Do Index Funds Monitor? Revisited*

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Abstract

We address a methodological flaw in the influential Heath, Macciocchi, Michaely, and Ringgenberg (2022) paper. Corrected findings reveal that index inclusion and the resulting index ownership shifts do not affect a stock's level of active fund ownership. Instead, index funds displace institutional owners with fewer assets and for which the stock represents a smaller portion of their assets. Additionally, there is no evidence that index funds monitor companies less than the owners they displace or that their growth negatively impacts firm performance and managerial incentives. These findings suggest that the impact of index investing is less concerning than earlier research indicated.

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

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Index funds have fundamentally reshaped modern stock ownership. Once considered a niche investment strategy, indexing now plays a dominant role in capital markets, with three mutual fund families, Vanguard, BlackRock, and State Street—the "Big Three" indexers—collectively holding about 20 percent of outstanding shares across most U.S. publicly traded firms. This shift in ownership raises important questions regarding corporate governance. At the center of the debate lies a core concern: do index funds monitor less than the owners they displace?

A highly cited, influential study by Heath, Macciocchi, Michaely, and Ringgenberg (2022) (HMMR) argues the answer is "yes." Their evidence is alarming: index ownership is associated with lower firm performance, executive compensation decoupled from performance, and less independent boards. However, these conclusions are puzzling in light of other prominent studies (e.g., Appel, Gormley, and Keim, 2016, 2019 (AGK)), which reach different conclusions.[1]

Corum, Malenko, and Malenko (2023) argue that the type of ownership displaced by index ownership could explain these contradictory findings. For example, HMMR claims to analyze increases in index ownership that displace active mutual fund ownership, while AGK isolates increases in index ownership that replace other ownership types. However, this explanation is problematic because the identification settings and strategies are similar across HMMR and AGK. Specifically, why would one estimation show index ownership replaces active fund ownership while another approach in the same setting shows that index fund ownership does not?

This paper shows that HMMR's alarming narrative rests on a flawed empirical design. After correcting a key mistake in their specification, we find no evidence supporting HMMR's claims.

[1] Because of these conflicting findings, recent papers tend to argue that index investing's impact on corporate governance remains unresolved. Typically, these papers begin by highlighting competing arguments: index investors may lack the incentives or resources necessary to monitor firms effectively (e.g., Bebchuk and Hirst, 2019; Gilje, Gormley, and Levit, 2020), as opposed to reasons suggesting they enhance governance (e.g., Fisch, Hamdani, and Solomon, 2018; Kahan and Rock, 2020; Lewellen and Lewellen, 2022). The papers then point to these conflicting empirical findings, with particular emphasis on HMMR's evidence of a potential harmful impact.

Moreover, the corrected specifications shed new light on how index inclusion and the resulting shifts in index ownership actually impact a stock's overall ownership structure. Combined, the corrected and new findings point to a much more nuanced—and less concerning—relationship between index inclusion, ownership structures, and corporate outcomes.

Both AGK and HMMR use an index assignment identification strategy: comparing firms that just barely land in the Russell 2000 index versus those that just barely land in the Russell 1000 index. This index cutoff creates a natural experiment. Stocks on either side are similar, except for one key factor: stocks that fall into the Russell 2000 get snapped up by more index funds. AGK employs an instrumental variables estimation based on cross-sectional variation in index assignments. HMMR instead uses index switchers to estimate the effects of index fund ownership. On the surface, both strategies are valid and have their relative advantages. However, we show that HMMR's specification choice introduces a fundamental mismatch—one that leads to spurious conclusions.

Specifically, HMMR purports 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. 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 combines the two difference-in-differences into a single estimation that includes all switchers and stayers.

However, HMMR's approach is flawed. By using a single estimation but failing to control for a stock's starting index, they compare switchers to non-switchers from the *other* index. This comparison is problematic because, by construction, these two groups must have exhibited opposing past trends in their market capitalization rankings. One group experienced an increase, while the other group experienced a decrease. Unsurprisingly, these two groups will exhibit systematic differences in pre-switch trends across many outcomes of interest (see Figure 1). This apples-to-oranges comparison violates the identification assumptions and explains why their results diverge from those of AGK and others. One way to control for a stock's starting index, used in Coles, Heath, and Ringgenberg (2022), is to run the estimation separately for the two groups: stocks initially in the Russell 1000 and those initially in the Russell 2000. This approach ensures that index switchers are only compared against non-switchers from the same starting index.

Using HMMR's published code and data, we show how this simple correction yields notable changes. First, there is no evidence that index ownership displaces actively managed fund ownership, which could otherwise weaken or strengthen governance if actively managed funds monitor more (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. Second, HMMR's key 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.

Next, we introduce a combined stacked difference-in-differences specification that researchers can use to estimate the impact of index inclusion. A weakness of running two separate difference-in-differences is that it fails to impose (and test for) the symmetry one should expect to observe if index inclusion affects ownership and corporate outcomes. Specifically, the impact of moving from the Russell 2000 to the Russell 1000 should be the opposite of what is observed for stocks switching indexes in the opposing direction. The use of separate specifications also reduces testing power. To overcome these weaknesses, we show how to combine the two difference-in-differences into a single specification that simultaneously utilizes variation from both types of switches, thereby testing for symmetry and maximizing testing power.

This combined difference-in-differences approach continues to show no evidence that index ownership negatively impacts managerial incentives, firm value, or company performance. Once again, the estimates challenge the claim that increased index ownership is linked to outcomes that may indicate that index investors engage in less monitoring than the owners they displace.

Furthermore, the corrected findings closely match those obtained using the AGK approach, with one main exception—board independence. AGK (2016) claims that index ownership predicts greater board independence, but the corrected difference-in-differences does not. We show that AGK's use of a cross-sectional specification explains this difference. This suggests a potential tradeoff between the two estimation methods. For instance, the difference-in-differences approach assumes an immediate impact of index switching on the outcome, but it is unclear whether we should expect such an immediate effect for slow-changing outcomes like board independence.

While there is no evidence that index switching affects a stock's level of active fund ownership, the question remains: which other investors are displaced by index ownership? Using the corrected specification, we show that increased index ownership displaces institutional investors who likely have less incentive to monitor firms or the ability to exert influence—institutions where the stock represents a smaller share of their overall portfolio and institutions with smaller ownership stakes. Because much of the increase in index ownership is driven by The Big Three, these findings suggest a consolidation of ownership in the hands of fewer institutions.

The governance implications of this change in ownership structure are unclear. While index funds, all else equal, might have fewer incentives or resources to monitor firms (e.g., Bebchuk and Hirst, 2019; Gilje, Gormley, and Levit, 2020), the Big Three's size and large, direct economic exposures provide a countervailing monitoring motive (Lewellen and Lewellen, 2022). Economies of scale in monitoring and greater ability to exert influence due to large ownership stakes also suggest that the observed shift in ownership might not be detrimental to overall corporate governance and performance, which is consistent with our findings.

While our correction undoes nearly all of HMMR's published findings, it is not the only potential problem with HMMR's specification. We finish by discussing two other possible weaknesses of HMMR's difference-in-differences estimation. First, HMMR fails to limit its sample to observations with clean pre- and post-switch observations, which is necessary to ensure the stacked difference-in-differences specification yields unbiased estimates (Gormley and Matsa, 2011, 2016; Baker, Larcker, and Wang, 2022). Because of how they construct their sample, their estimation flags observations as 'treated' ('control') even though they are no longer 'treated' (or a 'control'). Another potential criticism of HMMR's difference-in-differences specification is that it does not control for changes in market capitalization—the factor that drives a stock's status as an index switcher or stayer. While these two issues might be important for other outcomes, adding corrections for them does not meaningfully change our findings. We continue to find no evidence of outcomes that suggest weakened investor monitoring following increases in index ownership.

We also discuss two additional issues with the HMMR's arguments that index fund investors do not monitor. First, HMMR argues 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 evidence supports that interpretation (Iliev and Lowry, 2015; Malenko and Malenko 2019, Iliev, Kalodimos, and Lowry, 2021; Malenko, Malenko, and

Spatt, 2021). Second, HMMR argues that indexers' decision to file a 13G form reflects an absence of monitoring. However, even 13G investors can engage in stewardship via exit and voting, and studies confirm that investors, including indexers, exert influence through such channels (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, investors, and firm performance. Existing findings suggest a nuanced shift in companies' corporate governance but with unclear performance implications (e.g., Appel, Gormley, and Keim, 2016, 2019; Schmidt and Fahlenbrach, 2017; Gormley, Gupta, Matsa, Mortal, and Yang, 2023; Brav, Jiang, Li, and Pinnington, 2024). Our findings add to this body of work by definitively showing there is no evidence that these index-induced governance changes negatively impact either managerial incentives or firm performance. While these findings do not settle the debate on whether index investors monitor as much as active funds, they do confirm a lack of evidence to suggest that index funds monitor less than the investors they displace.

Our findings also shed light on how the growing popularity of indexing is shifting ownership structures. While it may be tempting to assume that increased index fund ownership comes at the expense of actively managed funds, our findings show that the story is more complex. Index inclusion and the resulting increase in index ownership instead displace ownership by institutional investors with characteristics and holdings that suggest they are less likely to be active monitors.[2]

Our study also makes a methodological contribution to the literature on index fund ownership and corporate governance. Several studies have employed various methodologies to

[2] One must also be careful when interpreting the impact of index assignments. While index assignments lead to observable shifts in index fund ownership, the actual shift in ownership connected to indexing strategies is likely larger because of non-fund investors engaged in internal indexing, direct indexing, or closet indexing (Chinco and Sammon, 2024). Therefore, one should interpret any observed effects of index assignment as stemming from the overall shift in indexing-linked ownership, not just to the observed shift in indexed fund ownership.

exploit annual index reconstitutions,[3] and a few papers discuss the tradeoffs of earlier estimation approaches.[4] In our study, we address a new methodological approach that is causing confusion in the literature. HMMR argue that their novel difference-in-differences approach is a better way to utilize the Russell setting. By correcting HMMR's empirical approach and showing why it yields different findings, we provide a clear empirical path forward for future research on this important topic. We also provide clarity regarding the impact of index funds on corporate outcomes and show that the Russell index setting is not suitable for testing whether replacing active fund ownership with index ownership weakens governance. While indexing's growing popularity might displace actively managed funds in the aggregate, the Russell setting does not isolate such variation.

