The Governance Impact of Index Funds: Evidence from Regression Discontinuity

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Abstract

To examine the effect of institutional ownership on the governance dynamics of firms and corporate outcomes, I exploit an exogenous change in indexer ownership generated by the mechanical reconstitution of the Russell equity indices. Following reconstitution, I show that firms that are just included in the Russell 1000 index have higher institutional ownership (IO) levels and concentration than those just included in the Russell 2000 index of smaller firms. This is composed of both a change in indexers and closet index fund ownership, and a change in active IO, suggesting a complementarity between different types of institutional investors. Firms just included in the Russell 1000 substantially increase the performance sensitivity of their CEO's pay, and have a much higher likelihood of CEO turnover within two years. These firms also display greater resistance to management proposals at shareholder meetings and lower rate of failure for shareholder proposals. Finally, they have materially lower capital expenditures, and make fewer cash and diversifying acquisitions. Overall, these results are consistent with a significant impact of institutional preferences on corporate outcomes.

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1 Introduction

The principal-agent problem between shareholders and managers has been a central concern in economics since the seminal work of Berle and Means (1932). This paper focuses on the effectiveness of shareholder monitoring as a mechanism to influence managerial behavior, and in particular, on monitoring by financial institutions such as index mutual funds.

A large literature in corporate governance is built around the idea that institutional investors are relatively effective monitors of management both because of their sophistication and because their large size reduces the coordination and per-share costs of monitoring, and increases returns to governance (see, for example, Black, 1991; Gillan and Starks, 2000). However, it is difficult to identify the causal effect of institutional investors as monitors because of a well known endogeneity problem (Demsetz and Lehn, 1985) in which, among other things, institutions choose in which firms to invest so as to maximize returns. As a result they might choose to invest in firms they believe might benefit the most from additional monitoring, or conversely invest in firms they expect will have higher returns for other reasons. These selection issues imply observed cross-sectional correlations are likely to be a biased estimate of the impact of institutions as monitors.

We have even less evidence on the role - if any - that index funds play in corporate governance, despite the fact that they control a large and growing proportion of institutional assets under management: as of year end 2012, 24 percent of US institutional assets were in index funds and these funds are growing at a rate of 10 percent per year (Morningstar, 2013). Perhaps because of their volume-based, low margin business model, and passive stock selection approach, index funds are often believed to be unlikely to contribute in any way to monitoring of portfolio firms, and have been accused of being overly passive by activist investors (for example, Reuters, 2013: “U.S. activist investors gain from index funds’ passivity”).

Here I address the inference challenge by focusing on equity indices and by using exogenous variation due to the reconstitutions of the Russell 1000 index as an instrument for differences in index fund ownership. Firms are placed into the Russell equity indices for explicitly mechanical reasons, forcing index funds to hold their stock, breaking the link between firm and owner characteristics, and thereby obtaining clean identification of the causal effect of Index fund ownership on firm behavior. I use a regression discontinuity design (RDD) and find that firms whose index fund ownership rises as a result of index reconstitution do appear to be more heavily monitored, in contrast to the prevailing view of index funds which characterizes them as contributing nothing towards the monitoring of the firms they hold stakes in. I show that such firms raise the performance sensitivity of their CEO's pay without appearing to increase total pay, are more likely to have a new CEO within two years, have lower capital expenditures, and are less likely to engage in diversifying acquisitions in the two years following their inclusion in the index, in comparison to firms just excluded from the Russell 1000.

The natural experiment I consider compares firms near the threshold between the Russell 1000 and Russell 2000 equity indices. The Russell 1000 index (R1000 henceforth) contains the one thousand largest firms by market capitalization (i.e. firms ranked 1-1000), while the firms ranked 1001 to 3000 are placed in the Russell 2000 index (R2000 henceforth). On the last trading day in May, firms are ranked by a market capitalization measure and placed in each index for the entirety of the following year. If Firm A is ranked 1005th in May of year t-1 (i.e. in R2000) and then ranked 995th in May of year t, it will switch indices, moving from the R2000 to the R1000 in year t. Institutions that passively follow the R1000 are then obliged to buy the stock of firm A, and to sell the stock it replaces.

Importantly, in a narrow bandwidth around the index cutoff, whether a firm is placed in the R1000 or R2000 is largely random, based as it is on small price shocks on the day(s) immediately preceding reconstitution, and also on the shocks to surrounding firms. This means that firms on one side of the cutoff are well suited to be controls for firms on the other side because the only dimension along which they differ systematically is a market capitalization ranking which is explicitly controlled for. I focus on the reconstitutions occurring in the years 2002-2006 for which I was able to obtain the proprietary Russell market capitalization measure that assigns firms to each side of the cutoff. The evidence indicates firms are unable to manipulate the ranking based on this variable: I perform the McCrary (2008) test to determine whether manipulation of market capitalization occurs and find no evidence of bunching around the cutoff. To further establish the validity of the RDD in this setting I show that firms within the bandwidth do not differ across the threshold in terms of a series of observable characteristics in the period before reconstitution, that placebo tests using alternative index cutoffs show no differential effects, and that estimates are robust to the inclusion of a variety of controls and specification choices.

I first document that index inclusion leads to significant changes in firms' institutional shareholder base: firms just assigned to the R1000 index (composed of larger firms) see their total institutional ownership (IO henceforth) jump up by approximately 10% of firm equity (equivalent to a jump of \$150-\$200 million), providing a clean natural experiment for the effects of institutional ownership on firm behavior. This change is driven by firms moving up from the R2000 into the R1000, and is asymmetrical: firms moving down do not experience a statistically significant reduction in institutional ownership over the following year.

Interestingly, not all the change in IO is attributable to changes in the holdings of passive index funds. Using Bushee's (2001) three category classification of investors based on portfolio diversification and turnover, as well as the Thomson Reuters 13F type code I show that institutions that are likely to be index funds increase their holdings by 4%-8% of firm equity, concentrated in firms moving up into the R1000. The remaining change in IO is attributable to funds that are not obliged to switch their portfolio in lock step with the index, although many ostensibly more active investors choose to stay very close to their benchmark indices, effectively "closet indexing" (Cremers and Petajisto, 2009), while others employ explicit quasi-indexing strategies. Further, I show that the holdings of the largest ten institutions in each firm also rises by around 6-11% of firm equity on average, and that this increase is not driven solely by index fund ownership. In particular, institutions with low portfolio diversification and low turnover, as well as transient investors take relatively larger positions in firms just included in the index, in comparison to those just excluded. In the regression discontinuity design employed in this paper the increase in IO of non-indexers is also causally attributable to the instrument (index inclusion), suggesting a complementarity between index funds and other institutional owner types that appear more likely to exert an active monitoring role.

A natural question arises regarding which shareholders are displaced by the incoming institutional shareholders. Using a dataset of blockholder ownership collected by hand from proxy filings I show that total blockholdings do not differ across the threshold after reconstitution (either in terms of number or voting power), although there is weak evidence that blockholdings may increase in number. There are also no significant differences in insider (i.e. blockholder is an officer or director), outsider, institutional or non-institutional blockholdings, nor are there differences in the equity held by all officers and directors as a group. This implies that the increase in institutional ownership pushes out retail investors, the residual category. Given that retail investors are the group least likely to exert monitoring effort, it seems very likely that total monitoring rises.

I now turn to examining how ownership changes affects other governance dimensions within the firm. I first look at CEO compensation, which displays material differences in structure across the index cutoff, driven by firms moving up into the R1000. However, total CEO pay is not statistically higher for firms in the R1000 relative to the R2000 - the difference is instead in the structure of the pay package: option pay share is higher by 17% of total pay, which is offset by a salary pay share that is lower by 11% of total pay, and a restricted stock pay share that is 9% lower. This new pay structure, in which options are substituted for salary and restricted stock, is not unambiguously preferable for R1000 CEOs, and may in fact reduce their utility by increasing the amount of firm risk they bear without increasing total compensation. Additionally, the probability that a firm has a new CEO within two years of reconstitution is also 18% higher in firms just included in the R1000 relative to those just excluded, and this increase is concentrated among firms in the lowest quartile of stock returns in the first year in the index, suggestive of a stronger performance-turnover link in firms with higher institutional ownership.

The next governance dimension I examine is shareholder voting behavior at annual meetings to determine whether one of the central (and most observable) components of shareholder monitoring – voting at annual meetings – is affected by this change in IO. I find that the pass rate for management proposals is lower by approximately 3 percent for firms just in the R1000 relative to firms just included in the R2000, and the number of failed management proposals increases by 0.06 per meeting on average. This is a surprising result given management's structural control of shareholder meetings, reflected in the over 95 percent pass rate and the fact that management (almost) always wins close votes (Listokin, 2008). While the number of both management and shareholder proposals is no different across the cutoff I also find that the number of failed shareholder proposals falls by 0.22 per meeting, on average, over the two years following reconstitution. In short, management appears to face a less pliable shareholder population when institutional ownership rises in the natural experiment considered here. While shareholder voting behavior is a natural place to seek effects of IO, institutions often exert the greatest monitoring influence through private meetings with management rather than through voting (see for example Carleton et al., 1998, and Becht et al., 2009), so these results are likely to understate the changes in governance taking place.

Finally, I look at firm outcomes, first examining the corporate accounting variables that the literature suggests may change in response to increased monitoring. The only robust result is that capital expenditure as a percent of assets is over 2 percent lower for firms just included in the R1000 over the two years following reconstitution. R&D as a percentage of sales - a variable the literature suggests is particularly sensitive to institutional ownership - is 8 percent higher, but not significant at conventional levels. Leverage, profitability, market-to-book, and payout (in terms of both repurchases and dividends) do not differ significantly across the threshold after reconstitution. However, acquisitions behavior is different across the threshold: firms just included in the R1000 make approximately 0.5 fewer diversifying acquisitions per year than firms just in the R2000.

The main RDD specification in this paper is locally linear regression, which controls for the RDD assignment variable - market capitalization rank - linearly. A potential concern is that what drives the observed differences between firms on either side of the threshold is not the change in institutional ownership but rather each firm's movement over the past year in terms of market capitalization - for example, firms that are rising quickly through the rankings, or falling rapidly - and the level of market capitalization might be poorly suited to capture this dynamic aspect of the firm's size. To allay this concern I also run the RD including as controls both the change in Russell's relative ranking and the change in the value of market capitalization over the past year.

Relatedly, it is possible that the randomization carried out by the RDD may be imperfect, as occasionally occurs with randomized experiments that fail to stratify their sample on enough dimensions. As a result, differences in covariates between firms on either side of the threshold might be driving the results. This is of particular concern for the results on CEO pay, which is known to covary strongly with size and, to a lesser extent, tenure and other firm and industry level variables. To assuage this concern I re-run the RD controlling for a battery of variables that conceivably co-move with the dependent variable. For example, for the main CEO pay results I include controls for CEO age, CEO tenure, firm profits, sales, total assets, market value of the firm, sales growth, asset growth, market value growth, and number of employees. Coefficients and significance levels are largely unaffected, supporting effective randomization and validity of the RDD, as is also true when I include industry fixed effects.

The theoretical literature on managerial misconduct provides a variety of potential avenues for sub-optimal behavior from the perspective of the principal. For example, the manager may consume perks that do not enhance productivity (Jensen and Meckling, 1976); build "Empires" by focusing excessively on firm growth and acquisitions (Jensen, 1986) or "entrench" (Shleifer and Vishny, 1989); behave myopically in response to the signal extraction problem that the market faces in determining the firm's unobserved value (Holmstrom, 1982; Stein, 1989); or shirk and live the "quiet life" (Bertrand and Mullainathan, 2003).[1] These papers predict that if the main agency problems are empire building, entrenchment, and signal-jamming

[1] An additional stream of the literature posits managerial deviations from optimal behavior resulting from personality-based attributes of managers, such as acquisitions resulting from managerial hubris (Roll 1986), or overconfidence (Malmendier and Tate, 2008), and individual, fixed managerial styles (Bertrand and Schoar, 2003).

based myopia, increased monitoring should result in lower capital expenditures, greater R&D (which has low visibility and payoffs further in the future), and fewer acquisitions, especially diversifying ones. Alternatively, if the key agency problem is a desire for the quiet life and thus under-investment, then improved monitoring should, perhaps, be followed by greater capital expenditures and more acquisitions (albeit not necessarily diversifying ones) as management expends greater effort in expanding the firm.[2]

While the limited specificity of the theoretical predictions makes it difficult to argue that evidence clearly supports one class of models, the evidence in this paper is broadly consistent with the view that the main agency problems, at least as viewed by institutional investors, are related to over-investment rather than to the quiet life. Ferreira and Matos (2008) report that higher levels of independent and foreign institutional ownership are correlated with lower capital expenditures in cross sectional regressions in a large sample of non-US firms. However, they also highlight that institutional ownership is jointly determined with firm characteristics (and show that institutions have a preference for well-governed firms, among other characteristics), which means that the direction of causality remains an open question. Similarly, Baysinger et al. (1991) and Bushee (1998) show that institutional ownership is correlated with higher R&D expenditure, while Aghion et al. (2013) focus on R&D outcomes rather than inputs and instrument institutional ownership with S&P 500 inclusion, finding a positive effect on citation-weighted patenting. Two papers examine the effects of institutional owners of the acquirer on mergers and acquisitions activity. Chen et al. (2007) report that independent, long-term institutional blockholders are associated with better M&A performance, while Gaspar et al. (2005) find that long-horizon institutional shareholders are associated with higher bidder abnormal returns around merger announcements. Again however, the concern remains that the institutional shareholders whose portfolio firms have better acquisition outcomes may simply be better at identifying firms that make better acquisitions.

[2] It is theoretically possible that increased institutional ownership could exacerbate agency problems: Burkart et al. (1997) and Aghion and Tirole (1997), highlight the costs of monitoring in terms of foregone managerial initiative, and in Admati and Pfleiderer (2009) this occurs in some specifications, but requires the blockholder to sell its stake.

This paper provides new evidence on the link between Institutional Ownership and firm growth strategies (capital expenditure, acquisitions, R&D), CEO pay, and CEO turnover by providing causal estimates not subject to concerns about reverse causality or simultaneous determination of institutional ownership and outcomes. It also strengthens the evidence of active index fund monitoring provided by Matvos and Ostrovsky (2010), who show that, despite significant heterogeneity among such funds, S&P500 index funds can be very active monitors in terms of their voting behavior. Two contemporaneous papers have performed regression discontinuities using a similar quasi-experimental set up, but focusing on different aspects of the index inclusion. Chang et al. (2013) examine whether there is a price effect due to reconstitution in the month immediately following the reconstitution, and report a one month price effect, but no effect on liquidity or price volatility[3]. While they use a similar instrument they explicitly abstract from the governance effects which are considered here.

Crane et al. (2012) focuses on firms' payout policy. In contrast, my paper maps out the impact of index inclusion on firm governance, in particular on changes in the ownership structure of the firm, shareholder voting and CEO compensation and ultimately firm decisions such as M&A transactions, and investment policy. The other difference is that I use the proprietary market capitalization measure that that Russell Indexes uses to determine index assignment, and the results depend directly and materially on the use of this assignment variable. Crane et al. (2012) do not have access to such a measure, and instead use (a slightly transformed version of) the Russell index weights as the assignment variable. This identifies the wrong firms on either side of the discontinuity because after Russell places firms in each index based on each firm's market capitalization ranking, weights are assigned based on a *different* market capitalization measure: the free float component of market capitalization. As such, index weight rankings differ from the true assignment variable - the market capitalization ranking that assigns firms to indices - and the firms it identifies as being on either side of the threshold differ materially in terms of their market capitalization and free float, violating the basic RD

[3]There exists a stream of literature that highlights the role of liquidity in governance (e.g. Bhide, 1993; Maug, 1998; Kahn and Winton, 1998; Faure-Grimaud and Gromb, 2004; Edmans, 2009; Admati and Pfleiderer, 2009; Edmans and Manso, 2011), but the lack of a liquidity difference across the threshold means I cannot extract testable predictions from them

assumption of continuity across the threshold.[4] Intuitively, using Russell index weights to construct the assignment variable for a RDD compares the firms with the lowest free float (but not necessarily the lowest market capitalizations) in the Russell 1000 to the firms with both the highest market capitalizations and high free floats in the Russell 2000. This makes the firms on either side of the index cutoff inappropriate counterfactuals for each other, and as a result the use of the index weight ranking as the assignment variable (instead of the market capitalization ranking) for a RDD generates results with very different significance levels, coefficient magnitudes and even opposite signs in some cases.

This paper also contributes to the relatively small literature on CEO pay and institutional ownership. CEO compensation is a natural focus of institutional interest: a survey by McCahery et al. (2011) reports that institutions believe CEO compensation and ownership to be the two most important governance mechanisms. Hartzell and Starks (2003), and Almazan et al. (2005) report a positive relationship between institutional investor concentration and the performance sensitivity of CEO pay, as do Clay (2000) and Schmidt (2013), while Morse et al. (2011) provide evidence that institutions may reduce powerful CEOs' ability to rig pay composition to their advantage.

