The Effect of Institutional Ownership on Payout Policy: Evidence from Index Thresholds

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First Draft: 07/09/2012
This Draft: 9/29/2014

We thank David De Angelis, François Degeorge, François Derrien, Laurent Frésard, Todd Gormley, Gustavo Grullon, Ambrus Kecskés, Andrew Koch, Mark Leary, Alexander Ljungqvist, Garen Markarian, Pedro Matos, Brett Myers, William Mullins, Brad Paye, seminar participants at the CEPR European Summer Symposium in Financial Markets (Corporate Finance), 2014 FIRS, Lone Star Finance Symposium, 2013 Napa Conference on Financial Markets Research, Rothschild Caesarea Center 11th Annual Conference, 2013 SFS Cavalcade, and the 2013 FMA annual meeting for helpful discussions, and Russell for providing the index data. All errors are our own.
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Abstract

We show firms pay more dividends and repurchase more shares when they have higher levels of institutional ownership, even if the institutions are passive investors that are typically not considered activists. We also find that institutional ownership raises proxy voting, but does not raise profitability, or lower CEO compensation. Our identification strategy relies on an instrument for ownership based on the annual composition of the Russell indices. Overall, results support agency models where ownership by large institutions lowers the marginal cost of delegated monitoring.
Institutional investors have a distinct advantage in monitoring and controlling corporate policy. Through their expertise and concentrated holdings, institutions have a lower cost of delegated monitoring, and firms with high institutional ownership should reap the benefits of lower agency costs (Jensen and Meckling, 1976). Even passive index funds like Vanguard appear to be active monitors of the firms they own (Iliiev and Lowry, 2014). This is important because institutional investors own the bulk of public equity in the US. Over the past 40 years, ownership by both active and passive institutions combined has increased to more than 60% by 2006, with passive funds making up more than 20% of public equity holdings (Aghion, Van Reenen, Zingales, 2012). However, as noted by Grinstein and Michaely (2005), testing whether institutional investors influence corporate policy is difficult because of a fundamental identification problem. Institutions may cause differences in corporate policies, but they may also choose stocks because of expected differences in corporate policy.

In a frictionless world, ownership shouldn’t matter (Miller and Modigliani, 1961). But in the presence of frictions, there are some good reasons why it might. Grinstein and Michaely (2005) and Allen and Michaely (2003) provide a comprehensive summary of both the theoretical and empirical work in the literature with a special focus on payout. Consistent with the fundamental endogeneity between ownership structure and payout policy, the empirical evidence is mixed. Allen and Michaely (2003) conclude that there is some limited evidence for tax-clientele effects, little evidence in favor of signaling theories, and mixed evidence on agency theories. Central to this literature, Grinstein and Michaely (2005) present a battery of results testing different channels of causality between ownership and payout. They conclude that institutions are attracted to firms with positive payout but find little evidence that ownership causes payout.

In this paper we use a source of plausibly exogenous variation in institutional ownership driven largely by passive investors to test whether variation in ownership leads to differences in corporate policy. We show that firms pay more dividends and repurchase more shares when they have higher levels of institutional ownership, even if the institutions are passive investors. We do
not find that more institutional ownership leads to better operating performance or lower CEO pay. However, firms with more expected agency problems show a stronger effect in the data. Institutions appear to monitor and control the behavior of firms to drive differences in corporate policy that reduce agency costs.

Our source of plausibly exogenous variation in institutional ownership centers on the annual rebalancing of the Russell 1000 and 2000 indices. Each May 31st, the popular Russell equity indices are formed based on stock market capitalizations. The largest 1,000 firms are the Russell 1000 index while the next 2,000 make up the Russell 2000. At the 1000/2000 breakpoint, inclusion in the Russell 2000 is plausibly exogenous because index assignment is the outcome of trivial differences in market capitalization. Importantly, the largest firms of the Russell 2000 have much larger weights within their index compared to the smallest firms of the Russell 1000, despite small differences in market capitalization. Chang, Hong, and Liskovich (2014) also use this setting to study price pressure that results from changes in institutional ownership due to indexing. In our context, we study how these differences in institutional holdings affect payout and other corporate policies.

The exogenous variation in institutional ownership created by this Russell assignment rule stems from institutions’ incentives to minimize tracking error. Any institution that tracks or benchmarks their performance against the Russell indices is more likely to hold big positions in the largest components of the index. This is true for passive index funds who have very little leeway with respect to tracking error, but many actively managed funds that benchmark against a Russell index are also subject to similar constraints (Roll, 1992, Wurgler, 2010, Ma, et al., 2014).

To establish a direct link between institutional ownership and payout, we take an instrumental variables approach. Near the Russell threshold, firms are similar in size, but there is a significant difference in institutional ownership around the discontinuity in index weights. We find that the firms at the top of the Russell 2000 have institutional ownership that is roughly 9% greater than firms at the bottom of the Russell 1000. This large difference is plausibly exogenous because it is unlikely that index assignment and rankings on May 31st, around the threshold, are
related to payout policy and other corporate decisions. Thus, this instrument satisfies both relevance and exclusion.

Our instrumental variable approach is related to a regression discontinuity (RD) design, and follows Lee and Lemieux (2010), Angrist and Pischke (2009), and Roberts and Whited (2011). The nature of our question makes a single equation sharp RD inappropriate, so we instead implement a simple two-staged least squares estimation where the first stage models ownership as a function of our instrument (the Russell discontinuity) and the second stage tests the effect of instrumented ownership on a variety of corporate policy variables. Our method is best described as instrumental variables based on a sharp discontinuity. We argue that our identification strategy is both economically and statistically robust.

We find that payout is increasing in instrumented institutional ownership. For example, our results imply that a one percentage point increase in institutional ownership leads to higher dividends of 7 million dollars, which represents roughly one-tenth of the cross-sectional standard deviation in dividends near the threshold. Similarly, we find institutional ownership leads to higher repurchases and higher total payout. These results go against traditional size-based explanations: small firms typically pay out less cash to shareholders, but in our case, arbitrarily smaller firms at the index threshold pay out more. Higher institutional ownership does not lead to significantly lower cash holdings, better operating performance, or lower CEO compensation.

Our results are suggestive of a mechanism by which institutions affect payout through their monitoring activity. Institutional investors have long been associated with shareholder activism (see e.g. Gillan and Starks, 2007). However, even index funds or actively managed funds that do not engage in shareholder activism discipline managers and align incentives with

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1 We also use the semi-parametric regression discontinuity approach using local polynomials suggested by Hahn, Todd and Van Der Klaauw (2001) and Lee and Lemieux (2010). Our results are robust to different methodologies although the causal link between ownership and payout cannot be established clearly with these methodologies.
shareholders.\textsuperscript{2} All institutions have a fiduciary duty to their investors. If tracking error prevents them from selling, passive managers have to find other ways of getting their voice heard (Admati and Pfleiderer (2009)), Edmans (2009), Edmans and Manso (2011)). One way to do so is through the threat of voting, helped by proxy advisory services such as ISS or Glass-Lewis (see e.g. Matvos and Ostrovsky (2010), Morgan, Poulsen, Wolf, and Yang (2011), Iliev, Lins, Miller, and Roth (2012), and Fenn and Robinson (2009)).\textsuperscript{3} For example, in 2013 passive investor giant Vanguard opposed the election of several board members in high profile proxy contests, such as Hewlett Packard, Occidental, Cablevision, Apple, Nabors Industries and Vornado Realty Trust. These elections were directly identified by ISS as important governance events.\textsuperscript{4} In fact, Illiev and Lowry (2014) find that passive fund managers are especially likely to vote with the recommendations of proxy advisors. Even beyond elections, large institutional owners have access to management to directly communicate their views on corporate policies, and anecdotal and survey evidence suggests they engage with the firms and exert influence behind the scenes (McCahery, Sautner, and Starks (2014)).

Moreover, ISS and Glass-Lewis play an important coordination role by increasing the effectiveness of voting for firms that have a large institutional ownership base. ISS and Glass-Lewis consider payout a primary factor to evaluate governance. In their “proxy voting summary guidelines”, ISS and Glass-Lewis vote in favor of management proposals that increase payout, and payout considerations enter into their board of director voting recommendations even though

\textsuperscript{2} For example, Glen Booraem, Vanguard’s Fund Controller states “While our approach of quiet diplomacy focused on results, as opposed to noisy activism to gain attention, has led some to conclude that our voice is silent on many debates, we believe that our effectiveness is maximized by taking our message directly to those companies where we believe changes are needed, not by litigating matters in the court of public opinion.” Source: \url{https://personal.vanguard.com/us/insights/article/proxy-commentary-042013}

\textsuperscript{3} Proxy-voting is subject to the Employment Retirement Income Security Act (ERISA)’s fiduciary responsibility rules for pension funds (1974), SEC’s Proxy-Voting by Investment Advisers rule (2003), and SEC Rule 206(4)-6 of the Investment Advisers Act of 1940. Under these rules, pension funds and mutual funds should vote their proxies in the best interests of their clients, i.e. to increase the value of the funds’ holdings.

\textsuperscript{4} See “Passive fund manager Vanguard turns activist in some board votes” \url{http://www.reuters.com/article/2013/09/13/vanguard-proxyvotes-idUSL2N0H00YV20130913}
shareholders rarely get to vote on dividend proposals directly.footnote{5} These recommendations appear to be influential and increase passive investors’ monitoring ability, as Alexander et al. (2010) find that institutions often follow the advice of proxy advisers.

At a minimum, ownership by institutions reduces coordination costs (Grossman and Hart (1980), Shleifer and Vishny (1986), Crane and Koch (2014)) and can lower agency costs through economies of scale in delegated monitoring. Admati, Pfleiderer, and Zechner (1994) argue that if ownership is dispersed across uninformed investors, the marginal benefits of delegated monitoring are low and the costs high. Indeed, Brickley, Lease, and Smith (1988) find that proxy voting on governance issues increases with institutional ownership.

We test the hypothesis that institutions monitor and control firms by estimating the effect of the discontinuity on voting outcomes. We find that proxy-voting participation is 30 percentage points higher for firms just included in the Russell 2000 index. This is perhaps not surprising given the higher levels of institutional ownership and fiduciary requirements on voting. Nevertheless, this result speaks directly to the channel through which institutions influence corporate decisions.

Monitoring effects should be larger for firms with more agency costs. When we split the sample, firms with higher expected agency costs primarily drive the results, suggesting payout is related to external monitoring. While we interpret these findings as consistent with a monitoring role by institutions, this hypothesis is hard to separate from an adverse selection or signaling channel. That is, firms with higher institutional ownership may pay more dividends to retain a well informed clientele as in Allen, Bernardo, and Welch (2000). This effect should be stronger for firms with more asymmetric information. However, we find our effect is stronger for firms with higher analyst coverage, not less coverage as information asymmetry motives would predict. Further, we find no connection between media coverage due to index inclusion and corporate

footnote{5} For example see ISS’ “Proxy Voting Summary Guidelines” from 2004 to 2013 and the Proxy Paper Guidelines for Shareholder Initiatives from Glass Lewis & Co.
policy, suggesting that the effects we measure are not likely to be driven by changes in information asymmetry due to index inclusion.

In addition to Grinstein and Michaely (2005), our paper is closely related to other recent studies that examine the relationship between ownership and corporate policies. For example, Michaely, Popadak, and Vincent (2014) find that institutions influence capital structure decisions. Gaspar, Massa, Matos, Patgiri and Rehman (2012) find mixed evidence on the effect of institutions on payout and Desai and Jin (2011) show that plausibly exogenous changes in payout policy cause changes in ownership by “dividend-averse” investors. Perez Gonzales (2003) argues that exogenous changes in tax policy cause firms to cater their payout policy to the tax preferences of their shareholders. ⁶,⁷

Our identification strategy has some distinct advantages. First, the cross sectional variation in institutional ownership near the threshold is driven by mechanical index rules, not by firm fundamentals. This helps rule out both simultaneity bias and omitted variable bias in measuring the effects of ownership on payout. Given the quasi-experimental nature of our research design, we can suggest a more direct causal inference than some other studies. However, our setting has the drawback of only drawing inference from variation in ownership in a small sample of firms near these index thresholds, which may limit external validity of the results to the population of public firms. Furthermore, our research design can only speak to one direction of causality, from institutional ownership to corporate policies. Unlike Grinstein and Michaely (2005), we cannot confirm or rule out the other direction of causality.

Our approach is also distinct from past index inclusion studies (e.g. Pruitt and Wei (1989)). Many index inclusion decisions are based on unobserved and potentially endogenous

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⁶ Hartzell and Starks (2003) and Aghion, Van Reenen and Zingales (2012) use an instrumental variable approach to study causal effects of institutional ownership on other corporate policies, specifically CEO compensation and innovation, respectively.

decision rules. For example, a stock may be included in the S&P500 because of some expected changes in corporate policies or performance, or because institutional investors want to hold it. In addition, while firms recently included in the S&P500 index are observed, the firms that are just outside are not. In contrast, our identification strategy uses variation in institutional ownership for firms both included and excluded from the index based on mechanical rules.

Chang et al. (2014) and Mullins (2014) also examine Russell index inclusion to study differences in returns and compensation, respectively. While they find some results that differ from ours, their results are based on a different method and different sample years. Chang et al. (2014) focus on switchers and find a similar size but noisier estimate of the effect of index inclusion on ownership. Mullins (2014) predicts Russell 2000 index inclusion using a noisy measure of market capitalizations provided by Russell (even though actual inclusion is observed). In this setting, predicting index inclusion for a fixed index size (e.g. only 1,000/2,000 firms make up the Russell 1000/2000) is problematic because any misclassification in indices introduces a bias that weakens the instrument close to the threshold, and may even revert estimated effects, as shown in simulations.8

In work subsequent to ours, Appel, Gormley, and Keim (2014) use the same Russell index setting to examine similar questions and find some results consistent with ours. Unfortunately, their approach violates the basic exclusion restriction because their analysis does not focus on variation close to the threshold, which is arguably the only place where differences are exogenous. The empirical importance of the bias in their approach can be seen in large pretreatment effects in key firm characteristics.9 In our Internet Appendix, we explore these differences in methodology in more detail and show that our approach leads to more robust identification and inference.

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8 We discuss this issue in detail in our Internet Appendix. The issue is also addressed in Appel et. al (2014).
9 They critique our method based on a lack of continuity in the forcing variable, but their critique is misplaced. We present simulations in our Internet Appendix and show no violation of local continuity because true unobserved market capitalizations used by Russell cannot be discontinuous at the threshold. Empirically, we do not find any significant discontinuity in observed market capitalizations in our data.
Our paper makes several contributions. First, we provide evidence that institutional ownership may cause firms to pay out more cash to shareholders. We do not rule out that dividends may also cause changes in ownership, as in Grinstein and Michaely (2005), but we show that, in our setting, institutional ownership from passive investors affects payout. Second, our results suggest that institutions provide better delegated monitoring even if they are not overtly activist investors. Moreover, consistent with Bond, Edmans, and Goldstein (2011), and Grullon, Michenaud, and Weston (2014), we find evidence that capital market frictions, like mechanical index inclusion, can have an important impact on the economic behavior of publicly listed firms. Finally, our paper contributes to the corporate finance literature by providing a setting to identify plausibly exogenous variation in institutional ownership and our study is the first to use Russell index inclusion in a corporate finance setting.

The paper is organized as follows. Section II describes our sample and provides background on the Russell indices. In Section III we discuss our identification strategy. Section IV presents our main empirical results. Section V presents evidence on the connection between ownership and monitoring and Section VI tests for differences in the cross section. Section VII provides a battery of robustness tests and Section VIII concludes.

II. Data and Russell index background

A. Data

Our sample consists of the Russell 1000 and Russell 2000 index constituents from 1991 until 2006. These data are from Russell and are merged with firm level accounting data from Compustat, institutional holdings data from Spectrum 13F filings, and stock return data from CRSP. Our final sample includes 8,307 unique firms from 1991 to 2006. The average number of years for which a firm is in either the Russell 1000 or 2000 in our sample is about 11 years.

Table I presents the summary statistics for our sample. Panel A shows statistics for the Russell 1000 and Panel B shows the results for the Russell 2000. As expected, Russell 1000 firms (which are larger by definition) have a higher institutional ownership and have a higher payout on average. As a result, these firms also have a lower percentage of assets held in cash
and tend to be more profitable with slightly higher leverage. In general, these results are consistent with what we expect given a size-based classification of firms, and are particularly useful for our identification strategy. We will see below that subsequent to index inclusion, at the index threshold, firms that are just in the smaller index (Russell 2000) pay out more of their cash flows than the firms that are just in the larger index (Russell 1000). Therefore our results go against a purely size-based explanation.\(^{10}\)

**B. Russell index background**

The Russell 1000 is a value-weighted index of the largest 1000 U.S. listed firms. The Russell 2000 is a value-weighted index of the next 2000 U.S. firms. There are good economic reasons to expect differences in institutional ownership between the highest weighted firm in the Russell 2000 and the lowest weighted firm in the Russell 1000. The Russell 2000 is the most popular Russell index in terms of dollars benchmarked, meaning more fund managers (and dollars) benchmark to the Russell 2000 index relative to the Russell 1000. The Russell 1000 index competes against the popular S&P500 index for the large firms while the Russell 2000 index faces less competition in mid to small cap stocks. Chang et al. (2014) report that in 2005 the amount of institutional assets benchmarked to the Russell 2000 index was in excess of $200bn while only $90bn tracked the Russell 1000.

