

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

Alan D. Crane

Rice University

Sébastien Michenaud

DePaul University

James P. Weston

Rice University

We show that higher institutional ownership causes firms to pay more dividends. Our identification relies on a discontinuity in ownership around Russell index thresholds. Our estimates indicate that a one-percentage-point increase in institutional ownership causes a \$7 million (8%) increase in dividends. We also find differences in shareholder proposals and voting patterns that suggest that even nonactivist institutions play an important role in monitoring firm behavior. The effect of institutional ownership on dividends is stronger for firms with higher expected agency costs. (*JEL* G23, G30, G34, G35)

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In this paper, we identify a causal effect of institutional ownership on dividend policy based on the composition of the popular Russell indexes. A large discontinuity in Russell index weights drives a substantial difference in institutional ownership (IO) unrelated to firm characteristics or policies like dividends. We exploit this exogenous variation to test the hypothesis that institutional investors have a direct and measurable influence on dividends. Our estimates suggest that a one-percentage-point increase in institutional ownership causes a \$7 million (8%) increase in dividends.

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In perfect capital markets, dividends are irrelevant (Miller and Modigliani 1961). They may even be costly to investors if capital market frictions lead to higher external financing costs or larger tax burdens. Yet in practice, dividends are common and represent a substantial portion of historical equity returns. One explanation for the existence and popularity of dividends is that they help mitigate agency problems. Easterbrook (1984) and Jensen (1986) argue that shareholders will force firms to disgorge discretionary cash to prevent managers from wasting it. Under the constant threat of disciplinary action, managers rationally choose to pay dividends in response to shareholder monitoring.

Institutional shareholders may be especially good monitors. They are more likely to be professional investors with specialized expertise in evaluating firms' financial performance, management quality, and governance. Further, an ownership structure dominated by a few institutions with large positions should lower coordination costs and improve incentives to monitor relative to many small investors holding small positions (Shleifer and Vishny 1986; Admati, Pfleiderer, and Zechner 1994). Active monitoring is also not restricted to traditionally activist investors. Even some passive investors, such as State Street and Vanguard, claim to take an active role in monitoring.¹ Moreover, all institutions have access to, and often follow advice from, proxy advisory services like ISS or Glass-Lewis, who actively monitor firms' policies (Alexander et al. 2010).

As a result of better monitoring, institutional shareholders should pressure firms to pay more dividends to mitigate agency costs. Anecdotally, dividends and share repurchase programs are a common focal point for activist shareholders and proxy advisors who generally favor more payout.² However, it has proven difficult to show that institutions drive dividends because institutions simultaneously choose stocks based on payout. Consistent with this fundamental endogeneity, empirical results are mixed. For example, Grinstein and Michaely (2005) find that institutions choose firms based on payout, but not that they Granger-cause payout. On the other hand, Gaspar et al. (2013) find that long-run-oriented institutions do affect payout. Desai and Jin (2011) and Perez-Gonzales (2003) argue that exogenous changes in tax policy cause firms to adjust their payout policy to the tax preferences of their shareholders.³

Our approach to breaking this endogeneity centers on the rebalancing of the Russell indexes. Each May 31st, Russell indexes are formed based on market capitalization rankings. The largest thousand firms form the Russell 1000, and

¹ For example, see Ross Kerber, "Passive fund manager Vanguard turns activist in some board votes," *Reuters*, September 13, 2013, www.reuters.com/article/2013/09/13/vanguard-proxyvotes-idUSL2N0H00YV20130913.

² For example, see ISS's "Proxy voting summary guidelines" from 2004 to 2013 and the Proxy Paper Guidelines for Shareholder Initiatives from Glass Lewis & Co.

³ In addition, Michaely, Thaler, and Womack (1995) find no institutional ownership changes following dividend omissions, and Brav and Heaton (1998) find a drop in ownership around omissions after the 1974 ERISA regulations. Del Guercio (1996) finds a negative relationship between dividend yields and mutual funds' portfolio choice.

the next two thousand firms make up the Russell 2000. At the 1000/2000 cutoff, differences in capitalization are a tiny fraction of return variance. Since firms cannot control small variations in ranking, index assignment near the threshold is as good as random. This random assignment leads to big differences in value-weighted index weights around the threshold. In 2005, the ten smallest firms in the Russell 1000 had a combined index weight of 0.004%, and the next ten largest firms were in the Russell 2000 with a combined index weight of 2.3%.

The sharp difference in index weights around the 1000/2000 breakpoint drives exogenous variation in IO. Institutions that benchmark against these indexes are more likely to hold big positions in the largest components to reduce tracking error. This is not only true for index funds but also is true for actively managed funds that benchmark against the Russell indexes (Roll 1992; Wurgler 2010; Ma, Tang, and Gomez 2014).

The discontinuity in index weights is a good instrument for IO. Our identifying assumption is that index inclusion near the threshold is exogenous to payout, except through its effect on IO. This assumption is the standard exclusion restriction in an IV setting but can be restated as local continuity in potential outcomes in a regression discontinuity (RD) setting. We assume firms above the index cutoff are similar to those below, except for the exposure to higher IO. This assumption is not testable, but is reasonable because it is based on a mechanical rule with a direct economic channel that drives IO.

Our tests are based on two-stage least-squares specifications in which the first stage models ownership as a function of index inclusion at the threshold and the second stage tests the effect of instrumented ownership on dividends. Thus, our empirical specification is a sharp RD in the first stage and the exogenous variation we identify near the threshold is used to instrument IO in the second stage. Since our estimation exploits exogenous variation in IO, we can make stronger claims about the causal effects of institutional ownership. Given the mixed evidence in the prior literature, clean identification of a causal channel clarifies our understanding of how institutional ownership influences dividend policy.

We find that Russell index inclusion drives a large discontinuity in institutional ownership. Institutional ownership is roughly nine percentage points higher for firms at the top of the Russell 2000 compared to the bottom of the Russell 1000, and this difference is statistically significant. As an instrument, index inclusion meets the relevance requirement and the effect is economically large.

We test the hypothesis that institutional ownership causes an increase in dividends. Our estimates suggest that a one-percentage-point increase in instrumented IO leads to higher dividends of \$7 million, which is roughly one-tenth of the cross-sectional variation in dividends near the threshold, equating to 8% of the median dividends of Russell 1000 firms. These magnitudes are economically significant, and the sensitivities are roughly in line with other studies that document relationships between dividends and taxes (e.g., see

Perez-Gonzales 2003).⁴ It is noteworthy that we find smaller firms just included in the Russell 2000 pay out more than larger firms just to the left of the threshold. This is contrary to the strong correlation ($\rho=0.71$) between size and dividends in the full Russell 3000 sample in which small firms pay out less cash than large firms.

