Do Index Funds Monitor? Revisited*

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Abstract

This paper reassesses index investing's impact on corporate governance. After correcting several flaws in the Heath, Macciocchi, Michaely, and Ringgenberg (2022) empirical specification, we find different results. Our analysis reconciles conflicting findings in the literature and casts doubt on the claim that index funds do not monitor companies and that their growth harms firm performance. We also discuss why that paper's other findings cannot be interpreted as evidence that indexers do not monitor. Finally, we provide guidance for future researchers by showing why difference-in-differences specifications can differ from instrumental variable estimations when using Russell index switches for identification.

JEL category: G12, G14, G23, G30, G34

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Despite its growing importance in financial markets, index investing and its impact on corporate governance remains unresolved. Some argue that index investing may weaken corporate governance because index investors might lack the incentives or resources required to monitor firms effectively (e.g., Bebchuk and Hirst, 2019; Gilje, Gormley, and Levit, 2020). Others argue that as significant blockholders, index funds have an incentive to monitor and improve governance (e.g., Fisch, Hamdani, and Solomon, 2018; Kahan and Rock, 2020; Lewellen and Lewellen, 2022). Empirical evidence on this matter is mixed. For example, Heath, Macciocchi, Michaely, and Ringgenberg (2022) (HMMR) claim that increases in index ownership reduce monitoring and harm company performance, while studies by Appel, Gormley, and Keim (2016, 2019, 2024) (AGK) and Gormley, Gupta, Matsa, Mortal, and Yang (2023) provide evidence of the opposite.

It is unclear how one can reconcile these seemingly contradictory findings. Focusing on two of the most cited papers, HMMR and AGK, Corum, Malenko, and Malenko (2023) argue that one possible explanation is that the type of ownership displaced by index ownership matters. HMMR claim to analyze increases in index ownership that replace active mutual fund ownership, while AGK isolate increases in index ownership that replace retail ownership and other types of institutional ownership. However, this explanation is problematic because both HMMR and AGK use similar identification settings and strategies. Specifically, why would one estimation show index ownership replaces active fund ownership while another approach in the same setting shows that index fund ownership replaces retail and other institutional ownership? This paper analyzes another possible explanation: empirical misspecification.

The primary distinction between HMMR and AGK lies in their empirical methodologies. Both studies employ stock assignments to the Russell 1000 and 2000 indexes for identification. Because of the index's relative popularity among index funds, stocks in the Russell 2000 will tend to have more index ownership than otherwise similar Russell 1000 stocks. However, HMMR utilize a difference-in-differences approach, taking advantage of stocks that switch Russell indexes, while AGK employ instrumental variables estimation based on cross-sectional differences in index ownership between Russell 1000 and 2000 stocks. While both methodologies have their advantages, it is unclear why they should yield different findings. This paper provides an explanation and helps shed light on index investors' governance impact.

HMMR purport to employ two separate difference-in-differences estimations to compare changes in outcomes for firms that switch indexes to those that remain in their original index. First, firms that switch from the Russell 1000 to the Russell 2000 (i.e., switchers that experience an increase in index ownership) are supposedly compared to stocks that were close to switching but remain in the Russell 1000 (i.e., stayers that do not experience a change in index ownership). Such a comparison would yield a difference-in-differences estimation: switchers versus stayers and pre- versus post-switch. A similar comparison is also supposedly done for stocks that switch from the Russell 2000 to the Russell 1000, resulting in a second difference-in-differences estimate. To carry out this analysis, HMMR define two dummy variables, $R1000 \rightarrow R2000$ and $R2000 \rightarrow R1000$, which indicate firms that switch from the Russell 1000 to the Russell 2000 and those that switch from the Russell 2000 to the Russell 1000, respectively. They then estimate the coefficients of these two dummy variables in a single estimation that includes all switchers and stayers for both indexes.

However, HMMR's approach is flawed, providing a key explanation for why their findings differ from AGK. By combining the two groups of switchers and stayers into one estimation, their estimates are partly driven by comparing switchers of one index to stayers of the *other* index. This approach does not yield the claimed difference-in-differences estimates, and the control group for each type of switcher includes stocks that are considerably different in their starting characteristics.

To address this issue, one must use a different specification. One approach, used in Coles, Heath, and Ringgenberg (2022), is to run the estimation separately for the two groups: stocks initially in the Russell 1000 (i.e., “lower band”) and those initially in the Russell 2000 (i.e., “upper band”). This approach ensures that switchers are only compared against stayers from the same starting index.

Using the code and data published alongside HMMR, we show how this simple correction yields notable changes. First, index switching no longer effects actively managed fund ownership, a necessary criterion for an increase in index ownership to weaken governance (Corum et al., 2023). This finding is crucial and shows that the Russell setting is not suitable for assessing whether replacing active fund ownership with index ownership is detrimental to corporate governance, which is the question pursued by HMMR. Second, HMMR’s findings for managerial compensation and firm performance are not robust to the minor correction. For example, the negative impacts of index ownership on pay-performance-sensitivity and equity compensation disappear. Moreover, the negative impacts on Tobin’s q, Total q, market-to-book ratio, and ROA no longer hold. These findings underscore the importance of applying a correct methodology when examining the effects of index switching, as results can be sensitive to the modeling choices.

While this initial correction undoes many of HMMR’s published findings and largely reconciles the conflicting findings of HMMR and AGK, it is not the only problem of HMMR’s specification. In the next part of our paper, we discuss other issues and weaknesses of the above difference-in-differences estimation. First, HMMR’s specification fails to account for index status in non-cohort years of their stacked difference-in-differences specification. By including stocks that switch indexes outside the cohort years, their approach will underestimate the true impact of index assignment. To correct this issue, we restrict the sample to observations with a clean pre- and post-period in each cohort. As expected, this correction further alters their findings, leading to a larger observed change in index ownership among index switchers.

Another criticism of HMMR's difference-in-differences specification is that it does not control for the changes in market capitalization that determine a stock's status as a switcher or stayer. By construction, switchers experience different changes in their market capitalization relative to stayers, which casts doubt on the underlying parallel trends assumption that switchers would experience similar outcome trends absent the index switch. To address this concern, we include controls for market capitalization. This correction further weakens HMMR's findings.

Finally, we introduce a combined (and corrected) difference-in-differences methodology researchers can use to estimate the impact of index fund ownership. A weakness of running two separate difference-in-differences is the loss of testing power because each type of index switch is analyzed individually. The approach also forces researchers to compare across subsamples to see if the estimated coefficients on the $R1000 \rightarrow R2000$ and $R2000 \rightarrow R1000$ variables flip in sign. To overcome this weakness, we show how to combine the two difference-in-differences into one specification that simultaneously utilizes variation from both types of switches, thereby maximizing the testing power and making it easier to interpret the resulting point estimates.

We find that this combined (and corrected) difference-in-differences approach continues to show little evidence that index ownership weakens governance or company performance and, importantly, yields results that align closely with the approach of AGK. We also show that an increase in index ownership is now associated with higher, not lower, pay-for-performance sensitivity. This change reflects HMMR's failure to control for the determinant of index switches, a stock's market capitalization. Moreover, we find that HMMR's finding of the negative impact on board independence only holds in a tight window around the switch, where it is less plausibly affected. Changes in board independence typically require more time to manifest.

We also discuss two additional issues with the HMMR's arguments that index fund investors do not monitor. First, HMMR argue that index fund investors' greater likelihood of voting against ISS is evidence that they do not monitor. However, theory suggests that this voting pattern reflects greater monitoring, not less, and extant empirical evidence supports that interpretation (Iliev and Lowry, 2015; Malenko and Malenko 2019, Iliev, Kalodimos, and Lowry, 2021; Malenko, Malenko, and Spatt, 2021). Second, HMMR argue that indexers' decision to file a 13G form instead of a 13D form reflects an absence of monitoring. However, 13G investors can engage in other forms of stewardship, including communication and voting, and studies confirm investors' ability to exert influence through such forms of engagement, including index investors (e.g., Appel, Gormley, and Keim, 2019; Gormley, Gupta, Matsa, Mortal, and Yang, 2023).

Overall, our study contributes to the ongoing debate about the impact of index fund ownership on corporate governance. A combination of existing findings suggests a nuanced shift in governance. For example, Schmidt and Fahlenbrach (2017) argue that index investors are less likely to engage in some costlier forms of corporate governance, while Appel, Gormley, and Keim (2019) show that index investors' growth increases the ability of other investors to engage in those governance activities. Moreover, Appel et al. (2016) and Gormley et al. (2023) find that index providers successfully exert influence via pressure campaigns that target governance changes that are easy to monitor at scale. Similar tactics might also explain why index fund ownership predicts changes in carbon emissions (Azar, Duro, Kadach, and Ormazabal, 2021). More broadly, Brav, Jiang, Li, and Pinnington (2024) show that index funds actively monitor their portfolio firms and do not blindly follow proxy advisors' recommendations, particularly in high-stakes voting events. Our findings add to this body of work by challenging recent contradictory evidence that claims to show index ownership negatively impacts overall governance and performance. When appropriate empirical methodologies are employed, these seemingly conflicting findings disappear, suggesting that index funds do indeed play a more active role in governance than recent studies argue.

Our study also makes a methodological contribution to the literature on index fund ownership and corporate governance. Several studies have employed various methodologies to exploit the Russell's annual index reconstitution.[1] In our study, we address a new methodological approach that is causing confusion in that literature. By refining the empirical approach and showing why the methodologies differ, we provide a clear empirical path forward for future research on this important topic. We also show that the Russell index setting is not suitable for testing whether a replacement of active fund ownership with index ownership would weaken governance. Our work also builds on the debate sparked by studies such as Appel, Gormley, and Keim (2024), Wei and Young (2024), and Glossner (2024), which highlight a different methodological problem within this literature. Those papers highlight the importance of accounting for Russell's endogenous sorting of stocks within index. By identifying and correcting new methodological problems, we provide additional clarity regarding the role of index funds in corporate governance and why some studies reach different conclusions.