In addition, firms just included in the Russell 2000 have a large index weight while firms just included in the Russell 1000 only have trivial portfolio weights. Figure 1 shows the difference in index weights at the threshold. The largest firms in the Russell 2000 are likely to be held by any fund tracking the Russell 2000 (even for actively managed funds) in order to keep tracking error metrics reasonable (Roll, 1992, Wurgler, 2010, Ma, et al., 2014). On the other hand, funds tracking the Russell 1000 could hold none of the smallest firms in the index with little real impact on performance metrics. The combination of the total benchmarked dollars and

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\(^{10}\) We present a detailed discussion of pre-treatment firm characteristics around the discontinuity in section IV.B.
the difference in the relative index weights provides a strong economic motivation for our prediction that institutional investors hold a larger proportion of firms just included in the Russell 2000, and that this increase in institutional ownership is a function not of the individual firms' characteristics, but rather the composition of the benchmarks.

The Russell indices are reconstituted every year following a mechanical rule based on equity prices as of May 31st. The index constituents are determined using market value ranks of the firms at the end of May where market values are determined using closing share price and reported total common shares outstanding. In the event of multiple share classes, Russell uses the market value implied by the share price of the class with the largest float (as defined by a Russell proprietary algorithm). Assignment to the index happens at the end of May, but index weights are determined at the end of June. These index assignments and weights will then persist until the following June.

Russell uses some proprietary methods that are unobservable and may complicate our empirical design. First, Russell uses a proprietary float calculation to determine the primary share class when assigning firms to the index. This float calculation does not influence the shares outstanding used in the index assignment calculation, but rather determines which price to use in the case of multiple classes. They also use an independent data source for shares outstanding. This makes the true market capitalizations used by Russell to rank firms as of May 31st unobservable to the empiricist.

Second, one month after each firm is assigned to an index, Russell assigns index weights, within the assigned index, based on market capitalization at the end of June adjusted for investible shares (e.g. treasury stock, block holders etc.). The investible shares data are considered proprietary by Russell and are not made available to the public or the authors. This adjustment can be large in some cases. Indeed, the float adjustment by Russell and the June return may change the ranks of firms relative to the threshold decision made in May. For example, if two firms were ranked 1000th and 1001st in terms of market cap on May 31st, those firms will be in the Russell 1000 and 2000 index, respectively. However, after weights have been
assigned, those two firms may move away from the threshold. Thus, there is a difference
between the market capitalization used for index assignment and the market capitalization used
for index weights. It is important to note that this float adjustment does not re-assign firms to a
different index; it merely affects the assigned weight once the index has been determined.

Third, starting in 2007 Russell makes an adjustment to index assignment to maintain
consistency in the respective indices. For example, if two firms on the edge of the threshold
switch places in a given year, Russell may leave those firms in their prior year index provided the
market value differential is small. This policy is coined “banding”, and according to Russell was
not applied prior to 2007. In our analysis, we drop all observations after 2006.

The advantage of using the Russell index inclusion as variation in ownership is that the
index rules are generally transparent and mechanical close to the threshold. However, the
adjustments we describe above have the potential to introduce bias and may not be compatible
with simple one stage RD estimation. Our empirical strategy is designed to address these issues,
and we discuss them in detail in section III.

III. Identification

Our identification strategy is to use Russell index inclusion as a source of plausibly
exogenous variation in institutional ownership. In this section, we argue that our instrument is
both relevant to institutional ownership and that it meets the exclusion requirement in the sense
that our instrument is not driven by variation in the corporate policy variables we study. We also
describe our empirical strategy in detail and compare it to other approaches in the literature.

Our underlying assumption is that institutional ownership (IO) varies around the Russell
index threshold because of mechanical weighting differences that are orthogonal to firm
characteristics or potential corporate policy choices. To satisfy this assumption, assignment to an
index cannot be based on corporate policy. But clearly very large firms have different corporate
policies than very small firms, and index assignment is based on firm size. Thus, we need to
focus only on variation in a neighborhood close to the threshold where firms are similar enough so that the variation in $IO$ is plausibly exogenous to the corporate variables under study.

To isolate variation near the index threshold, we follow a method similar in spirit to a regression discontinuity design. However, the Russell index inclusion setting is not perfectly suited to a simple RD design because Russell makes a number of proprietary adjustments to their index construction noted above. As a result, we need to be careful that these adjustments do not invalidate our identification assumptions if they move firms closer to or farther from the threshold once they have been assigned to an index. To remain a valid instrument, we need to ensure that:

i) Russell index assignment is solely a function of market capitalization.

ii) We can identify firms close to the Russell 1000/2000 threshold at the time of index inclusion.

If these conditions are met then our empirical design is consistent with our identifying assumptions and well suited to our approach.

To ensure that condition i) is met, we drop all years after 2006 when Russell instituted its banding policy. This policy was designed to maintain some continuity in the indices. As such, it may violate the exclusion assumption because the selection of firms into the indices is related to characteristics other than market capitalization rankings.

To construct index weights, Russell uses a proprietary adjustment based on the available public float (the number of investable shares) to construct June 30th market capitalization rankings. Firms that were close in market capitalization on May 31st, when index assignment is made, may not be as close in index ranking after Russell makes their adjustment a month later. This difference is subtle, but important because our exclusion assumption depends on firms being otherwise comparable around the threshold. The problem is that, while index assignment is mechanical, the weights Russell uses may be correlated with unobservable firm characteristics due to the float adjustment. For instance, large firms that have a small public float could be family firms that happen to also have low payout for tax reasons that are unrelated to the presence
of institutional investors. In this case, the low institutional ownership would not be exogenous to the payout policy of the firm as both are driven by the fact that the firm is closely held.

To motivate the economic validity of our experiment we present simple univariate results in which we use the June 30th index ranking after the Russell float adjustment. This measure might be contaminated if Russell’s float adjustment is endogenous to the corporate policy variables we study. But, it is the most economically relevant measure for the effects of Russell index inclusion on institutional holdings in a simple and transparent setting. We address all concerns about the Russell float adjustment in our main IV specification. To make sure condition ii) holds, we only use the May 31st unadjusted market capitalization rankings based on data from CRSP as described in section IV.B. This measure is unaffected by the float adjustment, but is noisy with respect to the actual index weights.

We observe actual index assignment, but the proprietary market capitalization that Russell uses is not observed. As a result, Russell’s assignment might be different at the threshold from a prediction based on data from CRSP or Compustat. Dual class status or small differences in shares outstanding in Russell’s data drive these differences. This is not a problem in our design because we use the actual index assignment, not predicted assignment. Nevertheless, these differences can affect the distance to the threshold, but they are generally small and do not meaningfully change the neighborhood of firms around the threshold. As long as these differences are not related to future corporate policy, the errors in rankings within the assigned index will not bias our inference. Appel et al. (2014) argue that the local continuity assumption for RD may be violated because of mechanical differences in observed market caps at the threshold. Empirically we show that there is no statistically significant discontinuity in our data in section VII.B.11

11 Furthermore, a small discontinuity in observed market caps cannot be interpreted as a violation of local continuity in true market caps, as shown in simulations in our Internet Appendix. Observed market caps are a noisy proxy of market caps used by Russell to decide index inclusion. Firms with negative noise must be disproportionately represented next to the R1000 threshold when they are assigned to their actual index while firms with positive noise must be disproportionately represented next to the R2000 threshold.
A standard way of dealing with unobservable forcing variables that is widely accepted in the RD literature (see e.g. Lee and Lemieux (2010)) is to proceed via a “fuzzy” RD design. For example, Mullins (2014) uses this approach and forecasts index assignment (regardless of what index we know Russell actually assigned) using a noisy measure of the true market capitalization provided by Russell themselves. This approach has a subtle, but serious, problem. The treatment and control groups have a fixed sample size (only 1,000/2,000 firms make it to the Russell 1000/2000), so any firm incorrectly assigned to the Russell 2000 in a fuzzy setting must incorrectly assign another firm to the Russell 1000. Rather than simply adding noise, this reverses the sign of the treatment effect for that pair of observations and induces correlation in the errors. Another side effect of the issue is that, at the threshold, the discontinuity in the probability of treatment becomes too small, and creates a weak instrument problem. In our Internet Appendix, we present evidence from Monte Carlo simulations and show that a fuzzy RD provides unreliable estimates in a Russell 1000/2000 index setting: less than 2% of our 5,000 simulations correctly identify statistically significant discontinuities of the correct sign, and 40% of the simulations have point estimates of the wrong sign.

Our approach of using the actual assignment in an IV framework, although less efficient relative to a RD in which the true Russell market capitalization would be perfectly observed, is more appropriate in this setting since it provides estimates with standard errors about two orders of magnitudes smaller than the fuzzy estimates (5.8% vs 603% for a simulated discontinuity of 9%), which the literature argues is the appropriate way of dealing with unobservable forcing variables in the general case. Another benefit of the IV methodology, relative to a fuzzy or sharp RD, is that we can directly attribute differences in corporate policy around the threshold to

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12 Russell provides a noisy measure of their proprietary market capitalization ranking used on May 31st based on the previous year market capitalization rankings. We do not find this data improves the forecasting of index inclusion meaningfully, while it restricts usable data to 4 years (2003-2006). Misclassification of firms to the wrong Russell index remains a severe issue where it matters the most for identification: by construction, about 50% of the firms are predicted to be in the wrong index relative to the true observed Russell index assignment on both sides of the threshold.

13 Chang, Hong, and Liskovich (2014) focus exclusively on additions/deletions in a fuzzy RD design.
institutional ownership (or any alternative economic channels instrumented at the threshold). A fuzzy or sharp RD can only be used as a reduced-form instrument for index inclusion and cannot speak unambiguously to the economic channel that affects the firms’ policies around the threshold.

A final concern with our design is that some firms may have incentives to manipulate their inclusion in the index of their choice at the threshold. Such manipulation would introduce self-selection. However, the difference in size for firms at the threshold is so small that it seems hard to argue they can precisely control their ranking relative to other firms at the threshold, especially if other firms are simultaneously manipulating. Lee (2008) formally shows that even in the presence of manipulation, an exogenous discontinuity still allows for identification of the treatment effect as long as firms do not have precise control over their assignment.

Using Russell index inclusion as source of exogenous variation in $IO$, we can compare policy outcomes in a narrow bandwidth around the threshold as a function of instrumented institutional ownership following Lee and Lemieux (2010) as in:

\[
IO_{i,t+1} = \alpha_i + \tau D_{it} + \delta_1 R_{it} + \delta_2 D_{it} R_{it} + \delta_3 X_{it} + \epsilon_{it} \\
Policy_{i,t+1} = \theta_i + \beta IO_{it} + \gamma_1 R_{it} + \gamma_2 D_{it} R_{it} + \gamma_3 X_{it} + \eta_{it}
\] (1) (2)

Our first stage regression is comparable to a sharp regression discontinuity design with a binary treatment variable, $D_{it}$, which represents inclusion in the Russell 2000 in year $t$. The key to our IV approach is that we identify exogenous variation in $IO$, which we argue exists near the Russell 1000/2000 index inclusion threshold. To identify variation near the threshold, we control for the market capitalization ranking, $R_{it}$, (centered at zero around the threshold) for firm $i$ in year $t$ as well as the interaction ($D_{it} R_{it}$). By including $R_{it}$ and ($D_{it} R_{it}$), we control for the mechanical relationship with market capitalization ranking on either side of the threshold and thus isolate any difference in ownership around index inclusion at the threshold ($R_{it}=0$), controlling for distance to the threshold on either side. Therefore, our instrument is $D_{it}$, conditional on market capitalization ranking, $R_{it}$, and the interaction ($D_{it} R_{it}$), and as such, is excluded from the second stage. Another
standard approach would be to drop these control variables and focus only on a narrow bandwidth close to the threshold; in unreported results, we follow this approach and find similar results. We also include $X_{it}$, a proxy for the float adjustment by Russell. Including this variable ensures that the variation in outcomes we associate with instrumented $IO$ are not driven by variation in expectations of the Russell float adjustment.

In the second stage regression, equation (2), we estimate the effect of instrumented $IO$ on a variety of payout policies. The regression includes instrumented $IO$, and a vector of control variables defined above and included in the first stage. Both regressions also include year fixed effects.

IV. Results

In this section, we present results related to institutional ownership and payout policies. We first present evidence that Russell index assignment does drive differences in institutional ownership. We do this to establish the economic relevance of index inclusion near the threshold as an instrument for ownership. We then test the effects of institutional ownership on corporate policies related to payout using the two-stage least squares approach described in section III.

A. Univariate Results around the Russell index threshold

Russell index inclusion and weights are relevant to institutional ownership because there is a first order economic mechanism that connects them. As discussed earlier, the Russell 2000 is the most popular Russell index in terms of dollars benchmarked. Around the threshold, the largest firms that are just included in the Russell 2000 have index weights 40 times larger than the weights of the smallest firms just included in the Russell 1000. Institutions that benchmark, track, or in any way are compensated based on Russell indices will have an incentive to hold

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14 Our results are robust to inclusion of a variety of control variables such as leverage, profitability, etc.
15 See also Cremers, Ferreira, Matos, and Starks (2013)
stocks with high index weights. As a result, the discontinuity in Russell index weights should drive a discontinuity in institutional ownership.\textsuperscript{16}

We begin by showing simple univariate differences in mean institutional ownership for firms close to the Russell 1000/2000 threshold. For this first set of tests we use the true Russell ranks to define the neighborhood around the threshold. While these ranks are affected by Russell’s potentially endogenous ranking adjustments, these tables provide us with a simple and transparent test of the relevance of index weights on payout policies for the actual firms around the threshold. In Section IV.B we use unadjusted market capitalization ranks to define the neighborhood and correct for any potential endogenous adjustments. Following Thistlethwaite and Campbell (1960), we estimate the regression equation given by:

\begin{equation}
IO_{t,t+1} = \alpha + \tau D_{it} + \epsilon_{it}
\end{equation}

Where $D$ is a dummy variable indicating that the observation is in the Russell 2000. Estimation is by simple OLS with standard errors clustered by firm. In this simple comparison of means setting, construction of the sample window is important because, as we get farther away from the threshold, firms become less comparable to the left and right and the assumption of exogeneity becomes less tenable. While the size of our window is ad hoc, we employ a number of different windows but always stay close-in (within 35 ranks) on either side of the threshold.

Table 2 presents the results of this simple difference in mean institutional ownership around the threshold. Each column represents a different symmetric bandwidth choice, ranging from +/- 25 Russell ranks on either side of the threshold to +/- 35 ranks on either side. The discontinuity is consistent across bandwidth choices and ranges from 11% percent to 13%. These estimates are statistically significant at the ten percent level for the narrowest window and the one percent level for the wider windows. As the bandwidth increases, the significance of the

\textsuperscript{16} We argue the most plausible channel by which index weights affect corporate policies is through institutional ownership. An alternative channel through which other policies may be affected is through increased media exposure/visibility as a result of being included in an index. We test for this alternative in Section VII, but find no evidence of this channel.
estimates increases along with the power of the test. This is reassuring because the standard size effect in institutional ownership (larger firms have more ownership in general) biases against the effect we measure using simple differences. We also see that the difference in institutional ownership is driven by *Quasi Indexers* (as defined by Bushee (1998)). The difference in the ownership by this class of investors is between five and eight percentage points across the threshold.

We find the discontinuity in total institutional ownership is driven by a 8 percentage point difference in the ownership of *Quasi Index* investors and a 3 percentage point difference in *Transient* ownership (at the widest bandwidth). Only the difference in *Quasi Index* is statistically significant. We see a small negative difference in *Dedicated* ownership; however, this estimate is statistically insignificant.

It is important to note that both actively managed *Transient* owners and *Quasi Indexers* are likely to be evaluated relative to a benchmark, including a large number that are benchmarked to the Russell indices. Even if *Transient* owners take active bets on certain stocks, they are still more likely to hold bigger positions in firms with large index weights. Similarly, *Quasi Indexers* face significant tracking error incentives. Our results are consistent with these tracking error incentives and support the idea that index assignment and weight have an economically large impact on ownership around the threshold.17

Finally, Figure 2 presents a graphical analysis for total *Institutional Ownership* following Imbens and Lemieux (2008). In Panel A, we plot average institutional ownership (averaged over bins of 10 Russell ranks) relative to the Russell 1000/2000 threshold. The X-axis represents the distance from the Russell 1000/2000 threshold where 0 represents the smallest firm in the Russell

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17 These results are similar in magnitude to Chang, Hong, and Liskovich (2014) who find a positive but insignificant effect on institutional ownership. The difference in statistical significance is explained by our larger sample size (we do not rely solely on additions/deletions from the Russell 2000). Mullins (2014), finds a negative relationship between index weights and ownership, but we argue in our Internet Appendix that these results should be interpreted with caution.
1000, negative numbers represent larger firms away from the last Russell 1000 rank while positive numbers represent smaller firms just away from the first Russell 2000 index rank.