The natural experiment provided by the Russell Index reconstitution provides intriguing evidence of apparent complementarity in ownership between index funds and other institutional investor types. It is possible that the presence of index funds reduces coordination difficulties for non-blockholder institutional investors or activist investors. Equally, index funds might be complementary to other blockholders, augmenting their ability to align managers' interests with those of shareholders. More generally, it is unclear whether the central governance channel at work in this natural experiment is the large blockholder "voice" channel (Shleifer and Vishny, 1986; Burkart et al., 1997; Maug, 1998; Kahn and Winton, 1998) given index funds' large size and inability to sell their positions unless the index changes, or the "exit" channel (Parrino et al. 2003; Edmans, 2009; Admati and Pfleiderer, 2009; Edmans and Manso, 2011)

[4] Chang et al. (2013) also highlight the unsuitability of using the rankings based on index weights as a RD assignment variable

because the non-index fund institutional ownership that appears to move with index fund ownership is capable of relatively rapid exit. Both channels may be at work simultaneously, as might occur if funds within the same families coordinate to increase their governance impact (see for example Morgan et al., 2011) both in terms of voting and in terms of exercising governance via exit (or the threat of exit) by non-index fund members of the fund family. Finally, whether the changes documented here are value enhancing remains an open question, but the evidence presented here suggests that there is no value effect on average over the two years following index inclusion.

The structure of the paper is as follows. Section 2 describes the data. Section 3 describes the details of the Russell Index reconstitution, discusses the methodology, and provides evidence for the validity of the RDD. Section 4 reports the results, and section 5 concludes.

2 Data and Summary Statistics

I obtain Russell index constituents, index weights, and proprietary free float and market capitalization measures from Russell Indexes for 2002-2012 (Russell's proprietary measure of market capitalization is available only from 2002). Institutional ownership data is extracted from Thomson-Reuters Institutional Holdings (13F), which provides the equity positions every quarter end of all institutions that exercise investment discretion over at least USD \$100 million. Thus, institutions managing less than this amount are not in my data. I match this data by cusip and quarter to Quarterly CRSP-Compustat Merged data to obtain quarterly shares outstanding, because I find much of the shares outstanding data in the 13F data to be stale. I also remove stale 13F observations that are duplicate observations by manager number, cusip, fdate and rdate, keeping the oldest observation. I use Bushee's (2001) permanent three category classification of institutional investors as (i) "Quasi Indexers" (low turnover, high diversification), (ii) "Transient Investors" (high turnover, high diversification) and (iii) "Focused Investors" (low turnover, low diversification, which he calls "Dedicated"), available on his webpage to classify investors into types. To generate a proxy for index funds (which

I term "Indexers") I generate an indicator equal to one if the firm is classed as a "Quasi-Indexers" and if the Thomson Reuters 13F type code is that of an Investment Company or an Independent Investment Advisor (I use Bushee's extension of this, which does not distinguish between these types accurately). While this is a noisy identifier of index funds it does capture all the largest index funds managers that cover the Russell 1000, such as Dimensional Fund Advisors and Vanguard. I match the resulting dataset to the Russell data by cusip.

Table 1 reports summary statistics for the 13F data at the baseline (i.e. in the period(s) before index reconstitution), for a bandwidth of 100 firms each year, presented separately in the R1000 and the R2000. The final column presents the p value of a t test for differences in means across the R1000 and R2000 groups. No differences are significant except for market capitalization, which is expected given that it is the variable underlying the assignment variable of the RDD.

Blockholder information is collected by hand from annual proxy filings (Schedule 14A and 14C) available from the SEC's EDGAR database. I broadly follow the method described in Dlugosz et al. (2006) and Clifford and Lindsey (2011), although I also collect the total votes held by all officers and directors as a group, even if it is below the 5% threshold (it is always reported). I further use Schedule 13D and 13C information to determine whether shareholders have filed as 13D (investors with active intent towards the firm). Each proxy filing was carefully checked for double counting of holdings, which occurs frequently. Detailed decision rules for how the data was recorded are available from the author. Firms were randomly selected for the sample from within the bandwidth of 100 firms on either side of the threshold. Summary statistics obtained from Proxy filings in the year prior to reconstitution are in **Table 2**. It is worth noting that 65% of the annual meetings in my sample occur in April and May, the months immediately preceding reconstitution, and this is the point at which I obtain the baseline information on blockholdings. If blockholders strategically anticipate index inclusion to some degree, even if imperfectly, then this will generate some differences across the threshold at the baseline because of the short period of time between my baseline measure and the reconstitution event. Nonetheless, there are very few significant differences (although several are almost significant at the 10% level) and they do not appear to be economically large or significant. The average number of votes controlled by institutional shareholders appears to differ significantly in the table. However, on assignment the main specification of the RD with this baseline value as the dependent variable (i.e. after controlling for the assignment variable: market capitalization rank), the difference between firms in the R1000 and firms in the R2000 is not significant.

CEO compensation data is drawn from Execucomp. I identify CEOs using the CEOANN variable; for firms for which this does not identify the CEO I follow Landier et al. (2013) and use the information on the date the CEO took up the position and left the position (variables BECAMECEO and LEFTOFC) to identify the CEO, if possible. I drop firms whose CEO I cannot identify using the procedure just described, CEOs of firms with more than one CEO (fewer than ten), and CEOs recorded as becoming CEOs more than a year after the CEOANN variable reports them as being CEO of the firm "for all or most" of the fiscal year. I calculate option deltas using the method in Core and Guay (2002). To identify whether the CEO has changed in the years subsequent to index inclusion I use the information in the LEFTOFC (left office of CEO) variable, and remove from this measure CEOs that are reported in the reason variable as having left office because they died. Details regarding the construction of specific variables are provided in the legend of the summary statistics table. **Table 3** presents summary statistics for a variety of Execucomp variables in the baseline period: there are no statistically significant differences except for the share of pay in the form of perks (a 2% difference) and total pay (only significant at the 10% level and to be expected if pay covaries with firm size).

I match the Russell data to the CRSP-Compustat Merged database by historical cusip and year, and hand match firms with stale cusips in the Russell data.[5] I obtain month-end stock

[5] Because Compustat (and Execucomp) data is reported at fiscal year end, and Index reconstitutions occur near the end of June, using the data of firms reporting in the months immediately after the June reconstitution would lead to some firms only having received "treatment" (i.e. inclusion in the Russell 1000) for a few months. Moreover, many accounting variables are slow moving, and often determined at a yearly frequency (such as investment plans). I follow Cuñat et al. (2012) and require firms to have been treated for more than 6 months: firms that report July-December have their first year's values replaced with those of the following year.

prices from CRSP, and quarterly shares outstanding from the Quarterly Compustat database. I winsorize accounting variables that are not naturally top or bottom censored at the 2% level yearly to reduce the influence of outliers - this does not affect the results. Summary statistics for the Compustat-CRSP merged data in the period immediately preceding reconstitution are in **Table 4**. Repurchases as a percentage of outstanding equity appear significantly different across the index cutoff. However, they are not significant in a RDD run with the baseline values as the dependent variable (i.e. controlling for the assignment variable, market capitalization rank).

Acquisitions data is from the SDC Platinum database and consists initially of all mergers and acquisitions with US based acquirers or US based ultimate parents of the acquirers. Following the literature (e.g. Netter et al., 2011) I drop deals with missing announcement dates, deals between firms and their parents, deals that are not completed, deals where the acquirer already held a majority stake before announcing the deal, and deals where final ownership is below 95 percent or where final ownership cannot be inferred from the SDC data. I also drop deals occurring in July and August, as they are very likely to have been in process when the index reconstitution occurred due to the lead times required of a large transaction. For variables that use deal values I use only the sample that contains such values, and I drop deals with values of below USD $1 million or below 1 percent of acquirer assets. **Table 5** presents summary statistics.

Finally, Voting data comes from RiskMetrics. I drop proposals with missing information for pass/fail or vote totals, and keep only Annual Meetings. I match this data to my Russell-CRSP merged data by ticker, month and year. I then match remaining unmatched observations by ticker and year, and hand check all matches to ensure correct matching, dropping mismatched observations. I hand match the remainder of the observations as far as possible. Finally, for firms with more than one annual meeting in a Russell year (that runs from1 July in year t to 30 June in year t+1) I keep the annual meeting that is latest in the year, to ensure the firm has been treated for as long as possible. For the remaining observations and out of an abundance of caution I drop annual meetings that occur in July, August and September, because shareholders of record are likely to be the owners from before index reconstitution rather than the owners that result from the natural experiment considered here. **Table 6** presents summary statistics for these variables within a 100 firm bandwidth, separated by index.

3 Russell Index Construction

The construction of the R1000 and R2000 indexes that provide the natural experiment for this paper depends on firms' equity prices on a single day every year: the last trading day in May. At the market close of that day all firms are ranked by Russell according to their measure of market capitalization. According to Russell, this alone determines index membership. However, Russell considers their market capitalization measure, and the ranking it generates, to be proprietary information which it is unwilling to share.

How then did Russell Indexes compute the market capitalization of index constituents in my sample period? Firstly, Russell only included firms with headquarters in the US, and whose stocks trade at or above $1.00 on May 31. In addition, Russell must have had access to appropriate "documentation" regarding the company, and used FT Interactive data as the primary source for prices and total shares outstanding, although it also used information on share changes reported to the SEC and other unspecified sources "in cases of missing or questionable data" (Russell, 2004). Market capitalization is calculated by multiplying the total number of all outstanding common shares of all classes (but excluding non-common shares such as preferred stock) by the price of the "primary trading vehicle" only, as determined by Russell using a rule based on the relative trading volume, price and free float of the different types of equity securities outstanding.

Once index membership is determined by the firm's rank in terms of Russell market capitalization, Russell then determines index weights by reducing each firm's market capitalization by the number of shares not held to be freely floating according to their proprietary measure (holdings by institutions are not considered to reduce a firm's free float). This adjustment takes place *within* each index. Thus, firms in the Russell 1000 ranked around 800 but with a low free float could have a weight equivalent to that of a firm ranked 900 or lower by market capitalization. This means that firms near the bottom of the R1000 will include firms with much higher market capitalization – but low free float – than firms near the top of the R2000, while firms at the top of the R2000 will have both the highest market capitalizations in the R2000, and among the highest free floats. As a result, a regression discontinuity using *firm rank by index weights* as the assignment variable will generate spurious results because the firms on either side of the cutoff are substantially different in terms of – at a minimum – market capitalization and free float as determined by Russell, and as such are invalid controls for each other.

Finally, actual reconstitution, as opposed to the determination of index membership, occurs on the last Friday in June. However, changes in the constituents are announced before then, and "subject to change if any corporate activity occurs or if any new information is received prior to release" (Russell, 2004). The fact that index weight ranks are of no use for a RDD leaves the researcher with two alternatives: attempt to reconstruct the information used by Russell at the time, or to obtain the information directly from Russell themselves. I was unable to reconstruct Russell's proprietary market capitalization ranking adequately using CRSP and Compustat data, which is relatively unsurprising given that Russell appears to purposely make it hard to predict which firms will be included at the margin, so as to protect index customers from trading that seeks to exploit their relatively inelastic demand on the index reconstitution date.[6]

However, Russell Indexes consented to provide me with a slightly noisy version of their market capitalization variable from 2002 onwards – they did not retain the information before then. Unfortunately, Russell Indexes changed their index construction method starting in 2007, and implemented a banding procedure based on the market capitalization of the 1000th largest

[6] For the purposes of the natural experiment considered here, the non-public nature of Russell's measure, and the difficulty of replicating it accurately, reduces the ability of firms to manipulate their relative ranking precisely, bolstering the validity of the RDD

firm. This procedure was designed to eliminate index changes for firms whose market capitalization is little different to that of the cutoff firm, which is precisely the variation I exploit to identify my estimates. As a result the number of firms moving from the R2000 to the R1000 in 2007 drops to around 20, down from 90 the previous year, and market capitalization is no longer smooth across the 1000 rank cutoff. This change greatly reduces the power of the natural experiment, so for this reason my sample ends in 2006.

The proprietary measure of market capitalization provided to me by Russell consists of the previous year's portfolio's market capitalization, updated to May 31 of the current year. This is a good approximation of the final variable, but not a perfect fit because (i) it does not include all firms that list during the year (ii) the primary equity vehicle which determines price may have changed over the year (iii) the firm may no longer be eligible to be included in next year's index, (iv) new information may have become available during June, or existing information may have been deemed questionable.

Figure 1 displays a plot of the underlying market capitalization variable relative to the Russell rank, and the smoothness across the threshold supports the validity of the RD design. However, because the market capitalization variable I obtain from Russell Indexes is not the exact variable used for assignment, but one measured with what is likely random error, I implement a Fuzzy RD design, instrumenting index inclusion with an indicator that equals one when firm market capitalization rank is 1000 or below. The first stage of this fuzzy RD generates an adjusted R2 of 0.83 and a coefficient on the instrument of 0.72 (t-stat of 14.9) – a coefficient of 1 would indicate a sharp RD – and is thus a strong instrument, allaying potential concerns regarding weak instruments.

Figure 2 illustrates how the probability of treatment does not track the assignment variable precisely. The x axis is firm rank by Russell market capitalization centered on the cutoff at zero. To the left of zero firms are in the R1000; to the right they are in the R2000. The Y axis is the probability that a firm will be treated, and the dots are bin averages for 4 ranks. Instead of a sharp step function, there are ranks of firms for which the probability of treatment is not zero or one, but somewhere in between. The next section provides details of the methodology.

4 Methodology

I estimate the causal effect of institutional ownership on firm outcomes using a regression discontinuity design (RDD). The intuition behind the RDD is as follows. Firms in a narrow bandwidth around the R1000 – R2000 boundary are quasi-randomly assigned to one of the two indices, and index tracking behavior by institutional owners generates plausibly exogenous variation in total institutional ownership across the boundary in response to this assignment.

Importantly, in a narrow bandwidth around the index cutoff, whether a firm is placed in the R1000 or R2000 is largely random, based on small price shocks on the day(s) immediately preceding reconstitution, and also depends on the shocks to surrounding firms. This means that firms on one side of the cutoff are well suited to be controls for firms on the other side, as the only dimension along which they differ systematically is the Russell market capitalization ranking. Because the indices are associated with different populations of institutional investors with different quantities of assets under management, this generates a difference in total institutional ownership across the index boundary. In turn this may have effects on firm outcome variables subject to monitoring by shareholders. The difference in these outcome variables between firms in the R1000 and R2000 is what the RDD measures.

The RDD's suitability for causal inference derives from the relatively mild assumptions it requires. RDDs rely on a key assumption of imprecise control (Lee and Lemieux, 2010): that companies cannot precisely predict or control their Russell market capitalization rank, and thus cannot choose to be in the R1000 or R2000. This assumption implies that, in the absence of the index reconstitution the outcomes of firms just below the cutoff and those above would have been similar, so the only reason that the actual outcomes are different is that after the index reconstitution some firms are in the R1000 and thus will have shareholders that are benchmarked to the R1000, while other firms will be in the R2000 and have the corresponding shareholders.[7]

The assumption of imprecise control over firms' Russell market capitalization rank implies firms are randomly assigned to treatment, here defined as membership in the Russell 1000. Unlike an instrument variable's exclusion restriction, this assumption has three main testable implications: 1) Observed pre-determined characteristics should be identically distributed on either side of the index cutoff; 2) the density of firms on either side of the threshold should be the same; 3) RDD estimates estimate of should not vary greatly when we include baseline covariates, as these are not required for consistent estimation of the treatment effect. I provide evidence for all three in the following section.

I implement the fuzzy RDD following the standard procedure using two stage least squares. I pool all firm-year observations, including year fixed effects, and estimate the following regression:

$$Idx_{it} = \gamma_{0r} + \gamma_{1r} Mktcap\ rank_{it} + T_{it}[\gamma_{0l} + \gamma_{1l} Mktcap\ rank_{it}] + \delta_t + \epsilon_{it}$$

$$Y_{it} = \alpha_{0r} + \alpha_{1r} Mktcap\ rank_{it} + Idx_{it}[\alpha_{0l} + \alpha_{1l} Mktcap\ rank_{it}] + \delta_t + u_{it}$$

Where i indexes firms, and t indexes years. Equation 1 is the first stage: Idx is an indicator equal to one if the firm is placed in the R1000 index after reconstitution; T is an indicator equal to one if the firm's market capitalization exceeds that of the 1000th ranked firm, and is used as an instrument for Idx. Mktcap rank is the firm's Russell market capitalization rank – the assignment variable – minus 1000 to center the data on the cutoff. Subscripts r and l indicate coefficients estimated on data exclusively to the right and to the left respectively:

[7] Fuzzy RDD also requires two further assumptions (see Hahn et al., 2001). Firstly, monotonicity (i.e. crossing the cutoff cannot cause some units to take up the treatment and others to reject it) which is clearly satisfied by the Russell index procedure. Secondly, excludability (i.e. crossing the cutoff cannot impact the outcome variable other than through impacting receipt of treatment). This is likely true for most corporate finance variables in the narrow range of values of the assignment variable considered by the main specification used in this paper. However, it may pose a problem, at least formally, for dependent variables such as leverage, security issuance, and market-to-book. These are not the focus of this paper.

the regressions are estimated by triangle kernel weighted OLS separately on each side of the cutoff. Equation 2 is the second stage, and delta is a vector of year fixed effects. The above specification is estimated on a relatively small bandwidth of 100 observations to either side of the cutoff, and corresponds to local linear regression fuzzy RD.