1. Discussion of the Heath et al. (2022) Empirical Specification

In this section, we discuss several problems with HMMR's empirical specification and propose corrections. We also discuss the data and sample construction we use.

1.1. Heath et al. (2022) Difference-in-differences Approach

[1] The list includes, for example, Mullins (2014), Boone and White (2015), Chang, Hong, and Liskovich (2015), Appel, Gormley, and Keim (2016, 2019), Bird and Karolyi (2016, 2019), Crane, Michenaud, and Weston (2016), Khan, Srinivasan, and Tan (2017), Schmidt and Fahlenbrach (2017), Baghdadi, Bhatti, Nguyen, and Podolski (2018), Ben-David, Franzoni, and Moussawi (2018), Lin, Mao, and Wang (2018), Cao, Gustafson, and Velthuis (2019), Chen, Huang, Li, and Shevlin (2019), Chen, Dong, and Lin (2020), Coles, Heath, and Ringgenberg (2022), Heath, Macciocchi, Michaely, Ringgenberg (2022), and Chung and Kim (2023), among many others. Appel, Gormley, and Keim (2024) provide a comprehensive summary of the methodological differences across these studies.

While prior studies examine index ownership effects by exploiting cross-sectional differences in index ownership among stocks near the Russell 1000/2000 index threshold, HMMR's approach is distinct in that they analyze index switching across the Russell indexes on a yearly basis. Specifically, HMMR construct a sample each year (which they refer to as a "cohort") that consists of two distinct groups of firms, denoted as the "lower band" and "upper band". To switch from the Russell 1000 to the Russell 2000 indexes (and vice versa), a firm's market capitalization must fall (rise) below (above) the Russell 1000/2000 cutoff by more than 2.5% of the Russell 3000E index cumulative market capitalization. HMMR's lower (upper) band refers to all stocks within +/- 100 ranks around the -2.5% (+2.5%) threshold. Focusing on this sample of stocks that begin near a switching threshold, HMMR's identification strategy involves comparing stocks that switch to the Russell 2000 (Russell 1000), which they refer to as "switchers", to those that remain in the Russell 1000 (Russell 2000), which they refer to as "stayers".

Specifically, HMMR estimate the following model [see Equation (1) from HMMR]:

$$\begin{aligned} Y_{jct} = {} & \beta_1 (R1000 \rightarrow R2000)_{jc} \times PostAssignment_{ct} \\ & + \beta_2 (R2000 \rightarrow R1000)_{jc} \times PostAssignment_{ct} + \phi_{jc} + \psi_t + \epsilon_{jct} \end{aligned} \tag{1}$$

where j indexes firms, c indexes cohort years, and t indexes years. Y_{jct} denotes outcome variables. The indicator variable $(R1000 \rightarrow R2000)_{jc}$ denotes whether stock j switches from the Russell 1000 index to Russell 2000 index in cohort year c, while $(R2000 \rightarrow R1000)_{jc}$ is defined similarly. $PostAssignment_{ct}$ is an indicator variable that takes the value of one for the three years following cohort year c and zero otherwise. ϕ_{jc} and ψ_t are firm-by-cohort and year fixed effects, respectively. For each cohort, HMMR include three years of pre- and post-switch observations. By constructing observation cohorts for each sample year and combining them into one stacked dataset and estimation, the HMMR estimation shares similarity to a stacked difference-in-differences.

HMMR claim this estimation compares switchers to non-switchers for stocks that start in the same index. In other words, Russell 1000 stocks that switch to the Russell 2000 are supposedly compared against other Russell 1000 stocks that started near the same threshold but did not switch. And vice versa, Russell 2000 stocks that switch to the Russell 1000 are supposedly compared against other Russell 2000 stocks near the same threshold that do not switch. Specifically, they claim to simultaneously estimate two separate stacked difference-in-differences, where switchers are compared against non-switchers of the same index in a pre- versus post-switch comparison.

1.2. The Use of Problematic Comparisons

Unfortunately, HMMR's estimation is problematic and does not do a proper difference-in-differences comparison for either set of index switchers. Because the estimation codes $(R1000 \rightarrow R2000)_{jc}$ as zero for stocks that were initially in the Russell 2000 and $(R2000 \rightarrow R1000)_{jc}$ as zero for stocks that were initially in the Russell 1000, each group of switchers is compared against *two* sets of non-switchers: those remaining in the same index and those remaining in the *other* index. Such comparisons are problematic as non-switchers from the other index are significantly different in their market capitalization, thus casting doubt on whether they make a valid control group.

To implement a stacked difference-in-differences estimation for each subgroup of firms correctly, one can create two separate samples based on a stock's starting index and estimate two distinct models. To analyze the impact of switching to the Russell 2000 index, one can restrict their sample of threshold stocks to those that begin in the Russell 1000 index and estimate

$$Y_{jct} = \gamma(R1000 \rightarrow R2000)_{jc} \times PostAssignment_{ct} + \phi_{jc} + \psi_{ct} + \epsilon_{jct}. \tag{2}$$

To analyze the impact of switching to the Russell 1000 index, one would instead restrict their sample of threshold stocks to those that begin in the Russell 2000 index and estimate

$$Y_{jct} = \delta(R2000 \rightarrow R1000)_{jc} \times PostAssignment_{ct} + \phi_{jc} + \psi_{ct} + \epsilon_{jct}. \tag{3}$$

The above approach is nearly the same as that of Coles, Heath, and Ringgenberg (2022) [CHR].[2]

1.3. Additional Problems and Needed Corrections

Unfortunately, even the above approach is problematic in several ways. First, the sample construction for each estimation ignores subsequent index assignments, which will lead each difference-in-differences estimation to understate the true impact of index assignment. Second, the estimations fail to control for the endogenous variable that drives index switching. Third, the use of two separate estimations reduces statistical power and requires the researcher to compare point estimates across specifications. We now discuss these problems and how they can be addressed.

1.3.1. Failure to Account for Index Status in Non-Cohort Years

While separately estimating Equations (2) and (3) ensures a proper control group for each difference-in-differences estimation, it does not address a second issue with how HMMR select their sample. For each cohort, HMMR create a sample using a six-year window (three years before the switch and three years after the switch). However, their sample selection does not drop a stock's observations within that window if it switches indexes in a non-cohort year. For example, a stock that switches from the Russell 1000 to the Russell 2000 would have all three post-switch observations in the sample even if it switched back a year later. The same issue applies for non-switchers. A non-switcher that subsequently switches would remain in the sample. The failure to account for observations' index status in other years will cause the estimation to understate the true index assignment effect. Some switchers revert to non-switchers, and vice versa, but the estimation implicitly assumes that stock's index remains the same in each post-cohort year.

[2] Nearly, but not exactly. Coles, Heath, and Ringgenberg (2022) only include year fixed effects, while we include cohort-year fixed effects, which is the more standard (and robust) way to estimate a stacked difference-in-differences (see Gormley and Matsa, 2011 and 2016). In unreported tests, we find that the choice between year and year-cohort fixed effects has little impact on the resulting estimates.

To correct this problem, one must construct the sample for each cohort to only keep observations where there is a clean pre- vs. post-period. For instance, consider a stock that moves from the Russell 1000 to the Russell 2000 in cohort year 0. If that stock moves back to the Russell 1000 in year 2, one must drop its year 2 and later observations from that cohort. Otherwise, one is incorrectly coding it in years 2 and later as being part of the Russell 2000. Likewise, if it was in the Russell 2000 in years -2 and earlier, then one must drop years -2 and earlier from that cohort. In other words, if a stock moved from the Russell 2000 to the Russell 1000 in year -2 then back to the Russell 2000 in year 0 and then back to the Russell 1000 in year 2, the only observations one should keep for that stock in that cohort year 0 sample are -1, 0, and 1. The other observations are not valid. One must use the same approach when selecting observations for the non-switchers.

1.3.2. Failure to Control for the Variable that Drives Index Switching

The estimations in Equations (2)-(3) suffer an additional, fundamental weakness. Switchers and non-switchers exhibit different changes in their total market capitalization. These differing changes in market capitalization are what determine whether a stock switches indexes. However, changes in market capitalization likely correlate with changes in other company outcomes, including corporate governance, casting doubt on the underlying parallel trends assumption of the difference-in-differences estimation. I.e., are the differential trends observed for switchers and non-switchers driven by the index switch (and corresponding change in index ownership) *or* the corresponding differential change in stock market capitalization that drove the index switch?

To account for this additional weakness of the HMMR specification, we will add a control for a stock's total market capitalization in year *t*. Specifically, to mirror the approach of other papers that use the Russell setting for identification, we include a control for $Ln(Mktcap_{jt})$, which is the natural log of the stock *j*'s market capitalization in May of year *t*.