There is some anecdotal evidence consistent with our results. From 2001 to 2005, Columbia Sportswear was a low ranked Russell 1000 firm. In 2006, they fell to the top of the Russell 2000 and their index weight jumped by an order of magnitude. By the end of 2006, they initiated a dividend as a result of investor pressure to increase payout.⁵ While there are many similar examples, our analysis is not based on possibly endogenous switching between indexes. We compare the dividend policy of firms on one side of the threshold to counterfactual firms on the other side. If a firm is at the top of the Russell 2000, but has not changed its index membership, we should not expect a change in its policy, but on average its payout is higher than a counterfactual firm at the bottom of the Russell 1000.

We also test for differences in repurchases and total payout. The effect for repurchases should be a weaker commitment device for mitigating agency costs because, relative to dividends, repurchases respond more to temporary earnings shocks (e.g., see Guay and Harford 2000; Jagannathan, Stephens, and Weisbach 2000). Further, repurchases often arise to reverse the dilution effects of equity compensation (Farre-Mensa, Michaely, and Schmalz 2015), which should be less correlated with monitoring. In our tests, we find some evidence that institutional ownership leads to higher total payout and share repurchases. However, our results are not statistically robust for repurchases and the magnitudes are smaller. At a minimum, we find no evidence that the increases in dividends are undone by repurchase behavior and the causal effect of institutions on total net payout is robustly positive.

Institutional monitoring could come through either the threat of selling (exit) or active engagement, such as voting or direct communication (voice).⁶ Exit is less relevant in our setting, because institutions with benchmarking incentives are less likely to sell. Therefore, we expect to find more active engagement in our sample for all institutions, partly because of their fiduciary responsibility.⁷ In fact, Brickley, Lease, and Smith (1988) find proxy voting is positively correlated

⁴ These estimates compare to a negative and significant effect on the levels of dividends and dividend yields based on ordinary least-squares regressions with raw, uninstrumented IO using the same control variables.

⁵ Helen Jung, "Columbia heeds investors' call for dividends," *The Oregonian*, October 27, 2006.

⁶ See Gillan and Starks (2007), Admati and Pfleiderer (2009), Edmans (2009), and McCahery, Sautner, and Starks (2014).

⁷ Proxy voting is subject to significant regulatory oversight that generally requires funds to vote in the best interests of their clients. Specifically, it is subject to the Employment Retirement Income Security Act's (ERISA) 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, that is, to increase the value of the funds' holdings.

with institutional ownership. Such engagement will be less costly relative to individual investors in part as proxy advisory services help coordinate voting (Alexander et al. 2010; McCahery, Sautner, and Starks 2014). Even index funds appear to use proxy voting as an active governance tool (Iliev and Lowry 2015).

To isolate monitoring by institutions more directly, we test the effect of index inclusion on proxy voting outcomes. If there is more monitoring by institutions for firms at the top of the Russell 2000 index, we should see a larger number of shareholder proposals, especially governance-related proposals, and less support for management proposals. Indeed, we find statistically and economically significant differences. These differences in proposals and voting outcomes are consistent with institutions monitoring firms through the voting process. While we cannot directly observe whether institutions pressure firms to reduce free cash flow, our proxy voting results suggest an agency cost channel.

Next, we dig deeper into the cross-section and test whether our payout results are stronger for firms that benefit the most from monitoring by institutions. We find stronger results for firms for which proxies for expected agency costs are high, such as firms with low profitability, high free cash flow/low growth options, combined CEO and board chairman, and large board size. These measures are imperfect, but they provide additional support to an agency cost interpretation of our findings. While none of our tests directly isolate a free cash flow channel, collectively with the proxy voting evidence they suggest that monitoring by institutions influences firms' dividend policy to mitigate agency costs.⁸

Our study adds to the growing body of research that analyzes the relation between ownership structure and corporate policies. Specifically, we identify a causal link between institutional ownership and cash distributions to shareholders. Given that variation in IO around the Russell index threshold comes from institutions with strong benchmarking incentives, it appears that even owners, such as Vanguard or State Street, who may not be considered traditional activists, can play an important monitoring role. In addition, our study makes an important methodological contribution that can help guide future researchers. A number of other recent studies also use the Russell index inclusion as part of their experimental design.⁹ Because each of these studies uses a different approach, we provide a comprehensive analysis of the underlying econometrics, reconcile conflicting findings, and provide guidance for future research using this experimental design. Finally, our results add

⁸ In Section 4 we investigate whether channels other than monitoring drive our primary results. Overall, we find little support for tax clientele or myopia explanations.

⁹ Madhavan (2003) studies the impact of Russell reconstitution on trading behavior and liquidity, though not in a discontinuity design. Chang, Hong, and Liskovich (2015) study price effects around the threshold. In work subsequent to ours, Mullins (2014) focuses on executive compensation, and Boone and White (2015) study the effect of IO on disclosure. Appel, Gormley, and Keim (Forthcoming) focus on index funds and their effect on corporate governance. Our approach is distinct from past index inclusion studies (e.g., Pruitt and Wei 1989; Aghion, Van Reenen, and Zingales 2012) because inclusion decisions for other indices are often based on unobserved decision rules that may be endogenous to firm performance.

Table 1
Summary statistics

| <i>A. Russell 1000</i> | Mean | SD | p25 | Median | p75 |
|--------------------------------------|-------|-------|-------|--------|-------|
| <i>Total institutional ownership</i> | 0.65 | 0.22 | 0.50 | 0.67 | 0.81 |
| <i>Dedicated ownership</i> | 0.32 | 0.18 | 0.19 | 0.29 | 0.43 |
| <i>Quasi indexer</i> | 0.15 | 0.12 | 0.06 | 0.12 | 0.21 |
| <i>Transient ownership</i> | 0.11 | 0.10 | 0.03 | 0.09 | 0.17 |
| <i>Dividends (M\$ 2005)</i> | 119 | 201 | 0.00 | 34.59 | 127 |
| <i>Dividend yield</i> | 0.02 | 0.02 | 0.00 | 0.01 | 0.03 |
| <i>Repurchases (M\$ 2005)</i> | 148 | 295 | 0.00 | 9.64 | 129 |
| <i>Payout (M\$ 2005)</i> | 284 | 475 | 17 | 89 | 285 |
| <i>Total assets (B\$ 2005)</i> | 1.06 | 1.61 | 0.15 | 0.39 | 1.12 |
| <i>Market value (B\$ 2005)</i> | 7.29 | 11.40 | 1.55 | 3.00 | 6.99 |
| <i>B. Russell 2000</i> | Mean | SD | p25 | Median | p75 |
| <i>Total institutional ownership</i> | 0.52 | 0.28 | 0.28 | 0.50 | 0.73 |
| <i>Dedicated ownership</i> | 0.20 | 0.16 | 0.09 | 0.16 | 0.28 |
| <i>Quasi indexer</i> | 0.11 | 0.11 | 0.02 | 0.07 | 0.15 |
| <i>Transient ownership</i> | 0.10 | 0.11 | 0.02 | 0.07 | 0.14 |
| <i>Dividends (M\$ 2005)</i> | 7.42 | 24 | 0.00 | 0.00 | 6.21 |
| <i>Dividend yield</i> | 0.01 | 0.02 | 0.00 | 0.00 | 0.02 |
| <i>Repurchases (M\$ 2005)</i> | 7.83 | 28 | 0.00 | 0.00 | 2.65 |
| <i>Payout (M\$ 2005)</i> | 15.20 | 41 | 0.00 | 2.19 | 14 |
| <i>Total assets (B\$ 2005)</i> | 0.97 | 1.94 | 0.16 | 0.39 | 1.02 |
| <i>Market value (B\$ 2005)</i> | 0.46 | 0.41 | 0.177 | 0.320 | 0.607 |