Institutional ownership is clearly increasing in firm size (the downward sloping nature of the plot). However, at the Russell 1000/2000 threshold we see the slightly smaller firms (the largest firms of the Russell 2000) have much higher institutional ownership. The small firms of the Russell 1000 drive most of the effect. Because these firms make up such a small percentage of that benchmark, the institutions tracking this benchmark have little need to hold these firms, on average. While the actual index weight is a function of the float adjustment made by Russell, it is these weights that drive variation in ownership due to the incentives of institutional managers.

In Figure 2, Panel B, we graph OLS estimates and confidence intervals of institutional ownership around the threshold using a local polynomial estimate. The discontinuity is represented graphically by the difference in the fitted values at the threshold. The magnitudes of the discontinuity can be seen to match our regression estimates and the non-overlapping confidence bands demonstrate the statistical significance of the effect. The recurring theme from our analysis of institutional ownership is that there is a first-order economic difference in ownership in a neighborhood around the Russell index threshold.

Table 3 presents estimates of simple mean differences in payout policies across the threshold estimated with model (3), using the actual Russell ranks. These univariate results suggest that dividends, repurchases, and total payout are all higher to the right of the threshold. These estimates are small but statistically significant at the 10% level at the largest bandwidth. However, even this neighborhood is quite small, resulting in a low powered test. Further, while these findings suggest differences in policy around the threshold, they are reduced form tests and do not establish a direct link to institutional ownership.

Finally, in Table 4, we focus on the voting behavior of shareholders. Voting is, in the end, the real channel by which shareholders directly influence corporate policy. A higher level of voting participation suggests more monitoring, ceteris paribus. To measure voting behavior, we collect data from the ISS Risk Metrics Shareholder Proposal and Vote Results database. We
measure proxy-voting participation at the firm level in the fiscal year following the index inclusion.

The results are presented by column for each different bandwidth. We find that firms that are just included in Russell 2000 have higher voting participation of approximately 30 percentage points. This result is statistically significant at the one percent level for most bandwidths. To correct for any bias from the bounded nature of the proxy voting participation variable (between 0 and 1), Column 2 of Panels A and B present results from logit transformations of participation. The results are qualitatively similar. Our results are consistent with past studies that find institutions vote more actively (Brickley, Lease, and Smith (1988)) and act as substitutes for other governance mechanisms (Gillan and Starks (2005)).

These results are also consistent with anecdotal evidence suggesting that passive funds, such as passive investor giant Vanguard, rely on the threat of voting against management to promote policies aligned with the interests of shareholders. This notion is well described by Glen Booraem, Vanguard’s Fund Controller who suggests voting may only be the tip of the iceberg.18

“By its nature, voting reduces sometimes complex issues to a binary choice—between FOR and AGAINST a particular proposal—making it a rather blunt instrument. In contrast, our engagement with the directors and managers of the companies in which we invest provides us with the opportunity to target feedback and messaging more precisely than voting alone. So while voting is visible, it tells only part of the story. We believe that our active engagement demonstrates that passive investors don’t need to be passive owners. In fact, our involvement in hundreds of direct discussions every year has taught us that we can accomplish as much—if not more—through dialogue than through voting alone. Through engagement, we’re able to put issues on the table for discussion that aren’t on the proxy ballot.”

B. The effect of institutional ownership on corporate policy

In this section we directly test the hypothesis that variation in institutional ownership drives differences in corporate policy. Our results in Tables 2 and 3 are only suggestive of these effects, as they suffer from two potential problems. First, although these results show differences between both institutional ownership and payout around the Russell threshold, they do not directly link the two outcomes. While the most obvious economic channel for the increase in

18 Source: https://personal.vanguard.com/us/insights/article/proxy-commentary-042013
payout is through the direct effect of benchmarking on institutional ownership, we try establish
that connection directly. Secondly, while Table 2 and 3 are suggestive, the results are potentially
confounded by the float adjustments Russell makes to firms between the time they assign them to
the index and the time they assign the actual index weights. It is possible that this float
adjustment is somehow related to future payout differences. Therefore, from now on we use the
May 31st unadjusted market-cap rankings to define the neighborhood around (distance from) the
threshold.

Table 5 presents the two-staged least squares estimates of institutional ownership on firm
policies described in equations (1) and (2). Panel A reports estimation results for a small
bandwidth (+/- 100 firms) around the threshold. This helps alleviate any spurious results driven
by functional form specification problems. Panel B uses a much larger bandwidth (50% of
firms), which increases power but has the potential to confound inference if the distance to the
threshold does not capture the functional form appropriately. For both panels we report the first-
stage estimate on our excluded instrument in the top panel of the table as well as the F-statistic
from the first stage. Across all columns the first stage estimate is both economically and
statistically significant and satisfies the rules of thumb of Stock and Yogo (2005).

Column 1, Panel A, presents the estimate of the effect of instrumented institutional
ownership on \( \text{Ln}(\text{Dividends}) \) in the next year. Dividends are increasing in instrumented \( JIO \). A
one percentage point increase in institutional ownership is associated with a $7 million increase in
dividends that is statistically significant at the one percent level. This represents roughly one-
tenth of the cross-sectional standard deviation in dividends near the threshold.\(^{19}\) This evidence is
consistent with the view that institutional ownership leads to higher dividend payout.

Column 2 of Table 5 presents the results of instrumented institutional ownership on
\( \text{Ln}(\text{Total Payout}) \) in year \( t+1 \) estimated using actual Russell ranks. The coefficient estimate
represents a $1 million increase in total payout for a one percentage point increase in

\(^{19}\) This magnitude is roughly comparable to our univariate results which are essentially a reduced form
estimate of the treatment effect with a unit change in institutional ownership.
instrumented institutional ownership. This result is economically large and statistically significant at the one percent level. We observe smaller effects for share repurchases in $t+1$, presented in column 3. This result is also statistically significant at the one percent level. Our results are robust to using the larger bandwidth. Economically, we see slightly larger results for both total payout and share repurchases and slightly smaller estimates for dividends.

All specifications include controls for the Russell’s float adjustment. This ensures that we control for any systematic difference between firms subject to this adjustment. Our evidence suggests this is merely noise with respect to the outcome variables. We also control for year-fixed effects.

In unreported tests, we also explore whether variation in any specific type of institutional investor drives our results. That is, we construct our instrumental variable using only the variation in dedicated-, quasi-indexers-, or transient- investors. As expected, we find no differences in corporate policy using our instrument for dedicated institutional ownership but large differences for the quasi-indexers likely to use Russell index benchmarks. The magnitudes and significance of the effects we measure are roughly equal between quasi-indexers and transient owners. Taken together, all of our tests point to a causal effect of institutional ownership on dividend payment, share repurchases, and total payout. Institutional shareholders force managers to pay more to shareholders when they become owners of the firms for reasons exogenous to payout policy. This result is consistent with institutional shareholders reducing delegated costs of monitoring (Admati, Pfleiderer, and Zechner, 1994). Our findings suggest that both passive and active institutional investors play a monitoring role, a result that is at odds with the governance literature that generally views passive investors as passive owners. Our findings suggest that this view may be incomplete. In the next section, we explore the channel through which institutional investors, including passive investors, affect payout.

\footnote{Due to log transformation, the increase in share repurchases and dividends sum to less than the increase in total payout.}
V. Monitoring and the agency costs hypothesis

In this section, we test whether firms with higher institutional ownership are subject to increased monitoring by their shareholders. First, we examine other corporate policies that are associated with better monitoring. Finally, we use cross-sectional variation in ex-ante proxies for agency costs to test whether our payout results come from high agency costs firms to further lend credence to the agency cost interpretation of our results.

A. Institutional ownership and other firm characteristics

Next, we test whether firm policies that have been associated with better monitoring and governance are related to exogenous variation in institutional ownership. If higher institutional ownership results in better monitoring, we might expect to observe lower cash holdings (Jensen, 1986), higher investment in R&D (Bushee, 1998), better operating performance (e.g. Bertrand and Mullainathan, 2003), and lower CEO compensation (Bebchuk and Grinstein, 2005, Bebchuk and Fried, 2005). Table 6 presents the effects of instrumented institutional ownership on these policies. These effects are estimated following equations (1) and (2) above.

We do not find any statistically significant effects in our data. These results stand in contrast with Appel et al. (2014) who find strong statistically significant results using a different methodology that we argue selects firms on observable characteristics. An alternative view of these findings is that our instrument is noisy, and only allows for conservative identification of the effects of institutional owners on corporate policies.

VI. Institutional ownership and payout: Cross-sectional evidence

Stable, cash rich, poorly governed firms with low growth opportunities are typically expected to suffer more from agency costs of free cash flow. To test whether the differences we observe in payout are driven by firms with high ex ante expected agency costs, we dig deeper into the cross section of our results.

We rely on four proxies for agency costs. The first proxy is the GIM index (Gompers, Ishii, Metrick, 2003). Firms with high GIM are considered to have a larger number of policies in
place to protect managers at the expense of shareholders. The second proxy is the profitability of
the firm. Better operating performance should be evidence of lower agency costs (e.g. Bertrand
and Mullainathan, 2003). Third, we compare firms with high cash flows and low market-to-book
ratio to firms with low cash flows and high market-to-book (Jensen (1986)). Fourth, we use total
analyst coverage as a proxy for the degree of asymmetric information.

For each variable, we sort firms into two groups based on the median of each measure in
the year prior to the index assignment. We then run our IV analysis including an interaction term
to test for differences in the coefficient on institutional ownership. We present our results for
dividends, but our findings are qualitatively similar for total payout and for share repurchases.

Table 7 presents our results. We find that firms with a high GIM index, low ROA, and
low market-to-book and high cash flow firms drive the effect we observe in the overall sample.
The economic magnitude of the difference between the high agency costs firms and the low
agency costs firms is large and statistically significant. While these results are suggestive in
nature, they are broadly consistent with an agency cost explanations of our findings.

Finally, we also find that firms with higher analyst coverage have slightly lower estimates
for instrumented institutional ownership, though the differences are not statistically significant.
This is suggestive that the institutional ownership effect we observe is not necessarily related to
information asymmetry, but is more likely to be due to reduced agency costs as a result of threats
related to voting. Overall, our results are consistent with institutional investors reducing agency
costs of free cash flows (Jensen, 1986) and provide suggestive evidence of a direct monitoring
channel. Of course, the results could also be consistent with the predictions in Allen, Bernardo,
and Welch (2000) who suggest that firms may seek to attract institutions to monitor them. In
either interpretation, institutions are serving in a monitoring role that mitigates agency costs.

VII. Robustness

In this section, we test that our interpretation of the results is robust to alternative
explanations and whether our results are sensitive to methodological choices.
A. Media Exposure

Our tests in Sections IV and V suggest that institutions have an effect on payout policy through their role as external monitors of corporate behavior. However, there may be other channels by which Russell index inclusion drives differences in corporate policy. One potential channel is through a firm’s visibility with investors. For example, it could be the case that firms just included into the index receive more media coverage. As a result, firms may behave in a manner consistent with monitoring that has nothing to do with institutional holdings.

To test this hypothesis directly, we collect data on news coverage for all of the firms in our sample near the threshold. Our measure of media coverage is total (winsorized at 1%) news stories for a firm in the Factiva database for the 12 months of the Russell index year. We use this measure of media coverage in the first stage of an IV estimation as in Section III. That is, we use Russell index inclusion near the threshold as an instrument for media exposure and test whether instrumented media exposure drives any of the corporate policy variables we study.

Table 8 presents the results of our analysis. We find no significant effect of instrumented news coverage on dividends. In unreported results, we find no differences for any of the other corporate policy variables we study. In short, there does not appear to be any relationship between plausibly exogenous variations in news coverage that is systematically related to variation in corporate policy.

On a related note, we acknowledge it is difficult to rule out a catering interpretation of our results. That is, firms exposed to higher institutional ownership could choose to cater their payout policy to the institutions preferences in the absence of any agency concerns. However, we do not believe these effects are a first-order economic concern. First, in unreported tests, we find no differences in our results for years in which the dividend catering premium of Baker and Wurgler (2004) is high vs. low. Second, Hoberg and Prabhala (2009) find that the dividend catering premium is closely associated with firm risk, and we find no significant effects related to firm risk.
B. Pre-treatment effects

To test whether the index assignment made by Russell creates any obvious selection bias around the threshold, we compare firm characteristics at the index threshold prior to assignment by Russell using rankings based on May 31st observed market capitalizations from CRSP. To test for differences across a variety of firm characteristics, we use the regression methods described in Section III and measure characteristics to the right and the left of the threshold in the year prior to the index assignment.

Table 9 shows the results of these tests. Firms are very similar on both sides of the threshold. The discontinuity tests show that there are no significant discontinuities around the threshold in the prior year, suggesting there is no obvious selection bias near the threshold. Importantly, there are no significant differences in market capitalizations, institutional ownership, or the payout variables we study.

C. Robustness to the Russell weight adjustments

Our analysis in Section III rests on the assumption that Russell index assignment is plausibly exogenous, at least close to the threshold. However, Russell does make some adjustments described in Section II to preserve continuity in index composition (banding) as well as an adjustment the public float based on tradable shares. All IV results are presented using the unadjusted market cap index rankings. While this should provide evidence that the adjustment does not drive our results, we can also look directly at the adjustment itself. Although we cannot directly observe the adjustment, we can easily proxy for it because we observe both the adjusted and the unadjusted weights implied by market capitalization alone.

Given that we can estimate which firms have a large adjustment made by Russell, we can test whether these adjustments are related to firm policies. To measure where Russell has made large adjustments, we simple calculate the difference between the adjusted and unadjusted index weight. Large magnitudes of this difference indicate a large float adjustment. Indeed, we find that some firms with the lowest index weights in the Russell 1000 have large float
adjustments, and are persistently ranked at the lowest ranks of the Russell 1000 index. This may violate the exclusion restriction if this adjustment is correlated with potential outcomes in the case where we use actual ranks. In unreported results, we show that our conclusions are insensitive to these adjustments which are not a significant function of any corporate policy variable.

D. Alternative Specifications and tests

The results presented in Section III are based on a two-stage IV estimation where Russell index inclusion and ranking serve as an instrument for institutional ownership. However, another approach to our tests is to estimate a reduced form regression discontinuity design where we employ a sharp RD framework to test for differences in corporate policy variables as we did for institutional ownership in Section IV.A.

In unreported results, we find that firms just-included in the Russell 2000 pay more dividends, repurchase more shares, and have higher total payout than firms just included in the Russell 1000. We also find results consistent with most of our other tests in that they show higher proxy vote participation.

VIII. Conclusion

In this paper, we exploit a discontinuity in institutional ownership caused by the annual constitution of the Russell 1000 and Russell 2000 indices. We use the discontinuity at the threshold to instrument for institutional ownership and test for differences in dividends, share repurchases, total payout, and other corporate policy variables. We find that higher institutional ownership causes an increase in distribution of cash to shareholders.

Firms prone to agency conflicts tend to drive our results. This suggests that institutional investors play an important role in reducing manager/shareholder conflicts. Our results support Jensen’s (1986) prediction that delegated monitoring causes firms to payout more to shareholders.

In contrast with the existing governance literature that considers passive investors as passive owners, we find evidence suggesting that they are not. The effects we uncover come
largely from passive investors. Institutional investors (active and passive) influence voting participation and outcomes. Maybe equally important, anecdotal evidence suggests passive investors like Vanguard directly engage with management on governance issues and try to influence policies that are not put up for vote. Our findings suggest that passive investors are not passive monitors and may play a significant role influencing the corporate policy and governance choices of the firms they own.
References


# Appendix 1

## Definition of Main Variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Float adjustment</strong></td>
<td>Difference between the rank implied by the market capitalization and the actual rank assigned by Russell in June</td>
</tr>
<tr>
<td><strong>Institutional Ownership</strong></td>
<td>Thomson 13F Shares Held summed across all institutions scaled by CRSP shares outstanding (SHROUT)</td>
</tr>
<tr>
<td><strong>Ln (Cash Holdings)</strong></td>
<td>Ln (1+Cash and Short Term Investment (CHE))</td>
</tr>
<tr>
<td><strong>Ln (Dividends)</strong></td>
<td>Ln (1+Compustat Dividends (DVC+DVP))</td>
</tr>
<tr>
<td><strong>Ln (Repurchases)</strong></td>
<td>Ln(1+Purchase of Common and Preferred Shares (PRSTKC))</td>
</tr>
<tr>
<td><strong>Ln(R&amp;D Expense)</strong></td>
<td>Ln(1+Research and Development Expenses (XRD)) with XRD set to 0 if missing</td>
</tr>
<tr>
<td><strong>Ln (Total Payout)</strong></td>
<td>Ln (1+Compustat Dividends (DVC+DVP) plus Purchase of Common and Preferred Shares (PRSTKC))</td>
</tr>
<tr>
<td><strong>Market-to-Book ratio</strong></td>
<td>Market value of equity (PRCC x CSHPRI) plus total debt (DLC+DLTT) plus preferred stock (PSTKL) minus deferred taxes, all scaled by book value of total assets (AT).</td>
</tr>
<tr>
<td><strong>Market Leverage</strong></td>
<td>Compustat Total Debt (DLC + DLTT) scaled by Market value of equity (PRCC x CSHPRI)</td>
</tr>
<tr>
<td><strong>Market Value</strong></td>
<td>CRSP Price (PRC) multiplied by shares outstanding (SHROUT)</td>
</tr>
<tr>
<td><strong>ROA</strong></td>
<td>Operating Income Before Depreciation (OIBDP) scaled by lagged total assets (AT)</td>
</tr>
</tbody>
</table>
This figure shows the average index weights for firms in the Russell 1000 and firms in the Russell 2000. Firms are assigned to the Russell 1000 or 2000 based on the market cap of the firm at the end of May each year. Index weights are determined by using a float adjusted market cap within each index at the end of June.
Figure 2
Institutional ownership discontinuity

Panel A, and B show the Total Institutional Ownership for the first quarter ending after the reconstitution of the Russell indices for the Russell 3000 firms from 1991-2008. The X-axis represents the distance from the Russell 1000/2000 threshold using the actual Russell ranks in the indices, with 0 representing the last firm in the Russell 1000. Panel A plots the average Total Institutional Ownership over 10 ranks across all years. Panel B adds regression discontinuity estimates and the associated 90% confidence bands following equation (4).
Table 1
Summary statistics

These tables present the summary statistics for firms that belong to the Russell 1000 index (Panel A) and to the Russell 2000 index (Panel B). Variables are defined in Appendix 1.