Using a wider bandwidth provides additional statistical power at the cost of introducing greater bias because the RDD's randomization result is local: as one moves away from the cutoff it becomes increasingly less true that the firms on either side are similar ex ante. An alternative is to use a wider bandwidth and to control for increasing heterogeneity across the boundary using a flexible polynomial function. However, in their benchmarking of the RDD against experimental data, Black et al. (2007) report that local linear regressions have lower bias and less specification-sensitivity than polynomial regressions. Accordingly, the local linear regression above is the preferred specification throughout. The bandwidth choice of 100 firms on either side was chosen for simplicity and comparability of the sample across estimates, but in the robustness section of the paper I show estimates are generally robust to both smaller and larger bandwidths (as well as polynomial functions with very large bandwidths). Moreover, I provide an estimate using Imbens and Kalyanaraman's (2012) optimal bandwidth, constrained to a maximum of 200, and show that in general the algorithm-selected bandwidth is close to 100.

The above specification includes year fixed effects. While fixed effects (of any type) are not required for consistent inference in the RDD, they mitigate concerns that certain years may be different from other. In robustness checks, I show that I obtain similar results if I include SIC division fixed effects to capture unobserved heterogeneity at the industry level, or if I exclude year fixed effects. I also include a variety of covariates, including the change in the market capitalization and in the Russell market capitalization rank of firms over the preceding year. It is worth recalling, however, that a valid RDD with a local linear specification and a small bandwidth – my main specification – does not require the inclusion of covariates beyond the assignment variable for identification or consistency, and is not subject to omitted variable biases. To account for any potential within-firm dependence over time, I cluster standard errors by firm, although results are robust, and generally more statistically significant, when Huber–White standard errors are used, as is standard for pooled RDDs.

Lastly, I estimate the local linear regression above using a triangular kernel. This kernel has been shown to be optimal in estimating such regressions at boundaries (Fan and Gijbels, 1996). The triangular kernel weights observations nearer the threshold more than those further away – intuitively, the triangular kernel gives more weight to firms whose index placement is more likely to be random. Results are robust to using a uniform kernel in narrow bandwidths.

4.1 Tests for Quasi-Randomized Assignment

My identification strategy relies on random assignment to the "treatment" (i.e., being placed in the R1000). As mentioned in Section 3.2, this assumption has testable implications, akin to the tests of effective randomization in experimental data.

The first testable implication is that the distribution of the assignment variable should not exhibit any bunching around the discontinuity, as this constitutes prima facie evidence that firms can manipulate their value of the assignment variable, suggesting a violation of the key assumption of imprecise control. While the assignment variable in this case is a ranking, and so by construction is uniformly distributed over its range, I perform the McCrary (2008) test for discontinuities of the density of the underlying variable: Russell market capitalization. This test is run year by year as the distribution of market capitalizations shifts up every year in my sample. The absolute value of the average yearly t statistic for the McCrary test of discontinuity in the density of the log Russell market capitalization for a bandwidth of 500 firms is 0.59; for a bandwidth of 100 firms it is 0.81; none are statistically significantly different from zero at any conventional level, and the coefficients for the discontinuity fluctuate between positive and negative. In short, I am unable to reject the null of continuity of the density function at the threshold, suggesting firms are not manipulating their market capitalizations to ensure they are on a specific side of the Russell Index threshold.

The second testable implication is that firms to the left and to the right of the cutoff should be similar on the basis of ex ante characteristics. If they differ, then the treatment would not appear to be randomized and we would infer that companies are able to predict the election outcome and sort themselves accordingly. In the Data section I present summary statistics for a series of covariates in the baseline period immediately preceding reconstitution. The difference-in-means test provided in the last column of each summary statistics table confirms that the average difference in each characteristic in the bandwidth is statistically insignificant. In untabulated regressions, I further estimate the RDD with each baseline covariate as the dependent variable, and confirm the result that there do not appear to be systematic differences across the threshold before reconstitution, aside from those that are to be expected as a result of how the indices are constructed.

A third testable implication of random assignment to treatment is the relative invariability of estimates to the inclusion of baseline values of covariates and fixed effects. If the RD is valid, covariates beyond the assignment variable (and functions thereof) are not required for identification or consistency, and serve simply to reduce sampling variability, especially with a local linear specification in a narrow bandwidth. Thus they should not change the value of the coefficient materially on average, although some fixed effects could reduce the available variation to such an extent that little remains for estimation. In the Robustness section I show this is the case for the main results of the paper by including a battery of control variables and industry fixed effects[8].

5 Results

Tables 7.1 to 7.3 document that index inclusion leads to significant changes in firms' institutional shareholder base. The coefficients displayed are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between

[8] In unreported results I re-estimate the RDD for a number of placebo thresholds instead of the 1000 rank; no discrete jumps are observed.

the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.

In Table 7.1 we see that firms assigned to the Russell 1000 index (composed of larger firms) see their institutional ownership (IO) jump up by approximately 10% of firm equity (equivalent to between $150-$200 million) at the end of June, immediately following reconstitution. This provides evidence that there is clear natural experiment for the effects of institutional ownership on firm behavior associated with this index reconstitution threshold. This change is driven by firms moving up from the R2000 into the R1000, as is clear from the second panel, and is asymmetrical: firms moving down do not experience a statistically significant reduction in institutional ownership, as can be seen in the third panel, suggesting that offsetting movements from non-index funds dampen (amplify) the effect on institutional ownership when a firm is just excluded (included) from the index. In unreported results I show that the number of institutions owning the firms' equity does not differ across the cutoff.

Table 7.1 also shows that the ownership stake of the top 10 institutions by ownership in the sample firm also rises discontinuously on index inclusion. In particular, for the "up sample" (i.e. the second panel, composed of firms moving up and firms staying in the R2000) the increase is of approximately 7-10% of firm equity. This suggests a potential complementarity between index funds and other institutional owner types more likely to monitor, because the largest institutional owners have the greatest incentive to perform active monitoring. In the RD graphs section I present graphs of the RD estimate, along with a scatterplot of the data averaged over 10 or 15 rank positions to give an idea of the shape of the underlying data. The X axis has the assignment variable, while the Y axis presents the dependent variable. The estimated coefficients correspond to the difference between the estimated functions to the left and to the right at the index cutoff.

Table 7.2 presents the RD estimates for the top 10 institutional owners identified as "Indexers." These are institutions classified as Quasi-Indexers by Bushee's algorithm (i.e. high diversification and low turnover) and that are classified as an "Investment Company" or "Independent Investment Advisors" using the type code variable from Thomson Reuters' 13F data, as extended on Brian Bushee's website. While this is a noisy measure of true indexing if closet indexing is widespread, it captures the pure index funds (which tend to be very large, and hence in the top ten institutionals by category), as well as supposedly active mutual funds that actually have low portfolio turnover. The pattern of ownership change across the threshold displayed here is similar to that of both total IO and IO of the top 10 institutions in Table 7.1, suggesting that quasi-index funds are, at a minimum, partially driving the ownership change. Moreover, the noisiness of the measure likely explains why we do not see a statistically significant reduction in indexer IO for the down sample. For completeness I also include the RD estimated of ownership of the top 10 quasi-index institutional owners, which also display a similar pattern. Table 7.3 displays the RD estimates for the top 10 institutions in Bushee's other two institutional owner categories: Focused Investors (low diversification, low turnover) and Transient Investors (high diversification, high turnover). Focused investors (as well as transient investors) appear to increase in both the overall sample and in the up sample, suggesting that investors that are likely active monitors comove with index funds into the firm's shareholder base.

A potential concern might be that institutions, by increasing their ownership after index reconstitution, displace large owners or blockholders that were active monitors of the firm, producing an overall reduction in monitoring, and changing the interpretation of the results. Using a dataset of blockholder ownership collected by hand from proxy filings **Table 8** shows that there appear to be is no difference in total blockholdings after reconstitution across the threshold, nor are there significant differences in insider (i.e. blockholder is an officer or director), outsider, institutional or non-institutional blockholdings. There is some evidence that the number of blockholdings is 0.77 higher on average in the R1000 relative to the R2000 after reconstitution (and within the bandwidth), and in general the coefficients on blockholdings, both in terms of numbers of blocks and of votes controlled indicating that, if anything, there might be an increase in blockholder voting power or numbers on entering the R1000, although this increase is not statistically significant. The fact that blockholdings are not reduced (and may even increase) after reconstitution implies that the increase in institutional ownership pushes out retail investors, the residual category. Given that retail investors are the group least likely to exert monitoring effort, it seems likely that total monitoring rises when such investors are replaced by institutions, particularly when much of the increase in IO is concentrated in the top 5 institutional owners, as is shown in Tables 7.2 and 7.3.

Table 9 displays results for CEO pay in the year following reconstitution. Compensation also displays material differences across the threshold, driven by firms moving up into the R1000. For such firms CEOs' option pay share is higher by 17% of total pay, which is offset by a salary pay share that is 11% lower, a restricted stock pay share that is 9% lower, and a long term incentive pay share that is 9% lower. However, this different weighting of the components of CEO pay does not result in significantly higher total pay overall for CEOs of firms in the R1000 relative to the R2000. Moreover, it can be argued that the new composition of CEO pay is unfavorable to the CEO, which if true would suggest active monitoring and reduced CEO power when faced with an altered shareholder population. The first two columns show the probability that a firm has a new CEO within one and two years of reconstitution. There is no statistically significant difference in the one year probability, but the two year probability is 18% higher for firms in the R1000 relative to firms in the R2000, suggestive of a potentially stronger performance-turnover link in firms with higher institutional ownership.

Turning to firm outcomes in **Table 10**, I examine a variety of corporate accounting variables that the literature has argued may change in response to increased monitoring. The central result is that capital expenditure as a percent of assets is over 2 percent lower for firms just included in the R1000 over the two years following reconstitution. R&D as a percentage of sales is 13 percent higher (23 percent for firms moving up into the R1000) but, perhaps because of the relatively few firms reporting non-missing values for this variable, this is not significant at conventional levels: the p value is 0.11 (0.14). Leverage measures, profitability, market-to-book, and payout (both in terms of repurchases and of dividends) do not differ significantly across the threshold.

Acquisitions behavior is different across the threshold, as is evident in **Table 11**. Firms just included in the R1000 make 0.5 fewer cash deals than firms just in the R2000, and these tend to be of mainly private targets. Moreover, firms just in the R1000 make approximately 0.5 fewer diversifying acquisitions, and fewer large diversifying acquisitions in relation to the acquirer's size.

To make some progress on the channels through which these institutional investors affect governance, I examine shareholder voting behavior at annual meetings in **Table 12** to determine whether one of the central and observable components of shareholder monitoring – voting at annual meetings – is affected by this change to IO. The pass rate for management proposals is lower by approximately 3 percent for firms just in the R1000 relative to firms just included in the R2000, and the number of failed management proposals increases by 0.06 per meeting on average.

This is a striking result given management's structural control of shareholder meetings, reflected in the over 95 percent pass rate and the fact that management almost always wins close votes (Listokin, 2008). While the number of both management and shareholder proposals is no different across the cutoff the number of failed shareholder proposals falls by 0.22 per meeting, on average, over the two years following reconstitution. In short, management seems to face a less pliable shareholder population when institutional ownership rises. While this is a natural place to seek effects of IO, institutions often exert the greatest monitoring influence through private meetings with management rather than through voting (see for example Carleton, Nelson, and Weisbach, 1998, and Becht, Franks, Mayer, and Rossi, 2009), so these results might understate the actual changes in governance taking place.

6 Robustness

To evaluate the robustness of the main results, in **Tables 13 and 14** I present a variety of specifications for the main results on CEO Pay and Accounting variables. The first column is the main specification from the preceding tables. Columns 2 to 4 are described in detail below. Column 5 removes year fixed effects to evaluate the sensitivity of results to their inclusion. Column 6 winsorizes the data at 10% for both tails to evaluate the dependence of the results on outliers. Columns 7 and 8 present smaller and larger bandwidths for our main local linear specification. Column 9 uses a large bandwidth and a flexible polynomial - an alternative specification that is often used when power is limited. Finally, column 10 presents the main local linear specification using the Imbens and Kalyanaraman (2012) optimal bandwidth. Results appear robust to these permutations of the sample and or specification.

As noted earlier, my main RDD specification is locally linear regression, which controls for the assignment variable linearly. However, one might be concerned that this specification does not adequately capture the difference between firms on either side of the threshold, because it is a level variable that does not incorporate each firm's movement over the past year in terms of market capitalization – for example, firms that are rising quickly through the rankings, or falling rapidly. One could be concerned that it is this dynamic aspect of the firm's market capitalization that drives the results rather than the change in institutional ownership. To allay this concern I run the RD for the main results including as controls both the change in Russell's relative ranking over the past year, and the change in the value of market capitalization. Coefficients and significance levels are unaffected.

Relatedly, it is possible that the randomization carried out by the RDD may be imperfect, as occasionally occurs with randomized experiments that fail to stratify their sample on enough dimensions. As a result, differences in covariates between firms on either side of the threshold might be driving the results. This is of particular concern for the results on CEO pay, which is known to covary strongly with size and, to a lesser extent, tenure and other firm and industry level variables. To assuage this concern I re-run the RD for the main results controlling for a battery of variables that conceivably co-move with the dependent variable. For example, in Table 12 for the main CEO pay results I include controls for CEO age, CEO tenure, firm profits, sales, total assets, market value of the firm, sales growth, asset growth, market value growth, and number of employees. Coefficients and significance levels are unaffected, supporting effective randomization and validity of the RDD, as is also true when I include industry fixed effects.

7 Conclusion

I use exogenous variation in institutional ownership to examine the dimensions through which institutional investors in general and index investors in particular affect corporate behavior. Institutions are by far the largest group in the US public shareholder base; index funds control approximately a quarter of US institutional assets under management, while the volume of assets managed by quasi-indexers makes passive owners a majority of the shareholder population of many public firms. Institutions - at least of some types - are widely believed to be active monitors, and they are better equipped to monitor the firms they invest in than the retail investors they have progressively replaced. The degree to which this monitoring occurs via voice – engagement with the firm either privately or by voting or via exit from the position is still an open question, but the existing evidence suggests both are important.

This potential for such different types of monitoring makes empirical inference with regard to the effects of institutions on firms difficult, as does the fact that institutions choose which firms to invest in, potentially matching to certain types of firms. The type of the firm in turn may be correlated with the outcome variables, rendering invalid any conclusions about the independent effect of IO on firm outcomes. By focusing on information-free changes to an equity benchmark I address these problems. Index funds have no discretion to choose which firms to invest in and no ability to exit the shareholder base instead of exercising voice. This allows clean estimation of the effects of index funds on firm governance and outcomes. I also show evidence of an apparent complementarity between explicit index funds and other institutional investor types, as the latter accompany index funds into firms that are just included in the index relative to firms that are just excluded.

The theoretical literature is not very specific about, nor in agreement regarding the predicted direction of all of the effects considered here. Empire building and entrenchment theories, for example, would suggest that increased monitoring would result in reduced capital expenditures and acquisitions, as I report in this paper, in contrast to the predictions of the quiet life theory. Increased sensitivity of CEO turnover to performance could also induce less risk taking on the part of the CEO, leading to potentially reduced (diversifying) acquisition activity, as I find in my data. Changes to CEOs' compensation are likely to have important effects on firm risk taking, and a stream of literature exists that focuses on this relationship (for example see Gormley et al., 2013). However, given the yearly frequency of index reconstitution the horizon that I am able to examine is too short for the flow effects of changes in compensation structure to outweigh the stock of accumulated incentives.

This paper shows that institutions that closely follow an equity index appear to have large causal effects. Given the increasing popularity of indexing, exploring these effects further, especially their value implications, is an important question for future research.

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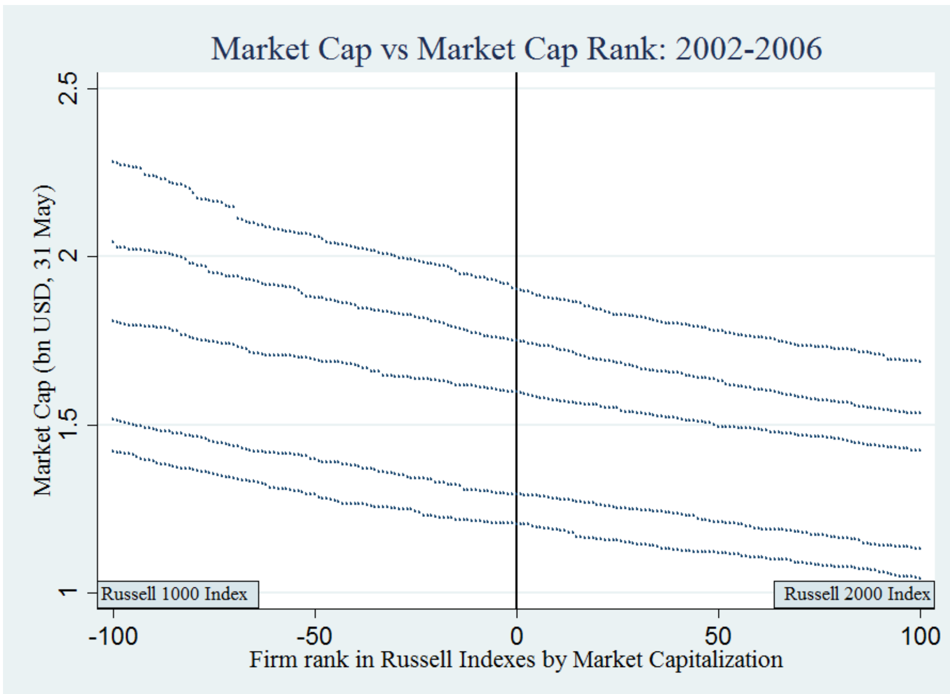
Stein, J. C. (1989). Efficient capital markets, inefficient firms: A model of myopic corporate behavior. The Quarterly Journal of Economics, 104(4), 655-669.