1.3.3. Subsample Comparisons and Reduced Statistical Power

Another weakness of the separate estimations of Equations (2) and (3) is that it estimates the impact of index switching in two separate samples, which reduces statistical power and forces researchers to compare across subsamples to see if the coefficients flip sign as expected. To overcome this weakness, one can pool the two samples and estimate the following combined (and corrected) stacked difference-in-differences specification:

$$Y_{jct} = \theta\{R2000_{post} - R2000_{pre}\}_{jc} \times PostAssignment_{ct}$$

$$+ \zeta Ln(Mktcap_{jt}) + \xi_{jcr} + \omega_{ctr} + \epsilon_{jct}, \qquad (4)$$

where *r* indexes a stock's Russell index assignment (Russell 2000 or Russell 1000) in the year before the cohort year c. $R2000_{post}$ is an indicator variable equal to one if stock j was in the Russell 2000 index in cohort year c. $R2000_{pre}$ is an indicator variable equal to one if firm j was in the Russell 2000 index in the year prior to cohort year c. Accordingly, $\{R2000_{post} - R2000_{pre}\}$ becomes a ternary variable equal to one if stock j switches indexes from the Russell 1000 to the Russell 2000 in cohort year c, zero if stock j stays in the same index before and after the index reconstitution in cohort year c, and *negative one* if stock j switches indexes from the Russell 2000 to the Russell 1000 in cohort year c. ξ_{jcr} denotes stock-by-cohort-by-pre-cohort-assignment Russell index fixed effects, and ω_{ctr} denotes cohort-by-year-by-pre-cohort-assignment Russell index fixed effects. The inclusion of the extra interaction in the fixed effects here is crucial. They create one set of fixed effects for stocks that start off in the Russell 2000 and another set for stocks that start off in the Russell 1000. This ensures that we are still only comparing stocks that started off in the same index against each other in the two separate difference-in-differences.

The above specification estimates both difference-in-differences simultaneously under the assumption that the effect of index assignment is the same for both sets of switchers. In other words, the specification assumes the effect of moving from the Russell 1000 to the Russell 2000 is the reverse of the effect of moving from the Russell 2000 to the Russell 1000. This assumption seems plausible given the underlying theories on why index assignment matters.

1.4. The Appel et al. (2019) Specification

For the sake of comparison, we also estimate a modified first stage of Appel et al. (2019)'s instrumental variable (IV) specification. The first stage of AGK's IV estimation is

$$Y_{jt} = \alpha + \eta_1 R2000_{jt} + \sum_{n=1}^{3} \zeta_n Ln\left(Mktcap_{jt}\right)^n + \rho Ln\left(Float_{jt}\right) \tag{5}$$

$$+\mu_1 Band_{jt} + \mu_2 R2000_{jt-1} + \mu_3 Band_{jt} \times R2000_{jt-1} + \tau_t + \varepsilon_{jt},$$

where *R*2000 is an indicator for whether the stock is in the Russell 2000 index in reconstitution year t, *Mktcap* is the end-of-May CRSP market cap, and *Float* is Russell's float-adjusted market cap. The indicator variable *Band* denotes a firm being "banded" by Russell in reconstitution year *t*, thereby not switching indexes because its distance from Russell 1000/2000 threshold is smaller than 2.5% of the total market cap of the Russell 3000E index. τ_t denote year fixed effects.

The AGK (2019) approach differs from the difference-in-differences estimation of Equation (4) in several important ways. First, AGK select their sample differently. Rather than restrict the sample to stocks near each switching cutoff, they restrict their sample to the 500 stocks at the bottom of the Russell 1000 and top 500 stocks of the Russell 2000. They also do not construct cohorts that limit the sample to three pre- and post-switch years. Second, their specification controls for firms' float-adjusted market capitalization. They control for *Float* because of how Russell weighs stocks within each index, which could cause a correlation between *Float* and *R2000* [see AGK (2024) for more details]. Third, they add three additional controls to account for the factors that determine index assignment after 2007. These additional controls are: 1) an indicator for having an end-of-May CRSP market capitalization that ensures firm *j* will be banded by Russell,

$band_{jt}$; (2) an indicator for being in the Russell 2000 in the last reconstitution year t–1, $R2000_{jt-1}$; and (3) the interaction of these two indicators. These three additional controls capture the additional criteria used by Russell beginning in 2007 when determining each firm's index assignment. The difference-in-differences estimation of Equation (4) does not require these additional *Float* and banding controls because of how it selects its sample and because of its fixed effects that control for a stock's initial index. Fourth, AGK use a more robust control for a stock's *Mktcap*. Specifically, they use a third-degree polynomial control for *Mktcap*.

However, the main distinction between the AGK specification and the difference-in-differences estimation of Equation (4) is the type of variation used to estimate the importance of index assignment. Specifically, the AGK estimation is a cross-sectional comparison. The outcomes of stocks in one index are compared against the outcomes of stocks in the other index. Unlike the difference-in-differences specification, the baseline AGK does not isolate and only use variation in index assignment coming from index switchers.

To isolate variation from index switchers and make the AGK estimation more comparable to the difference-in-differences estimation, one can augment the AGK estimation. As noted in AGK (2024), one simply adds stock fixed effects and estimates

$$Y_{jt} = \alpha + \eta_1 R2000_{jt} + \sum_{n=1}^{3} \zeta_n Ln\left(Mktcap_{jt}\right)^n + \rho Ln\left(Float_{jt}\right) \qquad (6)$$
$$+\mu_1 Band_{jt} + \mu_2 R2000_{jt-1} + \mu_3 Band_{jt} \times R2000_{jt-1} + \iota_j + \tau_t + \varepsilon_{jt},$$

where ι_j denotes stock fixed effects. The addition of stock fixed effects ensures that the identification of η_1 comes from within-stock variation in index assignment, $R2000_{jt}$.

2. Data, Sample Comparison, and Replication of HMMR

For our analysis we use two sample datasets. First, we download HMMR's data posted on the *Review of Financial Studies* Dataverse. This dataset allows us to show how some of our specification corrections change HMMR's point estimates in meaningful ways using their own data and posted code. Second, we construct our own version of the HMMR sample using the exact same databases they utilize, such as Russell, CRSP Security Files, CRSP Mutual Fund Database, Compustat, Thomson Reuters S12, ISS Voting, Execucomp, ISS Governance, and BoardEx.

The second dataset is necessary because some of our specification changes require variables that are not included in HMMR's posted data. Following the sampling procedure described by HMMR, we obtain a sample of 4,381 stock-year observations. Table 1 presents summary statistics for this second dataset. Overall, the summary statistics of our constructed sample closely mirror those of HMMR's sample across all variables.

<Table 1 About Here>

To further confirm the similarity of the two datasets, we replicate a few key figures in HMMR. First, we replicate HMMR's Figure 2, Panel B. This figure analyzes their 2007 cohort sample and plots each stock's index assignment and switching status as a function of the stock's market capitalization ranking that year. We find a similar pattern as HMMR. See Figure 1. Next, we replicate HMMR's Figure 5, which plots time-series trends in index fund ownership for stocks staying in the initial index and those that switch indexes. The graphs do not reveal any notable differences between the HMMR sample and our own. See Figure 2.

<Figure 1 About Here>

<Figure 2 About Here>

Our constructed dataset also closely replicates HMMR's main findings. To illustrate the similarity, we estimate Equation (1) using both HMMR's sample and our own sample using HMMR's Section 3.1 outcomes. Table 2 reports the findings. The findings are similar across the two datasets, both qualitatively and quantitatively. For example, using the HMMR dataset, the

Russell 1000 to Russell 2000 switch predicts a 1.31 percentage point increase in the stock's index fund ownership and a 2.21 percentage point decrease in its active fund ownership (Table 2, Columns 1-2; p-values < 0.01 and 0.05, respectively). The estimates are similar in magnitude and statistical significance in our dataset. Switching from the Russell 1000 to the Russell 2000 predicts a 1.24 percentage point increase in index fund ownership and a 2.27 percentage point decrease in active fund ownership (Columns 3-4; p-values < 0.01 and 0.05, respectively). The similarity provides confidence that our constructed dataset closely mirrors HMMR's dataset.[3]

<Table 2 About Here>

3. Findings After Correcting HMMR's Specification

In this section, we analyze, step-by-step, how the HMMR findings change with corrections to their specification. Additionally, we compare these results with those from the AGK model.

3.1. Avoiding Problematic Comparisons Matters (A Lot)

We first investigate the importance of avoiding problematic comparison groups. Specifically, we use HMMR's own data to estimate Equations (2) and (3). The estimation of Equation (2) uses stocks in the lower band (i.e., those in Russell 1000 during the pre-cohort years), while the estimation of Equation (3) uses stocks in the upper band (i.e., those in Russell 2000 during the pre-cohort years), respectively. While a seemingly minor change from the HMMR approach of Equation (1), this splitting of the sample avoids the problematic comparisons of switchers from one index to the non-switchers of the other index. Simply put, it is the standard (and correct) way to estimate each individual difference-in-differences. For ease of comparison, Table 3, Panel A, presents the original findings of HMMR, as obtained when using their posted

[3] While our point estimates in Table 2, Columns 1-2, are identical to those reported in HMMR (2022)'s Table 3, Columns 3-4, our standard errors are slightly different. It is unclear why this discrepancy exists as we are using HMMR's posted code and data to estimate Table 2, Columns 1-2.

data and code [which estimates Equation (1)]. Table 3, Panel B, presents the findings when using their posted data but instead using the corrected estimation [i.e., Equations (2)-(3)].

Problematic comparisons drive many of the HMMR's published findings. Most of their reported coefficients (Table 3, Panel A) change meaningfully and are no longer statistically significant after making this seemingly minor correction (Panel B).

<Table 3 About Here>

To be clear, avoiding problematic comparisons has little impact on HMMR's finding for index ownership. This finding is not surprising as many other papers using other methodologies have found similar differences in index ownership across the two Russell indexes. Using the corrected difference-in-differences specification, index ownership increases by 1.17 percentage points for stocks that switch to the Russell 2000 and decreases by 1 percentage point for stocks that switch to the Russell 1000 (Table 3, Panel B, Column 1). These findings are similar to those of the combined specification employed by HMMR (Panel A, Column 1).

