidence from IPO Firms*, 117 J. FIN. ECON. 307, 329 (2015).

<sup>35</sup> Ling Cen, Sudipto Dasgupta & Rik Sen, *Discipline or Disruption? Stakeholder Relationships and the Effect of Takeover Threat*, 62 MGMT. SCIENCE 2820, 2820 (2016).

<sup>36</sup> Cremers et al., *Staggered Boards*, *supra* note 5, at 423.

<sup>37</sup> *Id.*

<sup>38</sup> See, e.g., Jerry A. Hausman & William E. Taylor, *Panel Data and Unobservable Individual Effects*, 49 ECONOMETRICA 1377, 1377 (1981) (stating that using fixed effects represents a common method of controlling for omitted variables).

Furthermore, as pooled panel regressions of firm value with firm fixed effects estimate how the *average* firm value changes, such estimation may be noisy in periods when firm values fluctuate substantially in ways that are not highly systematic. As a result, pooled panel regressions of firm value with firm fixed effects may have “poor power,” i.e., have a poor ability to identify an association of board structure with firm value that is *actually present* in the data if the sample is too short, too noisy, or has too few changes in board structure.

The second time-series methodology we employ in CLS—regressions of changes in firm value on changes in board structure—allows us to control for changes in other firm characteristics that the previous literature found to be related to firm value. Similar to the coefficient on board structure in pooled panel regressions with firm fixed effects, the coefficient of changes in board structure estimated in change in firm value regressions is only identified from changes in board structure; that is, from cases where firms adopt or remove a staggered board. However, firm fixed effect regressions estimate differences in the average firm value before versus after changes in board structure. Instead, change-in-value regressions estimate whether firm value changes *immediately following* the change in board structure, where it is possible that such changes would subsequently be reversed (e.g., due to other, perhaps unrelated, changes affecting the firm). Further, in change-in-value regressions, changes to firm value following changes in board structure are not compared to firm value in the full period before the change in board structure but only to firm value *closely prior* to the change in board structure.

It follows that change-in-firm-value regressions can more easily pick up time-series associations in shorter samples—that is, this methodology has more power to find an association in samples that are shorter, noisier or with fewer changes. Yet, a potential downside of change-in-firm-value regressions is that the estimated changes may not be permanent, unlike the changes that are captured through the use of firm fixed effects in a sufficiently long sample. This explains why one should employ both methodologies whenever possible, as each methodology has different advantages and drawbacks.

In addition to using these time-series methodologies, CLS confirms the positive association between the adoption of a staggered board and firm value through several other identification strategies. As we discuss in more detail in Part IV, these strategies include the use of matched samples, a quasi-natural experiment employing a 1990 change in Massachusetts’s legislation on staggered boards, and a simultaneous-system approach that dynamically models endogenous changes in firm value, board structure, and other firm

characteristics.<sup>39</sup> Further, the main result in CLS is also consistent with the results we obtain in several other related studies.<sup>40</sup>

### C. *Settling the Debate?*

As highlighted in the Introduction, ASDS defends the view that staggered boards have no significant association with firm value. They reach this conclusion by examining the cross-sectional and time-series association of firm value and board structure in a sample of nearly 3,000 firms from 1990 to 2013 and, purportedly, showing that “prior studies [finding either a negative association or a positive association between staggered boards and firm value] are not robust to different estimation models.”<sup>41</sup> More particularly, ASDS focus on BC as the most prominent among earlier studies and CLS as the most comprehensive among more recent studies analyzing the association between firm value and board structure.<sup>42</sup>

ASDS’s methodological criticism of BC, which substantially replicates our own prior criticism of that study,<sup>43</sup> is that BC omit important explanatory variables in their analysis that affect both the likelihood of adopting a staggered board and firm value.<sup>44</sup> ASDS replicate BC’s cross-sectional analysis in a sample covering 1990–2013, finding that the negative association between staggered boards and firm value becomes insignificant when they add more controls.<sup>45</sup> They then show that the other entrenchment provisions included in the E-index developed by Bebchuk, Cohen, and Ferrell,<sup>46</sup> as well as ex ante low firm value, are important omitted variables in BC’s analysis.<sup>47</sup> These results are, again, fully consistent with similar tests we performed in CLS as well as the 2016 legal companion of that article, *The Shareholder Value of Empowered Boards*.<sup>48</sup>

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<sup>39</sup> See Cremers et al., *Staggered Boards*, *supra* note 5, at 423-24 (describing these additional tests).

<sup>40</sup> See Cremers et al., *CEO Pay Redux*, *supra* note 5, at 247-48 (examining the interplay between CEO pay and staggered boards); Cremers et al., *Commitment*, *supra* note 5, at 732, 761-74 (challenging the inclusion of the staggered board among the provisions that produce value-decreasing entrenchment); K.J. Martijn Cremers & Simone M. Sepe, *Institutional Investors, Corporate Governance, and Firm Value*, 41 SEATTLE U. L. REV. 387, 415-16 (2018) (examining the interplay between staggered boards and institutional investor horizons); Cremers & Sepe, *supra* note 2 (examining the time period 1978–2011 and documenting that staggered boards are associated with a positive increase in firm value by employing a time-series analysis with firm fixed effects).

<sup>41</sup> Amihud et al., *supra* note 6, at 1477.

<sup>42</sup> *Id.* at 1478-79.

<sup>43</sup> See *supra* notes 31–32 and accompanying text.

<sup>44</sup> Amihud et al., *supra* note 6, at 1489-95.

<sup>45</sup> *Id.* at 1490-91 tbl.1.

<sup>46</sup> Lucian Bebchuk, Alma Cohen & Allen Ferrell, *What Matters in Corporate Governance?*, 22 REV. FIN. STUD. 783, 784-85 (2009).

<sup>47</sup> Amihud et al., *supra* note 6, at 1492 tbl.2, 1496 tbl.3.

<sup>48</sup> Cremers & Sepe, *supra* note 2.

ASDS then moves to the methodological criticism of CLS: that a time-series analysis with firm fixed effects does not “account for the changing nature of the firm over time.”<sup>49</sup> They argue:

[S]ome firm characteristics change over time . . . and these changes may induce firms to adopt a staggered board or to destagger their board, while at the same time affecting firm value. The reason for the earlier results [in CLS] may be related to unobserved changing characteristics . . . which caused the relationship between a staggered board and firm value. It may have been these changing characteristics themselves rather than related changes in staggered board status that affected value.<sup>50</sup>

In order to test this criticism, ASDS first replicate the time-series analysis in CLS over their 1991–2013 sample<sup>51</sup> and then over two subsamples, 1991–2002 and 2003–2013.<sup>52</sup> The reason for splitting the sample, they say, is to “allow for the unobserved firm characteristics to vary over time and have their own effect on value . . . .”<sup>53</sup>

The results of ASDS Table 4 show that in the full ASDS sample (covering 1991–2013), the impact of staggered board on firm value is generally positive and statistically significant.<sup>54</sup> However, once they split their full sample in two, they find that the effect of the staggered board on firm value becomes statistically insignificant in both subsamples (covering 1991–2002 and 2003–2013 respectively).

They then conclude:

<sup>49</sup> Amihud et al., *supra* note 6, at 1475.

<sup>50</sup> *Id.* at 1498.

<sup>51</sup> It is also worth emphasizing that ASDS’s controlled full sample is approximately half of our controlled full sample in CLS. In fact, the statistically significant results they find in the specifications for their full sample are consistent with the results for the subsample (1996–2015) examined in Table 3, Column 6 of our *JFE* paper. Cremers et al., *Staggered Boards*, *supra* note 5, at 429.

<sup>52</sup> Amihud et al., *supra*, note 6, at 1500 tbl.4.

<sup>53</sup> *Id.* at 1498.

<sup>54</sup> *Id.* at 1500. In ASDS Table 4, column 1 (i.e., the model without ownership variables and without the Modified E-Index), ASDS find a positive staggered board coefficient with a t-statistic of 1.596, which is only marginally insignificant. *Id.* (If the t-statistic were 1.645 or higher, they should have reported a significant result also for that specification.) However, we cannot fully replicate this result. Indeed, if we use our sample and specifications as in Table 3 of CLS—*Q* regressions with firm and fixed effects and only basic controls—but then restrict our sample to 1991–2013 (as in ASDS), we find a statistically significant coefficient of *Staggered Board* equal to 0.075 with a t-statistic of 2.11. Using analogous regressions but using  $\log(Q)$  as in ASDS, we find a statistically significant coefficient of *Staggered Board* equal to 0.034 with a t-statistic of 2.25. Our sample size of 24,413 observations in these regressions is very similar to the sample size of 24,295 that ASDS report for column 1 of their Table 4. *See id.* One possible explanation for the difference between ASDS’s results and our replication is that ASDS may use staggered board data that are qualitatively different than ours. For example, ASDS report using the staggered board data from IRRC, which is only updated bi-annually before 2006. *Id.* at 1487–88. In our dataset, instead, we hand-checked changes in board structure in the missing years (e.g., 2001, 2003, and 2005). *See infra* Section III.A.

