Agency Conflicts and the Rise of Passive Investing

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ABSTRACT

We examine whether the recent shift from active to passive investing leads to increased agency conflicts. Using a new research design that generates exogenous variation in fund holdings, we find that passive funds are weak monitors. Unlike active funds, passive funds rarely vote against firm management on contentious corporate governance issues. Moreover, although passive funds do exit 16% of their holdings each year, they do not use exit to enforce good governance. Finally, we find no evidence that passive funds engage with firm management. Our results show the rise of passive investing is shifting control from investors to corporate managers.

Keywords: Governance, Monitoring, Index Investing, Voting, Exit

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I. Introduction

The separation of ownership and control generates an agency conflict between a firm's managers and its shareholders. This well-known problem has been studied since at least the time of Adam Smith (1776).[1] Yet recently there has been a fundamental shift in equity investing, potentially altering this classic agency conflict. Over the last 25 years public corporations have experienced a dramatic increase in ownership by passively managed index funds (see Figure 1), and passive funds are now the largest shareholders of many U.S. corporations (Azar, Tecu, and Schmalz (2018)). Although the increasingly large positions held by passive funds should motivate them to monitor their portfolio firms (Grossman and Hart (1980), Shleifer and Vishny (1986), Admati, Pfleiderer, and Zechner (1994)), these new intermediaries have different incentives than managers of traditional active funds (Bebchuk, Cohen, and Hirst (2017)). As a consequence, the rise of passive investing raises fundamental questions about monitoring and corporate governance. Notably, to what extent do passive funds monitor their portfolio companies? And also, does the rise of passive investing lead to increased agency conflicts that weaken corporate governance?

In this paper, we study the monitoring behavior of passive funds by examining the two main monitoring mechanisms predicted by theory: voice and exit. We find that passive funds are 12.5 percentage points less likely to vote against firm management compared to active funds. Moreover, we find that passive funds, surprisingly, do choose to exit up to 4% of their holdings each year however they do not use the exit mechanism to enforce good governance. Specifically, while active funds are more likely to exit a position after losing a vote, passive funds are not. We also find no evidence that passive funds engage with firm management

[1]Smith wrote, "The directors of such [joint stock] companies, however, being the managers rather of other people's money than of their own, it cannot well be expected that they should watch over it with the same anxious vigilance with which the partners in a private copartnery frequently watch over their own."

in a meaningful way. Consistent with the theoretical predictions in Bebchuk et al. (2017) and Edmans, Levit, and Reilly (2018), our results uniformly indicate that passive funds are weak monitors of their portfolio firms.

Given the increasingly large positions held by passively managed index funds, principal-agent theory would argue that these funds have strong incentives to monitor (Jensen and Meckling (1976), Grossman and Hart (1980), Shleifer and Vishny (1986), Admati et al. (1994)). Moreover, since the need to minimize tracking error makes it costly for index funds to exit a position, index funds should have strong incentives to enforce good governance through the voice mechanism (e.g., Fisch, Hamdani, and Davidoff Solomon (2018)). Consistent with this view, a number of recent studies argue that passively managed index funds are "closet activists" who improve a variety of corporate policies, from dividends and disclosure to competitive strategy.[2]

However, the business model of passive funds suggests that these funds have weak incentives to monitor, since they typically have a large number of firms in their portfolio and limited resources to invest in monitoring due to their low-cost structure.[3] Moreover, unlike active investing, passive investing creates a free-rider problem because improvements to firm value are shared with all funds that follow the same index, but the costs are borne only by the fund that exerts monitoring effort (Bebchuk et al. (2017); Bebchuk and Hirst (2019)). Empirically, it remains unclear which of these effects prevail. Our results uniformly indicate that the latter effects dominate: Passive funds do not exert the same monitoring effort, on average, that active funds do.

[2]Boone and White (2015), Appel, Gormley, and Keim (2016), Crane, Michenaud, and Weston (2016), Azar et al. (2018), Appel, Gormley, and Keim (2018). Yet, see also Schmidt and Fahlenbrach (2017) who find that passive fund ownership leads to worse corporate governance.

[3]In our data, the average index fund holds 357 stocks while the average active fund holds 114 stocks. We discuss this point further in Sections II and IV.

The main challenge in studying fund behavior is that fund holdings are endogenous. First, firm characteristics such as size and liquidity jointly affect ownership and governance. Second, different firm policies attract different types of investors.[4] Thus, there is the potential for endogeneity due to omitted variables and reverse causality. There is also the potential for selection bias: If a fund chooses not to hold a firm, we do not observe how that fund would have voted. Thus, if passive funds tend to hold well-run firms or active funds tend to hold poorly-run firms, differences in monitoring might simply reflect differences between firms.

To generate exogenous variation in fund holdings, we develop a new research design using Russell index reconstitutions. Importantly, we develop the first approach specifically designed to examine index investing in the post-2006 time period. In June of each year Russell Investments reconstitutes their indexes, originally by assigning the largest 1000 firms in terms of market capitalization to the Russell 1000 index and the next 2000 firms to the Russell 2000 index. However, starting in June 2007, Russell implemented a new assignment regime designed to make it less likely that firms near the cutoff would switch indexes. Specifically, each year Russell ranks each firm and computes a "band" around the 1000th ranked cutoff point that is equal to +/- 2.5% of the total market capitalization of the Russell 3000E.[5]

This new assignment regime prevents traditional regression discontinuity designs (RDD) from examining this setting in the post-2006 period. However, we proceed from the insight

[4]Grinstein and Michaely (2005) find that higher firm payouts attract institutional holdings, while Brav, Jiang, Partnoy, and Thomas (2008), Aghion, Van Reenen, and Zingales (2013), and Michaely, Popadak, and Vincent (2015) find that active investors target firms with weak governance and high leverage.

[5]In the pre-2007 regime, a firm that started the year in the Russell 1000 would move to the Russell 2000 if its market capitalization fell below the rank 1000 cutoff point. However, under the post-2006 regime, a firm that started the year in the Russell 1000 would move to the Russell 2000 if its market capitalization fell below the rank 1000 cutoff point by a sufficient margin, where the margin is defined as 2.5% of the Russell 3000E market capitalization.

that the new assignment regime introduced two yearly discontinuities, and we use these discontinuities to generate exogenous variation in index ownership in a difference-in-differences panel setting.[6] Moreover, our approach allows us to use exogenous variation in fund holdings in a Heckman (1979) model, to correct for selection bias in addition to reverse causality and omitted variable biases. As a result, our methodology does not suffer from bias due to noise in the forcing variable (Pei & Shen, 2017) or selection bias (Wei and Young (2017), Gloßner (2018)), which is a concern in existing studies that use an RDD around Russell Index reconstitutions.

We first examine funds' voting behavior across all agenda items. On consensus votes when ISS (a third party proxy advisor) and management agree, passive funds and active funds vote identically. By contrast, on contentious votes (when ISS and management disagree), passive funds are 12.5 percentage points more likely than active funds to vote with management.[7] Furthermore, passive funds with low expense ratios are more likely to vote with management than passive funds with high expense ratios. This second result is informative for two reasons: First, it indicates that voting with management is a weak monitoring behavior, since passive funds that have less resources to invest in monitoring are more likely to cede authority to firm's management.[8] Second, it provides support for the theoretical prediction that passive

[6]We find that when a stock switches into the Russell 2000, index fund ownership increases by 1.03% of its market capitalization on average, and when a stock switches out of the Russell 2000 index fund ownership falls by 0.86% of its market capitalization.

[7]In a further analysis, we examine voting at the fund family level rather than at the individual fund level. We find that fund families that have more assets under management (AUM) by passive funds are more likely to side with firm management.

[8]In other words, while it might not be clear *a priori* that voting with management is a weak monitoring behavior, we find that passive funds with higher expense ratios vote more like active funds do, i.e., are more likely to disagree with management. Moreover, it is clear that from a principal-agent perspective, voting with management cedes power from the investor (the principal) to firm management (the agent). We also find that passive funds are *less* likely to formally abstain on contentious items. Del Guercio, Seery, and Woidtke (2008) argue that both voting against management and abstaining ("a soft no") are active voting strategies.

funds lack the incentives and resources to actively monitor their portfolio firms (Bebchuk et al. (2017)).[9]

Arguably, not all votes are equally important, and our results might just describe passive funds' voting behavior on agenda items that do not impact corporate governance. To shed further light on passive funds' monitoring behavior, we examine voting on specific governance issues: board of directors elections, executive compensation, corporate disclosure, and managerial entrenchment. We find that passive funds are more likely than active funds to vote with management across all of these categories. This is striking as it shows that passive funds cede authority to management on *all* categories of votes that affect corporate governance. Results on managerial entrenchment are particularly relevant since some of the largest passive funds publicly claim to be against certain governance practices such as poison pills and golden parachutes.[10] Yet, when it comes to vote on these issues, passive funds continue to cede authority to management.

Three alternative hypotheses could explain these findings. First, instead of voting against management, passive funds could sell their shares as a monitoring mechanism to enforce good governance (the exit hypothesis). Second, passive funds could "actively" engage with managers either publicly or behind the scenes, and then vote in support of management proposals that they negotiated beforehand (the engagement hypothesis).[11] Third, even if passive funds do not actively engage with their portfolio firms, managers might be driven to appease these large shareholders by following policies that are preferred by passive funds (Fisch et al. (2018)).

[9]The economics of index investing restrict the resources that passive funds have to employ in monitoring since passive funds compete on providing a standardized product at the lowest price (i.e. expense ratio) possible.

[10]See Bebchuk, Cohen, and Ferrell (2008) and the ISG framework at https://isgframework.org.

[11]In a recent survey, McCahery, Sautner, and Starks (2016) find evidence of behind the scenes intervention by institutional investors. However, they do not distinguish between active funds and passive funds.

First, we investigate the exit hypothesis. Although one might think that passive funds hold each stock in their benchmark index, in fact they may omit holdings in individual stocks. In practice, they often omit small and illiquid firms that would otherwise increase the transaction costs of rebalancing their portfolio. We thus examine funds' decisions to exit and document strategic substitution between voting and exit for active funds but not for passive funds: When active funds lose a vote, they are more likely to exit the position subsequently, as theory predicts (Edmans et al. (2018)). Conversely, passive funds do not exit a position after losing a vote. In other words, while passive funds may exit stocks in their benchmark index, they do not use exit to express their dissatisfaction with management.

Next we examine the "active" engagement hypothesis in two ways. First, we split agenda items into shareholder proposals and management proposals. Behind the scenes engagement could explain passive funds' voting behavior on management proposals, but it cannot apply to shareholder proposals. In other words, passive funds that are "active" monitors could support management proposals that they negotiated beforehand. But when it comes to shareholder proposals, passive funds that are "active" monitors should be willing to oppose management. Yet we find that relative to active funds, passive funds are 9.2 percentage points more likely to vote with management on shareholder proposals.[12]

Second, we examine the funds' propensity to publicly engage. Shareholders are required to disclose a holding above 5% of the firm's market capitalization via either Schedule 13D, which allows the fund to officially engage with the firm, or Schedule 13G, which does not. We find that passive funds are significantly less likely to file Schedule 13D than active funds, which confirms that passive funds are weak monitors. This finding echoes the evidence

[12]These results are echoed in a contemporaneous working paper by Brav, Jiang, and Li (2018). They document that in proxy contests, an important and contentious subset of shareholder proposals, passive funds do not support activist shareholders but instead side with firm management.

in Bebchuk and Hirst (2019) that passive funds do not meet with the majority of their portfolio firms. Also, engagement relies on the fund expending resources to become informed. Funds must first do research on their portfolio firms to understand what policies need to be implemented and then they must engage with managers. In a recent working paper, Iliev, Kalodimos, and Lowry (2018) document that, relative to active funds, passive funds conduct significantly less research about their portfolio firms. In sum, our results on shareholder proposals and 13D filings, together with the evidence in Bebchuk and Hirst (2019), Brav et al. (2018) and Iliev et al. (2018), are inconsistent with the hypothesis that passive funds engage with firm management.

Finally, we examine the hypothesis that managers propose governance changes that are preferred by passive funds, without the funds' direct intervention. If passive funds indirectly encourage better governance in their portfolio firms, we would expect an increase in the fraction of agenda items that were approved by ISS and management together. We see no such change. Moreover, we do not find an increase in the number of either management proposals or shareholder proposals. These results are inconsistent with the hypothesis that firm managers are driven to appease passive funds by following policies that they prefer.

Overall, our results uniformly indicate that passive funds monitor their portfolio firms less than active funds do.[13] Relative to active funds, passive funds are less likely to vote against management, less likely to use exit to influence corporate governance, and less likely to engage with management. Thus, the recent shift from active to passive investing is altering the classic agency conflict between managers and shareholders, and it is weakening corporate governance. As such, our findings make important contributions to the literature on agency conflicts and monitoring incentives arising from dispersed ownership (e.g., Berle and Means

[13]There is a sizeable literature on the monitoring incentives of active investors (see, e.g., DeMarzo and Urošević (2006); Back, Collin-Dufresne, Fos, Li, and Ljungqvist (2018)).

(1932); Jensen and Meckling (1976); Demsetz (1983); Shleifer and Vishny (1986); Admati et al. (1994); Burkart, Gromb, and Panunzi (1997); Maug (1998)). Given the dramatic increase in ownership by passively managed funds, and since passive funds are now the largest blockholders of many U.S. corporations (Azar et al. (2018)), studying their monitoring incentives is of fundamental importance (Edmans (2014), Bebchuk and Hirst (2019)). Our results provide the first empirical evidence that the fundamental shift in equity investing from active to passive is shifting power from investors to firm managers.

II. Data and Summary Statistics

To examine the governance implications of index investing, we combine data from the Center for Research in Security Prices (CRSP), Compustat, Institutional Shareholder Services (ISS), and the Frank Russell Company (Russell), as discussed in detail below.

A. Data

We use Russell Index membership lists provided directly from Russell and we match this data to firm and stock-level data from CRSP and Compustat.[14] To measure fund voting behavior, we use the ISS Fund Voting data. Starting from 2003, ISS records the votes cast by individual mutual funds and exchange traded funds (ETFs) at shareholder meetings for the majority of publicly traded U.S. firms.[15] We link the ISS data by fund-year to the CRSP

[14]We do not impose filters on firm or stock characteristics, because our identification strategy requires all firms that are in the Russell 1000 or Russell 2000 in cohort year t and year $t-1$.

[15]One potential challenge for studies of fund voting is that funds incorporated as a trust, such as SPY and QQQ, are not subject to NP-X reporting requirements. As such, their voting data is not publicly reported anywhere. None of the Russell 2000 index funds including IWM are incorporated as trusts, so our voting results are not affected by the omission of this data. We thank Tara Bhandari and Amy Edwards at the Securities and Exchange Commission for helpful conversations on this topic.

mutual fund database, requiring that all sample funds be U.S. equity funds with at least $10 million in assets under management.