These tables present the summary statistics for firms that belong to the Russell 1000 index (panel A) and the Russell 2000 index (panel B). Variables are defined in Table A1.

to a growing body of research investigating the relation between ownership structure and corporate policy.¹⁰

1. Data and Russell Index Background

1.1 Data

Our sample consists of the Russell 1000 and Russell 2000 index constituents from 1991 until 2006. We obtain these data from Russell and merge them 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 1 presents the summary statistics for our sample. Panel A shows statistics for the Russell 1000, and panel B shows results for the Russell 2000. Since Russell 1000 firms are larger by definition, we expected them to have a higher IO and 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. These results are consistent with what we expect given a size-based classification of firms, and are particularly useful for our

¹⁰ See, for example, Bushee (1998), Hartzell and Starks (2003), Cronqvist and Fahlenbrach (2009), and Aghion, Van Reenen, and Zingales (2012).

identification strategy. We show 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.

1.2 Russell index background

The Russell 1000 is a valueweighted index of the largest 1,000 U.S.-listed firms. The Russell 2000 is a valueweighted index of the subsequent 2,000 firms. There are good economic reasons to expect differences in IO between the highest weighted firm in the Russell 2000 and the lowest weighted firm in the Russell 1000. The Russell 2000 is the principal 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&P 500 index for the large firms while the Russell 2000 Index faces less competition in mid to small cap stocks. Chang, Hong, and Liskovich (2015) report that in 2005 the amount of institutional assets benchmarked to the Russell 2000 index was in excess of \$200 billion, while only \$90 billion 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 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, including actively managed funds, tracking the Russell 2000 in order to keep tracking error metrics reasonable (Roll 1992; Wurgler 2010; Ma, Tang, and Gomez 2014). On the other hand, funds tracking the Russell 1000 could exclude the index's smallest firms with no significant/measurable impact on performance metrics. The combination of the total benchmarked dollars and 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 IO is a function not of the individual firms' characteristics but also of the composition of the benchmarks.

The Russell indexes are reconstituted annually following a mechanical rule based on equity prices as of May 31. 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. Firms are assigned to the index at the end of May, but index weights are determined at the end of June. These index assignments and weights hold until the following June.

Russell uses unobservable methods that complicate any empirical design, including ours. First, Russell uses their own float calculation. 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

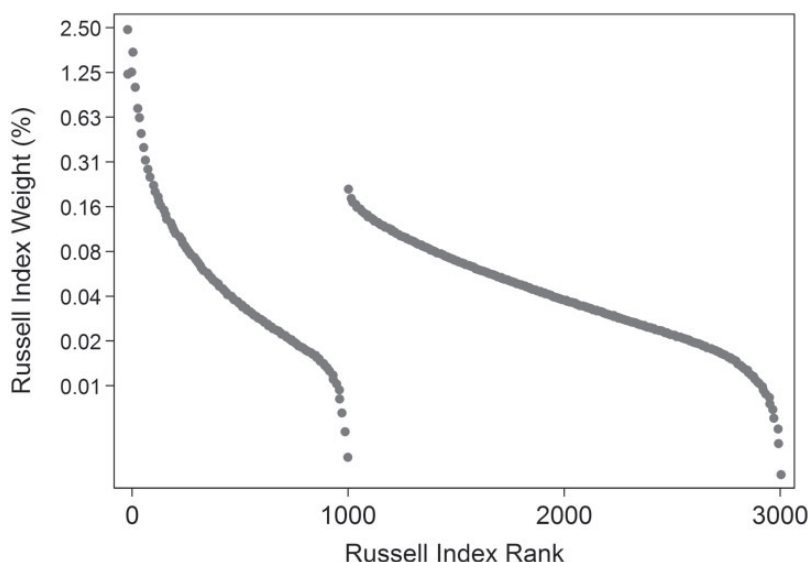


Figure 1

Russell index weights around the threshold

This figure shows the average index weights for firms in the Russell 1000 and the Russell 2000. Firms are assigned to the Russell 1000 or 2000 based on the firm's market capitalization at the end of May each year. Index weights are determined using a float-adjusted market capitalization within each index at the end of June.

of multiple classes. They also use an independent data source for shares outstanding. This makes the market capitalizations used by Russell to rank firms as of May 31 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 investable shares (e.g., treasury stock, block holders). The investable shares data are not publically available. 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 1,000th and 1,001st in terms of market capitalization on May 31, those firms will be in the Russell 1000 and 2000 indexes, respectively. However, after weights have been assigned, those two firms can 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 reassign firms to a different index; it merely affects the assigned weight once the index has been determined.

Third, in 2007 Russell began an adjustment to index assignment to maintain consistency in the respective indexes. 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 Russell index inclusion as variation in IO 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 3.

2. Identification

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

Our underlying assumption is that IO varies around the Russell index threshold because of mechanical weighting differences that are orthogonal to firm characteristics. To satisfy this assumption, assignment to an index cannot be based on payout policy or any determinant of payout policy outside of its effect on index inclusion. However, it is clear that large firms have corporate policies different from 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 in which firms are similar enough so that the variation in IO is plausibly exogenous to the payout 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 adjustments to their index construction as 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

1. Russell index assignment is solely a function of market capitalization rankings, and
2. we can identify firms close to the 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 (1) is met, we drop all years after 2006, when Russell instituted its banding policy. This policy was designed to maintain some continuity in the indexes. As such, it will likely violate the exclusion assumption

because the selection of firms into the indexes 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 the June 30th market capitalization rankings. Firms close in market capitalization on May 31st, when index assignment was made, may not be as close in index ranking after Russell made their adjustment a month later. This difference is 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.

We address these concerns about the Russell float adjustment in our main IV specification. To make sure condition (2) holds, we only use the May 31st unadjusted market capitalization rankings based on data from CRSP. This measure is unaffected by the float adjustment. This measure will be noisy with respect to the actual index weights, which should ultimately drive ownership. Therefore, the measure is likely to bias against our findings.

We observe actual index assignment, but not the market capitalization that Russell uses. As a result, Russell's assignment at the threshold might be different from a prediction based on market capitalization 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 actual index assignment, not predicted assignment. These differences in market capitalization 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 payout policy, the errors in rankings within the assigned index will not bias our inference.