In sum, the evidence on the value of staggered boards is mixed. Bebchuk, Cohen, and others have found a staggered board has a wealth-decreasing effect; Cremers, Litov, Sepe, and others have found a staggered board has a wealth-increasing effect. The differences between the studies is attributable to the different methods they employ to account for omitted variables . . . .<sup>55</sup>

Before discussing ASDS's specific criticisms of CLS, we note here that ASDS's conclusion about existing staggered board studies is only partially accurate. While the different results in BC and CLS can certainly be attributed to different estimation methods, ASDS omits the substantial difference between identifying a cross-sectional versus a time-series association. Specifically, BC rely on a *cross-sectional* study to examine the association of staggered boards with firm value over an *eight-year period*. CLS, instead, employ a *time-series* analysis over a much longer *thirty-seven-year period*. The difference between these methodologies, as explained above, reflects a fundamentally improved identification strategy, which amounts to the difference between comparing *different firms* with different board structures in a cross-sectional analysis, and comparing changes *within the same firm* that modifies its board structure in a time-series analysis. It follows that the time-series evidence of CLS—namely that firm value tends to increase (decrease) after firms adopt (remove) a staggered board—should be distinguished from prior cross-sectional results that are more subject to endogeneity (or selection) concerns,<sup>56</sup> rather than ambiguously concluding that the current evidence on staggered boards is “mixed.”

## II. ON THE ROBUSTNESS OF RECENT STUDIES

In this Part, we examine ASDS's main criticism of CLS—that our results are not robust in taking into account the changing nature of firm characteristics over time. We begin our discussion by detailing some preliminary observations concerning their analysis. After that, we will explain that the ASDS methodology of examining our results in fairly short subperiods involves an important tradeoff, which ASDS omits to discuss. On the one hand, splitting a longer sample into shorter subperiods—as ASDS do when they split their 1991–2013 full sample in two subperiods for 1991–2001 and 2002–2013—allows one to better control for changes in unobserved firm characteristics, addressing an important endogeneity concern. On the other hand, however, using two subperiods and analyzing those periods independently assumes that there is nothing we can learn across periods. More importantly, doing so has the downside of making less data available, resulting in a loss of statistical power and weakening the ability of a model with

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<sup>55</sup> Amihud et al., *supra* note 6, at 1484.

<sup>56</sup> See *supra* Section I.A.

firm fixed effects—such as that employed in ASDS to replicate CLS—to find an association even when such association is *actually supported by the data*.

#### A. Preliminary Observations

The first preliminary observation concerns the sample ASDS employ in replicating the pooled panel regressions with firm fixed effects in CLS. Their sample covers the period 1991–2013, while our sample is much longer and covers the period 1978–2015. Further, they state that “from 1990–2006, [ASDS] follow Professors Bebchuk, Cohen, Ferrell, Gompers, Ishii, Metrick, and others by assuming a firm’s governance provisions as reported in a given IRRC volume remained in place during the period following the publication of the volume until the publication of the subsequent volume.”<sup>57</sup>

Conversely, the CLS study handchecked all missing years that were skipped by the IRRC volumes, incorporating many changes that occurred during the years in which IRRC did not update their data.<sup>58</sup> In particular, for those firms, the changes in board structure actually occurred one year before the changes appear in any IRRC volume. While we do not have the exact data used in ASDS, we demonstrate below that the positive time-series association between firm value and staggered boards is stronger in our sample even when we restrict it to exactly the same period as used in ASDS and have otherwise the same specifications, suggesting that differences in board structure data may be the cause of some of the different results in CLS and ASDS.

A second preliminary observation concerns the econometric specification employed in ASDS. Unlike BC and CLS, which use a linear-linear (or level-level) model, where firm value is proxied by Tobin’s  $Q$ , ASDS uses a log-level model by employing the logarithmic transformation of Tobin’s  $Q$ .<sup>59</sup> Using the log-linear functional form does not change much, as the correlation between  $Q$  and  $\log(Q)$  in the sample is approximately ninety-seven percent. Intuitively, the  $\log(Q)$  specification reduces the influence of outlier observations: here, the influence of firms with high values of  $Q$ . In CLS, as in most of the literature,  $Q$  itself is the dependent variable, and the influence of outliers is reduced through winsorizing the data.<sup>60</sup> When we use log-level specifications (i.e., with the log of  $Q$  as the dependent variable) in our sample, our results become even more significant, economically and statistically. Therefore, this Essay’s use of a level-level model—that is, the use of  $Q$  as the dependent variable rather than the log of  $Q$ , as in ASDS—should be interpreted as a conservative strategy.

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<sup>57</sup> Amihud et al., *supra* note 6, at 1488.

<sup>58</sup> See Cremers et al., *Staggered Boards*, *supra* note 5, at 424-25.

<sup>59</sup> Amihud et al., *supra* note 6, at 1489.

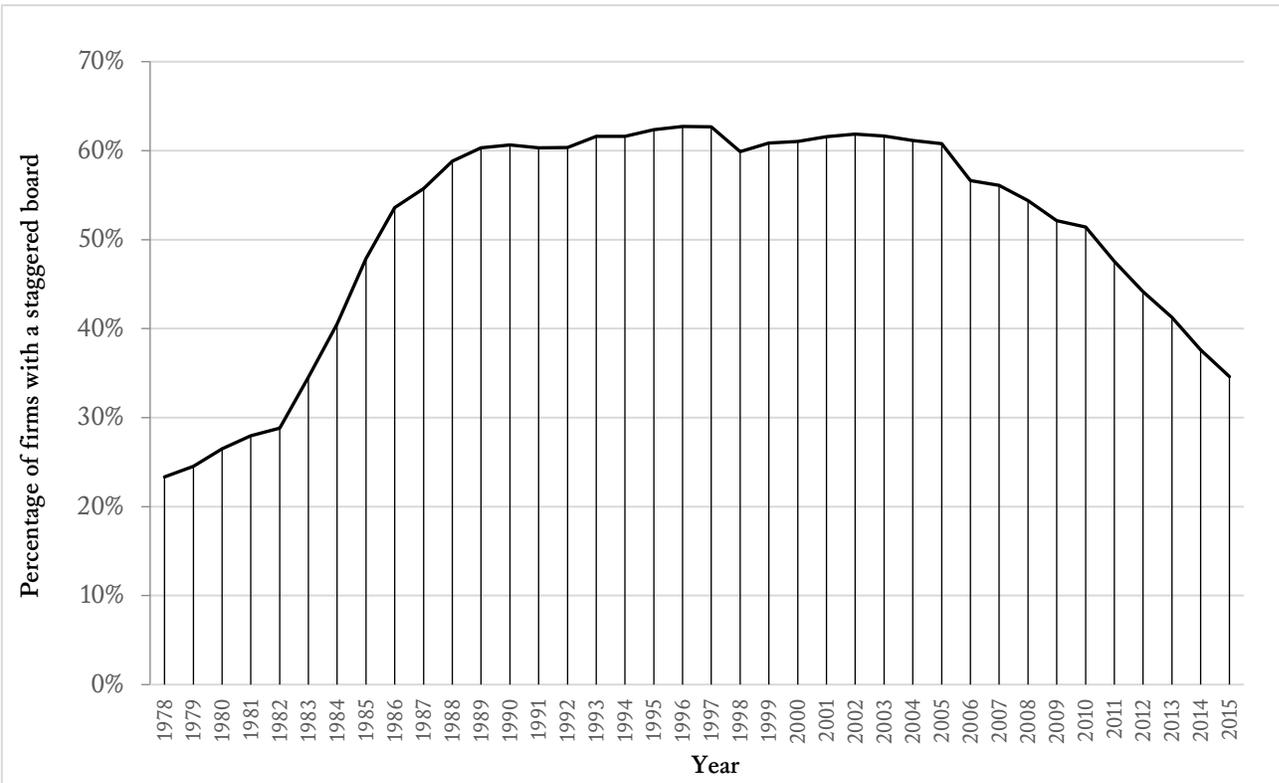
<sup>60</sup> See Cremers et al., *Staggered Boards*, *supra* note 5, at 427.

### B. Data and Subsamples

While ASDS end their sample in 2013, for consistency with CLS, in this Essay we use data up to 2015. However, we verified that our results remain similar if we end the sample in 2013, as in ASDS.

The “correction” ASDS employs for the alleged inability of our model with firm fixed effects to “account for the changing nature of the firm over time”<sup>61</sup> is splitting the full sample in two, arguing that this is useful to “allow for the unobserved firm characteristics to vary over time and have their own effect on value . . . .”<sup>62</sup> It is thus important to understand the implications of this test in full. Figure 1 of CLS, which we reproduce below and which shows the percentage of firms with a staggered board in 1978–2015 in our sample, helps to the task.

FIGURE 1: PERCENTAGE OF FIRMS WITH A STAGGERED BOARD IN 1978–2015



<sup>61</sup> Amihud et al., *supra* note 6, at 1475.

<sup>62</sup> *Id.* at 1498.

As shown by Figure 1, there are two major trends in board structure variation in the sample: one from 1978 to 1990, with many firms adopting a staggered board, and a second from 2003 (more rapidly from 2005) to 2015, with many firms removing a staggered board. In the period 1991–2002 there is, instead, very little time variation in board structure, as very few firms either adopted or removed a staggered board over this period.

These trends matter because, as noted above,<sup>63</sup> the purpose of employing a time-series analysis with firm fixed effects in examining the value implications of staggered boards is to allow empiricists to compare the average  $Q$  *before versus after* a change in board structure, which emphasizes the importance of having sufficient time variation in a panel to accurately estimate such changes in valuation over time.

It follows that in a time-series analysis, we should expect to find no significant association between  $Q$  and board structure during the period 1991–2002—even if such association was present in the actual data—as Figure 1 shows that there is very little variation in board structure during that period. This means, as we discuss in more details in CLS,<sup>64</sup> that in the time-series we cannot derive any inference about the value implications of staggered boards during that period.