However, avoiding problematic comparisons considerably effects HMMR's key finding for active ownership. Following this correction, little evidence exists that index switching affects active ownership (Column 2). The point estimates for active ownership (Panel B) are half the magnitude of those reported in HMMR (Panel A) and no longer statistically significant. This finding is particularly important in the context of Corum et al. (2023), which argues that reduced monitoring for stocks with higher index ownership is only likely to occur if increased index ownership reduces active ownership. The non-finding also mirrors earlier non-findings in the literature for the impact on active holdings (e.g., Appel, Gormley, and Keim, 2016, 2019).

Moreover, many other HMMR findings are not robust to the use of a proper control group. Using the corrected specification, we find little evidence that index assignment matters for CEOs' pay-for-performance sensitivity (PPS) or share of compensation received as equity (Columns 3-4). Nor do we find evidence that index assignment matters for various measures of performance, including Tobin's Q, Total Q, market-to-book ratio, or return on assets (ROA) (Columns 6-9). While the corrected specification does show similar point estimates for board independence (Column 5), there continues to be a lack of symmetry in the coefficients for the two types of index switches, which suggest this result is also not particularly robust.

3.2. Importance of Additional Corrections to the HMMR Specification

We next show the importance of making additional corrections and improvements to the HMMR specification. First, we analyze the importance of accounting for index status in non-cohort years. Second, we analyze the importance of controlling for the endogenous factor that drives index switching, a stock's market capitalization.

Because these additional tests require variables not included in the posted HMMR data, we now switch to using our constructed dataset. For brevity, we will also focus these (and subsequent) tests on just two of HMMR's main outcomes: index ownership and active ownership. As a baseline, we see that the findings when estimating Equations (2)-(3) for these two outcomes are similar in the HMMR sample and our own constructed sample. Table 4, Columns 1-2, show this similarity. Index switching associates with about a 1 percentage point change in index ownership in both samples (Table 4, Panel A, Columns 1-2), but there is little evidence that index switching predicts a similar change in active ownership (Panel B, Columns 1-2).

<Table 4 About Here>

With this similarity across samples established, we investigate the importance of accounting for a stock's index status in non-cohort years. We accomplish this by tossing observations affected by an index switch that occurs in a non-cohort year. As noted in Section

1.3.1, a failure to account for index switches in non-cohort years will cause the estimation to understate the true impact of index switches. This occurs because the HMMR estimation assumes that each stock's index assignment remains the same within the pre-cohort years and within the post-cohort years, but the sampling approach of HMMR fails to ensure that this assumption is true. Our sampling approach corrects this error. Table 4, Column 3 reports the findings.

As expected, the correction increases the magnitude of the observed increase in index ownership. The increase in index ownership is 1.34 percentage points for stocks switching into the Russell 2000 index (Table 4, Panel A, Column 3; p-value < 0.01), which is more than 70 percent larger than the observed increase when one does not account for index assignment in other years (Column 2). The difference highlights how HMMR's sampling approach understates the true effect of index switching on a stock's level of index ownership. This correction, however, has little impact on the non-finding for active ownership. We continue to find little evidence that index assignment associates with active ownership (Panel B, Column 3).

We next assess the importance of controlling for the factor that determines index switching, stock market capitalization. As noted in Section 1.3.2, a failure to control for market capitalization could lead to violations of the parallel trends assumption and an omitted variable bias if market capitalization affects the outcome of interest. Table 4, Column 4 reports these findings.

The importance of controlling for total market capitalization differs for the two outcomes, index and active ownership. The estimates for index ownership are largely unchanged, suggesting that changes in market capitalization have little impact on index ownership beyond their importance for index assignment. However, the evidence for a potential impact on active ownership becomes even weaker. Rather than seeing opposing (but statistically insignificant) shifts in active ownership (Panel B, Column 3), we see that index switching is associated with a suggestive drop in active ownership for both types of switches (Panel B, Column 4).

3.3. The Combined (and Corrected) Difference-in-Differences

With these corrections in place, we next estimate the combined (and corrected) specification in Equation (4) that simultaneously looks at both difference-in-differences. As noted in Section 1.3.3, one can increase statistical power and avoid subsample comparisons by analyzing the two sources of index switching variation in one specification that utilizes the symmetry in expected impacts. Table 5, Column 1 presents the results.

The combined (and corrected) specification confirms the earlier findings. The point estimate on $\{R2000_{post} - R2000_{pre}\} \times PostAssignment_t$ shows that index ownership is, on average, about 1.36 percentage points higher (lower) after stocks switch into the Russell 2000 (1000) index (Table 5, Panel A, Column 1). As one would expect, the findings mirror those in subsample estimates (Table 4, Panel A, Column 4). For the active ownership, we continue to find little evidence that index switching affects active ownership. The point estimate, -0.17 percentage points, is both economically small and statistically insignificant. The lack of a shift in active fund ownership confirms earlier findings that the shift in index ownership comes at the expense of other types of ownership, including retail ownership (e.g., Appel, Gormley, and Keim, 2016, 2019).

3.4. AGK's Specification versus the Combined (and Corrected) Difference-in-Differences

With these corrections in place, the combined (and corrected) difference-in-differences yields similar results to the AGK specification that includes stock-level fixed effects [i.e., Equation (6)]. Table 5, Column 2 reports these findings. Switching to the AGK specification, we find a nearly identical 1.35 percentage point shift in index ownership for stocks that switch indexes (Panel A, Column 2). However, there is a slight increase in the estimate precision, likely because AGK's estimation includes a float-adjusted market cap control and more robust size controls. We continue to find little evidence of a change in active ownership (Panel B, Column 2).

Switching to AGK's sampling approach has little impact. This similarity is seen by comparing Columns 2 and 3. Column 2 uses the HMMR sampling technique, which only includes stocks that are within +/- 100 ranks around the cutoff thresholds each year. Column 3 instead uses the sampling approach of AGK, which includes the bottom (top) 500 stocks of the Russell 1000 (2000) index each year. As one might expect, estimate precision increases when using the larger AGK sample, but the findings are otherwise similar. Index ownership is about 1.3 percentage points higher for stocks in the Russell 2000, and there is no impact on active ownership.

<Table 5 About Here>

Overall, the findings in this section show that HMMR's estimates pertaining to the effects of index switching on active fund ownership become weaker both statistically and economically once their specification is corrected. The suggestive drop in active ownership in response to index switching from the Russell 1000 to the Russell 2000 (and vice versa) no longer holds with the corrected specification. The AGK specification yields similar non-finding for active ownership. Overall, both estimation approaches show that the Russell setting is not suitable for assessing whether a replacement of active fund ownership by index ownership weakens corporate governance. Russell identification strategies instead isolate differences in index ownership that come at the expense of other types of ownership, including retail ownership.

4. Revisiting HMMR's Other Findings

Having established the similarity of the combined (and corrected) difference-in-differences estimates to those of the AGK specification, we now revisit the other findings of HMMR. While we know many of HMMR's findings are not robust in their own sample once one stops using problematic controls (see Section 3.1), it remains possible that some of HMMR's findings return after making additional corrections to their difference-in-differences estimation. To analyze this possibility, we now re-estimate their main findings using both the combined (and corrected) difference-in-differences and AGK specification. For robustness, we also estimate the AGK specification using both the sampling approach of HMMR and that of AGK.

4.1. Management and Shareholder Proposals

We first reassess HMMR's analysis of whether index switching associates with changes in the composition of management and shareholder proposals or their likelihood of passage. We analyze the same six outcomes analyzed in HMMR's Table 8, Panel B. Specifically, we analyze the number and fraction of contentious proposals and the share of such proposals that pass. Contentious proposals are those where the ISS vote recommendation differs from that of management. Table 6 presents the results. Panel A reports the results of the combined (and corrected) difference-in-differences specification, while panels B and C report those of the AGK specification using the HMMR's two-band sample and the AGK's larger sample, respectively.

<Table 6 About Here>

The results here confirm HMMR's finding of little change in share of contentious proposals or fraction passed when looking at index switchers. While there is some evidence of a decline in the number of contentious management proposals when using the difference-in-differences estimation (Table 6, Panel A, Column 1), it is not robust to using the AGK specification (Panels B and C). It is also worth noting that HMMR's flawed specification shows evidence of a decline in the fraction of passed shareholder proposals [see Table 8, Panel B, Column 6 of their paper]. They argue that finding is inconsistent with behind-the-scenes engagement by index investors. However, the corrected specifications show no such finding (Table 6, Column 6). [4]

[4] It is worth noting that HMMR's evidence on this dimension was never robust to begin with. They find weak evidence

4.2. Index Switching and Managerial Incentives

HMMR argue that index fund ownership reduces managers' pay-for-performance sensitivity and the equity fraction of executive compensation. HMMR interpret these two findings as evidence of less monitoring by index funds. We now revisit their managerial compensation and incentive findings and investigate what happens when employing the corrected specifications.

Following HMMR, we examine five compensation- and incentive-related outcomes: pay-for-performance sensitivity (PPS), total executive compensation, the equity fraction of total compensation, a dummy variable of whether a golden parachute is included in the CEO's compensation package, and a dummy variable of whether the CEO departs the firm in the given year. Table 7, Panel A reports the findings of the combined (and corrected) difference-in-differences specification, while Panels B and C present the estimation results of the AGK specification on the HMMR's two-band sample and the AGK's larger sample, respectively.

<Table 7 About Here>

In contrast to HMMR, we find no evidence that increased index ownership lowers managers' pay-for-performance sensitivity or the share of pay from equity. See Table 7, Columns 1 and 3. If anything, the effect on PPS is *positive* (see Column 1, Panel B). In fact, the only evidence that mirrors HMMR's original findings is an increase in total compensation (Column 2). However, that increase in compensation does not necessarily reflect reduced investor monitoring.