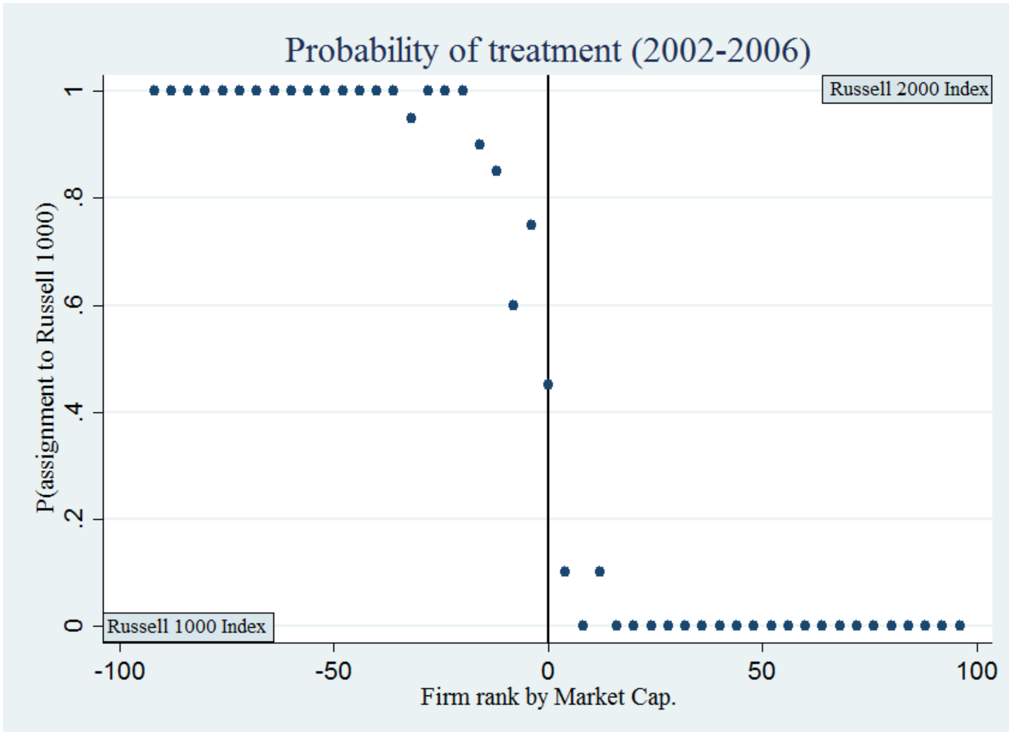
9 Figures and Tables

Figure 1



This Figure presents the assignment variable for the RDD - firms' rank according to a version of Russell's proprietary market capitalization measure - on the X axis, and the market capitalization measure itself on the Y axis. Note that firm rank is here centered on the cutoff between the Russell 1000 and Russell 2000 indices: firms to the left of the line are in the R1000, and firms to the right are in the R2000. A rank of 50 in the graph is equivalent to an uncentered ranking of 1000+50=1050, while a rank of -50 is equivalent to an uncentered rank of 1000-50=950 i.e. the 950th largest firm by market capitalization. Each dot represents a firm, and each line is a year in my sample. The lowest line is 2002, followed in order by each year until the top line, which is 2006. Market capitalization is as of 31 May of each year. The crossing points indicate the market capitalization cutoffs between the indices each year, and range from approximately USD $1.2 to $1.9 billion. The smoothness of each line as it crosses the threshold at zero (equivalent to an uncentered rank of 1000) is crucial to the RD design, thus this graph contributes prima facie evidence in favor of the validity of the RDD considered here.

Figure 2

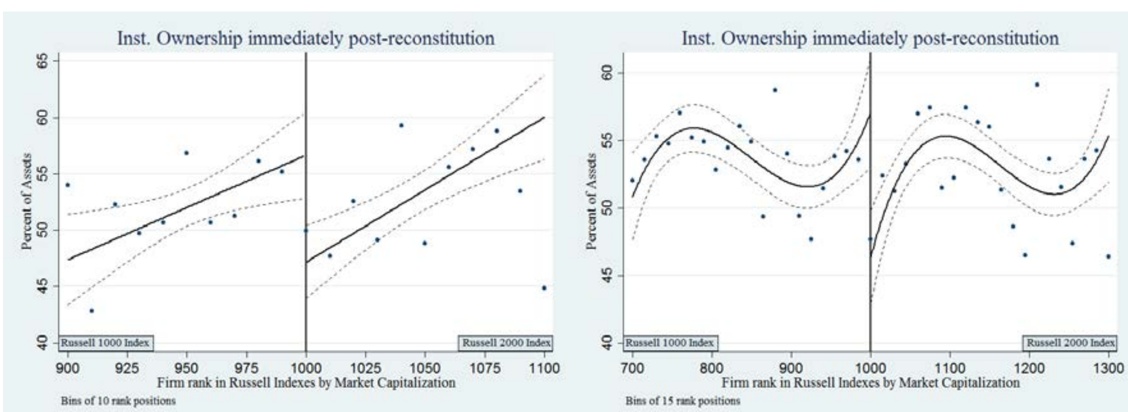


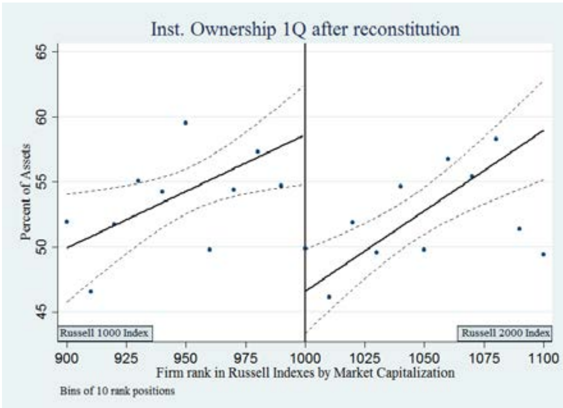
The Y axis displays the probability of a firm being "treated" - that is, assigned to the Russell 1000 equity index - as a function of its market capitalization rank, plotted on the X axis. The X axis is centered on the cutoff between the Russell 1000 (to the left) and Russell 2000 (to the right) indices: the cutoff is labelled 0. On this axis a firm rank of 50 signifies a firm whose rank is ranked 50 positions greater than the cutoff i.e. it is smaller than the cutoff rank in terms of market capitalization. Each dot represents the average probability of 4 rank positions over 5 years, i.e. 20 firms. The graph is not a step function, indicating that a sharp RD design would be biased downwards in this setting, but is close enough to a step function to indicate that the data is well suited to a fuzzy RD, whereby the jump in probability of treatment is used to identify the treatment effect, rather than just treatment status itself.

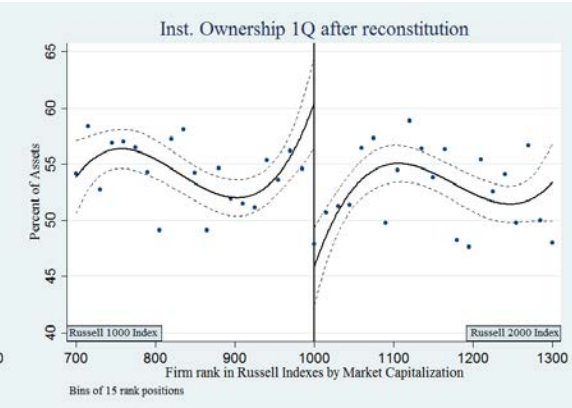
RD graphs

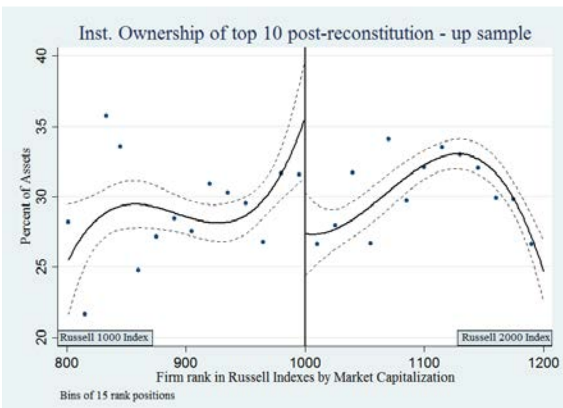
The graphs in this section present the main results of the RDD analysis graphically. The X axis presents the assignment variable - Russell's market capitalization rank - in a narrow window centered on the index cutoff, which is placed at rank 1000. To the left of the cutoff firms are in the larger Russell 1000 index of the 1000 largest firms by market capitalization; firms to the right of the cutoff are in the Russell 2000. Two specifications are estimated. The graphs on the left are the main specification (locally linear regression in a 100 firm bandwidth on either side of the cutoff, estimated with a triangle kernel). In general the graphs on the right have a cubic polynomial over a larger bandwidth (200 to 800 firms) to give an impression of the shape of the data. For clarity regarding the ultimate data no year fixed effects are included in either specification. The lines are estimated using the full underlying data, and there is also a superimposed scatterplot of the data's average value in bins of the assignment variable (bins are either 10 or 15 rank positions in size). The dashed lines represent one standard error bounds. It is also worth highlighting that for graphs using the up sample (CEO compensation) there are relatively few firms far to the left of the graph (because few firms move 200 positions beyond the cutoff in a single year), making the function much less informative about the average position as one moves further left.

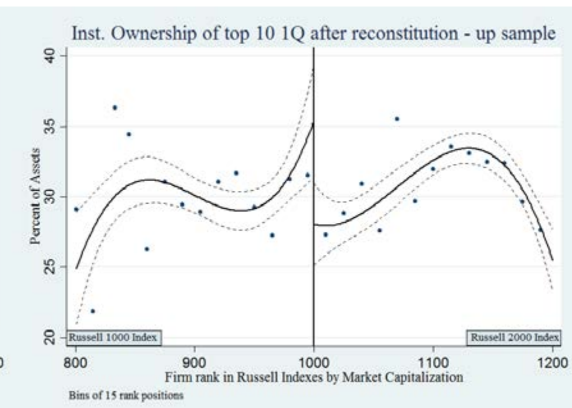
RD graphs for Institutional ownership




Inst. Ownership 1Q after reconstitution
Percent of Assets
Russell 1000 Index
Russell 2000 Index
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions

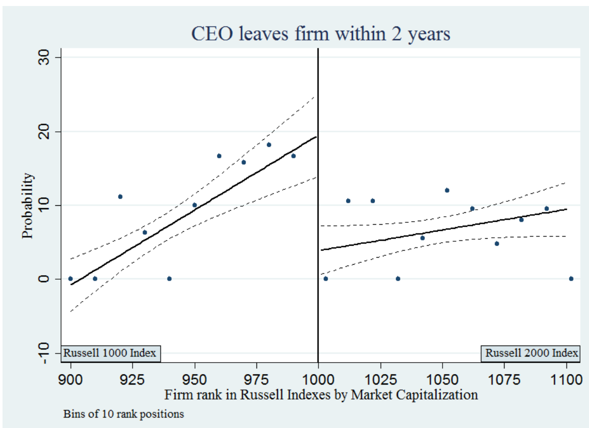

Inst. Ownership 1Q after reconstitution
Percent of Assets
Russell 1000 Index
Russell 2000 Index
Firm rank in Russell Indexes by Market Capitalization
Bins of 15 rank positions

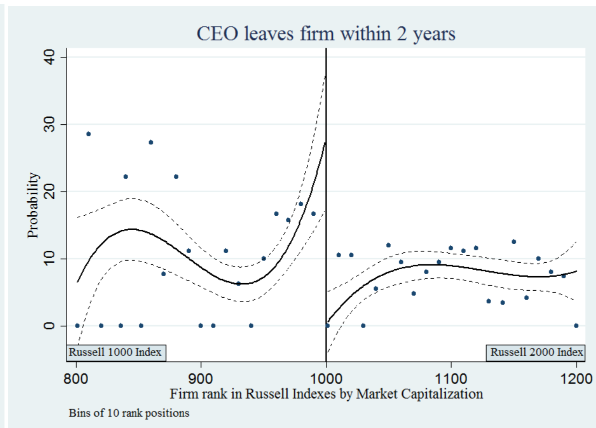

Inst. Ownership of top 10 post-reconstitution - up sample
Percent of Assets
Russell 1000 Index
Russell 2000 Index
Firm rank in Russell Indexes by Market Capitalization
Bins of 15 rank positions

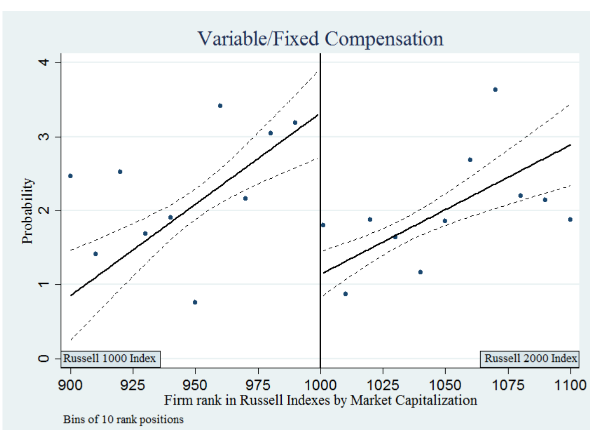

Inst. Ownership of top 10 1Q after reconstitution - up sample
Percent of Assets
Russell 1000 Index
Russell 2000 Index
Firm rank in Russell Indexes by Market Capitalization
Bins of 15 rank positions

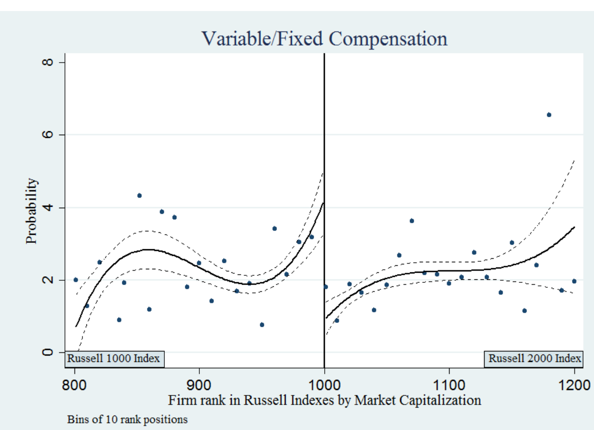
RD graphs for CEO Compensation

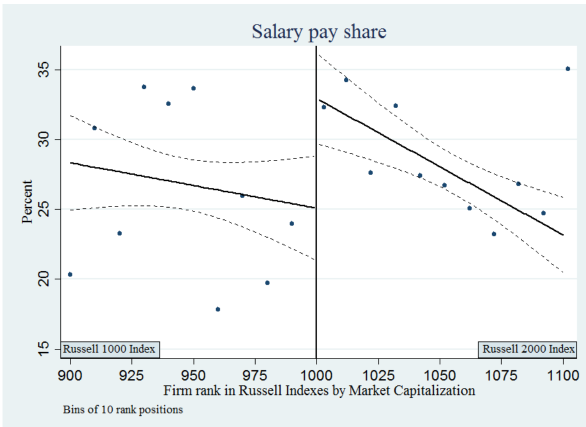
All CEO compensation graphs shown here are for the up sample: i.e. they compare firms moving up into the Russell 1000 index to firms staying in the Russell 2000.