This conclusion makes the choice of ASDS subsamples hard to understand, as their first subsample (1991–2002) covers exactly the period for which CLS document almost no time-series variation in board structure. It is thus unsurprising that they find no statistically significant results for the first subsample. We accordingly ignore the results for that subsample and focus on the second ASDS subsample, spanning from 2003 to 2013 (2003 to 2015 in our reply, where we use the full available data).

### C. Firm Fixed Effects Regressions and Change in $Q$ Regressions

In examining the ASDS results for the subsample starting in 2003, we begin again with a few preliminary observations. The general idea behind employing smaller subperiods is that doing so provides a robustness check to results obtained for a longer period. However, doing so also reduces the amount of data available for the estimation by limiting both time variation in board structure and the amount of data available for estimating average levels of  $Q$ . This implies that using firm fixed effects regressions with only eleven years, as ASDS do for the second subsample, may so reduce the statistical power of firm fixed effects regressions as to make this methodology unable to identify any association between staggered boards and firm value, even if there is actually one present in the data. As explained above, employing regressions of changes in firm value on changes in board

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<sup>63</sup> See *supra* Section I.B.

<sup>64</sup> CLS also includes a very instructive figure (Figure 2) showing the cohorts of staggering up and staggering down. Cremers et al., *Staggered Boards*, *supra* note 5, at 429–30.

structure can mitigate this limitation, but ASDS do not consider CLS's use of this additional methodology.

### 1. Firm Fixed Effects Regressions

With these observations in mind, we first examine the robustness of CLS's results for pooled panel  $Q$  regressions with firm fixed effects in Table 1, Panel A below. We start in Column 1 with the 1996–2015 sample, which we use as our reference regression, finding a statistically significant positive time-series association between staggered boards and firm value. We start in 1996 rather than 1993 (as in ASDS) mainly because the subsample period 1996–2015 is exactly the same we use in CLS, when we split our own full sample in two (i.e., 1978–1995 and 1996–2015).<sup>65</sup> We verify, however, that our results remain statistically significant if we start the sample earlier (e.g., in 1990, 1991, 1992, 1993, 1994, or 1995).

We then report the results for the additional following subsamples: 1997–2015, 1998–2015, 1999–2015, 2000–2015, 2001–2015, 2002–2015, and 2003–2015. We find that when we start the subsample in 2000 or earlier, rather than in 2003 as in ASDS, the association between staggered boards and firm value remains positive and statistically significant. Only if we start the sample in 2001, 2002, or 2003, does the coefficient of *Staggered Board* (i.e., the dummy variable indicating that the firm has a staggered board) become insignificant, as in ASDS.

Next, in Table 1, Panel B, we use the ASDS specification with the log-level model ( $\log(Q)$ ). With this different specification, the staggered board results remain significant when the sample starts in 2001 or earlier. This means that if ASDS had started their second subsample in 2001 rather than 2003 in their regressions of Table 4, they would presumably have found a positive and statistically significant coefficient. We also observe that the t-statistics with the log-transformation of  $Q$  are generally higher, confirming that the level-level model that we use is a conservative choice.

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<sup>65</sup> *Id.* at 429 tbl.3, col.6.

TABLE 1: FIRM VALUE AND STAGGERED BOARDS

This table presents annual pooled panel  $Q$  regressions on *Staggered Board* with firm and year fixed effects (f.e.). All specifications include the following regression variables: *Staggered Board*<sub>[t-1]</sub>,  $\ln(\text{Assets})$ <sub>[t-1]</sub>, *Delaware incorporation*<sub>[t-1]</sub>,  $ROA$ <sub>[t-1]</sub>,  $CAPX/Assets$ <sub>[t-1]</sub>,  $R\&D/Sales$ <sub>[t-1]</sub>, and *Industry M&A volume*<sub>[t-1]</sub>.<sup>66</sup> We use  $Q$  as the dependent variable in Panel A, and  $\log(Q)$  in Panel B. In each column, we use a different time period, starting with 1996–2015 in column (1), which replicates the result in column (6) of Table 3 in CLS. We only show the coefficient of *Staggered Board*<sub>[t-1]</sub> in order to save space.  $T$ -statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by \*\*\*, \*\*, and \*, respectively.

Panel A. Pooled Panel Regressions Using  $Q$ 

		Dependent variable: $Q_{[t]}$							
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Period:		1996– 2015	1997– 2015	1998– 2015	1999– 2015	2000– 2015	2001– 2015	2002– 2015	2003– 2015
<i>Staggered board</i> <sub>[t-1]</sub>		0.098**** (2.72)	0.103*** (2.88)	0.097*** (2.75)	0.087*** (2.59)	0.067** (2.06)	0.051 (1.58)	0.038 (1.22)	0.024 (0.81)
Control included		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm + year f.e.		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$N$		21,454	20,593	19,733	18,561	17,473	16,359	15,261	13,866
Adj. $R^2$		0.758	0.760	0.763	0.773	0.784	0.791	0.795	0.807

<sup>66</sup>  $\ln(\text{Assets})$ <sub>[t-1]</sub> is the log of the book value of total assets; *Delaware incorporation*<sub>[t-1]</sub> is a dummy variable indicating if the firm is incorporated in Delaware;  $ROA$ <sub>[t-1]</sub> is the return on assets calculated as the ration of the firm's EBITDA142 over the book value of total assets;  $CAPX/Assets$ <sub>[t-1]</sub> is the ratio of capital expenditures over the book value of total assets;  $R\&D/Sales$ <sub>[t-1]</sub> is the ratio of research and development expenditures over sales; and *Industry M&A volume*<sub>[t-1]</sub> is the ratio of mergers and acquisitions' dollar volume in SDC to the total market capitalization from CRSP per Fama-French 49 industries.

Panel B. Pooled panel regressions using  $\log(Q)$ 

		Dependent variable: $\log(Q_{[it]})$							
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Period:		1996– 2015	1997– 2015	1998– 2015	1999– 2015	2000– 2015	2001– 2015	2002– 2015	2003– 2015
<i>Staggered board</i> <sub>[t-1]</sub>		0.045*** (3.06)	0.048*** (3.22)	0.046*** (3.13)	0.042*** (2.95)	0.034** (2.39)	0.028* (1.95)	0.021 (1.45)	0.017 (1.21)
Control included		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm + year f.e.		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>N</i>		21,454	20,593	19,733	18,561	17,473	16,359	15,261	13,866
Adj. <i>R</i> <sup>2</sup>		0.779	0.782	0.784	0.793	0.802	0.806	0.810	0.821

The trends in Figure 1 and the results in Table 1 cast doubt on the conclusion that ASDS derive by splitting their sample in two. More generally, these findings highlight how using separate subsamples with firm fixed effects is an extreme way to deal with how associations might change over time. This is because, with firm fixed effects, as discussed above,<sup>67</sup> the regression's coefficients are only identified from within-firm time variation. Therefore, the smaller the time period considered—and hence the fewer the changes in board structure contained in the data and the more limited the availability of data to estimate how the average level of  $Q$  has changed—the more difficult it is to identify any time-series associations even if these associations do actually exist. Therefore, as we will explore in more detail in Part III, a reasonable explanation for the insignificant time-series association between firm value and board structure ASDS find when splitting the sample in two is that models with firm fixed effects have “poor power”; that is, a poor ability to find any association even if such association is actually present in the data, especially when these models are employed over relatively short samples.

Also recall that once firm fixed effects are included, the coefficient of *Staggered Board* captures the difference in the average  $Q$  before versus after changes in board structure. Thus, when the ASDS 1991–2013 sample is split into the two sub-periods of 1991–2002 and 2003–2013, changes in board structure that occur around the split of 2002–2003 are removed from the data, no longer showing up as changes in board structure in either of the separate samples. This further means that changes in 2004

<sup>67</sup> See *supra* Section I.B.

have only one observation of  $Q$  in the period before the change, rendering a comparison between the average  $Q$  before and after the change considerably more noisy. Moreover, changes in 2005 appear in the ASDS sample as changes in 2006, as IRRRC did not update their data in 2005.<sup>68</sup> As a result, for the firms where the board structure changed in 2005, the methodology in ASDS estimates the average  $Q$  before the change as the average  $Q$  in 2003, 2004, and 2005 (controlling for other firm characteristics in the regression, of course), though 2005 already covers the period after the change in board structure.

## 2. Change-in- $Q$ Regressions

A way to address the methodological limitation affecting firm-fixed-effects regressions is using change-in-firm-value regressions to verify a time-series association. Indeed, as we explained above, change-in-firm-value regressions can more easily pick up time-series associations in shorter samples, because changes in firm value are not compared to the average firm value in the full period before the change in board structure, but only to the firm value closely prior to the change in board structure.

Using change-in- $Q$  regressions, CLS obtains similar results to the firm-fixed-effects regressions for the full 1978–2015 sample, with an increase (decrease) in  $Q$  after a firm adopts (removes) a staggered board, and where the changes in  $Q$  are not immediate but materialize in the second year after the change in board structure.<sup>69</sup> In Table 2 below, we thus use the same methodology as in CLS of regressing the changes in  $Q$  from one to three years after the change in board structure for the same sub-samples as in Table 1 (1996–2015, 1997–2015, 1998–2015, 1999–2015, 2000–2015, 2001–2015, 2002–2015, and 2003–2015).

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<sup>68</sup> See *supra* notes 63–64 and accompanying text.

<sup>69</sup> Cremers et al., *supra* note 6, at 431 tbl.4.