The sharp difference in our findings on pay-for-performance sensitivity is driven by the inclusion of size controls. It has been well documented in the literature that firm size is a key determinant of managerial pay (e.g., Jensen and Murphy, 1990; Hall and Liebman, 1998; Gabaix

of a decline in passed proposals only when looking at switches from the Russell 1000 to the Russell 2000. They find no evidence of a symmetric change in fraction passed for stocks switching in the other direction. Instead, the coefficient goes in the wrong direction. Unless there is a clear rationale as why there would be asymmetric impacts, a combined analysis will show that these are not robust.

and Landier, 2008; Frydman and Saks, 2010, among many others). However, HMMR's specification fails to control for the changes in firm size that determine index switching. Table 8 illustrates the importance of this failure. For comparison, Table 8, Column 1 reports the combined (and corrected) difference-in-differences specification with size controls (as already reported in Table 7, Panel A, Column 1). If one drops the size controls, one recovers HMMR's finding that index ownership is associated with a large drop in pay-for-performance sensitivity (Column 2). The huge difference in findings highlights why it is important to control for size in estimating the effects of index switching in the Russell setting, especially for managerial incentives.

<Table 8 About Here>

Overall, there is little evidence that index ownership changes managerial turnover or compensation in the sample period analyzed by HMMR (2004-2018). In other words, an analysis of managerial incentives does not support HMMR's claim that index ownership reduces investor monitoring. The findings also highlight how a failure to control for the determinant of index switches, a stock's market capitalization, can yield misleading inferences.

4.3. Index Switching and Corporate Governance

We next turn to governance impact of index ownership. HMMR argue that an increase in index ownership leads to less board independence but has no impact on other measures of governance, including the adoption of poison pills, supermajority voting requirements, limitation on shareholders' ability to call a special meeting or to act by written consent, and dual class shares. They interpret the reduction in board independence as evidence of less monitoring by index funds. We reexamine the index ownership-governance relationship using the same governance outcome variables that HMMR analyze. Table 9 presents the results.

<Table 9 About Here>

The key takeaway from Table 9 is that there is only weak evidence of a drop in board independence and little evidence for other governance outcomes. The magnitude of the drop in board independence becomes smaller and less statistically significant when the difference-in-differences specification is fully corrected. Moreover, the AGK specification with firm fixed effects does not consistently show a drop in board independence (Table 9, Column 1).

A comparison of Panels B and C also shows the potential importance of how one constructs the sample. For example, the board independence coefficient changes sign when using AGK's sampling approach (see Table 9, Panels B to C, Column 1). The AGK sampling approach allows for a longer panel because it does not restrict the analysis to three years before and after an index switch. The longer panel could be important for slow moving outcomes, like board independence.

The potential importance of sampling and specification choices for slow-moving governance outcomes becomes more evident when we drop firm fixed effects from the AGK specification. Table 10, Panel A, which estimates the cross-sectional AGK specification in Equation (5) for these same outcomes, illustrates this finding.

The governance findings are considerably different when one excludes firm fixed effects. Specifically, one now finds a positive association between Russell 2000 inclusion and board independence that mirrors the findings of Appel, Gormley, and Keim (2016), which analyzed an earlier sample period, 1998-2006. Like Appel, Gormley, and Keim, one also finds a negative association with the likelihood of dual class shares and limits on shareholders' ability to call a special meeting or to act by written consent. Interestingly, one also finds a positive association between Russell 2000 index inclusion and supermajority voting requirements.

The difference in findings could reflect the slow-moving nature of these outcomes. Both the HMMR specification and the AGK specification with stock-level fixed effects implicitly assume an immediate impact of index switching on the outcome being analyzed. However, it is unclear whether we should expect a change in index ownership to immediately affect governance outcomes, like board independence. For such slow-moving outcomes, the cross-sectional approach without stock fixed effects might be better suited to capture the actual effect of index ownership. This possibility is similar to the argument of McKinnish (2008) that fixed effect estimations can lead researchers to make incorrect inferences when the dependent variable of interest responds to sustained rather than transitory changes in the independent variable.[5]

In a similar spirit, Table 10, Panel B reports the estimation results of the AGK specification without firm fixed effects for the proposal and voting outcomes analyzed in Section 4.1 and Table 6. If one excludes stock-level fixed effects, one finds that Russell 2000 inclusion (and higher index ownership) predicts fewer contentious management proposals and more contentious shareholder proposals (Table 10, Panel B, Columns 1-2 & 4-5). These findings could be additional evidence that the impact of indexers on some outcomes, especially those related to governance, is not immediate and requires sustained differences in stock ownership.

<Table 10 About Here>

However, these findings do not necessarily suggest that the cross-sectional version of the AGK specification is superior to a specification that isolates variation from index switching. The use of within-stock variation and index switches reduces the risk of omitted variables. Appel, Gormley, and Keim (2024) discuss this potential tradeoff in more detail.

Overall, our findings confirm that researchers should choose a specification and a sampling method carefully, depending on the nature of the outcome variables.

[5] HMMR's board independence finding was not conceptually robust for another reason. They document a decrease in board independence for stocks switching from the Russell 1000 to the Russell 2000, but they never find a corresponding increase in independence for stocks jumping from the Russell 2000 to the Russell 1000. This asymmetry is problematic because they never justify why the impact of index assignment would be asymmetric.

4.4. Index Switching and Firm Value

Finally, we revisit HMMR's findings regarding firm value and performance. If greater index ownership leads to weaker governance and less monitoring by investors, one might observe decreases in firm performance and value. To assess this possibility, we follow HMMR and examine the outcomes in Table 10 of their paper: Tobin's q, Peters and Taylor (2017)'s total q, market-to-book ratio, and return on assets (ROA). The results are presented in Table 11.

The message is clear: there is no impact on firm value or performance once a proper specification is used. Neither the combined (and corrected) difference-in-differences specification nor the AGK specification show an impact on measures of firm value and performance (see Table 11). The non-finding for firm value and performance reinforces the argument that HMMR's findings are not robust. The non-impact on performance also undercuts their argument that increased index ownership is associated with evidence of less monitoring by investors.

<Table 11 About Here>

The non-findings, however, do not necessarily mean that HMMR's argument is wrong. HMMR interpret their findings as the consequence of index funds replacing active funds. If index funds were replacing active funds, monitoring intensity could become weaker, which could lead to less incentives, weaker governance, and lower firm value (Corum et al, 2023). However, the Russell index setting does not provide such variation. Russell index inclusion changes the level of index fund ownership, but it does not change the level of active fund ownership.

5. Additional Interpretation Issues with HMMR

While HMMR's findings that utilize Russell index switches are not robust, HMMR present two additional pieces of evidence to support their claim that index investors do not monitor and that their increased presence weakens corporate governance. First, index fund investors are less likely to vote against management in contentious governance proposals. Second, index fund investors are more likely to file a 13G form than a 13D form.

However, the interpretation for both pieces of evidence is inconclusive. Neither finding precludes the possibility that index investors monitor firms and improve governance.

First, siding with management on contentious shareholder proposals is not evidence of an absence of monitoring. If anything, the finding suggests the opposite. Contentious proposals are those where management and Institutional Shareholder Services (ISS) give different vote recommendations. Therefore, voting with management on such proposals, as HMMR find, means the index fund investors are less likely to follow ISS vote recommendations. However, the literature typically interprets that voting pattern as evidence the investor is paying *more* attention and *more* likely to be monitoring. Iliev and Lowry (2015) and Malenko and Malenko (2019) posit that if fund families devote more resources towards becoming informed, they will be less likely to follow proxy advisory firm recommendations indiscriminately. Malenko et al. (2021) also show that voting against ISS is the equilibrium outcome for more attentive investors when ISS uses its vote recommendations to create controversy. Consistent with such a voting pattern reflecting increased monitoring, not less, Iliev and Lowry (2015) observe a greater likelihood of disagreeing with ISS for active mutual funds where the net benefits of being attentive are greater. Moreover, Iliev et al. (2021) find that this voting behavior positively correlates with an institutional investor becoming informed before a vote, including the downloading of EDGAR filings.

Second, filing a 13G form instead of a 13D form is not evidence of an absence of monitoring. By not filing a 13D, the institutional investor is stating that it will not engage in certain, more active forms of governance, like nominating directors, soliciting proxies, or trying to force the sales of the company. However, that does *not* mean the institutional investor is necessarily passive. The investor can still communicate her views about compensation and governance issues to management, and more importantly, the investor can still vote based on those views. Moreover, multiple studies have shown the ability for investors to exert considerable influence through such forms of engagement, including index investors (e.g., see Appel, Gormley, and Keim, 2019; Gormley, Gupta, Matsa, Mortal, and Yang, 2023).

6. Concluding Remarks

In this paper, we revisit the findings of Heath et al. (2022) and highlight flaws in their methodology when examining the relationship between index funds, corporate governance, and firm value. By implementing corrections to their difference-in-differences specification, we reveal that Heath et al.'s problematic specification overstates the effects of index switching on active mutual fund ownership, managerial incentives, and corporate governance outcomes. Our corrected results show that index funds do not replace active mutual funds following index switching, nor is index switching associated with changes in governance, managerial incentives, or performance that might suggest reduced investor monitoring. Moreover, most of Heath et al.'s published findings disappear after making just one minor correction in their posted code and data.

By correcting their specification and comparing different methodologies, we provide a more accurate understanding of the nuanced dynamics at play when index ownership increases. We also shed light on why differing methodologies can yield conflicting conclusions. The corrected Heath et al. difference-in-differences specification yields very similar findings to that of the Appel, Gormley, and Keim (2019) specification. Combined, the new findings paint a more consistent picture regarding index investing's potential impact on corporate governance.