The difference between Russell market capitalization and the one we observe generates a mechanical discontinuity in observed market values around the threshold. However, this does not mean there is a discontinuity in the unobserved market values that Russell uses to assign firms to the treatment group, and we use this index assignment for identification. In our context, ranks are used only as a control variable once actual index assignment is determined by Russell. Whether the mechanical discontinuity is large enough to cause specification errors is an empirical question, but it does not directly confound identification. In Section 7.1 and our Internet Appendix, we present a detailed analysis of this issue, compare our approach to other recent studies, and assert that it is unlikely to be a problem.

A final concern with our design is that some firms could 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 a source of exogenous variation in IO, we can compare policy outcomes in a narrow bandwidth around the threshold (e.g., 100 observations) as a function of instrumented IO following Lee and Lemieux (2010):

$$IO_{i,t} = \alpha_t + \tau Russell2000_{it} + \delta_1(Rank_{it}^* - 1000) + \delta_2 Russell2000_{it}(Rank_{it}^* - 1000) + \delta_3 FloatAdj_{it} + \varepsilon_{it}, \quad (1)$$

$$Policy_{i,t} = \theta_t + \beta \hat{IO}_{it} + \gamma_1(Rank_{it}^* - 1000) + \gamma_2 Russell2000_{it}(Rank_{it}^* - 1000) + \gamma_3 FloatAdj_{it} + \eta_{it}. \quad (2)$$

Our first-stage regression is comparable to a sharp regression discontinuity design with a binary treatment variable, $Russell2000_{it}$, that represents inclusion in the Russell 2000 in year t . This analysis is restricted to firms in either the Russell 1000 or the Russell 2000. The key to our 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 distance to the threshold of observed market capitalization rankings, $(Rank_{it}^* - 1000)$, for firm i in year t , as well as for the interaction $Russell2000_{it}(Rank_{it}^* - 1000)$. For the rank variables, year t refers to the market capitalization ranking at the time of assignment, which holds for the next 12 months. By including $(Rank_{it}^* - 1000)$ and $Russell2000_{it}(Rank_{it}^* - 1000)$, 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, where $(Rank_{it}^* - 1000) = 0$.¹¹ Therefore, our instrument is $Russell2000$, conditional on market capitalization ranking, $(Rank_{it}^* - 1000)$, and the interaction $Russell2000_{it}(Rank_{it}^* - 1000)$, and as such, it is excluded from the second stage. We also include $FloatAdj_{it}$, a proxy for the float adjustment by Russell, computed as the difference between the rank implied by the May 31st market capitalization and the actual rank assigned by Russell in June. Including this variable ensures that we control for the variation in index weights caused by Russell's float adjustment made at the end of June.

In the second-stage regression, Equation (2), we estimate the effect of instrumented IO on a variety of payout policies. IO is measured in the next available quarter after index assignment in year t . The policy variables are all measured in the next available fiscal year-end after the year of index assignment.

¹¹ We use the distance from the threshold rather than the raw rank so that this term is equal to zero at the threshold. This transformation ensures that τ represents the treatment effect at the threshold.

The regression includes instrumented IO and the control variables defined above and included in the first stage.¹² Both regressions also include year fixed effects.

There is some confusion in the literature about how best to exploit the Russell index setting and whether to use an IV or fuzzy RD estimation. Our approach exploits a sharp RD specification in the first stage to isolate exogenous variation near the threshold and then uses that exogenous variation as an instrument in the second stage.¹³ The discontinuity in index weights around the threshold provides a source of exogenous variation in institutional ownership. In the RD setting, this variation is exploited as a continuous treatment variable, and the effect of exogenous IO is identified through the assumption that counterfactual firms on the other side of the threshold would have had the same outcomes, except the exposure to higher IO (local continuity). In an IV setting, the index inclusion provides an instrument for institutional ownership as long as the variation in IO is orthogonal to the outcome variables (exclusion restriction). In either case, movement away from the threshold, or failure to properly condition on the functional form of the forcing variable, can introduce violations of local continuity or exclusion. The underlying set of assumptions required for robust causal inference is essentially the same. We believe this setting and our empirical specification lead to valid assumptions for causal inference.

3. Results

In this section, we present results related to IO and payout policies. We first present evidence that Russell index assignment drives differences in IO to establish the economic relevance of index inclusion near the threshold as an instrument for ownership. We then test the effects of IO on payout using the two-stage least-squares approach described in Section 2.

3.1 Institutional ownership 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 (see also Cremers et al. Forthcoming). Around the threshold, the largest firms just included in the Russell 2000 have index weights forty times larger than the smallest firms just included in the Russell 1000. Institutions that benchmark, track, or compensate managers based on performance relative to the Russell indexes have an incentive to hold stocks

¹² Our results are robust to the inclusion of book leverage, profitability, annual stock return, and market capitalization as additional control variables. While the nature of our experimental design reduces the need for additional control variables, it is reassuring that our results are not affected by their inclusion.

¹³ Part of the confusion in the literature comes from differences in terminology. Our approach is similar to what Lee and Lemieux (2010) label as fuzzy RD with a continuous treatment variable. Angrist and Pischke (2009) argue that a fuzzy RD is just an IV estimation. We argue such definitions are mostly labeling differences in this setting.

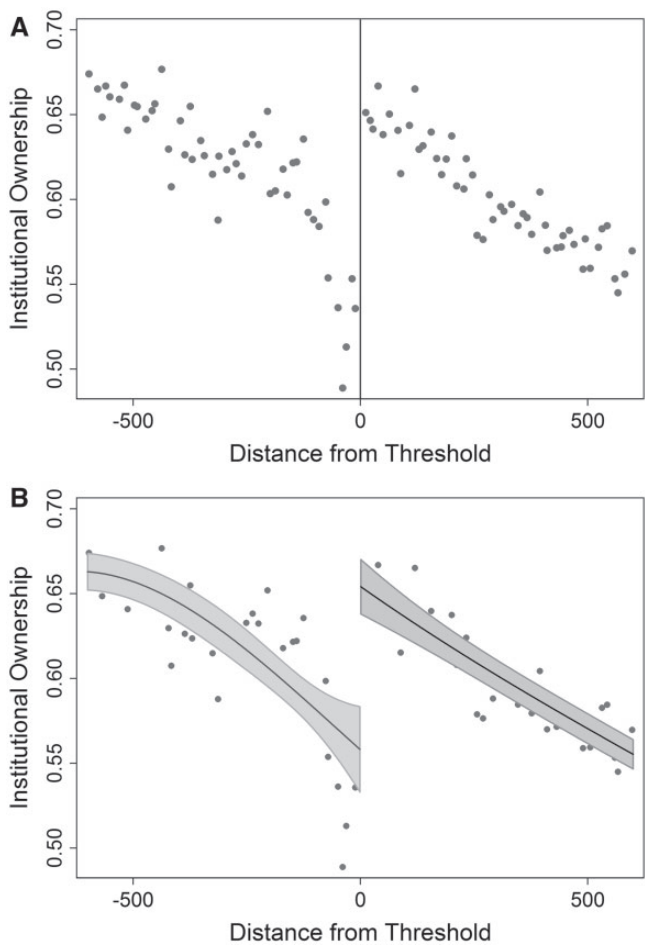


Figure 2
Institutional ownership discontinuity

This figure shows the total institutional ownership for the first quarter ending after the reconstitution of the Russell indexes for the Russell 3000 firms from 1991–2008. The x -axis represents the distance from the Russell 1000/2000 thresholds using the actual Russell ranks in the indexes, with zero representing the last firm in the Russell 1000. The figure plots the average total institutional ownership over ten ranks across all years (A) and adds regression discontinuity estimates and the associated 90% confidence bands following Equation (1) (B).

with high index weights. As a result, discontinuity in Russell index weights should drive discontinuity in IO.