TABLE 2: CHANGES IN FIRM VALUE AND CHANGES  
IN STAGGERED BOARDS

This table presents pooled panel first difference regressions with the dependent variable being the change in  $Q$  from  $t$  to  $t+1$  in Panel A (i.e.,  $\Delta Q_{[t,t+1]}$ ), the change in  $Q$  from  $t$  to  $t+2$  in Panel B (i.e.,  $\Delta Q_{[t,t+2]}$ ), and the change in  $Q$  from  $t$  to  $t+3$  in Panel C (i.e.,  $\Delta Q_{[t,t+3]}$ ). The dependent variables have been demeaned with their annual cross-sectional averages. As independent variables, we include the following:  $\Delta Staggered Board_{[t-1,t]}$ ,  $\Delta Ln(Assets)_{[t-1,t]}$ ,  $\Delta ROA_{[t-1,t]}$ ,  $\Delta CAPX/Assets_{[t-1,t]}$ ,  $\Delta R\&D/Sales_{[t-1,t]}$ , and  $\Delta Industry M\&A volume_{[t-1,t]}$ . In each column, we use a different time period, starting with 1996–2015 in column (1), which replicates the result in columns (7)–(9) of Table 4 in CLS. We only show the coefficient of  $\Delta Staggered Board_{[t-1]}$  in order to save space. All variables are defined in Appendix Table 1.  $T$ -statistics (in their absolute value) are based on robust standard errors clustered by firm and presented in parentheses below the coefficients. Statistical significance of the coefficients is indicated at the 1%, 5%, and 10% levels by \*\*\*, \*\*, and \*, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Period:	1996– 2015	1997– 2015	1998– 2015	1999– 2015	2000– 2015	2001– 2015	2002– 2015	2003– 2015
<b>Panel A: Dependent variable: <math>\Delta Q_{[t,t+1]}</math></b>								
$\Delta Staggered board_{[t-1]}$	0.00138 (0.06)	0.028 (1.13)	0.027 (1.12)	0.031 (1.35)	0.030 (1.28)	-0.004 (-0.17)	0.006 (0.25)	-0.001 (-0.06)
<b>Panel B: Dependent variable: <math>\Delta Q_{[t,t+2]}</math></b>								
$\Delta Staggered board_{[t-1]}$	0.0728* (1.82)	0.094** (2.43)	0.095** (2.47)	0.094** (2.37)	0.092** (2.33)	0.082** (2.26)	0.080** (2.18)	0.074* (1.95)
<b>Panel C: Dependent variable: <math>\Delta Q_{[t,t+3]}</math></b>								
$\Delta Staggered board_{[t-1]}$	0.135*** (2.91)	0.123*** (2.88)	0.123*** (2.86)	0.131*** (2.85)	0.138*** (3.02)	0.120** (2.50)	0.113** (2.35)	0.112** (2.15)

Consistent with our criticism of the results in ASDS (and consistent with our prior results in CLS), Table 2, Panel A shows that the average change in  $Q$  a year after the change in board structure is insignificant, but it becomes consistently positive and statistically significant in the second and third years after the change in board structure, for all periods considered (including 2003–2015, for which the firm fixed effects results are insignificant). It is also worth highlighting that the

effect on firm value is monotonically increasing over time after the change in board structure. In particular, even when we only use the 2003–2015 time period, we find that after the adoption (or dismissal) of a staggered board, the value of  $Q$  increases (decreases) over time, where the changes in  $Q$  are strongly significant three years after the change in board structure.<sup>70</sup>

We thus conclude that the results in Table 1 and Table 2 of this Essay contradict the ASDS criticism of the results in CLS about the positive time-series association between firm value and staggered boards, showing that CLS’s results are robust

### III. THE PURPORTED INSIGNIFICANT ASSOCIATION OF STAGGERED BOARDS AND FIRM VALUE

In this Part, we employ the bootstrapping methodology, which uses random sampling techniques to estimate the statistical properties of an estimator,<sup>71</sup> to further explore the claims in ASDS and the validity of the results in CLS.

This advanced methodology shows, first, that firm-fixed-effects regressions and change-in-value regressions are not more likely to find evidence for any association if no such association is actually present in the data; that is, both these tests have good statistical “size.” Second, the use of bootstrapping also confirms that the difference in results for firm-fixed-effects regressions and change-in- $Q$

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<sup>70</sup> A further criticism of ASDS deserves attention here. ASDS observe that the conclusion that staggered boards have no impact on firm value is consistent with the results obtained in another of our staggered board studies, *Board Declassification Activism: The Financial Value of the Shareholder Right Project* (the SRP study). See K.J. Martijn Cremers & Simone M. Sepe, *Board Declassification Activism: The Financial Value of the Shareholder Rights Projects* (June 2017) (working paper), available at [https://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2962162](https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2962162). In that work, we employ the declassification activity of the Harvard Shareholder Rights Project (SRP)—a clinical program at Harvard Law School assisting institutional investors with submitting board declassification proposals during the period 2011–2014—as a quasi-natural experiment to examine the value implications of staggered boards. In other words, we treat the SRP’s activity as a source of exogenous variation in recent declassification campaigns to test the wealth effects of staggering/destaggering activity while mitigating well-known endogeneity concerns. Consistent with the results in CLS, we find that declassifying SRP targets declined in value after declassification. In ASDS’s words, however, the SRP study would ultimately contradict the results in CLS as it also finds that “destaggering has no significant effect on the value of firms not targeted by the Harvard Rights Project.” Amihud et al., *supra* note 6, at 1501. However, we have explained elsewhere why this is an inaccurate interpretation of that study’s results. See Martijn K.J. Cremers & Simone M. Sepe, *Board Declassification Activism: Why Run Away from the Evidence?* (June 2017) (working paper), available at [https://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=2991854](https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2991854) (replying to criticism of the SRP study by Lucian Bebchuk and Alma Cohen). If one wants to focus on results for non-SRP declassifications, one should do so in conjunction with the evidence in CLS, as the non-SRP declassifications in the SRP study are only a small subset of the declassifications studied in CLS. Accordingly, under the argument that non-SRP declassifications matter independently from SRP declassifications—which ASDS seem to accept—the results in the SRP study need to be interpreted jointly with the results for the much longer sample in CLS. Performing this analysis shows that the results in the SRP study are fully consistent with the evidence in CLS. See *id.*

<sup>71</sup> An estimator is the methodology one uses to estimate a given empirical association, i.e., in this case the association between staggered boards with firm value.

regressions over shorter sample periods can be explained by the difference in the statistical “power” of each methodology. Indeed, the change-in-value regressions have substantially better statistical power than the firm-fixed-effects regressions and hence are more capable of finding a time-series association between firm value and board structure.

Therefore, the analysis in this Part further confirms the validity of the results in CLS and the lack of robustness of the ASDS results.

#### A. *Bootstrapping and Statistical Inference*

Before starting the discussion of the bootstrapping analysis of the results in ASDS, in order to make this analysis accessible to a wide audience of nontechnical readers, it is useful to briefly recap here some key statistical concepts. These concepts include Type I and Type II errors, as well as the basic principles of using bootstrapped samples to assess the size and power of statistical tests. To this end, we start by considering the hypothesis that, as argued by ASDS, *Q* is not associated with board structure, i.e., that the “true” coefficient of the staggered board in *Q* regressions is equal to zero (the null hypothesis in ASDS). Type I errors occur when the null hypothesis is true—in this case, when there is actually no association between firm value and board structure—but one empirically rejects the null (false positive). Instead, Type II errors occur when one does not reject the null hypothesis and the null is actually false (false negative). Hence, here, a Type II error would occur if one found no statistically significant association between firm value and board structure but such association was actually present in the data.

According to ASDS, the results in CLS would have a high probability of suffering from a Type I error, meaning that the methodologies used in CLS are likely to find a statistically significant association between firm value and board structure even if there is actually no such association in the data. Indeed, the high likelihood of a Type I error is the reason in ASDS for both adding controls and considering shorter subperiods. Conversely, our results from Table 1 and Table 2 above seem to suggest that the analysis in ASDS might suffer from a Type II error, under which the methodology ASDS use does not allow them to find any statistically significant association between firm value and board structure but such association is actually present in the data.

In order to examine these opposite hypotheses, we start by employing the bootstrapping methodology to test the “size” of both firm-fixed-effect regressions and change-in-value regressions. A size test estimates how often one would find a statistically significant coefficient in artificial samples, which are constructed through bootstrapping to be very similar to the actual data, but in such a way that

we know for a fact (i.e., by construction) that there is no actual association.<sup>72</sup> We then also employ the bootstrapping methodology to test the “power” of each methodology, constructing bootstrapped samples for the “power” test in a very similar way to the bootstrapped samples for the “size” test, except that in this case we know for sure that in each bootstrapped sample there is, by design, a positive association between firm value and board structure.<sup>73</sup>

While a full discussion of the technicalities of bootstrapping is not within the scope of this Essay, we hope to illustrate the intuition behind the size and power tests by considering the following,

- i.*  $X_0$ : means finding *no* statistically significant time-series association between  $Q$  and board structure in the data;
- ii.*  $X_1$ : means *finding* a statistically significant time-series association between  $Q$  and board structure in the data;
- iii.*  $Y_0$ : means the *actual* time-series association between  $Q$  and board structure is *zero*; and

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<sup>72</sup> Specifically, each bootstrapped sample is constructed in the following way. We first estimate the regression of  $Q$  on *Staggered Board* plus the set of standard controls, exactly as in column 1 of Table 1, Panel A above. Next, we decompose each observation of  $Q$  into (i) the part that is fitted by the regression coefficients excluding the *Staggered Board* coefficient (i.e., multiplying all other coefficients, including on all fixed effects, with the values of all variables), (ii) the part that is fitted by the regression coefficient on *Staggered Board* (i.e., that coefficient times the value of the *Staggered Board* dummy variable), and (iii) the part of actual  $Q$  that is not explained by any of the variables, including the *Staggered Board* indicator, the controls and all of the fixed effects, i.e., the residual part.