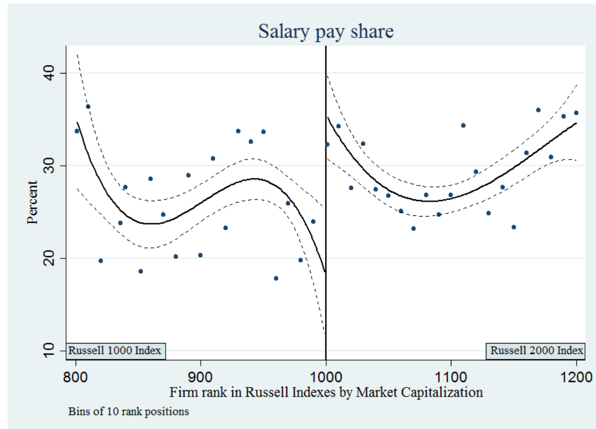


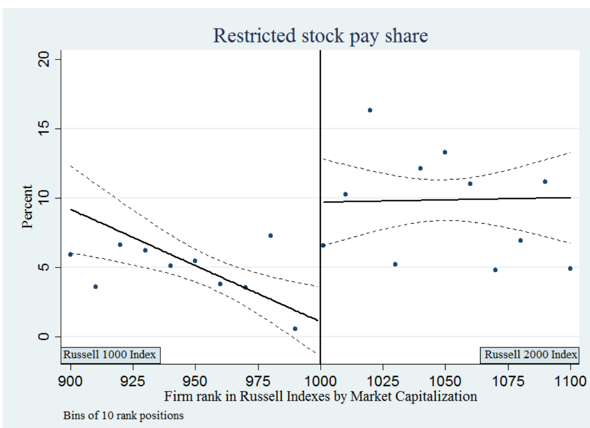


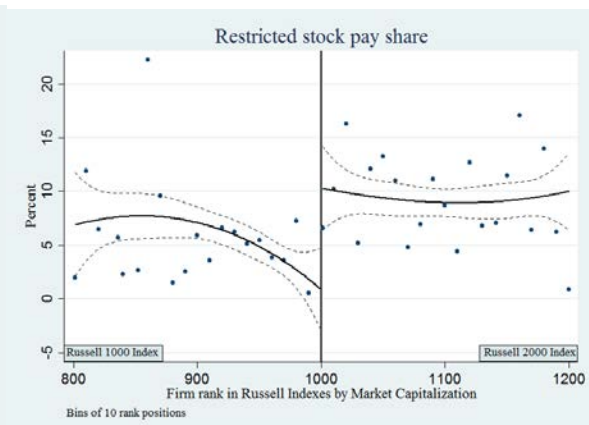


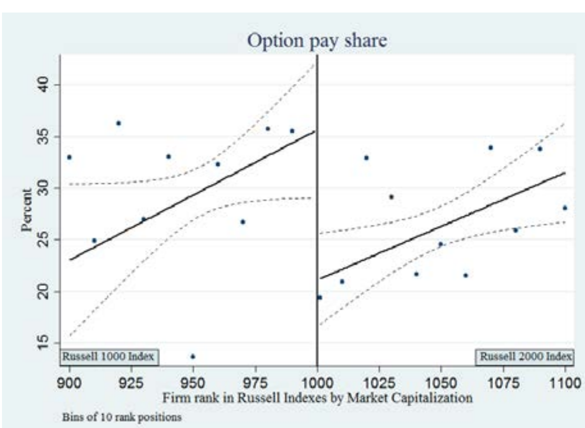


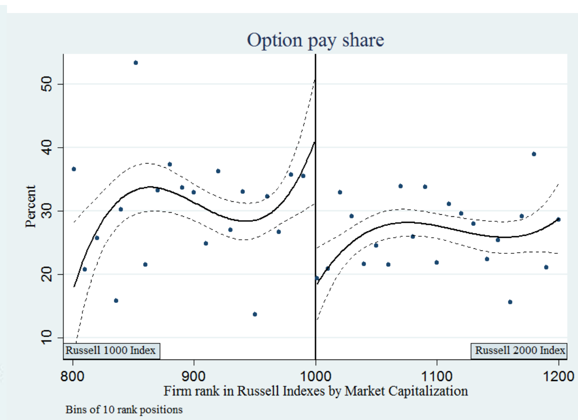

Salary pay share
Percent
15
20
25
30
35
Russell 1000 Index
Russell 2000 Index
900
925
950
975
1000
1025
1050
1075
1100
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions

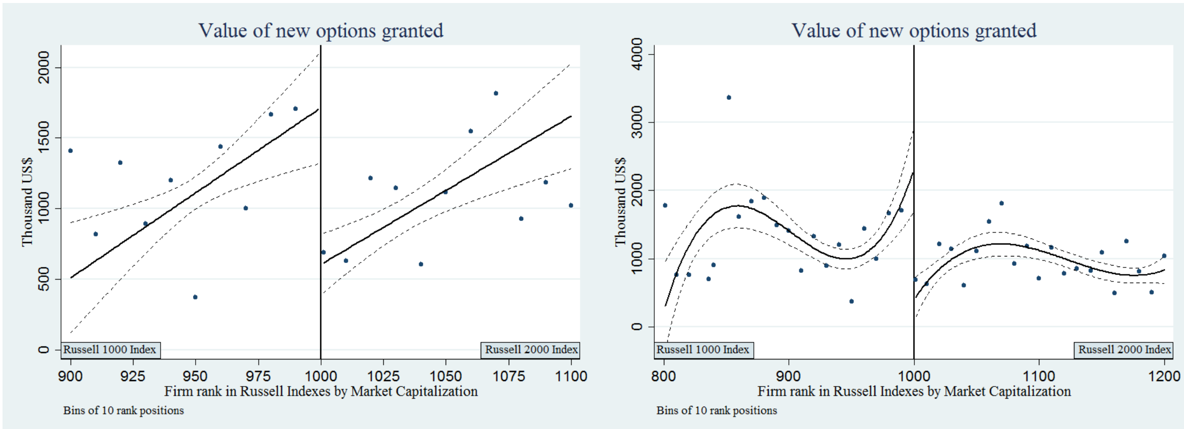

Salary pay share
Percent
10
20
30
40
Russell 1000 Index
Russell 2000 Index
800
900
1000
1100
1200
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions

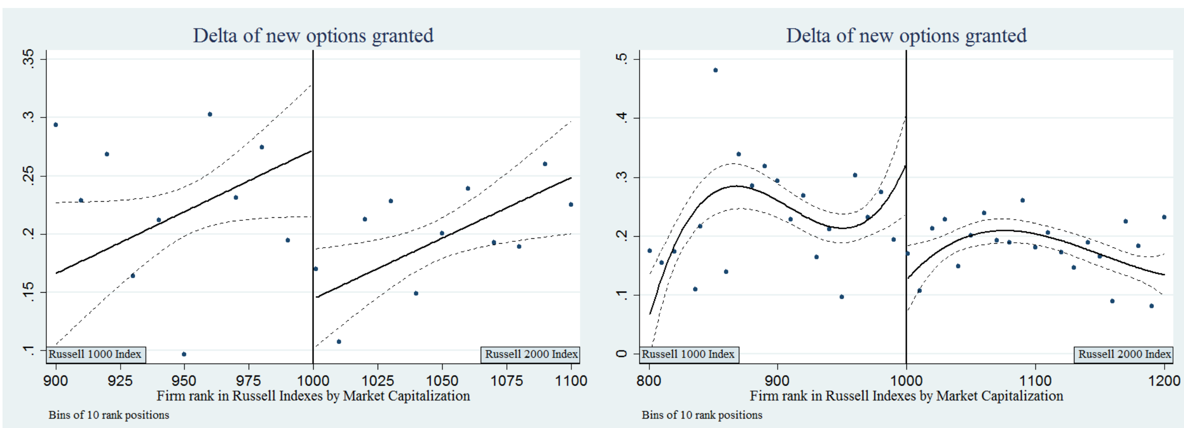

Restricted stock pay share
Percent
0
5
10
15
20
Russell 1000 Index
Russell 2000 Index
900
925
950
975
1000
1025
1050
1075
1100
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions


Restricted stock pay share
Percent
-5
0
5
10
15
20
Russell 1000 Index
Russell 2000 Index
800
900
1000
1100
1200
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions

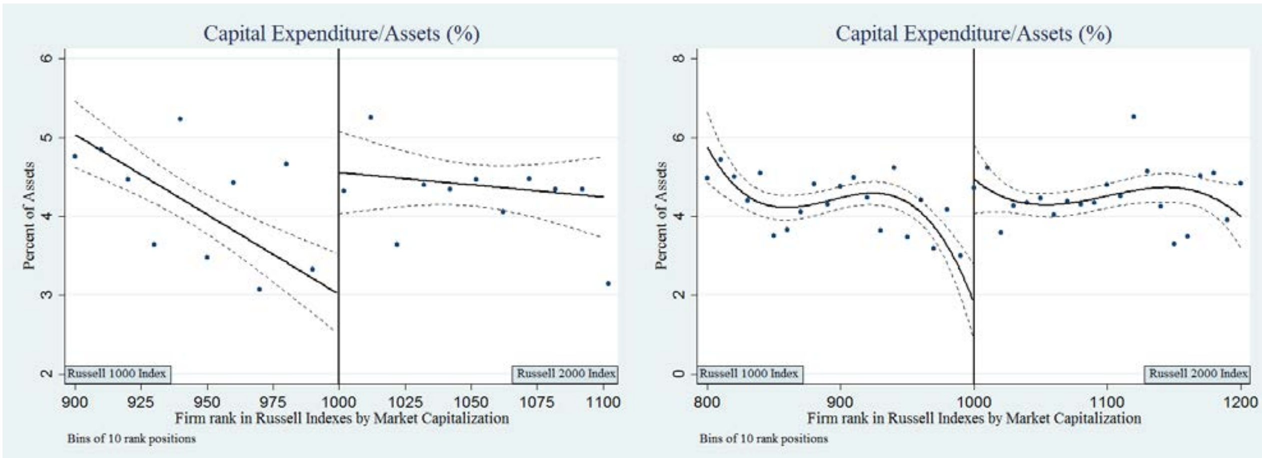

Option pay share
Percent
15
20
25
30
35
40
Russell 1000 Index
Russell 2000 Index
900
925
950
975
1000
1025
1050
1075
1100
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions


Option pay share
Percent
10
20
30
40
50
Russell 1000 Index
Russell 2000 Index
800
900
1000
1100
1200
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions

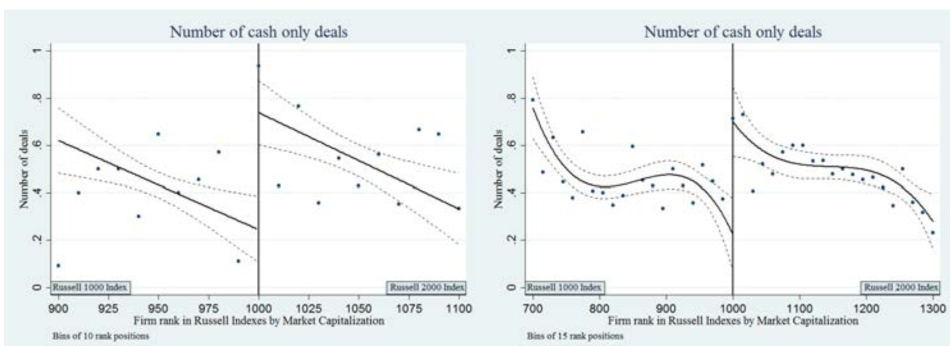

Value of new options granted
Thousand US$
Russell 1000 Index
Russell 2000 Index
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions
Value of new options granted
Thousand US$
Russell 1000 Index
Russell 2000 Index
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions

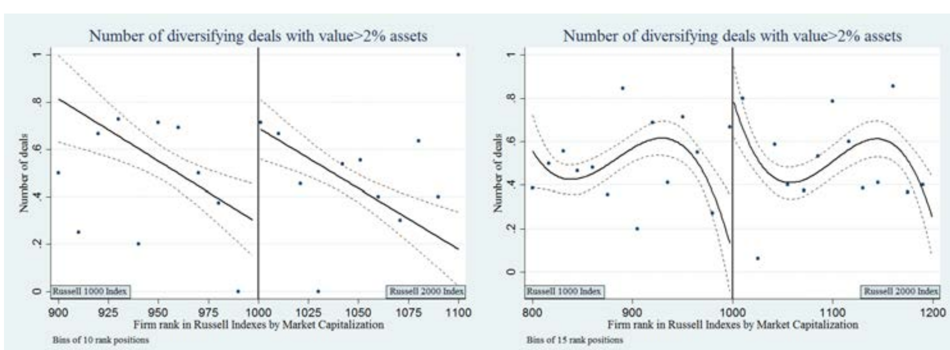

Delta of new options granted
Russell 1000 Index
Russell 2000 Index
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions
Delta of new options granted
Russell 1000 Index
Russell 2000 Index
Firm rank in Russell Indexes by Market Capitalization
Bins of 10 rank positions

RD graphs for Capital Expenditures

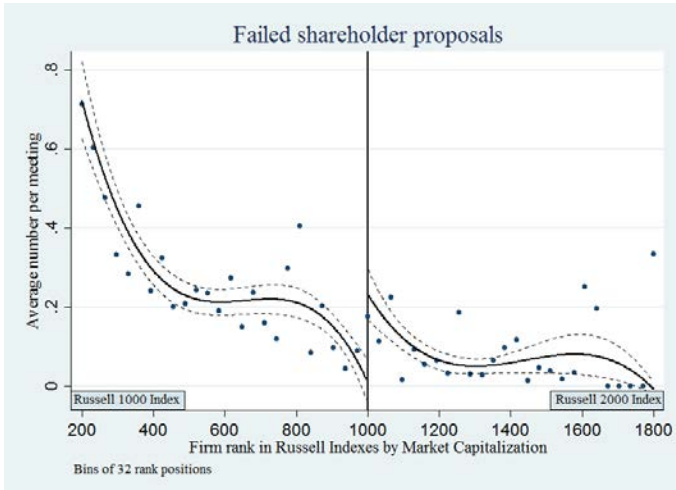


RD graphs for Mergers and Acquisitions





RD graphs for Shareholder Voting



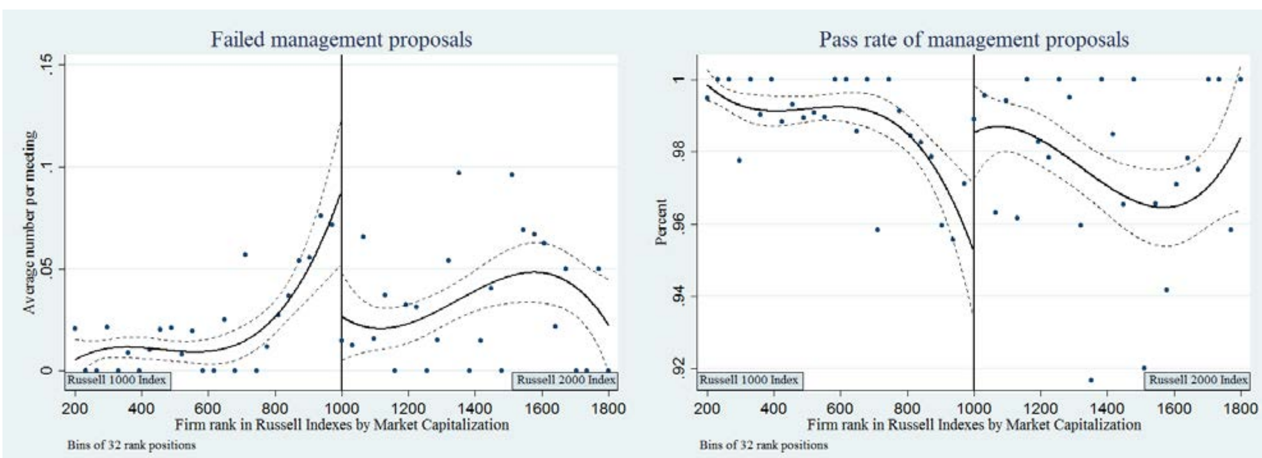


Table 1

Summary Statistics for Institutional Ownership

	Firms in the Russell 1000 index						**Firms in the Russell 2000 index**						
	Mean	Std. Dev.	10th	Median	90th	Number	Mean	Std. Dev.	10th	Median	90th	Number	*p* -value (difference in means)
Market capitalization (billion $)	1.67	0.3	1.3	1.7	2.1	500	1.45	0.3	1.1	1.5	1.8	505	**0.00*** **
Total Institutional Ownership (IO) lagged 4 qtrs	48.6%	26.9%	12.9%	47.0%	86.1%	435	49.9%	28.5%	10.3%	49.8%	87.1%	441	0.50
Total IO lagged 3 qtrs	49.3%	27.5%	10.8%	48.1%	86.6%	441	50.4%	29.1%	2.0%	50.3%	88.1%	449	0.55
Total IO lagged 2 qtrs	50.0%	27.4%	12.4%	48.2%	87.1%	443	49.7%	29.2%	0.5%	49.1%	88.0%	450	0.89
Total IO lagged 1 qtr	51.2%	28.5%	9.1%	50.1%	89.2%	451	51.4%	30.0%	0.0%	52.0%	89.7%	456	0.90
Maximum IO lagged 4 qtrs	7.4%	5.3%	2.1%	6.3%	13.9%	435	7.1%	4.6%	1.7%	6.3%	13.4%	441	0.39
Maximum IO lagged 3 qtrs	7.5%	5.3%	2.0%	6.5%	14.1%	441	7.2%	4.8%	1.5%	6.5%	14.0%	449	0.46
Maximum IO lagged 2 qtrs	7.4%	5.1%	2.1%	6.3%	13.6%	443	7.4%	4.7%	1.7%	6.7%	13.9%	450	0.94
Maximum IO lagged 1 qtr	7.5%	5.1%	2.3%	6.4%	13.6%	451	7.5%	4.8%	1.8%	7.0%	14.1%	456	0.92
Total IO of top 10 institutions lagged 4 qtrs	27.9%	14.6%	9.5%	27.3%	48.3%	435	28.4%	15.1%	8.8%	28.5%	48.5%	441	0.61
Total IO of top 10 instutions lagged 3 qtrs	28.4%	14.8%	9.5%	28.4%	49.3%	441	28.8%	15.5%	8.2%	28.7%	49.3%	449	0.73
Total IO of top 10 instutions lagged 2 qtrs	28.4%	14.8%	9.7%	27.0%	48.0%	443	29.0%	15.5%	8.2%	28.5%	50.8%	450	0.55
Total IO of top 10 instutions lagged 1 qtr	29.4%	15.1%	9.8%	28.4%	49.0%	451	29.9%	15.5%	8.8%	30.1%	51.4%	456	0.61
# of IO over 5% lagged 4 qtrs	1.3	1.4	0.0	1.0	3.0	435	1.3	1.4	0.0	1.0	3.0	441	0.82
# of IO over 5% lagged 3 qtrs	1.3	1.4	0.0	1.0	3.0	441	1.4	1.4	0.0	1.0	3.0	449	0.54
# of IO over 5% lagged 2 qtrs	1.4	1.4	0.0	1.0	3.0	443	1.5	1.5	0.0	1.0	3.0	450	0.38
# of IO over 5% lagged 1 qtr	1.4	1.4	0.0	1.0	3.0	451	1.5	1.5	0.0	1.0	3.0	456	0.21
# of IO over 10% lagged 4 qtrs	0.3	0.5	0.0	0.0	1.0	435	0.3	0.6	0.0	0.0	1.0	441	0.77
# of IO over 10% lagged 3 qtrs	0.3	0.6	0.0	0.0	1.0	441	0.3	0.6	0.0	0.0	1.0	449	0.42
# of IO over 10% lagged 2 qtrs	0.3	0.6	0.0	0.0	1.0	443	0.3	0.6	0.0	0.0	1.0	450	0.51
# of IO over 10% lagged 1 qtr	0.3	0.6	0.0	0.0	1.0	451	0.3	0.6	0.0	0.0	1.0	456	0.90
# of "Quasi Indexer" lagged 2 qtrs	88.5	22.5	58.0	89.0	116.0	220	86.9	25.5	55.0	88.0	117.0	221	0.50
# of "Focused Investors" lagged 2 qtrs	4.4	1.6	2.0	4.0	6.0	218	4.5	1.8	2.0	4.0	7.0	219	0.52
# of "Transient Investors" lagged 2 qtrs	46.9	17.4	26.0	46.0	70.0	219	45.4	17.2	23.0	47.0	68.0	218	0.36

The table shows baseline summary statistics for a bandwidth of 100 firms on each side of the index cutoff for the quarters before index reconstitution in the sample period, which occurs near the end of June for the years 2002 to 2006. All variables are calculated from Thomson-Reuters Institutional Holdings (13F) data merged to Russell Indexes data. Total Institutional Ownership (IO) is the sum of all reported ownership of a security (identified by cusip) in the 13F database as a percentage of shares outstanding at the quarter end (obtained from the quarterly CRSP-Compustat Merged Database). Maximum IO is the single highest ownership stake reported by a 13F institution. Total IO of the top 10 institutions it the sum of the ownership of the ten 13F institutions with the largest stake in the firm. Number of IO over 5 (10) percent is the number of 13F institutions that report a stake of over 5% (10%) of the security's outstanding shares. The rightmost column reports the *p* value of a t test for differences in means between the Russell 1000 and Russell 2000 samples. Market Capitalization is as of May 31, the day index reconstitution is determined based on market capitalization ranking, and is Russell Indexes' proprietary measure.