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

In this section, we discuss a key problem with HMMR's empirical specification and how to correct it. We also discuss how one can improve statistical power and how HMMR's specification compares to that of AGK. Finally, we discuss the data and sample construction we use.

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

While prior studies examine index ownership effects by exploiting cross-sectional differences in index ownership among stocks near the Russell 1000/2000 index threshold,

[3] For the Russell indexes, the list includes, 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, and Ringgenberg (2022), and Chung and Kim (2023), among many others.

[4] Appel, Gormley, and Keim (2024), Wei and Young (2024), and Glossner (2024) provide a comprehensive summary of the methodological differences across many of these earlier studies, with a particular emphasis on the importance of accounting for Russell's endogenous sorting of stocks within an index. Wei and Young (2025) does mention the potentially problematic comparisons in HMMR and implements a similar correction for it in the paper's simulation exercises. However, it does not explore the important implications of this correction, including how these problematic comparisons drive HMMR's most alarming findings regarding corporate performance and executive incentives and how those comparisons obscure the true impact of index inclusion on firms' ownership structures.

HMMR's approach is distinct in that they analyze index switching across the Russell indexes on a yearly basis. Specifically, HMMR constructs 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 estimates the following model [see Equation (1) from HMMR]:

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

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 includes 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 similarities to a stacked difference-in-differences.

HMMR claims 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 does not correctly perform a difference-in-differences comparison for either set of index switchers. Because the estimation codes $(R1000 \rightarrow R2000)_{jc}$ as zero for stocks initially in the Russell 2000 and $(R2000 \rightarrow R1000)_{jc}$ as zero for stocks 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 because non-switchers from the other index differ significantly in their market capitalization and its recent changes, raising doubts about their validity as a control group.

Plots of key HMMR outcomes for switchers and non-switchers illustrate why using stayers from the other index as a control group is likely problematic. We start by classifying firms of each cohort into three groups: 1) switchers, 2) stayers in the same index, and 3) stayers in the other index. For each group, we then compute the average of three key HMMR outcome variables—pay-for-performance sensitivity, Tobin's q, and ROA—in the three years before the cohort year. We perform this separately for firms that were in the Russell 1000 and Russell 2000 in the year before the cohort year. The resulting plots, shown in Figure 1, highlight a key problem: while switchers and stayers in the same index exhibit similar pre-trends, stayers in the other index follow markedly different trends. However, HMMR's difference-in-differences assumes that switchers would have exhibited similar trends as *both* stayer types, absent the switch. That assumption is dubious because stayers of the other index exhibit pre-existing differential trends.

<Figure 1 About Here>

The pre-existing differential trends for stayers of the other index are unsurprising. Because HMMR only samples stocks near the two banding cutoffs for switching indexes, the Russell 1000 (2000) stayers reflect stocks that have shrunk (grown) in their relative market capitalization but not quite enough to switch indexes. This relative shift in market capitalization is the opposite of what is occurring for the stocks that successfully switch into the Russell 1000 (2000). In other words, by construction, the estimation employs a control group that *must have* exhibited an opposing past trend in its market capitalization rankings. Unsurprisingly, this particular control group will exhibit systematic differences in pre-switch trends across many outcomes of interest.

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].[5] Figure 1 shows parallel pre-trends in key outcome variables between switchers and stayers in the same index, providing evidence to support the parallel trends assumption of each estimation.

[5] Nearly, but not exactly. Coles, Heath, and Ringgenberg (2022) only include year fixed effects, while we include cohort-by-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.

1.3. Incomplete Subsample Comparisons and Reduced Statistical Power

A weakness of the separate estimations of Equations (2) and (3) is that they estimate the impact of index switching in two separate samples, which reduces statistical power and fails to impose (and test for) the symmetry one would observe if index assignment affects the outcomes of interest. Indeed, Heath et al. (2022) at times highlights coefficients estimated using Russell 1000 to Russell 2000 switchers to support their argument, while overlooking coefficients estimated using Russell 2000 to Russell 1000 switchers that do not show such evidence. A combined estimation would see this asymmetry and appropriately suggest that index inclusion does not affect the outcome of interest. To overcome this weakness, one can pool the two samples and estimate the following combined stacked difference-in-differences specification:

$$Y_{jct} = \theta\{R2000_{post} - R2000_{pre}\}_{jc} \times PostAssignment_{ct} + \xi_{jc} + \omega_{ctr} + \epsilon_{jct}, \tag{4}$$

where *r* indexes a stock's Russell assignment (Russell 2000 or Russell 1000) in the year before the cohort year *c*. $R2000_{post}$ is an indicator equal to one if stock *j* was in the Russell 2000 in year *c*. $R2000_{pre}$ is an indicator equal to one if firm *j* was in the Russell 2000 in the year prior to 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 year *c*, zero if stock *j* stays in the same index before and after the reconstitution in year *c*, and *negative one* if stock *j* switches indexes from the Russell 2000 to the Russell 1000 in year *c*. ξ_{jc} denotes stock-by-cohort fixed effects, and ω_{ctr} denotes year-by-cohort-by-pre-cohort-assignment fixed effects.

The inclusion of the extra interaction in the year-cohort fixed effects here is crucial. Its inclusion creates one set of year fixed effects for stocks that start in the Russell 2000 and another set for stocks that start in the Russell 1000. This ensures that we only compare stocks that started in the same index against each other in the combined specification. It is not necessary to include the extra interaction for the stock-cohort fixed effects because pre-cohort assignment does not vary within a stock-cohort. In untabulated results, we confirm that the resulting point estimates are identical if one instead includes stock-by-cohort-by-pre-cohort-assignment fixed effects.

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 impact 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.[6]

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 of whether the stock is in the Russell 2000 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 selects their sample differently. Rather than restrict the sample to stocks near each switching cutoff, they restrict their sample to the 500 stocks

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[6] If an asymmetry is expected and theoretically justified, a researcher would not want to utilize the combined difference-in-differences. In that case, a researcher should make the other proposed adjustment to HMMR's specification but skip this one. For completeness, we report these subsample estimates in our subsequent analysis.

at the bottom of the Russell 1000 and the 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 include 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 control for $Ln(Mktcap_{jt})$, which is the natural log of the stock j's market capitalization in May of year t. Specifically, they use a third-degree polynomial control for $Ln(Mktcap_{jt})$. They control for market capitalization because it determines index assignment and could be correlated with outcomes of interest.

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 employ 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

$$Y_{jt} = \alpha + \eta_1 R2000_{jt} + \sum_{n=1}^{3} \zeta_n Ln\left(Mktcap_{jt}\right)^n + \rho Ln\left(Float_{jt}\right) \tag{6}$$

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

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 our specification correction changes 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 the AGK specification and some of our later specifications require variables that are not included in HMMR's posted data. Following the sampling procedure described by HMMR, we obtain a sample of 4,522 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 2. 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 3.

<Figure 2 About Here>

<Figure 3 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 1.38 percentage point increase in index fund ownership and a 2.25 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.[7]

<Table 2 About Here>

3. Findings After Correcting HMMR's Specification

In this section, we analyze how the HMMR findings change after correcting their specification using their own data and code. Then, we compare these results with those obtained when using the AGK specification and our constructed data.

[7] 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. This discrepancy likely reflects minor differences in the Stata ADO file versions used in our estimation versus what HMMR used when writing their paper.

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). While a seemingly minor change from the HMMR estimation 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 HMMR findings, as obtained when using their posted 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)]. For brevity, Table 3 only includes HMMR's most noteworthy findings, but in subsequent analysis, we report estimates for the other outcomes analyzed in HMMR.

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). In the following subsections, we describe these differences and their important implications in more detail.

<Table 3 About Here>

However, avoiding problematic comparisons has little impact on HMMR's finding for index ownership. This finding is unsurprising, 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 one percentage point for stocks that switch to the Russell 1000 (Table 3, Panel B, Column 1). These findings are similar to those reported by HMMR in their combined specification (Panel A, Column 1).

3.1.1. Revisiting Active Fund Ownership

HMMR claims that index fund ownership replaces active fund ownership. Specifically, their estimates show about a two-percentage-point swing in active fund ownership (Table 3, Panel A, Column 2) that moves in the opposite direction of the observed changes in index fund ownership (Column 1). If accurate, this shift in ownership could imply a potential decrease in investor monitoring. Corum et al. (2023) argue that reduced monitoring of stocks with higher index ownership might occur if increased index ownership reduces active fund ownership.

However, avoiding problematic comparisons significantly impacts HMMR's active ownership findings. After this correction, there is little evidence that index switching influences active fund ownership (Panel B, Column 2). The point estimates for active fund ownership are no longer statistically significant and are less than half the magnitude of those reported in HMMR. Additionally, the corrected results for active ownership now align with earlier non-findings in the literature (e.g., Appel, Gormley, and Keim, 2016, 2019). While the smaller, insignificant estimates might still suggest some active ownership displacement, our subsequent tests indicate that index ownership is instead replacing a different type of institutional investor ownership, which has important implications for the potential impact on investor monitoring.

3.1.2. Revisiting Managerial Incentives

HMMR asserts that increased index fund ownership weakens managerial incentives. Specifically, HMMR states that higher index ownership results in less pay-for-performance sensitivity (PPS) and a smaller portion of executive pay granted through equity. Table 3, Panel A, Columns 3-4, shows HMMR's estimated effects for these compensation metrics. HMMR views these findings as evidence that index funds do less monitoring of managers.

However, with the corrected specification, we observe no evidence indicating that index assignment influences CEOs' pay-for-performance sensitivity or the proportion of compensation received as equity (Table 3, Panel B, Columns 3-4). In other words, there is little evidence that index ownership changes managerial compensation in ways that might be concerning.

3.1.3. Revisiting Firm Value and Performance

Next, we revisit HMMR's findings regarding firm value and performance. If greater index ownership leads to weaker governance and less investor monitoring, one might see decreases in firm performance and value. HMMR examines this possibility by analyzing four outcomes: Tobin's Q, Peters and Taylor (2017)'s total Q, market-to-book ratio, and return on assets (ROA). They present their findings in Table 10 of their paper, and we replicate their results in Table 3, Panel A, Columns 5-8. As shown, they find evidence of declines across all four value and performance measures for firms that switch into the Russell 2000. They interpret these findings as evidence that increased index ownership weakens investor monitoring of managers.