In Figure 2 we present the discontinuity for total IO graphically. In panel A, we plot average IO (averaged over bins of ten Russell ranks) relative to the Russell 1000/2000 threshold. The x -axis represents the distance from the Russell 1000/2000 threshold, where zero represents the smallest firm in the Russell 1000, negative numbers represent larger firms away from the last

Russell 1000 rank, and positive numbers represent smaller firms just away from the first Russell 2000 index rank.

Institutional ownership is increasing in firm size (the negative global slope over the range). However, at the threshold we see that the slightly smaller firms (the largest firms of the Russell 2000) have much higher IO. 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 also a function of the float adjustment, these weights drive variation in ownership due to the incentives of institutional managers.

In Figure 2, panel B, we graph ordinary least-squares (OLS) estimates and confidence intervals of institutional ownership around the threshold. 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 nonoverlapping confidence bands demonstrate the statistical significance of the effect. This suggests a first-order economic difference in ownership in a neighborhood around the Russell index threshold.

While the graphical analysis illustrates stark differences in ownership, we also use regressions to identify point estimates of the causal effect of index inclusion on ownership. For this test, we use a sharp regression discontinuity design following Equation (1) using the unadjusted May 31st market caps to define the neighborhood of firms near the threshold. Table 2 presents the results. $Russell2000_{it}$ is a dummy variable for inclusion in the Russell 2000, and the coefficient represents the discontinuity in the independent variable at the index threshold. Estimation uses OLS with standard errors clustered by firm. As discussed in Section 2, we include controls for distance to the threshold, as well as an interaction term to allow the functional form to differ on either side of the cutoff. Panel A reports estimation results for a small bandwidth (± 100 firms) around the threshold. The small bandwidth helps alleviate specification problems. Panel B uses a larger bandwidth (50% of firms), which increases power but could confound inference if the distance to the threshold does not capture the functional form appropriately.

In Table 2, panel A, column 1, we report the discontinuity estimate for total IO. Consistent with the discontinuity in index weights, we see a large jump in total IO. Those firms just included in the Russell 2000 have nine percentage points more IO. This is a roughly 23% difference relative to firms in the Russell 1000 and is statistically significant at the 1% level.

We repeat this analysis for a wider bandwidth of firms and present the results in panel B of Table 2. If our parametric specification were perfect, then we should use the largest bandwidth in order to minimize type II errors. By expanding the estimation window, we see the sensitivity of our results to this choice. Using the larger bandwidth, our results are largely consistent with panel A. Our estimate of the discontinuity in IO is, as expected, smaller (roughly three

Table 2
Differences in institutional ownership around the Russell 1000/2000 threshold

A. Small bandwidth

| | (1) <i>Institutional ownership_{it}</i> | (2) <i>Dedicated ownership_{it}</i> | (3) <i>Quasi indexer_{it}</i> | (4) <i>Transient ownership_{it}</i> |
|--|--|--|--|--|
| <i>R2000_{it}</i> | 0.09*** (4.20) | 0.00 (0.16) | 0.04*** (3.07) | 0.04*** (3.14) |
| <i>(Rank_{it}[*]-1000)</i> | -0.08*** (-2.82) | 0.00 (0.32) | -0.06*** (-3.24) | 0.00 (0.05) |
| <i>(Rank_{it}[*]-1000) × R2000_{it}</i> | 0.12*** (3.60) | -0.01 (-0.57) | 0.10a (4.43) | 0.00 (0.05) |
| <i>Float adj_{it}</i> | 0.03*** (17.18) | 0.01*** (7.29) | 0.02*** (11.27) | 0.01*** (9.84) |
| <i>Constant</i> | 0.36*** (16.91) | 0.12*** (11.64) | 0.17*** (11.70) | 0.08*** (3.63) |
| Observations | 3,041 | 2,208 | 2,504 | 2,492 |
| R-squared | 0.22 | 0.10 | 0.14 | 0.11 |

B. Large bandwidth

| | | | | |
|--|---------------------|--------------------|----------------------|--------------------|
| <i>R2000_{it}</i> | 0.06*** (7.20) | -0.00 (-0.72) | 0.04*** (7.59) | 0.02*** (4.29) |
| <i>(Rank_{it}[*]-1000)</i> | -0.01*** (-7.94) | -0.00 (1.01) | -0.01*** (-10.69) | 0.00 (1.47) |
| <i>(Rank_{it}[*]-1000) × R2000_{it}</i> | 0.00 (1.43) | 0.00 (0.63) | 0.01*** (3.63) | -0.01*** (3.84) |
| <i>Float adj_{it}</i> | 0.03*** (24.70) | 0.01*** (3.87) | 0.02*** (27.17) | 0.00*** (21.20) |
| <i>Constant</i> | 0.40*** (46.99) | 0.11*** (21.41) | 0.20*** (36.59) | 0.10*** (24.45) |
| Observations | 23,167 | 17,034 | 19,212 | 19,131 |
| R-squared | 0.27 | 0.10 | 0.13 | 0.33 |

This table presents the regression discontinuity test results, where τ is estimated by fitting

$$IO_{i,t} = \alpha_t + \tau \text{Russell2000}_{it} + \delta_1 (\text{Rank}_{it}^* - 1000) + \delta_2 \text{Russell2000}_{it} (\text{Rank}_{it}^* - 1000) + \delta_3 \text{FloatAdj}_{it} + \varepsilon_{it},$$

where *Russell2000* represents a dummy variable equal to one if the firm is in the Russell 2000, in a neighborhood around the Russell 1000/2000 threshold. All results are estimated using ranks implied by the firm's market capitalization within the assigned index as of the index assignment date. Panel A presents estimates calculated over ± 100 ranks from the threshold, and panel B presents estimates over a large bandwidth made up of half the overall sample (± 750 firms). We report estimates of τ , with *t*-statistics given in parentheses. Standard errors are clustered by firm. Variables are defined in Table A1. *, **, and *** indicate significance of less than 10%, 5%, and 1% level, respectively.

percentage points different). The firms in the Russell 1000 are larger across this bandwidth and should have higher IO on average as a result.