<table>
<thead>
<tr>
<th>Panel A: Russell 1000</th>
<th>Mean</th>
<th>StdDev</th>
<th>p25</th>
<th>Median</th>
<th>p75</th>
</tr>
</thead>
<tbody>
<tr>
<td>Institutional Ownership</td>
<td>0.65</td>
<td>0.22</td>
<td>0.50</td>
<td>0.67</td>
<td>0.81</td>
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<tr>
<td>ROA</td>
<td>0.16</td>
<td>0.12</td>
<td>0.08</td>
<td>0.14</td>
<td>0.21</td>
</tr>
<tr>
<td>Dividend Yield</td>
<td>0.02</td>
<td>0.02</td>
<td>0.00</td>
<td>0.01</td>
<td>0.03</td>
</tr>
<tr>
<td>Book Leverage</td>
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<td>0.19</td>
<td>0.10</td>
<td>0.24</td>
<td>0.37</td>
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<tr>
<td>Payout/Assets</td>
<td>0.05</td>
<td>0.06</td>
<td>0.01</td>
<td>0.02</td>
<td>0.06</td>
</tr>
<tr>
<td>Repurchases/Assets</td>
<td>0.03</td>
<td>0.05</td>
<td>0.00</td>
<td>0.00</td>
<td>0.03</td>
</tr>
<tr>
<td>Cash/Assets</td>
<td>0.14</td>
<td>0.21</td>
<td>0.02</td>
<td>0.06</td>
<td>0.17</td>
</tr>
<tr>
<td>Total Assets (B$ 2005)</td>
<td>1.06</td>
<td>1.61</td>
<td>0.15</td>
<td>0.39</td>
<td>1.12</td>
</tr>
<tr>
<td>Market Value (B$ 2005)</td>
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<td>11.40</td>
<td>1.55</td>
<td>3.00</td>
<td>6.99</td>
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<tr>
<td>Cash Holdings (M$ 2005)</td>
<td>798</td>
<td>1,409</td>
<td>58</td>
<td>218</td>
<td>753</td>
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<tr>
<td>Payout (M$ 2005)</td>
<td>284</td>
<td>475</td>
<td>17</td>
<td>89</td>
<td>285</td>
</tr>
<tr>
<td>Repurchases (M$ 2005)</td>
<td>148</td>
<td>295</td>
<td>0.00</td>
<td>9.64</td>
<td>129</td>
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<td>Dividends (M$ 2005)</td>
<td>119</td>
<td>201</td>
<td>0.00</td>
<td>34.59</td>
<td>127</td>
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</table>

<table>
<thead>
<tr>
<th>Panel B: Russell 2000</th>
<th>Mean</th>
<th>StdDev</th>
<th>p25</th>
<th>Median</th>
<th>p75</th>
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<tr>
<td>Institutional Ownership</td>
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<td>0.28</td>
<td>0.28</td>
<td>0.50</td>
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<td>ROA</td>
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<td>0.03</td>
<td>0.11</td>
<td>0.19</td>
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<tr>
<td>Dividend Yield</td>
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<td>0.02</td>
<td>0.00</td>
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<tr>
<td>Book Leverage</td>
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<td>0.22</td>
<td>0.02</td>
<td>0.17</td>
<td>0.35</td>
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<tr>
<td>Payout/Assets</td>
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<td>0.05</td>
<td>0.00</td>
<td>0.01</td>
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<td>0.04</td>
<td>0.00</td>
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<tr>
<td>Cash/Assets</td>
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<tr>
<td>Total Assets (B$ 2005)</td>
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<td>0.16</td>
<td>0.39</td>
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</tr>
<tr>
<td>Market Value (B$ 2005)</td>
<td>0.46</td>
<td>0.41</td>
<td>0.177</td>
<td>0.320</td>
<td>0.607</td>
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<tr>
<td>Cash Holdings (M$ 2005)</td>
<td>83</td>
<td>182</td>
<td>10.17</td>
<td>34.4</td>
<td>92</td>
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<tr>
<td>Payout (M$ 2005)</td>
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<td>41</td>
<td>0.00</td>
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<td>14</td>
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<td>Repurchases (M$ 2005)</td>
<td>7.83</td>
<td>28</td>
<td>0.00</td>
<td>0.00</td>
<td>2.65</td>
</tr>
<tr>
<td>Dividends (M$ 2005)</td>
<td>7.42</td>
<td>24</td>
<td>0.00</td>
<td>0.00</td>
<td>6.21</td>
</tr>
</tbody>
</table>
Table 2
Differences in institutional ownership around the Russell 1000/2000 threshold

This table presents the regression discontinuity test results where \( \tau \) is estimated by fitting \( Y_{it} = \alpha + \tau D_{it} + \varepsilon_{it} \), where \( D \) represents a dummy variable equal to one if the firm is in the Russell 2000, in a neighborhood around the Russell 1000/2000 threshold. Column headers indicate the number of actual Russell ranks to each side of the threshold included in the estimate. We estimate this effect within various bandwidths around the threshold from +/- 15 ranks to +/- 35 ranks. We report estimates of \( \tau \) and the t-stats in parentheses. Variables are defined in detailed in Appendix 1. Superscript a, b, and c indicate a significance level of less than 1%, 5%, and 10% respectively. Standard errors are clustered by firm.

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>( \tau_{25} )</th>
<th>( \tau_{30} )</th>
<th>( \tau_{35} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Institutional Ownership(_{r+1})</td>
<td>0.11(^a)</td>
<td>0.12(^a)</td>
<td>0.13(^a)</td>
</tr>
<tr>
<td></td>
<td>(2.74)</td>
<td>(3.23)</td>
<td>(3.48)</td>
</tr>
<tr>
<td>Dedicated Ownership(_{r+1})</td>
<td>-0.01</td>
<td>-0.01</td>
<td>-0.01</td>
</tr>
<tr>
<td></td>
<td>(-0.54)</td>
<td>(-0.47)</td>
<td>(-0.91)</td>
</tr>
<tr>
<td>Quasi Indexer(_{r+1})</td>
<td>0.07(^b)</td>
<td>0.08(^a)</td>
<td>0.08(^a)</td>
</tr>
<tr>
<td></td>
<td>(2.54)</td>
<td>(3.10)</td>
<td>(3.90)</td>
</tr>
<tr>
<td>Transient Ownership(_{r+1})</td>
<td>0.01</td>
<td>0.02</td>
<td>0.03</td>
</tr>
<tr>
<td></td>
<td>(0.64)</td>
<td>(1.07)</td>
<td>(1.36)</td>
</tr>
</tbody>
</table>
Table 3
Differences in payout around the Russell 1000/2000 threshold

This table presents the regression discontinuity test results where $\tau$ is estimated by fitting $Y_t = \alpha + \tau D + \epsilon_t$, where $D$ represents a dummy variable equal to one if the firm is in the Russell 2000. Column headers indicate the number of actual Russell ranks to each side of the threshold included in the estimate. We estimate this effect within various bandwidths around the threshold from +/-15 ranks to +/-35 ranks. We report estimates of $\tau$ and the t-stats in parentheses. Variables are defined in detailed in Appendix 1. Superscript a, b, and c indicate a significance level of less than 1%, 5%, and 10% respectively. Standard errors are clustered by firm.

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>$\tau_{25}$</th>
<th>$\tau_{30}$</th>
<th>$\tau_{35}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\ln(\text{Dividends})_{t+1}$</td>
<td>0.57$^b$</td>
<td>0.65$^b$</td>
<td>0.58$^a$</td>
</tr>
<tr>
<td></td>
<td>(1.94)</td>
<td>(2.44)</td>
<td>(2.53)</td>
</tr>
<tr>
<td>$\ln(\text{Total Payout})_{t+1}$</td>
<td>0.60$^c$</td>
<td>0.61$^b$</td>
<td>0.53$^b$</td>
</tr>
<tr>
<td></td>
<td>(1.91)</td>
<td>(2.24)</td>
<td>(2.25)</td>
</tr>
<tr>
<td>$\ln(\text{Repurchases})_{t+1}$</td>
<td>0.42</td>
<td>0.39$^c$</td>
<td>0.37$^c$</td>
</tr>
<tr>
<td></td>
<td>(1.56)</td>
<td>(1.72)</td>
<td>(1.82)</td>
</tr>
</tbody>
</table>
Table 4
Difference in voting participation around the Russell 1000/2000 threshold

This table presents the regression discontinuity test results where \( \tau \) is estimated by fitting \( Y_{it} = \alpha + \tau D_{it} + \varepsilon_{it} \), where \( D \) represents a dummy variable equal to one if the firm is in the Russell 2000. Column headers indicate the number of actual Russell ranks to each side of the threshold included in the estimate. We estimate this effect within various bandwidths around the threshold from +/- 15 ranks to +/- 35 ranks. We report estimates of \( \tau \) and the t-stats in parentheses. Variables are defined in detailed in Appendix 1. Superscript a, b, and c indicate a significance level of less than 1%, 5%, and 10% respectively. Standard errors are clustered by firm.

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>( \tau^{25} )</th>
<th>( \tau^{30} )</th>
<th>( \tau^{35} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Voting Participation ( t+1 )</td>
<td>0.30(^{a})</td>
<td>0.25(^{a})</td>
<td>0.27(^{a})</td>
</tr>
<tr>
<td></td>
<td>(3.93)</td>
<td>(3.44)</td>
<td>(4.62)</td>
</tr>
<tr>
<td>Logit(Voting Participation) ( t+1 )</td>
<td>1.69(^{a})</td>
<td>1.42(^{a})</td>
<td>1.57(^{a})</td>
</tr>
<tr>
<td></td>
<td>(4.21)</td>
<td>(3.48)</td>
<td>(4.58)</td>
</tr>
</tbody>
</table>
### Table 5

**Institutional ownership and payout: instrumental variable estimates**

This table presents an instrumental variable estimation based on equations (1) and (2). Stage one estimates institutional ownership as a function of the Russell 1000/2000 threshold,

\[
IO_t = \alpha_i + \tau D_{it} + \delta R_{it} + \delta \delta D_{it} R_{it} + \delta \delta R_{it} + \delta \delta X + \varepsilon_t.
\]

The second stage regression presents payout policy variables as a function of instrumented institutional ownership,

\[
Policy_t = \theta + \beta IO_t + \gamma \gamma R_{it} + \gamma \gamma D_{it} R_{it} + \gamma \gamma X + \eta_t.
\]

All results are estimated using ranks implied by the market cap of the firm within the assigned index as of the index assignment date. Panel A presents estimates calculated over +/- 100 ranks from the threshold while Panel B presents estimates over a large bandwidth made up of half the overall sample (+/- 750 firms). The estimation is performed using two stage least squares. First stage control variable estimates are suppressed for brevity. Coefficients are presented and standard errors clustered by firm. Rank variable coefficients are reported per 100 ranks. Variables are defined in detailed in Appendix 1. Superscript a, b, and c indicate a significance level of less than 1%, 5%, and 10% respectively.

#### Panel A: Small Bandwidth

<table>
<thead>
<tr>
<th></th>
<th>First Stage</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>IO_0</td>
<td>IO_1</td>
<td>IO_2</td>
<td></td>
</tr>
<tr>
<td>\tau</td>
<td>8.00^a</td>
<td>9.71^b</td>
<td>9.73^a</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.40)</td>
<td>(4.09)</td>
<td>(4.13)</td>
<td></td>
</tr>
<tr>
<td>F-stat</td>
<td>15.08</td>
<td>14.66</td>
<td>14.78</td>
<td></td>
</tr>
</tbody>
</table>

#### Second Stage

<table>
<thead>
<tr>
<th></th>
<th>Ln(Dividends)_{i,t}</th>
<th>Ln(Total Payout)_{i,t}</th>
<th>Ln(Repurchases)_{i,t}</th>
</tr>
</thead>
<tbody>
<tr>
<td>IO_{i,t}</td>
<td>6.57^b</td>
<td>4.57^c</td>
<td>2.53</td>
</tr>
<tr>
<td></td>
<td>(1.98)</td>
<td>(1.93)</td>
<td>(1.31)</td>
</tr>
<tr>
<td>R_{i,t}</td>
<td>-0.22</td>
<td>-0.15</td>
<td>0.05</td>
</tr>
<tr>
<td></td>
<td>(-0.92)</td>
<td>(-0.73)</td>
<td>(0.29)</td>
</tr>
<tr>
<td>D_{i,t} * R_{i,t}</td>
<td>-0.14</td>
<td>-0.24</td>
<td>-0.40</td>
</tr>
<tr>
<td></td>
<td>(-0.26)</td>
<td>(-0.53)</td>
<td>(-1.05)</td>
</tr>
<tr>
<td>Float Adj_{i,t}</td>
<td>-0.03</td>
<td>0.02</td>
<td>0.03</td>
</tr>
<tr>
<td></td>
<td>(-0.26)</td>
<td>(0.22)</td>
<td>(0.34)</td>
</tr>
<tr>
<td>Year Effects</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Observations</td>
<td>2,667</td>
<td>2,332</td>
<td>2,342</td>
</tr>
<tr>
<td>RMSE</td>
<td>2.50</td>
<td>2.08</td>
<td>1.83</td>
</tr>
</tbody>
</table>

#### Panel B: Large Bandwidth

<table>
<thead>
<tr>
<th></th>
<th>First Stage</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>IO_0</td>
<td>IO_1</td>
<td>IO_2</td>
<td></td>
</tr>
<tr>
<td>\tau</td>
<td>4.34^a</td>
<td>5.19^a</td>
<td>5.19^a</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(5.10)</td>
<td>(6.04)</td>
<td>(6.06)</td>
<td></td>
</tr>
<tr>
<td>F-stat</td>
<td>178.99</td>
<td>164.97</td>
<td>166.10</td>
<td></td>
</tr>
</tbody>
</table>

#### Second Stage

<table>
<thead>
<tr>
<th></th>
<th>Ln(Dividends)_{i,t}</th>
<th>Ln(Total Payout)_{i,t}</th>
<th>Ln(Repurchases)_{i,t}</th>
</tr>
</thead>
<tbody>
<tr>
<td>IO_{i,t}</td>
<td>5.46^a</td>
<td>4.39^a</td>
<td>4.13^a</td>
</tr>
<tr>
<td></td>
<td>(2.45)</td>
<td>(2.73)</td>
<td>(2.80)</td>
</tr>
<tr>
<td>R_{i,t}</td>
<td>-0.22^a</td>
<td>-0.28^a</td>
<td>-0.20^a</td>
</tr>
<tr>
<td></td>
<td>(-9.72)</td>
<td>(-17.54)</td>
<td>(-14.45)</td>
</tr>
<tr>
<td>D_{i,t} * R_{i,t}</td>
<td>0.17^a</td>
<td>0.18^a</td>
<td>0.15^a</td>
</tr>
<tr>
<td></td>
<td>(6.12)</td>
<td>(7.86)</td>
<td>(7.13)</td>
</tr>
<tr>
<td>Float Adj_{i,t}</td>
<td>-0.07</td>
<td>-0.03</td>
<td>-0.09</td>
</tr>
<tr>
<td></td>
<td>(-0.94)</td>
<td>(-0.63)</td>
<td>(1.62)</td>
</tr>
<tr>
<td>Year Effects</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Observations</td>
<td>20,614</td>
<td>18,007</td>
<td>18,056</td>
</tr>
<tr>
<td>RMSE</td>
<td>2.28</td>
<td>1.97</td>
<td>1.99</td>
</tr>
</tbody>
</table>
This table presents an instrumental variable estimation based on equations (1) and (2). Stage one estimates institutional ownership as a function of the Russell 1000/2000 threshold,

\[ IO_i = \alpha_i + \tau R_i + \delta_1 D_i R_i + \delta_2 D_i X_i + \varepsilon_i. \]

The second stage regression presents payout policy variables as a function of instrumented institutional ownership,

\[ Policy_i = \theta + \beta IO_i + \gamma_1 R_i + \gamma_2 D_i R_i + \gamma_3 X_i + \eta_i. \]

All results are estimated using ranks implied by the market cap of the firm within the assigned index as of the index assignment date. The table presents estimates calculated over +/- 100 ranks from the threshold. The estimation is performed using two stage least squares. First stage results are suppressed for brevity. Coefficients are presented and standard errors clustered by firm. Rank variable coefficients are reported per 100 ranks. Variables are defined in detailed in Appendix 1. Superscript a, b, and c indicate a significance level of less than 1%, 5%, and 10% respectively.