However, there is no evidence of a negative impact on firm value or performance once a proper specification is used. The corrected difference-in-differences specifications show no evidence of an adverse effect on these outcomes for stocks switching into the Russell 2000, and none of the coefficients are statistically significant (Table 3, Panel B, Columns 5-8). Moreover, for none of the performance and value outcomes do we observe an asymmetry across subsamples, which further undermines the argument that index ownership affects them. The only statistically significant estimate is for stocks switching into the Russell 1000 and the market-to-book ratio (Column 7), but that coefficient goes in the opposite direction of HMMR's claims.

The lack of robustness originates from stayers of the other index being a poor control group for switchers. As shown earlier in Figure 1, stayers of the other index exhibit pre-existing differential trends in these key outcomes. By including them as controls, HMMR's estimation incorrectly attributes these pre-existing differential trends as an effect of index switching. Once the difference-in-differences control group is corrected, these spurious findings disappear.

3.1.4. Revisiting Board Independence and Corporate Governance

We now examine the impact of index ownership on governance. HMMR claims that a rise in index ownership reduces board independence but does not affect other governance measures, including the adoption of poison pills, supermajority voting requirements, limits on shareholders' ability to call a special meeting or act by written consent, and dual-class shares. Table 3, Panel A, Column 9, reproduces their results for board independence. In HMMR's model, switching to the Russell 2000 (which increases index ownership) predicts a decrease in board independence, while switching to the Russell 1000 (which decreases index ownership) shows no corresponding increase. They interpret these findings as evidence that index funds engage in less monitoring.

Unlike the other outcomes, the corrected specification shows similar point estimates for board independence (Panel B, Column 9). However, the lack of symmetry between the two types of index switches remains. This asymmetry casts doubt on their claim that greater index ownership reduces board independence. If index ownership decreases board independence, why does board independence *not* improve for stocks switching to the Russell 1000? The inconsistency also suggests that the finding might not hold up under a combined difference-in-differences analysis. We now turn to those combined difference-in-differences estimates.

3.2. The Combined Difference-in-Differences

We next estimate the combined specification in Equation (4) that simultaneously looks at both difference-in-differences. As noted in Section 1.3, one can increase statistical power and test for the expected symmetric effects of stocks switching indexes in opposing directions by analyzing the two sources of index switching variation in a single specification. Table 4 presents the results.

The combined specification confirms earlier findings on how index assignment influences (or does not influence) a stock's ownership structure. The point estimate on $\{R2000_{post} - R2000_{pre}\} \times PostAssignment_t$ indicates that index ownership is, on average, about 1.20 percentage points higher (lower) after stocks switch into the Russell 2000 (1000) index (Table 4, Column 1, p-value < 0.01). As expected, these results align with the subsample estimates (Table 3, Panel B, Column 1) but are more precisely estimated. The standard error is approximately 25 percent smaller in the combined difference-in-differences. We continue to find little evidence that index switching affects active fund ownership. The coefficient for active fund ownership decreases in magnitude and remains statistically insignificant in the combined specification (Table 4, Column 2).

<Table 4 About Here>

More importantly, we continue to find no evidence that index switching—and consequently, changes in index ownership—predicts managerial incentives, profitability, or firm value. Index switching does not correlate with pay-for-performance sensitivity (Table 4, Column 3), managerial pay from equity (Column 4), *Q* (Columns 5-6), the market-to-book ratio (Column 7), or ROA (Column 8). Once again, the corrected estimates challenge HMMR's claim that increased index ownership is linked to outcomes that may indicate less investor monitoring.

Moreover, we now find no evidence that index switching predicts simultaneous changes in board independence (Column 9). The asymmetry in the subsample estimates explains the non-result for board independence. This finding undermines HMMR's argument that index switching —and the related changes in index ownership—immediately impact board independence.

3.4. AGK's Specification versus the Combined Difference-in-Differences

The corrected difference-in-differences estimations yield similar results to the AGK specification that includes stock-level fixed effects [i.e., Equation (6)]. Table 5 reports these findings. Switching to the AGK specification, we find a 1.29 percentage-point shift in index ownership for stocks that switch indexes (Column 1, p-value < 0.01), which is very similar to the 1.20 percentage-point estimate from the combined difference-in-differences estimation (Table 4, Column 1). Moreover, the AGK specification continues to show no evidence of a change in active ownership (Table 5, Column 2), pay-for-performance sensitivity (Column 3), the proportion of compensation received as equity (Column 4), Tobin's Q and total Q (Columns 5-6), the market-to-book ratio (Column 7), ROA (Column 8), and board independence (Column 9).

<Table 5 About Here>

In fact, the estimates of the combined difference-in-differences and AGK's specification are quite similar. The similarity is striking given AGK's different choice of controls and sampling technique. For example, HMMR's sample only includes stocks that are within +/- 100 ranks around the cutoff thresholds each year, while AGK's sample includes the bottom (top) 500 stocks of the Russell 1000 (2000) index each year. HMMR also limit their sample to three years around each switch, while AGK does not. In Appendix Table A1, we show that the findings remain similar if we instead estimate the AGK specification on HMMR's smaller sample.

3.5. Potential Tradeoffs of Cross-sectional versus Fixed Effects Specifications

Overall, with this minor correction, the findings from HMMR's difference-in-differences specification now align with those of the existing literature, with one possible exception—board independence. AGK (2016) documents indexers' focus on increasing board independence and argues that their ownership predicts greater board independence. Given this, the non-findings regarding board independence in Tables 4 and 5 seem to conflict with AGK (2016).

However, an analysis of board independence using AGK's 2016 cross-sectional specification [i.e., Equation (5)] both explains this discrepancy and highlights the importance of specification choice. Table 6 presents this cross-sectional estimate. The board independence finding is considerably different when stock fixed effects are excluded. Specifically, there is now a positive relationship between Russell 2000 inclusion and board independence, which aligns with the findings of AGK (2016), who examined an earlier sample period, 1998-2006.

The variation in findings might be due to the slow-changing nature of these outcomes. Both the HMMR specification and the AGK specification with stock-level fixed effects implicitly assume an immediate effect of index switching on the outcome being examined. However, it is uncertain whether a change in index ownership should be expected to immediately influence sticky governance outcomes, such as board independence. For outcomes that develop slowly, using a cross-sectional approach without stock fixed effects may be more appropriate to capture the true effect of index ownership. This idea is similar to McKinnish (2008)'s argument that fixed effect estimations can cause researchers to draw incorrect conclusions when the dependent variable reacts to sustained rather than transitory changes in the independent variable.

<Table 6 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.

4. Which Investors are Displaced by Index Ownership?

While there is no evidence that index switching affects a stock's level of active fund ownership, that does not negate the possibility of other significant changes in ownership structure. We now assess how index membership matters for other dimensions of a company's ownership, including the number of institutional owners and their average ownership stake. As noted in Corum et al. (2023), the ownership type displaced by index ownership likely matters for the monitoring of managers. However, there is little understanding of which investors are displaced by indexers, and the corrected HMMR findings now provide no new insights on that front.

Because active mutual fund ownership does not change with index assignment, the shift in index fund ownership must occur at the expense of other types of ownership. However, previous work finds limited evidence of a shift in total institutional ownership with index assignment, suggesting that the increase in index ownership does not come solely from a drop in retail investors (e.g., Appel, Gormley, and Keim, 2016; Wei and Young, 2024). Table 7, Column 1 confirms that finding using both the combined difference-in-differences specification and AGK's estimation.

<Table 7 About Here>

While total institutional ownership does not change, we do find evidence of a shift in the number of institutional investors holding the stock. On average, stocks assigned to the Russell 2000 are held by eight to fourteen fewer institutional investors (Table 7, Column 2). Relative to the average number of institutional investors that hold a stock (195), this corresponds to a 4.1 to 7.2 percent change. This finding is robust to using either the HMMR specification (Panel A) or that of AGK (Panel B) and suggests that the increase in ownership by index funds (and their larger fund families) partly comes at the expense of ownership by other institutional investors.

The institutional investors displaced by index investors are those with smaller ownership stakes and those where the stock represented a smaller share of their overall portfolio. Table 7, Columns 3-6, present these findings. On average, stocks moving to the Russell 2000 experience a drop of nine to fourteen institutional investors with an ownership stake smaller than one percent of the outstanding market capitalization (Column 3). The drop persists even among institutional investors with less than 0.1% of the outstanding market capitalization (Column 4). The drops are similar when we instead look at investors where the stock represents a small proportion of their portfolio. Stocks in the Russell 2000 have, on average, seven to twelve fewer institutional investors where the stock accounts for less than 1% or 0.1% of their portfolio (Columns 5-6).

These findings suggest a shift in ownership away from institutional investors with small ownership stakes and toward a few institutions with significant ownership stakes. The market for index funds is concentrated, with more than 75% of index fund assets managed by 'The Big Three' institutional investors—BlackRock, State Street, and Vanguard. Moreover, The Big Three tend to hold significant ownership positions. For example, Gormley et al., (2023) find that, on average, the ownership stakes of BlackRock, State Street, and Vanguard, accounted for 6.2%, 1.9%, and 4.9% of a stock's market capitalization between 2014 and 2019. Because The Big Three will account for much of the observed increase in index ownership for stocks moving to the Russell 2000, the findings suggest a shift in ownership toward investors with greater ability to be influential because of their large ownership stake. The large, direct economic exposure of these investors also implies they have similar monitoring motives to activist investors, despite a smaller indirect (via flow-for-performance) monitoring incentive (Lewellen and Lewellen, 2022).

We find no evidence that index assignment correlates with non-financial blockholder ownership. Israelsen, Schwartz-Ziv, and Weston (Forthcoming) argue that such ownership affects corporate governance and correlates with Russell index assignments, potentially confounding inferences about the effects of index ownership in the Russell setting. However, as Appendix Table A2 shows, index switching does not predict changes in non-financial blockholder ownership across any of the index switching specifications. These non-findings show that differences non-financial blockholder ownership do not drive the observed institutional ownership differences.[8]

[8] However, in untabulated results, we do find that the AGK specification without stock fixed effects does show a correlation between index assignment and non-financial blockholder ownership. These findings suggest a potential

While both specifications display similar patterns in how different types of institutional ownership change with index assignment, the estimated magnitudes differ significantly. For instance, the HMMR specification (Table 7, Panel A) indicates that shifts in other ownership are 50% larger than those shown by the AGK specification (Panel B). In the next section, we show that these differences are probably due to two additional issues with the HMMR specification.