While the rise of index investing continues to reshape the investment landscape, its governance impact is still hotly debated. Future work should focus on disentangling the complex interactions between active and index fund ownership in a broader range of market contexts. Such efforts will ultimately provide a more comprehensive understanding of how this fundamental shift in ownership matters for aggregate stewardship activities and firm performance.

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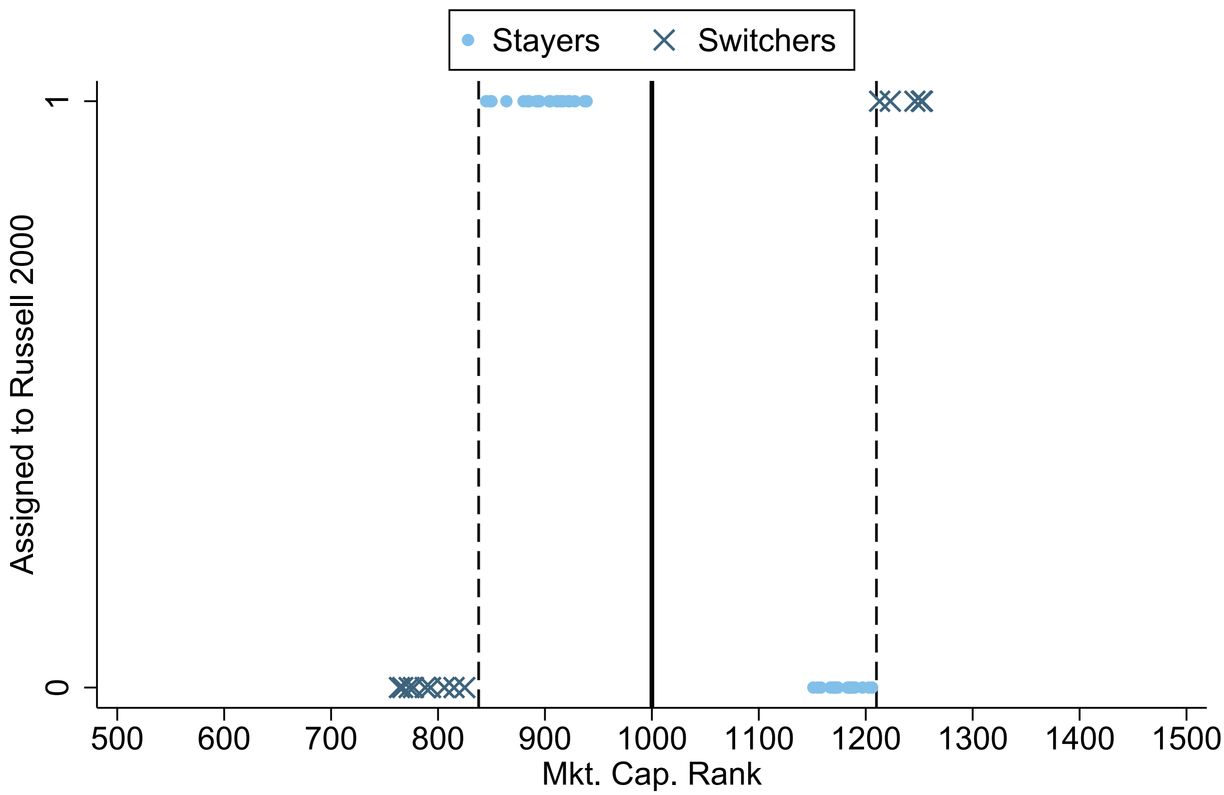
Figure 1
Selection of Cohort Samples

In this figure, we replicate Panel B of Figure 2 from Heath et al. (2002) using the sample we construct ourselves. Specifically, we plot the index assignments of the 2007 cohort that includes all Russell stocks within ± 100 ranks (i.e., the "upper" and the "lower" bands) of each index cutoff based on Russell's "banding" policy. *Stayers* are stocks that were close to switching indexes but remain in their original index while *Switchers* are those that switch indexes.



(b) 2007 Cohort Firms

Figure 2
Index switching and Index Fund Ownership

In this figure, we replicate Figure 5 from Heath et al. (2022) using the sample we construct that consists of all Russell stocks within ± 100 ranks (i.e., the “upper” and the “lower” bands) of each index cutoff based on Russell’s “banding” policy. We plot average ownership (%) by Russell 2000 index funds in event time for potential switchers of Russell 1000 stocks near the lower band and those of Russell 2000 stocks near the upper band in the left and the right figures, respectively.

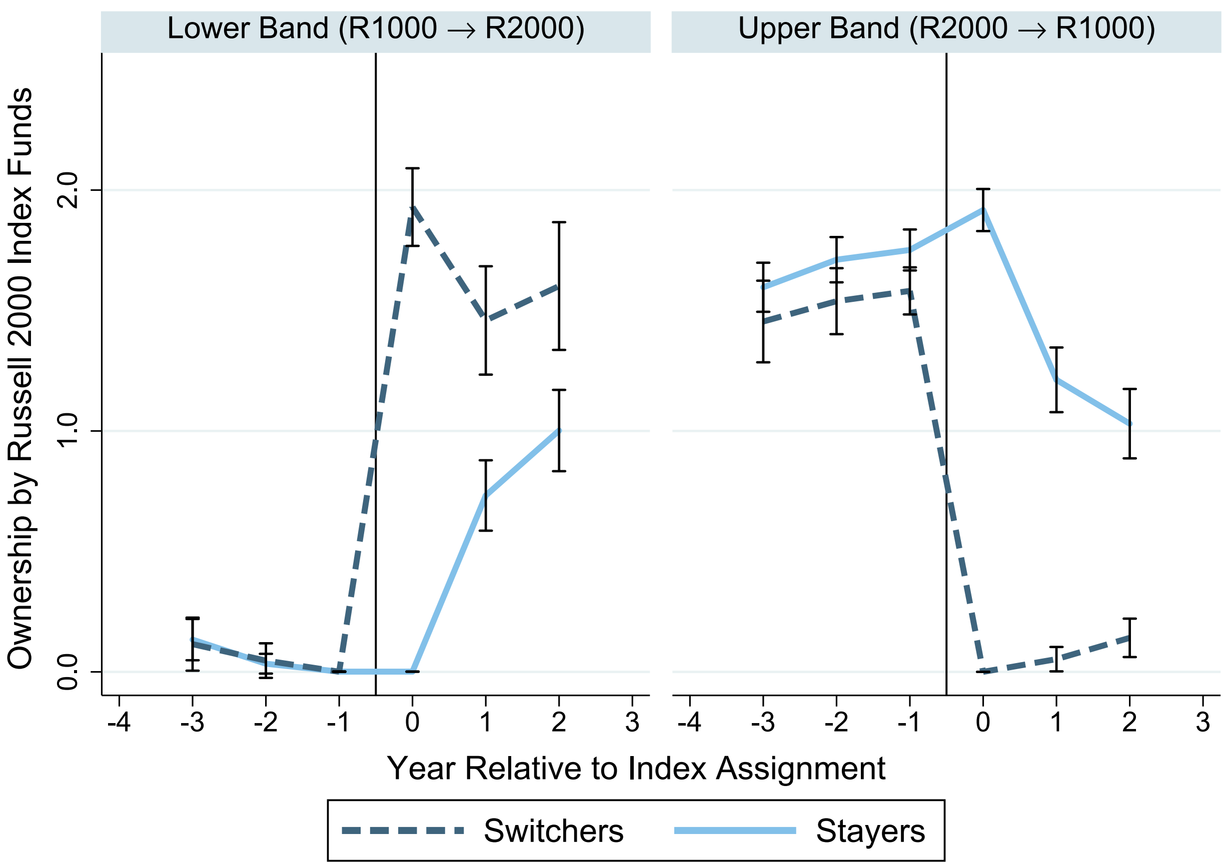


Table 1
Summary Statistics

This table reports summary statistics of the sample we construct that consists of firms in the Russell cohort within ± 100 ranks (i.e., the "upper" and the "lower" bands) of each index cutoff based on Russell's "banding" policy. The sample is from 2004 through 2018 as years -3, -2, -1, 0, 1, and 2 around the cohorts of 2007 to 2016 are used. Observations are at the firm-year level. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper.