In addition to differences in total institutional ownership, we also separate institutions based on the three broad definitions of Bushee (2001): *Dedicated*, *Quasi indexers*, and *Transient*. These classifications are based on the turnover and diversification of holdings. *Dedicated* institutions have large, long-term holdings in a small number of firms. This is the largest category with 20%–30% of total institutional ownership. These institutions are less likely to track or benchmark against a large index. On the other hand, *Quasi indexers* have diversified holdings and low portfolio turnover. *Quasi indexers* are roughly 15% of total institutional ownership in our sample and should have stronger benchmarking incentives. Finally, *Transient* owners have high diversification,

high portfolio turnover, and make up about 10% of total institutional holding. Even if *Transient* owners take short-term active bets on stocks, they still benchmark performance against indices and are likely to hold bigger positions in firms with large index weights.

Columns 2 through 4 of Table 2 show that the effect of index inclusion on institutional ownership is strongest for *Quasi indexers* and *Transient* investors and that there is no significant effect for *Dedicated* owners.¹⁴ These results are reassuring because they are consistent with benchmarking incentives arising from the exogenous mechanical index inclusion rule. On the other hand, *Quasi indexers* and *Transient* owners are not typically considered activist investors which raises questions as to the plausibility of the monitoring/agency cost channel. However, even nonactivist institutions may have a causal effect on payout policy if improved monitoring raises the value of their overall holdings (Del Guercio and Hawkins 1999). Similarly, if benchmarking incentives prevent exit, these institutions may be more active at voicing their beliefs and in engaging managers on payout policy (Carleton, Nelson, and Weisbach 1998). In fact, Appel, Gormley, and Keim (Forthcoming) find these institutions are active in promoting good firm governance. We view these results cautiously because classifications are at the broad institution level (e.g., Fidelity investments as an entity). Many institutions have funds that fall into multiple categories. For the remainder of the paper, we focus on measuring the variation in total institutional ownership (IO), though we note that our results are robust to including only *Quasi indexers* investors as in Appel, Gormley, and Keim (Forthcoming).

3.2 The effect of institutional ownership on payout policy

In this section we test the hypothesis that an increase in institutional ownership causes an increase in payout. Table 3 presents the two-stage least-squares estimates of IO on payout policies as described in Equations (1) and (2). Table 3, panel A, reports estimation results for a small bandwidth (± 100 firms) around the threshold. As discussed above, using the smaller threshold reduces the chance of a spurious result driven by functional form specification problems. Table 3, panel B, again uses the larger bandwidth (50% of firms), which increases power but can introduce bias. For both panels, we report the first-stage estimate (similar to what is presented in Table 2) on our excluded instrument in the top panel of the table. Across all columns, the first-stage estimate is both economically and statistically significant. F-statistics and t-statistics exceed the thresholds suggested by Stock and Yogo (2005).¹⁵

¹⁴ These results are similar in magnitude to those of Appel, Gormley, and Keim (Forthcoming) and Boone and White (2015), but they are different from those of Chang, Hong, and Liskovich (2015) and Mullins (2014). We discuss the similarities and differences in Section 7.1, where we compare our results to other recent studies.

¹⁵ 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 these adjustments are not systematically related to the outcome variables. All regressions include year fixed effects.

Table 3
Institutional ownership and payout: Instrumental variable estimates

A. Small bandwidth

| <i>First stage</i> | (1) IO | (2) IO | (3) IO | (4) IO | (5) IO |
|--|-------------------|-------------------|-------------------|----------------------|-------------------|
| τ | 8.00*** (3.40) | 8.19*** (3.50) | 8.40*** (3.55) | 9.71*** (4.09) | 9.73*** (4.13) |
| <i>Second stage</i> | <i>Ln(Div.)</i> | <i>Div. Yield</i> | <i>Pr(Div.)</i> | <i>Ln(Total Pay)</i> | <i>Ln(Rep)</i> |
| IO | 6.57** (1.98) | 0.10*** (2.30) | 1.95* (1.83) | 4.57* (1.93) | 2.53 (1.31) |
| $(Rank_{it}^* - 1000)$ | -0.22 (-0.92) | -0.00 (-0.87) | -0.16 (-1.25) | -0.15 (-0.73) | 0.05 (0.29) |
| $(Rank_{it}^* - 1000) \times R2000_{it}$ | -0.14 (-0.26) | -0.00 (-0.07) | 0.12 (0.46) | -0.24 (-0.53) | -0.40 (-1.05) |
| <i>Float adj.</i> | -0.03 (-0.26) | -0.00 (-1.36) | 0.04 (0.60) | 0.02 (0.22) | 0.03 (0.34) |
| Year effects | y | y | y | y | y |
| Observations | 2,667 | 2,680 | 2,531 | 2,332 | 2,342 |

B. Large bandwidth

| <i>First stage</i> | IO | IO | IO | IO | IO |
|--|---------------------|-------------------|-------------------|----------------------|----------------------|
| τ | 4.34*** (5.10) | 4.35*** (5.14) | 4.38*** (7.48) | 5.19*** (6.04) | 5.19*** (6.06) |
| <i>Second stage</i> | <i>Ln(Div.)</i> | <i>Div. Yield</i> | <i>Pr(Div.)</i> | <i>Ln(Total Pay)</i> | <i>Ln(Rep)</i> |
| IO | 5.46*** (2.45) | 0.05* (1.93) | 0.92 (0.86) | 4.39*** (2.73) | 4.13*** (2.80) |
| $(Rank_{it}^* - 1000)$ | -0.22*** (-9.72) | 0.00 (0.24) | -0.06 (-3.64) | -0.28*** (-17.54) | -0.20*** (-14.45) |
| $(Rank_{it}^* - 1000) \times R2000_{it}$ | 0.17*** (6.12) | 0.00 (0.27) | 0.03 (2.32) | 0.18*** (7.86) | 0.15*** (7.13) |
| <i>Float adj.</i> | -0.07 (-0.94) | 0.00 (0.03) | 0.03 (0.81) | -0.03 (-0.63) | -0.09 (1.62) |
| Year effects | y | y | y | y | y |
| Observations | 20,614 | 20,740 | 19,486 | 18,007 | 18,056 |

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:

$$IO_{i,t} = \alpha_t + \tau Russell2000_{it} + \delta_1 (Rank_{it}^* - 1000) + \delta_2 Russell2000_{it} (Rank_{it}^* - 1000) + \delta_3 FloatAdj_{it} + \varepsilon_{it}.$$

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

$$Policy_{i,t} = \theta_t + \beta \widehat{IO}_{it} + \gamma_1 (Rank_{it}^* - 1000) + \gamma_2 Russell2000_{it} (Rank_{it}^* - 1000) + \gamma_3 FloatAdj_{it} + \eta_{it}.$$

All results are estimated using ranks implied by the firm's market capitalization within the assigned index as of the index assignment date. Panel A presents estimates calculated over ± 100 ranks from the threshold, and panel B presents estimates over a large bandwidth made up of half the overall sample (± 750 firms). The estimation is performed using a two-stage least squares. First-stage control variable estimates are suppressed for brevity. Coefficients are reported with the *t*-statistics in parentheses. Standard errors are clustered by firm. Rank variable coefficients are reported per 100 ranks. Variables are defined in Table A1. *, **, and *** indicate significance of less than 10%, 5%, and 1% level, respectively.