5. Additional Potential Specification Issues with HMMR

The above correction does not address other potential issues with the HMMR approach. First, HMMR's sample construction overlooks subsequent index assignments, which may cause the difference-in-differences estimation to underestimate the impact of index assignment. Second, the HMMR estimation fails to control for the variable that drives index switching—changes in market capitalization—resulting in possible omitted variable bias. We now discuss these two potential problems and how to address them. These corrections create an even closer alignment between the HMMR and AGK estimates, eliminating the observed differences in magnitude seen in Table 7.[9]

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

While estimating Equations (2) and (3) separately ensures a proper control group for each difference-in-differences analysis, it does not address a second issue related to how HMMR selects its sample. For each cohort, HMMR creates a sample using a six-year window—three years before and three years after the switch. However, HMMR's sample selection and estimation fail to consider index status during non-cohort years. For instance, a stock that switches from the Russell

difficulty in interpreting cross-sectional estimates in the Russell setting. However, the differences in non-financial blockholder ownership disappear when dropping AGK's Float control and using their alternative sampling approach (see Appel, Gormley, and Keim, 2016; Appendix Table A.9). To construct *non-financial blockholder ownership*, we use the blockholder classifications provided by Israelsen, Schwartz-Ziv, and Weston (Forthcoming) and Schwartz-Ziv and Volkova (2025), which are available on WRDS. We thank the authors for making the full dataset accessible.

[9] In Appendix A, we provide yet additional discussion on why these next two corrections are likely necessary adjustments to the HMMR difference-in-differences specification.

1000 to the Russell 2000 would have all three post-switch observations in the sample, even if it switched back a year later. This is problematic because the estimation implicitly assumes index status remains constant after the switch. Specifically, it assumes that a stock labeled as 'treated' with Russell 2000 assignment in the cohort year is also 'treated' with that same assignment in subsequent years. A similar issue exists for non-switchers because HMMR's sampling approach ignores switches that might occur for these controls in non-cohort years. A stock that remained in the Russell 1000 during the cohort year and served as a proper control in that year may not be a proper control in later years if it switches indexes. Failing to account for an observations' index status in other years will lead to underestimating the true effect of index assignment.

To further illustrate the problem with HMMR's sampling approach, suppose one wants to analyze the impact of index assignment (and higher index ownership) by analyzing stocks that switch from the Russell 1000 to the Russell 2000 in year 0. However, suppose that every treated stock (i.e., switchers) immediately switches back to the Russell 1000 one year later (i.e., in year 1), and every control stock (i.e., stayers) switches to the Russell 2000 that same year. Thereafter, assignments persist for both groups. In this case, year -1 is a valid pre-treatment observation in that cohort, and the only valid post-treatment observation is year 0. To analyze the impact of index assignment (and higher index ownership), one could look at that [-1, 0] window. However, suppose one instead looks at the -1 to +5 window using the HMMR approach and finds a negative ROA impact. It would clearly be misleading to attribute this finding to higher index ownership. In this example, the 'treated' group only has higher index ownership (because of being in the Russell 2000) in year 0, while from years +1 to +5, the 'treated' stocks have lower index ownership than the 'controls' because the two groups swapped indexes in year 1 and later. Failing to account for index assignment in non-cohort years fundamentally undermines the analysis and its conclusions.

To address this issue, one must construct the sample for each cohort to only keep observations for each stock that comprise a clean pre- and post-period free of other index switches. In the previous example, that would entail keeping only the [-1,0] observations for each stock of that cohort. This type of sample construction procedure is standard practice for stacked difference-in-differences estimations and is essential to ensure clean pre- and post-treatment periods and unbiased estimates (Gormley and Matsa, 2011, 2016; Baker, Larcker, and Wang, 2022).[10]

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

The estimates in Equations (2)-(3) have a further potential flaw. Switchers and non-switchers often experience different changes in their total market capitalization. These differential changes in market capitalization drive index switching. However, changes in market capitalization are probably connected to other company outcomes, which challenge the parallel trends assumption used in the difference-in-differences method. In other words, are the observed different trends for switchers and non-switchers caused by the index switch (and the related change in index ownership) *or* by the change in stock market capitalization that triggered the index switch?

To account for this potential weakness of the HMMR specification, we will add a lagged control for a stock's total market capitalization. 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. Because Russell indexes are reconstituted at the end of June, this control is measured before that year's index switches, and because the estimation includes stock-cohort fixed effects, this additional control will capture the

[10] This sampling procedure is not subject to the potential sample selection concern raised in Pavlova and Sikorskaya (2023, footnote 66). Within each cohort, we do not exclude all observations of a stock that switches indexes in a non-cohort year. As mentioned in Pavlova and Sikorskaya, that sampling approach could be problematic. Instead, we only exclude the subset of observations for the stock that do not match the treatment versus control assignment being assumed in the difference-in-differences estimation for that cohort.

potential importance of lagged within-stock changes in market capitalization.[11]

5.3. Importance of Additional Corrections to the HMMR Specification[12]

Generally, accounting for index status in non-cohort years and controlling for market capitalization has minimal effect on the difference-in-differences estimates. Appendix Table A3 presents the results after applying the changes outlined in Sections 5.1 and 5.2. Index assignment continues to be linked with increased index ownership, but there is no evidence of an effect on active fund ownership. Similarly, there is no evidence of an impact on managerial incentives, firm performance, or firm value. The point estimates for these outcomes are nearly the same. The findings are similar if one replaces the market capitalization controls with market capitalization *ranking* controls (Appendix Table A4),[13] or if one constructs the market capitalization controls in a way that is immune to concerns about 'bad controls' (Appendix Table A5).[14] The findings are also similar when separately estimating the two difference-in-differences [i.e., Equations (2) and (3)] with these two additional modifications (Appendix Table A6).

<Table 8 About Here>

However, the two additional modifications to HMMR's specification significantly reduce the estimated impact of index assignment on other types of institutional ownership. Table 8 shows these findings. Stocks assigned to the Russell 2000 are now held by nine fewer institutional

[11] In essence, the estimation will now attempt to compare two stocks that both experienced similar changes in market capitalization over the past year, but where that change in market capitalization only caused one of the two stocks to switch Russell indexes. The lack of perfect co-linearity between index switches and changes in market capitalization occurs because of differences in stocks' starting market capitalization.

[12] Because these additional corrections require variables not included in the posted HMMR data, we now switch to using our constructed dataset rather than HMMR's posted data.

[13] Because Russell uses market capitalization rankings, not market capitalization, to determine index assignment, one could argue that a more appropriate control might be a stock's market capitalization ranking. Whether that is true depends on assumptions on which control is more likely to correlate with the outcomes of interest.

[14] To avoid any concerns that the market capitalization controls might be impacted by index switching (thus making it a potential bad control), Appendix Table A5 replaces the cohort-year t+1 and t+2 *Ln(Mktcap)* controls with their cohort-year t=0 value, which is measured before index switching occurs in that cohort. See Appendix A.2 for details regarding the potential bad controls concern that can arise when including lagged market capitalization controls.

investors (Table 8, Column 2), which is 50% less than the decline observed without these additional corrections (Table 7, Panel A, Column 2). A similar reduction occurs when analyzing the presence of institutions with small ownership stakes (Table 8, Columns 3-4) and for institutions where the stock makes up a small part of their portfolio (Columns 5-6). The new estimated effects are now very similar to what is found using the AGK specification (Table 7, Panel B). We still find no evidence of an overall change in institutional ownership levels (Table 8, Column 1), again indicating that the increase in ownership by index funds (and their larger fund families) comes at the expense of ownership by institutional investors with smaller ownership stakes.

6. 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 of both pieces of evidence is inconclusive. Neither finding precludes the possibility that index investors monitor firms.

First, siding with management on contentious shareholder proposals does not indicate a lack of monitoring; rather, it can suggest the opposite. Contentious proposals are those where management and Institutional Shareholder Services (ISS) provide differing vote recommendations. Therefore, voting with management on such proposals, as found by HMMR, implies that index fund investors are less likely to follow ISS vote recommendations. However, the literature usually interprets this voting pattern as evidence that investors are paying *closer* attention and are *more* likely to be monitoring. Iliev and Lowry (2015) and Malenko and Malenko (2019) suggest that if fund families allocate more resources to becoming informed, they will be less inclined to follow proxy advisory recommendations blindly. Malenko et al. (2021) also show that voting against ISS can be the equilibrium outcome for more attentive investors. Supporting the idea that this voting pattern reflects increased monitoring, Iliev and Lowry (2015) observe a higher likelihood of disagreement with ISS for active mutual funds, where the benefits of being attentive are greater. Additionally, Iliev et al. (2021) find that this voting behavior positively associates with investors becoming informed before a vote, including downloading EDGAR filings.

Second, filing a 13G form instead of a 13D form is not necessarily 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 sale 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 investors' ability 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). 13G investors can also still govern through 'exit' and use their monitoring of the firm to decide whether to retain their stake, exit, or buy more (e.g., Edmans, 2009; Edmans and Manso, 2011; Edmans, Fang, and Zur, 2013).

7. Additional Findings

Finally, we report findings for other outcomes analyzed in HMMR and for investors' buy-and-hold returns, which is an alternative way to analyze the impact of index switching (and changes in index fund ownership) on a stock's investors.

7.1. Other Compensation and Governance Outcomes

HMMR examines three additional outcomes related to managerial compensation: total executive pay, a dummy variable indicating whether a golden parachute is included in the CEO's compensation package, and a dummy variable indicating whether the CEO leaves the firm in the specified year. HMMR also investigates several other governance outcomes, such as the adoption of poison pills, supermajority voting requirements, restrictions on shareholders' ability to call a special meeting or act by written consent, and dual-class shares. HMMR contends that there is no evidence that index switching influences these other outcomes, except for total compensation. They find that total pay is higher for stocks assigned to the Russell 2000.