We measure fund holdings by combining the CRSP mutual fund holdings database with the Thomson Reuters S12 database. We find that both databases omit some holdings of certain mutual funds in certain years, but the omissions are largely orthogonal across the two databases.[16] In unreported analyses, we find that all our results are similar when we use only S12 or only CRSP holdings data.

B. Summary Statistics

We begin our analysis by examining the cross-sectional variation of voting outcomes between active and passive funds using univariate summary statistics. Consistent with the literature, we define an passively managed index fund as a fund with fund flag "D" in the CRSP Mutual Fund Database, and we classify all other funds as active funds (all variables are defined in Appendix A).[17] Row 1 of Table I shows the distribution of fund votes across the entire set of agenda items (i.e., the full matched sample). Unconditionally, index funds vote Yes 90.4% of the time compared to 89.4% of the time for active funds.

Many agenda items are largely procedural, such as renewing the board of directors or voting to adjourn the meeting. Accordingly, in the next four rows of Table I we split agenda items into two categories: "consensus votes", i.e. items for which management and ISS made

[16]For example, S12 omits some data on the Vanguard Russell 2000 fund, which is well covered in CRSP. Conversely, prior to 2008 CRSP omits some data on the iShares Russell 2000 fund. Combining the two databases yields good coverage of both funds in all sample years. Formally, we take the union of the two databases; if a fund-firm-year holding is in one databases but not the other, we include it; if it is in both databases, we take the larger of the two positions.

[17]Throughout the paper we use "passive funds" and "index funds" interchangeably. In CRSP, a fund with flag D is a "pure index fund" whose "objective is to match the total investment performance of a publicly recognized securities market index." In unreported tests, we classify funds according to their fund name or their active shares (Petajisto (2009)) and our results are similar.

the same recommendation (rows 2-3), and "contentious votes", i.e. items for which management and ISS made opposing recommendations (rows 4-5). For items that management and ISS both approve, index funds vote Yes 95.6% of the time while active funds vote Yes 96.0% of the time. Similarly, for votes that management and ISS both oppose, index funds vote Yes 4.2% of the time while active funds vote Yes 5.1% of the time. The rates at which active and index funds vote no, abstain, or fail to record a vote are also similar. Thus, on consensus votes, index funds and active funds vote identically.

On contentious items the results are very different. For items which management supports but ISS opposes, index funds vote Yes 54.3% of the time compared to 41.9% for active funds. For items which management opposes but ISS supports, index funds vote No 53.5% of the time compared to 46.0% for active funds. Thus, in both cases index funds are significantly more likely to side with management. Summing across all contentious votes and coding abstentions as "no" votes, index funds voted with management 55.5% of the time while active funds voted with management 46.2% of the time.

Interestingly, index funds are *less* likely than active funds to abstain on contentious items. As argued in Maug and Rydqvist (2001), if voting is costless, no shareholder should ever abstain. Hence, the significant number of abstentions in our analysis implies that voting on contentious items is costly to funds. If a fund wishes to maintain its relationship with firm management, voting "abstain" may be preferred to voting "against". Since most items require a majority of all votes cast to approve a measure, abstention can have the same effect as voting against a proposal but be perceived as a "*soft* no" (Del Guercio et al. (2008), Bebchuk et al. (2017)). Hence, finding that active funds are more likely than index funds to abstain on contentious items again suggests that active funds are more likely to oppose management than index funds are.

These results provide broad descriptive evidence that index funds are weak monitors in that they are more likely than active funds to vote with firm management. While *a priori* it may be unclear if ceding authority to firm management shuld be considered a weak monitoring behavior, it is clear that voting with management transfers power from the principals (from investors) to the agents (the firm's managers). Hence, from a principal-agent perspective (e.g., Berle and Means (1932), Jensen and Meckling (1976), Maug (1998)) such a voting strategy is clearly weak.

Of course, it remains possible that index funds use other mechanisms to monitor their portfolio companies, such as the exit mechanism. To understand whether index funds are good monitors, it is necessary to examine both voice and exit behavior (e.g., Edmans et al. (2018)).[18] Using the fund holdings data, we observe if a fund exits a given stock in a given year. We further distinguish between voluntary and involuntary exit: All funds must exit a position if a firm is acquired or delisted, so we code these as involuntary exits.

In untabulated results, we find that each year on average an active fund exits 36 (or 32%) of their 114 positions. By comparison, on average each year an index fund exits 61 (16%) of its 371 positions. This comparison is conservative, because it does not take into account stocks that switch out of the fund's benchmark index. When we take index switching into account, we find that on average a Russell 2000 index fund holds 1789 of the Russell 2000 stocks each year and exits 290 stocks each year: 223 (12.5%) because the stock delisted or left the index and 67 (3.7%) voluntarily. Thus, the data suggest that index funds do voluntarily exit from a nonzero fraction of their positions each year, although much less frequently than active funds. While it may seem surprising that index funds hold less than 100% of their index stocks, they are more likely to hold stocks with higher index weights, so

[18]The voice mechanism also includes engagement. We examine this monitoring mechanism in Section IV B.

on a value-weighted basis they hold most of their benchmark index.

A limitation of the results presented so far is that both active funds and index funds (to a lesser extent) choose which stocks they hold. Hence, there is the potential for both an *endogeneity bias* – if fund holdings are correlated with firm governance – and a *selection bias* – since if a fund chooses not to hold a firm then we do not observe how the fund would have voted. To address these potential biases, in the next section we develop a new research design that uses post-2006 Russell index reconstitutions. We show that this empirical approach produces exogenous variation in fund holdings, and we use this variation to examine fund voting, engagement, and exit.

III. Research Design

A. Background on Russell Indexes

In June of each year Russell Investments reconstitutes their popular Russell 1000 (large-cap) and Russell 2000 (small-cap) indexes. To determine index assignment, Russell ranks all qualifying U.S. common stocks by their market capitalization as of the last business day in May.[19] Before June 2007, index assignment followed a simple threshold rule: Stocks ranked from 1-1000 were assigned to the Russell 1000 while stocks ranked from 1001-3000 were assigned to the Russell 2000.

Starting in June 2007, Russell implemented a new assignment regime ("banding"). After

[19]Russell reports the index weights on the component stocks, which are based on their proprietary calculation of *float-adjusted* market capitalization. However, Russell does not disclose the initial rankings that determine index assignment, which are based on *unadjusted* market capitalization. We compute our own proxy market capitalizations and rankings at the end of May each year using CRSP and Compustat data following Chang, Hong, and Liskovich (2015). Our predicted Russell membership recovers the actual Russell Index membership for 99.5% of firm-years, and all results are similar when we use alternative methods of imputing the Russell rankings.

sorting stocks by their market capitalization, Russell computes an upper and lower band around the rank-1000 cutoff; the bands are calculated as +/- 2.5% of the total market capitalization of the Russell 3000E.[20] Stocks within the bands do not switch indexes. That is, if a stock that was in the Russell 2000 last year is above the rank-1000 cutoff but below the upper band, it will stay in the Russell 2000 the following year, and *vice versa*.

Figure 2 plots index assignments in 2007, the first year of the banding regime. We see that banding entirely eliminated the discontinuity across the rank-1000 cutoff; hence, an RDD across the cutoff is no longer feasible. However, Figure 2 also shows there are two new discontinuities at the upper and lower bands (dashed vertical lines). These discontinuities correspond to whether stocks *switched* indexes or stayed in their previous index.

Consider a stock in the Russell 2000 that is nearby the upper band when the indexes are reconstituted. The stock's index assignment depends on four parameters as calculated by Russell:

1. The stock's ranking in the Russell 3000
2. The market capitalization of the rank-1000 stock
3. The total market capitalization of the Russell 3000E
4. The cumulative market cap of the stocks ranked below the focal stock but above the rank-1000 stock

All four parameters are difficult to predict ex ante – indeed, Russell does not make their unadjusted market capitalizations or rankings available *ex post*. All four parameters are difficult or impossible to manipulate. This line of reasoning suggests that within a

[20]The 3000E is an "extended" version of the Russell 3000 that includes microcap stocks.

sufficiently narrow window around each band in each year, whether a stock ranks above or below the band – and therefore switches or stays – is as good as randomly assigned.

B. Research Design

For each index reconstitution since June 2007, we select a *cohort* that consists of two sets of treated and control stocks. Specifically, we select all stocks that were potential switchers (based on their lagged index membership) in windows of +/-100 ranks around the upper and lower band. Consider two stocks A and B that are similar in every way, including that both are in the Russell 1000 index. Both stocks experience negative returns in the year prior to treatment and fall in the rankings. Firm A's market capitalization falls by 10% while Firm B's market capitalization falls by 10% plus epsilon. As a result, A stays in the Russell 1000 (and serves as a control) while B switches to the Russell 2000 (and is treated). Importantly, our identification strategy compares stocks that started in the same index and are similar in every dimension, including their lagged returns, except that they landed on different sides of the same band.

Figure 3 shows the treated and control stocks around both bands in the 2007 cohort. For each stock in each cohort, we include firm-years from three years prior to the cohort year (pretreatment years -3, -2, -1) and three years after the cohort year (post-treatment years 0, 1, 2). Formal balance tests show that the treated and control firms were indistinguishable *ex ante* across both bands on every dimension including their market capitalization (see Appendix D).

In Table II Panel A, we report summary statistics for the firm-years in our Russell sample from 2004 to 2017. The average firm has a market capitalization of 2.5 billion dollars, total ownership by mutual funds of 9.56% of the firm's market capitalization, and an entrenchment

("E")-index of 3.2. The average ownership by index funds is 3.86% of market capitalization (0.93% of which is by Russell 2000 index funds, and 0.09% of which is by Russell 1000 index funds), and the average ownership by active funds is 5.70% of market capitalization.

In Table II Panel B we report summary statistics for the mutual funds in the sample from 2004 to 2017. Relative to active funds, index mutual funds are less numerous, similar in size in terms of assets under management, have lower expense ratios, and are more diversified on average.

The discontinuity in treatment status based on the unadjusted Russell rankings suggests a regression discontinuity design (RDD). However, there are features of the setting that make an RDD undesirable. The main feature is that we do not observe the true rankings that determined index assignment; instead, we must impute them using the CRSP and Compustat data. Our proxy rankings predict the true index assignments with 99.5% accuracy, but there could still be significant errors in the rankings of individual firms. This is a concern because errors in measuring the forcing variable bias the RDD control function to be too flat, and produce spurious or upward biased estimates of treatment effects (Pei & Shen, 2017).[21]

To deal with this concern we exploit the panel nature of our data. Specifically, we estimate a cohort difference-in-differences design with firm-by-cohort fixed effects. To see why this approach addresses measurement error in the forcing variable, consider the RDD estimate from the following model:

$$\begin{aligned} Y_j = \beta_1 \, I\{R1000 \to R2000_j\} + \beta_2 \, I\{R2000 \to R1000_j\} \\ + \gamma \, (truerank_j + measurementerror_j) + \epsilon_j, \end{aligned} \tag{1}$$

[21] Note that a fuzzy RDD, which adjusts for non-compliance with treatment assignment, does not address this issue.

where $I\{R1000 \rightarrow R2000_j\}$ is an indicator variable that takes the value one if a stock switches from the Russell 1000 to the 2000, $I\{R2000 \rightarrow R1000_j\}$ is an indicator variable that takes the value one if a stock switches from the Russell 2000 to the 1000, and γ is the coefficient on a linear control function (that is measured with error). Standard arguments (Wooldridge, 2008) show that measurement error in *caprank* causes $\hat{\gamma}$ to be biased toward zero. Since *truerank* is correlated with treatment status, the estimated treatment effect $\hat{\beta}$ is biased away from zero. This bias may be present for *any* choice of control function.

Instead, we estimate the following difference-in-differences model:

$$\begin{aligned} Y_{jt} = \beta_1 \, I\{R1000 \rightarrow R2000_j\} \times I\{PostTreat_t\} + \\ \beta_2 \, I\{R2000 \rightarrow R1000_j\} \times I\{PostTreat_t\} + \phi_j + \lambda_t + \epsilon_{jt}, \end{aligned} \tag{2}$$

where ϕ_j and λ_t are firm and date fixed effects and $I\{PostTreat_t\}$ is an indicator variable that takes the value one after index re-balancing.[22] We compare the outcome variable before treatment versus after treatment, with a fixed effect applied to each firm in each cohort. Because each firm had a fixed ranking within the cohort, the fixed effects ϕ_j absorb any association of the outcome variable with both the true ranking *and* the error in the proxy ranking for each firm. Thus, the specification (2) estimates the treatment effect of switching indexes as would a correctly measured RDD, but in a way that is not sensitive to measurement error in the forcing variable.

This approach is not a panacea. Errors in the proxy rankings could also cause us to select firms that were farther away from the bands than we know, which would introduce selection

[22]Importantly, this means that β_1 and β_2 – the effects of switching from the R2000 to the R1000 and vice versa – are identified from disjoint sets of treated and control stocks. The stock-by-cohort fixed effects sweep out any non-time-varying differences between treated and control stocks, while the year fixed effects remove aggregate trends in firm behavior or ownership.

bias into the sample. We examine the possibility of selection bias in two ways. First, if our treated and control firms are different *ex ante* it should be visible in their pretreatment characteristics.[23] In Appendix D we present formal balance tests which show that the firms on either side of each band are indistinguishable on a variety of measures. Second, in Appendix F we document that our estimates remain stable as we vary the windows around the bands. These results are inconsistent with selection bias.

Our methodology differs from previous papers that use Russell reconstitutions in two important dimensions. First, we are the first to develop a research design that explicitly uses Russell index reconstitutions in the post-2006 period. Thus, our results reflect this more recent period, during which index investing is at all-time highs. Second, unlike previous RDD research designs, our difference-in-differences specification uses firm fixed effects to sweep out unobserved heterogeneity among firms. Among other advantages, this means that our estimates are not biased by noise in the measurement of the forcing variable, which can be an issue in sharp and fuzzy RDD specifications (Pei & Shen, 2017).

C. Effects of Index Switching on Fund Ownership

Next, we examine the effects of Russell index assignment on fund ownership. In Column 1 of Table III, we present estimates (2) of the effects of index switching on ownership by Russell 2000 index funds. We find that ownership by Russell 2000 index funds rises by an average 1.45% of market capitalization for stocks that switch into the Russell 2000 relative to nearby stocks that stay in the Russell 1000. At the same time, we find that ownership falls by 1.34% of market capitalization for stocks that switch into the Russell 1000 relative

[23] Wei and Young (2017) show via balance tests that Russell RDD specifications from the existing literature have large pretreatment differences between treated and control firms, suggesting the presence of selection bias.

to similar stocks that stay in the Russell 2000. The two coefficient estimates are very similar in magnitude, even though they are estimated from two *disjoint* sets of stocks.