Second, each new sample consists of “artificial” values of  $Q$  that are constructed by combining the first and third part, but leaving out the second part. By construction, the first and third parts are statistically uncorrelated with the *Staggered Board* indicator, and thus the sum of the first and third part will be uncorrelated with the *Staggered Board* indicator as well. In each bootstrapped sample, each firm’s first part is combined with the full available history of the residual part of a randomly chosen firm (with replacement, though results are identical when we randomly assign residual parts without replacement), matched by year (such that the first part for, e.g., the year 2006 is combined with the residual part of a randomly chosen firm for 2006 as well).

Because a key feature of Tobin’s  $Q$  is that it tends to be persistent at the firm level and heteroskedastic (i.e., the variance of the error terms differ across firm, and residuals are correlated within firms), in the procedure we retain the full correlation structure of the actual Tobin’s  $Q$  (of the data) in our bootstrapped samples. The use of the full history of the residual part ensures that the bootstrapped data will have similar cross-sectional and time-series correlations as the actual data.

Finally, we randomly assign a staggered board to firms in the sample, such that in each bootstrapped sample, in each year the number of firms with a bootstrapped staggered board is equal to the number of firms with a staggered board in the actual data, and such that the number of firms adopting and removing a staggered board is as well equal to the actual data. In summary, the bootstrapped samples for the size test that result are very similar to the actual data, except that we know for sure that in each bootstrapped “artificial data” sample there is, by construction, no association between firm value and the randomly assigned board structure.

<sup>73</sup> This positive association is created by taking the bootstrapped samples from the “size” tests, and permanently increasing (decreasing) the bootstrapped values of  $Q$  after a firm is randomly assigned to adopt (remove) a staggered board, where our choices of the magnitude of these changes are explained in more detail below.

*iv.*  $Y_1$ : means the *actual* time-series association between  $Q$  and board structure is *nonzero*.

When an empiricist uses a methodology with “poor size,” then it is likely that she finds  $X_1$  even if  $Y_0$  is “true.” Put differently, if size is poor, then  $P(X_1|Y_0)$ —the probability of  $X_1$  conditional on  $Y_0$ —is high. On the other hand, if size is strong, then it is very unlikely that one would find  $X_1$  if  $Y_0$  is “true,” that is,  $P(X_1|Y_0)$  is very low. In brief, only if size is strong, it is reasonable to conclude that the data supports  $Y_1$ , if you find  $X_1$ . But if size is poor, then finding  $X_1$  does not allow one to conclude much, as it is likely that one finds  $X_1$  even if  $Y_0$  is true. Therefore, if the models in CLS were shown to have strong size, this would add to the robustness of the conclusion in CLS that staggered boards matter for firm value. Vice versa, if these models were shown to have poor size, the ASDS criticism of the results in CLS would be more likely to be accurate.

Correspondingly, when an empiricist uses a test with “poor power,” it is likely that one can find  $X_0$  even if  $Y_1$  is “true” (meaning that scholars may not find an association between staggered boards and firm value even if such association is actually present in the data). That is, if power is poor, then  $P(X_0|Y_1)$ —the probability of  $X_0$  conditional on  $Y_1$ —is high. Correspondingly, this implies that if power is poor, then finding  $X_0$  should not be interpreted as strong evidence for  $Y_0$ . On the other hand, if power is strong, then it is very unlikely that one can find  $X_0$  if  $Y_1$  is “true,” that is,  $P(X_0|Y_1)$  is low. In brief, only if power is strong, it is reasonable to conclude that the data support  $Y_0$  if you find  $X_0$ . But if power is poor, then finding  $X_0$  does not allow one to conclude much. Therefore, only if the model in ASDS was shown to have strong power, their conclusion that staggered boards do not matter for firm value would likely be robust, while if the ASDS model had poor power, this would cast doubt on their conclusion.

On these assumptions, we run 10,000 bootstrapped samples across firm-fixed-effects regressions and change-in-value regressions. As shown below, our bootstrap results contradict the claim in ASDS that these methodologies likely find a statistically significant association even when in actuality there is none, as they show that both methodologies as employed in CLS have good “size,” that is, do not suffer from high Type I error.<sup>74</sup>

Next, we show that the firm-fixed-effect regressions have poor power, that is, a limited ability to find a statistically significant association even if the sample is constructed such that there is an actual association between firm value and board structure. However, we also find that the change-in- $Q$  regressions have much better power, indicating that one is much more likely to find a statistically significant association if there is an actual association in the data. Consistent with our results from Table 1 and Table 2 above, these results thus indicate that the only

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<sup>74</sup> More precisely, one could say that the likelihood that our results in CLS may suffer from a Type I error falls within the confidence interval that we documented in both CLS and Tables 1 and 2 above.

methodology considered by ASDS—namely regressions with firm fixed effects—has poor power and hence a high likelihood of Type II error.

### B. Size Test

The first bootstrapping test we run, presented in Table 3 below, is the size test for the 1996–2015 sample of Table 1, Panel A, column 1. (However, the results are similar if we use the sample 1993–2013 or 1991–2013 as in ASDS.) Recall that with our procedure, the resulting bootstrapped samples are all very similar to the actual data. This means that each sample has the same percentage of firms with a staggered board in each calendar year, and the same number of firms staggering down and staggering up each year, as in the actual data. However, the board structure is randomly assigned such that there is actually *no association* between firm value and board structure in the bootstrapped samples *by construction*.

TABLE 3: SIZE TEST—REGRESSIONS OF THE LEVEL OF Q

This table presents bootstrap results to test the size of the pooled panel  $Q$  regressions as shown in column 1 of Table 1, Panel A. Each bootstrapped sample is constructed to be very similar to the actual data, including the cross-sectional and time-series correlation structure, as described in the text. We construct a total of 10,000 bootstrapped samples in which there is no association between  $Q$  and *Staggered Board*. For each bootstrapped sample, we run a pooled panel  $Q$  regression on *Staggered Board*<sub>[ $t-1$ ]</sub>,  $\ln(\text{Assets})_{[t-1]}$ , *Delaware incorporation*<sub>[ $t-1$ ]</sub>,  $ROA_{[t-1]}$ ,  $CAPX/Assets_{[t-1]}$ ,  $R\&D/Sales_{[t-1]}$ , and *Industry M&A volume*<sub>[ $t-1$ ]</sub>, with year and firm fixed effects. In Panel A, we show the 0.5th, 1st, 2.5th, 5th, 95th, 99th and 99.5th percentile of the coefficient of *Staggered Board*<sub>[ $t-1$ ]</sub> and its t-statistic across all 10,000 pooled panel regressions, based on robust standard errors that are clustered by firm. In Panel B, we report the percentage of bootstrapped samples where the t-statistic of the coefficient of *Staggered Board*<sub>[ $t-1$ ]</sub> is smaller or larger than the standard critical values for double-sided tests at the 10% level (+/- 1.645), 5% level (+/- 1.96), and 1% level (+/- 2.326).

#### Panel A. Percentiles of bootstrapped coefficients and t-statistics of *Staggered Board*<sub>[ $t-1$ ]</sub>

Percentile	0.50%	1%	2.50%	5%	95%	97.50%	99%	99.50%
Coefficient	-0.226	-0.202	-0.169	-0.140	0.139	0.167	0.201	0.227
t-statistic	-2.55	-2.29	-1.92	-1.62	1.49	1.78	2.11	2.30

Panel B. Percentage of bootstrapped t-statistics of *Staggered Board*<sub>[t-1]</sub> below/above some critical values

Condition	% of bootstraps
t-statistic < -1.645	4.82%
t-statistic < -1.96	2.21%
t-statistic < -2.326	0.91%
t-statistic > 1.645	3.60%
t-statistic > 1.96	1.53%
t-statistic > 2.326	0.44%

Table 3, Panel A presents the percentiles of coefficient and t-statistics (which is the measure of statistical significance) of staggered boards on  $Q$  for the pooled panel regressions based on 10,000 bootstraps. In other words, we report how often we find coefficients and t-statistics of a certain size in the set of 10,000 bootstraps.

As shown by Table 3, Panel A, we find a coefficient of the *Staggered Board* indicator of -0.226 (or even more negative) in only 0.50% of the bootstrapped samples. Similarly, we find a t-statistic of -2.55 (or even more negative) of the *Staggered Board* coefficient in only 0.50% of the bootstrapped samples. Using 5% as the level of statistical significance and using a two-sided test, we look at the values for the 2.5 and 97.5 percentiles. For the coefficient of the *Staggered Board* dummy, we find that a coefficient as negative as -0.169 (or even more negative) or as positive as 0.167 (or even more positive) occurs in no more than 5% of the bootstrapped samples. For the t-statistic of the coefficient of the staggered board dummy, we find that a t-statistic as negative as -1.92 (or even more negative) or as positive as 1.78 (or even more positive) occurs in no more than 5% of the bootstrapped samples.