The AGK and corrected HMMR specifications produce similar results. Appendix Table A7 details these findings. There is weak evidence of an increase in total compensation across different specifications, but the size is less than a quarter of what is shown in HMMR's original estimates (see Table 9A, Column 2 of HMMR). For the other outcomes, we also find little consistent evidence that index switching has an immediate effect on other aspects of governance, compensation, or managerial turnover. The only exception is some evidence that increased index ownership is linked to fewer restrictions on shareholders' ability to act by written consent, which would generally be seen as a governance improvement (Column 8).

7.2. Buy-and-hold Abnormal Returns

One can also question whether HMMR uses proper measures of managerial performance. Tobin's Q and total q can also capture overvaluation, growth opportunities, and other factors, making their use as a measure of performance problematic. For example, a lower Q could reflect a company where a manager has properly invested in valuable growth opportunities. Likewise, ROA does not necessarily capture long-term value or performance. A firm that accepts moderately profitable and positive-NPV projects might have a lower ROA.

An alternative way to assess the impact of index switching on value is to analyze buy-and-hold abnormal returns (BHAR) for switchers versus non-switchers in a stacked simple difference estimation that is similar to the combined difference-in-differences specification. Appendix Table A8 presents these findings. Using both a market model and a 3-factor model, and year -1 as the base year for calculating subsequent returns, we find relatively little evidence of a difference in the 1-, 2-, or 3-year BHARs between switchers and non-switchers and no evidence that higher index ownership (i.e., inclusion in the Russell 2000 index) reduces buy-and-hold returns.[15] These findings bolster the conclusion that index switching is not clearly associated with firms' value.[16]

8. Concluding Remarks

We reveal that Heath et al.'s problematic specification overstates the effects of index switching on active 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. Most of Heath et al.'s published findings disappear after making one simple correction in their posted code and data.

Instead of changing a stock's level of active fund ownership, we find that being assigned to the Russell 2000 index and the resulting increase in index-linked ownership pushes out institutional investors who are less likely to monitor companies—such as those for whom the stock is a smaller part of their overall portfolio or who hold smaller positions. The governance implications of that ownership shift are less clear, which might explain why there is little evidence

[15] If anything, the 3-factor model shows marginally higher 2- and 3-year BHARs for stocks switching to the Russell 2000, suggesting a modest positive long-run value consequence of index investing.

[16] However, these non-findings must also be interpreted with caution. Inelastic index demand could induce changes in market values (e.g., Chang, Hong, and Liskovich, 2015). For example, the forced buying of index funds might increase market values and potentially offset any negative value impact of less investor monitoring.

that index assignment leads to changes in managerial incentives or corporate performance.

By correcting their specification and comparing different methodologies, 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 clear picture of index investing's impact (and non-impact) on corporate governance and performance.

As the rise of index investing continues to reshape the investment landscape, its impact is still heavily debated. While our findings do not settle the debate over whether index investors monitor as much as active funds, they do show a lack of evidence to suggest that index funds monitor less than the investors they displace. 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 market outcomes.

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Appendix

A. Further Arguments to Support Section 5's Modifications

One might question whether the difference-in-differences estimation needs to account for index status in non-cohort years (Section 5.1) and control for market cap (Section 5.2). While these additional modifications do not have much impact on the outcomes analyzed by HMMR, we provide below additional discussion on why those two adjustments are likely necessary and why potential concerns about selection bias or bad controls are likely unwarranted.

A.1. Failure to control for index status in non-cohort years

One might worry that our recommended sampling approach introduces selection biases. It is unclear why it would. With our proposed sampling approach, every stock's observation in the year before the assignment (i.e., in year -1) and the year of the assignment (i.e., in year 0) is always included. These years form the clean window for assessing the treatment effect and, by construction, are valid for every stock. They are never dropped, mitigating concerns about selection bias. We only drop observations outside that [-1, 0] window when they are no longer valid comparisons. This occurs when a stock's index assignment is inconsistent with the underlying assumption of the estimation (which assumes non-switchers are in their original index during the entire sample window, and switchers are in the new (starting) index in the post-(pre-)switch window). Our proposed approach follows the stacked difference-in-differences methodology developed by Gormley and Matsa (2011). That sampling technique is why their stacked estimator avoids a bias common to staggered difference-in-differences (Baker, Larcker, and Wang, 2022). The HMMR estimation fails to follow that standard sampling technique.

A.2. Failure to control for market capitalization

One might argue that HMMR's inclusion of stock fixed effects already controls for market capitalization. It does not. Stock fixed effects only control for time-invariant characteristics of the firm, but a stock's market capitalization is not time-invariant. One can also easily see that stock fixed effects do not control for market cap (or changes in it) by adding *Ln(Mktcap)* as a control variable to the HMMR estimation. *Ln(Mktcap)* is not collinear with the fixed effects, and it does not drop out of the estimation. And as we show, its inclusion changes the observed estimates.

One might next worry that adding *Ln(Mktcap)* controls will introduce a bad control problem and estimation bias. However, this concern is likely unwarranted for several reasons.

First, it is the change in market capitalization that drives the change in index assignment. It drives treatment and hence should probably be controlled for because changes in market capitalization are typically different for switchers and non-switchers ***irrespective*** of any potential impact that treatment might have on the market capitalization itself. Second, there is little evidence that index switching affects stock returns (see Appendix Table A8), and by implication, market capitalization.[17] If index switching does not affect market capitalization, controlling for market capitalization cannot introduce a bad control problem. Finally, our market capitalization control is measured before index assignment (i.e., treatment), which further undercuts a potential bad control concern. Specifically, the specification controls for year *t*'s end-of-May market capitalization. That market capitalization occurs *before* index assignment, which is measured from July (*t*) to June (*t*+1). Thus, the market capitalization control cannot yet be affected by the index assignment (and its potential impact on index ownership). That is different than the outcome variables analyzed in

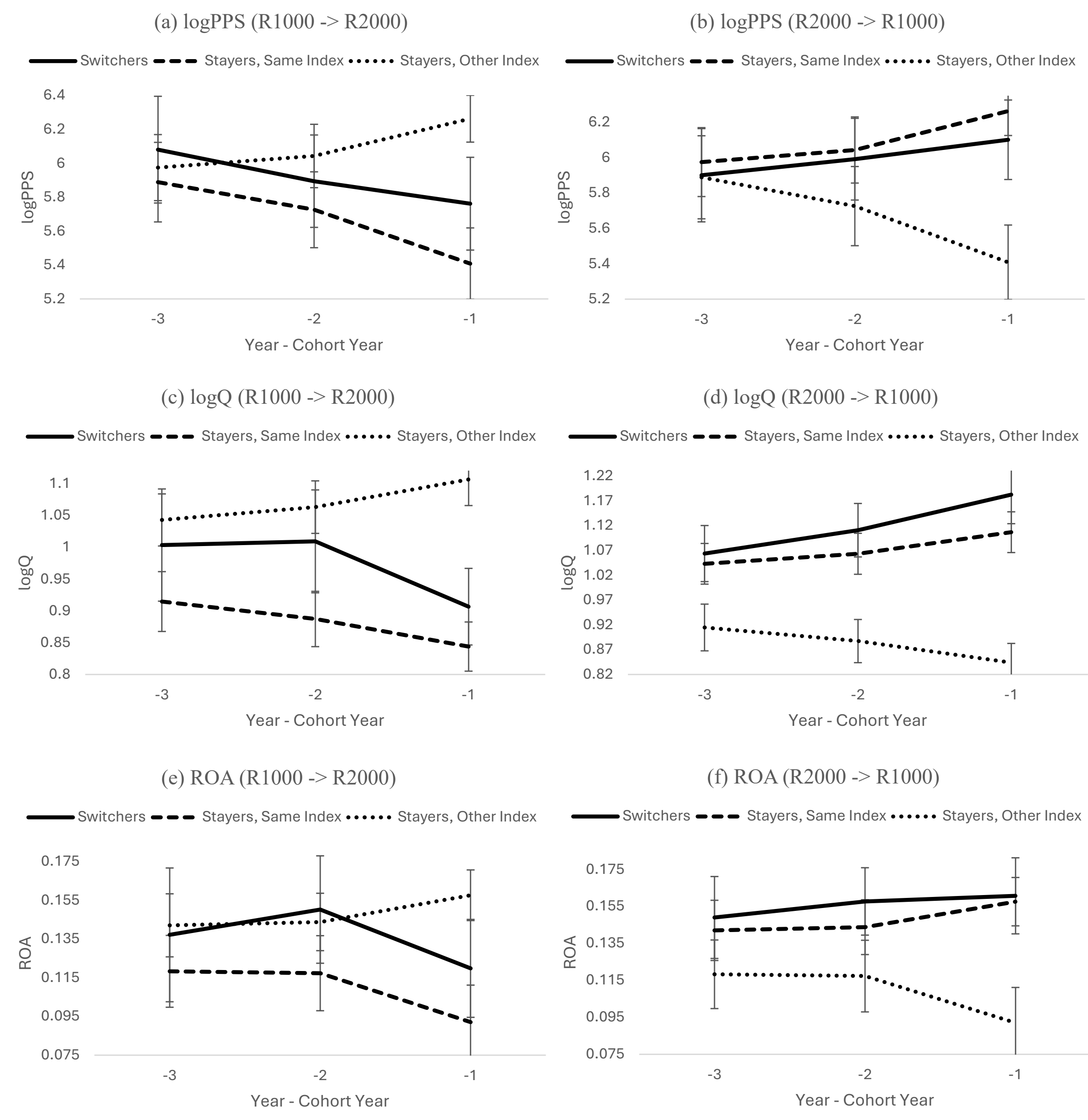
[17] If index assignment affects firms' stock issuance (Sammon and Shim, 2025), then market capitalization could be changing even in the absence of differences in stock returns. However, in untabulated estimations, we find no evidence in our setting that index switching predicts changes in firms' stock issuance activity, their number of outstanding shares, or their vested shares from stock-based compensation. This non-finding holds in both the HMMR and AGK specifications. The findings also suggest firms are not strategically issuing shares to affect their index assignment.

the regression, which are usually as of year t+1 and measured no earlier than Dec. 31.