In Column 2 of Table III, we report the effects of index switching on ownership by Russell 1000 funds. As expected, we find the opposite effect (relative to the change in ownership by Russell 2000 funds shown in Column 1). However, the coefficient is smaller for Russell 1000 fund holdings, falling by 0.18% of market capitalization in the lower band treatment group and rising by 0.17% of market capitalization in the upper band treatment group. This is as expected, because the index weights of stocks at the bottom of the Russell 1000 are significantly smaller than the index weights of stocks at the top of the Russell 2000.

In Column 3 of Table III, we examine the effects of index switching on ownership by index funds that replicate the S&P500 index. (This is by far the largest category of index funds both numerically and by assets under management.) Russell index assignments should be largely irrelevant to the holdings of these funds. Indeed, although the assets under management of the S&P 500 funds are an order of magnitude larger than those of the Russell funds, the changes in holdings by S&P 500 funds are tiny, on the order of 0.03% of the firm's market capitalization.

The net effect on holdings by all passive mutual funds in the data (Table III Column 4) is similar to the net effect on holdings by Russell 1000 and 2000 index funds. By contrast, in Column 5 we examine the effects of index switching on ownership by active mutual funds. The changes in ownership by active funds are small and not statistically significant. As a result, total holdings by all mutual funds (Table III Column 6) are entirely driven by changes in holdings by index funds.

Figure 4 plots Russell 2000 fund ownership for the four groups (switchers vs. stayers near the upper band; switchers vs stayers near the lower band) in event time, that is, the

observation year minus the cohort year. The results clearly show that: (i) Switchers and stayers in both groups have the same pre-treatment levels *and* trends, and (ii) switching into the Russell 2000 leads to higher index fund ownership and vice versa. Because firms in any group may also switch indexes in the post-treatment years, the treated and control groups converge toward each other after the treatment year.

In sum, the evidence shows that index switching is plausibly random among sample firms near the yearly Russell bands, and this is followed by symmetric shifts in ownership by index funds.

IV. Voting

In this and the next section, we examine the monitoring behavior of index funds, moving from broad cross-sectional comparisons to the cleanly identified estimates in our Russell cohort setting. We examine voice and exit and find that index funds are weak monitors.

We start our analysis by examining funds' voting behavior. In Table IV Columns 1 and 2, we estimate the difference in fund voting on all contentious votes across the universe of firms. The dependent variable *VotedWithMgmt* is an indicator equal to 1 if a fund votes with management's recommendation, and 0 otherwise.[24] The independent variable *IndexFund* is an indicator equal to 1 if the fund is an index fund and 0 if the fund is an active fund, as defined in Section II B and Appendix A. The estimates include firm fixed effects, which remove non-time-varying differences across firms in management quality or governance, and year fixed effects which remove aggregate trends.

In Column 1 we find that, compared to active funds, index funds are 12.5 percentage

[24]Following management's recommendation is defined as voting Yes on a recommendation of Yes, and No or Abstain on a recommendation of No or Withhold.

points more likely to side with management on contentious votes. This is a larger difference than in the summary statistics (Table I) and is due to the addition of firm fixed effects, so that we now compare index versus active funds' voting within each firm.

In Column 2 we add as an explanatory variable each fund's yearly expense ratio. We estimate the coefficient on the expense ratio separately for index and active funds because of the different incentives that the two types of funds face, and their different distributions of expense ratios. We find that active funds' voting behavior does not vary significantly with their expense ratio. By contrast, among index funds, funds with higher expense ratios are significantly *less* likely to side with management on contentious votes. The coefficient of -0.238 means that an index fund with an expense ratio that is 25 basis points higher (about one standard deviation) is 6.1 percentage points less likely to side with management – half of the overall difference between index and active funds. This result is strikingly consistent with the prediction of Bebchuk et al. (2017): The economics of index investing restricts the resources that the fund has to employ in monitoring, since index funds compete on providing a standardized product at the lowest price. This result also supports the interpretation that siding with management is a weak monitoring behavior. When index funds have more resources to employ in monitoring, they behave more like active funds, i.e., they side less with management on contentious votes.

The firm and year fixed effects in Columns 1 and 2 mitigate concerns of endogeneity bias, since they compare how index funds and active funds vote within the same firm. In Appendix G we present alternate specifications that compare fund voting within firm-years and within individual agenda items. The difference in voting between index funds and active funds is identical to our main estimates. These results suggest that firm and year fixed effects, as in our main specification, account for most of the relevant variation across firm-years and

agenda items that explain fund voting behavior. In other words, within our panel, relevant characteristics such as firm governance or shareholder engagement vary widely between firms but little within firms over time.

However, there is still the potential for selection bias because funds choose which firms they hold. If index funds tend to hold better-run firms, or vice versa, then the gap in fund voting behavior might be due to selection. To examine the potential for selection bias, we next compare fund voting on contentious votes within the Russell cohort sample. Columns 3 and 4 of Table IV present results for the Russell subsample, and find similar results to those in the entire sample. Columns 5 and 6, instead, present results that explicitly correct for selection bias in fund holdings using a Heckman (1979) approach. Specifically, we estimate the following two-stage model:

$$\begin{aligned} Observed_{ijt} = Probit(&\tau IndexFund_i \\ &+ \xi_1 R1000 \rightarrow R2000_j \times Post_t \times IndexFund_i \\ &+ \xi_2 R2000 \rightarrow R1000_j \times Post_t \times IndexFund_i \\ &+ \mu_1 R1000 \rightarrow R2000_j \times Post_t + \mu_2 R2000 \rightarrow R1000_j \times Post_t \\ &+ \phi_j + \chi_t + \nu_{ijt}) \end{aligned} \tag{3}$$

$$Y_{ijt} = \beta IndexFund_i + \alpha InverseMillsRatio_{ijt} + \lambda_j + \kappa_t + \epsilon_{ijt} \tag{4}$$

In Equation (3) *Observed* is an indicator variable equal to 1 if a fund j holds a stock i on date t, and zero otherwise; *IndexFund* is an indicator variable equal to 1 if the fund is an index fund, and 0 otherwise; *R1000 → R2000* is an indicator variable equal to 1 if a firm switches from the Russell 1000 to the Russell 2000, whereas *R2000 → R1000* is an

indicator variable equal to 1 if a firm switches from the Russell 2000 to the Russell 1000. $Post_t$ is an indicator variable equal to 1 if the stock-year is post Russell assignment, and 0 if it is pre-Russell assignment. In Equation (4) the outcome variable is *VotedWithMgmt* (as defined above), *InverseMillsRatio* is the Heckman correction term from Equation (3). ϕ_j, λ_j are firm fixed effects and χ_t, κ_t are year fixed effects.

The results for the first stage (Equation (3)) are reported in Appendix Table A1. As documented in the previous section, index switching generates significant variation in fund ownership, which is plausibly exogenous for firms in the Russell cohort sample. In columns 5 and 6 of Table IV we report the second-stage estimates (4). The gap in voting behavior between index and active funds is still present but smaller: Index funds are 8.4 percentage points more likely than active funds to side with management, and again index funds with higher expense ratios are less likely to side with management. These results suggest that part of the gap in voting behavior is due to selection bias: Active funds may choose to hold firms whose management they are more likely to disagree with relative to index funds.[25]

Appendix G presents another observation: Index funds are significantly *less* likely than active funds to abstain on contentious votes. This result is consistent with the argument of Del Guercio et al. (2008) and Bebchuk et al. (2017) that it is costly for shareholders to openly oppose firm management. That index funds abstain less than active funds suggests again that index funds cede power to management, whereas active funds may prefer to either directly oppose firm management (vote No) or to abstain (soft No).

In sum, across a wide range of comparisons, index funds are more likely than active

[25]In many cases, funds belong to fund families such as Fidelity or Vanguard, and voting might be decided at least partly at the fund-family level. Such coordination is clear in the data: We find that the fund-family identity explains 26% of the variation in fund voting, while fund identity (which is nested within fund-family identity) explains 33%. Appendix Appendix G presents results when we examine voting policy at the fund-family level. The results are consistent with our main estimates: Funds in families with more AUM in index funds were more likely to vote with firm management.

funds to side with firm management on contentious agenda items. They are also less likely to abstain on contentious items. Moreover, index funds with higher expense ratios are less likely to side with firm management, voting more like active funds do. These results are consistent with the prediction that, owing to their incentives, index funds are weak monitors of the firms in their portfolios.

A. Categories of Agenda Item

A concern with our results in Table IV is that they might just describe index funds' voting behavior on less relevant agenda items, i.e. votes that do not affect corporate governance policies. Accordingly, in this section we examine how index funds' voting differs within categories of contentious items related to corporate governance issues. Similar to prior studies of corporate governance,[26] we examine the following voting categories:

1. Board of Directors: Items whose description includes "director" or "board";
2. Compensation: Items whose description includes "executive compensation". This category is mostly (83%) made up of say-on-pay votes;
3. Disclosure: Items whose description includes "disclosure" or "reporting";
4. Entrenchment: Items whose description includes "staggered", "bylaw", "poison pill" or "parachute".

We report results in Table V. In Column 1, we find that index funds are 13.2 percentage points more likely to side with management on contentious items relating to the board of directors. A small subset of these items relate to formal proxy battles between the incumbent

[26] E.g., Ertimur, Ferri, and Oesch (2017)

board and an activist shareholder. That is, our results in Column 1 are consistent with those of Brav et al. (2018), who focus on fund voting in proxy battles, and again inconsistent with the idea that index funds are strong monitors or their portfolio firms.

Next, in Columns 2 to 4 we find that the gap in fund voting between index and active funds is positive and of similar magnitude for items related to compensation, disclosure, and managerial entrenchment. Thus, on four important categories of agenda items related to firm governance, we document that index funds consistently side with management.

The results on managerial entrenchment are particularly relevant since some of the largest index funds publicly claim to be against certain governance practices such as poison pills and golden parachutes.[27] Yet we find that when it comes to voting on these issues, index funds continue to cede authority to management. Furthermore, these findings suggest that the rise of index investing has consequences for (at least) board structure, managerial compensation, disclosure, and managerial entrenchment. In other words, index funds give managers more power in decisions related to corporate governance across the board.

V. Other Monitoring Mechanisms

Three alternative hypotheses could explain our voting results. First, index funds could use exit instead as a monitoring mechanism to enforce good governance. We test the exit hypothesis in Section V.A, below. Second, it might be that index funds engage with managers either publicly or behind the scenes (McCahery et al. (2016)), and then vote in support of management proposals that they negotiated beforehand. We test the engagement hypothesis in Section V.B. Third, even if index funds do not actively engage with their portfolio firms, managers might be driven to appease these large shareholders by following policies that are

[27] See Bebchuk et al. (2008) and the ISG framework at https://isgframework.org.

preferred by index funds (Fisch et al. (2018)). We test the appeasement hypothesis in Section V.C.

A. Exit

In this section we examine the second channel by which shareholders monitor and exert influence: exit. According to Edmans (2009), Dasgupta and Piacentino (2015) and others, in addition to voting, shareholders can influence a firm's actions by selling the stock or threatening to sell the stock.

In Table VI we examine fund exit behavior. The dependent variable *VoluntaryExit* is equal to 1 if a fund exits a stock voluntarily as defined in Section II.B, and 0 otherwise. The independent variables of interest are *IndexFund* (as defined in Section II.B), *ActiveFund*, an indicator equal to 1 if a fund is an active fund, and 0 if a fund is an index fund, and *LostVote*, an indicator equal to 1 if a fund voted Yes on an item that failed (did not pass) or No on an item that passed. As in our prior analysis, we include firm and year fixed effects. First, in Columns 1, 3, and 5 we examine the probability of exit, whereas in Columns 2, 4, and 6 (see next Subsection A.1) we examine the probability of exit conditional on a voting outcome.

In Table VI Column 1, we find that across the full sample, index funds are 17.9 percentage points less likely to voluntarily exit a position relative to active funds. These findings are in line with the summary statistics in Section II.B, and suggest that index funds may use the exit channel as a monitoring mechanism, but they do so significantly less than active funds.

In Table VI Column 3, we estimate the probability of exit within our Russell cohort sample, and in Column 5 we add the Heckman correction term (*InverseMillsRatio*). The coefficient on the *InverseMillsRatio* is negative and statistically significant, which is consis-

tent with significant selection bias in studies of fund exit behavior.[28] However, there is little effect on the comparison between index and active funds' exit behavior. After the correction, index funds are still much less likely to exit a position than active funds (18.5 percentage points compared to 17.4 percentage points in the uncorrected OLS estimate).

A.1. Voting and Exit as Strategic Substitutes

We next examine fund exit behavior after a lost vote to provide an empirical test of theoretical models that predict strategic substitution between *voting* and *exit* (e.g., Edmans et al. (2018)). Specifically, when a fund loses a vote, theory predicts the fund will be more likely to exit the position.

The results are shown in Columns 2, 4, and 6 of Table VI. Across the full cross-section (Column 2) we find that if a fund "lost" a vote (that is, the fund voted Yes on an item that failed or No on an item that passed) over the previous year, an active fund is 0.9 percentage points *more* likely to exit that position the following year. On the other hand, an index fund that loses a vote is 0.4 percentage points *less* likely to exit. In Column 4, we estimate the probability of exit conditional on a voting outcome within our Russell cohort sample and we find results similar to those in Column 2. Finally, adjusting for both endogeneity and selection bias (Column 6), we continue to find that subsequent to a vote that went against their wishes, active funds are more likely to exit that position, while index funds are not.

These findings further support the notion that index funds make less use of the exit channel compared to active funds. Our results are consistent with strategic substitution between the voting and exit mechanisms, but only by active funds. Active funds – who are more likely to vote against management *a priori* – are also more likely to exit a position

[28]See Bhide (1993), and Edmans, Fang, and Zur (2013) that discuss the implications of liquidity for governance.

after a vote goes against them (Dasgupta and Piacentino (2015)). Thus, the difference in exit behavior conditional on previous voting outcomes is again consistent with weaker monitoring by index funds – given that voting and exit are strategic substitutes for funds to affect firm policy (e.g., Admati and Pfleiderer (2009); Edmans et al. (2018)).

Overall, in light of these empirical findings we reject the exit hypothesis and conclude that for both the voting and exit channels, index funds are relatively weak monitors of the firms in their portfolios.

B. Engagement

The engagement hypothesis predicts that index funds – rather than voting against management or exiting if they disagree with a firm's policies – may prefer to engage with a firm's management. Arguably, we cannot observe engagement between index funds and the managers of their portfolio firms (although the data in Bebchuk and Hirst (2019) suggest that these events are rather rare). However, we can test the engagement hypothesis in two ways.

First, we examine index funds' voting behavior on shareholder proposals. The intuition behind this test is that engagement could explain index funds' voting behavior on management proposals, but it cannot apply to *shareholder* proposals. In other words, "active" index funds could support management proposals that they negotiated (or proposed to managers) beforehand. But when it comes to vote on shareholder proposals "active" index funds should be more willing to oppose management. We present results for this analysis in the next Subsection B.1.