Our bootstrapped samples thus indicate that our tests based on t-statistics have good size. Indeed, when one finds a t-statistic above 2 in absolute value (as we found in CLS and Table 1 above), it is quite rare for such a large t-statistic to occur in bootstrapped samples where there is no actual association (recall that we ensured that there is in fact no association by construction in each of these bootstrapped samples). Otherwise this t-statistic would be less likely to occur than in 2.5% of cases. We hence conclude that our pooled panel regressions have good size, which means that it would be very unlikely that one would find a statistically significant coefficient on the *Staggered Board* dummy using our pooled panel setup, if there was actually no association between board structure and  $Q$  at all (i.e., if the ASDS null hypothesis was “true”).

To substantiate this assertion, consider now Table 3, Panel B, where we count the percentage of bootstrapped samples in which we would conclude that there is a statistically significant coefficient using the default critical values for the t-statistic, namely 1.645 for a double-sided test at the 10% confidence level, 1.96 for the 5%

confidence level, and 2.326 for the 1% confidence level (in graphical terms, one, two, and three stars after the coefficient, respectively).

Table 3, Panel B shows that only 2.21% of the bootstrapped samples generates a t-statistic (of the coefficient of the *Staggered Board* dummy) that is -1.96 or more negative, and only 1.53% have a t-statistic of 1.96 or higher. Therefore, using the 5% confidence level (critical value of +/- 1.96), we would incorrectly conclude that there is a significant coefficient in 2.21%+1.53% = 3.74% of cases, which is lower—and thus better—than the 5% level allowed. This means that using the 5% confidence level (critical value of +/- 1.96), the estimation of the probability of Type I error for CLS is around 3.74%. In other words, there is only a 3.74% probability that CLS rejected the null hypothesis ( $Q$  is not associated with board structure) when the null is indeed “true.”

Next, in Table 4, we essentially perform the same tests as in Table 3 but considering the change in  $Q$  regressions (see Table 2 above). For this test, we use the same bootstrapped samples as those presented in Table 3, and then calculate 1-year, 2-year and 3-year changes in  $Q$  (i.e., three different time-horizons).

TABLE 4: SIZE TESTS—REGRESSIONS OF CHANGES IN  $Q$

This table presents bootstrap results to test the size of the pooled panel change in  $Q$  regressions as shown in column (1) of Table 2. Each bootstrapped sample is constructed to be very similar to the actual data, including the cross-sectional and time-series correlation structure, as described in the text. We construct a total of 10,000 bootstrapped samples in which there is no association between  $Q$  and *Staggered Board*. For each bootstrapped sample, we run a pooled panel regression of the change in  $Q$  over the next 1, 2, or 3 years on  $\Delta$  *Staggered Board*<sub>[t-1,t]</sub>,  $\Delta$   $\ln(\text{Assets})$ <sub>[t-1,t]</sub>,  $\Delta$   $ROA$ <sub>[t-1,t]</sub>,  $\Delta$   $CAPX/Assets$ <sub>[t-1,t]</sub>,  $\Delta$   $R\&D/Sales$ <sub>[t-1,t]</sub>, and  $\Delta$  *Industry M&A volume*<sub>[t-1,t]</sub>. We report the percentage of bootstrapped samples where the t-statistic of the coefficient of  $\Delta$  *Staggered Board*<sub>[t-1,t]</sub> is smaller or larger than the standard critical values for double-sided tests at the 10% level (+/- 1.645), 5% level (+/- 1.96) and 1% level (+/- 2.326). The change in  $Q$  over the next 1, 2, or 3 years is captured by the variable  $\Delta Q_{[t,t+1]}$ ,  $\Delta Q_{[t,t+2]}$  and  $\Delta Q_{[t,t+3]}$ , respectively.

Dependent variable:	$\Delta Q_{[t,t+1]}$	$\Delta Q_{[t,t+2]}$	$\Delta Q_{[t,t+3]}$
Condition	% of bootstraps		
t-statistic < -1.645	4.39%	4.19%	4.00%
t-statistic < -1.96	1.86%	2.20%	1.97%
t-statistic < -2.326	0.71%	0.76%	0.65%
t-statistic > 1.645	6.41%	6.63%	4.17%
t-statistic > 1.96	3.53%	3.70%	1.90%
t-statistic > 2.326	1.39%	1.68%	0.62%

The results for the change in  $Q$  regressions show again that these tests have good size. Over the 1-year, 2-year, and 3-year horizons, and using the 5% confidence level (critical value of  $\pm 1.96$ ), we would incorrectly conclude that the coefficient of staggered board is significant only in  $(1.86+3.53=)$  5.39%,  $(2.2+3.7=)$  5.9%, and  $(1.97+1.9) = 3.97%$  of the cases, respectively, all very similar to the 5% level.

### C. Power Test

In this section, we report the results for the “power” test. In this test, we generate bootstrapped samples where there is an *association* between adopting and dismissing a staggered board and  $Q$  by *construction*. In other words, for the power test we use samples that are constructed such that the level of  $Q$  permanently increases (decreases) after a firm adopts (removes) a staggered board, and where we test whether firm-fixed-effect regression and change-in-value regressions can find this association. Recall that every year, a certain number of firms adopts and dismisses a staggered board in the data. The number of firms in each bootstrapped sample that adopts and dismisses a staggered board is the same as the number of firms that do so in the actual data, for each calendar year.

We consider two scenarios: an immediate change in  $Q$  after a change in board structure, and a gradual change in  $Q$  after a change in board structure. In both cases, we consider only a symmetric change in  $Q$ , so that  $Q$  always increases after the adoption of a staggered board by the same amount as  $Q$  decreases after a staggered board’s removal. This choice is motivated by the finding in CLS that the differences between changes in  $Q$  (in absolute value) for adoptions versus dismissals of staggered boards are statistically insignificant.<sup>75</sup> In particular:

i. in the first scenario of an immediate and permanent change in  $Q$  after a change in board structure, the permanent change in  $Q$  is set equal to 0.098 (based on Table 1, Panel A, column 1 above and Table 3, column 6 of CLS)<sup>76</sup> for adoptions and -0.098 for removals.

ii. in the second scenario of a gradual change in  $Q$  after a change in board structure, we use the results of Table 4, columns 7, 8, and 9 of CLS<sup>77</sup> and increase  $Q$  for the first year after the change by 0.00138, for the second year by 0.0728, and for the third year and thereafter by 0.135.

In Table 5, we show the results for pooled panel regressions where the “true” coefficient of staggered board on firm value equals 0.098. (To save space, here we only show the results for the immediate change, as the results for a gradual change are basically the same.) In other words, we bootstrap 10,000 samples where the data is constructed such that the adoption (dismissal) of a staggered board is associated

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<sup>75</sup> See Cremers et al., *Staggered Boards*, *supra* note 5, at 430.

<sup>76</sup> *Id.* at 429.

<sup>77</sup> *Id.* at 431.

with an increase (decrease) in  $Q$  of 0.098, and report the percentage of bootstrapped samples in which we would (correctly) conclude that the coefficient of the *Staggered Board* indicator is statistically significant at various levels of statistical significance using firm-fixed-effect regressions.

TABLE 5: POWER TESTS—REGRESSIONS OF THE LEVEL OF  $Q$

This table presents bootstrap results to test the power of the pooled panel  $Q$  regressions as shown in Table 1. Each bootstrapped sample is constructed to be very similar to the actual data, including the cross-sectional and time-series correlation structure, as described in the text. We construct a total of 10,000 bootstrapped samples in which there is an association between  $Q$  and *Staggered Board*, where the data is constructed such that an adoption (removal) of a staggered board is associated with an increase in  $Q$  of 0.098. For each bootstrapped sample, we run a pooled panel  $Q$  regression on  $Staggered\ Board_{[t-1]}$ ,  $Ln(Assets)_{[t-1]}$ ,  $Delaware\ incorporation_{[t-1]}$ ,  $ROA_{[t-1]}$ ,  $CAPX/Assets_{[t-1]}$ ,  $R\&D/Sales_{[t-1]}$ , and  $Industry\ M\&A\ volume_{[t-1]}$ , with year and firm fixed effects. We report the percentage of bootstrapped samples where the t-statistic of the coefficient of  $Staggered\ Board_{[t-1]}$  is smaller or larger than the standard critical values for double-sided tests at the 10% level ( $\pm 1.645$ ), 5% level ( $\pm 1.96$ ) and 1% level ( $\pm 2.326$ ).

Condition	% of bootstraps
t-statistic < -1.645	0.47%
t-statistic < -1.96	0.17%
t-statistic < -2.326	0.05%
t-statistic > 1.645	18.68%
t-statistic > 1.96	11.17%
t-statistic > 2.326	5.23%

Table 5 indicates that the pooled panel regressions with firm fixed effects have poor power, by showing that for samples with an actual association (here by construction) between having a staggered board and  $Q$ , one would be unlikely to find this association in the data using this estimator. Indeed, Table 5 shows that we find a statistically significant coefficient that is positive at the 5% confidence level (i.e. with a critical value of  $\pm 1.96$ ) in only 11.17% of cases. This implies that, with 88.8% likelihood, we would reject any association between staggered boards and  $Q$ , even if there were a “true,” strongly positive association in the data. Correspondingly, this means that at the 5% confidence level (critical value of  $\pm 1.96$ ), the probability of a Type II error is 88.8%.<sup>78</sup>

<sup>78</sup> Figure 2, which is included in this document’s appendix, visualizes some of our bootstrap results as reported in Table 5. In particular, Figure 2 presents the histogram of the bootstrapped coefficients of *Staggered*

As shown by Table 6 below, the power test produces, instead, much improved results when we use change in  $Q$  regressions. In this case, we show results for both the first scenario, which considers a permanent, immediate change in  $Q$  after a change in board structure (in Panel A), and the second scenario, which assumes a gradual change in  $Q$  (in Panel B).