Admittedly, one could argue that even controlling for lagged market capitalization is problematic if the change in index ownership persists and immediately affects market capitalization. Then, at some point, even the lagged market capitalization control (i.e., those for years t+1 and t+2) could be affected by the earlier index switch. If true, there could be a rather nuanced bad control issue in years two and later of the estimation. Unfortunately, that only says that researchers might be "damned if you do" (i.e., control for market capitalization) and "damned if you don't" (i.e., do not control for it). But as noted earlier, there is no evidence that index switching affects market capitalization. Moreover, as discussed in Section 5.3, replacing the cohort-year t+1 and t+2 *Ln(Mktcap)* controls with their cohort-year t=0 value, which is measured before index switching occurs in that cohort, does not meaningfully impact the HMMR findings (Appendix Table A5). In combination, these findings suggest bad controls are an unlikely concern.

Figure 1
Pre-trends

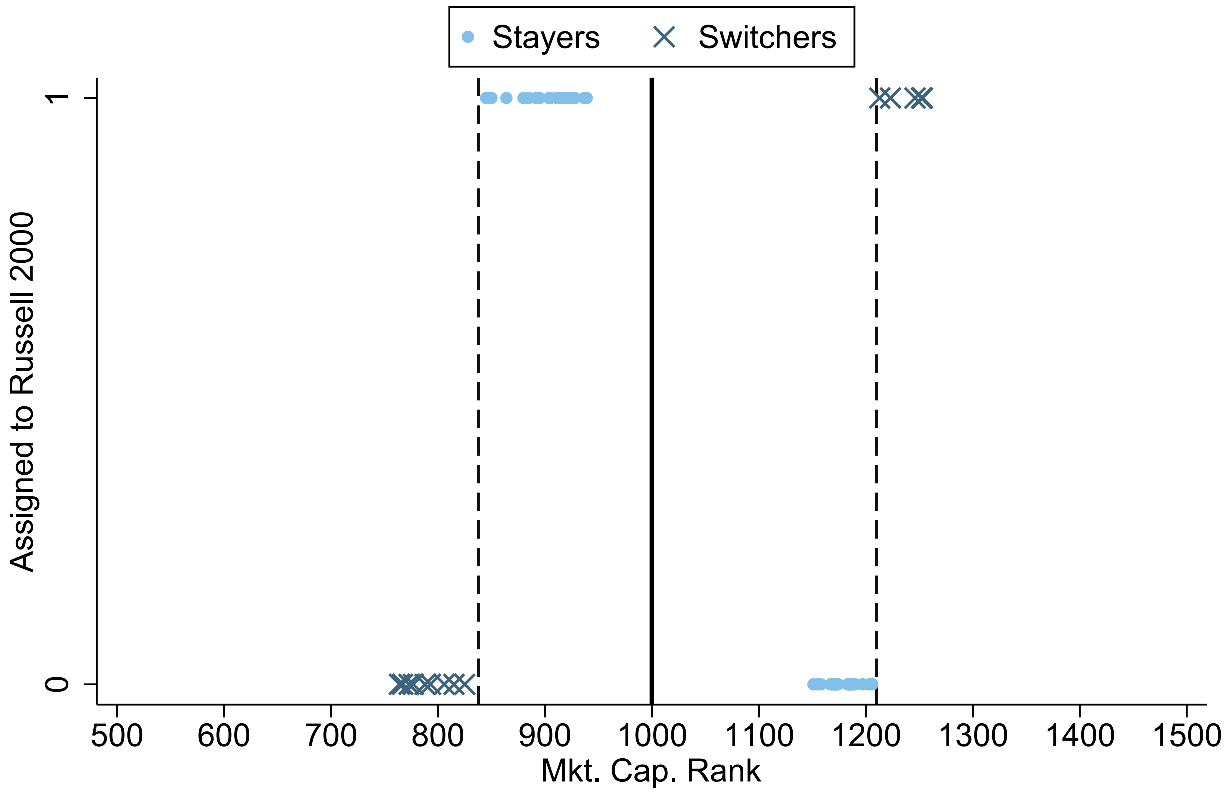
This figure plots the time series of key HMMR outcome variables over the three years prior to index assignment, separately for switchers, stayers in the same index, and stayers in the other index. Panels (a) and (b) show the average pay-for-performance sensitivity for firms in the Russell 1000 and Russell 2000, respectively, in the year prior to the cohort year; panels (c) and (d) show the average Tobin's q for the same groups; and panels (e) and (f) show the average ROA. Error bars indicate 95% confidence intervals.



ALT TEXT: Graphs showing differential trends in pay-for-performance, log Q, and ROA for switchers versus stayers of the other index in the three years before the switch.

Figure 2
Selection of Cohort Samples

In this figure, we replicate Panel B of Figure 2 from Heath et al. (2022) 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.

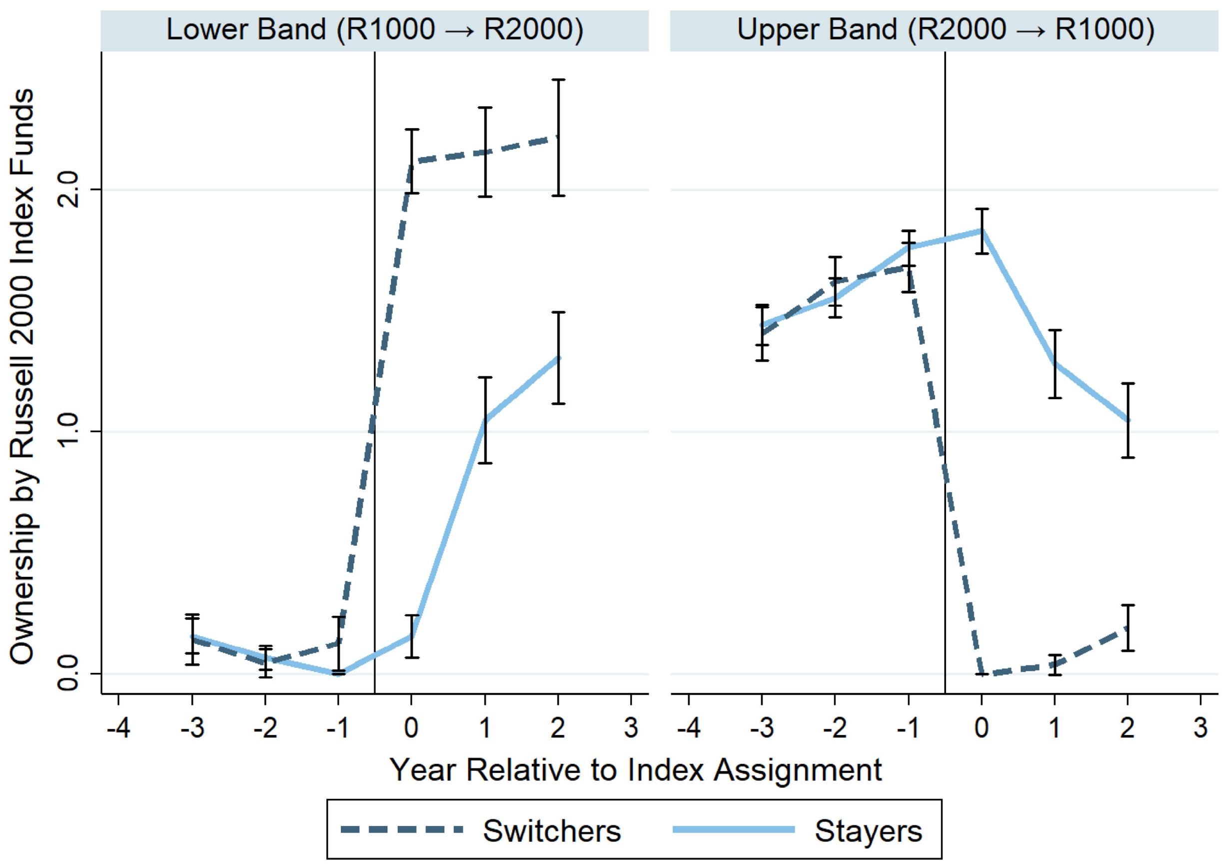


(b) 2007 Cohort Firms

ALT TEXT: Graphical representation showing Russell 1000 and 2000 index assignments of the 2007 cohort based on observed market capitalization rank and whether the stock switches indexes.

Figure 3
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.



ALT TEXT: Graphs showing Russell 2000 index fund ownership for stocks in the six years surrounding index switches, broken out by whether stocks switched indexes or not and whether the switch was to the Russell 2000 or Russell 1000 indexes.

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)	2,527	1,558	1,077	2,168	4,309	4,522
$IndexOwn^{R2000}$	1.05%	1.07%	0.00%	0.88%	2.47%	4,522
$IndexOwn^{R1000}$	0.11%	0.14%	0.00%	0.00%	0.31%	4,522
$IndexOwn^{All}$	9.16%	5.57%	1.99%	8.95%	16.64%	4,522
$ActiveOwn$	25.41%	12.24%	8.80%	26.01%	40.31%	4,522
logPPS	5.88	1.35	4.52	5.91	7.45	3,353
logTotalComp	8.27	1.80	7.62	8.54	9.47	3,134
EquityFrc	0.46	0.21	0.18	0.46	0.73	3,058
GldnPara	0.74	0.44	0	1	1	2,602
CEOTurnover	0.11	0.32	0	0	1	3,821
BoardIndep	0.77	0.12	0.60	0.80	0.90	2,557
E-index	3.82	1.22	2	4	5	2,602
PoisonPill	0.21	0.41	0	0	1	2,602
Supermaj	0.95	0.22	1	1	1	2,602
LimSpecMeet	0.50	0.50	0	0	1	2,602
WrConsent	0.49	0.50	0	0	1	2,602
DualClass	0.06	0.23	0	0	0	2,602