Second, in Subsection B.2, we further study the engagement hypothesis by examining index funds' propensity to file a Schedule 13D. Shareholders are required to disclose a holding

above 5% of the firm's market capitalization via either Schedule 13D, which allows the fund to officially engage with the firm, or Schedule 13G, which does not. Hence, if index funds are in fact active monitors of their portfolio firms they should be more likely to file a Schedule 13D than a Schedule 13G.

B.1. Voting on Shareholder Proposals

We report results for the first test of the engagement hypothesis in Table VII, where we split contentious items between items proposed by shareholders and items proposed by management and we estimate an OLS fixed-effects regression. We see that the pattern that index funds are more likely to side with management holds true regardless of who proposed the agenda item. On management proposals that are opposed by ISS, index funds are 14.4 percentage points more likely to vote with management. On the other hand, on shareholder proposals that are opposed by management, when "active" index funds should be more willing to support the shareholders, index fund are still 10.3 percentage points more likely to vote with management.

The results in Table VII allow us to address the hypothesis that index funds might be voting in agreement with management after they coordinated with managers behind the scenes. Arguably, such a story may apply to index funds' voting on proposals by management. However, it cannot apply to index funds' voting on proposals by shareholders. In other words, if an index fund is an active monitor, it should be willing to oppose management on contentious shareholder proposals. Yet we find the opposite: For contentious votes on shareholder proposals, index funds again cede authority to management. These results are echoed in a contemporaneous working paper by Brav et al. (2018). They show that in proxy contests, an important and contentious category of shareholder proposals, index funds

do not support activist shareholders but instead side with firm management.

B.2. Disclosure: Schedule 13D vs Schedule 13G

Next we examine funds' propensity to publicly signal their intention to be active monitors by filing a 13D schedule. The SEC requires shareholders to disclose a holding above 5% of any public company via either Schedule 13D or Schedule 13G. Schedule 13D is required if the shareholder has *"the purpose or the effect"* of influencing the control of the firm. This category includes actions such as *"proposing governance changes... or engaging with the portfolio company to propose or facilitate the appointment of particular individuals as directors"* (Bebchuk and Hirst (2019)). The short-form Schedule 13G, by contrast, requires that the shareholder has no such purpose or effect. A blockholder who files Schedule 13G and then engages with firm management opens themselves up to SEC investigations or class action lawsuits.[29]

Table VIII presents fixed-effects probit estimates.[30] The dependent variable is an indicator variable for whether each filing was under the "activist" Schedule 13D (*Filed 13D*=1) or the short-form and passive Schedule 13G (*Filed 13D*=0). The independent variable $FracAUMPassive_{jt}$ is the fraction of fund family j's AUM that was managed by index funds in year t. Thus it ranges from 0 for a fund family entirely populated by active funds, to 1 for a fund family entirely populated by index funds. Column 1 shows that fund families with more index fund assets under management are significantly less likely to file Schedule 13D. The marginal effect (which corresponds to moving from 100% active to 100% passive) is -27 percentage points, which is more than 100% of the base rate probability. The same

[29]E.g. Levie v Sears Roebuck & Co, 2009.

[30]Because blockholdings are disclosed at the level of the fund family, we match disclosure filings to fund families and not to the individual funds. In all, we match 30,864 disclosure filings since 2004 to a fund family in our data.

conclusion holds when we control for the fund family's total AUM (Column 2) and for the number of blockholding disclosures the family filed in that year (Column 3).

Thus, more passive fund families (those with more index-fund assets under management) are less likely to file Schedule 13D and more likely to file Schedule 13G. However, because this analysis is at the fund-family level, these results do not directly measure the individual funds' propensity to engage. In a further step we match blockholdings by individual funds, as revealed in the merged S12 and CRSP holdings data, to SEC disclosure filings by that fund's parent family. We keep only matches that are unambiguous at the fund-firm-year level. In all, we match 4,475 disclosure filings to individual funds. For active mutual funds, 64 of 4085 filings were under Schedule 13D. For index funds, 0 of 390 filings were under Schedule 13D (two-sample t-statistic=8.1).

Thus, both at the fund family level and at the individual fund level, index funds are less likely to file the activist Schedule 13D and more likely to file the passive Schedule 13G. These findings are inconsistent with the hypothesis that index funds affect governance through engagement with their portfolio firms.

C. Changes in the Supply of Agenda Items

Finally, we test the appeasement hypothesis, which predicts that even if index funds do not actively engage with their portfolio firms, managers might still be driven to follow policies that are preferred by index funds (Fisch et al. (2018)). In other words, the mere fact of higher index fund ownership might drive firm managers to follow the index funds' stated preference with no need for engagement.

We test this hypothesis by examining the effect of changes in fund holdings on the number and types of agenda items that appear at the firm's annual meeting. Specifically, we use

the firm-year panel and estimate the difference-in-differences specification as in equation 2. These estimates show the changes in the number of agenda items and the proportion of agenda items supported by management and/or ISS that are caused by exogenous changes in index fund ownership.

Table IX presents the results. Neither set of treated firms (i.e. firms that switched indexes in either direction) significantly changed the number of agenda items at their annual meetings in the post-treatment period (Column 1); there was no change in the number of proposals by firm management or by shareholders (Columns 2 and 3). Moreover, we observe no change in the fraction of agenda items that were opposed by management (Column 4) or opposed by ISS (Column 5). Finally, if index funds indirectly encourage better governance in their portfolio firms, we would expect to see an increase in the fraction of agenda items that were approved by ISS and management together. We see no such change (Column 6).[31]

Thus, the results in Table IX are inconsistent with the hypothesis that index fund holdings lead to a change in the supply of consensus agenda items that are up for a vote at firms' annual shareholder meetings. These results are inconsistent with the appeasement hypothesis, and consistent with index funds being weak monitors of their portfolio companies.

VI. Conclusion

We examine whether the rise of passive investing has implications for monitoring and corporate governance. While the increasingly large positions held by passive index funds should motivate them to monitor their portfolio firms, these new intermediaries also have different incentives than managers of traditional active funds.

[31]In untabulated results, we also find no change in the fraction of contentious items (regardless of who – ISS or management – opposed the agenda item). Moreover, we find no changes in the number of agenda items (i) opposed by ISS, (ii) opposed by management, or (iii) consensus items.

Our results show that passive funds are weak monitors of the firms in their portfolios, consistent with theoretical predictions in Bebchuk et al. (2017), Edmans et al. (2018), and Bebchuk and Hirst (2019). We find that, relative to active funds, passive funds are significantly more likely to side with firm management on contentious corporate governance votes. Passive funds are also less likely to exit their position in a firm, both unconditionally and after they lose a vote. Furthermore, we find that passive funds rarely if ever file a Schedule 13D, which indicates that they do not intend to affect firm policies. Finally, when firms experience an exogenous shift in passive fund ownership there is no change in the number and contentiousness of management and shareholder proposals, inconsistent with firms adopting policies preferred by passive funds in order to appease those funds.

Uniformly, our findings show that passive funds cede power to firm managers. These results are consistent with the theoretical prediction that the rise of passive index investing is weakening corporate governance by exacerbating the agency conflict between managers and investors.

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Wooldridge, J. (2008). *Introductory econometrics: A modern approach*. South-Western College Pub.

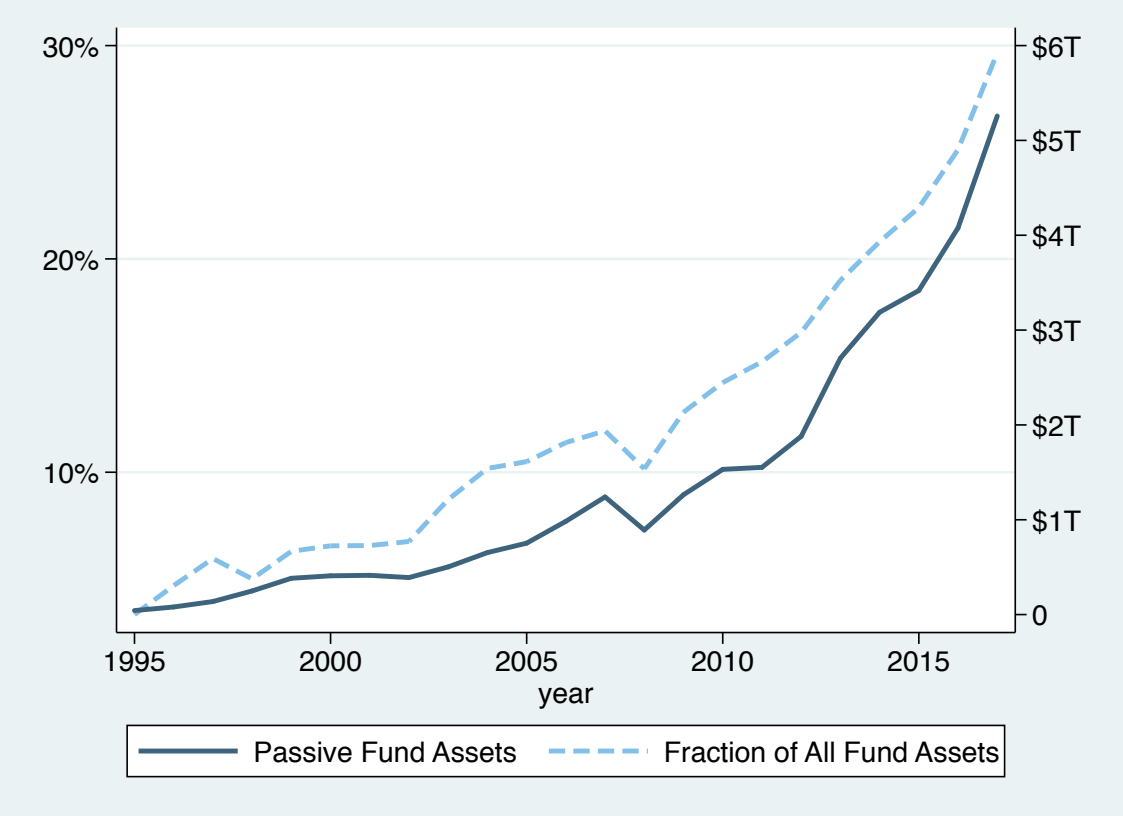


Figure 1. Yearly Passive Assets Under Management

The figure plots the total assets under management (AUM) for index funds in the CRSP Mutual Fund database, by year, in total dollars (solid line) and as a fraction of AUM (dashed line) across all funds.

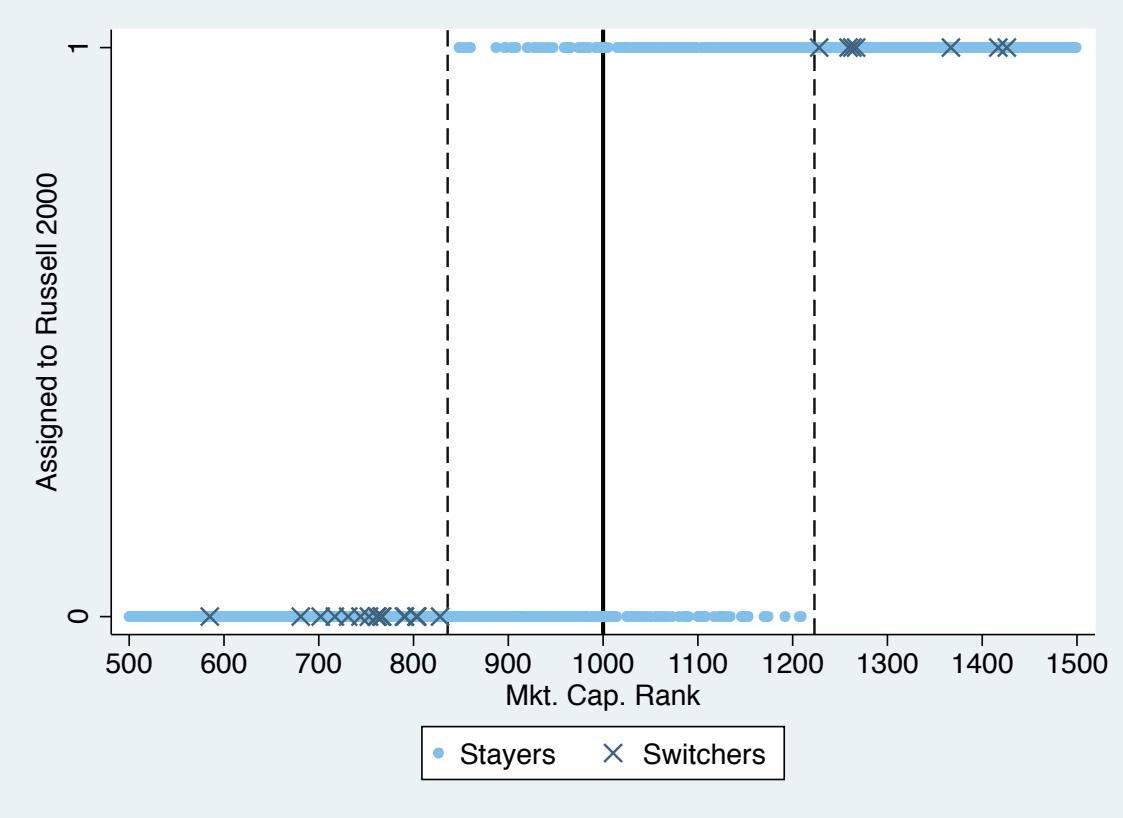


Figure 2. Index Assignment Post-Banding

The figure plots assignments to the Russell 1000 and 2000 indexes in June of 2007 (vertical axis) against our proxy for Russell's proprietary market cap rankings (horizontal axis). In 2007, the first year of the banding regime, stocks near the threshold all stayed in their previous years' index, breaking the discontinuity in index assignment at rank 1000. Close to the estimated upper and lower bands (dashed lines), however, there are clear discontinuities in index switching.