Table 2

Summary statistics for Blockholders

	Firms in the Russell 1000 index						**Firms in the Russell 2000 index**						
	Mean	Std.	10th	Median	90th	Number	Mean	Std.	10th	Median	90th	Number	*p* -value (difference in means)
Total votes controlled by:													
-all blockholders	18.7%	14%	0%	17%	37%	105	21.2%	13%	6%	20%	38%	101	0.19
-outside blockholders	18.4%	11%	6%	16%	34%	89	20.8%	11%	7%	20%	36%	89	0.15
-inside blockholders	14.3%	10%	5%	9%	28%	23	12.6%	7%	6%	10%	23%	23	0.50
-institutional blockholders	18.1%	11%	6%	15%	32%	87	19.7%	10%	7%	19%	34%	87	0.32
-non-institutional blockholders	15.0%	11%	5%	9%	30%	26	15.3%	12%	6%	12%	29%	28	0.93
-All Officers and Directors (total)	4.4%	3%	1%	4%	9%	105	3.8%	3%	1%	3%	7%	101	0.14
Average votes controlled by:													
-all blockholders	8.8%	3%	6%	8%	13%	92	8.5%	3%	6%	8%	11%	93	0.54
-outside blockholders	8.5%	3%	6%	8%	12%	89	8.0%	3%	6%	8%	11%	89	0.30
-inside blockholders	11.6%	8%	5%	8%	27%	23	11.0%	5%	6%	10%	20%	23	0.76
-institutional blockholders	8.6%	3%	6%	8%	12%	87	7.9%	2%	6%	8%	10%	87	**0.06***
-non-institutional blockholders	10.6%	7%	5%	8%	20%	26	12.2%	10%	6%	10%	22%	28	0.48
Firms with no blockholders	12.4%	33%	0.0	0.0	1.0	105	7.9%	27%	0.0	0.0	0.0	101	0.29
Number of													
-blockholders	2.1	1.5	0.0	2.0	4.0	105	2.4	1.4	1.0	2.0	4.0	101	0.10
-outside blockholders	2.2	1.4	1.0	2.0	4.0	89	2.5	1.1	1.0	2.0	4.0	89	0.11
-inside blockholders	0.3	0.5	0.0	0.0	1.0	103	0.3	0.5	0.0	0.0	1.0	101	0.84
-institutional blockholders	2.1	1.3	1.0	2.0	4.0	87	2.4	1.1	1.0	2.0	4.0	87	0.12
-non-institutional blockholders	1.3	0.5	1.0	1.0	2.0	26	1.2	0.4	1.0	1.0	2.0	28	0.44
Number of 13d blockholders:													
-in total	1.2	1.8	0.0	0.0	4.0	105	1.1	1.6	0.0	0.0	4.0	101	0.65
-outsiders	0.9	1.4	0.0	0.0	3.0	89	0.8	1.3	0.0	0.0	3.0	89	0.74
-insiders	0.4	0.7	0.0	0.0	1.0	103	0.3	0.6	0.0	0.0	1.0	101	0.33
-institutionals	0.9	1.4	0.0	0.0	3.0	87	0.8	1.3	0.0	0.0	3.0	87	0.70
-non-institutionals	0.4	0.7	0.0	0.0	1.0	105	0.4	0.6	0.0	0.0	1.0	101	0.44

The table shows baseline summary statistics for a bandwidth of 100 firms on each side of the index cutoff for the period before index reconstitution. The sample period is index reconstitutions from 2002-2006. All variables are calculated from hand collected SEC Schedule 14A or 14C Proxy filings from the EDGAR database. Outsiders are blockholders that are not also officers or directors; insiders are blockholders with an officer or a director representative. Institutionals are hand coded from the shareholder name and are all financial institutions; Non-Institutionals are blockholders not categorized as Institutionals. All Officers and Directors as a group is the total given in each Proxy filing, and must always be reported, even if the total is below 5%. Number of institutional blockholders is not significantly different between the two groups when a RD is run using the baseline values as the dependent variable (i.e. controlling for market capitalization). 13d Blockholders are blockholders that have filed a Schedule 13d, which is for shareolders with less than 20% of the equity but more than 5%, and when the intent is to "changing or influencing the control of the issuer" of the security. This is understood to mean an intent to exert control of the firm.

Table 3

Summary Statistics for CEO compensation

	Firms in the Russell 1000 index						Firms in the Russell 2000 index						*p* -value (difference in means)
	Mean	Std. Dev.	10th	Median	90th	Number	Mean	Std. Dev.	10th	Median	90th	Number	
Variable compensation pay share	57%	27%	7%	63%	85%	336	58%	27%	9%	65%	86%	313	0.69
Variable / Fixed Compensation	2.86	4.95	0.07	1.75	5.89	336	3.18	5.83	0.10	1.84	6.37	313	0.46
Options are part of CEO's pay	66%	48%	0%	100%	100%	341	65%	48%	0%	100%	100%	315	0.94
Option pay share	28%	28%	0%	21%	72%	341	27%	27%	0%	22%	69%	315	0.61
Salary pay share	29%	23%	10%	22%	59%	341	30%	22%	10%	24%	62%	315	0.42
Bonus pay share	16%	20%	0%	10%	42%	341	16%	19%	0%	11%	45%	315	0.77
Stock pay share	10%	22%	0%	0%	47%	341	8%	18%	0%	0%	29%	315	0.15
Restricted stock pay share	8%	19%	0%	0%	33%	341	9%	19%	0%	0%	34%	315	0.52
Long Term Incentive pay share	4%	15%	0%	0%	0%	341	2%	11%	0%	0%	0%	315	0.23
Perks pay share	7%	17%	0%	2%	16%	341	5%	11%	0%	2%	13%	315	**0.03****
Nonequity compensation pay share	8%	19%	0%	0%	28%	341	7%	15%	0%	0%	28%	315	0.30
Total pay ($000)	3,830	4,057	895	2,886	7,106	347	3,336	3,560	941	2,659	5,647	322	**0.10***
New option grants ($000)	1,077	1,475	0	632	2,867	341	1,232	2,800	0	550	2,816	315	0.37
New option Delta	0.21	0.25	0.00	0.14	0.55	338	0.19	0.24	0.00	0.11	0.50	313	0.46

The table shows baseline summary statistics for a bandwidth of 100 firms on each side of the index cutoff for the period before index reconstitution. The sample period is centered on index reconstitutions ocurring in June for yeas 2002-2006 inclusive. All variables are calculated from Execucomp data merged to Russell Indexes data. Options equals option_awards_fv if present, and option_awards_blk_value if not. Variable compensation payshare equals (bonus + options + stock_awards_fv+rstkgrnt)/tdc1. Variable over fixed compensation is (bonus + options + stock_awards_fv+rstkgrnt) / (tdc1-numerator). Any options in pay package is an indicator equal to 1 if the option variable is not missing or zero. Pay share variables are the respective variables (options, salary, bonus, stock_awards_fv, rstkgrnt, ltip, othcomp, noneq_incent) divided by tdc1. Total pay is tdc1; new option grants is options; new option delta is calculated following Core and Guay (2002). Pay shares sum exactly to one for all observations in the sample, so missing components are set to zero.

Table 4

Summary Statistics for Accounting variables

	Firms in the Russell 1000 index						**Firms in the Russell 2000 index**						
	Mean	Std. Dev.	10th	Median	90th	# obs	Mean	Std. Dev.	10th	Median	90th	# obs	*p* -value (difference in means)
Operating Income/ Assets	12.6%	9.9%	2.1%	11.6%	25.4%	438	12.8%	10.4%	2.1%	12.2%	26.2%	447	0.76
Profits/ Enterprise Value	1.8%	4.9%	-1.2%	2.5%	5.1%	476	1.7%	4.9%	-2.6%	2.6%	5.2%	480	0.72
Sales Growth	11.9%	22.6%	-10.1%	7.7%	39.8%	465	10.0%	22.8%	-9.3%	6.2%	37.5%	465	0.22
Capex/ Assets	4.4%	4.8%	0.0%	3.1%	10.1%	446	4.5%	4.9%	0.1%	3.1%	10.9%	450	0.70
Capx/ Ent. Value	2.7%	3.1%	0.0%	1.7%	6.7%	446	2.7%	3.1%	0.1%	1.7%	6.5%	450	0.93
PPE/ Assets	25.2%	23.5%	1.4%	17.8%	64.5%	439	25.3%	24.2%	1.0%	17.1%	65.4%	448	0.94
R&D/ Sales	16.7%	36.8%	1.0%	5.0%	36.7%	180	20.0%	57.8%	0.7%	5.2%	31.6%	171	0.52
Debt/ (Debt+Equity)	25.6%	22.4%	0.0%	21.5%	59.2%	476	26.1%	23.1%	0.0%	21.3%	60.1%	480	0.70
Financial Debt/ Assets	24.9%	20.7%	0.0%	22.2%	53.1%	476	25.8%	20.8%	0.0%	24.8%	53.9%	480	0.50
Financial Debt/ Ent. Value	18.3%	16.5%	0.0%	14.7%	41.8%	476	19.0%	17.3%	0.0%	15.6%	43.9%	480	0.55
Balance Sheet Leverage	56.8%	23.8%	22.7%	58.0%	90.7%	475	57.1%	24.2%	22.1%	56.9%	91.5%	479	0.85
Debt issuance / Assets	13.8%	20.0%	0.0%	4.3%	45.5%	421	13.9%	19.9%	0.0%	2.9%	45.3%	416	0.95
Debt retirement / Assets	10.7%	14.0%	0.0%	4.1%	32.6%	432	10.2%	14.1%	0.0%	3.3%	32.6%	428	0.65
Dividend Yield	1.6%	2.4%	0.0%	0.4%	4.9%	472	1.6%	2.5%	0.0%	0.5%	4.9%	480	0.93
Repurchases (% of equity)	1.7%	3.1%	0.0%	0.0%	6.1%	414	2.1%	3.8%	0.0%	0.3%	7.8%	408	**0.05****
Market-to-Book	1.9	1.3	1.0	1.4	3.7	476	1.9	1.3	1.0	1.4	3.5	480	0.83
Q	2.0	1.3	1.0	1.5	3.9	419	2.1	1.4	1.1	1.5	4.0	404	0.33
Enterprise Value ($m)	4,214	3,683	1,608	2,906	9,561	476	3,752	3,414	1,436	2,513	8,010	480	**0.04****
Enterprise Value (%Δ)	17.3%	38.4%	-14.9%	9.9%	61.5%	465	16.3%	35.7%	-21.1%	9.3%	60.3%	465	0.70

Summary statistics for a 100 firm bandwidth on either side of the index cutoff. The sample period is centered on Index reconstitutions ocurring in June for years 2002-2006 inclusive. All variables are calculated from the CRSP-Compustat Merged Database merged to Russell Indexes data. Operating income/Assets is oibdp/at; Profits over enterprise value is ib / enterprise value, where the latter is at - seq -pstock + txditc + csho*prcc_f. Sales growth is the 1 year change in sale. Capex is capx, PPE is ppent, R&D/Sales is xrd/sale. Debt/(Debt+Equity) is (dltt+dlc)/[dltt+dlc +csho*prcc_f]. Financial Debt/Assets is (dltt+dlc)/at. Balance Sheet leverage is total liabilities/at. Debt issuance is dltis, debt retirement is dltr. Dividend yield is (dvc+dvp)/(csho*prcc_f). Repurchases are prstkc/(shout*prcc_f). Market to book is enterprise value/at. Q is (prcc_f*csho + at - ceq - txdb)/at. Repurchases are not significant in a RDD run with the baseline values as the dependent variable (i.e. controlling for the assignment variable, market capitalization rank).

Table 5

Summary statistics for Mergers and Acquisitions

	Firms in the Russell 1000 index						**Firms in the Russell 2000 index**						
	Mean	Std. Dev.	10th	Median	90th	Number	Mean	Std. Dev.	10th	Median	90th	Number	*p* -value (difference in means)
% of firms with no deals	0.63	0.48	0.00	1.00	1.00	479	0.65	0.48	0.00	1.00	1.00	476	0.50
# deals per acquirer	1.57	0.98	1.00	1.00	3.00	178	1.77	1.45	1.00	1.00	3.00	167	0.12
# of public targets	0.09	0.28	0.00	0.00	0.00	279	0.06	0.25	0.00	0.00	0.00	296	0.29
# of private targets	0.57	0.50	0.00	1.00	1.00	279	0.58	0.49	0.00	1.00	1.00	296	0.75
# of subsidiary targets	0.34	0.48	0.00	0.00	1.00	279	0.34	0.47	0.00	0.00	1.00	296	0.89
# of deals with all cash payment	0.26	0.44	0.00	0.00	1.00	279	0.33	0.47	0.00	0.00	1.00	296	0.13
# of deals involving stock payment	0.54	0.50	0.00	1.00	1.00	279	0.48	0.50	0.00	0.00	1.00	296	0.31
# of deals with all stock payment	0.02	0.15	0.00	0.00	0.00	279	0.02	0.14	0.00	0.00	0.00	296	0.93
# of diversifying acquisitions	0.56	0.50	0.00	1.00	1.00	279	0.55	0.50	0.00	1.00	1.00	296	0.81
# of domestic targets	0.82	0.39	0.00	1.00	1.00	279	0.86	0.35	0.00	1.00	1.00	296	0.25
Total value of all acquisitions in year (% of assets)	12.5	16.6	1.3	7.2	28.5	103	20.7	55.3	2.1	6.1	51.9	105	0.15
Average value of all acquisitions in year (% of assets)	11.5	16.6	1.3	6.0	25.6	103	15.8	33.0	1.9	5.1	41.6	105	0.23
Deal value ($ million)	216	476	6	67	481	153	203	536	8	61	425	170	0.82
# deals >2% of assets	0.62	0.49	0.00	1.00	1.00	153	0.65	0.48	0.00	1.00	1.00	170	0.59
# deals >3% of assets	0.51	0.50	0.00	1.00	1.00	153	0.52	0.50	0.00	1.00	1.00	170	0.90
# deals >10% of assets	0.22	0.42	0.00	0.00	1.00	153	0.22	0.41	0.00	0.00	1.00	170	0.92
# diversifying deals >2% of assets	0.35	0.48	0.00	0.00	1.00	153	0.31	0.46	0.00	0.00	1.00	170	0.56
# diversifying deals >3% of assets	0.29	0.45	0.00	0.00	1.00	153	0.25	0.43	0.00	0.00	1.00	170	0.47
# diversifying deals >10% of assets	0.12	0.32	0.00	0.00	1.00	153	0.09	0.28	0.00	0.00	0.00	170	0.37
# deals >$50million	0.57	0.50	0.00	1.00	1.00	153	0.55	0.50	0.00	1.00	1.00	170	0.81
# deals >$100million	0.42	0.50	0.00	0.00	1.00	153	0.38	0.49	0.00	0.00	1.00	170	0.48
# deals >$200million	0.22	0.41	0.00	0.00	1.00	153	0.22	0.42	0.00	0.00	1.00	170	0.87
# deals >$400million	0.14	0.35	0.00	0.00	1.00	153	0.11	0.32	0.00	0.00	1.00	170	0.40

The table shows baseline summary statistics for a bandwidth of 100 firms on each side of the index cutoff for the period before index reconstitution. The sample period is centered on index reconstitutions ocurring in June for years 2002-2006 inclusive. All variables are calculated from SDC platinum data merged to Russell Indexes data, except for firm assets, which is obtained from the CRSP-Compustat Merged Database. SDC classifies targets as public (publicly listed firms), private (unlisted firms), and subsidiaries (over 50% is owned by a parent entity, which may itself be publicly listed). Diversifying deals are deals in which the primary 3 digit SIC code of the acquirer does not match that of the target. All deals in the sample have either a US based acquirer or US based ultimate parent of the acquirer. The rightmost column reports the *p* value of a t test for differences in means between the Russell 1000 and Russell 2000 samples.