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 $IndexOwn_{jt}^{All}$, 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 * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	HMMR sample		Constructed sample	
	$IndexOwn_{jt}^{All}$	$ActiveOwn_{jt}$	$IndexOwn_{jt}^{All}$	$ActiveOwn_{jt}$
$R1000 \rightarrow R2000_j \times$	1.31***	-2.21**	1.38***	-2.25**
$PostAssignment_t$	(0.34)	(0.75)	(0.35)	(0.77)
$R2000 \rightarrow R1000_j \times$	-1.20***	1.60**	-1.14***	1.60**
$PostAssignment_t$	(0.24)	(0.57)	(0.26)	(0.60)
Observations	4,649	4,649	4,521	4,521
Adjusted R^2	0.870	0.759	0.873	0.756
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 specification 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 fixed effects, firm-by-cohort fixed effects, and year-by-cohort fixed effects as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, 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) $\log(q)$	(6) log-(q^{TOT})	(7) $\log(\frac{M}{B})$	(8) ROA	(9) Board Indep
A. HMMR specification using HMMR sample									
$R1000 \rightarrow R2000_j \times PostAssignment_t$	1.31***	-2.21**	-0.43***	-0.06**	-0.10***	-0.21***	-0.12**	-0.03***	-0.03***
	(0.34)	(0.75)	(0.11)	(0.02)	(0.03)	(0.05)	(0.04)	(0.01)	(0.01)
$R2000 \rightarrow R1000_j \times PostAssignment_t$	-1.20***	1.60**	0.27**	0.03**	0.01	0.06*	-0.03	0.00	0.00
	(0.24)	(0.57)	(0.10)	(0.01)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)
Observations	4,649	4,649	3,445	3,138	4,296	3,403	4,552	4,188	2,613
R^2	0.870	0.759	0.697	0.699	0.849	0.851	0.816	0.738	0.769
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									
$R1000 \rightarrow R2000_j \times PostAssignment_t$	1.17***	-0.98	-0.07	-0.01	-0.03	-0.03	-0.07	-0.02	-0.04***
	(0.32)	(0.95)	(0.13)	(0.02)	(0.03)	(0.06)	(0.05)	(0.01)	(0.01)
Observations	1,618	1,618	1,289	1,191	1,459	1,162	1,589	1,519	1,022
R^2	0.896	0.781	0.760	0.686	0.788	0.859	0.763	0.701	0.829
$R2000 \rightarrow R1000_j \times PostAssignment_t$	-1.00***	0.94	0.11	-0.00	-0.02	-0.02	-0.06**	-0.01	0.01
	(0.27)	(0.72)	(0.10)	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
Observations	3,031	3,031	2,156	1,945	2,837	2,241	2,963	2,669	1,591
R^2	0.864	0.759	0.686	0.727	0.869	0.869	0.830	0.766	0.748
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4
Index Switching and Key Variables - Combined Difference-in-Differences

This table presents the estimation results of the combined difference-in-differences specification in Equation (4) that avoids problematic control group comparisons across selected outcome variables. 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 firm-by-cohort fixed effects and year-by-cohort-by-pre-cohort-assignment fixed effects. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, 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) log(q)	(6) log-(q^{TOT})	(7) log($\frac{M}{B}$)	(8) ROA	(9) Board Indep
$R2000_{post} - R2000_{pre}$	1.20***	-0.80	-0.11	0.01	0.01	0.02	0.02	0.00	-0.01
$\times\ PostAssignment_t$	(0.22)	(0.61)	(0.08)	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)
Observations	4,649	4,649	3,445	3,136	4,296	3,403	4,552	4,188	2,613
R^2	0.875	0.767	0.723	0.712	0.863	0.870	0.827	0.750	0.778
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Cohort × $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5
Index Switching and Key Variables - AGK Specification

This table presents the estimation results of the AGK specification in Equation (6) that includes stock-level fixed effects across selected outcome variables. Our constructed sample 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 size controls (i.e., end-of-May market capitalization), firm fixed effects, and year fixed effects in all specifications. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, 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) $\log(q)$	(6) log-(q^{TOT})	(7) $\log(\frac{M}{B})$	(8) ROA	(9) Board Indep
$R2000_{jt}$	1.29***	-0.39	0.04	0.00	-0.01	-0.01	0.02	-0.00	0.00
	(0.11)	(0.44)	(0.06)	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)
Observations	8,181	8,181	6,613	6,660	7,649	6,818	7,456	7,634	5,580
R^2	0.823	0.734	0.785	0.619	0.850	0.842	0.768	0.648	0.779
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6
Board Independence - AGK Specification without Firm FEs

This table presents the results for board independence using the cross-sectional AGK specification in Equation (5). Our constructed sample is used for this table. The dependent variable, *BoardIndep*, is defined as the fraction of board members classified as independent. The key independent variable, $R2000_{jt}$, is an indicator equal to one if firm j is assigned to the Russell 2000 index in year t. As specified in Equation (5), we include year fixed effects and AGK's controls for float-adjusted market cap, end-of-May market capitalization, and banding. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	BoardIndep
$R2000_{jt}$	0.31***
	(0.10)
Firm FE	No
Year FE	Yes
Observations	5,780
R^2	0.075

Table 7
Index Switching and Institutional Ownership

This table presents our analysis of investors displaced by index ownership. Panel A presents results from the combined difference-in-differences specification described in Equation (4) using our constructed HMMR sample, while Panel B employs the AGK specification with firm fixed effects as in Equation (6) using the constructed AGK sample. The dependent variables are defined as follows: IO^{Total} is the fraction of shares outstanding held by 13F institutional investors; $\#Inst$ is the number of 13F institutional investors holding the firm's stock; $Share < 1\%$ is the number of institutional investors with ownership stakes below 1% of shares outstanding; $Share < 0.1\%$ counts those with ownership below 0.1%; $PFwt < 1\%$ is the number of institutional investors for whom the stock constitutes less than 1% of their portfolio; $PFwt < 0.1\%$ counts those for whom it accounts for less than 0.1% of their portfolio. We include firm-by-cohort fixed effects, year-by-cohort-by-pre-cohort-assignment fixed effects, firm fixed effects, and year fixed effects, as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) IO^{Total}	(2) $\#Inst$	(3) $Share <$ 1%	(4) $Share <$ 0.1%	(5) $PFwt <$ 1%	(6) $PFwt <$ 0.1%
A. Combined diff-in-diffs (Eq. 4) on constructed sample						
$R2000_{post} - R2000_{pre\ j} \times$ $PostAssignment_t$	-0.25	-14.06***	-13.96***	-12.08***	-13.28***	-11.73***
	(1.30)	(4.23)	(4.06)	(3.10)	(3.92)	(2.96)
Observations	4,427	4,427	4,427	4,427	4,427	4,427
R^2	0.767	0.747	0.742	0.765	0.758	0.786
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Cohort × $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes
B. AGK specification (Eq. 6) on AGK sample						
$R2000_{jt}$	-0.21	-8.54***	-9.19***	-8.68***	-7.84***	-6.98***
	(1.13)	(3.01)	(3.17)	(2.27)	(2.80)	(2.16)
Observations	9,140	9,140	9,140	9,140	9,14z0	9,140
R^2	0.733	0.806	0.810	0.838	0.807	0.810
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 8
Index Switching and Institutional Ownership - Additional Modifications

This table presents our analysis of investors displaced by index ownership using the combined difference-in-differences specification in Equation (4), with two additional modifications: (i) accounting for index assignment in non-cohort years, and (ii) controlling for the endogenous determinant of index switching, i.e., changes in firm size (measured by end-of-May market capitalization). The dependent variables are defined as follows: IO^{Total} is the fraction of shares outstanding held by 13F institutional investors; $\#Inst$ is the number of 13F institutional investors holding the firm's stock; $Share < 1\%$ is the number of institutional investors with ownership stakes below 1% of shares outstanding; $Share < 0.1\%$ counts those with ownership below 0.1%; $PFwt < 1\%$ is the number of institutional investors for whom the stock constitutes less than 1% of their portfolio; $PFwt < 0.1\%$ counts those for whom it accounts for less than 0.1% of their portfolio. We use the constructed HMMR sample for these estimations. We include size controls, firm-by-cohort fixed effects, and year-by-cohort-by-pre-cohort-assignment fixed effects in all specifications. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) IO^{Total}	(2) $\#Inst$	(3) $Share <$ 1%	(4) $Share <$ 0.1%	(5) $PFwt <$ 1%	(6) $PFwt <$ 0.1%
$R2000_{post} - R2000_{pre\ j} \times$	1.03	-8.88**	-8.96**	-9.26***	-8.44**	-8.54***
$PostAssignment_t$	(1.53)	(4.10)	(3.88)	(2.86)	(3.72)	(2.78)
Observations	3,790	3,790	3,790	3,790	3,790	3,790
R^2	0.794	0.810	0.806	0.826	0.819	0.838
Size control	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Cohort × $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes

Internet Appendix: Supplementary Tables

Table A1
Index Switching and Key Variables - AGK Specification on Constructed Sample

This table presents the estimation results of the AGK specification in Equation (6) on our constructed two-band sample, across selected outcome variables. Our constructed sample is used for this table. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. As specified in Equation (5), we include year fixed effects and AGK's controls for float-adjusted market cap, end-of-May market capitalization, and banding. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, 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) $\log(q)$	(6) log-(q^{TOT})	(7) $\log(\frac{M}{B})$	(8) ROA	(9) Board Indep
$R2000_{jt}$	1.35***	-0.23	0.22**	-0.00	0.01	0.05*	0.05*	0.00	-0.01
	(0.27)	(0.69)	(0.10)	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)
Observations	3,346	3,346	2,615	2,321	3,094	2,723	3,275	3,312	2,021
R^2	0.876	0.776	0.678	0.694	0.871	0.870	0.821	0.784	0.751
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A2
Index Switching and Non-financial Blockholder Ownership