<table>
<thead>
<tr>
<th>Small Bandwidth</th>
<th>Ln(R&amp;D) t+1</th>
<th>ROA t+1(%)</th>
<th>Ln(Cash) t+1</th>
<th>Comp/Assets t+1</th>
</tr>
</thead>
<tbody>
<tr>
<td>( IO_{i,t} )</td>
<td>-0.20</td>
<td>6.73</td>
<td>-1.24</td>
<td>0.52</td>
</tr>
<tr>
<td></td>
<td>(-0.09)</td>
<td>(0.45)</td>
<td>(-1.28)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>( R_{i,t} )</td>
<td>-0.01</td>
<td>-0.01</td>
<td>0.17( a )</td>
<td>1.89( a )</td>
</tr>
<tr>
<td></td>
<td>(-0.05)</td>
<td>(-0.78)</td>
<td>(2.71)</td>
<td>(2.66)</td>
</tr>
<tr>
<td>( D_{i,t} * R_{i,t} )</td>
<td>0.04</td>
<td>0.00</td>
<td>-0.26( b )</td>
<td>-2.80</td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
<td>(0.18)</td>
<td>(-2.06)</td>
<td>(-1.24)</td>
</tr>
<tr>
<td>( Float Adj_{i,t} )</td>
<td>0.02</td>
<td>-0.01</td>
<td>0.07( c )</td>
<td>-0.41</td>
</tr>
<tr>
<td></td>
<td>(0.18)</td>
<td>(-1.10)</td>
<td>(1.65)</td>
<td>(-0.81)</td>
</tr>
<tr>
<td>Year Effects</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Observations</td>
<td>2,681</td>
<td>2,516</td>
<td>2,678</td>
<td>1,712</td>
</tr>
<tr>
<td>RMSE</td>
<td>0.01</td>
<td>0.07</td>
<td>0.02</td>
<td>6.56</td>
</tr>
</tbody>
</table>
Table 7
Institutional Ownership and Dividends: Cross Sectional Effects

This table presents an instrumental variable estimation based on equations (1) and (2). Stage one estimates institutional ownership as a function of the Russell 1000/2000 threshold,

\[ IO_t = \alpha + \tau D_t + \delta R_t + \delta D_t R_t + \delta X_t + \varepsilon_t. \]

The second stage regression presents other financial policy variables as a function of instrumented institutional ownership,

\[ Policy_t = \theta \tilde{IO}_t + \beta_1 \tilde{IO}_t (I\text{Agency})_t + \gamma R_t + \gamma D_t R_t + \gamma I\text{Agency}_t + \gamma X_t + \eta_t. \]

All results are estimated using ranks implied by the market cap of the firm within the assigned index as of the index assignment date. Panel A presents estimates calculated over the large bandwidth (+/-750 ranks). The estimation is performed using two stage least squares. First stage results are suppressed for brevity. Coefficients are presented and standard errors clustered by firm. Rank variable coefficients are reported per 100 ranks. Variables are defined in detail in Appendix 1. I\(I\text{Agency}\) a dummy variable equal to one if our measure of agency is above the median for each of our four proxies for agency costs. We include I\(I\text{Agency}\) both alone and interacted with IO. Superscript a, b, and c indicate a significance level of less than 1%, 5%, and 10% respectively.

### Estimate of Institutional ownership on Ln(Dividends)\(_{t+1}\)

<table>
<thead>
<tr>
<th>Agency Measure</th>
<th>G-index</th>
<th>ROA</th>
<th>High CF/ Low MTB</th>
<th>Analyst Coverage</th>
</tr>
</thead>
<tbody>
<tr>
<td>(IO_{i,t})</td>
<td>2.79</td>
<td>7.35</td>
<td>1.34</td>
<td>3.10</td>
</tr>
<tr>
<td></td>
<td>(0.89)</td>
<td>(2.70)</td>
<td>(0.87)</td>
<td>(1.38)</td>
</tr>
<tr>
<td>(IO_{i,t} \ast I\text{Agency}_{i,t})</td>
<td>8.18(^a)</td>
<td>-3.62</td>
<td>5.40(^a)</td>
<td>3.71(^a)</td>
</tr>
<tr>
<td></td>
<td>(2.99)</td>
<td>(3.05)</td>
<td>(2.76)</td>
<td>(3.81)</td>
</tr>
<tr>
<td>(I\text{Agency}_{i,t})</td>
<td>-4.45(^a)</td>
<td>0.98</td>
<td>-2.62(^b)</td>
<td>-0.19(^a)</td>
</tr>
<tr>
<td></td>
<td>(-2.61)</td>
<td>(1.65)</td>
<td>(-2.23)</td>
<td>(-7.31)</td>
</tr>
<tr>
<td>(R_{i,t})</td>
<td>-0.18(^a)</td>
<td>-0.23</td>
<td>-0.27(^a)</td>
<td>0.16(^a)</td>
</tr>
<tr>
<td></td>
<td>(-5.56)</td>
<td>(-10.41)</td>
<td>(-14.34)</td>
<td>(5.10)</td>
</tr>
<tr>
<td>(D_{i,t} \ast R_{i,t})</td>
<td>0.08(^c)</td>
<td>0.17</td>
<td>0.18(^a)</td>
<td>-0.10</td>
</tr>
<tr>
<td></td>
<td>(1.81)</td>
<td>(5.55)</td>
<td>(6.30)</td>
<td>(-1.25)</td>
</tr>
<tr>
<td>(\text{Float Adj}_{i,t})</td>
<td>-0.19</td>
<td>-0.09</td>
<td>-0.03</td>
<td>-1.55(^a)</td>
</tr>
<tr>
<td></td>
<td>(-1.55)</td>
<td>(-1.16)</td>
<td>(-0.49)</td>
<td>(-2.97)</td>
</tr>
</tbody>
</table>

Year Effects       Y       Y       Y       Y
Observations        10,937   17,349  14,582  19,315
RMSE                2.57     2.23    1.92    2.44
Table 8
Media and payout: instrumental variable estimates

This table presents an instrumental variable estimation based on equations (1) and (2). Stage one estimates media coverage as a function of the Russell 1000/2000 threshold,

$$Media_{it} = \alpha_i + \tau D_{it} + \delta_j R_{jt} + \delta_k D_{jt} R_{jt} + \delta_l X_{jt} + \epsilon_{jt}.$$  

The second stage regression presents payout policy variables as a function of instrumented media coverage,

$$Policy_{ij} = \theta_i + \beta_i Media_{it} + \gamma_j R_{jt} + \gamma_k D_{jt} R_{jt} + \gamma_l X_{jt} + \eta_{ij}.$$  

All results are estimated using ranks implied by the market cap of the firm within the assigned index as of the index assignment date. Panel A presents estimates calculated over the entire support of the data while Panel B presents estimates over +/- 100 ranks from the threshold. The estimation is performed using two stage least squares. First stage control variables are suppressed for brevity. Coefficients are presented and standard errors clustered by firm. Rank variable coefficients are reported per 100 ranks. Variables are defined in detailed in Appendix 1. Superscript a, b, and c indicate a significance level of less than 1%, 5%, and 10% respectively.

### Small Bandwidth

<table>
<thead>
<tr>
<th>First Stage</th>
<th>(Ln(Media)_{it})</th>
</tr>
</thead>
<tbody>
<tr>
<td>(\tau)</td>
<td>-0.18</td>
</tr>
<tr>
<td></td>
<td>(-1.32)</td>
</tr>
<tr>
<td>F-stat</td>
<td>15.72</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Second Stage</th>
<th>(Ln(Dividends)_{i,t})</th>
</tr>
</thead>
<tbody>
<tr>
<td>(Ln(Media)_{i,t})</td>
<td>-3.02</td>
</tr>
<tr>
<td></td>
<td>(-1.17)</td>
</tr>
<tr>
<td>(R_{i,t})</td>
<td>0.622</td>
</tr>
<tr>
<td></td>
<td>(0.90)</td>
</tr>
<tr>
<td>(D_{i,t} * R_{i,t})</td>
<td>-1.65</td>
</tr>
<tr>
<td></td>
<td>(-1.15)</td>
</tr>
<tr>
<td>Float Adj (i,t)</td>
<td>0.31^b</td>
</tr>
<tr>
<td></td>
<td>(2.15)</td>
</tr>
<tr>
<td>Year Effects</td>
<td>Y</td>
</tr>
<tr>
<td>Observations</td>
<td>1300</td>
</tr>
<tr>
<td>RMSE</td>
<td>2.96</td>
</tr>
</tbody>
</table>
Table 9
Pre-treatment sample differences at the Russell 1000/2000 threshold

This table presents differences in means at the Russell 1000/2000 threshold of the dependent variables in the year prior to the index assignment. Regression discontinuity test results are presented where τ is estimated by fitting

\[ Y_i = \alpha + \tau D_i + \varepsilon_i, \]

where \( D \) represents a dummy variable equal to one if the firm is in the Russell 2000, in a neighborhood around the Russell 1000/2000 threshold. Column headers indicate the number of Russell ranks to each side of the threshold included in the estimate. We report estimates of \( \tau \) and the t-stats clustered by firm. Variables are defined in detailed in Appendix 1. Superscript a, b, and c indicate a significance level of less than 1%, 5%, and 10% respectively.

<table>
<thead>
<tr>
<th>Dependent Variable ((Y))</th>
<th>Market Cap Ranks</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>( \tau_{25} )</td>
</tr>
<tr>
<td>Market Value(_{t-1})(M$)</td>
<td>15.66</td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
</tr>
<tr>
<td>Institutional Ownership(_{t-1})</td>
<td>-0.00</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
</tr>
<tr>
<td>Ln(Dividends)(_{t-1})</td>
<td>-0.22</td>
</tr>
<tr>
<td></td>
<td>(-1.34)</td>
</tr>
<tr>
<td>Ln(Total Payout)(_{t-1})</td>
<td>-0.28</td>
</tr>
<tr>
<td></td>
<td>(-0.58)</td>
</tr>
<tr>
<td>Ln(Repurchases)(_{t-1})</td>
<td>-0.19</td>
</tr>
<tr>
<td></td>
<td>(-1.15)</td>
</tr>
</tbody>
</table>
In this appendix, we provide a more detailed explanation of the empirical methods in our paper. We also compare and contrast our estimation strategy to other related papers in the literature and provide further evidence of robustness. First, we compare our IV approach with an alternative fuzzy regression discontinuity (RD) where a noisy measure of market capitalization predicts index inclusion. This method has been advocated by Mullins (2014) and Chang, Hong, and Liskovich (2014). In Monte Carlo simulations, we show that this approach is biased in the Russell index setting, while our approach is not. Second, we show that the IV specification advocated by Appel et al. (2014) results in a first-order violation of the exclusion restriction, resulting in a serious sample selection bias. We argue that the empirical approach in our paper successfully avoids subtle but important difficulties in research design using Russell index thresholds.

I. Robustness of our Empirical Design

A. Comparison of the IV approach with a traditional fuzzy RD design

Our identification strategy described in Section III is to use Russell index inclusion near the 1000/2000 threshold as a source of plausibly exogenous variation in institutional ownership and use that variation to instrument for ownership using a standard IV setting. However, as we note in Section III, we encounter some difficulty in our implementation because the index weights used by Russell do not translate exactly into the observed market capitalization rankings. As a result, while we observe which index every stock winds up in, we cannot perfectly predict inclusion in the index based on observed market capitalization.

An alternative approach in the presence of an unobservable threshold, or imperfect treatment assignment, is to proceed via a fuzzy RD design (see e.g. Lee and Lemieux (2010)). For example, Mullins (2014), who is interested in whether index inclusion affects certain firm policies and characteristics, uses this approach. Mullins (2014) forecasts index assignment using a noisy measure of market capitalization. In this specification, IO is a second stage outcome based on instrumented
inclusion in the index. This differs importantly from our analysis where, since we observe actual inclusion in the index, we use it.

The alternative fuzzy RD approach is implemented using two stage least squares. In the case where treatment is defined as inclusion in the Russell 2000, this alternative estimate is based on the following specification:

\[
D_{it} = \alpha_{0l} + \alpha_{1l} R_{it} + \alpha_{0r} T_{it} + \alpha_{1r} R_{it} + X' \beta + \epsilon_{it}
\]

\[
IO_{i,t+1} = \beta_{0l} + \beta_{1l} R_{it} + \beta_{0r} \hat{D}_{it} + \beta_{1r} \hat{D}_{it} R_{it} + Z' \lambda + \alpha_{i} + \eta_{it}
\]

where \(D_{it}\) is the actual treatment (Russell 2000 inclusion) and \(T_{it}\) is the predicted treatment based on market capitalization rankings. As described in Section II, actual treatment might differ from predicted treatment because of Russell’s market capitalization adjustments. The fuzzy RD predicts index inclusion based on noisy market capitalizations, and then uses predicted index inclusion to estimate the effects of index inclusion on \(IO\) in the second stage.

For this fuzzy approach to be valid, there has to be a discontinuous increase in the probability of treatment at the threshold. In our setting, this translates to a discontinuity in predicted index inclusion at the threshold. Unfortunately, we argue that this assumption is violated because of the fixed size of the Russell 2000 and 1000 indices. There can only be 1000 firms the Russell 1000 index, so any error in ranking that incorrectly pushes one firm into the Russell 1000 index simultaneously pushes another firm out.

To see the effect of this fixed order statistic property, consider a simple illustrative example in which only one firm, with true rank 1,000 (i.e. firm #1000) is subject to a negative noise term in its observed market capitalization: its true market capitalization is \(X\), but the empiricist only observes \(X-\gamma\) with \(\gamma > 0\), such that its rank is now predicted to be 1,001 vs. a true rank of 1,000. Because of this small noise in the observed market capitalization, the empiricist ranks firm #1000 below firm #1001 with true ranking 1,001. The empiricist therefore predicts that firm #1000 is part of the Russell 2000 because it is now (wrongly) ranked 1,001. But the Russell 2000 only has 2,000 firms in it, therefore
the empiricist also predicts firm #1001 to be ranked 1,000 and (wrongly) pushes it into the Russell 1000. The error in the observed market capitalization of only one firm results in the misclassification of two firms at the threshold. Rather than simply adding noise, this mistake reverses the predicted treatment for that pair of observations and, because the true index assignment is observed and used to predict index inclusion in the first stage, induces a negative correlation of the error terms in model (3) for both observations. The result is a bias in the estimate of the treatment effect and no discontinuity in the probability of treatment at the threshold because, in expectation, 50% of the firms are misclassified at the threshold.

To see the intuition behind the bias more formally we can use a toy model with only two pairs of observations. Assume we observe \( (X_0, Y_0), (X_1, Y_1) \). In this simple example, consider \( X \) is market capitalization and \( Y \) is institutional ownership. In truth,

\[
\begin{align*}
X_1 - X_0 &= c < 0 \\
Y_1 - Y_0 &= \tau > 0
\end{align*}
\]
That is, \( X_1 \) is smaller than \( X_0 \) by a constant, \( c \), and that \( causes \) \( Y_1 \) to be larger than \( Y_0 \) by the amount of the treatment effect \( \tau \). However, suppose the econometrician only observes \( X \) with error as in:

\[
\begin{align*}
\tilde{X}_1 &= X_1 + \varepsilon_1, \quad \varepsilon_1 \sim iid(0, \sigma_1) \\
\tilde{X}_0 &= X_0 + \varepsilon_0, \quad \varepsilon_0 \sim iid(0, \sigma_0)
\end{align*}
\]

Now, since \( E[\tilde{X}_1 - \tilde{X}_0] = c \), there is no bias introduced in the differences in \( X \), only noise.

However, there is now the chance that we observe \( \tilde{X}_1 - \tilde{X}_0 > 0 \) if \( \varepsilon_1 - \varepsilon_0 > c \). Let

\[
\Pr(\varepsilon_1 - \varepsilon_0 \geq c) = \alpha.
\]

The problem can now be seen when we construct our estimator of the treatment effect with rank order dependent misclassification as:

\[
Z = \begin{cases} 
Y_1 - Y_0 & \text{if } \tilde{X}_1 \leq \tilde{X}_0 \\
Y_0 - Y_1 & \text{if } \tilde{X}_1 > \tilde{X}_0 
\end{cases}
\]

In this case, we mistakenly predict assignment of \( Y_1 \) to the treatment group and \( Y_2 \) to the control group if \( \tilde{X}_1 - \tilde{X}_0 > 0 \). The misclassification forces our predicted assignments to reverse. The key here is that reordering \( X \) reverses the places of \( Y \) when we construct our estimator, as opposed to simply adding noise to both sides as in a traditional fuzzy RD. This leads to a biased estimator of the treatment effect, which is easily shown as:

\[
E[Z] = E(Y_1 - Y_0) \Pr(\tilde{X}_0 > \tilde{X}_1) + E(Y_0 - Y_1) \Pr(\tilde{X}_1 > \tilde{X}_0)
\]

\[
= \tau \Pr(X_0 - X_1 > \varepsilon_1 - \varepsilon_0) - \tau \left[ 1 - \Pr(X_0 - X_1 > \varepsilon_1 - \varepsilon_0) \right]
\]

\[
= \tau \left[ 1 - \Pr(\varepsilon_1 - \varepsilon_0 > c) \right] - \tau \Pr(\varepsilon_1 - \varepsilon_0 > c)
\]

\[
= \tau (1 - \alpha) - \tau \alpha
\]

\[
= \tau (1 - 2\alpha)
\]

Since \( \alpha \in (0, 1) \), then \( -\tau \leq E[Z] \leq \tau \). Thus, the instrument is biased for any \( \alpha < 1 \). As \( c \) approaches zero close to the threshold, then \( \alpha \) might reasonably be in the range of 0.5, in which case
\( E[Z] = 0 \). In finite samples, we could easily measure estimates of \( Z < 0 \). While a full derivation of this bias is beyond the scope of our paper, we provide Monte Carlo simulations to illustrate the impact of the bias in the next section. These simulations also speak to the lack of expected discontinuity in the probability of treatment at the threshold. As argued earlier, a crucial assumption when using a fuzzy RD approach is that the predicted treatment (using stock market capitalization ranks and the true index assignment) helps identify a discontinuous increase in the probability of treatment, \( \hat{D} \), at the threshold. However, because the misclassification must simultaneously affect firms on both sides, the expected discontinuity in treatment at the threshold will be zero. In a sharp discontinuity, the jump in the outcome at the cutoff is the estimate of the causal impact of the treatment. But in a fuzzy RD design, the jump in the outcome is divided by the jump in the probability of treatment at the cutoff to produce the local estimate of the causal impact. Therefore, when the expected jump in the treatment at the cutoff is zero, the estimation of the causal effect becomes problematic.