Table 6

Summary Statistics for Voting at Shareholder Meetings

	Firms in the Russell 1000 index						Firms in the Russell 2000 index						
	Mean	Std. Dev	10th	Median	90th	# obs	Mean	Std. Dev	10th	Median	90th	# obs	*p* -value (difference in means)
Avg. votes for Management Proposals	82%	15%	58%	86%	98%	85	81%	15%	59%	85%	97%	82	0.79
Avg. votes for Shareholder Proposals	41%	22%	9%	32%	72%	19	46%	25%	8%	51%	73%	22	0.50
# of proposals per meeting	1.6	1.0	1.0	1.0	3.0	97	1.7	0.9	1.0	1.0	3.0	93	0.30
# of Management Proposals per meeting	1.3	0.8	0.0	1.0	2.0	97	1.3	0.8	0.0	1.0	2.0	93	0.64
Pass rate of Management Proposals	95%	19%	100%	100%	100%	85	96%	19%	100%	100%	100%	82	0.99
# of Shareholder Proposals per meeting	0.27	0.7	0.0	0.0	1.0	97	0.35	0.8	0.0	0.0	1.0	93	0.42
Pass rate of Shareholder Proposals	32%	45%	0%	0%	100%	19	44%	50%	0%	0%	100%	22	0.42
Meetings with Shareholder Proposals	0.20	0.40	0.00	0.00	1.00	97	0.24	0.43	0.00	0.00	1.00	93	0.50
# of failed Management Proposals per meeting	0.05	0.22	0.00	0.00	0.00	97	0.05	0.23	0.00	0.00	0.00	93	0.95
# of failed Shareholder Proposals per meeting	0.18	0.46	0.00	0.00	1.00	97	0.22	0.64	0.00	0.00	1.00	93	0.62

The table shows baseline summary statistics for a bandwidth of 150 firms on each side of the index cutoff for the period before index reconstitution. The sample period is centered around reconstitutions ocurring in June for years 2002-2006 inclusive. Almost identical results obtain with a bandwidth of 100 firms, but the 150 firm bandwidth is chosen to fit the robustness sample bandwidth. The *p* -values are from t-tests for difference in means. All variables are calculated from RiskMetrics data merged to Russell Indexes data. Average votes for management (shareholder) proposals is the votes_for variable in the RiskMetrics data taken only for proposals submitted by management (shareholders: Irrc issue codes 2000 or over) and averaged across all shareholders proposals at each meeting. Number of proposals per meeting is the number of proposals recorded in the RiskMetrics data for each meeting-sponsor combination. Pass rate of management proposals is the number of management proposals that are recorded as passed divided by the total number of management proposals. Meetings with shareholder proposals is an indicator that takes a value of one if at least one shareholder proposal is recorded by RiskMetrics. Number of failed shareholder (management) proposals is the number of proposals per Annual Shareholder Meeting that fail to pass and are sponsored by shareholders (management).

Table 7.1 - Institutional Ownership (Total)

	Ownership by all institutions Total Institutional Ownership						Ownership by all institutions Total equity ownership of Top 10 institutions					
	June (Reconstit.)	**Sept.**	**Dec.**	**Mar. (t+1)**	**June (t+1)**	**Sept. (t+1)**	**June (Reconstit.)**	**Sept.**	**Dec.**	**Mar. (t+1)**	**June (t+1)**	**Sept. (t+1)**
R2000 vs R1000												
Coefficient	**10.2%***	**12%****	7.1%	**9.6%***	7.9%	7.8%	3.5%	2.1%	2.6%	**4.7%***	**6.6%****	**8.2%*****
s.e.	5.4%	5.5%	5.7%	5.9%	6.1%	6.2%	2.8%	2.8%	3.0%	2.8%	2.9%	3.0%
p-value	0.06	0.03	0.21	0.10	0.20	0.21	0.21	0.45	0.39	0.10	0.03	0.01
# obs.	905	900	886	872	860	846	905	900	886	872	860	846
# clusters	521	519	513	505	498	491	521	519	513	505	498	491
STAY R2000 vs UP												
Coefficient	**15.3%****	**18%****	**13.2%***	**16.8%****	**13.6%***	9.7%	**7.9%****	**6.4%***	**7.0%***	**9.1%*****	**9.4%****	**11.2%*****
s.e.	7.4%	7.5%	7.8%	8.2%	8.3%	8.3%	3.7%	3.6%	3.8%	3.7%	3.9%	3.9%
p-value	0.04	0.02	0.09	0.04	0.10	0.24	0.03	0.08	0.06	0.01	0.02	0.00
# obs.	534	531	523	513	507	498	534	531	523	513	507	498
# clusters	387	385	381	374	369	362	387	385	381	374	369	362
DOWN vs STAY R1000												
Coefficient	4.3%	4.0%	1.0%	0.9%	0.5%	7.2%	-0.6%	-2.5%	-1.5%	0.4%	4.1%	5.6%
s.e.	8.6%	8.7%	9.0%	9.4%	10.5%	10.8%	4.6%	4.6%	4.9%	4.7%	4.9%	5.1%
p-value	0.62	0.64	0.91	0.93	0.96	0.51	0.90	0.59	0.76	0.93	0.41	0.27
# obs.	355	353	347	343	337	332	355	353	347	343	337	332
# clusters	264	263	259	255	251	250	264	263	259	255	251	250

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff, in the period following Index reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions from June 2002 to June 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Thomson Reuters 13F. Total IO is total Institutional ownership in the firm as a percent of outstanding equity by institutions in the 13F database. Ownership by top 10 is ownership by the ten institutions with the largest holdings in that security. Coefficients significant at ten percent or below are in bold; *** denotes statistical significance at the 1% level or below, ** at the 5% level and * at the 10% level.

Table 7.2 - Institutional Ownership (Top 10 institutions - "Indexers" and "Quasi Indexers")

	Ownership by "Indexers" Total equity ownership of Top 10 institutions						Ownership by "Quasi Indexers" Total equity ownership of Top 10 institutions					
	June (Reconstit.)	**Sept.**	**Dec.**	**Mar. (t+1)**	**June (t+1)**	**Sept. (t+1)**	**June (Reconstit.)**	**Sept.**	**Dec.**	**Mar. (t+1)**	**June (t+1)**	**Sept. (t+1)**
R2000 vs R1000												
Coefficient	3.4%	2.2%	2.1%	**4.4%***	**5.3%****	**6.4%*****	2.5%	1.6%	1.4%	3.8%	**5.6%****	**7.3%*****
s.e.	2.3%	2.3%	2.4%	2.4%	2.4%	2.5%	2.4%	2.3%	2.4%	2.4%	2.5%	2.5%
p-value	0.14	0.35	0.38	0.06	0.03	0.01	0.29	0.51	0.56	0.12	0.02	0.00
# obs.	905	900	886	872	860	846	900	895	881	867	855	841
# clusters	521	519	513	505	498	491	517	515	509	501	494	487
STAY R2000 vs UP												
Coefficient	**5.8%****	**4.6%***	**5.1%***	**7.5%*****	**7.0%****	**8.4%*****	**6.7%****	**5.6%***	**5.1%***	**8.2%*****	**8.5%*****	**10.1%*****
s.e.	3.0%	3.0%	3.1%	3.0%	3.1%	3.1%	3.0%	2.9%	3.0%	3.1%	3.3%	3.3%
p-value	0.05	0.10	0.10	0.01	0.03	0.01	0.03	0.06	0.09	0.01	0.01	0.00
# obs.	534	531	523	513	507	498	531	528	520	510	504	495
# clusters	387	385	381	374	369	362	384	382	378	371	366	359
DOWN vs STAY R1000												
Coefficient	1.1%	-0.7%	-0.6%	0.7%	3.0%	3.8%	1.5%	-2.5%	-2.3%	-1.0%	3.3%	5.4%
s.e.	3.8%	3.8%	4.0%	3.9%	4.0%	4.2%	4.1%	4.1%	4.2%	4.0%	4.2%	4.3%
p-value	0.77	0.86	0.87	0.85	0.45	0.37	0.72	0.54	0.58	0.80	0.44	0.21
# obs.	355	353	347	343	337	332	353	351	345	341	335	330
# clusters	264	263	259	255	251	250	262	261	257	253	249	248

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions from June 2002 to June 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Thomson Reuters 13F. Total ownership of top 10 institutions is ownership by the ten institutions with the largest holdings in that security by category (i.e. Indexers, Quasi Indexers). Ownership by "Quasi-Indexers" is ownership by institutions classified as such by Bushee (2001), i.e. they have high diversification and low turnover. Ownership by "Indexers" is ownership by investment companies and independent investment advisors (determined from Thomson Reuters typecodes as extended by Brian Bushee) that are also classified as Quasi-Indexers. Coefficients significant at ten percent or below are in bold; *** denotes statistical significance at the 1% level or below, ** at the 5% level and * at the 10% level.

Table 7.3 - Institutional Ownership (Top 10 institutions - "Focused investors" and "Transient investors")

	Ownership by "Focused Investors" Total equity ownership of Top 10 institutions						Ownership by "Transient Investors" Total equity ownership of Top 10 institutions					
	June (Reconstit.)	**Sept.**	**Dec.**	**Mar. (t+1)**	**June (t+1)**	**Sept. (t+1)**	**June (Reconstit.)**	**Sept.**	**Dec.**	**Mar. (t+1)**	**June (t+1)**	**Sept. (t+1)**
R2000 vs R1000												
Coefficient	**5.6%***	**4.7%***	4.4%	**7.5%****	**9.4%*****	**8.4%*****	**6.4%****	**6.8%*****	**4.5%***	4.6%	**5.3%***	**8.5%*****
s.e.	2.8%	2.8%	2.8%	3.2%	3.3%	3.3%	2.7%	2.6%	2.8%	2.9%	3.0%	2.9%
p-value	0.06	0.10	0.12	0.02	0.00	0.01	0.02	0.01	0.10	0.12	0.08	0.00
# obs.	898	887	874	864	852	832	901	891	879	867	855	839
# clusters	515	511	504	498	492	483	517	513	507	500	494	486
STAY R2000 vs UP												
Coefficient	**8.5%*****	**7.4%***	5.4%	**10.6%*****	**13.3%*****	**10.5%*****	**8.5%****	**9.2%*****	**7.8%****	**9.9%****	**9.1%****	**11.0%*****
s.e.	3.5%	3.8%	3.6%	4.0%	4.1%	4.2%	3.5%	3.5%	3.6%	4.1%	4.1%	4.0%
p-value	0.01	0.05	0.14	0.01	0.00	0.01	0.02	0.01	0.03	0.02	0.03	0.01
# obs.	530	523	516	509	503	489	532	526	519	511	505	494
# clusters	383	380	375	370	365	355	385	382	378	372	367	359
DOWN vs STAY R1000												
Coefficient	0.5%	0.9%	3.1%	4.3%	4.1%	5.7%	5.1%	5.4%	1.0%	-1.8%	1.5%	6.1%
s.e.	4.9%	4.7%	5.0%	5.4%	5.6%	5.8%	4.4%	4.4%	4.7%	4.7%	4.9%	4.7%
p-value	0.91	0.85	0.53	0.43	0.46	0.32	0.25	0.22	0.83	0.69	0.76	0.20
# obs.	352	348	342	339	333	327	353	349	344	340	334	329
# clusters	261	258	254	251	247	246	262	259	256	252	248	247

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions from June 2002 to June 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Thomson Reuters 13F. Total ownership of top 10 institutions is ownership by the ten institutions with the largest holdings in that security by category (i.e. Focused and Transient Investors). Ownership by "Focused Investors" and "Transient Investors" is ownership by institutions classified as "Dedicated" and "Transient" respectively by Bushee (2001), i.e. they have low diversification with low turnover, and high diversification with high turnover respectively. Coefficients significant at ten percent or below are in bold; *** denotes statistical significance at the 1% level or below, ** at the 5% level and * at the 10% level.

Table 8 - Blockholder ownership

R2000 vs R1000	Total BH votes	Outsider BH votes	Insider BH votes	Institutional BH votes	Non-institutional BH votes	Total votes of Officers and Directors	Firms without BH	# BH	# Outside BH	# Inside BH	# of Institutional	# of non-Institutional	# of 13d BH
Coefficient	7.0%	1.4%	9.4%	0.8%	8.7%	-1.0%	**-0.15***	**0.77***	0.39	0.13	0.25	0.39	0.41
s.e.	4.6%	4.4%	8.0%	4.6%	8.4%	1.0%	0.08	0.45	0.46	0.14	0.46	0.24	0.65
p-value	0.13	0.75	0.24	0.86	0.31	0.35	0.06	0.09	0.39	0.38	0.58	0.11	0.53
# obs.	252	222	59	219	66	252	252	252	222	250	219	66	252
# clusters	150	139	39	139	44	150	150	150	139	150	139	44	150

STAY R2000 vs UP	Total BH votes	Outsider BH votes	Insider BH votes	Institutional BH votes	Non-institutional BH votes	Total votes of Officers and Directors	Firms without BH	# BH	# Outside BH	# Inside BH	# of Institutional BH	# of non-Institutional BH	# of 13d BH
Coefficient	8.8%	1.7%	-3.4%	1.0%	-2.2%	-1.2%	**-0.26***	**1.32***	0.76	0.18	0.57	0.33	0.21
s.e.	6.8%	6.0%	13.0%	6.1%	11.6%	1.7%	0.13	0.70	0.69	0.23	0.69	0.35	1.05
p-value	0.20	0.77	0.80	0.87	0.85	0.49	0.05	0.06	0.28	0.42	0.41	0.35	0.84
# obs.	143	124	34	122	38	143	143	143	124	141	122	38	143
# clusters	108	97	28	96	32	108	108	108	97	108	96	32	108

DOWN vs STAY R1000	Total BH votes	Outsider BH votes	Insider BH votes	Institutional BH votes	Non-institutional BH votes	Total votes of Officers and Directors	Firms without BH	# BH	# Outside BH	# Inside BH	# of Institutional BH	# of non-Institutional BH	# of 13d BH
Coefficient	6.5%	2.0%	**0.5%***	0.4%	**0.5%****	-0.4%	-0.09	0.35	0.14	-0.01	-0.06	**0.01****	0.72
s.e.	7.0%	6.9%	0.2%	6.9%	0.2%	1.1%	0.13	0.67	0.64	0.20	0.64	0.00	0.88
p-value	0.35	0.77	0.05	0.95	0.02	0.73	0.47	0.60	0.83	0.96	0.93	0.01	0.42
# obs.	109	98	25	97	28	109	109	109	98	109	97	28	109
# clusters	86	79	21	78	23	86	86	86	79	86	78	23	86

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1K) and Russell 2000 (R2K) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1K (of larger firms) instead of the R2K among firms close to the cutoff in the period following reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1K and within the bandwidth against all firms in the R2K and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1K to those that remain in the R2K. The third panel compares firms in the bandwidth that move down into the R2K with those that remain in the R1K.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is index reconstitutions from 2002-2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and SEC's EDGAR database. Outsiders are blockholders that are not also officers or directors; insiders are blockholders with an officer or a director representative. Institutionals are hand coded from the shareholder name and are all financial institutions; Non-Institutionals are blockholders not categorized as Institutionals. All Officers and Directors as a group is the total given in each Proxy filing, and must always be reported, even if the total is below 5%. Coefficients significant at ten percent or below are in bold; p values are displayed in the table.

Table 9 - CEO Compensation

R2000 vs R1000	CEO leaves firm within year	CEO leaves firm within 2 years	Variable/ Fixed Compensation	Option pay share	Salary pay share	Restricted Stock pay share	LongTerm Incentive pay share	Total pay ($000)	New option grants ($000)	New option Delta
Coefficient	-3.9%	6.1%	**1.29***	7.3%	**-9.5%****	-1.8%	-2.2%	239	513	0.07
s.e.	6.0%	6.3%	0.7	6.2%	4.9%	3.5%	2.8%	675	386	0.06
p-value	0.51	0.33	0.07	0.24	0.05	0.60	0.42	0.72	0.18	0.27
# obs.	679	679	643	650	650	650	650	662	650	645
# clusters	386	386	369	372	372	372	372	377	372	369

STAY R2000 vs UP	CEO leaves firm within year	CEO leaves firm within 2 years	Variable/ Fixed Compensation	Option pay share	Salary pay share	Restricted Stock pay share	LongTerm Incentive pay share	Total pay ($000)	New option grants ($000)	New option Delta
Coefficient	-5.4%	**17.7%****	**1.74****	**16.7%***	**-10.9%***	**-9.6%****	**-9.1%****	887	**1156****	**0.14***
s.e.	6.6%	8.7%	0.9	8.9%	6.0%	4.5%	3.9%	941	489	0.08
p-value	0.41	0.04	0.05	0.06	0.07	0.03	0.02	0.35	0.02	0.07
# obs.	397	397	376	378	378	378	378	386	378	378
# clusters	290	290	275	276	276	276	276	282	276	276

DOWN vs STAY R1000	CEO leaves firm within year	CEO leaves firm within 2 years	Variable/ Fixed Compensation	Option pay share	Salary pay share	Restricted Stock pay share	LongTerm Incentive pay share	Total pay ($000)	New option grants ($000)	New option Delta
Coefficient	-3.5%	-7.5%	0.5	-5.9%	-7.3%	7.8%	6.1%	-890	-321	-0.04
s.e.	11.0%	9.3%	1.3	9.2%	8.7%	5.8%	4.0%	989	688	0.10
p-value	0.75	0.42	0.68	0.52	0.40	0.18	0.13	0.37	0.64	0.66
# obs.	281	281	266	271	271	271	271	275	271	266
# clusters	210	210	199	203	203	203	203	206	203	199

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff in the period after reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions occurring in years 2002 to 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Execucomp. CEO leaves firm variables are indicators equal to one if Execucomp records the CEO as having left the firm in the July-June30 period following reconstitution (or two such periods for the 2 year variable). Options equals option_awards_fv if present, and option_awards_blk_value if not. Variable over fixed compensation is (bonus + options + stock_awards_fv+rstkgrnt) / (tdc1-numerator). Options are part of CEO's pay is an indicator equal to 1 if options is not missing or zero. Pay share variables are the respective variables (options, salary, rstkgrnt, ltip) divided by tdc1. Total pay is tdc1; new option grants is options; new option delta is calculated following Core and Guay (2002). Pay shares sum exactly to one for all observations in the sample, so missing components are set to zero. Coefficients significant at ten percent or below are in bold; p values are displayed in the table.

Table 10 - Accounting variables

R2000 vs R1000	Capex/Assets	R&D/Sales	Debt/(Debt + Equity)	Financial Debt/ Ent. Value	Debt Issuance /Assets	Debt retirement/Assets	Dividend Yield	Repurchases as % of equity	Op. Profits/Assets	Market-to-book
Coefficient	**-2.04%****	7.9%	-5.5%	-4.3%	-4.3%	-2.2%	0.2%	-0.1%	-2.3%	0.3%
s.e.	1.0%	5.0%	5.0%	3.6%	2.9%	2.3%	0.5%	0.9%	2.2%	0.2%
p -value	0.04	0.11	0.27	0.23	0.13	0.32	0.67	0.87	0.29	0.22
# obs.	877	473	880	880	869	864	875	837	825	880
# clusters	521	283	521	521	516	516	521	500	491	521

STAY R2000 vs UP	Capex/Assets	R&D/Sales	Debt/(Debt + Equity)	Fin. Debt/Assets	Debt Issuance /Assets	Debt retirement/Assets	Dividend Yield	Repurchases as % of equity	Op. Profits/Assets	Market-to-book
Coefficient	**-2.51%***	10.7%	-4.2%	-3.9%	-5.4%	-2.6%	0.8%	0.0%	-1.8%	0.5%
s.e.	1.4%	8.2%	6.4%	4.3%	3.9%	3.0%	0.7%	1.2%	3.2%	0.3%
p -value	0.07	0.20	0.51	0.37	0.17	0.39	0.24	0.98	0.58	0.14
# obs.	516	268	519	519	512	508	515	493	481	519
# clusters	381	204	381	381	377	375	381	367	354	381

DOWN vs STAY R1000	Capex/Assets	R&D/Sales	Debt/(Debt + Equity)	Fin. Debt/Assets	Debt Issuance /Assets	Debt retirement/Assets	Dividend Yield	Repurchases as % of equity	Op. Profits/Assets	Market-to-book
Coefficient	-1.7%	7.7%	-5.5%	-3.8%	-2.0%	-0.8%	-0.7%	-0.4%	-3.6%	0.0%
s.e.	1.5%	5.7%	8.3%	6.1%	5.0%	4.2%	0.7%	1.5%	3.2%	0.3%
p -value	0.27	0.18	0.50	0.54	0.70	0.84	0.30	0.77	0.26	0.87
# obs.	343	200	343	343	339	338	342	327	328	343
# clusters	261	150	261	261	259	261	260	252	250	261

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000, among firms close to the cutoff, averaged over the two years following index reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000. All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions occurring June 2002 to June 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Crsp-Compustat. Capex is capx. R&D/Sales is xrd/sale. Debt/(Debt+Equity) is (dltt+dlc)/[dltt+dlc +csho*prcc_f]. Financial Debt/Ent. Value is (dltt+dlc)/Ent. value; Ent. value is at - seq -pstock + txditc + csho*prcc_f. Debt issuance is dltis, debt retirement is dltr. Dividend yield is (dvc+dvp)/(csho*prcc_f). Repurchases are prstkc/(shout*prcc_f). Op. profits /Assets is oibdp/at; Market to book is enterprise value/at.

Table 11 - Mergers and Acquisitions

R2000 vs R1000	# deals per acquirer	# cash	# diversifying	# over2% +div	# over3% +div	# over10% +div	# >$50m
Coefficient	-0.47	**-0.53****	**-0.49***	**-0.45****	**-0.38***	**-0.26***	-0.24
s.e.	0.30	0.22	0.28	0.22	0.22	0.15	0.22
p value	0.11	0.02	0.09	0.04	0.09	0.08	0.29
# obs.	342	342	342	206	206	206	206
# clusters	259	259	259	174	174	174	174
Only non-missing deal values	N	N	N	Y	Y	Y	Y

STAY R2000 vs UP	# deals per acquirer	# cash	# diversifying	# over2% +div	# over3% +div	# over10% +div	# >$50m
Coefficient	0.01	**-0.52****	-0.14	-0.36	-0.20	-0.16	-0.05
s.e.	0.49	0.26	0.42	0.28	0.28	0.19	0.28
p value	0.98	0.05	0.74	0.20	0.47	0.41	0.86
# obs.	213	213	213	135	135	135	135
# clusters	183	183	183	125	125	125	125
Only non-missing deal values	N	N	N	Y	Y	Y	Y

DOWN vs STAY R1000	# deals per acquirer	# cash	# diversifying	# over2% +div	# over3% +div	# over10% +div	# >$50m
Coefficient	-1.46	-0.63	**-1.49***	-0.60	**-0.83****	**-0.49***	**-0.61***
s.e.	0.94	0.42	0.82	0.38	0.36	0.28	0.35
p value	0.12	0.14	0.07	0.12	0.03	0.08	0.09
# obs.	120	120	120	66	66	66	66
# clusters	106	106	106	58	58	58	58
Only non-missing deal values	N	N	N	Y	Y	Y	Y

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is index reconstitutions from 2002-2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and SDC Platinum. # means number of deals. # cash refers to deals with all cash payment. # diversifying means number of deals where the primary SIC code of the acquirer differs from that of the target at the 3 digit SIC level. # over X% + div refers to the number of deals that are both diversifying and for a value exceeding x% of acquirer assets. #>$50m is the number of deals with a transaction value over USD $50 million. Coefficients significant at ten percent or below are in bold; *p* values are displayed in the table.

Table 12 - Voting at Shareholder Meetings

R2000 vs R1000	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	**-0.22*** **	**0.06***	**-3.15***	13.7%
s.e.	0.07	0.03	1.8%	12.6%
p value	0.00	0.08	0.09	0.28
# obs.	1983	1983	1845	508
# clusters	1007	1007	986	254

STAY R2000 vs UP	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	-0.14	0.10	-4.8%	12.0%
s.e.	0.15	0.07	3.1%	28.7%
p value	0.34	0.17	0.13	0.68
# obs.	791	791	768	81
# clusters	492	492	484	56

DOWN vs STAY R1000	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	-0.18	0.00	0.4%	-0.2%
s.e.	0.12	0.05	3.7%	0.5%
p value	0.15	0.94	0.92	0.68
# obs.	1186	1186	1072	426
# clusters	611	611	589	214

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff, averaged across the two years after Index reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000. All columns are estimated by OLS with a separate cubic polynomial in the running variable on either side of the cutoff and year fixed effects on a bandwidth of 800 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on index reconstitutions from June 2002 to 2006 inclusive. Standard errors are clustered by firm. Data is from Russell Indexes and RiskMetrics. All dependent variables are from shareholder meetings that occur between Russell index reconstitution dates (end of June), and are averaged over the two years subsequent to each June reconstitution. Number of failed shareholder (management) proposals is the average number of proposals per Annual General Meeting that fail to pass and are sponsored by shareholders (management). Pass rate of management proposals is the number of management proposals that pass divided by the total number of proposals recorded in the Riskmetrics data for each meeting. Average votes for shareholder proposals is the votes_for variable in the RiskMetrics data taken only for proposals submitted by shareholders (Irrc issue codes 2000 or over) and averaged across all shareholders proposals at each meeting. Coefficients significant at ten percent or below are in bold; *p* values are displayed in the table.

Table 13 - CEO Compensation (Robustness table)	Main local linear specification	With firm and CEO controls	With controls for Δ in rank and market cap.	With Industry fixed effects	Removing year fixed effects	Winsorized at 10%	Bandwidth of 75	Bandwidth of 150	5th degree poly. with large bandwidth & controls	Imbens & Kalyanaraman (2012) optimal bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CEO leaves firm within 2 years										
Coefficient	17.7%**	18.5%*	21.8%**	16.6%*	17.1%**	17.7%**	19.6%*	12.7%**	30.9%**	15.9%**
s.e.	8.7%	10.7%	9.1%	8.8%	8.9%	8.7%	10.5%	6.6%	14.0%	7.4%
p-value	0.04	0.08	0.02	0.06	0.05	0.04	0.06	0.05	0.03	0.03
# obs.	397	343	298	397	397	397	306	582	1609	493
# clusters	290	251	240	290	290	290	233	383	721	339
Bandwidth	100	100	100	100	100	100	75	150	800	126
Variable/ Fixed Compensation										
Coefficient	1.74**	2.19***	1.87*	1.32	1.80**	1.04*	2.20**	0.55	2.39	0.72
s.e.	0.88	0.88	1.06	0.86	0.92	0.58	1.12	0.65	1.60	0.67
p-value	0.05	0.01	0.08	0.13	0.05	0.08	0.05	0.40	0.14	0.28
# obs.	376	336	279	376	376	376	287	551	1579	525
# clusters	275	245	222	275	275	275	220	366	713	354
Bandwidth	100	100	100	100	100	100	75	150	800	140
Option pay share										
Coefficient	16.7%*	16.4%*	19.3%**	14.6%*	14.5%*	14.9%*	22.6%**	11.1%*	23.2%*	11.7%*
s.e.	8.9%	9.7%	9.0%	8.5%	8.9%	8.2%	11.4%	6.4%	12.6%	6.7%
p-value	0.06	0.09	0.03	0.09	0.10	0.07	0.05	0.09	0.07	0.08
# obs.	378	341	280	378	378	378	289	554	1602	528
# clusters	276	249	223	276	276	276	221	368	719	356
Bandwidth	100	100	100	100	100	100	75	150	800	142
Salary pay share										
Coefficient	-10.9%*	-11.5%*	-10.4%*	-9.3%	-11.5%*	-6.4%	-13.1%*	-5.6%	-13.8%*	-7.4%
s.e.	6.0%	6.2%	6.3%	6.0%	6.0%	4.1%	7.8%	4.2%	7.5%	4.7%
p-value	0.07	0.07	0.10	0.12	0.06	0.12	0.09	0.19	0.07	0.12
# obs.	378	341	280	378	378	378	289	554	1602	481
# clusters	276	249	223	276	276	276	221	368	719	329
Bandwidth	100	100	100	100	100	100	75	150	800	129
Restricted stock pay share										
Coefficient	-9.6%**	-10.0%**	-8.8%*	-10.2%**	-8.6%*	-5.1%*	-9.8%*	-8.8%***	-5.9%	-8.7%***
s.e.	4.5%	4.6%	5.1%	4.6%	4.7%	2.7%	5.4%	3.5%	5.6%	3.5%
p-value	0.03	0.03	0.09	0.03	0.07	0.06	0.07	0.01	0.29	0.01
# obs.	378	341	280	378	378	378	289	554	1602	537
# clusters	276	249	223	276	276	276	221	368	719	360
Bandwidth	100	100	100	100	100	100	75	150	800	144

This table displays alternative specifications of the main results for the sample composed of firms entering the Russell 1000 from the Russell 2000 and firms remaining in the Russell 2000 in a 100 firm bandwidth on either side of the cutoff. Column 1 reproduces the main specification: a locally linear regression with year fixed effcts and a triangle kernel. Column 2 adds the following controls: CEO age, CEO tenure, firm operating income before depreciation (oibdp), firm sales (sale), total assets (at), market value of the firm (at-seq-pstock+txditc+shrout*price), 1 year sales growth, 1 year asset growth, 1 year market value growth, and number of employees(empl). Column 3 controls for the change in both the firm's market capitalization (source: Russell Indexes) and Russell index ranking over the preceding year. Column 4 adds SIC division fixed effects and column 5 removes year fixed effects from the main specification. Column 6 winsorizes both tails at 10% to test for sensitivity to outliers (the first variable is an indicator so this has no effect). Column 7 reduces the bandwidth to 75 observations, below which ststistical power is limited, and column 8 extends the bandwidth to 150 observations. Column 9 estimates the RDD with a fifth degree polynomial specification and bandwidth of 800 observations on each side of the cutoff and the controls in columns 2 and 3 (including controls becomes more important the larger the bandwidth). Finally Column 10 runs the local linear specification using the optimal bandwidth chosen by Imbens and Kalyanaraman's (2012) algorithm, capped at 200. All columns except for column 9 are estimated by local linear regression.

Table 14 - Accounting variables (Robustness table)

	Main local linear specification	With firm controls	With controls for Δ in rank and market cap.	With Industry fixed effects	Removing year fixed effects	Winsorized at 10%	Bandwidth of 50	Bandwidth of 150	Flexible poly. with large bandwidth & controls	Imbens & Kalyanaraman (2012) optimal bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Capex/Assets										
Coefficient	**-2.04****	**-1.79****	**-1.92***	**-1.32***	**-2.08****	**-1.50****	**-3.46***	**-1.18***	**-1.72***	**-1.76***
s.e.	0.97	0.92	1.11	0.73	0.99	0.66	1.95	0.69	0.95	1.01
p -value	0.04	0.05	0.08	0.07	0.04	0.02	0.08	0.09	0.07	0.08
# obs.	877	868	669	877	877	877	440	1304	2613	740
# clusters	521	516	422	521	521	521	323	661	1152	490
Bandwidth	100	100	100	100	100	100	50	150	800	86

This table displays alternative specifications for Capital Expenditure. The sample is composed of all firms in either index and within the bandwidth for capital expenditures, and firms entering the Russell 1000 from the Russell 2000 and firms remaining in the Russell 2000 for net change in short term debt. Column 1 reproduces the main specification: a locally linear regression with year fixed effcts and a triangle kernel. Column 2 adds the following controls: firm sales (sale), market value of the firm (at-seq-pstock+ txditc+ shrout*price), 1 year sales growth, 1 year asset growth, 1 year market value growth, and number of employees(empl). Column 3 controls for the change in both the firm's market capitalization (source: Russell Indexes) and Russell index ranking over the preceding year. Column 4 adds SIC division fixed effects and column 5 removes year fixed effects from the main specification. Column 6 winsorizes both tails at 10% to test for sensitivity to outliers. Column 7 reduces the bandwidth to 50 observations, below which stastistical power is limited, and column 8 extends the bandwidth to 150 observations. Column 9 estimates the RDD with a flexible polynomial specification (3rd degree for capital expenditures, 4th for change in short term debt) and a bandwidth of 800 observations on each side of the cutoff and the controls in columns 2 and 3 (including controls becomes more important the larger the bandwidth). Finally Column 10 runs the local linear specification using the optimal bandwidth chosen by Imbens and Kalyanaraman's (2012) algorithm, capped at 200. All columns except for column 9 are estimated by local linear regression. All variables are averaged over the two years subsequent to each June reconstitution. Capital Expenditures is capx/at; Net change in short term debt is dlcch/at lagged 1 year.

Table 15 - Voting at Shareholder Meetings (Robustness table)

R2000 vs R1000	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	**-0.16***	0.07	-4.4%	14.9%
s.e.	0.09	0.05	2.7%	17.2%
p value	0.07	0.12	0.11	0.39
# obs.	349	349	326	61
# clusters	244	244	236	40

STAY R2000 vs UP	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	-0.18	0.07	-3.7%	19.0%
s.e.	0.12	0.06	2.6%	26.1%
p value	0.13	0.24	0.16	0.47
# obs.	202	202	190	32
# clusters	155	155	148	24

DOWN vs STAY R1000	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	-0.17	0.03	-4.3%	0.0%
s.e.	0.13	0.07	5.9%	0.1%
p value	0.20	0.60	0.47	0.89
# obs.	136	136	126	27
# clusters	117	117	111	24

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff, averaged across the two years after Index reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000. All columns are estimated via local linear regression with a triangle kernel, and year fixed effects on a bandwidth of 150 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on index reconstitutions from June 2002 to 2006 inclusive. Standard errors are clustered by firm. Data is from Russell Indexes and RiskMetrics. All dependent variables are from shareholder meetings that occur between Russell index reconstitution dates (end of June), and are averaged over the two years subsequent to each June reconstitution. Number of failed shareholder (management) proposals is the average number of proposals per Annual General Meeting that fail to pass and are sponsored by shareholders (management). Pass rate of management proposals is the number of management proposals that pass divided by the total number of proposals recorded in the Riskmetrics data for each meeting. Average votes for shareholder proposals is the votes_for variable in the RiskMetrics data taken only for proposals submitted by shareholders (Irrc issue codes 2000 or over) and averaged across all shareholders proposals at each meeting. Coefficients significant at ten percent or below are in bold; p values are displayed in the table.