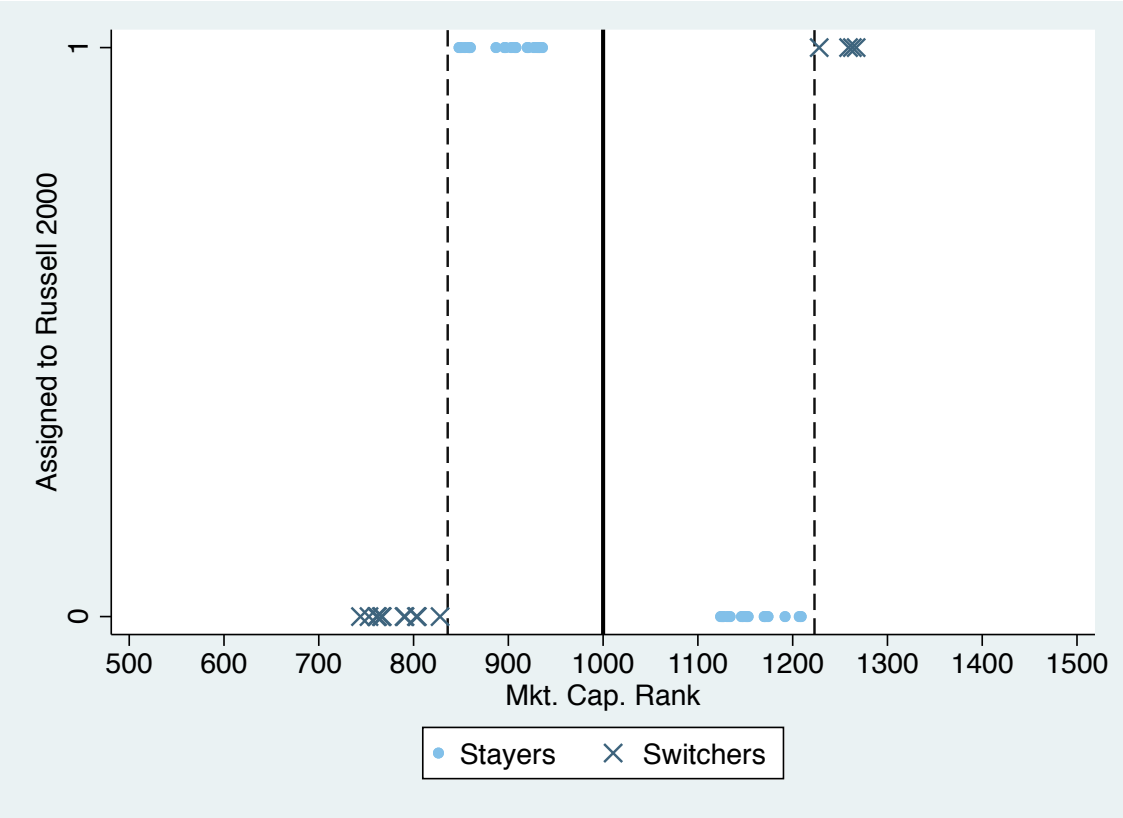


Figure 3. Sample Selection

The figure plots the sample for the 2007 cohort consisting of all Russell stocks that lay within a +/-100 rank window of the upper and lower bands, and are potential switchers, i.e. were in the Russell 2000 in 2006 for those near the upper band or were in the Russell 1000 in 2006 for those near the lower band.

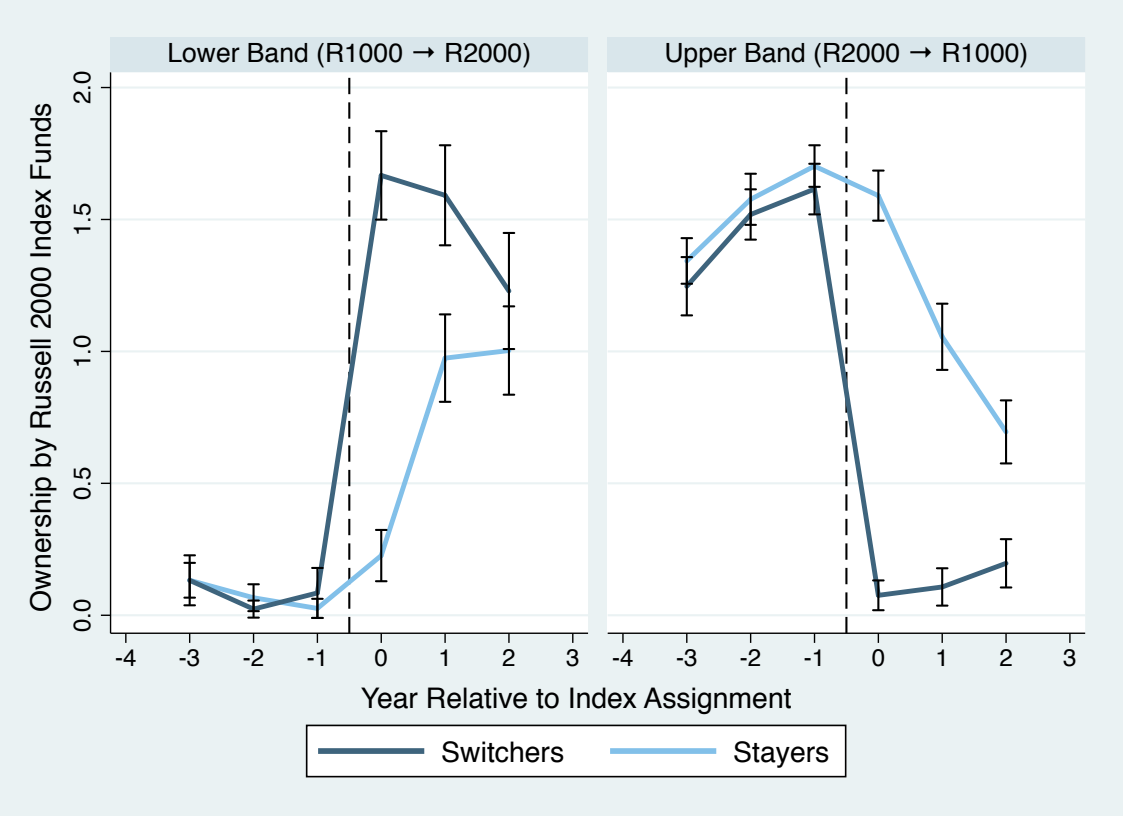


Figure 4. Index Switching and Index Fund Ownership

The figure plots the evolution of index fund ownership in event time relative to index assignment. On the left hand side is average ownership by Russell 2000 index funds, in event time, for firms near the lower band that were in the Russell 1000 prior to index assignment. On the right hand side is average ownership by Russell 2000 index funds, in event time, for firms near the upper band that were in the Russell 2000 prior to index assignment. The error bars represent 95% confidence intervals.

Table I
Summary Statistics of Fund Voting

The table summarizes the ISS voting data and presents comparisons of fund voting between active and index investment funds. The table shows the fraction of each type of fund that voted Yes, No, Abstain or that failed to vote ("did not vote", DNV) on each agenda item across all shareholder meetings of U.S. firms recorded by ISS from 2003-2017. N is the number of individual fund-vote observations.

Management	ISS	Index funds				Active Funds				Difference	
Recommend	Recommend	Yes	No	Abstain	DNV	Yes	No	Abstain	DNV	PctYes	N
All		90.4%	6.2%	3.2%	0.2%	89.4%	7.1%	3.1%	0.4%	1.0%	23,221,799
Consensus											
Yes	Yes	95.6%	2.8%	1.4%	0.1%	96.0%	2.6%	1.1%	0.3%	-0.4%	20,669,238
No	No	4.2%	84.6%	8.8%	2.4%	5.1%	82.7%	10.7%	1.5%	-0.9%	362,447
Contentious											
Yes	No	54.3%	19.0%	24.9%	1.8%	41.9%	25.1%	30.4%	2.5%	12.4%	1,426,904
No	Yes	41.5%	53.5%	4.9%	0.1%	47.7%	46.0%	6.0%	0.3%	-6.2%	763,210

Table II
Summary Statistics of Firms and Funds

Panel A presents statistics for firms in our Russell cohort sample from 2004 to 2017. Sample firms are selected on lagged index membership and proximity to the upper and lower Russell bands in each June cohort from 2007 to 2015. Each firm is included for three years before and after its cohort year. Panel B presents statistics for all funds – assets under management (AUM), yearly expense ratio, and the number of firms held – in our sample from 2004 to 2017.

Panel A: Russell Sample Firms

	Mean	Std. Dev.	10th Pctile	Median	90th Pctile	Firm-Years
Market Cap ($M)	2,456	920	1,354	2,394	3,815	4,392
$PassiveOwn^{R2000}$	0.93%	1.00%	0.00%	0.63%	2.29%	4,392
$PassiveOwn^{R1000}$	0.09%	0.12%	0.00%	0.00%	0.27%	4,392
$PassiveOwn$	3.86%	2.60%	0.46%	3.72%	7.26%	4,392
$ActiveOwn$	5.70%	4.71%	0.39%	4.78%	11.66%	4,392
$TotalFundOwn$	9.56%	5.93%	1.58%	9.25%	16.70%	4,392
E-Index (/ 6)	3.2	1.2	2	3	5	2,036

Panel B: All Funds

	Mean	Std. Dev.	10th Pctile	Median	90th Pctile	Fund-Years
			Index Funds			
AUM ($M)	3,335	16,769	31	344	4,924	5,698
Expense Ratio	0.47%	0.33%	0.15%	0.43%	0.74%	5,698
# Firms Held	370.6	593.8	14	109	971	4,763
			Active Funds			
AUM ($M)	2,246	7,826	35	391	4,391	25,807
Expense Ratio	1.16%	0.41%	0.68%	1.12%	1.72%	25,807
# Firms Held	115.9	228.1	12	62	230	20,940

Table III
Index Switching and Fund Ownership

The table presents estimates of the effects of Russell index switches on investment fund ownership expressed as a percentage (1=1%) of stocks' market capitalization. The sample consists of stocks that were "potential switchers" within a +/- 100-rank window of the yearly Russell upper and lower bands from 2007 to 2015, three years before and after index assignment for each firm in each cohort. Robust standard errors clustered by firm are shown in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) $PassiveOwn_{jt}^{R2000}$	(2) $PassiveOwn_{jt}^{R1000}$	(3) $PassiveOwn_{jt}^{S\&P500}$	(4) $PassiveOwn_{jt}$	(5) $ActiveOwn_{jt}$	(6) $TotalFundOwn_{jt}$
$R1000 \rightarrow R2000_j \times$	1.45***	-0.18***	-0.03**	1.03***	-0.06	0.97*
$PostAssignment_t$	(0.10)	(0.01)	(0.01)	(0.24)	(0.36)	(0.48)
$R2000 \rightarrow R1000_j \times$	-1.34***	0.17***	0.02***	-0.86***	-0.06	-0.93**
$PostAssignment_t$	(0.08)	(0.02)	(0.01)	(0.14)	(0.27)	(0.34)
Observations	4,392	4,392	4,392	4,392	4,392	4,392
Adjusted R^2	0.468	0.474	0.361	0.674	0.569	0.582
Years	2004-2017	2004-2017	2004-2017	2004-2017	2004-2017	2004-2017
Cohorts	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table IV
Fund Voting

The table presents comparisons of fund voting, on contentious items only, for index funds versus active funds. Columns 1-2 show estimates for all firms in the sample. Columns 3-6 show estimates for firms that were potential switchers near the yearly Russell bands from 2007-2015. *ExpenseRatio* is the fund's total expense ratio in that year expressed in percentage points (so 25 basis points = 0.25). The sample consists of votes on contentious items i.e. items on which ISS and firm management were opposed. Robust standard errors clustered by fund are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) $VotedWithMgmt$	(2) $VotedWithMgmt$	(3) $VotedWithMgmt$	(4) $VotedWithMgmt$	(5) $VotedWithMgmt$	(6) $VotedWithMgmt$
$IndexFund_i$	0.125***	0.126***	0.150***	0.150***	0.084***	0.079***
	(0.025)	(0.024)	(0.030)	(0.030)	(0.032)	(0.029)
$InverseMillsRatio_{ijt}$					-0.114	-0.111
					(0.040)	(0.034)
$ExpenseRatio_{it}\times$		-0.238***		-0.209**		-0.209**
$IndexFund_i$		(0.073)		(0.085)		(0.084)
$ExpenseRatio_{it}\times$		0.021		0.071		0.071
$ActiveFund_i$		(0.046)		(0.060)		(0.060)
Model	OLS	OLS	OLS	OLS	Heckman	Heckman
Sample Firms	All	All	Russell	Russell	Russell	Russell
Observations	2,187,598	2,187,598	189,319	189,319	189,319	189,319
Adjusted R^2	0.074	0.083	0.076	0.084	0.076	0.084
Firm FE	Yes	Yes	No	No	No	No
Firm × Cohort FE	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table V
Fund Voting – Split on Item Type

The table presents comparisons of fund voting on contentious items between index funds versus active funds, in the full sample of firms, splitting contentious agenda items into categories as defined in the text. The sample consists of votes on contentious items (i.e. items on which ISS and firm management were opposed). Robust standard errors clustered by fund are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Item Type			
	Board of Directors	Compensation	Disclosure	Entrenchment
	$VotedwithMgmt$	$VotedwithMgmt$	$VotedwithMgmt$	$VotedwithMgmt$
$IndexFund_i$	0.132***	0.127***	0.095***	0.116***
	(0.029)	(0.028)	(0.029)	(0.026)
Observations	1,173,740	44,953	106,314	77,189
Adjusted R^2	0.086	0.057	0.021	0.101
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table VI
Fund Exit

The table presents comparisons of voluntary exit between index funds versus active funds. Columns 1-2 show estimates for all firms in the sample. Columns 3-6 show estimates for firms that were potential switchers near the yearly Russell bands from 2007-2015. Robust standard errors clustered by fund are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels respectively.

	(1) $VoluntaryExit$	(2) $VoluntaryExit$	(3) $VoluntaryExit$	(4) $VoluntaryExit$	(5) $VoluntaryExit$	(6) $VoluntaryExit$
$IndexFund_i$	-0.179***	-0.138***	-0.174***	-0.136***	-0.185***	-0.141***
	(0.012)	(0.012)	(0.015)	(0.014)	(0.015)	(0.014)
$InverseMillsRatio_{ijt}$					-0.021***	-0.008**
					(0.005)	(0.004)
$ActiveFund_i \times LostVote_{ijt-1}$		0.009**		0.005		0.005
		(0.004)		(0.008)		(0.006)
$IndexFund_i \times LostVote_{ijt-1}$		-0.004		-0.007		-0.007
		(0.004)		(0.007)		(0.007)
Model	OLS	OLS	OLS	OLS	Heckman	Heckman
Sample Firms	All	All	Russell	Russell	Russell	Russell
Observations	4,192,281	2,211,016	452,902	282,738	452,902	282,738
Adjusted R^2	0.093	0.074	0.072	0.058	0.072	0.058
Firm FE	Yes	Yes	No	No	No	No
Firm × Cohort FE	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table VII
Fund Voting on Proposals by Management versus Shareholders

The table presents comparisons of fund voting on contentious items between index funds versus active funds, in the full sample of firms, splitting contentious agenda items into items proposed by firm management and items proposed by shareholders. The sample consists of votes on contentious items (i.e. items on which ISS and firm management were opposed). Robust standard errors clustered by fund are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Management Proposals			Shareholder Proposals		
	$VotedYes$	$VotedNo$	$Abstained$	$VotedYes$	$VotedNo$	$Abstained$
$IndexFund_i$	0.144***	-0.050***	-0.085***	-0.092***	0.103***	-0.009
	(0.031)	(0.011)	(0.020)	(0.023)	(0.022)	(0.008)
Observations	1,408,736	1,408,736	1,408,736	778,846	778,846	778,846
Adjusted R^2	0.079	0.232	0.218	0.089	0.071	0.055
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table VIII
Blockholding Disclosures: Schedule 13D versus Schedule 13G

The table presents comparisons of fund families' blockholding disclosure filings. "Filed 13D" is an indicator variable for whether each filing was under the activist Schedule 13D as opposed to the passive Schedule 13G. $FracAUMPassive$ is the fraction of fund family j's assets under management (AUM) that was managed by index funds in year t. $logAUM$ is the logarithm of the fund family's total AUM. $numFilings$ is the number of blockholding disclosures the family filed in that year. Robust standard errors clustered by fund family are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels respectively.

	(1) Filed 13D	(2) Filed 13D	(3) Filed 13D
$FracAUMPassive_{jt}$	-1.13**	-1.05**	-1.15**
	(0.48)	(0.46)	(0.49)
$logAUM_{jt}$		-0.052	
		(0.042)	
$numFilings_{jt}$			0.00028
			(0.00032)
Model	Probit	Probit	Probit
Observations	920	920	921
Pseudo R^2	0.018	0.018	0.018

Table IX
Changes in the Supply of Agenda Items

The table presents comparisons of the number and type of agenda items at sample firms' shareholder meetings. $NumItems$ is the number of agenda items voted on in a given year. $NumShrProp$ and $NumMgmtProp$ is the number of items tabled by shareholders and management, respectively. $FracISSAgainst$ and $FracISSMgmtAgainst$ are the fraction of agenda items that were opposed by ISS and firm management respectively. $FracConsensus$ is the fraction of agenda items for which ISS and management made the same recommendation. Robust standard errors clustered by firm are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) $NumItems_{jt}$	(2) $NumShrProp_{jt}$	(3) $NumMgmtProp_{jt}$	(4) $FracISSAgainst_{jt}$	(5) $FracMgmtAgainst_{jt}$	(6) $FracConsensus_{jt}$
$R1000 \rightarrow R2000_j \times$	0.02	-0.02	0.05	-0.01	0.003	0.012
$PostAssignment_t$	(0.34)	(0.07)	(0.32)	(0.02)	(0.004)	(0.017)
$R2000 \rightarrow R1000_j \times$	-0.28	0.001	-0.29	-0.00	0.004	-0.00
$PostAssignment_t$	(0.37)	(0.03)	(0.37)	(0.01)	(0.003)	(0.013)
Observations	3,726	3,726	3,726	3,726	3,726	3,726
Adjusted R^2	0.614	0.119	0.623	0.430	-0.031	0.431
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

VII. Online Appendix

A. Data Description

Russell index membership data come directly from Russell. Stock trading and firm accounting data are from CRSP and merged CRSP-Compustat. We use the most recent data for each firm from June 1 to the following May 31 of each year.

Data on funds comes from the CRSP Mutual Fund database. We classify funds as index or active using their index fund flag in CRSP. Both mutual funds and ETFs are included in our sample.

Data on funds' holdings is based on the union of the Thomson Reuters S12 database and the CRSP Mutual Fund Holdings database. Our measures of fund holdings are defined below. All holdings measures are for each stock i as of December in year t, and are expressed as a percent of the stock's market capitalization.

B. Variable Descriptions

In this section we define all variables used in the paper.

$VotedwithMgmt$: An indicator variable for each fund's vote on each agenda item, that equals 1 if (i) management recommended a Yes vote and the fund voted yes or (ii) management recommended a No or Withhold vote and the fund voted No or Abstain, and equals 0 otherwise.

$IndexFund_i$: An indicator variable that equals 1 if the CRSP index fund flag is "D". This corresponds to a "pure" index fund whose objective is to replicate a benchmark. Otherwise the fund is classified as an active fund ($IndexFund_i = 0$).

$TotalFundOwn_{jt}$: The fraction of firm j's market cap held in December of year t by all

mutual funds based on the combined S12 and CRSP holdings data.

$ActiveOwn_{it}$: The fraction of firm j's market cap held in December of year t by active mutual funds.

$PassiveOwn_{it}$: The fraction of firm j's market cap held in December of year t by index mutual funds.

$PassiveOwn_{jt}^{R2000}$: The fraction of firm j's market cap held in December of year t by index funds whose name contains "Russell" and "2000".

$PassiveOwn_{jt}^{R1000}$: The fraction of firm j's market cap held in December of year t by index funds whose name contains "Russell" and "1000".

$PassiveOwn_{jt}^{S\&P500}$: The fraction of firm j's market cap held in December of year t by index funds whose name contains "S&P500" or "SandP500" or "SP500".

$ExpenseRatio_{it}$: The total expense ratio of fund i in year t. For funds with multiple share classes this is the average total expense ratio, weighted by the assets under management (AUM) in each share class.

$Observed_{ijt}$: An indicator variable that equals 1 if fund i held at least one share of firm j in December of year t and 0 otherwise.

$VoluntaryExit_{ijt}$: An indicator variable that equals 1 if (i) fund i held at least one share of firm j in December of year t (that is, $Observed_{ijt} = 1$) and (ii) fund i did not hold any shares of firm j in December of year $t+1$ and (iii) both fund i and firm j were still in the sample as of year $t+1$.

$LostVote_{ijt}$: An indicator variable that equals 1 if during year t, on at least one agenda item at firm j, the fund i voted Yes and the item failed, or voted No or Abstain and the item passed.

$Filed13D$: An indicator variable for each blockholding disclosure filing, which equals 1

if the filing used Schedule 13D and 0 if it used Schedule 13G.

$FracAUMPassive_{jt}$: The fraction of total assets under management (AUM) of the fund family j in year t that were managed by index funds.

$logAUM_{jt}$: The natural logarithm of the total AUM of fund family j in year t.

$numFilings_{jt}$: The total number of blockholding disclosure filings by fund family j in year t.

$DailyRtn_{ik}$: The market-adjusted return to firm i's stock on the date when agenda item k was officially voted upon.

$VotedYes_{ik}$: An indicator variable that equals 1 if fund i voted Yes on item k and 0 otherwise.

$ItemPassed_k$: An indicator variable that equals 1 if item k passed and 0 if item k failed to pass.

C. Selection of the Yearly Cohorts

The following steps describe the selection of the June 2007 cohort:

1. Rank all qualifying U.S. common stocks by their unadjusted market capitalization as of the last business day in May 2007.

2. Select all stocks that i) ranked within +/-N ranks of the upper band and ii) were members of the Russell 2000 as of May 2007. This is the set of potential switchers near the upper band.

3. Select all stocks that i) ranked within +/-N ranks of the lower band, and ii) were members of the Russell 1000 as of May 2007. This is the set of potential switchers near the lower band.

4. For each selected stock, collect its information from CRSP-Compustat over the 3 years prior to index assignment (here, 2004-2006) and the 3 years post index assignment (here, 2007-2009). Add all collected firm-years to the sample.

Thus, our research design is a cohort design that compares outcomes Y for stock i in cohort c for three years pre-treatment versus three years post-treatment. The pre-vs-post periods are compared between:

1. Stocks near the upper band that switched out of the Russell 2000 versus those that stayed (coefficient β_1) .

2. Stocks near the lower band that switched *into* the Russell 2000 versus those that stayed (coefficient β_2).

D. Balance Tests

It is important that our treated and control firms are similar *ex ante* and differ only by their index switching status. For example, if the firms just above the upper band were systematically worse governed than the firms just below, our results would be biased toward finding a spurious association between index fund investment and good governance. We run balance tests to check that treated and control firms on either side of both bands are similar *ex ante*, particularly in terms of fund ownership and firm governance.

In Panel A of Table A2 we compare pre-treatment means of total market cap and fund ownership for switchers versus stayers. In each case we measure market cap or fund ownership in the last pre-treatment year. There is no significant difference between firms that ultimately switch compared to those that stay, in market cap or any of the categories of fund ownership.

Similarly, Table A2 Panel B compares measures of firm governance in the last pre-treatment year. Specifically, we examine the entrenchment index (*E-Index*) of Bebchuk et al. (2008) as well as its six individual components. Again, we find no significant difference in any of the governance measures between treated and control firms *ex ante*. In sum, in Table A2 we find no evidence of differences in pre-treatment firm characteristics between treated and control firms.

Figure A2 presents formal regression discontinuity (RD) plots for fund ownership and firm governance, measured in the last pretreatment year for each firm, with local polynomial control functions fitted on either side of each band. Again, we observe no significant difference at the treatment cutoff (the upper or lower band respectively). Furthermore, in each case the treated and control firms also have similar overall *levels* of fund ownership and governance. Hence, we conclude that our treated and control groups are well-balanced *ex ante*, and that our research design does not suffer from selection bias (Wei and Young (2017)).

E. Pretrends

The results in the previous section suggest that our treated and control firms on either side of both bands are similar *ex ante* in terms of fund ownership and firm governance. However, this is not strictly necessary for the validity of our difference-in-differences design, as the firm-by-cohort fixed effects difference out any non-time-varying imbalance between treated and control firms. Rather, the exclusion restriction is *parallel trends*: in the absence of treatment, the treated firms would have had the same average trend in outcomes as the control firms did. This is inherently untestable, but the standard test of its plausibility is to compare trends in outcomes between treated and control firms in the years prior to treatment (pretrends).

In Panel A of Table A3 we compare pretrends in market capitalization and fund ownership for switchers versus stayers. We compute the pretrend for each firm in each cohort by regressing the outcome variables measured in years -3, -2, -1 on event-time t plus firm and calendar-year fixed effects. There is no significant difference in pretrends between firms that ultimately switch compared to those that stay, in terms of market capitalization or any of the categories of fund ownership.

Similarly, Table A3 Panel B compares pretrends in firm governance. Specifically, we examine the entrenchment index (*E-Index*) of Bebchuk et al. (2008) as well as its six individual components. Again, we find no significant difference in any of the governance measures between treated and control firms *ex ante*. In sum, in Table A3 we find no evidence of differences in the pretrends of market cap, fund ownership or firm governance between treated and control firms.

Figure A3 presents formal regression discontinuity (RD) plots for pretrends in fund ownership and firm governance, with local polynomial control functions fitted on either side of each band. Again, there is no significant difference at the treatment cutoff (the upper or lower band respectively). Furthermore, in each case the treated and control firms also have similar overall pretreatment trends in fund ownership and governance across the full range of sample market cap rankings.

Hence, we conclude that our treated and control groups show no evidence of differences in their pretreatment trends.

F. Varying the Selection Window

Next, we present the results when we vary the size of the window, around each band in each year, in which our treated and control firms must fall. If measurement error in the

forcing variable is affecting our results – in particular, biasing our estimates due to selection or causing our estimates to have low power – then as we narrow the window our results should disappear and vice versa.

Table A4 presents the results when we vary the window of selection around the bands each year. In contrast to the prediction above, with both the narrower and wider windows, across both bands and across all categories of fund ownership, the results are very similar to our main estimates in Table III. Thus, the results in Table A4 are inconsistent with selection bias in our setting.

G. Alternate Specifications

This section looks at our results on fund voting in alternative ways.

First, our main specification of choice includes firm and year fixed effects, which mitigate concerns of bias due to endogeneity because they compare how index funds and active funds voted within the same firm and within the same year. Table A5 presents estimates that compare fund voting within firm-years and within individual agenda items. The difference in voting between index funds and active funds is identical to the firm and year fixed effects specification. These results suggest that firm and year fixed effects account for most of the relevant variation across firm-years and also across agenda items. In other words, within our panel, relevant characteristics such as firm governance or shareholder engagement vary widely between firms and perhaps in the aggregate over time, but vary little within firms over time.

Second, most mutual funds belong to a fund family, and fund voting is often coordinated at the family level. We next investigate fund voting when index versus active status is measured at the level of the fund family. Table A6 presents results for comparisons of fund

voting in which the independent variable $FracPassive_{it}$ is the fraction of fund i's *family's* total AUM that was passively managed in year t. We see that the pattern in fund voting is consistent with our main estimates and indeed stronger: A fund belonging to a family that was 100% passive was 32-34% more likely to vote with firm management on a contentious agenda item compared to a fund belonging to a family that was 100% active, across a variety of specifications.

Third, we investigate funds' decision to abstain from voting. Table A7 shows that across a range of estimates, index funds are significantly *less* likely than active funds to abstain on contentious votes. This result is consistent with the argument of Del Guercio et al. (2008) and Bebchuk et al. (2017) that it is costly for shareholders to openly oppose firm management. That index funds abstain less than active funds suggests again that index funds openly cede power to management, whereas active funds may prefer to either directly oppose firm management (vote No) or abstain (soft No).

H. Identification when the Forcing Variable is Measured with Noise

This section provides additional detail on the motivation for our research design as outlined in section B.

Pei and Shen (2017) point out that when treatment is assigned by an otherwise arbitrary threshold (a regression discontinuity setting), treatment status is observed perfectly, but the forcing variable is observed with noise, conventional RDD estimates may be biased away from zero – that is, produce spurious estimates of the treatment effect. To illustrate this, we simulate data and show that i) a variety of regression discontinuity designs generate spurious estimates, and ii) our panel difference-in-differences research design recovers the true treatment effect.

We simulate a sample of 200 firms ranked from -100 to +100 with a treatment threshold at rank = 0. We generate an outcome variable that is a smooth function of the true ranking, with a zero treatment effect – that is, there is no "jump" at the threshold (see Figure A1 panel A). We then add normally distributed measurement noise to each firm's ranking and re-sort them on the basis of the ranking measured with noise. Figure A1 panel B shows the result graphically. The control functions on either side of the threshold are attenuated i.e. their slope is too flat, and as a result there is a large, spurious jump in the outcome variable at the threshold.

Table A8 columns 1-6 show that a variety of RDD estimators produce a large and significant spurious treatment effect. We proceed from the insight that if the econometrician observes the sample firms repeatedly – at least once before and after treatment – she can instead use a firm-year panel to compare the change in outcomes before and after treatment, for treated versus untreated firms near the cutoff, with firm fixed effects. The firm fixed effects eliminate the need for a control function – since each firm has a single ranking relative to the cutoff, any control function would be absorbed by the fixed effects – and eliminates the bias that is present in the RDD estimates. Table A8 column 7 shows the result when we expand the simulated data for each firm to three years pre- and post-treatment, and run the difference-in-differences (DiD) estimate with firm fixed effects. We see that the DiD estimate recovers the true treatment effect.

I. Announcement Returns

After documenting uniform evidence that index funds are weak monitors of their portfolio firms, we finally examine whether their monitoring behavior exacerbates agency conflicts in a way that impacts firm value. To do this, we examine the stock market reaction on the

days when agenda items are decided, conditional on how different funds voted. This test also provides additional evidence to rule out the alternative hypothesis that index funds intervene to improve firms' governance through unobserved channels. In other words, if stock returns do not react to fund voting, then index funds might be "rationally weak" monitors, who eschew costly monitoring actions that do not affect firm strategy. But, if (i) index funds affect firm value positively by other means (e.g., engagement), or (ii) the difference in voting between active and index funds is immaterial to firm outcomes, then average announcement returns conditional on index funds' voting should be positive or zero, respectively.

In Table A9 we present results for the comparisons of announcement returns to the firm's stock on the day each item is decided. We condition on (i) whether the fund voted for the item or against it (we use *VotedYes*, an indicator equal to 1 if a fund voted yes on an agenda item, and 0 otherwise), (ii) whether the item passed or failed (we use *ItemPassed*, an indicator equal to 1 if an agenda item passed, and 0 otherwise), and (iii) whether the fund was an index fund or active fund (we use *IndexFund* as previously defined). We estimate the following equation, where i denotes funds and k denotes agenda items:

$$\begin{aligned} DailyRtn_{ik} = {} & \beta_1 IndexFund_i \times VotedYes_{ik} \times ItemPassed_k \\ & + \beta_2 IndexFund_i \times VotedYes_{ik} \\ & + \beta_3 ActiveFund_i \times VotedYes_{ik} \times ItemPassed_k \\ & + \beta_4 ActiveFund_i \times VotedYes_{ik} \\ & + \text{Main Effects} + \text{Fixed Effects} + \epsilon_{ik} \end{aligned} \tag{5}$$

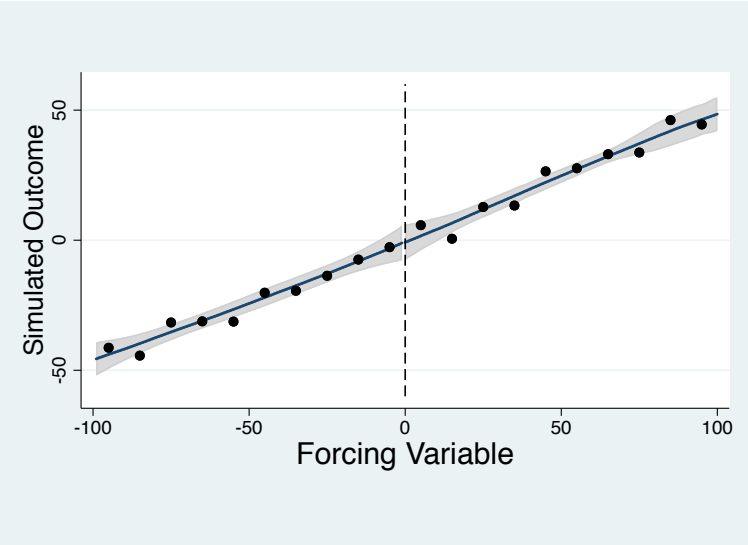
Thus, the first two coefficients β_1, β_2 compare the average announcement return for agenda items which index funds supported when the item passes versus when it fails. The

third and fourth coefficients β_3, β_4 compare the average return for agenda items which active funds supported when the item passes versus when it fails.

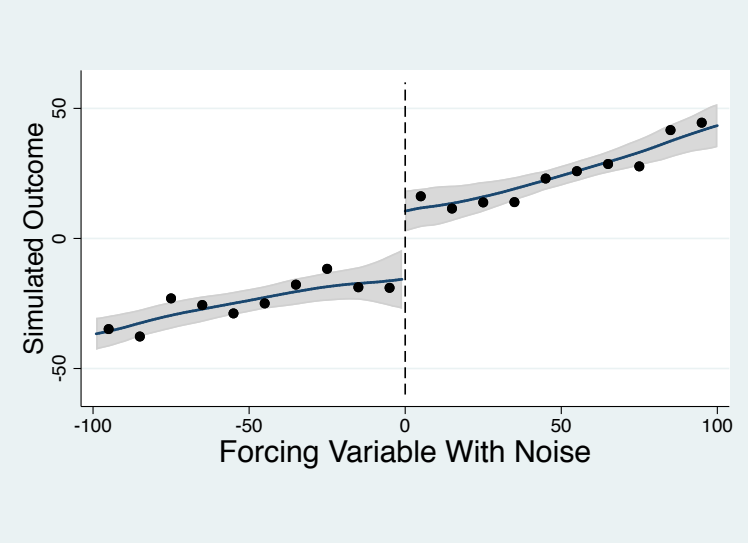
We find that when an index fund votes for an item that passes, the firm's stock falls by 4 basis points, while when an index fund votes in favor of an item that fails, the firm's stock rises by 4 basis points (Column 1). By contrast, the difference in announcement returns conditional on active funds' voting is of the opposite sign: higher when the item passes, and lower when the item fails. These results suggest that the difference in funds' voting behavior may have an impact on firm value. On average, the agenda items that index funds support reduce firm value when they pass, and raise firm value when they fail. Conversely, the results for active funds' voting have the opposite signs, consistent with the idea that weak monitoring reduces shareholder value.

In Column 2 we repeat the same analysis within the Russell cohort sample. The results are similar. When an index fund votes in support of an item, the average daily return is -14 basis points if the item passes compared to +12 basis points if the item fails. There is no similar pattern in daily returns for active funds' votes.

In sum, when index funds vote in favor of agenda items that pass, the average daily return to the firm's stock is negative. Conversely, when index funds vote in favor of an item that fails, the average return is positive. These results are inconsistent with a world in which fund voting is irrelevant to firm strategy or firm value; they are consistent with a world in which index funds' voting behavior relative to active funds may be detrimental to firm value.

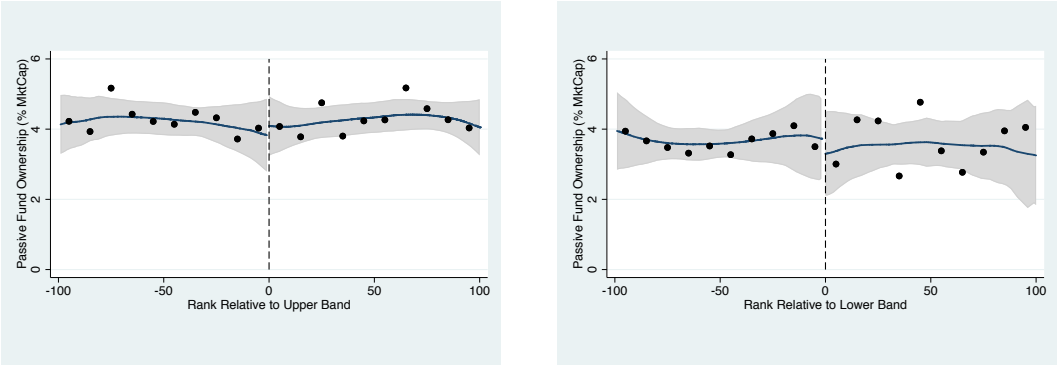


(a) No measurement error

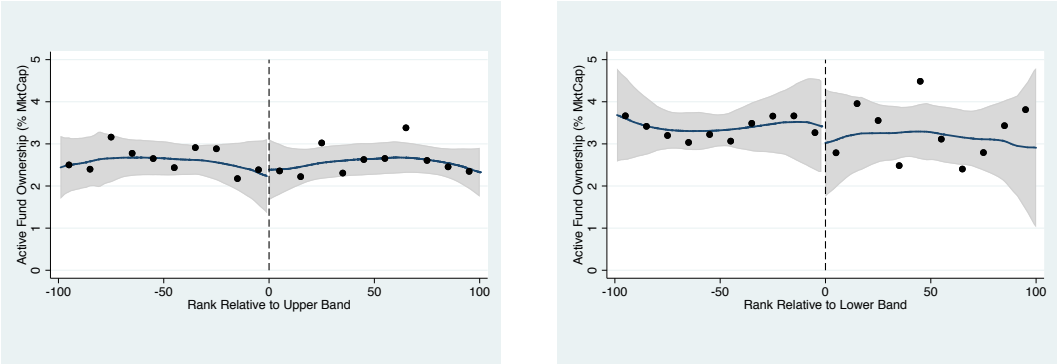


(b) With measurement error

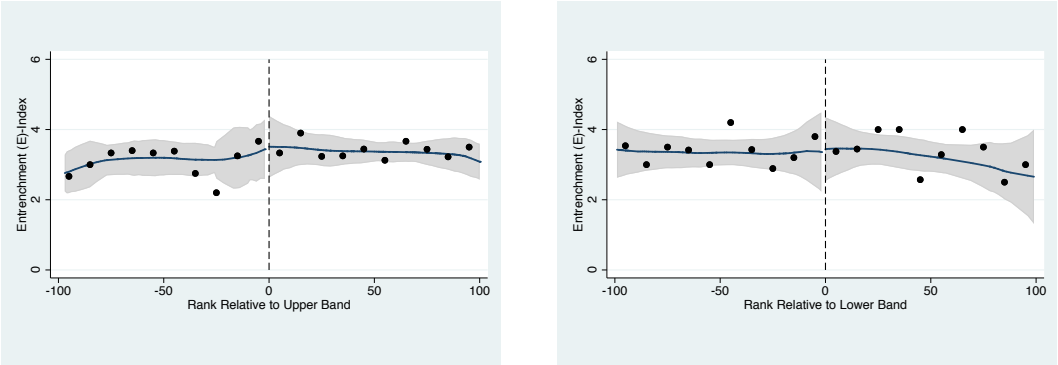
Figure A1. Identification when the Forcing Variable is Measured with Noise
The figure presents regression discontinuity plots, using simulated data, when the forcing variable is measured perfectly (A) and when the forcing variable is measured with noise (B). The true treatment effect in the simulated data is zero *i.e.* there is no discontinuity in the outcome variable across the threshold. Local polynomial regression lines are in blue. 99% confidence intervals are in grey.



(a) Pretreatment ownership by index funds



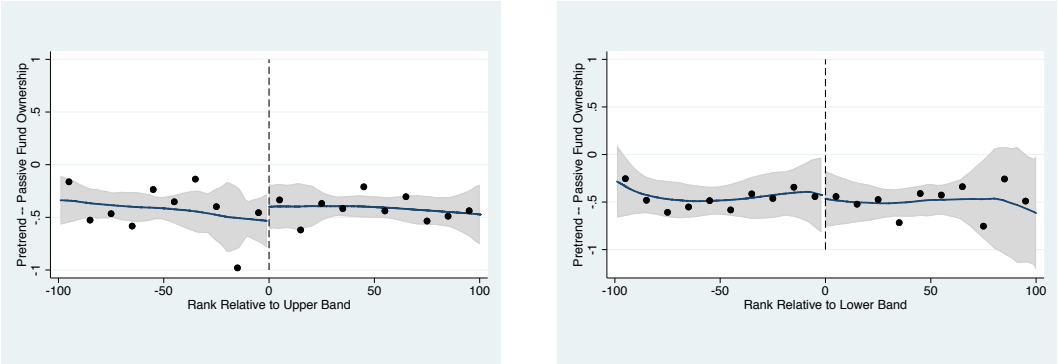
(b) Pretreatment ownership by active funds



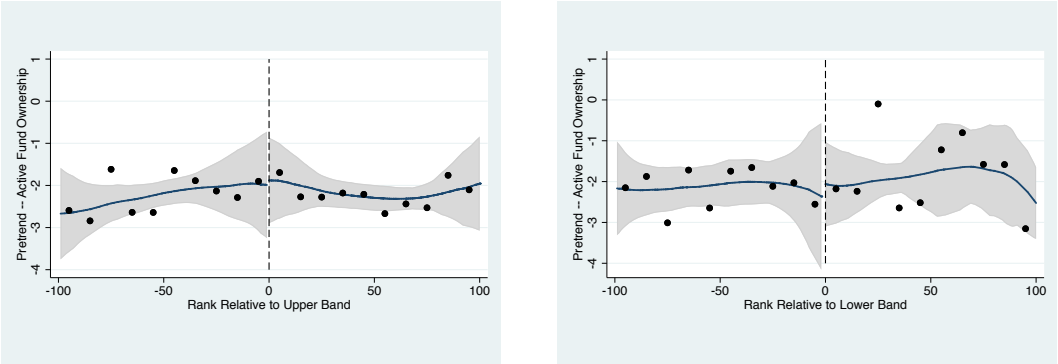
(c) Pretreatment governance (E-Index)

Figure A2. Balance Tests: Pretreatment Regression Discontinuity

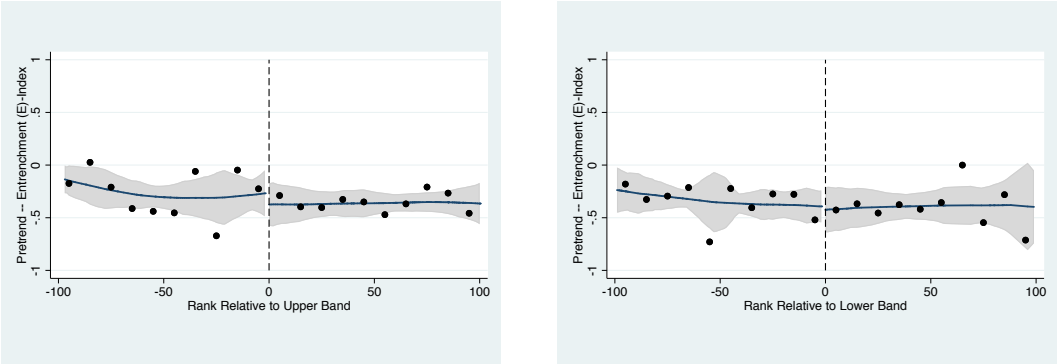
The figure presents regression discontinuity plots of (a) ownership by index funds, (b) ownership by active funds, and (c) the Entrenchment Index (E-Index) of Bebchuk Cohen and Ferrell across the upper (left side) and lower (right side) bands as of the last pretreatment year, for firms in the Russell switching cohorts. Local polynomial regression lines are in blue. 99% confidence intervals are in grey.



(a) Pretreatment trends, ownership by index funds



(b) Pretreatment trends, ownership by active funds



(c) Pretreatment trends, governance (E-Index)

Figure A3. Regression Discontinuity Comparison: Pretrends

The figure presents regression discontinuity plots of pretreatment trends for (a) ownership by index funds, (b) ownership by active funds, and (c) the Entrenchment Index (E-Index) of Bebchuk Cohen and Ferrell across the upper (left side) and lower (right side) bands, for firms in the Russell switching cohorts. Local polynomial regression lines are in blue. 99% confidence intervals are in grey.

Table A1
Observation Equation

The table presents the estimated observation equation (the Heckman first stage, equation (3) in the main text) that a given fund is observed holding a given firm. The sample for this estimate is the panel of all firm-years in the Russell sample, interacted with all mutual funds that held at least one firm in the sample. The dependent variable $Observed_{ijt}$ is a dummy that equals 1 if fund i held a position in firm j in year t. Robust standard errors clustered by fund are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)
	$Observed_{ijt}$
$IndexFund_i$	0.696***
	(0.057)
$R2000 \rightarrow R1000_j \times$	0.071***
$PostAssignment_t$	(0.021)
$R1000 \rightarrow R2000_j \times$	-0.224***
$PostAssignment_t$	(0.025)
$R2000 \rightarrow R1000_j \times$	-0.055*
$PostAssignment_t \times IndexFund_i$	(0.032)
$R1000 \rightarrow R2000_j \times$	0.067***
$PostAssignment_t \times IndexFund_i$	(0.024)
Model	Probit
Observations	6,586,669
Pseudo R^2	0.054
Firm × Cohort FE	Yes
Year FE	Yes

Table A2
Balance Tests: Comparison of Pretreatment Means

The table presents comparisons of pretreatment means between switchers (firms that switched indexes) versus stayers (firms in the same cohort and near the same band that did not switch indexes). Panel A compares treated and control firm's total market capitalization and measures of fund ownership. Panel B compares the Entrenchment (E)-Index of Bebchuk, Cohen & Ferrell and its subcomponents. Robust standard errors clustered by firm are shown in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Market Cap and Fund Ownership

	(1) $log(MarketCap)$	(2) $PassiveOwn_{jt}^{R2000}$	(3) $PassiveOwn_{jt}^{R1000}$	(4) $PassiveOwn_{jt}^{S\&P500}$	(5) $PassiveOwn_{jt}$	(6) $ActiveOwn_{jt}$	(7) $TotalFundOwn_{jt}$
$R1000 \rightarrow R2000_j \times$	0.03	-0.02	0.01	0.00	0.02	-1.23	-1.22
$PostAssignment_t$	(0.04)	(0.08)	(0.02)	(0.03)	(0.43)	(1.57)	(1.66)
$R2000 \rightarrow R1000_j \times$	-0.01	-0.04	0.01	-0.00	-0.12	1.19	1.07
$PostAssignment_t$	(0.01)	(0.12)	(0.01)	(0.01)	(0.35)	(0.85)	(0.98)
Observations	732	732	732	732	732	732	732
Adjusted R^2	0.939	0.677	0.782	0.085	0.249	0.021	0.065
Window	100	100	100	100	100	100	100
Cohorts	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Control Fn Degree	2	2	2	2	2	2	
Cohort × Band FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Firm Governance

	(1) E-Index	(2) S/H Chg Bylaws	(3) Supmaj. BusComb	(4) Supmaj. Charter	(5) Poison Pill	(6) Conf. Vote	(7) Cumul. Vote
$R1000 \rightarrow R2000_j \times$	0.34	0.05	0.04	0.14	-0.01	0.15	0.02
$PostAssignment_t$	(0.35)	(0.07)	(0.14)	(0.15)	(0.11)	(0.11)	(0.11)
$R2000 \rightarrow R1000_j \times$	-0.29	-0.07	0.02	-0.18	0.15	-0.02	-0.07
$PostAssignment_t$	(0.38)	(0.10)	(0.14)	(0.17)	(0.14)	(0.08)	(0.13)
Observations	365	365	365	365	365	365	365
Adjusted R^2	-0.002	-0.022	0.011	-0.028	0.016	0.016	-0.033
Window	100	100	100	100	100	100	100
Cohorts	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Control Fn Degree	2	2	2	2	2	2	2
Cohort × Band FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A3
Comparison of Pretrends

The table presents comparisons of pretrends between switchers (firms that switched indexes) versus stayers (firms in the same cohort and near the same band that did not switch indexes). Panel A compares total market capitalization and measures of fund ownership. Panel B compares the Entrenchment (E)-Index of Bebchuk, Cohen & Ferrell and its subcomponents. Robust standard errors clustered by firm are shown in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Pretrends in Market Cap and Fund Ownership

	(1) $log(MarketCap)$	(2) $PassiveOwn_{jt}^{R2000}$	(3) $PassiveOwn_{jt}^{R1000}$	(4) $PassiveOwn_{jt}^{S\&P500}$	(5) $PassiveOwn_{jt}$	(6) $ActiveOwn_{jt}$	(7) $TotalFundOwn_{jt}$
$R1000 \to R2000_j \times$	-0.01	-0.02	0.01	-0.01	0.00	-0.38	-0.38
$PostAssignment_t$	(0.05)	(0.06)	(0.01)	(0.02)	(0.13)	(0.51)	(0.53)
$R2000 \to R1000_j \times$	-0.07	0.00	0.00	0.00	-0.10	0.26	0.17
$PostAssignment_t$	(0.05)	(0.06)	(0.01)	(0.00)	(0.14)	(0.41)	(0.46)
Observations	731	732	732	732	732	732	732
Adjusted R-squared	0.517	0.219	0.216	0.121	0.137	0.029	0.023
Window	100	100	100	100	100	100	100
Cohorts	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Control Fn Degree	2	2	2	2	2	2	
Cohort × Band FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Pretrends in Firm Governance

	(1) E-Index	(2) S/H Chg Bylaws	(3) Supmaj. BusComb	(4) Supmaj. Charter	(5) Poison Pill	(6) Conf. Vote	(7) Cumul. Vote
$R1000 \to R2000_j \times$	0.03	-0.03	-0.02	0.01	0.04	0.01	0.03
$PostAssignment_t$	(0.12)	(0.04)	(0.04)	(0.01)	(0.08)	(0.03)	(0.02)
$R2000 \to R1000_j \times$	-0.10	-0.01	-0.01	-0.02	0.03	0.02	-0.00
$PostAssignment_t$	(0.12)	(0.01)	(0.05)	(0.02)	(0.03)	(0.03)	(0.00)
Observations	365	365	365	365	365	365	365
Adjusted R^2	0.149	0.025	0.177	0.009	0.162	-0.005	0.082
Window	100	100	100	100	100	100	100
Cohorts	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Control Fn Degree	2	2	2	2	2	2	2
Cohort × Band FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A4
Varying Window Size

The table presents estimates of the effects of Russell index switches on investment fund ownership when we vary the window of selection around the bands each year. Panel A shows the results when we narrow the window from 100 ranks to 50 ranks around each band. Panel B shows the results when we widen the window from 100 to 150 ranks around each band. The sample consists of stocks that were "potential switchers" near the yearly Russell upper and lower bands from 2007 to 2015, three years before and after index assignment for each firm in each cohort. Robust standard errors clustered by firm are shown in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Window = 50 Ranks

	(1) $PassiveOwn_{jt}^{R2000}$	(2) $PassiveOwn_{jt}^{R1000}$	(3) $PassiveOwn_{jt}^{S\&P500}$	(4) $PassiveOwn_{jt}$	(5) $ActiveOwn_{jt}$	(6) $TotalFundOwn_{jt}$
$R1000 \rightarrow R2000_j \times$	1.25***	-0.16***	-0.03**	0.69**	0.05	0.74
$PostAssignment_t$	(0.13)	(0.02)	(0.01)	(0.29)	(0.49)	(0.63)
$R2000 \rightarrow R1000_j \times$	-1.27***	0.16***	0.02***	-0.66***	0.26	-0.40
$PostAssignment_t$	(0.12)	(0.02)	(0.01)	(0.21)	(0.41)	(0.51)
Observations	2,148	2,148	2,148	2,148	2,148	2,148
Adjusted R^2	0.390	0.415	0.303	0.643	0.542	0.533
Window	50	50	50	50	50	50
Years	2004-2017	2004-2017	2004-2017	2004-2017	2004-2017	2004-2017
Cohorts	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Window = 150 Ranks

	(1) $PassiveOwn_{jt}^{R2000}$	(2) $PassiveOwn_{jt}^{R1000}$	(3) $PassiveOwn_{jt}^{S\&P500}$	(4) $PassiveOwn_{jt}$	(5) $ActiveOwn_{jt}$	(6) $TotalFundOwn_{jt}$
$R1000 \rightarrow R2000_j \times$	1.41***	-0.18***	-0.02*	0.93***	-0.06	0.88*
$PostAssignment_t$	(0.09)	(0.01)	(0.01)	(0.19)	(0.35)	(0.45)
$R2000 \rightarrow R1000_j \times$	-1.32***	0.16***	0.02***	-0.99***	-0.26	-1.24***
$PostAssignment_t$	(0.07)	(0.02)	(0.00)	(0.13)	(0.27)	(0.33)
Observations	6,972	6,972	6,972	6,972	6,972	6,972
Adjusted R^2	0.522	0.516	0.342	0.690	0.578	0.595
Window	150	150	150	150	150	150
Years	2004-2017	2004-2017	2004-2017	2004-2017	2004-2017	2004-2017
Cohorts	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A5
Fund Voting Comparisons Within Firm-Years and Within Agenda Items

The table presents comparisons of fund voting on contentious items when we add more granular fixed effects to the model. Columns 1 and 4 reproduce the estimates from the main paper Table IV. Columns 2 and 5 show estimates using firm-by-year fixed effects, so that the estimate compares how funds voted within each firm-year. Columns 3 and 6 show estimates using fixed effects for every agenda item, so that the estimate compares how funds voted on the *same* agenda item. In Columns 1-3 the sample consists all firms in the data; in Columns 4-6 the sample consists of firms that were potential switchers near the yearly Russell bands from 2007-2015. We only compare votes on contentious items i.e. those on which ISS and firm management were opposed. Robust standard errors clustered by fund are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) $VotedwithMgmt$	(2) $VotedwithMgmt$	(3) $VotedwithMgmt$	(4) $VotedwithMgmt$	(5) $VotedwithMgmt$	(6) $VotedwithMgmt$
$IndexFund_i$	0.125***	0.124***	0.124***	0.150***	0.152***	0.152***
	(0.025)	(0.024)	(0.024)	(0.030)	(0.030)	(0.030)
Sample Firms	All	All	All	Russell	Russell	Russell
Observations	2,187,598	2,187,433	2,186,506	189,319	189,317	189,315
Adjusted R^2	0.074	0.118	0.158	0.075	0.116	0.152
Firm × Cohort FE	No	No	No	Yes	No	No
Firm FE	Yes	No	No	No	No	No
Year FE	Yes	No	No	Yes	No	No
Firm × Year FE	No	Yes	No	No	Yes	No
Agenda Item FE	No	No	Yes	No	No	Yes

Table A6
Results at the Fund-Family Level

The table presents comparisons of fund voting on contentious items between fund families. $FractionAUMPassive_{it}$ is the fraction of fund i's family's total AUM that was passively managed in year t. Column 1 shows estimates for all firms in the sample. Columns 2 and 3 show estimates for firms that were potential switchers near the yearly Russell bands from 2007-2015. The sample consists of votes on only contentious items i.e. those on which ISS and firm management were opposed. Robust standard errors clustered by fund *family* are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) $VotedwithMgmt$	(2) $VotedwithMgmt$	(3) $VotedwithMgmt$	(4) $VotedwithMgmt$	(5) $VotedwithMgmt$	(6) $VotedwithMgmt$
$FractionAUMPassive_{it}$	0.324***	0.324***	0.323***	0.340***	0.341***	0.341***
	(0.073)	(0.073)	(0.073)	(0.027)	(0.027)	(0.027)
Observations	2,137,470	2,137,305	2,136,367	185,661	185,659	185,657
Adjusted R-squared	0.117	0.162	0.202	0.124	0.163	0.201
Firm × Cohort FE	No	No	No	Yes	No	No
Firm FE	Yes	No	No	No	No	No
Year FE	Yes	No	No	Yes	No	No
Firm × Year FE	No	Yes	No	No	Yes	No
Agenda Item FE	No	No	Yes	No	No	Yes

Table A7
Abstentions

The table presents comparisons of fund abstention on contentious items between index funds versus active funds. Columns 1 and 2 show estimates for all firms in the sample. Columns 3 and 4 show estimates for firms that were potential switchers near the yearly Russell bands from 2007-2015. The sample consists of votes on only contentious items i.e. those on which ISS and firm management were opposed. Robust standard errors clustered by fund are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels respectively.

	(1)	(2)	(3)	(4)
	Abstained	*Abstained*	*Abstained*	*Abstained*
$IndexFund_i$	-0.059***	-0.058***	-0.079***	-0.081***
	(0.013)	(0.013)	(0.018)	(0.018)
Sample Firms	All	All	Russell	Russell
Observations	2,187,598	2,187,433	189,319	189,317
Adjusted R^2	0.215	0.304	0.194	0.259
Firm x Cohort FE	No	No	Yes	Yes
Firm FE	Yes	No	No	No
Year FE	Yes	Yes	Yes	Yes
Firm × Year FE	No	Yes	No	Yes

Table A8
Identification when the Forcing Variable is Measured with Error

The table presents comparisons of RDD and difference-in-differences estimates on simulated data in which there is no change in the outcome variable across the cutoff (true treatment effect = 0.0) and the forcing variable is measured with error. Robust standard errors clustered by firm are shown in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Outcome	(2) Outcome	(3) Outcome	(4) Outcome	(5) Outcome	(6) Outcome	(7) Outcome
$Treated_i$	29.058***	23.465***	31.130***	20.920***	28.965***	33.391***	
	(10.658)	(5.770)	(4.927)	(8.984)	(7.403)	(9.848)	
$Treated_i \times PostTreat_t$							-0.004
							(0.707)
Control Function	LPoly	LPoly	LPoly	Linear	Quadratic	Cubic	
Kernel	Triangular	Epachenikov	Rectangular	Rectangular	Rectangular	Rectangular	
Observations	200	200	200	200	200	200	1,200
Adjusted R^2	0.690	0.690	0.690	0.690	0.691	0.690	0.963
Firm FE	No	No	No	No	No	No	Yes

Table A9
Fund Votes and Announcement Returns

The table presents comparisons of the daily stock return minus the market return on the day that the agenda item was decided, conditional on how the fund voted on the item ($VotedYes_{ik}$), whether the item passed ($ItemPassed_k$), and the fund's type (active or index fund). Robust standard errors clustered by item are in parentheses. *, **, *** indicates statistical significance at the 10%, 5%, and 1% levels respectively.

	(1) $DailyRtn_{ik}$	(2) $DailyRtn_{ik}$
$VotedYes_{ik} \times IndexFund_i$	0.0004	0.0012
	(0.0006)	(0.0015)
$VotedYes_{ik} \times IndexFund_i \times ItemPassed_k$	-0.0004	-0.0014
	(0.0007)	(0.0016)
$VotedYes_{ik} \times ActiveFund_i$	-0.0003	0.0000
	(0.0006)	(0.0012)
$VotedYes_{ik} \times ActiveFund_i \times ItemPassed_k$	0.0003	-0.0002
	(0.0007)	(0.0012)
Sample Firms	All	Russell
Observations	22,148,249	2,514,263
Adjusted R^2	0.175	0.209
Firm FE	Yes	No
Firm × Cohort FE	No	Yes
Year FE	Yes	Yes