This table examines the effect of index switching on non-financial block ownership. The dependent variable is *non-financial blockholder ownership*, defined as the fraction of a firm's shares held by non-financial blockholders, using blockholder classification data from Israelsen, Schwartz-Ziv, and Weston (Forthcoming) and ownership data from Schwartz-Ziv and Volkova (2025). Columns 1 and 2 present the estimation results of the separate difference-in-differences specifications in Equations (2) and (3) that avoids problematic control group comparisons across selected outcomes. Columns 3 reports the estimation results of the combined difference-in-differences specification in Equation (4). Columns 4 and 5 show the estimation results of the Appel, Gormley and Keim (2019) specification with firm fixed effects in Equation (6). Columns 1–4 use HMMR's two-band sample while Column 5 uses the larger AGK sample that includes the bottom (top) 500 stocks of the Russell 1000 (2000) index each year. We include size controls, firm-by-cohort fixed effects, year-by-cohort fixed effects, year-by-cohort-by-pre-cohort-assignment fixed effects, firm fixed effects, and year fixed effects, as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Non-financial blockholder ownership (%)				
$R1000 \rightarrow R2000_j \times$	0.01				
$PostAssignment_t$	(0.01)				
$R2000 \rightarrow R1000_j \times$		-0.02			
$PostAssignment_t$		(0.01)			
$R2000_{post} - R2000_{pre\ j} \times$			0.01		
$PostAssignment_t$			(0.01)		
$R2000_{jt}$				-0.00	-1.20
				(0.01)	(0.94)
HMMR sample	Yes	Yes	Yes	Yes	No
AGK sample	No	No	No	No	Yes
Firm × Cohort FE	Yes	Yes	Yes	No	No
Year × Cohort FE	Yes	Yes	No	No	No
Year × Cohort × $R2000_{pre}$ FE	No	No	Yes	No	No
Firm FE	No	No	No	Yes	Yes
Year FE	No	No	No	Yes	Yes
Observations	1,602	2,919	4,521	3,871	8,181
R^2	0.709	0.718	0.716	0.720	0.704

Table A3
Index Switching and Key Variables - Additional Modifications

This table presents the estimation results of the combined difference-in-differences specification in Equation (4) across selected outcome variables, with two additional modifications: (i) accounting for index assignment in non-cohort years when constructing the sample (as discussed in Section 5.1), and (ii) controlling for the endogenous determinant of index switching, i.e., changes in firm size measured by end-of-May market capitalization (as discussed in Section 5.2). We use the constructed HMMR sample for these estimations. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include size controls, firm-by-cohort fixed effects, and year-by-cohort-by-pre-cohort-assignment fixed effects in all specifications. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, 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) $\log(q)$	(6) log-(q^{TOT})	(7) $\log(\frac{M}{B})$	(8) ROA	(9) Board Indep
$R2000_{post} - R2000_{pre}$ $\times$ $PostAssignment_t$	1.70*** (0.26)	-0.66 (0.70)	0.03 (0.09)	0.01 (0.01)	0.01 (0.01)	0.03 (0.03)	0.02 (0.03)	0.01 (0.01)	-0.02 (0.01)
Observations	3,848	3,848	2,870	2,575	3,548	2,874	3,773	3,509	2,188
R^2	0.890	0.793	0.756	0.725	0.885	0.900	0.848	0.797	0.786
Size control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm $\times$ Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A4
Index Switching and Key Variables - Controlling for Ranking

This table presents the estimation results of the combined difference-in-differences specification in Equation (4) for selected outcome variables, additionally accounting for index assignment in non-cohort years and controlling for the endogenous determinant of index switching, i.e., changes in firm size, though measured by end-of-May market capitalization *ranking* instead of market capitalization. We use the constructed HMMR sample for these estimations. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include firm-by-cohort fixed effects and year-by-cohort-by-pre-cohort-assignment fixed effects. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, 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) log(q)	(6) log-(q^{TOT})	(7) log($\frac{M}{B}$)	(8) ROA	(9) Board Indep
$R2000_{post} - R2000_{pre}$	1.69***	-0.74	-0.01	0.00	0.01	0.03	0.01	0.01	-0.02
$\times$ $PostAssignment_t$	(0.26)	(0.70)	(0.09)	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)
Observations	3,863	3,863	2,882	2,583	3,563	2,886	3,788	3,523	2,194
R^2	0.891	0.793	0.755	0.725	0.884	0.900	0.848	0.797	0.786
Rank control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm $\times$ Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A5
Index Switching and Key Variables - Controlling for Pre-assignment Size Changes

This table presents the estimation results of the combined difference-in-differences specification in Equation (4) for selected outcome variables, additionally accounting for index assignment in non-cohort years (see Section 5.1 for details) and controlling for the market capitalization in a way that is immune to concerns about 'bad controls'. Specifically, the size control is defined as market capitalization in year t when ($year$ - $cohort$) = -3 , -2, or -1 and assignment-year market capitalization (i.e., market capitalization in the cohort year) when ($year$ - $cohort$) = 0, 1, and 2. We use the constructed HMMR sample for these estimations. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include firm-by-cohort fixed effects and year-by-cohort-by-pre-cohort-assignment fixed effects. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, 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) $\log(q)$	(6) log-(q^{TOT})	(7) $\log(\frac{M}{B})$	(8) ROA	(9) Board Indep
$R2000_{post} - R2000_{pre}$	1.76***	-0.95	-0.07	0.00	-0.01	-0.01	-0.01	0.00	-0.02
$\times$ $PostAssignment_t$	(0.25)	(0.68)	(0.09)	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
Observations	3,856	3,856	2,878	2,584	3,551	2,877	3,776	3,517	2,188
R^2	0.890	0.793	0.751	0.727	0.881	0.895	0.847	0.793	0.786
Pre-size control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm $\times$ Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A6
Index Switching and Key Variables - Corrected Specification with Additional Modifications

This table presents the estimation results of the separate difference-in-differences specifications in Equations (2) and (3) that avoids problematic control group comparisons across selected outcome variables, after making additional corrections that account for index status in non-cohort years (see Section 5.1 for details) and the endogenous factor that drives index switching as well (see Section 5.2 for details). We use the constructed HMMR sample for these estimations. All variables are defined in accordance with Heath et al. (2022), as detailed in Appendix A of their paper. We include firm-by-cohort fixed effects and year-by-cohort fixed effects, as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, 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
$R1000 \to R2000_j \times PostAssignment_t$	1.38***	-1.54	0.04	-0.00	-0.04***	-0.02	-0.03	-0.04	-0.01
	(0.35)	(1.00)	(0.11)	(0.02)	(0.01)	(0.03)	(0.05)	(0.05)	(0.01)
Observations	1,325	1,325	1,071	998	861	1,188	943	1,309	1,251
R^2	0.901	0.813	0.801	0.699	0.835	0.822	0.892	0.800	0.743
$R2000 \to R1000_j \times PostAssignment_t$	-1.36***	0.67	0.02	-0.01	0.01	-0.03	-0.03	-0.06**	-0.01*
	(0.32)	(0.80)	(0.11)	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)	(0.01)
Observations	2,523	2,523	1,799	1,577	1,327	2,360	1,931	2,464	2,258
R^2	0.883	0.781	0.720	0.742	0.758	0.889	0.899	0.848	0.817
Size control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A7
Index Switching and Other Incentive and Governance Outcomes

This table analyzes other managerial incentive and governance outcomes. Panel A reports results from the combined difference-in-differences (DID) in Equation (4) on the dataset provided by Heath et al., Panel B presents the results from the AGK specification in Equation (6) on the AGK sample, and Panel C reports the combined DID results on our constructed two-band sample with the corrected sampling procedure that accounts for index assignment in non-cohort years while controlling for firm-year level market capitalization (see Sections 5.1 and 5.2 for details). 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), firm-by-cohort fixed effects, year-by-cohort-by-pre-cohort-assignment fixed effects, firm fixed effects, and year fixed effects, as indicated. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) log Total Comp	(2) Gldn Para	(3) CEO Turn over	(4) E-index	(5) Poison Pill	(6) Super maj	(7) Lim Spec Meet	(8) Wr Con sent	(9) Dual Class
A. Combined diff-in-diffs (Eq. 4) on HMMR sample									
$R2000_{post} - R2000_{pre}$ $\times$ $PostAssignment_t$	0.15**	-0.04	-0.02	-0.09	-0.00	-0.03*	-0.00	-0.08***	-0.00
	(0.07)	(0.03)	(0.02)	(0.06)	(0.03)	(0.02)	(0.03)	(0.03)	(0.01)
Observations	3,219	2,592	3,923	2,592	2,592	2,592	2,592	2,592	2,592
R^2	0.858	0.768	0.235	0.870	0.785	0.786	0.850	0.835	0.952
Firm $\times$ Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
B. AGK specification (Eq. 6) on AGK sample									
$R2000_{jt}$	0.06*	-0.01	0.03	0.02	-0.00	0.03*	0.00	-0.00	0.01
	(0.03)	(0.02)	(0.02)	(0.06)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)
Observations	6,659	5,672	6,660	5,672	5,672	5,672	5,672	5,672	5,672
R^2	0.894	0.686	0.167	0.882	0.715	0.894	0.923	0.874	0.945
Size control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
C. Combined diff-in-diffs (Eq. 4) on constructed and corrected sample									
$R2000_{post} - R2000_{pre}$ $\times$ $PostAssignment_t$	0.08	-0.04	0.01	-0.06	0.02	-0.03*	-0.01	-0.08**	0.00
	(0.08)	(0.03)	(0.03)	(0.07)	(0.03)	(0.02)	(0.03)	(0.03)	(0.01)
Observations	2,647	2,164	3,276	2,164	2,164	2,164	2,164	2,164	2,164
R^2	0.843	0.785	0.273	0.882	0.814	0.754	0.847	0.840	0.951
Size control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm $\times$ Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year $\times$ Cohort $\times$ $R2000_{pre}$ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A8
Index Switching and Long-run Stock Returns

This table presents a comparison of long-run stock performance between index switchers and non-switchers, using a stacked difference approach similar to the combined difference-in-differences specification. Buy-and-hold abnormal returns (BHAR) are calculated over 1-, 2-, and 3-year horizons, using year -1 as the base year. Panel A reports results based on a market model, while Panel B uses a three-factor model to estimate subsequent returns. We include cohort-by-pre-cohort-assignment fixed effects in all specifications. Standard errors reported in parentheses are heteroscedasticity-robust and clustered by firm. ***, **, and * indicate the statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Buy-and-Hold Abnormal Return		
	1-year	2-year	3-year
A. Market model			
R2000post - R2000pre	-0.01	0.02	0.02
	(0.03)	(0.05)	(0.05)
Observations	747	718	678
R^2	0.088	0.082	0.149
B. Fama-French three-factor model			
R2000post - R2000pre	-0.00	0.11*	0.18*
	(0.03)	(0.06)	(0.10)
Observations	720	691	652
R^2	0.136	0.185	0.183
Cohort × $R2000_{pre}$ FE	Yes	Yes	Yes