	Mean	SD	P10	Median	P90	No. obs.
Market cap ($M)	2,501	1,436	1,124	2,191	4,108	4,381
$IndexOwn^{R2000}$	1.01%	1.09%	0.00%	0.69%	2.47%	4,381
$IndexOwn^{R1000}$	0.09%	0.12%	0.00%	0.00%	0.26%	4,381
$IndexOwn^{All}$	9.31%	5.78%	1.07%	9.33%	16.84%	4,381
$ActiveOwn$	25.35%	13.56%	6.14%	26.16%	40.79%	4,381
logPPS	6.06	1.31	4.46	6.04	7.70	3,628
logTotalComp	8.67	0.67	7.87	8.65	9.50	3,344
EquityFrc	0.46	0.20	0.20	0.46	0.72	3,653
GldnPara	0.78	0.41	0	1	1	2,730
CEOTurnover	0.04	0.21	0	0	0	3,653
BoardIndep	0.79	0.10	0.62	0.80	0.90	2,652
E-index	3.23	1.06	2	3	5	2,730
PoisonPill	0.21	0.40	0	0	1	2,730
Supermaj	0.65	0.48	0	1	1	2,730
LimSpecMeet	0.50	0.50	0	1	1	2,730
WrConsent	0.25	0.43	0	0	1	2,730
DualClass	0.06	0.23	0	0	0	2,730

Table 2
Index Switching and Fund Ownership - HMMR Specification

This table presents the estimation results from the difference-in-differences specification by Heath et al. (2022) in Equation (1), i.e.,

$$\begin{aligned} Y_{jct} &= \beta_1 \left(R1000 \rightarrow R2000\right)_{jc} \times PostAssignment_{ct} \\ &\quad + \beta_2 \left(R2000 \rightarrow R1000\right)_{jc} \times PostAssignment_{ct} + \phi_{jc} + \psi_t + \epsilon_{jct} \end{aligned}$$

where Y_{jct} is either $IndexOwn^{All}_{jt}$, the fraction of firm j's market capitalization held by all index mutual funds and ETFs at the end of year t, or $ActiveOwn_{jt}$, the fraction owned by active mutual funds. Columns 1 and 2 report the results using the dataset provided by Heath et al., while columns 3 and 4 report results based on our own dataset, constructed following Heath et al.'s sampling procedure. $(R1000 \rightarrow R2000)_{jc}$ is a binary variable indicating whether stock j switches from the Russell 1000 index to Russell 2000 index in cohort year c. $(R2000 \rightarrow R1000)_{jc}$ is defined similarly. $PostAssignment_t$ is an indicator for the three years following cohort year c. We include year fixed effects and firm-by-cohort fixed effects as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	HMMR sample		Constructed sample	
	$IndexOwn^{All}_{jt}$	$ActiveOwn_{jt}$	$IndexOwn^{All}_{jt}$	$ActiveOwn_{jt}$
$R1000 \rightarrow R2000_j \times$ $PostAssignment_t$	1.31***	-2.21**	1.24***	-2.27**
	(0.34)	(0.75)	(0.24)	(0.82)
$R2000 \rightarrow R1000_j \times$ $PostAssignment_t$	-1.20***	1.60**	-1.09***	0.98
	(0.24)	(0.57)	(0.17)	(0.85)
Observations	4,649	4,649	4,378	4,378
Adjusted R^2	0.870	0.759	0.837	0.786
Year FE	Yes	Yes	Yes	Yes
Firm $\times$ Cohort FE	Yes	Yes	Yes	Yes

Table 3
Importance of Avoiding Problematic Comparisons in HMMR Dataset

This table makes overall comparisons between the difference-in-differences specification by Heath et al. (2022) [Equation (1)] and our corrected difference-in-differences specifications that avoids problematic control group comparisons [Equations (2)-(3)] across selected outcome variables. Specifically, Panel A reports the estimation results of the following specification:

$$\begin{aligned} Y_{jct} &= \beta_1 \left(R1000 \rightarrow R2000\right)_{jc} \times PostAssignment_{ct} \\ &\quad + \beta_2 \left(R2000 \rightarrow R1000\right)_{jc} \times PostAssignment_{ct} + \phi_{jc} + \psi_t + \epsilon_{jct}, \end{aligned}$$

while Panel B presents the estimation results of the following specifications:

$$Y_{jct} = \gamma (R1000 \rightarrow R2000)_{jc} \times PostAssignment_{ct} + \phi_{jc} + \psi_{ct} + \epsilon_{jct}$$

for the sample of Russell 1000 stocks near the lower threshold, and

$$Y_{jct} = \gamma (R2000 \rightarrow R1000)_{jc} \times PostAssignment_{ct} + \phi_{jc} + \psi_{ct} + \epsilon_{jct}$$

for the sample of Russell 2000 stocks near the upper threshold. The dataset provided by Heath et al. is used for this table. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include year-by-cohort fixed effects and firm-by-cohort fixed effects as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Index* Own^{All}	(2) *Active* *Own*	(3) log-PPS	(4) Equity -Frc	(5) Board Indep	(6) $\log(q)$	(7) log-(q^{TOT})	(8) $\log(\frac{M}{B})$	(9) ROA
A. HMMR specification using HMMR sample									
$R1000 \rightarrow R2000_j$ ×	1.31***	-2.21**	-0.43***	-0.06**	-0.03***	-0.10***	-0.21***	-0.12**	-0.03***
$PostAssignment_t$	(0.34)	(0.75)	(0.11)	(0.02)	(0.01)	(0.03)	(0.05)	(0.04)	(0.01)
$R2000 \rightarrow R1000_j$ ×	-1.20***	1.60**	0.27**	0.03**	0.00	0.01	0.06*	-0.03	0.00
$PostAssignment_t$	(0.24)	(0.57)	(0.10)	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)	(0.01)
Observations	4,649	4,649	3,445	3,138	2,613	4,296	3,403	4,552	4,188
R^2	0.870	0.759	0.697	0.699	0.769	0.849	0.851	0.816	0.738
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
B. Corrected specification that avoids problematic comparisons using HMMR sample									
$R1000 \rightarrow R2000_j$ ×	1.17***	-0.98	-0.07	-0.01	-0.04***	-0.03	-0.03	-0.07	-0.02
$PostAssignment_t$	(0.32)	(0.95)	(0.13)	(0.02)	(0.01)	(0.03)	(0.06)	(0.05)	(0.01)
Observations	1,618	1,618	1,289	1,191	1,022	1,459	1,162	1,589	1,519
R^2	0.896	0.781	0.760	0.686	0.829	0.788	0.859	0.763	0.701
$R2000 \rightarrow R1000_j$ ×	-1.00***	0.94	0.11	-0.00	0.01	-0.02	-0.02	-0.06**	-0.01
$PostAssignment_t$	(0.27)	(0.72)	(0.10)	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)
Observations	3,031	3,031	2,156	1,945	1,591	2,837	2,241	2,963	2,669
R^2	0.864	0.759	0.686	0.727	0.748	0.869	0.869	0.830	0.766
Year × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4
Index Switching and Fund Ownership - Fully Corrected Specification

This table presents the estimation results of the separate difference-in-differences specifications in Equations (2) and (3) after making additional corrections that account for index status in non-cohort years and the endogenous factor that drives index switching. Panels A and B report results with $IndexOwn_{jt}^{All}$ and $ActiveOwn_{jt}$ as the dependent variables, respectively. The results in Columns 1 and 2 are based on the dataset from Heath et al. (2022) and our own constructed dataset, respectively. Columns 3 and 4 use our constructed sample, applying a corrected sampling procedure that tosses observations affected by an index switch that occurs in a non-cohort year. Column 4 includes size controls (i.e., end-of-May market capitalization). All columns include year-by-cohort fixed effects and firm-by-cohort fixed effects. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Sample	HMMR	Constructed	Corrected	
A. Dependent Variable = Index Ownership				
$R1000 \rightarrow R2000_j \times$	1.17***	0.78***	1.34***	1.42***
$PostAssignment_t$	(0.32)	(0.23)	(0.24)	(0.24)
Observations	1,618	1,534	1,311	1,311
R^2	0.896	0.856	0.874	0.875
$R2000 \rightarrow R1000_j \times$	-1.00***	-0.96***	-1.31***	-1.30***
$PostAssignment_t$	(0.27)	(0.15)	(0.17)	(0.18)
Observations	3,031	2,844	2,538	2,538
R^2	0.864	0.836	0.854	0.854
B. Dependent Variable = Active Ownership				
$R1000 \rightarrow R2000_j \times$	-0.98	-1.40	-2.04	-1.51
$PostAssignment_t$	(0.95)	(1.17)	(1.28)	(1.28)
Observations	1,618	1,534	1,311	1,311
R^2	0.781	0.800	0.818	0.819
$R2000 \rightarrow R1000_j \times$	0.94	-0.12	0.44	-0.59
$PostAssignment_t$	(0.72)	(0.77)	(0.82)	(0.84)
Observations	3,031	2,844	2,538	2,538
R^2	0.759	0.787	0.792	0.794
Size control	No	No	No	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes

Table 5
Index Switching and Fund Ownership

This table reports the estimation results of the combined (and corrected) difference-in-differences specification in Equation (4) and the Appel, Gormley and Keim (2019) specification with firm fixed effects in Equation (6). Panels A and B report results with $IndexOwn_{jt}^{All}$ and $ActiveOwn_{jt}$ as the dependent variables, respectively. The results in Columns 1 and 2 are based on HMMR's two-band sample, applying a corrected sampling procedure that tosses observations affected by an index switch that occurs in a non-cohort year, while for column 3, we use the larger AGK sample that includes the bottom (top) 500 stocks of the Russell 1000 (2000) index each year. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include size controls (i.e., end-of-May market capitalization), and year-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm fixed effects, and year fixed effects as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
Specification	Combined DID	AGK	
A. Dependent Variable = Index Ownership			
$R2000_{post} - R2000_{pre\ j} \times$ $PostAssignment_t$	1.36***		
	(0.14)		
$R2000_{jt}$		1.35***	1.29***
		(0.12)	(0.11)
Observations	3,849	3,810	8,181
R^2	0.862	0.848	0.823
B. Dependent Variable = Active Ownership			
$R2000_{post} - R2000_{pre\ j} \times$ $PostAssignment_t$	-0.09		
	(0.71)		
$R2000_{jt}$		-1.00	-0.39
		(0.64)	(0.44)
Observations	3,849	3,810	8,181
R^2	0.805	0.788	0.734
HMMR sample	Yes	Yes	No
AGK sample	No	No	Yes
Size control	Yes	Yes	Yes
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	No	No
Firm $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	No	No
Firm FE	No	Yes	Yes
Year FE	No	Yes	Yes