The basic problem boils down to a weak instrument bias as in Bound, Jaeger, and Baker (1995), Angrist, Imbens, and Rubin (1996), Bartels (1991) and Shea (1993). But the weak instrument problem is local and similar to the issue the empiricist faces when the discontinuity in the probability of treatment is close to zero at the threshold in a fuzzy RD. Using the arguments in Bound et al. (1995) in the special case of one dichotomous instrument the estimated treatment effect can take a large range of values that do not reflect the true treatment.\(^1\) Moreover, given that the identification of the causal effect is at the threshold, \( F \)-tests of instrument strength are misspecified. Predicted market capitalization rank may look like a very strong instrument over the full support because it predicts more than 98% of the actual index inclusions. But at the threshold, it is a weak instrument, which is

---

\(^1\) This estimator is also potentially inconsistent. The instrument for index inclusion may cause spurious inference if the change in the \( \Pr(\text{treatment}) \) is small at the threshold. This will be the case by construction in our setting because misclassification at the threshold becomes a coin toss.
where it really matters for the RD, because around the threshold observed market capitalization ranks have a predictive power close to zero.

The two-stage fuzzy RD estimator may yield biased and inconsistent results. The approach we take in the paper is only subject to noise relative to our sharp IV in which firms’ market capitalization ranks are perfectly observed. The noise affects the location of firms in the neighborhood of treatment. This affects the precision of our estimates, but we argue it will not be subject to bias. To test the empirical relevance of these concerns, and to provide robustness results for our approach, we provide Monte Carlo simulations of the bias and test the discontinuity in the probability of treatment at the threshold.

B. Monte-Carlo simulations

To illustrate problems detailed above, we run Monte-Carlo simulations in a population with a known \( IO \) treatment effect at the threshold. We estimate a simple model to predict \( IO \) as in the first stage of the IV approach we use, and we estimate the fuzzy RD approach using two stage least squares. For simplicity, we only allow a few parameters to affect the simulations. These parameters are detailed in Table I. First, we simulate a set of “true” market capitalizations. Based on these true market capitalizations, we define treated firms (those with ranks above 1000) and simulate institutional ownership as a draw from the cross sectional distribution of \( IO \) in the period 2003 to 2006. For observations that we simulate as treated, we add a treatment effect of 9%. We then simulate observed market capitalizations as true market capitalization plus mean zero noise. We set the standard deviation of the noise in the observed market capitalizations in such a way that a total of 1% of the 3,000 firms in the sample are misclassified in the Russell indices compared to the true observed Russell index inclusion (30 firms in total, or 15 on each side of the threshold, about half of the misclassification we get in our sample).\(^2\) This sets a lower bound on the estimation problems

---

\(^2\) We also allow the misclassification to be 2% in alternative specifications that we present in Table AII. The results are similar to the 1% case. This is not surprising as the estimation is local around the threshold of index
created by mis-classification of firms at the threshold. We then run 5,000 simulations in which we
draw noise for the observed stock market capitalizations and estimate institutional ownership using
both the sharp IV and fuzzy approaches.

Table AII presents the simulated point estimates and t-statistics for estimates of $IO$ for each
of the two different empirical designs. We also report the probability of treatment around the
threshold, and the distribution of observed market capitalizations for both methodologies. Following
the methodology in our paper, we first present results from our sharp IV approach. In Table AII,
panel A, we estimate a mean treatment effect of 9.1% on $IO$. The standard deviation of this point
estimate is 5.8%. T-statistics from these simulations are significant in the vast majority of
simulations and there is only one negative estimate that is statistically significant. Since our estimates
fall close to the true simulated treatment, the level of noise in observed market capitalizations does
not appear to have any meaningful impact on inference in our sharp IV this approach.

In contrast, the fuzzy RD methodology leads to a significant bias. The estimated treatment
effect on IO has the wrong sign in 40% of the 5,000 simulated samples when using a standard fuzzy
RD design (vs. 4% for the sharp IV design). The mean predicted IO under this method is 6% with a
standard deviation of 603% and values ranging between -35,877% and +10,767%. Figure 1 presents
a histogram of the simulation results. Further, t-stats from the estimated treatment effect in the fuzzy
RD are greater than 1.95 in less than 2% of the 5,000 simulations. The standard errors of the point
estimates in the fuzzy RD are two orders of magnitudes higher than in the sharp IV. Even a small
amount of noise in the market capitalization has a major effect on the fuzzy estimator, whereas the
sharp methodology is relatively insensitive to this noise since is uses the true treatment.3

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3 We also run simulations in which we construct no treatment effect on IO for firms in the Russell 2000. In the
sharp setting we see no effect. In the fuzzy setting we see a distribution that looks very similar to the previous
simulations with a true 9% effect. Because of the lack of discontinuity in treatment at the threshold, the
estimates are essentially noise.

---
We also report the probabilities of treatment around the threshold and confirm the intuition that there is no expected discontinuity at the threshold for the reasons we discussed in the previous section. Over the 5,000 simulations, the jump in the probability of treatment from rank 1000 to rank 1001 is only 0.9% (from 50.3% to 51.2%, see Figure 1 Panel F). Given the discrete nature of the rank ordering, this jump in probability is close to the expected 0% in a continuous setting. By construction, in a sharp RD, the jump in the probability of treatment is 100% at the threshold (see Figure 1 Panel E). We attribute the large range of values for the estimated treatment effect in the fuzzy RD to the lack of discontinuity in the probability of treatment at the threshold that is induced by the methodology.

Moreover, we note that the distribution of observed market capitalization around the threshold, as presented in Figure 2 Panel C, is smooth in the case of the fuzzy RD. On the other hand, the distribution of observed market capitalizations in the sharp IV (Figure 2 Panel A) displays a small discontinuity at the threshold. This is expected because the firms just to right of the threshold are subject to a positive noise relative to the true unobserved market capitalization, while firms just to the left of the threshold are subject to negative noise relative to the true unobserved market capitalization. Regardless, the distribution of the true market capitalizations in the sharp IV is perfectly smooth by definition. As long as Russell is including firms in its indices solely based on their market capitalization rankings as of the end of May, then our simulations suggest that small observable discontinuities in observed market capitalizations are not indicative of a violation of the local continuity assumption.\(^4\) This demonstrates that the specification we use in our paper does not violate any assumptions required to make causal inference in this setting (the local continuity assumption) nor does it bias our coefficients, provided that Russell is constructing indices as claimed. Moreover,

\(^4\) The only way in which this issue would be problematic is if the differences in observed market caps and Russell market cap are systematically related to future policies and are large enough that they re-sort firms outside the estimation window. This is unlikely given that the differences are driven by small discrepancies between the shares outstanding used by CRSP vs. Russell.
in our data, we find no significant differences in the observed market caps. These results suggest that criticisms of this framework put forth in Appel et. al. (2014) are without merit. It should be noted that these authors also rely on the assumption that Russell includes firms in indices solely based on a market capitalization mechanical rule.

Overall, the evidence from Monte Carlo simulations shows that a fuzzy RD may lead an empiricist to falsely infer that there are no treatment effects on IO, or even worse, a negative treatment effect on IO in a small sample. The weak instrument problem arising from the fixed order statistic nature of the setting creates a significant bias in fuzzy RD estimates of the treatment effect. As a result, despite some shortcomings, our approach of using the actual assignment in a sharp IV seems more appropriate.

C. Identification through index inclusion only

Appel et al. (2014) also attempt to identify the effects of index inclusion on corporate policies as it relates to quasi-index ownership. We show that, unfortunately, their study cannot speak to the causal effects of ownership because their empirical design leads to endogenous sample selection. Their analysis is focused on two subsets of 250 to 500 firms around the Russell threshold on either side. Because they do not identify the effect of index inclusion on ownership exactly at the threshold (the only place where treatment is plausibly random) their results are confounded by ex-ante differences between firms on either side of the threshold. These biases exist even after controlling for nonlinear size effects and for the float adjustment by Russell.

To calibrate the selection bias caused by the methodology, we replicate their specification in our data. In the first stage, we estimate IO using their large symmetric bandwidths around the

---

5 This suggests that the smoothness of the observed market capitalizations at the threshold is a poor indicator of the validity of the RD design in the Russell setting. The first stage prediction of IO, similar to a sharp RD, estimates the true treatment effect without any biases, unlike the standard fuzzy RD. But the unobservability of the true market capitalizations used by Russell on May 31st of each year to decide index inclusion generates a small discontinuity in observed market capitalizations at the threshold that is solely due to the selection of positive and negative noise terms in observed market capitalizations at the threshold.
threshold using only size and float adjustment as control variables. Using the notation in Appel et al. (2014), we estimate:

\[ IO_{it} = \eta + \lambda R2000_{it} + \sum_{s=1}^{3} \chi_s \left( \ln(Mktcap_{it}) \right) + \sigma \ln(Float_{it}) + \delta_i + \varepsilon_{it} \]

If selection into the treatment group generates exogenous variation in institutional ownership, then instrumented ownership based on predicted values from the first stage, \(\widehat{IO}\), should be orthogonal to past firm characteristics, especially past \(IO\) and market capitalization. If they are not, then instrumented ownership may simply proxy for systematic differences between the treated and untreated samples which would result in classic selection bias. To test this hypothesis, we regress past values of these characteristics on instrumented institutional ownership and size. Using the notation in Appel et al. (2014), we estimate:

\[ \text{Characteristics}_{t-1} = \eta + \beta \widehat{IO}_{t-1} + \sum_{s=1}^{3} \theta_s \left( \ln(Mktcap_{t}) \right) + \gamma \ln(Float_{t}) + \delta_i + \varepsilon_{t} \]

We also present results on quasi-index ownership, \(QIX\), and on firm characteristics lagged two years prior to the treatment. The results presented include the bandwidth of +/-250 firms but results for wider bandwidths are qualitatively similar. We present results for size polynomials of degree 1 but results with higher order polynomials are generally even larger.

The results presented in Table A-III are striking. Treatment firms differ from control firms on a variety of observable firm characteristics in the years prior to index inclusion. When we regress lagged market capitalization, lagged institutional ownership, and lagged quasi-index ownership, we find negative and significant coefficients in all specifications. All these lagged variables are highly correlated with instrumented institutional ownership or quasi-index ownership. This finding suggests that instrumented ownership correlates strongly with firm characteristics in years prior to the index assignment. This systematic correlation with lagged characteristics suggest results in Appel et al. (2014) are not driven by exogenous variation in institutional ownership, but are instead the result of selection on observables in prior periods. Pre-treatment on lagged market capitalization is
particularly worrisome, because it suggests their instrument does not prevent sorting on firm size, which is correlated with most of the outcome variables they study. This is not entirely surprising. By not allowing identification near the threshold and by forcing the slopes on both sides of the threshold to be identical, this method forces the instrument to capture a size effect that contaminates causal inference. Appel et al (2014) argue that, since the assignment to the index is a purely mechanical algorithm based on size, controlling for size is sufficient to ensure their instrument meets the exclusion restriction. However, the mechanical rule is not based on the size of the firm, but rather on the relative size ranking of the firm compared to other firms. Controlling for the market capitalization over such a wide window does not adequately control for pre-treatment effects. As a result, firms differ on many unobserved dimensions that are not directly testable by the empiricist. Their instrument violates the exclusion restriction, and the subsequent validity of their empirical design is questionable. In contrast, our approach does not select firms on observable characteristics and we show in section VII.B of our paper that our empirical design does not suffer from pretreatment effects.
Figure 1 – Monte-Carlo Simulations
These figures present the results of 5,000 simulations, estimating a “true” simulated treatment effect of Russell 2000 inclusion on institutional ownership. The simulated treatment effect is 9%. Panel A shows the estimated treatment effect from the 5,000 simulations under the fuzzy approach. Panel B shows the t-statistics from these simulations. Panel C shows the estimated treatment effect from the 5,000 simulations under the sharp approach and Panel D shows the t-statistics from these simulations. Panel E and F present the Probability of Treatment as a function of predicted market capitalization rank for the sharp approach and the fuzzy approach. Results are presented for simulations that result in 1% of observations misclassified.

Panel A: Fuzzy RD treatment effect estimates

Panel B: Fuzzy RD simulated T-statistics

Panel C: Sharp IV treatment effect estimates

Panel D: Sharp IV simulated T-statistics

Panel E: Sharp IV Pr(Treatment) Simulations

Panel F: Fuzzy RD Pr(Treatment) Simulations
Figure 2 – Observed and True Market Capitalizations in Monte-Carlo Simulations
These figures present the observed market capitalization values as a function of the observed market capitalization ranks and the true market capitalizations as a function of the true market capitalization ranks that we obtain in the 5,000 simulations for the “Sharp” approach and the fuzzy approach. Results are presented for simulations that result in 1% of observations misclassified.

Panel A: Sharp IV Observed Market Capitalization

Panel B: Sharp IV True Market Capitalization

Panel C: Fuzzy RD Observed Market Capitalization

Panel D: Fuzzy RD True Market Capitalization
Table A-I
Monte Carlo Simulations Parameters

Simulation of “True” Parameters

True Market Value:
We simulate true market values for firm $i$, $M_i^{true}$, as draws from a normal distribution $M_i^{true} \sim N(\mu_E, \sigma_E)$ where $\mu_E$ and $\sigma_E$ are chosen to be the mean and standard deviation of the empirical distribution of Market Values observed for the Russell 3000 in 2005.

True Russell 2000 indicator:
We simulate the Russell 2000 indicator as a dummy variable equal to one if the simulated true market capitalization ranking is greater than 1000 as in: $D_i := 1_I (\text{Rank}_{i}^{true}(M_i^{true}) > 1000)$

True Institutional Ownership with true treatment Effect:
We simulate institutional ownership for firm $i$, $IO_i$ as draws from a normal distribution plus a simulated treatment effect of 0.09 if the simulated market capitalization for firm $i$ places it in the Russell 2000 as in: $IO_i = 0 + 0.09D_i + \eta_i$ where $\eta_i \sim N(0, \sigma_{IO})$. We choose $\sigma_{IO}$ to be the empirical cross sectional standard deviation of $IO$ in the Russell 3000 in 2005.

Simulation of “Observed” Parameters

Observed Market Value:
We simulate observed market values by adding a normally distributed error term to the simulated true market values as in $M_i^{obs} = M_i^{true} + \nu_i$ where $\nu_i \sim N(0, \sigma_{\nu})$. Based on our definition of $D_i$, we choose $\sigma_{\nu}$ such that the additional variance in observed market capitalizations results in 1% of our observations being misclassified into the wrong index.

Observed Russell 2000 indicator:
We simulate the observed Russell 2000 indicator as a dummy variable equal to one if the simulated observed market capitalization ranking is greater than 1000 as in: $T_i := 1_O (\text{Rank}_{i}^{obs}(M_i^{obs}) > 1000)$

Estimated Models

Using the simulated data, we estimate both sharp IV and two-stage fuzzy RD models as in:

Sharp IV: $IO_i = \alpha_0 + \tau D_i + \delta R_i + \gamma D_i R_i + \varepsilon_i$

Fuzzy: $D_i = \alpha_0 + \alpha_1 R_i + \alpha_2 T_i + \alpha_3 T_i R_i + \varepsilon_i$

$IO_i = \beta_0 + \tau \hat{D}_i + \beta_1 R_i + \beta_2 \hat{D}_i R_i + \eta_i$
This table presents the results of our Monte Carlo simulations. In Panel A, we present regression discontinuity test results where $\tau$ is estimated by fitting a sharp IV model:

$$IO_{it} = \alpha_0 + \tau D_{it} + \delta R_{it} + \gamma D_{it}R_{it} + \epsilon_{it}$$

$D_{it}$ represents inclusion in the Russell 2000 in year $t$. $R_{it}$ is the market capitalization ranking centered at zero around the threshold for firm $i$ in year $t$. $\tau$ represents the estimated treatment effect at the threshold. In Panel B, we report regression discontinuity test results where $\tau$ is estimated by fitting a two stage fuzzy RD model:

$$D_i = \alpha_0 + \alpha_1 R_i + \alpha_2 T_i + \alpha_3 R_i + \epsilon_i$$

$$IO_{i, t+1} = \beta_{01} + \beta_{11} R_i + \beta_{02} \hat{D}_i + \beta_{12} \hat{D}_i R_i + \eta_i$$

where $T_i$ represents predicted inclusion in the Russell 2000 based on observed market caps. In both simulations, the “true” treatment effect is simulated at 9%. To test the effect of market capitalization misclassification in the Russell 2000 index, we introduce noise into the estimates of market capitalization such that 1% of observations are misclassified. We report simulated distribution of estimates of $\tau$ and the t-stats.