TABLE 6: POWER TESTS—REGRESSIONS OF CHANGES IN  $Q$

This table presents bootstrap results to test the power of the pooled panel change in  $Q$  regressions as shown in Table 2. Each bootstrapped sample is constructed to be very similar to the actual data, including the cross-sectional and time-series correlation structure, as described in the text. We construct a total of 10,000 bootstrapped samples in which there is an association between  $Q$  and *Staggered Board*. For each bootstrapped sample, we run a pooled panel regression of the change in  $Q$  over the next 1, 2, or 3 years on  $\Delta$  *Staggered Board*<sub>[t-1,t]</sub>,  $\Delta \ln(\text{Assets})_{[t-1,t]}$ ,  $\Delta \text{ROA}_{[t-1,t]}$ ,  $\Delta \text{CAPX}/\text{Assets}_{[t-1,t]}$ ,  $\Delta \text{R\&D}/\text{Sales}_{[t-1,t]}$ , and  $\Delta$  *Industry M\&A volume*<sub>[t-1,t]</sub>. We report the percentage of bootstrapped samples where the t-statistic of the coefficient of  $\Delta$  *Staggered Board*<sub>[t-1,t]</sub> is smaller or larger than the standard critical values for double-sided tests at the 10% level (+/- 1.645), 5% level (+/- 1.96), and 1% level (+/- 2.326). The change in  $Q$  over the next 1, 2, or 3 years is captured by the dependent variable  $\Delta Q_{[t,t+1]}$ ,  $\Delta Q_{[t,t+2]}$ , and  $\Delta Q_{[t,t+3]}$ , respectively.

**Panel A. Power tests assuming an immediate change in  $Q$  following a change in board structure**

Dependent variable:	$\Delta Q_{[t,t+1]}$	$\Delta Q_{[t,t+2]}$	$\Delta Q_{[t,t+3]}$
Condition	%	%	%
t-statistic < -1.645	0	0.03%	0.04%
t-statistic < -1.96	0	0.01%	0.02%
t-statistic < -2.326	0	0	0.01%
t-statistic > 1.645	84.10%	68.38%	55.35%
t-statistic > 1.96	76.56%	57.71%	43.98%
t-statistic > 2.326	65.77%	44.97%	31.24%

*Board* in Panel A and the bootstrapped t-statistics of *Staggered Board* in Panel B. The results in Panel A show that the estimated coefficients of *Staggered Board* center around 0.098, consistent with the actual change in  $Q$  after a change in board structure, while the results in Panel B show that these coefficients are estimated with considerable noise or statistical uncertainty.

Panel B. Power tests assuming a gradual change in  $Q$  following a change in board structure

Dependent variable:	$\Delta Q_{[t, t+1]}$	$\Delta Q_{[t, t+2]}$	$\Delta Q_{[t, t+3]}$
Condition	% of bootstraps		
t-statistic < -1.645	5.25%	0.06%	0.01%
t-statistic < -1.96	2.78%	0.02%	0.00%
t-statistic < -2.326	1.25%	0.01%	0.00%
t-statistic > 1.645	5.70%	49.51%	76.45%
t-statistic > 1.96	2.79%	37.92%	66.70%
t-statistic > 2.326	1.06%	25.36%	54.38%

Table 6, Panel A—for the immediate and permanent change scenario—shows that the change in  $Q$  regressions have much better power than the pooled panel regressions with firm fixed effects, but also that this power declines if we look at longer horizons. This make sense when the change in  $Q$  is actually immediate, such as in the bootstrapped samples used in Panel A, as in this case looking at longer horizons only adds statistical noise.

The results in Table 6, Panel A imply that if the bootstrapped samples are such that the “true” coefficient of *Staggered Board* equals 0.098, and using the 5% confidence level (critical value of +/- 1.96), we would reject the null hypothesis of no statistically significant association in 77% of cases over a 1-year horizon, 58% of cases over a 2-year horizon, and 44% of cases over a 3-year horizon. In other words, the regressions of changes in  $Q$  on changes in board structure would indicate a statistically significant coefficient of *Staggered Board* over a 1-year horizon in about 77% of the cases (compared to about 11% for the firm-fixed-effects regressions).

Finally, Table 6, Panel B—for the gradual change scenario—corresponds to what we find in the actual data (see Table 2 above). Recall that here the “true” coefficient of *Staggered Board* equals 0.00138 for the 1-year change, 0.0728 for the 2-year change, and 0.135 for the 3-year change (see Table 2, column 1 above).

As shown by Table 6, Panel B using the 5% confidence level (critical value of +/- 1.96), there is, as expected, very low power to find the small 1-year change. This is consistent with the results reported in Table 2 above (and in Table 4 of CLS), where we show that the change in  $Q$  is significant only *after* the first year in the change in the board structure. Conversely, the 2-year change of 0.0728 has reasonably good power, and the 3-year change has even better power. More specifically, with the 5 percent confidence level (critical value of +/- 1.96), we would reject the null hypothesis of no statistically significant association in 66.7% of the

cases. Hence, Table 6, Panel B naturally finds that power increases over longer time-horizons, consistent with the “true” coefficient actually increasing over longer time-horizons.

In summary, the power test indicates that if there is actually an association between firm value and board structure, one would be considerably more likely to find this association by using regressions of changes in  $Q$  on changes in board structure than through firm-fixed-effects regressions. This is consistent with the results presented above, where even for the shorter time period starting in 2003, the change-in- $Q$  regressions using actual data show a large, positive, and statistically significant coefficient of *Staggered Board*, while the analogous results for the same sample but using firm fixed effects regressions are insignificant.<sup>79</sup>

Therefore, our bootstrapping tests confirm that the results in ASDS can be explained by their use of a statistical methodology with poor power (i.e. firm fixed effect regressions). It follows that their finding of insignificant results when they use fairly short time periods cannot be interpreted as evidence against an association between firm value and board structure, especially when another methodology with much stronger statistical power (i.e., change-in-value regressions) results in robust evidence for such association using exactly the same data sample.

#### IV. ADDITIONAL ROBUSTNESS

As hinted above, CLS uses multiple identification strategies, in addition to pooled panel regression with firm fixed effects and change-in- $Q$  regressions, to address the selection concerns that follow from the endogenous choice of board structure.<sup>80</sup> In discussing CLS’s findings, however, ASDS exclusively focus on the firm-fixed-effects pooled panel regressions, while ignoring the results for regressions of changes in  $Q$  on changes in board structure, or the various other tests CLS perform. In this last section, we offer a brief recap of these additional tests, which *all* confirm the result of a positive association between staggered boards and firm value.

In particular:

*i.* We confirm the positive impact of the staggered board through a stock portfolio analysis, which can be interpreted as a long-term event study around changes in board structure.<sup>81</sup> We present abnormal stock returns of monthly portfolios of firms that have staggered up (in the long portfolio) and firms that have staggered down (in the short portfolio) around board staggering and de-staggering events in our sample of firms during the time period from 1978 to

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<sup>79</sup> See *supra* Section II.C.

<sup>80</sup> See *supra* Section I.B.

<sup>81</sup> See Cremers et al., *Staggered Boards*, *supra* note 5, at 430.

2015.<sup>82</sup> In particular, in the 12-12 portfolio (constructed so to hold stocks in the 12-month period before the change in board structure until 12 months thereafter, for a total of 24 months for each stock with a change in board structure), we obtained positive and statistically significant alphas in the four-factor, three-factor, and market factor models.<sup>83</sup>

*ii.* We incorporate possible selection effects through the creation of multiple matched samples based on different matching procedures.<sup>84</sup> In each matched sample, each firm with a changing board structure (i.e., a “treated” firm) in a given year is matched to a firm with the same *ex ante* board structure and similar observable characteristics that relate to board structure, but which did not change its board structure in that year (i.e., a “control” firm).<sup>85</sup> The matched samples confirm the positive (negative) relation between the adoption (removal) of a staggered board and firm value.<sup>86</sup>

ASDS briefly observe that CLS performs a “matched sample study.”<sup>87</sup> However, they do not discuss this additional methodology, except for observing in a footnote that results for matched samples “naturally depend on the quality of the match between firms subject to the change in staggered board and those serving as control firms.”<sup>88</sup> For this reason, we employ four different matching procedures in CLS,<sup>89</sup> although ASDS do not discuss whether our matched samples are of sufficient quality, or by what criteria to assess matching quality. In CLS we also show detailed comparisons between treated and control firms, which indicate that differences across these samples are consistently minor, both economically and statistically.<sup>90</sup> This is consistent with the assumption that the reliability of matched samples in the staggered board context depend on the control firms matching treated firms with changing board structures in essential characteristics but for the changes in board structures.<sup>91</sup> Of course, we welcome constructive criticism on the quality of our matching, though in the meantime it is worth highlighting that matching represents a worthwhile strategy to mitigate endogeneity concerns in the staggered board debate.

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<sup>82</sup> *Id.*

<sup>83</sup> *Id.* at 432 tbl.5.

<sup>84</sup> *Id.* at 434.

<sup>85</sup> *Id.*

<sup>86</sup> *Id.* at 435 tbl.7.

<sup>87</sup> Amihud et al., *supra* note 6, at 1484.