Table 6
Index Switching and Proposals for Voting

This table analyzes management and shareholder proposal outcomes using the combined (and corrected) difference-in-differences specification in Equation (4) and the Appel, Gormley and Keim (2019) specification with firm fixed effects in Equation (6). The combined (and corrected) DID results on our constructed two-band sample with the corrected sampling procedure are reported in Panel A, while the results from the AGK specification on the same sample are reported in Panel B. The AGK results on our constructed sample based on Appel et al.'s sampling procedure are reported in Panel C. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include size controls (i.e., end-of-May market capitalization), and year-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm fixed effects, and year fixed effects as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	Management Proposals			Shareholder Proposals		
	Number contentious (1)	Fraction contentious (2)	Fraction passed (3)	Number contentious (4)	Fraction contentious (5)	Fraction passed (6)
A. Combined (and corrected) diff-in-diffs (Eq. 4) on constructed sample						
$R2000_{post} - R2000_{pre\ j} \times PostAssignment_t$	-0.19*	-0.01	0.00	0.35	-0.14	0.13
	(0.11)	(0.01)	(0.01)	(0.32)	(0.24)	(0.08)
Observations	3,519	3,519	3,519	131	131	135
R^2	0.552	0.563	0.420	0.723	0.645	0.782
Size control	Yes	Yes	Yes	Yes	Yes	Yes
Year × Cohort × $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort × $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes
B. AGK specification (Eq. 6) on constructed sample						
$R2000_{jt}$	-0.08	0.01	0.00	0.23	0.01	-0.09
	(0.11)	(0.01)	(0.01)	(0.14)	(0.07)	(0.10)
Observations	3,499	3,499	3,499	224	224	230
R^2	0.507	0.530	0.388	0.813	0.662	0.697
Size control	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
C. AGK specification (Eq. 6) on AGK sample						
$R2000_{jt}$	-0.08	0.00	0.00	0.38	0.16	0.03
	(0.09)	(0.01)	(0.01)	(0.36)	(0.10)	(0.13)
Observations	7,557	7,557	7,562	538	538	545
R^2	0.494	0.480	0.379	0.615	0.615	0.573
Size control	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7
Index Switching and Managerial Incentives

This table analyzes managerial incentive outcomes using the combined (and corrected) difference-in-differences specification in Equation (4) and the Appel, Gormley and Keim (2019) specification with firm fixed effects in Equation (6). The combined (and corrected) DID results on our constructed two-band sample with the corrected sampling procedure are reported in Panel A, while the results from the AGK specification on the same sample are reported in Panel B. The AGK results on our constructed sample based on Appel et al.'s sampling procedure are reported in Panel C. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include size controls (i.e., end-of-May market capitalization), and year-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm fixed effects, and year fixed effects as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) logPPS	(2) logTotalComp	(3) EquityFrc	(4) GldnPara	(5) CEOTurnover
A. Combined (and corrected) diff-in-diffs (Eq. 4) on constructed sample					
$R2000_{post} - R2000_{pre\ j} \times$	0.01	0.01	-0.00	-0.02	-0.00
$PostAssignment_t$	(0.07)	(0.04)	(0.01)	(0.03)	(0.02)
Observations	3,188	3,209	3,209	2,323	3,209
R^2	0.832	0.908	0.686	0.744	0.239
Size control	Yes	Yes	Yes	Yes	Yes
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes
Firm $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes
B. AGK specification (Eq. 6) on constructed sample					
$R2000_{jt}$	0.12*	0.08**	-0.00	-0.01	0.02
	(0.07)	(0.04)	(0.01)	(0.03)	(0.02)
Observations	3,163	3,182	3,182	2,341	3,182
R^2	0.795	0.894	0.659	0.709	0.186
Size control	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
C. AGK specification (Eq. 6) on AGK sample					
$R2000_{jt}$	0.04	0.06*	0.00	-0.01	0.03
	(0.06)	(0.03)	(0.01)	(0.02)	(0.02)
Observations	6,613	6,659	6,660	5,672	6,660
R^2	0.785	0.894	0.619	0.686	0.167
Size control	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Table 8
The Importance of Controlling for Size

This table compares the estimation results for pay-performance sensitivity from the combined (and corrected) difference-in-differences specification in Equation (4), with and without controlling for firm size. The sample used for this analysis is our constructed two-band sample with the corrected sampling procedure. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include size controls (i.e., end-of-May market capitalization) in Column 2 but not in Column 1. Both specifications include year-by-cohort-by-pre-cohort-assignment Russell index fixed effects and firm-by-cohort-by-pre-cohort-assignment Russell index fixed effects. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	logPPS	
$R2000_{post} - R2000_{pre\ j} \times$	0.01	-0.18**
$PostAssignment_t$	(0.07)	(0.07)
Observations	3,188	3,188
R^2	0.832	0.819
Size control	Yes	No
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes
Firm $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes

Table 9
Index Switching and Governance

This table analyzes governance outcomes using of the combined (and corrected) difference-in-differences specification in Equation (4) and the Appel, Gormley and Keim (2019) specification with firm fixed effects in Equation (6). The combined (and corrected) DID results on our constructed two-band sample with the corrected sampling procedure are reported in Panel A, while the results from the AGK specification on the same sample are reported in Panel B. The AGK results on our constructed sample based on Appel et al.'s sampling procedure are reported in Panel C. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include size controls (i.e., end-of-May market capitalization), and year-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm fixed effects, and year fixed effects as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) Board Indep	(2) E-index	(3) Poison Pill	(4) Super -maj	(5) LimSpec Meet	(6) Wr Consent	(7) Dual Class
A. Combined (and corrected) diff-in-diffs (Eq. 4) on constructed sample							
$R2000_{post} - R2000_{pre\ j} \times$	-0.02**	-0.03	0.01	0.00	0.02	-0.02	-0.00
$PostAssignment_t$	(0.01)	(0.06)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)
Observations	2,306	2,323	2,323	2,323	2,323	2,323	2,323
R^2	0.808	0.872	0.820	0.933	0.856	0.929	0.886
Size control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Cohort × $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort × $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
B. AGK specification (Eq. 6) on constructed sample							
$R2000_{jt}$	-0.02*	-0.07	0.00	0.01	0.01	-0.00	0.00
	(0.01)	(0.05)	(0.03)	(0.02)	(0.03)	(0.01)	(0.01)
Observations	2,313	2,341	2,341	2,341	2,341	2,341	2,341
R^2	0.760	0.850	0.777	0.910	0.839	0.920	0.886
Size control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
C. AGK specification (Eq. 6) on AGK sample							
$R2000_{jt}$	0.00	0.02	-0.00	0.03*	0.00	-0.00	0.01
	(0.01)	(0.06)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)
Observations	5,580	5,672	5,672	5,672	5,672	5,672	5,672
R^2	0.779	0.882	0.715	0.894	0.923	0.874	0.945
Size control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10
Governance and Proposals for Voting - AGK Specification without FEs

This table analyzes governance outcomes using the cross-sectional AGK specification in Equation (5). Panel A analyzes various governance proxies used in HMMR, while Panel B analyzes the number and type of agenda items proposed and the fraction that are passed. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include size controls (i.e., end-of-May market capitalization) and year fixed effects in all specifications. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

A. Governance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	BoardIndep	E-index	PoisonPill	Supermaj	LimSpecMeet	WrConsent	DualClass
$R2000_{jt}$	0.31***	0.19	0.02	0.08*	0.03	-0.09**	-0.08***
	(0.10)	(0.13)	(0.03)	(0.04)	(0.04)	(0.04)	(0.02)
Observations	5,780	5,880	5,880	5,880	5,880	5,880	5,880
R^2	0.075	0.027	0.095	0.004	0.013	0.008	0.013
Size ctrl	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

B. Proposals for voting

	Management Proposals			Shareholder Proposals		
	Number contentious (1)	Fraction contentious (2)	Fraction passed (3)	Number contentious (4)	Fraction contentious (5)	Fraction passed (6)
$R2000_{jt}$	-0.35***	-0.04***	0.00	0.74*	0.17**	0.01
	(0.13)	(0.01)	(0.00)	(0.40)	(0.07)	(0.10)
Observations	7,916	7,916	7,923	742	742	762
R^2	0.102	0.103	0.098	0.033	0.039	0.034
Size control	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 11
Index Switching and Firm Value

This table analyzes value outcomes using of the combined (and corrected) difference-in-differences specification in Equation (4) and the Appel, Gormley and Keim (2019) specification with firm fixed effects in Equation (6). The combined (and corrected) DID results on our constructed two-band sample with the corrected sampling procedure are reported in Panel A, while the results from the AGK specification on the same sample are reported in Panel B. The AGK results on our constructed sample based on Appel et al.'s sampling procedure are reported in Panel C. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include size controls (i.e., end-of-May market capitalization), and year-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm-by-cohort-by-pre-cohort-assignment Russell index fixed effects, firm fixed effects, and year fixed effects as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm and year. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) logQ	(2) logqtot	(3) logMB	(4) ROA
A. Combined (and corrected) diff-in-diffs (Eq. 4) on constructed sample				
$R2000_{post} - R2000_{pre\ j} \times$	0.00	0.03	-0.03	0.00
$PostAssignment_t$	(0.02)	(0.02)	(0.04)	(0.01)
Observations	3,849	3,444	3,732	3,839
R^2	0.897	0.904	0.798	0.714
Size control	Yes	Yes	Yes	Yes
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes
Firm $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes
B. AGK specification (Eq. 6) on constructed sample				
$R2000_{jt}$	0.00	0.02	0.02	0.01
	(0.02)	(0.02)	(0.05)	(0.01)
Observations	3,810	3,415	3,696	3,800
R^2	0.874	0.887	0.765	0.693
Size control	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
C. AGK specification (Eq. 6) on AGK sample				
$R2000_{jt}$	-0.01	-0.01	0.02	-0.00
	(0.01)	(0.02)	(0.03)	(0.01)
Observations	7,649	6,818	7,456	7,634
R^2	0.850	0.842	0.768	0.648
Size control	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes