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Do Index Funds Monitor?

Finance Working Paper N° 638/2019
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Abstract

Passively managed index funds now hold over 30% of U.S. equity fund assets; this shift raises fundamental questions about monitoring and governance. We show that, relative to active funds, index funds are less effective monitors: (a) they are less likely to vote against firm management on contentious governance issues; (b) there is no evidence they engage effectively publicly or privately; and (c) they promote less board independence and worse pay-performance sensitivity at their portfolio companies. Overall, the rise of index funds decreases the alignment of incentives between beneficial owners and firm management and shifts control from investors to managers.

Keywords: Corporate Governance, Index Funds, Monitoring, Passive Investing, Voting

JEL Classifications: G12, G14

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I. Introduction

The separation of ownership and control generates agency conflicts between a firm's managers and its shareholders. This well-known problem has been studied since at least the time of Smith (1776).[1] Yet a recently occurring fundamental shift may potentially exacerbate this classic agency conflict; namely, index investing has changed the intermediary connecting investors to firms. Over the last 25 years public corporations have experienced a dramatic increase in ownership by passively managed index funds (see Figure 1), and index funds are now the largest shareholders of many U.S. corporations Bebchuk and Hirst (2019b). Although the increasingly large positions held by index funds should motivate them to monitor their portfolio firms Grossman and Hart (1980); Shleifer and Vishny (1986), these new intermediaries have different incentives than traditional active mutual funds (Bebchuk, Cohen, & Hirst, 2017; Edmans, Levit, & Reilly, 2018). As a consequence, the rise of index investing raises fundamental questions about monitoring and governance. Specifically, to what extent do index funds monitor their portfolio companies? Does ownership by index funds affect corporate governance?

We answer these questions by studying the relation between index fund ownership, monitoring, and corporate governance. We find that index funds monitor less than the active funds they are replacing. Moreover, higher index fund ownership decreases the alignment of incentives between owners and managers; namely, increased index fund ownership is associated with less board independence and worse pay-performance sensitivity. Overall, we find no evidence that index funds improve the governance of their portfolio firms, contrary

[1] Smith (1776) 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."

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to recent claims that index funds are good monitors who improve corporate governance.

Two opposing forces could affect the monitoring behavior of index funds. On the one hand, classic agency theory (e.g. Grossman and Hart (1980)) posits the increasingly large positions held by index funds results in strong incentives to monitor. 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 their voice (Bhide, 1993; Fisch, Hamdani, & Davidoff Solomon, 2019; Rock & Kahan, 2019). 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]

On the other hand, the business model of passively managed index funds suggests that these funds have weaker incentives to monitor compared to traditional active funds, 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. For example, in our sample, the average index fund holds 308 stocks, whereas the average active fund holds 98 stocks. Bebchuk and Hirst (2019a) point out that the top three index fund families have on average only 21 investment stewardship personnel to cover 17,849 firms in their portfolio. Moreover, index 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). To date, the net effect of index investing on firm governance is unclear. Our results provide novel evidence that index funds do not exert the same monitoring effort that active funds do.

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

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The main challenge in studying the effects of index fund investment is that holdings by active and passive funds are endogenous. First, firm characteristics, such as size and liquidity, may jointly affect ownership and governance. Second, different firm policies may attract different types of investors.[3] Thus, there is the potential for endogeneity due to both omitted variables and reverse causality.[4] We address these issues in two ways. First, because we examine voting at the fund-firm-agenda item level, we can include firm-by-year fixed effects. Thus, we are able to sweep out time-varying firm effects. Second, to examine the firm-level consequences of index funds ownership, we develop a new research design that uses Russell index reconstitutions in the post-2006 period to generate exogenous variation in fund holdings. Specifically, we use a difference-in-differences specification with a fixed effects structure that absorbs both unobserved firm heterogeneity and time-varying aggregate shocks. Importantly, we develop a research design that does not suffer from bias due to noise in the forcing variable (Pei & Shen, 2017), which is an issue in prior studies that use Russell index reconstitutions (see Wei and Young (2020)). Across all specifications, our estimates remain highly stable, suggesting our results are not sensitive to influence by omitted confounders (Oster, 2019).

We start by examining the voting behavior of funds across all agenda items. In a recent survey, McCahery, Sautner, and Starks (2016) document that voting against management is a primary channel through which mutual funds can use their voice to change corporate policies. On consensus votes (i.e., when there is agreement between firm management and ISS, a third-party proxy advisor) we find that index funds and active funds vote identically.

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

[4]Selection bias also could be an issue: if a fund chooses not to hold a firm, we do not observe how that fund would have voted. We address this concern using a Heckman (1979) correction model. Section C of the Online Appendix presents these results, and all of our conclusions are unchanged.

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By contrast, on contentious votes (i.e., when ISS and firm management disagree), index funds are 10.1 percentage points more likely than active funds to vote with management. The fact that index funds are more likely to vote with firm management is consistent with the predictions in Bebchuk et al. (2017), who argue that index funds lack the incentives and resources to actively monitor their portfolio firms. Our findings provide novel evidence confirming this prediction. Indeed, we also find that index funds with low expense ratios are more likely to vote with management than index funds with high expense ratios. We also examine voting at the fund-family level, instead of the fund level, and find that fund families with a higher fraction of their assets in index funds are more likely to vote with firm management. All of the results point to the same conclusion: index funds with lower resources tend to invest less in costly monitoring (Lewellen & Lewellen, 2018); as a result, these funds are more likely to cede power to a firm's management.

Arguably, not all votes are equally important. To shed further light on index funds' monitoring behavior, we provide the first evidence on index fund voting relative to active funds across a variety of governance issues including director elections, executive compensation, corporate disclosure, and managerial entrenchment. We find that index funds are more likely than active funds to vote with firm management on all of these agenda items. We find the largest difference between active and passive funds on compensation votes; in particular, passive funds are 11.3 percentage points more likely to vote with management. Prior studies have shown that a vote against management significantly affects compensation policies, since firms with substantial (actual or anticipated) voting dissent respond by removing controversial compensation practices (Ertimur, Ferri, & Muslu, 2010; Larcker, McCall, & Ormazabal, 2015). Moreover, the results on managerial entrenchment are particularly relevant, since some of the largest index funds publicly state that they are against certain

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governance practices, such as poison pills and golden parachutes.[5] Yet, when it comes to voting on these issues, we find that index funds are actually less likely than active funds to vote against managerial entrenchment.

Overall, our voting results suggest that index funds are weaker monitors than the active funds they replace. However, index funds could possibly monitor through other channels. For example, index funds could either publicly or privately engage with managers and then vote in support of the management proposals they negotiated beforehand.[6] We investigate index funds' private engagement in two ways. First, we split agenda items into shareholder proposals and management proposals (Gillan & Starks, 2000). Behind-the-scenes engagement could explain index funds' tendency to support management proposals, since they could be supporting management proposals that they negotiated beforehand. Yet the same reasoning does not apply to shareholder proposals. In other words, if index funds affect firm governance through private engagement, their tendency to vote with management should be mostly or entirely on management proposals and not on shareholder proposals. Yet we find that relative to active funds, index funds are significantly more likely to vote with firm management on both management and shareholder proposals.

Second, if index funds engage with the managers of their portfolio firms to change governance, then we would expect to see a change in the number and/or type of agenda items proposed at the annual meeting after an exogenous increase in index funds' holdings. For example, we would expect to see fewer contentious management proposals. Yet we do not

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

[6]For example, in its Investment Stewardship annual report, ? (?) writes that over a 12-month period, the Vanguard Investment Stewardship team "engaged with nearly 900 portfolio companies around the globe." Similarly, in its Investment Stewardship report, ? (?) writes that "State Street Global Advisors held 676 comprehensive engagements with 610 companies." Furthermore, McCahery et al. (2016) find evidence of behind-the-scenes intervention by institutional investors (however, they do not distinguish between active funds and index funds).

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see a change in the number of either management or shareholder proposals, or in the fraction of contentious proposals. Moreover, after an exogenous increase in index funds' ownership we find a reduction (by 17%) of the likelihood that a shareholder proposal is approved. These results are not consistent with the notion that index funds privately engage with the management of their portfolio firms: while private engagement is not observable, we find no evidence that engagement efforts of index funds (that they emphasize in their reports) lead to observable improvements.

We also examine publicly observable evidence of mutual funds' intent to engage. Shareholders are required to disclose a holding above 5% of the firm's market capitalization via Schedule 13D, which allows the fund to engage with the firm, or Schedule 13G, which does not. We find that fund families with more AUM in index funds are much less likely to file Schedule 13D. Moreover, for a subset of 13D filings that we can match to individual funds, we do not find a single instance of an index fund filing a Schedule 13D. These results are consistent with those of Morley (2019), who argues that index funds could incur legal and compliance issues if they publicly engage with their portfolio companies.[7]

Our results so far consistently show that index funds are weaker monitors than active funds. Next, we examine the consequences of this reduced monitoring on corporate governance. We start by examining the alignment of incentives between managers and shareholders via compensation and the threat of the CEO's dismissal (Jensen & Murphy, 1990). If index funds are active monitors of their portfolio firms, an exogenous increase in index fund ownership should increase incentive alignment between managers and shareholders through

[7]Bebchuk and Hirst (2019a) find that the "Big Three" fund families filed only nine Schedule 13Ds between 2007 to 2018. However, Bebchuk and Hirst (2019a) do not specifically examine whether these filings were related to holdings by index funds or active funds within the "Big Three," nor do they examine funds outside these three fund families. We expand on their results by examining the universe of all funds and by directly comparing the behavior of index funds and active funds.

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higher pay-performance sensitivity, higher CEO turnover, and reduced use of insurance mechanisms like golden parachutes. We find the opposite. The sensitivity of top executives' pay to the firm's stock return declines by 43 log points after an exogenous increase in index funds ownership. This effect is explained by an increase in total compensation driven by higher cash compensation (salaries and bonuses), which replaces equity-linked stock grants and options. Importantly, the effect is symmetric: after an exogenous decrease in index fund ownership, the sensitivity of the top executives' pay to performance increases, with a reduction in total compensation driven by less cash compensation but more equity-linked grants and options. Moreover, we find that an increase in index funds ownership is not followed by changes in CEO turnover and golden parachutes. Combined, these results are consistent with index funds worsening the incentive alignment between managers and shareholders.

We also assess the impact of index fund ownership on other firm-level governance measures that have been examined in prior studies, including (a) the fraction of independent directors on the board, (b) the E-index of Bebchuk et al. (2008), (c) the presence of poison pills or supermajority voting rights, (d) shareholder restrictions to call special meetings or to act by written consent, and (e) dual class share structures. In contrast to prior studies, we find that exogenous increases in index fund holdings are followed by zero or negative effects on all of these measures of governance. Importantly, we show that these nonresults are not due to limited statistical power; there is simply no effect.

Overall, our results provide uniform evidence that index funds do not act to improve corporate governance through their voice (vote or engagement). We also show that, unsurprisingly, index funds do not use the exit channel to improve governance (see Section B of the Online Appendix). Further, our evidence consistently shows that corporate governance does not improve and actually deteriorates along several dimensions when index funds re-

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place active funds.[8] Taken together our findings all point to the same conclusion: index funds have limited incentives and resources to invest in costly monitoring. It follows that they monitor less than active funds do. Across a wide variety of tests and specifications, the data uniformly indicate that index funds cede power to firm management. As a result, when index fund ownership increases, corporate governance worsens.

II. Literature Review

Our paper relates to the literature on agency conflicts and monitoring incentives arising from dispersed ownership (Berle & Means, 1932; Jensen & Meckling, 1976; Demsetz, 1983; Shleifer & Vishny, 1986; Burkart, Gromb, & Panunzi, 1997).[9] Given the dramatic increase in ownership by passively managed index funds, and since index funds are now the largest blockholders of many U.S. corporations (Bebchuk & Hirst, 2019b), studying their monitoring behavior is of fundamental importance (Edmans, 2014). Hence, our paper contributes to several recent studies in Finance and Law that examine the relation between passively managed index funds and corporate governance. A number of papers argue that index funds are active monitors of their portfolio firms (e.g. Boone and White (2015); Appel et al. (2016); Crane et al. (2016); Appel, Gormley, and Keim (2019b); Fisch et al. (2019); Rock and Kahan (2019)). However, some papers argue that index funds are lacking in their incentives to monitor (e.g. Bebchuk et al. (2017); Lund (2017); Bebchuk and Hirst (2019a, 2019b)). Moreover, Schmidt and Fahlenbrach (2017), Brav, Jiang, Li, and Pinnington (2019) and Iliev, Kalodimos, and

[8]For the subset of our results that relies on the index switching experiment, this interpretation requires the assumption that index switching affects governance only through its effects on fund ownership, similar to other papers in the Russell literature.

[9]A sizable literature tackles the monitoring behavior of active investors (e.g. Admati, Pfleiderer, and Zechner (1994); DeMarzo and Urošević (2006); Gillan and Starks (2007); Back, Collin-Dufresne, Fos, Li, and Ljungqvist (2018)).

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Lowry (2018) document evidence that accords with our findings that index funds do not monitor to the same extent that active funds do. Overall, the evidence about the impact of index funds on monitoring and governance is mixed.

In an early study, Mullins (2014) uses the Russell 1000/2000 setting in a regression discontinuity design (RDD) to examine the effect of institutional investor ownership on corporate governance. He finds that firms included in the Russell 1000 (which have more active ownership) have better monitoring and incentive alignment between CEO and shareholders. Along the same lines, but using Russell switching as an instrumental variable for institutional investors ownership, Schmidt and Fahlenbrach (2017) find that an increase in passive ownership leads to worse governance measured as board independence and quality of M&As. Similar to these studies, we examine pay-performance sensitivity (Mullins, 2014), board independence and number of shareholder proposals (Schmidt & Fahlenbrach, 2017). However, we contribute to these studies in several dimensions. First, we provide novel results about the voting and engagement behavior of index funds. Second, we examine a broader and more comprehensive set of governance measures. Third, we develop a research design that is out of sample with respect to these studies (i.e., post-2006, a period when index funds ownership is much higher) and addresses the endogeneity problems discussed in Section III.

A number of other papers use the Russell 1000/2000 setting but find opposite results in terms of governance implications. In these studies, an increase in index funds ownership leads to higher dividends (Crane et al., 2016), greater firm transparency (Boone & White, 2015), better governance (Appel et al., 2016), more successful activist campaigns (Appel et al., 2019b). Hence, these four studies conclude that passive investors are active monitors. Similar to Appel et al. (2016), we examine board independence, poison pills, shareholders' ability to call special meetings, and dual class shares. Yet, we differ from the above studies

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in our research design (which solves the methodological issues highlighted by Wei and Young (2020)), scope of the analysis, and conclusions. Across a large variety of tests, our findings show that—compared to active funds—index funds are weaker monitors of their portfolio firms and that they do not improve corporate governance.

Importantly, our paper differs from the existing literature in that we specifically examine the mechanism connecting owners to managers; that is, we provide novel evidence on the voting and engagement behavior of index funds. While our comparison of the voting behavior of index funds to active funds is unique, several papers examine proxy voting by institutional investors in general. For example, Gillan and Starks (2000) find that shareholder proposals sponsored by institutions are more likely to be supported than shareholder proposals sponsored by individuals. Matvos and Ostrovsky (2010) study voting in corporate director elections, while Davis and Kim (2007) and Cvijanović, Dasgupta, and Zachariadis (2016) find that mutual fund families vote with firm management more when the fund family is also a manager of the company's pension plan. More recent studies examine the reliance of mutual fund families on proxy advisor recommendations and conclude that such recommendations have an influence over shareholders' voting (e.g. Ertimur, Ferri, and Oesch (2013); Iliev and Lowry (2014); Larcker et al. (2015); N. Malenko and Shen (2016)). Furthermore, Brav et al. (2019) conduct a detailed analysis of shareholders voting during proxy fights, and find that active funds are significantly more pro-dissident than index funds. Proxy fights are a subcategory of our analysis on board of directors elections, and their results are consistent with our finding that index funds are more passive monitors. Yet, different from our paper, Brav et al. (2019) focus only on voting in proxy contests and do not use the Russell methodology to address the selection issues of fund voting nor do they examine the implications for corporate governance. Finally, three recent papers have examined proxy voting

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by large institutional investors: Bolton, Li, Ravina, and Rosenthal (2020), Bubb and Catan (2018), and Bebchuk and Hirst (2019a). The first two papers study institutional investor voting preferences and map shareholders' ideology into a left-right dimension (Bolton et al., 2020), or into three parties (Bubb & Catan, 2018). Similar to us, they find that the "Big Three" fund families (Vanguard, BlackRock, and State Street Global Advisors), which hold a large portion of their assets in passive portfolios, rarely disagree with firm management. Finally, Bebchuk and Hirst (2019a) provide descriptive statistics for voting on say-on-pay issues. They show that the "Big Three" fund families rarely vote *no* on executive compensation issues and conclude that index funds have weak incentive to be active monitors of their portfolio firms. Consistent with this view, a working paper by Iliev et al. (2018) shows that, relative to active funds, index funds conduct significantly less research about their portfolio firms.

Our paper complements and extends these contemporaneous studies. They provide a systematic classification of mutual funds' voting preferences into categories that share similar attributes (Bolton et al., 2020; Bubb & Catan, 2018), provide rich descriptive statistics that broaden the picture of our study (Bebchuk & Hirst, 2019a), and conclude that the "Big Three" are passive investors. Our analysis expands upon these studies in multiple dimensions: (1) we examine voting by all funds (instead of just the "Big Three"); (2) we examine voting on all governance issues (instead of examining one particular type of vote); (3) we develop an identification strategy to account for endogeneity concerns (while these papers only examine summary statistics or correlations); (4) we use active funds as a counterfactual at the agenda item level, which allows us to have a much more precise benchmark for how funds should vote if they are monitoring; and (5) we examine the consequences of index funds monitoring for corporate governance. To put it differently, our approach allows us to

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document whether index funds monitor more, less, or the same amount as active funds, and we show this affects corporate governance.

III. Data and Methodology

Our analyses use 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, for the years from 2004 to 2018.

A. Data

We use the ISS Fund Voting data to measure mutual funds' voting behavior. 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.[10] We link the ISS data by fund-year to the CRSP Mutual Fund Database, requiring that all sample funds be U.S. equity funds with at least $10 million in assets under management. We define a passively managed index fund as a fund with fund flag "D" in the CRSP Mutual Fund Database, and we classify all other mutual funds as active funds.[11] Table I reports summary statistics for all funds in our sample from 2004 to 2018. Relative to active funds, index funds are less numerous (approximately 4,000 fund-years vs. 25,000 fund-years) and more diversified (308 vs. 98 stocks) with more assets under management ($2.6B vs. $1.6B) and

[10]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 are not publicly reported anywhere. None of the Russell 2000 index funds is incorporated as trusts, so our voting results for Russell funds are not affected by the omission of these data. We thank Tara Bhandari and Amy Edwards at the Securities and Exchange Commission for helpful conversations on this topic.

[11]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 share (Petajisto, 2009), and all results are similar.

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lower expense ratios (0.47% vs. 1.14%).

We use Russell Index membership lists directly provided by Russell, and we match these data to firm and stock-level data from CRSP and Compustat.[12] We measure fund holdings by combining the CRSP mutual fund holdings database with the Thomson Reuters S12 database.[13] Finally, we add data on managerial compensation from Execucomp and on governance measures from ISS and BoardEx. Appendix A defines all variables.

B. Methodology

We use two distinct methodologies to examine index funds monitoring and the implications for corporate governance: (1) a high-dimensional fixed effect panel model and (2) a difference-in-differences regression around Russell index reconstitution.

B.1. High-dimensional fixed effect model

As previously discussed, one challenge in studying the voting behavior of investors is that time-varying firm characteristics may jointly affect ownership and governance. Because the ISS voting data contains observations at the firm-year-agenda item level, we can include firm-by-year fixed effects in our panel regressions. These fixed effects absorb time-varying firm level characteristics, allowing us to account for any possible omitted variables that could confound our estimates. In addition, reverse causality is also a possible concern. For example, better-governed firms may be more likely to attract index funds (making index funds appear to lead to better governance). Our high-dimensional fixed effect model compares the voting behavior of passive funds to active funds *within a given firm* at the same annual meeting. As

[12] We do not impose any 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 $t-1$.

[13] We use the most recent version of the S12 data as of mid-2020, which corrects errors that were present in some previous versions of the data (Ben-David, Franzoni, Moussawi, & Sedunov, 2020).

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a result, it accounts for preexisting differences in firm governance or other firm characteristics. In sum, the richness of the voting data allows to use a high-dimensional fixed effect panel model that uses the entire sample of firms from 2004 to 2018 in a manner that accounts for both reverse causality and possible omitted variables.

B.2. Difference-in-differences regression

While the high-dimensional fixed effect model allows us to address endogeneity concerns with minimal assumptions, it works because the voting data contains observations at the firm-year-agenda item level. Some of our analyses examine lower frequency measures of firm governance. For these analyses we need a different identification strategy. Accordingly, we develop a new research design that uses a difference-in-differences regression around Russell Index reconstitutions.

B.3. Post-2006 Russell index assignment

Each June 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.[14] Before June 2007, index assignment followed a simple rule: stocks ranked from 1 to 1000 were assigned to the Russell 1000, whereas stocks ranked from 1001 to 3000 were assigned to the Russell 2000. Starting in June 2007, Russell implemented a new assignment regime

[14]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 in May. 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 imputed Russell rankings recover 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.

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("banding"). After 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.[15] 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 2A plots index assignments in 2007, the first year of the banding regime. We observe that banding entirely eliminated the discontinuity across the rank-1000 cutoff; hence, an RDD across the cutoff is no longer feasible. However, Figure 2A also reveals 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. For example, 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; and (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 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.

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

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B.4. Difference-in-differences

The fact that the upper and lower bands create discontinuities in treatment status suggests a regression discontinuity design (RDD). However, a general issue with the Russell setting, both pre- and post-2006, makes a RDD undesirable: namely, Russell does not publicly release the *unadjusted* rankings ("May rankings") that determine index assignment.[16] Researchers who want to use an RDD are then faced with two options: (1) use the *float-adjusted* market capitalizations and rankings published by Russell in June ("June rankings") as an instrument for the true rankings or (2) impute the *unadjusted* rankings using the CRSP and Compustat data. Both options are subject to serious issues.

First, the use of June rankings to identify index assignment leads to preexisting differences between treatment and control firms, which can lead to biased estimates (Wei & Young, 2020). To avoid this problem, we use unadjusted market capitalization data from CRSP and Compustat (as suggested by Gloßner (2020)). We show that our sample does not exhibit preexisting differences in means or pretrends between treated and control firms. Specifically, we run formal balance tests on firm characteristics, such as index fund ownership, firm governance, and stock returns, all of which are measured over the last pretreatment year for each firm (see Figure 3). We observe no significant difference at the treatment cutoff (the upper or lower band respectively). Moreover, the overall levels for fund ownership, governance and lagged returns are similar across the bands.[17] We conclude that our treated and control groups are balanced *ex ante*, and our research design does not suffer from preexisting

[16]This concern holds for traditional RDDs as well as instrument variable approaches that use an RDD in the first-stage regression.

[17]Section D of the Online Appendix reports the results for additional firm characteristics.

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differences between treated and control firms.[18]

However, imputing the unadjusted rankings (May rankings) using the CRSP and Compustat data to determine index assignment introduces a second critical problem: the forcing variable is observed with noise. Pei and Shen (2017) show that noise in the forcing variable in an RDD causes the control function to be too flat, leading to biased or spurious estimates. Measurement error in the forcing variable is also a concern for fuzzy RDD, which adjusts for noncompliance with treatment assignment but does not correct for measurement error in the control function.[19]

Figure 4 illustrates the issue. We simulate a sample of 200 firms and rank them on a simulated forcing variable from -100 to +100, with a treatment threshold at rank = 0. We then simulate an outcome variable as a linear function of the ranking. There is no treatment effect (i.e., there is no discontinuity at rank = 0 or any other point). Figure 4A illustrates the true relation; a one unit increase in ranking leads to a one unit increase in the outcome variable, and there is no treatment effect. To examine the effect of the measurement error in the forcing variable, we add noise to each firm's ranking and re-sort them on the basis of the ranking measured with noise. We then estimate a standard regression discontinuity design. Figure 4B shows the resultant estimate. Consistent with Pei and Shen (2017), the control functions on either side of the threshold have an attenuation bias (i.e., the slope is too flat), and as a result there is a large spurious treatment effect at the threshold.

Our difference-in-differences research design addresses this issue within the Russell setting. For each Russell index reconstitution year from 2007 to 2016, we select a *cohort* that

[18]In a panel difference-in-differences design like ours, the exclusion restriction is not pretreatment balance in levels but the parallel trends assumption. In Section D of the Online Appendix, we also compare pretrends between treated and control firms. The two groups also have indistinguishable pretrends in all cases.

[19]We thank Toni Whited for this insight. Pei and Shen (2017) also show that fuzzy RDD does not fix the measurement error in the forcing variable.

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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. For example, consider two stocks A and B that are identical *ex ante* and in the Russell 1000 index in the year prior to treatment. Both stocks experience negative returns in the year prior to treatment and fall in the rankings. Firm A's market capitalization falls by X% to just above the lower band, while Firm B's market capitalization falls by X% plus epsilon. As a result, stock A stays in the Russell 1000 (and serves as a control), whereas stock B switches to the Russell 2000 (and is treated). Thus, our identification strategy compares stocks that started in the same index and are similar in every dimension, including their lagged returns (see Figure 3), except for landing on different sides of the same Russell band. Figure 2(b) shows the treated and control stocks around both bands in the 2007 cohort.

We use a firm-year panel design that compares firm outcomes before and after treatment. For each stock in each cohort, we include firm-years from 3 years prior to the cohort year (pretreatment years -3, -2, -1) and 3 years after the cohort year (post-treatment years 0, 1, 2) in the sample. We measure the effects of switching indexes using the following difference-in-differences specification:

$$\begin{aligned} Y_{jct} = {} & \beta_1 \, I\{R1000 \rightarrow R2000_{jc}\} \times PostAssignment_{ct} \\ & + \beta_2 \, I\{R2000 \rightarrow R1000_{jc}\} \times PostAssignment_{ct} \\ & + \phi_{jc} + \lambda_t + \epsilon_{jct}, \end{aligned} \tag{1}$$

where ϕ_{jc} and λ_t are firm-by-cohort and year fixed effects, $PostAssignment$ is an indicator variable that equals one after index assignment, *R1000* $\rightarrow$ *R2000* equals one if a firm switched from the Russell 1000 to the Russell 2000, and *R2000* $\rightarrow$ *R1000* equals one if a firm switched

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from the Russell 2000 to the Russell 1000.[20]

This specification compares firm outcomes before treatment versus after treatment, with a fixed effect applied to each firm in each cohort. Because each firm had a *single* ranking within a given cohort, the fixed effects ϕ_{jc} absorb any correlation of the outcome variable with both the true ranking *and* the error in the proxy ranking. Thus, the specification estimates the effects of switching indexes, as would a perfectly measured RDD, but in a way that is not sensitive to the measurement error in the forcing variable because the fixed effect eliminates the need for a control function. Indeed, any control function would be subsumed by the firm-by-cohort fixed effects. A further advantage of the panel research design is that the firm-by-cohort fixed effects sweep out cross-sectional heterogeneity at the level of each individual firm. This includes any non-time-varying differences among firms in, for example, their market capitalization, return volatility, or lagged returns. As a result, our estimates have higher statistical power than corresponding RDD estimates.[21]

The difference-in-differences design recovers the effects of index switching, whatever they are. The further interpretation of any changes in firm behavior being caused by changes in fund ownership relies on the assumption that index assignment affects treated firms only through its effects on fund ownership. This only-through condition is a common assumption in papers that use instrumental variables designs based on Russell index assignment (Appel et al., 2016; Crane et al., 2016).[22]

[20]Importantly, this means that β_1 and β_2—the effects of switching from the R1000 to the R2000 and vice versa—are identified from *disjoint* groups of treated and control stocks.

[21]In Section E of the Online Appendix, we demonstrate this with both simulated and real-world data.

[22]For example, Appel et al. (2016) write "This assumption seems reasonable in our setting in that it is unclear why index inclusion would be directly related to governance and corporate performance after restricting the sample to stocks near the Russell 1000/2000 cutoff and after controlling for the factor that determines index inclusion . . . ." Similarly, as discussed in the paragraphs above, our analyses are conducted on a narrow sample of firms around the Russell 1000/2000 cutoff, and the fixed effects structure sweeps out *all* covariation with the forcing variable.

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In Table II, we report summary statistics for this Russell cohort sample. The Russell cohort sample consists of a subset of midsize firms, and the average Russell cohort firm has a market capitalization of $2.5 billion. Along other observable dimensions these firms are similar to the population of all U.S. public firms.

IV. Monitoring

Index funds have been increasingly replacing active funds in U.S. equities (Figure 1). In this section, we examine the monitoring implications of this shift. We start by verifying that Russell index assignment does change ownership by index funds and index funds are replacing active funds in our Russell sample. We then examine monitoring by index funds relative to active funds.

A. Effects of index switching on fund ownership

In column 1 of Table III, we present estimates of the effects of index switching on ownership by index funds that track the Russell 2000, using the Russell cohort difference-in-differences specification (Equation 1).[23] On average, stocks that move from the Russell 1000 to the Russell 2000 switch from being one of the smallest stocks in the Russell 1000 to one of the largest stocks in the Russell 2000. Because both indexes are market capitalization weighted, these stocks should experience an increase in ownership by index funds. We find that ownership by Russell 2000 index funds rises by an average 1.72% of market capitalization for stocks that switch to the Russell 2000 relative to similar stocks just above the

[23]The table separately examines fund holdings based on each fund's benchmark index: column 1 examines ownership by Russell 2000 funds only (31 funds); column 2 examines ownership by Russell 1000 funds only (32 funds); and column 3 examines ownership by all index funds combined.

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lower band that stay in the Russell 1000. Similarly, we find that ownership falls by 1.63% of market capitalization for stocks that switch to the Russell 1000 relative to similar stocks just below the upper band that stay in the Russell 2000. Importantly, the two coefficient estimates are similar in magnitude, even though they are estimated from two *disjoint* sets of stocks.

Figure 5 plots ownership by Russell 2000 index funds for the four groups of stocks (switchers vs. stayers near the upper band and switchers vs. stayers near the lower band) in event time, that is, the observation year minus the cohort year. The results clearly show that (a) switchers and stayers in both groups have the same pretreatment levels *and* trends and (b) switching to the Russell 2000 leads to higher index fund ownership and vice versa. In sum, the evidence shows that index switching is plausibly random among sample firms near the yearly Russell bands and is followed by symmetric shifts in ownership by index funds.

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 0.21% of market capitalization in the lower band treatment group and rising by 0.22% 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. Further, we continue to observe symmetrical treatment effects. Next, in column 3, we show the effect of index switching on holdings by all index funds together. The result is close to the net effect on holdings by Russell 1000 and 2000 index funds: an increase of 1.31% and a decrease of 1.20%, respectively.

In column 4, we examine the effects of index switching on ownership by active mutual

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funds. The changes in ownership by active funds are opposite in sign and similar in magnitude; on average there is a decrease of 2.21% and an increase of 1.6%, respectively. Thus, active funds sell to index funds when a stock switches to the Russell 2000, and vice versa. The fact that index funds are primarily replacing active funds is also clear in column 5, which shows that the change in holdings by all other investors (that is, other institutional investors and individual investors) is smaller in magnitude and not statistically significant in both directions.

Overall, the results are clear: stocks that are added to the Russell 2000 experience an increase in ownership by passive funds, and a similar decrease in ownership by active funds. According to theory (e.g. Corum, Malenko, and Malenko (2020)), the effect of passive ownership on corporate governance depends on whether or not passive funds replace active funds. If passive funds replace active funds as the owners of public equity, corporate governance may worsen since active investors have greater incentives to monitor. Accordingly, in the next section we examine the monitoring behavior of passive funds relative to active funds.

B. Univariate voting behavior

Because index funds are increasingly replacing active funds as the owners of U.S. equities, it is important to examine if index funds monitor their portfolio companies differently than active funds. Theory presumes two monitoring mechanisms, namely, voice and exit. However, exit is much less likely to be relevant for index funds.[24] To understand how index funds monitor their portfolio firms, we thus examine the voice channel.

We start by examining voting behavior for a variety of agenda items using our broad

[24]Index funds have less discretion to exit positions because their stated objective is to mimic their benchmark index. However, many index funds are not required to fully replicate their benchmark index; as such, they have some discretion to exit a position. In Section B of the Online Appendix, we examine the exit mechanism and confirm that index funds are significantly less likely than active funds to exit a position.

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sample of all companies in the Russell 3000 (essentially all public U.S. equities excluding microcaps) over the years 2004 to 2018. We first examine univariate patterns of voting outcomes between active and index funds. Row 1 of Table IV shows the distribution of fund votes across the entire set of agenda items (i.e., the full sample). Unconditionally, index funds vote *Yes* 89.8% of the time compared to 88.9% of the time for active funds. Many agenda items are largely procedural, such as voting to adjourn the meeting. Hence, we use the voting recommendations of the largest proxy advisor, Institutional Shareholder Services (ISS), to understand agenda items for which there is disagreement with firm management. Proxy advisors fulfill a key economic role in processing a substantial amount of information to help institutional investors make informed decisions (A. Malenko & Malenko, 2019). Hence, we use ISS recommendations as a way to identify contentious issues. Specifically, in the next four rows of Table IV we split agenda items into two categories: (1) consensus items: items for which firm management and ISS made the same recommendation (rows 2 and 3), and (2) contentious items: items for which firm management and ISS made opposing recommendations (rows 4 and 5).

Another challenge must be mitigated to examine the monitoring behavior of index funds. A priori, it is unclear how index funds should behave if they are good monitors. Put differently, it is not clear what the null hypothesis is; for example, how often should index funds vote against managers? Our empirical design addresses this difficult challenge by comparing the monitoring behavior of index funds to the monitoring behavior of active funds. Because index funds are increasingly replacing active funds as the owners of U.S. equities, they are a natural counterfactual. In other words, comparing the voting behavior of index funds to active funds is informative about the overall change in monitoring behavior.

We find that for items that firm management and ISS both approve, index funds vote

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Yes 94.9% of the time while active funds vote Yes 95.2% of the time. Similarly, for items that firm management and ISS both oppose, index funds vote No 82.7% of the time while active funds vote No 82.9% of the time. The rates at which active and index funds 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 that firm management supports but ISS opposes, index funds vote Yes 53.4% of the time compared to 43.9% for active funds. For items that firm management opposes but ISS supports, index funds vote No 51.3% of the time compared to 46.8% for active funds. Thus, in both cases index funds are significantly more likely to side with firm management. Summing across all contentious items and coding abstentions as "no" votes (consistent with Del Guercio, Seery, and Woidtke (2008)), index funds voted with firm management 54.3% of the time while active funds voted with firm management 47.3% of the time. Overall, these descriptive results suggest that on contentious issues, index funds consistently vote with management more often than active funds do. These results are particularly relevant since in a recent survey McCahery et al. (2016) document that voting against management is a primary channel through which mutual funds can use their voice and change corporate policies.

Interestingly, index funds are *less* likely than active funds to abstain on contentious items, especially items that were supported by firm management but opposed by ISS. If a fund wishes to maintain its relationship with firm management, voting "abstain" may be preferred to voting against management's recommendation (Bebchuk et al., 2017). Openly voting against firm management carries a higher cost because it may damage the relationship between the investor and firm management (Cvijanović et al., 2016). Since most items require a majority of all votes cast to approve a measure, abstention can have the same effect as

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voting against a proposal but be perceived as a "soft no" (Del Guercio et al., 2008). Hence, finding that active funds abstain more often than index funds on contentious items again suggests that index funds are weaker monitors than active funds.

C. Regression estimates on voting behavior

The voting statistics clearly show that on contentious votes index funds are significantly more likely to side with firm management. However, a limitation of the results presented so far is that fund holdings are potentially endogenous with unobservable firm characteristics as well as time-varying aggregate shocks. For example, differences in firm characteristics (e.g., governance, entrenchment, managerial quality) may drive fund voting behavior. To address this concern, we turn to a regression approach using our high-dimensional fixed effect model and the full sample of all funds and firms from 2004 to 2018. Table V shows the results.

In columns 1 and 2, we estimate the difference in voting between index funds and active funds on contentious agenda items. The dependent variable *VotedWithMgmt* equals one if a fund votes with management's recommendation, and zero otherwise.[25] The independent variable *IndexFund* equals one if the fund is an index fund, and zero if the fund is an active fund. The model includes firm and year fixed effects to account for time-invariant firm characteristics and time-varying aggregate trends. In column 1, we find that, compared to active funds, index funds are 10.1 percentage points more likely to side with firm management on contentious votes. This is a larger difference than the result in the univariate voting statistics (Table IV) and is due to the addition of firm fixed effects, so that we now compare voting by index funds to active funds within each firm.

Lewellen and Lewellen (2018) argue that a fund's expense ratio directly affects the fund's

[25]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."

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incentive to monitor. Thus, in column 2 of Table V, we add each fund's yearly expense ratio as an explanatory variable. We estimate the coefficient for the expense ratio separately for index and active funds because of the different incentives and expense ratios that the two types of funds have. We find that active funds' voting behavior does not vary significantly with their expense ratio. This could indicate that active fund expense ratios are always high enough to provide monitoring incentives. By contrast, index funds with higher expense ratios are *less* likely to side with firm management on contentious items. The coefficient of -0.286 means that an index fund with an expense ratio that is 25 basis points higher (about one standard deviation) is 7.2 percentage points less likely to side with firm management, which is more than half of the overall difference in voting between index and active funds. This result is strikingly consistent with the prediction of Bebchuk et al. (2017): The economics of index investing restrict the resources that a fund has to employ in monitoring, since index funds compete on providing a standardized product at the lowest price. In other words, when index funds have more resources to invest in costly monitoring, they behave more like active funds (i.e., they oppose firm management more on contentious issues).

A remaining concern with the analyses in columns 1 and 2 is that corporate policies may change *over time*, and these changes may be related to fund voting behavior. To address this concern, we exploit the richness of the voting data which contains multiple observations per firm-date. Specifically, we include firm-by-year fixed effects, which allow us to compare voting by active and index funds at the same annual meeting. Put differently, these specifications absorb even time-varying firm characteristics. As a result, they address any possibility of confounding variables that could bias our comparisons.[26] We find that the difference in voting between index funds and active funds is identical as is the relation with

[26] Our analyses compare the behavior of index funds *relative* to active funds. This approach helps mitigate the potential impact of confounding variables.

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fund expense ratios. These results suggest that firm and year fixed effects, as in columns 1 and 2, account for most or all of the unobserved heterogeneity that could bias our treatment effect. In other words, voting-relevant characteristics, such as firm governance, managerial quality, or shareholder engagement, vary widely between firms but vary little within firms over time.[27]

In most cases, funds belong to fund families, such as Fidelity or Vanguard, and voting might be decided (at least partly) at the fund-family level or at the investment advisor level (Bubb & Catan, 2018; Bolton et al., 2020). For example, some funds like Vanguard have a centralized voting authority, whereas others like BlackRock have a more decentralized approach. To understand whether fund-family considerations affect a fund's voting behavior, Figure 6 examines voting policy at the fund-family level.[28] In this analysis, our independent variable of interest is the fraction of a fund family's assets under management (AUM) that is invested in index funds. Once again, the conclusion is the same: funds in families that have more assets in index funds are more likely to vote with firm management. Figure 6 highlights results for the Big-Three fund families, which hold the majority of index fund assets. Consistent with prior studies, we find that the Big-Three often support firm management (Bubb & Catan, 2018; Bebchuk & Hirst, 2019b).[29] Different from these studies, our specification compares passive funds to the active funds they are replacing and our high-dimensional fixed effect specification addresses endogeneity concerns.

[27]We also note the possibility that our results are confounded by selection bias, since we only observe monitoring behavior for funds that chose to hold a stock. We address selection bias concerns by implementing a Heckman (1979) model. Section C of the Online Appendix shows the results. Even after correcting for selection bias, we find a statistically and economically significant difference in the voting behavior between active and index funds.

[28]Section A of the Online Appendix presents regression estimates using our high-dimensional fixed effect specification. The results confirm the findings in Figure 6.

[29]Consistent with this notion, in the prospectus for Vanguard Index Funds dated April 25, 2018, ? (?) states: "We will give substantial weight to the recommendations of the company's board, absent guidelines or other specific facts that would support a vote against management."

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In sum, across a wide range of specifications and samples, we consistently find the same result: index funds are more likely than active funds to vote with firm management on contentious agenda items. Moreover, index funds with higher expense ratios are less likely to vote with firm management; that is, they vote more like active funds do. The results suggest that index funds lack the incentives and resources to regularly monitor their portfolio firms, consistent with the predictions in Bebchuk et al. (2017). All together our evidence suggests that, for index funds (a) the benefit of monitoring is low, (b) the cost of monitoring is positive, and (c) disagreeing with a firm's management adds additional costs. Accordingly, unless the benefit for voting against management is incredibly large, index funds choose to vote in accordance with managerial preferences. In other words, owing to their incentives, index funds are more likely to cede power to firm management.

D. Voting on agenda items by category

We note that not all votes are equally important and sometimes votes may be procedural and nonbinding. Hence, we examine whether the gap in voting behavior between active and index funds varies across votes on different corporate governance policies. Accordingly, we examine the following categories of agenda items related to corporate governance where management and ISS disagree: (a) Board of Directors; (b) Compensation; (c) Disclosure; and (d) Entrenchment.

Table VI reports the results. In column 1, we find that index funds are 11 percentage points more likely to side with firm management on contentious items relating to the board of directors. A small subset of these items relates to formal proxy battles between the incumbent board and an activist shareholder. That is, our results in column 1 are consistent with those of Brav et al. (2019), who focus on fund voting in proxy battles, and again

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consistent with the idea that index funds cede power to firm management on contentious governance issues.

Next, the results in column 2 shows that compensation votes are particularly important. We find the largest difference between active and passive funds' voting on compensation votes (11.3 percentage points). Prior studies have shown that a vote against management significantly affects compensation policies, since firms with substantial (actual or anticipated) voting dissent respond by removing controversial compensation practices (Ertimur et al., 2010; Larcker et al., 2015). Finally, in columns 3 and 4, we find that the gap in fund voting between index and active funds is positive and of similar magnitude for items related to disclosure and managerial entrenchment. Taken together, these findings reinforce our more general results in Table V, and suggest that the rise of index investing has consequences for (at least) board structure, managerial compensation, disclosure, and managerial entrenchment.

E. Public engagement: Schedule 13D versus Schedule 13G

Our analysis so far provides consistent evidence that index funds, compared to active funds, do not use the voting mechanism to change firm policies. However, index funds could monitor through other channels, that is, index funds could engage with the managers of their portfolio firms either *publicly* or *privately* (Dimson, Karakaş, & Li, 2015; McCahery et al., 2016), and then vote in support of management proposals that they negotiated beforehand. In this subsection, we examine public engagement as measured by filings of form 13D. In the next subsection, we examine two different tests to investigate the *private* engagement hypothesis.

We study index funds' propensity to *publicly* engage with the management of their portfolio firms by examining index funds' propensity to file a Schedule 13D. The SEC requires

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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, etc. The short-form Schedule 13G, by contrast, requires that the shareholder has no such purpose. A blockholder who files Schedule 13G and then engages with firm management opens themselves up to SEC investigation or class action lawsuits ("Levie v. Sears Roebuck & Co., 676 F. Supp. 2d 680", 2009; Morley, 2019).

Table VII presents probit estimates of 13D filing behavior at the fund-family level. Because the Schedules 13D and G are filed 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. The dependent variable is the fraction of each family-year's filings that were under the "activist" Schedule 13D or the short-form and passive Schedule 13G. 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 zero for a fund family entirely populated by active funds, to one for a fund family entirely populated by index funds. Column 1 shows that fund families with a higher fraction of index fund AUM are significantly less likely to file Schedule 13D. The marginal effect (which corresponds to moving from 100% active to 100% index) is -24 percentage points to the likelihood of filing via 13D, which is more than 100% of the base rate probability. The same 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). Put differently, since overall the passive fraction of equity fund assets has gone from 1% to 33% in the last 20 years (see

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Figure 1), this corresponds to equity fund blockholders being 8% less likely to intervene with a given portfolio company (as measured by filing Form 13D).

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 4,330 disclosure filings to individual funds. For active mutual funds, 67 of 4,079 filings were under Schedule 13D. For index funds, 0 of 251 filings were under Schedule 13D. In other words, for the subsample of filings that were verifiably by index funds, not a single fund filed form 13D.

Morley (2019) supports these findings by arguing that index funds could incur legal, organizational, and technical risks if they publicly engage with their portfolio companies. Once again, our results are consistent with the idea that, owing to their incentives, index funds cede power to firm management. Moreover, our findings complement the evidence in Bebchuk and Hirst (2019a) that index funds do not meet with the majority of their portfolio firms and accord with a working paper by Iliev et al. (2018), who document that, relative to active funds, index funds conduct significantly less research about their portfolio firms.

F. Private engagement

Next, we study index funds' propensity to *privately* engage with the management of their portfolio firms. Private engagement could explain our voting results if index funds engage with managers of their portfolio firms behind the scenes and then vote in support of management proposals that they negotiated beforehand. To investigate the *private* engagement hypothesis, we first split our estimates of fund voting behavior between agenda items that were proposed by management and those that were proposed by shareholders. Proposals by

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management could be affected by private engagement, but proposals by shareholders should not. Thus, if index funds affect firm governance through private engagement, their tendency to vote with management should be mostly or entirely on management proposals and not on shareholder proposals.

In Table VIII, we split all contentious items in the sample by whether they were proposed by management or shareholders. We find that the pattern that index funds are more likely to side with firm management holds true, regardless of who proposed the agenda item. On management proposals opposed by ISS, index funds are 11.7 percentage points more likely to vote "Yes." Similarly, on shareholder proposals opposed by firm management, index funds are still 6.9 percentage points more likely to vote "No" (*i.e.*, in support of firm management). Table VIII presents another interesting result: index funds are significantly less likely than active funds to abstain on management proposals that are opposed by ISS. This is again consistent with the argument of Del Guercio et al. (2008) and Bebchuk et al. (2017) that it is costly for shareholders to oppose firm management.

As a second test of the *private* engagement hypothesis, we examine changes in the number and fraction of contentious management and shareholder proposals. We also examine the likelihood that a given management or shareholder proposal passes. The logic we follow is simple. If index funds engage with the managers of their portfolio firms behind the scenes in order to implement governance changes, we would expect to see fewer contentious proposals tabled by firm management, because behind-the-scenes engagement would lead management to propose positive governance policies that are more likely to win the support of proxy advisors. As a consequence, those proposals that were tabled would be more likely to pass. Similarly, we would expect fewer contentious shareholder proposals to be tabled, as behind-the-scenes engagement would reduce the conflict between management and shareholders, and

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those shareholder proposals that were tabled would be more likely to pass.

While our results so far have used our fixed effects panel model, for this analysis we use data at the firm-year level (so we cannot include firm-by-year fixed effects). Accordingly, we use the Russell firm-year sample and estimate the difference-in-differences specification as shown in Equation (1). The analysis uses changes in index membership to identify the monitoring behavior of index funds. Table VIII, panel B, presents the results. Columns 1 through 3 show that after exogenous changes in index fund holdings, the number and the fraction of contentious management proposals does not change, and management proposals are no more or less likely to pass. Similarly, columns 4 and 5 show that the number and fraction of contentious shareholder proposals does not change.

Interestingly, column 6 shows that after an exogenous increase in index fund holdings, shareholder proposals are *less* likely to pass. This finding is not consistent with behind-the-scenes engagement, but rather suggests that activist shareholders lose support after an exogenous increase in index funds ownership of a firm.[30] In fact, while management proposals almost always pass, which explains our results in column 3, shareholder proposals are unconditionally less likely to pass. Hence, the fact that we find a 17% reduction in the likelihood that a shareholder proposal passes following an increase in index fund ownership suggests that changes in index fund ownership could have material effects on voting outcomes.

In sum, we find no evidence that engagement efforts of index funds lead to a change in observable measures like the number and/or type of agenda items proposed at the annual meeting or the probability of filing a 13D. While our results do not say that index funds never engage with their portfolio firms, all of our tests are inconsistent with the idea that engagement is an effective monitoring mechanism used by index funds.

[30] This result broadens the analysis of Brav et al. (2019) (since their analysis of proxy contests is a subgroup of shareholder proposals) and also lends a more causal interpretation to their results.

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V. Effects on Firm Governance

Our analyses so far focus on the channels through which investors affect corporate behavior. Our results on the main channel, voting behavior, as well as on other engagement channels show that index funds monitor less than the active funds they are replacing. In this section we examine whether this change in monitoring materially affects corporate governance. Specifically, we examine firm-level outcomes to see if corporate behavior actually changes. Because our measures of corporate behavior are observed at the firm-year frequency, we cannot use our high-dimensional fixed effects specification. Thus, we use the difference-in-differences specification shown in Equation (1), which allows us to estimate changes in corporate governance policies following exogenous changes in fund ownership.

First, we look at the alignment of incentives between shareholders and firm management. Because index funds have less resources and less incentives to increase the share price of the companies they hold, they may exert less effort to ensure corporate managers have incentives to maximize stock price. Indeed, the results in Table VI show that index funds are significantly more likely to vote with firm management on agenda items about executive compensation and managerial entrenchment. To see whether this voting behavior changes actual firm policies, we examine a variety of managerial incentives including pay-performance sensitivity.[31]

Table IX, panel A, shows the results. Column 1 shows that after a firm switches from the Russell 1000 to the Russell 2000 (which leads to an increase in index fund ownership and a decrease in active fund ownership), the sensitivity of management's pay to changes in the

[31] McCahery et al. (2016) show that institutional investors consider managerial compensation to be one of the most important governance mechanisms. Importantly, executive compensation is something that frequently changes, so a change in monitoring behavior could plausibly affect it.

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stock price falls by an average of 43 log points. The drop in pay-performance sensitivity goes along with an increase in total compensation (column 2) and is driven by an increase in the fraction of cash compensation (and a fall in the fraction of equity-linked compensation; see column 3). Importantly, these results are mirrored for firms that switch from the Russell 2000 to the Russell 1000. For those firms, pay-performance sensitivity rises, total compensation falls, and the fraction of equity-linked compensation rises. In other words, pay-performance sensitivity decreases after an increase in ownership by index funds (and vice versa).

We also examine the presence of a golden parachute and CEO turnover (columns 4 and 5). For these variables, we observe no significant changes. However, unlike compensation, which changes frequently, both of these variables rarely change in our sample regardless of index fund ownership, so it is not surprising we see no effect. Taking all the evidence together, the results are consistent with index funds having less incentive to raise their portfolio firms' share price and thus bargaining less effectively with firm management. As a result, managers get higher compensation and lower pay-performance sensitivity.[32]

Next, in light of our voting results and the results on managerial compensation, we investigate the effects of index fund ownership on broader firm-level measures of governance. The governance principles generally highlighted by the Big Three fund families are (1) to support board independence, which we measure by the fraction of independent directors on the board; (2) to oppose managerial entrenchment and antitakeover policies, which we measure by the E-index of Bebchuk et al. (2008), poison pills, supermajority votes requirements, restrictions on the ability of shareholders to call a special meeting and to act by written consent; and (3) to oppose unequal voting rights, which we measure by dual-class share

[32]These results agree with the results in Mullins (2014) in an earlier sample that is disjoint to ours. It is worth noting that other effects of index assignment could contribute to these effects. For example, Coles, Heath, and Ringgenberg (2020) find that switching to the Russell 2000 raises a firm's return correlation with the aggregate stock market.

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structure. Table IX, panel B, shows the results.

In column 1, we find that an increase in index fund ownership is followed by a decrease in board independence. The results confirm the findings in Schmidt and Fahlenbrach (2017) using a different sample period.[33] In column 2, we find no significant change in the E-index for either group of treated firms. Similarly, in columns 3 through 7, we find no changes in other governance measures, specifically, poison pills, supermajority voting requirements, shareholders' ability to call a special meeting or to act by written consent, and firms' dual-class share structure.

These findings contrast with some of the findings in Appel et al. (2016), which show that index fund ownership improves corporate governance in several dimensions. One possible interpretation of the results in Appel et al. (2016) is that index funds do some monitoring. However, we show that index funds monitor *less* than the active funds they are replacing. As a result, corporate governance gets worse. Consistent with the notion that a reduction in monitoring leads to worse firm performance, in Table X, we find that firms that switch from the R1000 to the R2000 experience a reduction in valuation and performance (as measured by Tobin's q, market-to-book, and return on assets). The value implications of different governance structures are unclear and constantly debated in the literature. Our results are consistent with an agency conflicts view; in particular, greater index funds ownership is followed by less board independence and weaker incentive alignment between managers and shareholders, and this is associated with worse firm performance.

Why do our findings conflict with the results in Appel et al. (2016)? As discussed in Section III, there are two main concerns with many existing papers that use Russell index

[33]While Schmidt and Fahlenbrach (2017) examine a different sample period (pre-2007), their approach is similar to ours in that they examine changes in outcomes (*i.e.*, they examine the year of index assignment minus the year prior). Thus, their approach implicitly removes preexisting differences among sample firms.

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reconstitutions. First, using the Russell *float-adjusted* market capitalizations and rankings creates pretreatment differences between treated and control firms (Wei & Young, 2020). Second, the need to impute the unadjusted rankings used by Russell to reconstitute the index introduces a measurement error into the forcing variable that can lead to biased estimates (Pei & Shen, 2017). Appel, Gormley, and Keim (2019a) argue that their methodology is robust to these concerns. However, their main estimates use the *float-adjusted* rankings and do not use firm fixed effects to control for errors in the imputed rankings. Using the research design and sample period from Appel et al. (2016), we are able to replicate their findings. Specifically, we find that index fund ownership, instrumented with assignment to the Russell 2000 index, appears to lead to improvements in corporate governance. For example, they find (and we replicate using their method) that assignment to the Russell 2000 in June seemingly causes firms to remove a dual-class share structure over the next 12 months.[34]

Real-world data make it impossible to know if an estimator is biased. However, it seems implausible that an increase in index fund ownership leads to the removal of dual class share structures. Imbens and Lemieux (2008) note that one way to examine identification assumptions is to examine dependent variables "known not to be affected by the treatment." An unbiased methodology should find no effect for these variables. We believe dual-class shares should be considered "known not to be affected," because firms very rarely change their share class structure.[35] Of the 1,702 firm-years in our replication for which we observe dual-class share status, only six *total* cases of firms change their share structure. In contrast, the standardized coefficient in Appel et al. (2016) of -1.005, times their first-stage coefficient of 0.473, times their sample standard deviation of $DualClass$ of 0.33 implies that assignment

[34]See Section E of the Online Appendix for the full replication details.

[35]If one examines enough dependent variables, it is always possible to find a significant result due to a Type I error; we examine dual class shares because they are a key dependent variable in the existing literature.

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to the Russell 2000 in June caused treated firms to be 16 percentage points less likely to have a dual-class share structure over the next year. The implication is that Russell index assignment caused one-sixth of the treated firms in the sample (approximately 135 firms in total) to change their share structure within 12 months of index assignment. Since only six firms change their share structure in the replication sample, ownership by index funds could not have possibly generated a change of such magnitude. Moreover, among the six firms that did change their share structure, the direction of the change is unrelated to their index assignment (see Online Appendix Table A11).

By contrast, our cohort difference-in-differences design finds zero treatment effects on dual-class shares. It is important to note that these zero results are not due to low statistical power. To see this, for each coefficient estimate in Table IX, we compute the minimum detectable effect size (MDES) following Bloom (1995). The MDES is a simple measure of the magnitude of a treatment effect that a test could reliably detect. Focusing on dual-class shares (column 7), the MDES is on the order of 1 to 3 percentage points, meaning we have the ability to detect a treatment effect that changed the status of as few as (2,592 $\times$ 1%) = 26 firms throughout our entire Russell sample. Similarly, for supermajority requirements (column 4) the MDES for switching to the Russell 2000 is 3%, meaning we have the ability to detect an effect that changed as few as 78 firms' status throughout our entire Russell sample. Thus, our approach is adequately powered to detect relatively small effects. Index funds ownership and these variables are simply unrelated.

Thus, the data suggest that our results on corporate governance differ from the existing literature due to the issues noted by Wei and Young (2020) and Pei and Shen (2017). Moreover, we note that our evidence on monitoring behavior exploits data at the fund-firm-agenda item level which allows us to include firm-by-year fixed effects that sweep out time-varying

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heterogeneity at the firm level. As such, we provide clear evidence that index funds monitor less than the active funds they replace using a cleanly identified panel regression that does not rely on identification assumptions about Russell index rebalancing. Combined, all of the evidence points in the same direction: The rise of index funds results in weaker monitoring thereby shifting power from investors to firm management.

VI. Conclusion

Theoretically, the increasingly large positions held by index funds should motivate them to monitor their portfolio firms. Yet, these new intermediaries have different incentives than traditional active funds. To date, a large literature has examined the effect of index fund ownership on corporate outcomes without examining the mechanism that allows investors to influence firm management. We provide comprehensive evidence on this fundamental issue. Specifically, using data that covers the dramatic increase in index investing from 2004 to 2018, we examine the monitoring behavior of index funds relative to active funds. We examine a wide variety of samples and tests, ranging from the universe of funds and firms to smaller and precisely identified subsamples. The results uniformly indicate that, relative to active funds, index funds monitor less effectively and cede power to firm management.

Our findings are consistent with the theoretical predictions in Bebchuk et al. (2017), Edmans et al. (2018), and Corum et al. (2020). Specifically, we find that relative to active funds, index funds are significantly more likely to side with firm management on contentious corporate governance votes. Low-fee index funds are even more likely to vote with firm management, which indicates that the low-cost structure of index funds directly affects their capacity to monitor. We find no evidence that index funds effectively engage with firm

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management either publicly or privately. As a result, we find that corporate governance worsens following an increase in index fund holdings. In sum, our findings all point to the same conclusion: the rise of index investing shifts power from investors to corporate managers.

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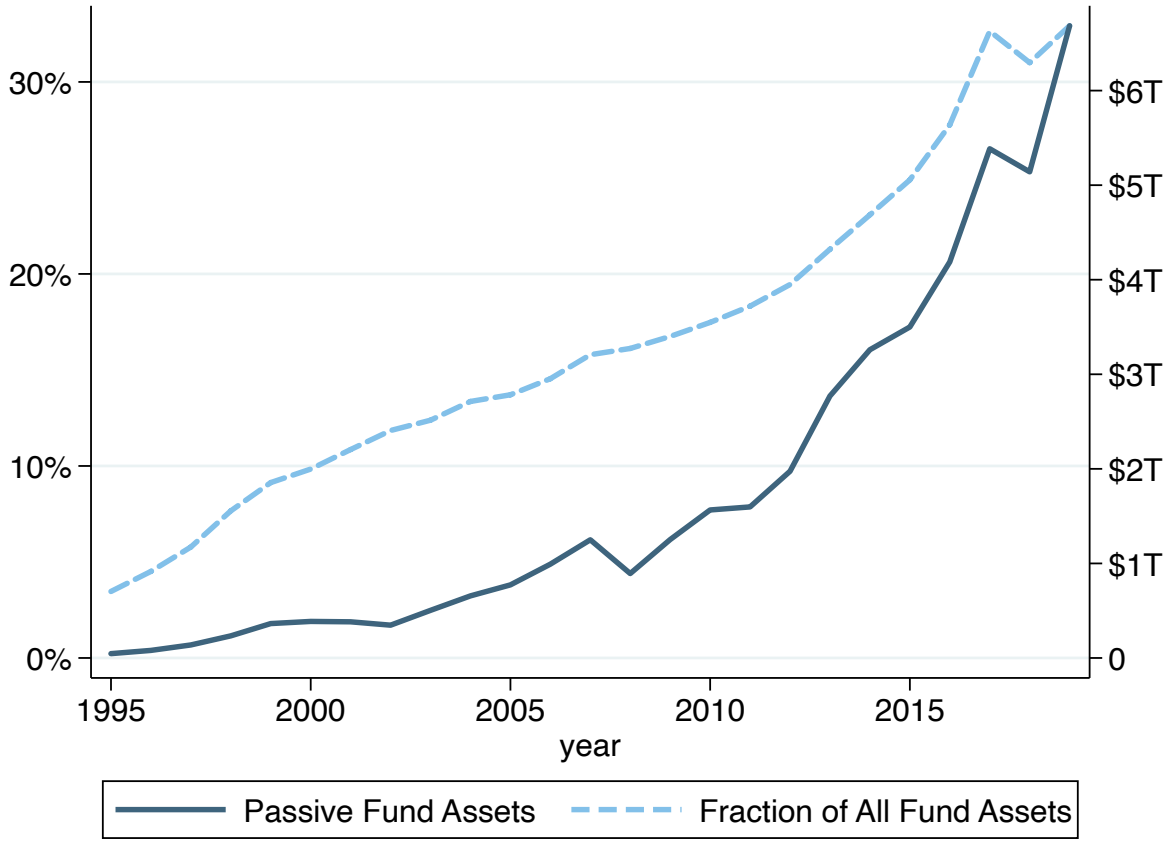

Figure 1. Yearly assets under management for index funds
The figure plots the total assets under management (AUM) for passively managed index funds in the CRSP Mutual Fund database, by year, in total dollars (solid line) and as a fraction of AUM (dashed line) across all funds.

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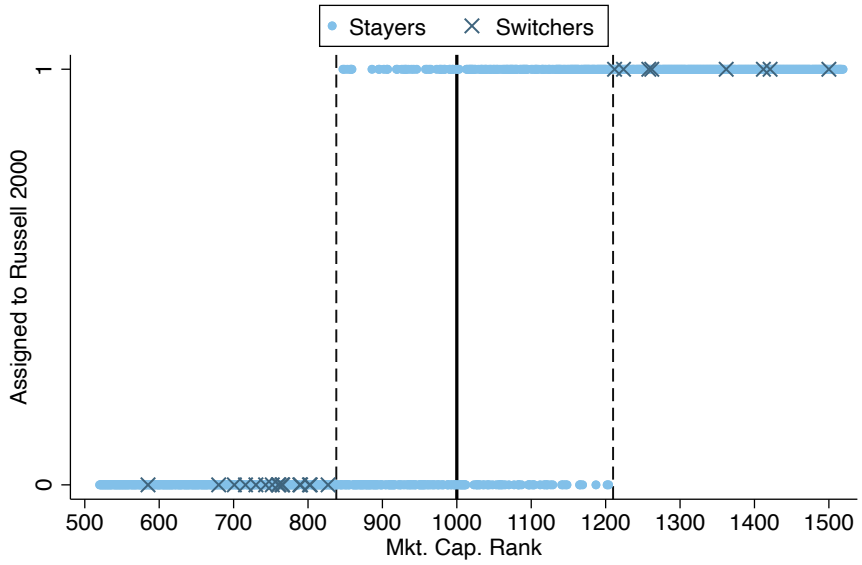
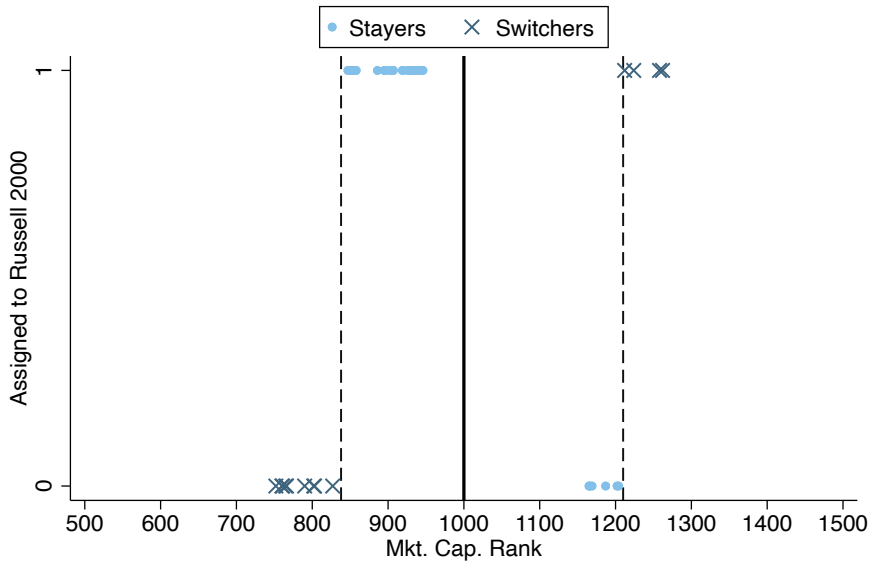


(a) June 2007 Index Assignment



(b) 2007 Cohort Firms

Figure 2. Selection of cohort samples

Panel A plots the cutoff, bands, and index assignments for the Russell index assignment in June 2007. Panel B shows the 2007 cohort, consisting of all Russell stocks that lay within a +/-100-rank window of the upper and lower bands, and that were potential switchers, that is, 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.

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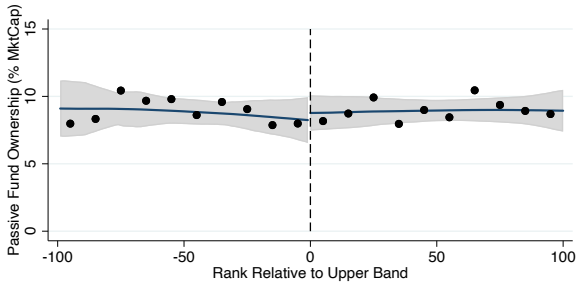
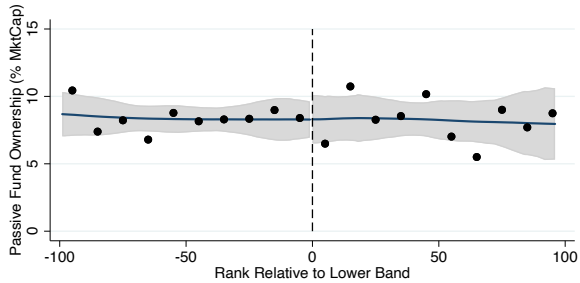
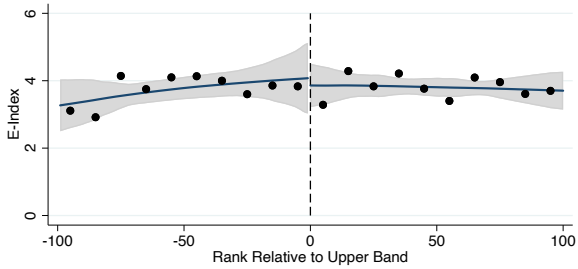


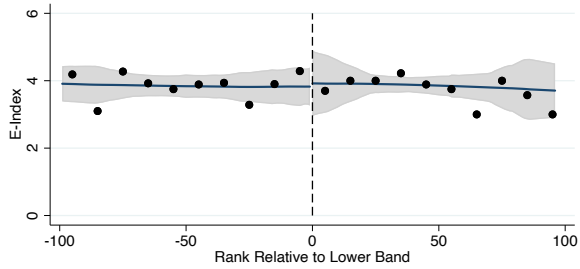
(a) Pretreatment index fund ownership, Upper band



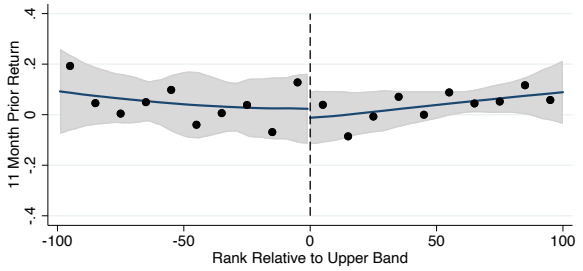
(b) Pretreatment index fund ownership, Lower band



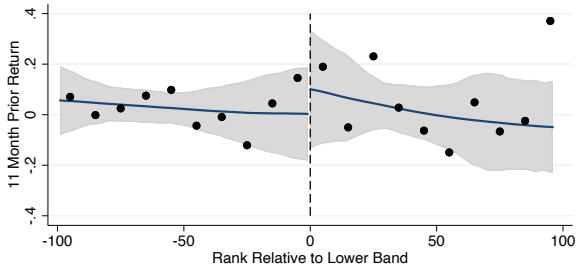
(c) Pretreatment E-Index, Upper band



(d) Pretreatment E-Index, Lower band



(e) Pretreatment stock returns, Upper band



(f) Pretreatment stock returns, Lower band

Figure 3. Balance tests

The figure presents regression discontinuity plots of (A, B) ownership by index funds, (C, D) the Entrenchment index (E-index) of Bebchuk et al. (2008), and (E, F) lagged 11-month stock returns, across the upper (left side) and lower (right side) bands as of the last pretreatment year, for firms in the Russell cohorts. Local polynomial regression lines are in blue. Ninety-nine percent confidence intervals are in gray.

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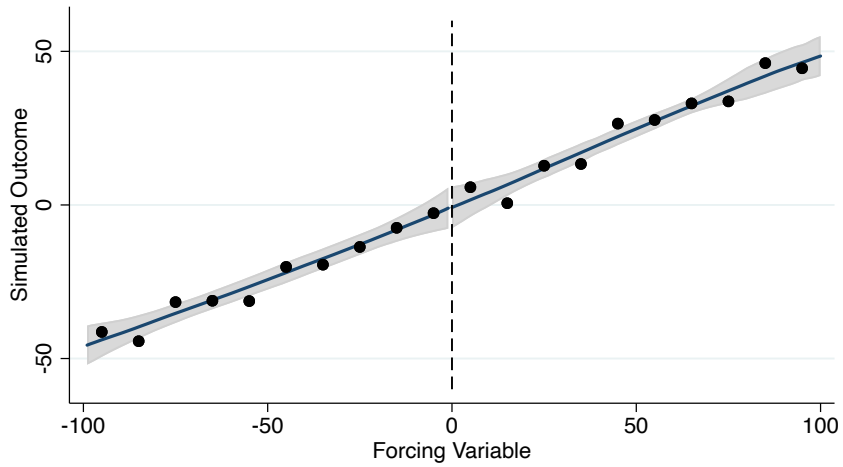
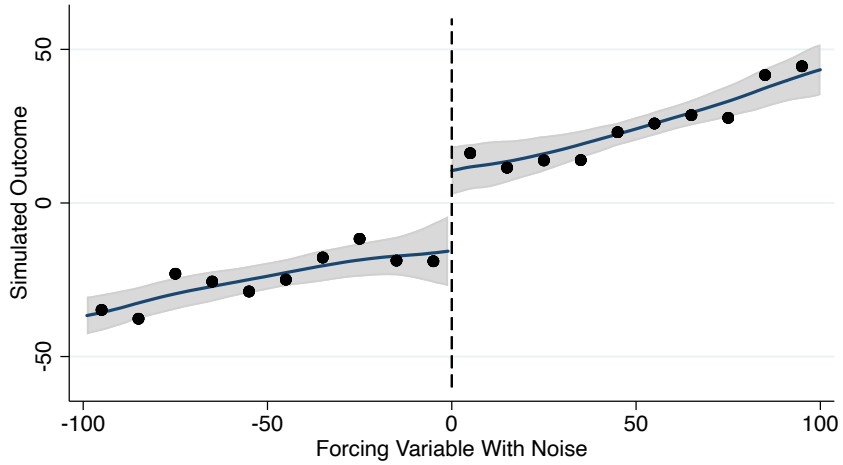


(a) No measurement error



(b) With measurement error

Figure 4. 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 (panel A) and when the forcing variable is measured with noise (panel B). The true treatment effect in the simulated data is zero, that is, by construction, there is no discontinuity in the outcome variable across the threshold. Local polynomial regression lines are in blue. Ninety-nine percent confidence intervals are in gray.

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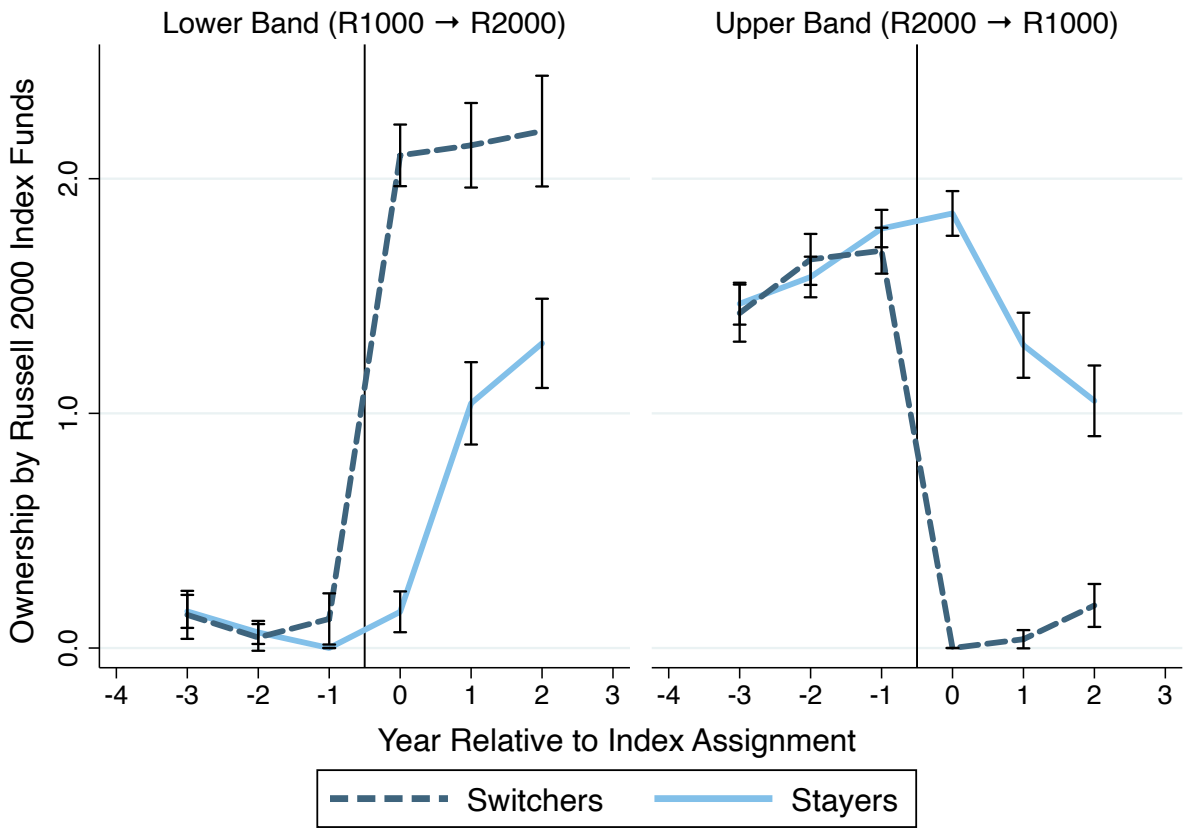

Figure 5. Index switching and index fund ownership

The figure plots the evolution of index fund ownership, in percentage points of the stock's market capitalization, relative to index assignment. The left figure displays 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. The right figure displays 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 vertical bars represent 95% confidence intervals.

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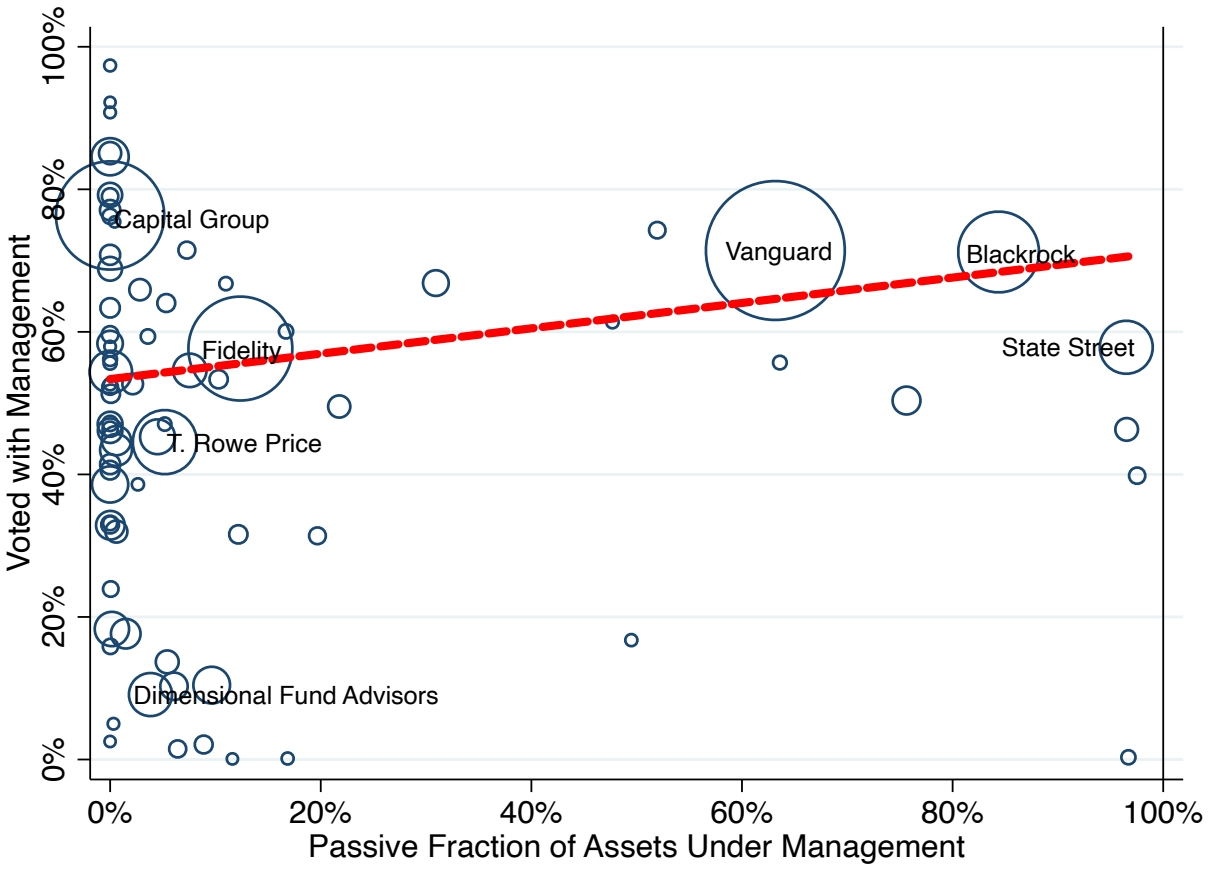

Figure 6. Voting behavior by fund families

The figure plots the fraction of votes cast on contentious agenda items that followed management's recommendation, against the fraction of total assets under management that was in passively managed index funds, across mutual fund families from 2004 to 2018. The area of each circle corresponds to the family's average total assets under management from 2004 to 2018. The dashed line represents the line of best fit.

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Table I
Summary statistics of funds

	Mean	SD	P10	Median	P90	No. obs.
		Index funds (N=882)				
AUM ($M)	2,570	9,803	31	317	4,214	4,489
Expense ratio	0.47%	0.32%	0.12%	0.42%	0.75%	4,489
# stocks held	308	527	12	92	653	4,308
		Active funds (N=4,335)				
AUM ($M)	1,647	5,685	26	229	3,297	25,385
Expense ratio	1.14%	0.41%	0.67%	1.11%	1.69%	25,385
# stocks held	98	175	12	57	191	23,882

The table presents summary statistics for all investment funds in our sample from 2004 to 2018. *AUM* is assets under management, in millions of US$; *Expense ratio* is from the CRSP mutual fund database; and # *Stocks held* is the number of stocks held in each fund on a given date.

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Table II
Summary statistics of firms

	Mean	SD	P10	Median	P90	No. obs.
Market cap ($M)	2,534	1,555	1,085	2,181	4,311	4,650
$IndexOwn^{R2000}$	1.06%	1.09%	0.00%	0.92%	2.47%	4,650
$IndexOwn^{R1000}$	0.11%	0.14%	0.00%	0.00%	0.31%	4,650
$IndexOwn^{All}$	9.23%	5.62%	1.99%	8.98%	16.77%	4,650
$ActiveOwn$	25.25%	12.19%	8.79%	25.72%	40.14%	4,650
logPPS	5.87	1.35	4.52	5.90	7.44	3,445
logTotalComp	8.26	1.81	7.62	8.53	9.47	3,225
EquityFrc	0.46	0.21	0.18	0.46	0.73	3,143
GldnPara	0.74	0.44	0	1	1	2,690
CEOTurnover	0.11	0.31	0	0	1	3,930
BoardIndep	0.77	0.12	0.60	0.80	0.90	2,635
E-index	3.81	1.22	2	4	5	2,690
PoisonPill	0.21	0.41	0	0	1	2,690
Supermaj	0.95	0.22	1	1	1	2,690
LimSpecMeet	0.51	0.50	0	1	1	2,690
WrConsent	0.50	0.50	0	0	1	2,690
DualClass	0.06	0.23	0	0	0	2,690

The table presents summary statistics for firms in the Russell cohort sample. The sample runs from 2004 to 2018. Firms in the Russell cohort sample are selected on the basis of lagged index membership and proximity to the upper and lower Russell bands, in each June cohort from 2007 to 2016. Each selected firm is included for 3 years before and after its cohort year. *Market cap* is market capitalization, in millions of US$, calculated from CRSP data. Appendix A defines the other variables.

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Table III
Index switching and investor composition

	Dependent variable = Ownership (percentage points of market capitalization) by				
	$IndexOwn_{jt}^{R2000}$ (1)	$IndexOwn_{jt}^{R1000}$ (2)	$IndexOwn_{jt}^{All}$ (3)	$ActiveOwn_{jt}$ (4)	$OtherOwn_{jt}$ (5)
$R1000 \rightarrow R2000_j \times$ $PostAssignment_t$	1.72***	-0.21***	1.31***	-2.21**	0.89
	(0.14)	(0.02)	(0.35)	(0.75)	(0.89)
$R2000 \rightarrow R1000_j \times$ $PostAssignment_t$	-1.63***	0.22***	-1.20***	1.60**	-0.40
	(0.08)	(0.01)	(0.25)	(0.57)	(0.68)
Observations	4,649	4,649	4,649	4,649	4,649
Adjusted R^2	.510	.528	.841	.705	.738
Year FE	Yes	Yes	Yes	Yes	Yes
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes

The table presents differences-in-differences estimates of the effects of Russell index switching on equity ownership expressed as a percentage (1=1%) of the firm's market capitalization. Each yearly cohort consists of firms that were "potential switchers" within a +/-100-rank window of the yearly Russell upper and lower bands in that year. The sample consists of observations for 3 years before and after index assignment for each firm in each cohort. *R1000* $\rightarrow$ *R2000* equals one if a firm switches from the Russell 1000 to the Russell 2000, whereas *R2000* $\rightarrow$ *R1000* equals one if a firm switches from the Russell 2000 to the Russell 1000. $PostAssignment_t$ is an indicator variable that equals one after index rebalancing. The dependent variables are ownership by (1) Russell 2000 index funds, (2) Russell 1000 index funds, (3) all index funds, (4) all active mutual funds, and (5) other investors. Robust standard errors clustered by firm and year are shown in parentheses. *p <.1; **p <.05; ***p <.01.

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Table IV
Summary statistics of fund voting

Management recommends	ISS recommends	Index funds vote				Active funds vote				Difference	
		Yes	No	Abstain	DNV	Yes	No	Abstain	DNV	PctYes	N
All		89.8%	6.7%	3.3%	0.2%	88.9%	7.6%	3.1%	0.4%	0.9%	27,297,366
Consensus											
Yes	Yes	94.9%	3.7%	1.4%	0.1%	95.2%	3.3%	1.2%	0.3%	-0.4%	24,293,163
No	No	5.1%	82.7%	9.9%	2.3%	5.4%	82.9%	10.1%	1.6%	-0.3%	398,666
Contentious											
Yes	No	53.4%	20.1%	24.7%	1.8%	43.9%	24.9%	28.7%	2.5%	9.5%	1,761,341
No	Yes	43.1%	51.3%	5.5%	0.1%	47.2%	46.8%	5.7%	0.3%	-4.1%	844,196

The table summarizes the ISS voting data and presents comparisons of fund voting between active and index funds. The table shows the fraction of each type of fund that voted Yes, No, or 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 2004 to 2018. N is the number of individual fund-vote observations.

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Table V
Fund voting

	Dependent variable = $VotedWithMgmt$			
	(1)	(2)	(3)	(4)
$IndexFund_i$	0.101***	0.102***	0.101***	0.103***
	(0.024)	(0.023)	(0.024)	(0.023)
$ExpenseRatio_{it}\times$		-0.286***		-0.288***
$IndexFund_i$		(0.062)		(0.061)
$ExpenseRatio_{it}\times$		-0.022		-0.024
$ActiveFund_i$		(0.036)		(0.035)
Observations	2,601,806	2,601,806	2,601,605	2,601,605
Adjusted R^2	.061	.072	.105	.116
Firm FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Firm × Year FE	No	No	Yes	Yes

The table presents panel regression estimates of funds' voting on contentious items for index funds versus active funds. The dependent variable, $VotedWithMgmt$, equals one if a fund votes with management's recommendation and zero otherwise. $IndexFund$ equals one if the fund is an index fund, and zero if the fund is an active fund. $ExpenseRatio$ is the fund's total expense ratio in that year expressed in percentage points (1% = 1). Columns 1 and 2 show estimates with firm and year fixed effects; columns 3 and 4 show estimates with firm-by-year fixed effects. The sample consists of all firms and funds, for votes on contentious items only (i.e., items on which ISS and firm management were opposed). Robust standard errors clustered by fund and firm are shown in parentheses. *p <.1; **p <.05; ***p <.01.

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Table VI
Fund voting, split by vote type

	Dependent variable = $VotedWithMgmt$			
Vote type:	Board of directors (1)	Compensation (2)	Disclosure (3)	Entrenchment (4)
$IndexFund_i$	0.110***	0.113***	0.063**	0.048***
	(0.027)	(0.031)	(0.032)	(0.017)
Observations	1,428,041	35,131	122,322	80,766
Adjusted R^2	.108	.049	.019	.125
Firm×Year FE	Yes	Yes	Yes	Yes

The table presents panel regression estimates that examine funds' voting for index funds versus active funds, when we split the sample by agenda item type. Item type is *Board of Directors* in column 1, *Compensation* in column 2, *Disclosure* in column 3, and *Entrenchment* in column 4. The dependent variable, $VotedWithMgmt$, equals one if a fund votes with management's recommendation and zero otherwise. $IndexFund$ equals one if the fund is an index fund and zero if the fund is an active fund. The sample consists of all firms and funds for votes on contentious items only (i.e., items on which ISS and firm management were opposed). Robust standard errors clustered by fund and firm are shown in parentheses. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$.

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Table VII
Evidence on public engagement

	Dependent variable = Fraction 13D		
	(1)	(2)	(3)
$FracAUMPassive_{jt}$	-1.00**	-0.91**	-1.03**
	(0.46)	(0.44)	(0.47)
	[-24%]	[-22%]	[-25%]
$logAUM_{jt}$		-0.042	
		(0.038)	
$numFilings_{jt}$			0.00032
			(0.00025)
Model	Probit	Probit	Probit
Observations	1,070	1,070	1,070
Pseudo R^2	.012	.015	.014

The table presents comparisons of fund families' blockholding disclosure filings. The dependent variable is "Fraction 13D," which is the fraction of each family-year's filings that were under the activist Schedule 13D versus 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. Marginal effects are displayed in square brackets. Robust standard errors clustered by fund-family are shown in parentheses. *p <.1; **p <.05; ***p <.01.

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Table VIII
Evidence on private engagement

A. Fund voting on management versus shareholder proposals

	Management proposals			Shareholder proposals		
	$VoteYes$	$VoteNo$	*Abstain*	$VoteYes$	$VoteNo$	*Abstain*
	(1)	(2)	(3)	(4)	(5)	(6)
$IndexFund_i$	0.117***	-0.043***	-0.068***	-0.067***	0.069***	0.001
	(0.028)	(0.011)	(0.018)	(0.023)	(0.022)	(0.008)
Observations	1,738,780	1,738,780	1,738,780	862,819	862,819	862,819
Adjusted R^2	.106	.355	.298	.118	.093	.105
Firm×Year FE	Yes	Yes	Yes	Yes	Yes	Yes

B. Index switching and management versus shareholder proposals

	Management Proposals			Shareholder Proposals		
	Number contentious	*Fraction contentious*	*Fraction passed*	*Number contentious*	*Fraction contentious*	*Fraction passed*
	(1)	(2)	(3)	(4)	(5)	(6)
$R1000 \rightarrow R2000_j \times$ $PostAssignment_t$	-0.14	-0.01	-0.00	0.22	0.08	-0.17*
	(0.16)	(0.01)	(0.01)	(0.32)	(0.11)	(0.09)
$R2000 \rightarrow R1000_j \times$ $PostAssignment_t$	0.01	0.00	-0.00	0.24	0.10	-0.06
	(0.10)	(0.01)	(0.01)	(0.27)	(0.19)	(0.12)
Observations	4,137	4,137	4,137	198	198	198
Adjusted R^2	.425	.443	.130	.161	.208	.517
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

The table presents estimates of how funds vote on contentious agenda items depending on who proposed the item (panel A), and differences-in-differences estimates of the effects of Russell index switching on the number and type of agenda items proposed (panel B). Both panels split the sample into agenda items proposed by firm management versus agenda items proposed by shareholders. In panel A, the dependent variable is $VoteYes$, $VoteNo$, or *Abstain*. In panel B, the dependent variables measure the number of contentious proposals; the fraction of proposals that were contentious; and the likelihood of passage for the two types of proposal. Robust standard errors clustered by fund and firm (panel A) and firm and year (panel B) are shown in parentheses. *p <.1; **p <.05; ***p <.01.

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Table IX
Index switching, managerial incentives, and firm governance

A. Managerial incentives

	(1) logPPS	(2) logTotalComp	(3) EquityFrc	(4) GldnPara	(5) CEOTurnover
$R1000 \rightarrow R2000_j \times$	-0.43***	0.56***	-0.06**	0.00	-0.06
$PostAssignment_t$	(0.11)	(0.08)	(0.02)	(0.03)	(0.05)
MDES	±0.33	±0.23	±0.07	±0.09	±0.14
$R2000 \rightarrow R1000_j \times$	0.27**	-0.41***	0.03**	0.02	0.02
$PostAssignment_t$	(0.10)	(0.06)	(0.01)	(0.02)	(0.02)
MDES	±0.32	±0.18	±0.04	±0.07	±0.08
Observations	3,445	3,219	3,138	2,592	3,923
Adjusted R^2	.630	.815	.629	.698	.020
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

B. Firm governance

	(1) BoardIndep	(2) E-index	(3) PoisonPill	(4) Supermaj	(5) LimSpecMeet	(6) WrConsent	(7) DualClass
$R1000 \rightarrow R2000_j \times$	-0.03***	-0.07	-0.06	-0.01	-0.00	-0.05	0.00
$PostAssignment_t$	(0.01)	(0.08)	(0.05)	(0.01)	(0.02)	(0.03)	(0.00)
MDES	±0.02	±0.25	±0.14	±0.03	±0.06	±0.10	±0.01
$R2000 \rightarrow R1000_j \times$	0.00	0.05	-0.01	0.01	0.00	0.06	-0.01
$PostAssignment_t$	(0.01)	(0.06)	(0.04)	(0.02)	(0.03)	(0.03)	(0.01)
MDES	±0.03	±0.19	±0.12	±0.06	±0.09	±0.10	±0.03
Observations	2,613	2,592	2,592	2,592	2,592	2,592	2,592
Adjusted R^2	.712	.830	.720	.720	.801	.778	.938
Firm × Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The table presents differences-in-differences estimates of the effects of Russell index switching on managerial incentives (panel A) and on measures of firm governance (panel B). 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 2016, 3 years before and after index assignment for each firm in each cohort. The table also displays the minimum detectable effect size (MDES) below each coefficient. Robust standard errors clustered by firm and year are shown in parentheses. *p <.1; **p <.05; ***p <.01.

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Table X
Index switching and firm value

	(1) $\log(q)$	(2) $\log(q^{TOT})$	(3) log(Mkt/book)	(4) ROA
$R1000 \rightarrow R2000_j \times$ $PostAssignment_t$	-0.10***	-0.21***	-0.12**	-0.03***
	(0.03)	(0.05)	(0.04)	(0.01)
$R2000 \rightarrow R1000_j \times$ $PostAssignment_t$	0.01	0.06*	-0.03	0.00
	(0.01)	(0.03)	(0.02)	(0.01)
Observations	4,296	3,403	4,552	4,188
Adjusted R^2	.814	.812	.774	.678
Year FE	Yes	Yes	Yes	Yes
Stock × Cohort FE	Yes	Yes	Yes	Yes

The table presents difference-in-differences estimates of the effects of Russell index switching on measures of firm value and performance. Each yearly cohort consists of firms that were "potential switchers" within a +/-100-rank window of the yearly Russell upper and lower bands in that year. The sample consists of observations for 3 years before and after index assignment for each firm in each cohort. *R1000* $\rightarrow$ *R2000* equals one if a firm switches from the Russell 1000 to the Russell 2000, whereas *R2000* $\rightarrow$ *R1000* equals one if a firm switches from the Russell 2000 to the Russell 1000. $PostAssignment_t$ is an indicator variable that equals one after index rebalancing. The dependent variables are $\log(q)$, which is the natural logarithm of one plus Tobin's q (column 1), $\log(q^{TOT})$, which is the natural logarithm of one plus adjusted Tobin's q calculated by Peters and Taylor (2017) (column 2), log(Mkt/Book), which is the natural logarithm of one plus the market-to-book ratio (column 3), and accounting return on assets (column 4). Robust standard errors clustered by firm and year are shown in parentheses. *p <.1; **p <.05; ***p <.01.

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VII. Appendix

A. Variable Definitions

$VotedwithMgmt$: An indicator variable for each fund's vote on each agenda item that equals one if (a) management recommends a Yes vote and the fund votes yes or (b) management recommends a No or Withhold vote and the fund votes No or Abstain, and zero otherwise

$IndexFund_i$: An indicator variable that equals one 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 ($IndexFund_i = 0$)

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

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

$IndexOwn_{jt}^{All}$: The fraction of firm j's market capitalization held in December of year t by index mutual funds or ETFs, where index funds are those funds with fund flag = "D" in the CRSP Mutual Fund Database

$ActiveOwn_{jt}$: The fraction of firm j's market capitalization held in December of year t by active mutual funds or ETFs, where active funds are all funds that are not index funds

$OtherOwn_{jt}$: The fraction of firm j's market capitalization held in December of year t by investors who are not index funds or active funds (*i.e.*, 100 - ActiveOwn - IndexOwn)

$logPPS_{jt}$: The log of one plus the total sensitivity (Delta) of compensation to changes in the firm's stock price, summed across the firm's top officers

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$logTotalComp_{jt}$: The log of one plus the total compensation paid to the firm's top officers, scaled by the firm's lagged market capitalization

$EquityFrc_{jt}$: The fraction (between zero and one) of total compensation

$TotalComp$ that is accounted for by equity-linked compensation, that is, stock and option grants

$GldnPara_{jt}$: An indicator variable that equals one if the firm's CEO has a golden parachute in place in that year

$CEOTurnover_{jt}$: An indicator variable that equals one if the current CEO leaves the firm in year t

$BoardIndep_{jt}$: The fraction of the board of directors in year t that are classified as independent

$E - index_{jt}$: The entrenchment (E-) index of Bebchuk et al. (2008)

$PoisonPill_{jt}$: An indicator variable that equals one if the firm has a poison pill provision in place in year t

$Supermaj_{jt}$: An indicator variable that equals one if the firm requires a supermajority of votes for (a) merger approvals, (b) amendments of bylaws, or (c) charter amendments in year t

$LimSpecMeet_{jt}$: An indicator variable that equals one if the firm has a policy in place that limits the ability of shareholders to call a special meeting in year t

$WrConsent_{jt}$: An indicator variable that equals one if a firm has a policy in place that limits the ability of shareholders to act by written consent in year t

$DualClass_{jt}$: An indicator variable that equals one if the firm has a dual-class share structure in year t

$ExpenseRatio_{it}$: The total expense ratio of fund i in year t. For funds with multiple

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share classes this is the average total expense ratio, weighted by the assets under management (AUM) in each share class

$VoteYes$: An indicator variable for each fund's vote on each agenda item, that equals one if the fund voted Yes

$VoteNo$: An indicator variable for each fund's vote on each agenda item, that equals one if the fund voted No

$Abstain$: An indicator variable for each fund's vote on each agenda item, that equals one if the fund voted Abstain

$Filed13D$: An indicator variable for each blockholding disclosure filing, which equals one if the filing used Schedule 13D and zero if it used Schedule 13G

$Fraction13D$: The fraction of each fund family's blockholder filings, in a given year, that used Schedule 13D

$FracAUMPassive_{jt}$: 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

$NumberContentious$: The number of contentious agenda items voted on at all firm meetings (both annual and special) in a given year

$FractionContentious$: The fraction of agenda items voted on at all firm meetings (both annual and special) in a given year that were contentious

$FractionPassed$: The fraction of agenda items voted on at all firm meetings (both annual and special) in a given year that passed

$log(q)$: The natural logarithm of one plus Tobin's q

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$log(q^{TOT})$: The natural logarithm of one plus adjusted Tobin's q as calculated by Peters and Taylor (2017)

$log(Mkt/Book)$: The natural logarithm of one plus the market-to-book ratio

ROA: Operating income before depreciation divided by lagged total assets

A.2 Categories of Agenda Items

In Table VI, we present voting on agenda items in the following four subcategories:

1. Board of Directors: Items whose description includes "director" or "board"

2. Compensation: Items whose description includes "compensation" or "incentive," excluding mandatory say-on-pay votes

3. Disclosure: Items whose description includes "disclosure" or "reporting"

4. Entrenchment: Items whose description includes "declassify," "supermaj," "poison pill," or "parachute"

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