### Panel A: Sharp IV ($\tau = 9\%$, Misclassification = 1%)

<table>
<thead>
<tr>
<th>Estimate</th>
<th>mean</th>
<th>median</th>
<th>Standard deviation</th>
<th>min</th>
<th>max</th>
<th>range</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\tau$</td>
<td>0.09</td>
<td>0.09</td>
<td>0.06</td>
<td>-0.12</td>
<td>0.32</td>
<td>0.44</td>
</tr>
<tr>
<td>t-stats</td>
<td>1.61</td>
<td>1.59</td>
<td>1.02</td>
<td>-1.97</td>
<td>5.88</td>
<td>7.85</td>
</tr>
</tbody>
</table>

### Panel B: Fuzzy RDD ($\tau = 9\%$, Misclassification = 1%)

<table>
<thead>
<tr>
<th>Estimate</th>
<th>mean</th>
<th>median</th>
<th>Standard deviation</th>
<th>min</th>
<th>max</th>
<th>range</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\tau$</td>
<td>0.06</td>
<td>0.09</td>
<td>6.03</td>
<td>-358.77</td>
<td>107.67</td>
<td>466.44</td>
</tr>
<tr>
<td>t-stats</td>
<td>0.26</td>
<td>0.22</td>
<td>0.82</td>
<td>-2.26</td>
<td>3.26</td>
<td>5.53</td>
</tr>
</tbody>
</table>
Table A-III

Pre-treatment effects using in the Appel, Gormley and Keim (2014) methodology

This table presents estimates of pretreatment effects following the methodology in Appel et. al (2014) by estimating the following two-stage regression specification:

\[ IO_{it} = \eta + \lambda R2000_{it} + \sum_{n=1}^{3} \chi_n (\text{Ln}(\text{Mktcap}_{it}))^n + \sigma \text{Ln}(\text{Float}_{it}) + \delta_t + \epsilon_{it} \]

\[ \text{Characteristic}_{it-k} = \eta + \beta IO_{it} + \sum_{n=1}^{3} \theta_n (\text{Ln}(\text{Mktcap}_{it}))^n + \gamma \text{Ln}(\text{Float}_{it}) + \delta_t + \epsilon_{it} \]

Where \( IO_i \) is institutional ownership of firm \( i \) in year \( t \), \( R2000 \) is an indicator for inclusion in the Russell 2000, \( Mktcap \) is market capitalization, \( \text{Float} \) is the float adjustment made by Russell, and \( \text{Characteristic}_{i,t-k} \) is one of the firm characteristics measured at lag \( k \). We also test differences using instrumented quasi-index ownership, \( QIX \). The results presented include the bandwidth of \( \pm 250 \) firms. We present results for size polynomials of degree 1. All regressions include time dummies.

<table>
<thead>
<tr>
<th>Characteristic(_{i,t-k})</th>
<th>(\text{Ln(Mval)}_{1,t})</th>
<th>(\text{Ln(Mval)}_{2,t})</th>
<th>(IO_{t-1})</th>
<th>(IO_{t-2})</th>
<th>(\text{Ln(Mval)}_{1,t-1})</th>
<th>(\text{Ln(Mval)}_{2,t-1})</th>
<th>(QIX_{t-1})</th>
<th>(QIX_{t-2})</th>
</tr>
</thead>
<tbody>
<tr>
<td>Instrumented ( IO_i )</td>
<td>-1.85(^a)</td>
<td>-2.09(^b)</td>
<td>0.74(^a)</td>
<td>0.59(^a)</td>
<td>(-2.45)</td>
<td>(-1.97)</td>
<td>(-5.17)</td>
<td>(-3.11)</td>
</tr>
<tr>
<td>Instrumented ( QIX_i )</td>
<td>-2.15(^a)</td>
<td>-2.37(^b)</td>
<td>0.69(^a)</td>
<td>0.35(^a)</td>
<td>(-2.65)</td>
<td>(-2.06)</td>
<td>(-5.81)</td>
<td>(-2.37)</td>
</tr>
</tbody>
</table>

Control variables included but suppressed for brevity. Clustered t-statistics in parentheses.

\( a \ p<0.01, \ b \ p<0.05, \ c \ p<0.1 \)
Internet Appendix to:
“The Effects of Institutional Ownership on Payout Policy: Evidence from Index Thresholds”
In this appendix, we provide a more detailed explanation of the empirical methods in our paper. We also compare and contrast our estimation strategy to other related papers in the literature and provide further evidence of robustness. First, we compare our IV approach with an alternative fuzzy regression discontinuity (RD) where a noisy measure of market capitalization predicts index inclusion. This method has been advocated by Mullins (2014) and Chang, Hong, and Liskovich (2014). In Monte Carlo simulations, we show that this approach is biased in the Russell index setting, while our approach is not. Second, we show that the IV specification advocated by Appel et al. (2014) results in a first-order violation of the exclusion restriction, resulting in a serious sample selection bias. We argue that the empirical approach in our paper successfully avoids subtle but important difficulties in research design using Russell index thresholds.

I. Robustness of our Empirical Design

A. Comparison of the IV approach with a traditional fuzzy RD design

Our identification strategy described in Section III is to use Russell index inclusion near the 1000/2000 threshold as a source of plausibly exogenous variation in institutional ownership and use that variation to instrument for ownership using a standard IV setting. However, as we note in Section III, we encounter some difficulty in our implementation because the index weights used by Russell do not translate exactly into the observed market capitalization rankings. As a result, while we observe which index every stock winds up in, we cannot perfectly predict inclusion in the index based on observed market capitalization.

An alternative approach in the presence of an unobservable threshold, or imperfect treatment assignment, is to proceed via a fuzzy RD design (see e.g. Lee and Lemieux (2010)). For example, Mullins (2014), who is interested in whether index inclusion affects certain firm policies and characteristics, uses this approach. Mullins (2014) forecasts index assignment using a noisy measure of market capitalization. In this specification, $IO$ is a second stage outcome based on instrumented
inclusion in the index. This differs importantly from our analysis where, since we observe actual inclusion in the index, we use it.

The alternative fuzzy RD approach is implemented using two stage least squares. In the case where treatment is defined as inclusion in the Russell 2000, this alternative estimate is based on the following specification:

$$D_{it} = \alpha_0 + \alpha_1 l + \alpha_2 R_{it} + \alpha_3 L_{it} + \alpha_4 \bar{R}_{it} + X' \beta + \alpha_i + \epsilon_{it}$$

$$IO_{it+1} = \beta_0 + \beta_1 l + \beta_2 l + \beta_3 \bar{D}_{it} + \beta_4 \bar{R}_{it} + Z' \lambda + \alpha_i + \eta_{it}$$

where $D_{it}$ is the actual treatment (Russell 2000 inclusion) and $T_{it}$ is the predicted treatment based on market capitalization rankings. As described in Section II, actual treatment might differ from predicted treatment because of Russell’s market capitalization adjustments. The fuzzy RD predicts index inclusion based on noisy market capitalizations, and then uses predicted index inclusion to estimate the effects of index inclusion on $IO$ in the second stage.

For this fuzzy approach to be valid, there has to be a discontinuous increase in the probability of treatment at the threshold. In our setting, this translates to a discontinuity in predicted index inclusion at the threshold. Unfortunately, we argue that this assumption is violated because of the fixed size of the Russell 2000 and 1000 indices. There can only be 1000 firms the Russell 1000 index, so any error in ranking that incorrectly pushes one firm into the Russell 1000 index simultaneously pushes another firm out.

To see the effect of this fixed order statistic property, consider a simple illustrative example in which only one firm, with true rank 1,000 (i.e. firm #1000) is subject to a negative noise term in its observed market capitalization: its true market capitalization is $X$, but the empiricist only observes $X - \gamma$ with $\gamma > 0$, such that its rank is now predicted to be 1,001 vs. a true rank of 1,000. Because of this small noise in the observed market capitalization, the empiricist ranks firm #1000 below firm #1001 with true ranking 1,001. The empiricist therefore predicts that firm #1000 is part of the Russell 2000 because it is now (wrongly) ranked 1,001. But the Russell 2000 only has 2,000 firms in it, therefore
the empiricist also predicts firm #1001 to be ranked 1,000 and (wrongly) pushes it into the Russell 1000. The error in the observed market capitalization of *only one* firm results in the misclassification of *two* firms at the threshold. Rather than simply adding noise, this mistake *reverses* the predicted treatment for that pair of observations and, because the true index assignment is observed and used to predict index inclusion in the first stage, induces a negative correlation of the error terms in model (3) for both observations. The result is a bias in the estimate of the treatment effect and no discontinuity in the probability of treatment at the threshold because, in expectation, 50% of the firms are misclassified at the threshold.

To see the intuition behind the bias more formally we can use a toy model with only two pairs of observations. Assume we observe \((X_0, Y_0), (X_1, Y_1)\). In this simple example, consider \(X\) is market capitalization and \(Y\) is institutional ownership. In truth,

\[
X_1 - X_0 = c < 0 \\
Y_1 - Y_0 = \tau > 0
\]
That is, \( X_1 \) is smaller than \( X_0 \) by a constant, \( c \), and that causes \( Y_1 \) to be larger than \( Y_0 \) by the amount of the treatment effect \( \tau \). However, suppose the econometrician only observes \( X \) with error as in:

\[
\tilde{X}_1 = X_1 + \varepsilon_1, \quad \varepsilon_1 \sim iid(0, \sigma_1) \\
\tilde{X}_0 = X_0 + \varepsilon_0, \quad \varepsilon_0 \sim iid(0, \sigma_0)
\]

Now, since \( E[\tilde{X}_1 - \tilde{X}_0] = c \), there is no bias introduced in the differences in \( X \), only noise.

However, there is now the chance that we observe \( \tilde{X}_1 - \tilde{X}_0 > 0 \) if \( \varepsilon_1 - \varepsilon_0 > c \). Let

\[
Pr(\varepsilon_1 - \varepsilon_0 \geq c) = \alpha.
\]

The problem can now be seen when we construct our estimator of the treatment effect with rank order dependent misclassification as:

\[
Z = \begin{cases} 
Y_1 - Y_0 & \text{if } \tilde{X}_1 \leq \tilde{X}_0 \\
Y_0 - Y_1 & \text{if } \tilde{X}_1 > \tilde{X}_0 
\end{cases}
\]

In this case, we mistakenly predict assignment of \( Y_1 \) to the treatment group and \( Y_2 \) to the control group if \( \tilde{X}_1 - \tilde{X}_0 > 0 \). The misclassification forces our predicted assignments to reverse. The key here is that reordering \( X \) reverses the places of \( Y \) when we construct our estimator, as opposed to simply adding noise to both sides as in a traditional fuzzy RD. This leads to a biased estimator of the treatment effect, which is easily shown as:

\[
E[Z] = E(Y_1 - Y_0)Pr(\tilde{X}_0 > \tilde{X}_1) + E(Y_0 - Y_1)Pr(\tilde{X}_1 > \tilde{X}_0) \\
= \tau Pr(X_0 - X_1 > \varepsilon_1 - \varepsilon_0) - \tau \left[ 1 - Pr(X_0 - X_1 > \varepsilon_1 - \varepsilon_0) \right] \\
= \tau \left[ 1 - Pr(\varepsilon_1 - \varepsilon_0 > c) \right] - \tau \left[ Pr(\varepsilon_1 - \varepsilon_0 > c) \right] \\
= \tau (1 - \alpha) - \tau \alpha \\
= \tau (1 - 2\alpha)
\]

Since \( \alpha \in (0, 1) \), then \( -\tau \leq E[Z] \leq \tau \). Thus, the instrument is biased for any \( \alpha < 1 \). As \( c \) approaches zero close to the threshold, then \( \alpha \) might reasonably be in the range of 0.5, in which case
\[ E[Z] = 0 \]. In finite samples, we could easily measure estimates of \( Z < 0 \). While a full derivation of this bias is beyond the scope of our paper, we provide Monte Carlo simulations to illustrate the impact of the bias in the next section. These simulations also speak to the lack of expected discontinuity in the probability of treatment at the threshold. As argued earlier, a crucial assumption when using a fuzzy RD approach is that the predicted treatment (using stock market capitalization ranks and the true index assignment) helps identify a discontinuous increase in the probability of treatment, \( \hat{D} \), at the threshold. However, because the misclassification must simultaneously affect firms on both sides, the expected discontinuity in treatment at the threshold will be zero. In a sharp discontinuity, the jump in the outcome at the cutoff is the estimate of the causal impact of the treatment. But in a fuzzy RD design, the jump in the outcome is divided by the jump in the probability of treatment at the cutoff to produce the local estimate of the causal impact. Therefore, when the expected jump in the treatment at the cutoff is zero, the estimation of the causal effect becomes problematic.

The basic problem boils down to a weak instrument bias as in Bound, Jaeger, and Baker (1995), Angrist, Imbens, and Rubin (1996), Bartels (1991) and Shea (1993). But the weak instrument problem is local and similar to the issue the empiricist faces when the discontinuity in the probability of treatment is close to zero at the threshold in a fuzzy RD. Using the arguments in Bound et al. (1995) in the special case of one dichotomous instrument the estimated treatment effect can take a large range of values that do not reflect the true treatment.¹ Moreover, given that the identification of the causal effect is at the threshold, \( F \)-tests of instrument strength are misspecified. Predicted market capitalization rank may look like a very strong instrument over the full support because it predicts more than 98% of the actual index inclusions. But at the threshold, it is a weak instrument, which is

¹ This estimator is also potentially inconsistent. The instrument for index inclusion may cause spurious inference if the change in the \( \Pr(\text{treatment}) \) is small at the threshold. This will be the case by construction in our setting because misclassification at the threshold becomes a coin toss.
where it really matters for the RD, because around the threshold observed market capitalization ranks have a predictive power close to zero.

The two-stage fuzzy RD estimator may yield biased and inconsistent results. The approach we take in the paper is only subject to noise relative to our sharp IV in which firms’ market capitalization ranks are perfectly observed. The noise affects the location of firms in the neighborhood of treatment. This affects the precision of our estimates, but we argue it will not be subject to bias. To test the empirical relevance of these concerns, and to provide robustness results for our approach, we provide Monte Carlo simulations of the bias and test the discontinuity in the probability of treatment at the threshold.

**B. Monte-Carlo simulations**

To illustrate problems detailed above, we run Monte-Carlo simulations in a population with a known $IO$ treatment effect at the threshold. We estimate a simple model to predict $IO$ as in the first stage of the IV approach we use, and we estimate the fuzzy RD approach using two stage least squares. For simplicity, we only allow a few parameters to affect the simulations. These parameters are detailed in Table I. First, we simulate a set of “true” market capitalizations. Based on these true market capitalizations, we define treated firms (those with ranks above 1000) and simulate institutional ownership as a draw from the cross sectional distribution of $IO$ in the period 2003 to 2006. For observations that we simulate as treated, we add a treatment effect of 9%. We then simulate observed market capitalizations as true market capitalization plus mean zero noise. We set the standard deviation of the noise in the observed market capitalizations in such a way that a total of 1% of the 3,000 firms in the sample are misclassified in the Russell indices compared to the true observed Russell index inclusion (30 firms in total, or 15 on each side of the threshold, about half of the misclassification we get in our sample).\(^2\) This sets a lower bound on the estimation problems

\(^2\) We also allow the misclassification to be 2% in alternative specifications that we present in Table AII. The results are similar to the 1% case. This is not surprising as the estimation is local around the threshold of index
created by mis-classification of firms at the threshold. We then run 5,000 simulations in which we draw noise for the observed stock market capitalizations and estimate institutional ownership using both the sharp IV and fuzzy approaches.

Table AII presents the simulated point estimates and t-statistics for estimates of $IO$ for each of the two different empirical designs. We also report the probability of treatment around the threshold, and the distribution of observed market capitalizations for both methodologies. Following the methodology in our paper, we first present results from our sharp IV approach. In Table AII, panel A, we estimate a mean treatment effect of 9.1% on $IO$. The standard deviation of this point estimate is 5.8%. T-statistics from these simulations are significant in the vast majority of simulations and there is only one negative estimate that is statistically significant. Since our estimates fall close to the true simulated treatment, the level of noise in observed market capitalizations does not appear to have any meaningful impact on inference in our sharp IV this approach.