Table 3, panel A, column 1, presents an estimate of the effect of instrumented IO on $Ln(dividends)$ in the next year. Dividends are increasing in instrumented IO. A one-percentage-point increase in IO is associated with a \$7 million increase in dividends that is statistically significant at the 1% level. This represents an 8% increase in dividends for a one-percentage-point increase

in IO relative to the median dividend payment by Russell 1000 firms. Columns 2 and 3 add to this result by showing that dividend yield and the probability of paying a dividend increase significantly in instrumented IO. The economic magnitudes are again large. A 10% increase in IO increases the dividend yield by one percentage point relative to the average dividend yield of 2% for Russell 1000 firms. A 10% increase in IO also increases the probability of paying a dividend by approximately 20%. This evidence is consistent with the view that institutional ownership leads to economically significant higher dividend payout.

Columns 4 and 5 of Table 3 present the results of instrumented IO on *Total payout* and *Share repurchases (Rep)*. The coefficient estimate represents a \$1 million increase in total payout for a one-percentage-point increase in instrumented IO. This result is economically large and statistically significant at the 10% level. The effect on share repurchases is smaller, and the *p*-value of the effect is only 0.19 for the small bandwidth.¹⁶ In the larger bandwidth (Table 3, panel B), estimates are slightly larger estimates for both total payout and share repurchases and the repurchase result is significant at the 1% level. The directional effect of institutional ownership on share repurchases is consistent with our results for dividends, though the estimates are smaller and less robust. These results are consistent with repurchases serving as a less rigid commitment device to mitigate agency costs. At a minimum, the net effect on total payout appears to be positive, economically large, and statistically robust. Taken together, our tests point to a causal effect of IO on payout. Exogenous differences in levels of IO appear to cause managers to pay out more to shareholders.

4. Further Evidence on the Agency/Monitoring Channel

If institutional investors are engaged in monitoring and control of the firms they own, then a higher level of ownership by institutions will lead to an increase in shareholder proposals and proxy voting behavior. Voting behavior suggests the use of voice as a channel for institutions to influence payout policy. The mere threat of voting can impact the credibility of jawboning to influence firm policies. To measure shareholder proposals and voting behavior, we collect data from the ISS Risk Metrics Shareholder Proposal and Vote Results database.

We measure the number of shareholder proposals, as well as total voting participation and the percentage of votes against proposals from management. Of course, not all proposals are related to dividends. In fact, there are only a handful of dividend-specific proposals in our sample. Thus, it is difficult to directly test the effect of proxy voting on dividend policy, which is the focus of our study. However, to the extent that pressuring firms on their dividend policy

¹⁶ Due to the log transform, the increase in repurchases and dividends sum to less than the increase in total payout.

is more broadly correlated with voting on governance-related issues, we can use total shareholder proposals as a proxy for (at least the threat of) dividend voting. To further refine these tests, we also distinguish between proposals from management and those from shareholders. On average, voting on shareholder proposals should be more correlated with monitoring and participation in the governance process. Lastly, our measures of total proposals contain some votes that are clearly less related to monitoring and agency costs, like socially responsible investment (SRI) proposals. Since our measure of all proposals is a noisy proxy for dividend monitoring, removing less related SRI proposals improves the relevance of our instrument. While Flammer (2015) finds that SRI proposals improve firm value, they are unlikely to directly affect payout policy. Thus, we predict a stronger effect from governance-related proposals than from SRI proposals.

Table 4 presents results related to proxy proposals for firms just included in the Russell 2000. We find no differences in the number of management proposals. However, as shown in Column 2, there is a large difference in the number of shareholder-initiated proposals. Specifically, firms just to the right of the threshold, that is, in the Russell 2000, have approximately two more shareholder-initiated proposals in a given year compared to firms in the Russell 1000, a 4% difference. In Columns 3 and 4 we investigate the types of proposals that are different across the threshold. Importantly, as shown in Column 3, the overall difference is driven by an increase in the number of governance-related shareholder proposals (as defined by ISS). We see no significant difference in the number of social-responsibility-related proposals. Finally, in Columns 5 and 6, we look at the probability of having a governance or social responsibility proposal. We see that the probability of having a shareholder governance proposal is 9% higher for firms just included in the Russell 2000. While the point estimate for the probability of having a social responsibility proposal is also positive, this estimate is insignificant. Overall, these results suggest a monitoring role for institutions with respect to proposals.

We also present results related to the voting outcomes on management proposals. We find evidence that votes against management proposals are significantly higher for firms just included in the Russell 2000. In fact, they receive seven percentage points more against the proposal. We also see evidence indicating these votes matter. While only significant at the 12% level, the point estimate suggests that management proposals are 10% less likely to pass. Our results on voting are consistent with past studies that find institutions vote more actively (Brickley, Lease, and Smith 1988) and serve a monitoring role through voting (Iliev and Lowry 2015; Morgan et al. 2011).

Our results are also consistent with those of Appel, Gormley, and Keim (Forthcoming), who find an improvement in firm governance using a variety of other measures like poison pill adoption. Their focus is on index funds, but they measure the same source of variation we use in our study. As a result, their findings of improved firm governance on other dimensions bolster our argument

Table 4
The difference in shareholder proposals and voting outcomes around the Russell 1000/2000 threshold

| | Proposals | | | | | | Proxy voting | | |
|--|------------------|--------------------|--------------------|---------------------|-----------------------------|---------------------|---------------------|---------------------|---------------------|
| | Mgmt. | Shareholder | Gov. | SRI | Pr(gov) | Pr(SRI) | Participation | Votes against (%) | Pr(pass) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| <i>R2000_{it}</i> | 0.03 (0.39) | 0.63** (2.14) | 0.73** (2.44) | 0.33 (1.43) | 0.09** (2.15) | 0.06 (1.2) | 0.035 (0.92) | 0.07** (2.07) | −0.10 (−1.52) |
| <i>(Rank_{it}[*] − 1000)</i> | 0.01 (1.19) | −0.05* (−1.95) | −0.05** (−1.97) | −0.21*** (−8.59) | −0.01*** (−2.72) | −0.05*** (−9.29) | 0.01*** (3.12) | −0.01*** (−3.40) | 0.03*** (4.25) |
| <i>(Rank_{it}[*] − 1000) × R2000_{it}</i> | −0.02 (−1.42) | 0.09** (2.35) | 0.09** (2.28) | 0.20*** (6.11) | 0.01 ^b (2.26) | 0.05*** (6.23) | −0.02*** (−3.03) | 0.01** (2.43) | −0.03*** (−3.62) |
| <i>Float adj_{i,t}</i> | 0.01 (0.67) | 0.16** (2.46) | 0.20*** (2.9) | 0.04 (0.58) | 0.02 ^b (2.05) | −0.01 (−0.58) | 0.02*** (3.64) | −0.01 (−1.30) | 0.02 (1.32) |
| <i>Constant</i> | 0.48*** (8.9) | 3.98*** (22.31) | 2.67*** (14.09) | 0.61*** (3.63) | 0.70*** (22.12) | 0.20*** (5.37) | 0.32*** (15.58) | 0.34*** (14.11) | 0.80*** (19.51) |
| Observations | 1,029 | 3,137 | 3,137 | 3,137 | 3,137 | 3,137 | 2,067 | 1,988 | 1,988 |
| R-squared | 0.003 | 0.010 | 0.010 | 0.065 | 0.008 | 0.104 | 0.020 | 0.012 | 0.016 |