<sup>88</sup> *Id.* at 1485 n.32.

<sup>89</sup> See Cremers et al., *Staggered Boards*, *supra* note 5, at 434.

<sup>90</sup> *Id.*

<sup>91</sup> *Id.*

iii. We employ the dynamic generalized method of moments (GMM) estimator<sup>92</sup> proposed by Arellano and Bover<sup>93</sup> and Blundell and Bond.<sup>94</sup> As explained by Wintoki, Linck, and Netter,<sup>95</sup> this methodology estimates a simultaneous system in which firm value, board structure, and other key corporate characteristics are all endogenous and dynamically interrelated. Using a system where we can reject that the instruments are weak and that accounts for unobservable heterogeneity using firm fixed effects, the dynamic GMM results show a positive (negative) relation between adopting (removing) a staggered board and firm value that is strongly statistically significant.<sup>96</sup>

iv. We conduct a long-term event study exploiting plausibly exogenous variation in board structure due to changes in Massachusetts corporate law.<sup>97</sup> In 1990, Massachusetts made staggered boards “quasi-mandatory” by requiring firms incorporated in the state to adopt a staggered board by default and making it difficult to opt out of this requirement.<sup>98</sup> In CLS, we thus compare the value of Massachusetts firms in the few years before and after this legal change in a matched sample of firms, where the control firms are incorporated outside of Massachusetts but have a similar size, are in the same industry, and have the same board structure as the Massachusetts firms. After the legal change, the value of the Massachusetts firms increased more than the value of their control firms.<sup>99</sup> While ASDS mention older studies that use Massachusetts as a quasi-natural experiment or more recent but still unpublished studies, they do not discuss CLS’s long-term event study employing the change in Massachusetts corporate law.<sup>100</sup>

v. To mitigate the endogeneity concerns of the association between staggered boards and firm value, CLS also examine an important economic channel through which a staggered board could be associated with an increase in long-term firm value, i.e., the bonding hypothesis of takeover defenses.<sup>101</sup> Under this hypothesis, a staggered board would provide an efficient commitment device towards the firm-specific investments of a firm’s stakeholders, such as top employees, large customers, suppliers, and strategic alliance partners. Empirically, we find confirmation for the bonding hypothesis of staggered boards by documenting that

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<sup>92</sup> *Id.* at 434-36.

<sup>93</sup> Manuel Arellano & Olympia Bover, *Another Look at the Instrumental Variable Estimation of Error-Component Models*, 68 J. ECONOMETRICS 29 (1995).

<sup>94</sup> Richard Blundell & Stephen Bond, *Initial Conditions and Moment Restrictions in Dynamic Panel Data Models*, 87 J. ECONOMETRICS 115 (1998).

<sup>95</sup> M. Babajide Wintoki, James S. Linck & Jeffrey M. Netter, *Endogeneity and the Dynamics of Internal Corporate Governance*, 105 J. FIN. ECON. 581 (2012).

<sup>96</sup> Cremers et al., *Staggered Boards*, *supra* note 5, at 436 tbl.8.

<sup>97</sup> *Id.* at 436-37.

<sup>98</sup> MASS. GEN. LAWS ch. 156D, § 8.06(b)-(g) (2018).

<sup>99</sup> Cremers et al., *Staggered Boards*, *supra* note 5, at 438 tbl.9.

<sup>100</sup> Amihud et al., *supra* note 6, at 1502-03.

<sup>101</sup> Cremers et al., *Staggered Boards*, *supra* note 5, at 439-42.

the adoption (removal) of a staggered board has a more positive (negative) association with firm value among firms with stronger stakeholder relationships, such as firms with large customers, productive employees, and in strategic alliances.<sup>102</sup> We similarly find that the adoption (removal) of staggered boards has a more positive (negative) association with firm value among firms whose projects require longer-term investments and are likely harder to value by outside investors, such as firms with more investments in innovation and intangibles.<sup>103</sup>

## CONCLUSION

In this Essay, we examine the criticism offered by a recent paper coauthored by ASDS of our prior staggered board studies and, in particular, the 2017 CLS study published in the *Journal of Financial Economics*. Under this criticism, CLS's estimation method (i.e., pooled panel regressions with firm fixed effects) would not consider the changing nature of firm characteristics over time. Further, we also examine the main empirical claim of ASDS, namely that when the appropriate corrections are employed in estimation methods, there is no statistically significant association between staggered boards and firm value.

We show that ASDS's criticism of our finding that the adoption of a staggered board is positively associated with firm value is unwarranted. Correspondingly, their claim that there is no association between staggered boards and firm value is likely to be statistically inaccurate, as we also show that the insignificant findings of ASDS are the result of using a methodology with poor power (i.e., pooled panel  $Q$  regressions with firm fixed effects). In contrast, we show that the result that firm value tends to increase (decrease) after firms adopt (remove) a staggered board is strongly statistically significant even over fairly short time periods when a methodology is used that has relatively strong statistical power (i.e., change in  $Q$  regressions). Lastly, we also show that the estimation methods in CLS have strong size, meaning that these methods would be unlikely to find a statistically significant association between staggered boards and firm value if such association was not, in fact, supported by the data.

We conclude that ASDS does not settle the staggered board debate. Nonetheless, it does contribute to advance that debate by confirming that the weight of the available empirical evidence strongly suggests that the earlier value-decreasing view of staggered boards is unsupported by the data and should thus not inform policymaking.

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<sup>102</sup> *Id.* at 442-43.

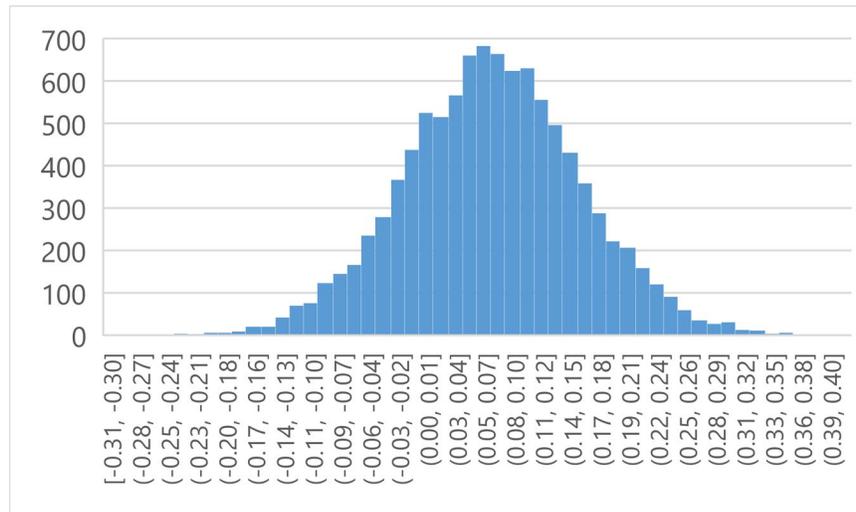
<sup>103</sup> *Id.* at 439-40.

## APPENDIX

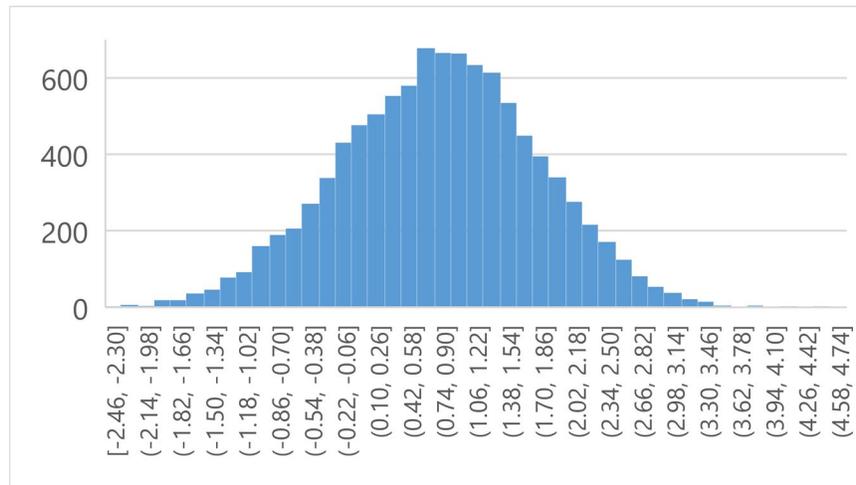
**FIGURE 2: HISTOGRAMS FOR POWER TESTS OF REGRESSIONS  
OF THE LEVEL OF Q**

Figure 2, Panel A presents the histogram of the bootstrapped coefficient of *Staggered Board* in Table 5, Panel A. Figure 2, Panel B presents the histogram of the bootstrapped t-statistics of coefficient of *Staggered Board* in Table 5, Panel B. The histograms are based on the same bootstrap results as reported in Table 5. Each bootstrapped sample is constructed to be very similar to the actual data, including the cross-sectional and time-series correlation structure, as described in the text. We construct a total of 10,000 bootstrapped samples in which there is an association between *Q* and *Staggered Board*, where the data is constructed such that an adoption (dismissal) of a staggered board is association with an increase in *Q* of 0.098.

**Panel A. Histogram of the bootstrapped coefficient of *Staggered Board***



Panel B. Histogram of the bootstrapped t-statistic of the coefficient of *Staggered Board*



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Preferred Citation: K.J. Martijn Cremers, Simone M. Sepe & Saura Masconale, 167 U. PA. L. REV. ONLINE 9 (2019), <http://www.pennlawreview.com/online/167-U-Pa-L-Rev-Online-9.pdf>.