In contrast, the fuzzy RD methodology leads to a significant bias. The estimated treatment effect on IO has the wrong sign in 40% of the 5,000 simulated samples when using a standard fuzzy RD design (vs. 4% for the sharp IV design). The mean predicted IO under this method is 6% with a standard deviation of 603% and values ranging between -35,877% and +10,767%. Figure 1 presents a histogram of the simulation results. Further, t-stats from the estimated treatment effect in the fuzzy RD are greater than 1.95 in less than 2% of the 5,000 simulations. The standard errors of the point estimates in the fuzzy RD are two orders of magnitudes higher than in the sharp IV. Even a small amount of noise in the market capitalization has a major effect on the fuzzy estimator, whereas the sharp methodology is relatively insensitive to this noise since it uses the true treatment.3

inclusion, and as long as there is misclassification at the threshold, the estimation in the two stage approach will be affected by the weak instrument problem. Model (1) is unaffected by misclassification since the actual index assignment is used.

3 We also run simulations in which we construct no treatment effect on IO for firms in the Russell 2000. In the sharp setting we see no effect. In the fuzzy setting we see a distribution that looks very similar to the previous simulations with a true 9% effect. Because of the lack of discontinuity in treatment at the threshold, the estimates are essentially noise.
We also report the probabilities of treatment around the threshold and confirm the intuition that there is no expected discontinuity at the threshold for the reasons we discussed in the previous section. Over the 5,000 simulations, the jump in the probability of treatment from rank 1000 to rank 1001 is only 0.9% (from 50.3% to 51.2%, see Figure 1 Panel F). Given the discrete nature of the rank ordering, this jump in probability is close to the expected 0% in a continuous setting. By construction, in a sharp RD, the jump in the probability of treatment is 100% at the threshold (see Figure 1 Panel E). We attribute the large range of values for the estimated treatment effect in the fuzzy RD to the lack of discontinuity in the probability of treatment at the threshold that is induced by the methodology.

Moreover, we note that the distribution of observed market capitalization around the threshold, as presented in Figure 2 Panel C, is smooth in the case of the fuzzy RD. On the other hand, the distribution of observed market capitalizations in the sharp IV (Figure 2 Panel A) displays a small discontinuity at the threshold. This is expected because the firms just to right of the threshold are subject to a positive noise relative to the true unobserved market capitalization, while firms just to the left of the threshold are subject to negative noise relative to the true unobserved market capitalization. Regardless, the distribution of the true market capitalizations in the sharp IV is perfectly smooth by definition. As long as Russell is including firms in its indices solely based on their market capitalization rankings as of the end of May, then our simulations suggest that small observable discontinuities in observed market capitalizations are not indicative of a violation of the local continuity assumption.4 This demonstrates that the specification we use in our paper does not violate any assumptions required to make causal inference in this setting (the local continuity assumption) nor does it bias our coefficients, provided that Russell is constructing indices as claimed. Moreover,

4 The only way in which this issue would be problematic is if the differences in observed market caps and Russell market cap are systematically related to future policies and are large enough that they re-sort firms outside the estimation window. This is unlikely given that the differences are driven by small discrepancies between the shares outstanding used by CRSP vs. Russell.
in our data, we find no significant differences in the observed market caps.\(^5\) These results suggest that criticisms of this framework put forth in Appel et. al. (2014) are without merit. It should be noted that these authors also rely on the assumption that Russell includes firms in indices solely based on a market capitalization mechanical rule.

Overall, the evidence from Monte Carlo simulations shows that a fuzzy RD may lead an empiricist to falsely infer that there are no treatment effects on IO, or even worse, a negative treatment effect on IO in a small sample. The weak instrument problem arising from the fixed order statistic nature of the setting creates a significant bias in fuzzy RD estimates of the treatment effect. As a result, despite some shortcomings, our approach of using the actual assignment in a sharp IV seems more appropriate.

C. Identification through index inclusion only

Appel et al. (2014) also attempt to identify the effects of index inclusion on corporate policies as it relates to quasi-index ownership. We show that, unfortunately, their study cannot speak to the causal effects of ownership because their empirical design leads to endogenous sample selection. Their analysis is focused on two subsets of 250 to 500 firms around the Russell threshold on either side. Because they do not identify the effect of index inclusion on ownership exactly at the threshold (the only place where treatment is plausibly random) their results are confounded by ex-ante differences between firms on either side of the threshold. These biases exist even after controlling for nonlinear size effects and for the float adjustment by Russell.

To calibrate the selection bias caused by the methodology, we replicate their specification in our data. In the first stage, we estimate IO using their large symmetric bandwidths around the threshold.

\(^5\) This suggests that the smoothness of the observed market capitalizations at the threshold is a poor indicator of the validity of the RD design in the Russell setting. The first stage prediction of IO, similar to a sharp RD, estimates the true treatment effect without any biases, unlike the standard fuzzy RD. But the unobservability of the true market capitalizations used by Russell on May 31\(^{st}\) of each year to decide index inclusion generates a small discontinuity in observed market capitalizations at the threshold that is solely due to the selection of positive and negative noise terms in observed market capitalizations at the threshold.
threshold using only size and float adjustment as control variables. Using the notation in Appel et al. (2014), we estimate:

\[
IO_{it} = \eta + \lambda R2000_{it} + \sum_{s=1}^{3} \chi_s \left( \ln \left( \text{Mktcap}_{it} \right) \right)^{s} + \sigma \ln(\text{Float}_{it}) + \delta + \epsilon_{it}
\]

If selection into the treatment group generates exogenous variation in institutional ownership, then instrumented ownership based on predicted values from the first stage, \(\hat{IO}\), should be orthogonal to past firm characteristics, especially past \(IO\) and market capitalization. If they are not, then instrumented ownership may simply proxy for systematic differences between the treated and untreated samples which would result in classic selection bias. To test this hypothesis, we regress past values of these characteristics on instrumented institutional ownership and size. Using the notation in Appel et al. (2014), we estimate:

\[
\text{Characteristics}_{t-1} = \eta + \beta \hat{IO}_{t} + \sum_{s=1}^{3} \theta_s \left( \ln \left( \text{Mktcap}_{it} \right) \right)^{s} + \gamma \ln(\text{Float}_{it}) + \delta + \epsilon_{t-1}
\]

We also present results on quasi-index ownership, \(QIX\), and on firm characteristics lagged two years prior to the treatment. The results presented include the bandwidth of +-250 firms but results for wider bandwidths are qualitatively similar. We present results for size polynomials of degree 1 but results with higher order polynomials are generally even larger.

The results presented in Table A-III are striking. Treatment firms differ from control firms on a variety of observable firm characteristics in the years prior to index inclusion. When we regress lagged market capitalization, lagged institutional ownership, and lagged quasi-index ownership, we find negative and significant coefficients in all specifications. All these lagged variables are highly correlated with instrumented institutional ownership or quasi-index ownership. This finding suggests that instrumented ownership correlates strongly with firm characteristics in years prior to the index assignment. This systematic correlation with lagged characteristics suggest results in Appel et al. (2014) are not driven by exogenous variation in institutional ownership, but are instead the result of selection on observables in prior periods. Pre-treatment on lagged market capitalization is
particularly worrisome, because it suggests their instrument does not prevent sorting on firm size, which is correlated with most of the outcome variables they study. This is not entirely surprising. By not allowing identification near the threshold and by forcing the slopes on both sides of the threshold to be identical, this method forces the instrument to capture a size effect that contaminates causal inference. Appel et al (2014) argue that, since the assignment to the index is a purely mechanical algorithm based on size, controlling for size is sufficient to ensure their instrument meets the exclusion restriction. However, the mechanical rule is not based on the size of the firm, but rather on the relative size ranking of the firm compared to other firms. Controlling for the market capitalization over such a wide window does not adequately control for pre-treatment effects. As a result, firms differ on many unobserved dimensions that are not directly testable by the empiricist. Their instrument violates the exclusion restriction, and the subsequent validity of their empirical design is questionable. In contrast, our approach does not select firms on observable characteristics and we show in section VII.B of our paper that our empirical design does not suffer from pretreatment effects.
Figure 1 – Monte-Carlo Simulations
These figures present the results of 5,000 simulations, estimating a “true” simulated treatment effect of Russell 2000 inclusion on institutional ownership. The simulated treatment effect is 9%. Panel A shows the estimated treatment effect from the 5,000 simulations under the fuzzy approach. Panel B shows the t-statistics from these simulations. Panel C shows the estimated treatment effect from the 5,000 simulations under the sharp approach and Panel D shows the t-statistics from these simulations. Panel E and F present the Probability of Treatment as a function of predicted market capitalization rank for the sharp approach and the fuzzy approach. Results are presented for simulations that result in 1% of observations misclassified.

Panel A: Fuzzy RD treatment effect estimates
Panel B: Fuzzy RD simulated T-statistics
Panel C: Sharp IV treatment effect estimates
Panel D: Sharp IV simulated T-statistics
Panel E: Sharp IV Pr(Treatment) Simulations
Panel F: Fuzzy RD Pr(Treatment) Simulations
Figure 2 – Observed and True Market Capitalizations in Monte-Carlo Simulations

These figures present the observed market capitalization values as a function of the observed market capitalization ranks and the true market capitalizations as a function of the true market capitalization ranks that we obtain in the 5,000 simulations for the “Sharp” approach and the fuzzy approach. Results are presented for simulations that result in 1% of observations misclassified.

Panel A: Sharp IV Observed Market Capitalization

Panel B: Sharp IV True Market Capitalization

Panel C: Fuzzy RD Observed Market Capitalization

Panel D: Fuzzy RD True Market Capitalization
Table A-I
Monte Carlo Simulations Parameters

Simulation of “True” Parameters

True Market Value:
We simulate true market values for firm \( i \), \( M^\text{True}_i \), as draws from a normal distribution \( M^\text{True}_i \sim N(\mu_E,\sigma_E) \) where \( \mu_E \) and \( \sigma_E \) are chosen to be the mean and standard deviation of the empirical distribution of Market Values observed for the Russell 3000 in 2005.

True Russell 2000 indicator:
We simulate the Russell 2000 indicator as a dummy variable equal to one if the simulated true market capitalization ranking is greater than 1000 as in: \( D_i := 1_{T_i} (\text{Rank}_{T_i}^{\text{True}} (M^\text{True}_i) > 1000) \)

True Institutional Ownership with true treatment Effect:
We simulate institutional ownership for firm \( i \), \( IO_i \), as draws from a normal distribution plus a simulated treatment effect of 0.09 if the simulated market capitalization for firm \( i \) places it in the Russell 2000 as in:
\[
IO_i = 0 + 0.09 D_i + \eta \quad \text{where} \quad \eta \sim N(0,\sigma_{IO})
\]
We choose \( \sigma_{IO} \) to be the empirical cross sectional standard deviation of \( IO \) in the Russell 3000 in 2005.

Simulation of “Observed” Parameters

Observed Market Value:
We simulate observed market values by adding a normally distributed error term to the simulated true market values as in \( M^\text{Obs}_i = M^\text{True}_i + \nu \) where \( \nu \sim N(0,\sigma_{\nu}) \). Based on our definition of \( D_i \), we choose \( \sigma_{\nu} \) such that the additional variance in observed market capitalizations results in 1% of our observations being misclassified into the wrong index.

Observed Russell 2000 indicator:
We simulate the observed Russell 2000 indicator as a dummy variable equal to one if the simulated observed market capitalization ranking is greater than 1000 as in: \( T_i := 1_{O_i} (\text{Rank}_{O_i}^{\text{Obs}} (M^\text{Obs}_i) > 1000) \)

Estimated Models

Using the simulated data, we estimate both sharp IV and two-stage fuzzy RD models as in:

**Sharp IV:**
\[
IO_i = \alpha_0 + \tau D_i + \delta R_i + \gamma D_i R_i + \varepsilon_i
\]

**Fuzzy:**
\[
D_i = \alpha_0 + \alpha_i R_i + \alpha_2 T_i + \alpha_3 T_i R_i + \varepsilon_i
\]
\[
IO_i = \beta_0 + \tau \hat{D}_i + \beta_1 R_i + \beta_2 \hat{D}_i R_i + \eta_i
\]
Table A-II
Monte Carlo Simulations Results

This table presents the results of our Monte Carlo simulations. In Panel A, we present regression discontinuity test results where \( \tau \) is estimated by fitting a sharp IV model:

\[
IO_{it} = \alpha_0 + \tau D_{it} + \delta R_{it} + \gamma D_{it}R_{it} + \epsilon_{it}
\]

\( D_{it} \) represents inclusion in the Russell 2000 in year \( t \). \( R_{it} \) is the market capitalization ranking centered at zero around the threshold for firm \( i \) in year \( t \). \( \tau \) represents the estimated treatment effect at the threshold. In Panel B, we report regression discontinuity test results where \( \tau \) is estimated by fitting a two stage fuzzy RD model:

\[
D_{it} = \alpha_0 + \alpha_1 R_{it} + \alpha_2 T_{it} + \alpha_3 T_{it}R_{it} + \epsilon_{it}
\]

\[
IO_{i,t+1} = \beta_0 + \beta_1 R_{it} + \beta_2 D_{it} + \beta_3 T_{it}D_{it} + \eta_{it}
\]

where \( T_{it} \) represents predicted inclusion in the Russell 2000 based on observed market caps. In both simulations, the “true” treatment effect is simulated at 9%. To test the effect of market capitalization misclassification in the Russell 2000 index, we introduce noise into the estimates of market capitalization such that 1% of observations are misclassified. We report simulated distribution of estimates of \( \tau \) and the t-stats.

### Panel A: Sharp IV (\( \tau = 9\% \), Misclassification = 1%)

<table>
<thead>
<tr>
<th>Estimate</th>
<th>mean</th>
<th>median</th>
<th>Standard deviation</th>
<th>min</th>
<th>max</th>
<th>range</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \tau )</td>
<td>0.09</td>
<td>0.09</td>
<td>0.06</td>
<td>-0.12</td>
<td>0.32</td>
<td>0.44</td>
</tr>
<tr>
<td>t-stats</td>
<td>1.61</td>
<td>1.59</td>
<td>1.02</td>
<td>-1.97</td>
<td>5.88</td>
<td>7.85</td>
</tr>
</tbody>
</table>

### Panel B: Fuzzy RDD (\( \tau = 9\% \), Misclassification = 1%)

<table>
<thead>
<tr>
<th>Estimate</th>
<th>mean</th>
<th>median</th>
<th>Standard deviation</th>
<th>min</th>
<th>max</th>
<th>range</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \tau )</td>
<td>0.06</td>
<td>0.09</td>
<td>6.03</td>
<td>-358.77</td>
<td>107.67</td>
<td>466.44</td>
</tr>
<tr>
<td>t-stats</td>
<td>0.26</td>
<td>0.22</td>
<td>0.82</td>
<td>-2.26</td>
<td>3.26</td>
<td>5.53</td>
</tr>
</tbody>
</table>
Table A-III
Pre-treatment effects using in the Appel, Gormley and Keim (2014) methodology

This table presents estimates of pretreatment effects following the methodology in Appel et. al (2014) by estimating the following two-stage regression specification:

\[
IO_{it} = \eta + \lambda R2000_{it} + \sum_{n=1}^{3} \chi_n (\ln(Mktcap_{it}))^n + \sigma \ln(Float_{it}) + \delta_t + \epsilon_{it}
\]

\[
\text{Characteristic}^{C}_{it-k} = \eta + \beta IO_{it} + \sum_{n=1}^{3} \theta_n (\ln(Mktcap_{it}))^n + \gamma \ln(Float_{it}) + \delta_t + \epsilon_{it}
\]

Where \(IO_i\) is institutional ownership of firm \(i\) in year \(t\), \(R2000\) is an indicator for inclusion in the Russell 2000, \(Mktcap\) is market capitalization, \(Float\) is the float adjustment made by Russell, and \(\text{Characteristic}^{C}_{i,t-k}\) is one of the firm characteristics measured at lag \(k\). We also test differences using instrumented quasi-index ownership, \(QIX\). The results presented include the bandwidth of +250 firms. We present results for size polynomials of degree 1. All regressions include time dummies.

<table>
<thead>
<tr>
<th>Characteristic(_{i,t-k})</th>
<th>(Ln(Mval)_{t-1})</th>
<th>(Ln(Mval)_{t-2})</th>
<th>(IO_{i,t-1})</th>
<th>(IO_{i,t-2})</th>
<th>(Ln(Mval)_{i,t-2})</th>
<th>(Ln(Mval)_{i,t-1})</th>
<th>(QIX_{i,t-1})</th>
<th>(QIX_{i,t-2})</th>
</tr>
</thead>
<tbody>
<tr>
<td>(\text{Instrumented } IO_{i})</td>
<td>-1.85(^a)</td>
<td>-2.09(^b)</td>
<td>0.74(^a)</td>
<td>0.59(^a)</td>
<td>(-2.45)</td>
<td>(-1.97)</td>
<td>(-5.17)</td>
<td>(-3.11)</td>
</tr>
<tr>
<td>(\text{Instrumented } QIX_{i})</td>
<td>-2.15(^a)</td>
<td>-2.37(^b)</td>
<td>0.69(^a)</td>
<td>0.35(^a)</td>
<td>(-2.65)</td>
<td>(-2.06)</td>
<td>(-5.81)</td>
<td>(-2.37)</td>
</tr>
</tbody>
</table>

Control variables included but suppressed for brevity. Clustered t-statistics in parentheses.

\(a\) \(p<0.01\), \(b\) \(p<0.05\), \(c\) \(p<0.1\)