This table presents the regression discontinuity test results, where τ is estimated by fitting

$$Y_{i,t} = \alpha_t + \tau \text{Russell2000}_{it} + \delta_1 (\text{Rank}_{it}^* - 1000) + \delta_2 \text{Russell2000}_{it} (\text{Rank}_{it}^* - 1000) + \delta_3 \text{FloatAdj}_{it} + \varepsilon_{it},$$

where *Russell2000* represents a dummy variable equal to one if the firm is in the Russell 2000. We report coefficient estimates calculated over the full bandwidth, with *t*-statistics in parentheses. Variables are defined in Table A1. *, **, and *** indicate significance of less than 10%, 5%, and 1% level, respectively. Standard errors are clustered by firm. The number of proposal regressions use log transformed variables.

that investors have an incentive to monitor and control firm behavior even if they are not traditionally considered to be activists. Given the prominence of payout policy as a focal point for activist investors, it seems natural that monitoring by nonactivist investors would meaningfully affect payout policy.

In addition to the evidence on voting, we use cross-sectional variation to test whether our payout results are stronger for firms with high expected agency costs. Stable, cash-rich, and poorly governed firms with low growth opportunities are typically expected to suffer more from agency costs of free cash flow. While these tests do not directly identify the agency costs of free cash flow, they are at least suggestive of the channel through which institutions have a causal impact on dividends.

We rely on six proxies for agency costs. The first measure is a dummy variable equal to one if the CEO is also the board chairman. This has been suggested by, for example, Brickley, Coles, and Jarrell (1997) as a proxy for agency problems. Our second proxy is the dollar value of CEO ownership exclusive of options. Higher ownership stakes should better align managers with shareholders and mitigate the effect of institutional ownership on payout policy (Jensen and Meckling 1976). Our third proxy is board size. Yermack (1996), among others, suggests that larger boards suffer from coordination problems and as a result monitor less effectively.

Our fourth measure of agency costs is the GIM index of Gompers, Ishii, and Metrick (2003). This index measures firm-level charter/by-law provisions and state-level antitakeover laws that restrict shareholder rights. Firms with a low GIM index have better shareholder rights protections and will, on the margin, require less external monitoring by institutional investors. As a result, the effect of institutional ownership on payout policy should be smaller for low GIM index firms if internal and external governance are substitutes. It is important to note that Cremers and Nair (2005) suggest internal and external governance are complements in some circumstances. However, since the GIM index has a negative correlation with dividends in our sample, the substitution effect dominates. In any event, our prediction should be interpreted with this caveat in mind.¹⁷

We also use firm profitability as our fifth measure of agency. Better operating performance may be evidence of lower agency costs (e.g., Bertrand and Mullainathan 2003). Thus, we predict that the effect of institutional ownership on payout will be greater for firms with lower ROA. This measure is controversial. It is possible that agency problems are worse for firms with high profitability (and thus a free cash flow problem), which would reverse our prediction. Here, we note that the stand-alone effect of profitability on dividends

¹⁷ Overall, there is mixed evidence with respect to whether internal and external governance are complements or substitutes. While Cremers and Nair (2005) study pension funds and find evidence of complementarity using the GIM measure, Denis and Kruse (2000) and Huson, Parrino, and Starks (2001) suggest substitution using different measures of governance.

helps guide our interpretation. Since profitability is positively associated with payout in our sample, this adds credence to our interpretation of low profitability as a measure of greater agency problems as in Bertrand and Mullainathan (2003).

Our sixth and final measure splits firms with high (above median) cash flows and low (below median) market-to-book ratio from firms with low cash flows and high market-to-book (Jensen 1986). All of our proxies are imperfect measures of agency costs. As a result, we interpret these tests cautiously.

For CEO ownership, profitability, GIM index, and board size, we sort firms into two groups based on the median of each measure in the year prior to the index assignment and set a dummy variable equal to one if we expect high agency costs. For the CEO/chairman and high cash flow/low market-to-book variables, we use the simple dummy variable from their construction. We then estimate our IV analysis, including an interaction term with the agency proxy dummy variable to test for differences in the coefficient on IO. We present our results for dividends, but our findings are qualitatively similar for total payout and for share repurchases.

Table 5 presents our results. We find that firms with a CEO also holding the chairman position, firms with low CEO ownership, firms with large board size, low ROA, and firms with low market-to-book and high cash flow, and firms that score high in the governance measure of Gompers, Ishii, and Metrick (2003) are the firms that drive the effect we observe in the overall sample. 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 all broadly consistent with an agency cost explanation for our findings. For each of our agency cost measures, it appears the effect of institutional ownership is stronger where the marginal value of monitoring is likely to be higher.

5. Alternative Explanations

Of course, agency costs are not the only friction that links ownership to payout policy. Our experimental design excludes reverse causality hypotheses, such as institutional sorting based on tax preferences (Grinstein and Michaely 2005) or firm signaling (Bhattacharya 1979; Miller and Rock 1985). However, there are some alternative causal explanations for why institutions might pressure firms to change dividends beyond agency costs. Firms may adapt their payout policy in response to the tax-clientele preferences of institutions (Allen, Bernardo, and Welch 2000). While Desai and Jin (2011) find evidence that dividend-averse institutions can affect dividend policy, results across all institution types show little effect, a fact that is consistent with Grinstein and Michaely (2005). Given our focus on total institutional ownership, the tax-clientele channel may be a less likely explanation of our results.

In addition to clientele effects, firms may raise dividends in response to myopic institutions with a preference for short-run payout (Bushee 2001;

Table 5
Institutional ownership and dividends: Cross-sectional effects

| | Agency measures (XS) | | | | | |
|--|----------------------|----------------------|---------------------|---------------------|----------------------|----------------------|
| | CEO/ Chairman | CEO ownership | Board size | GIM index | ROA | High CF/ Low MTB |
| $IO_{i,t}XS_{i,t}$ | 3.77** (2.25) | -1.47*** (-7.08) | 7.70* (1.67) | 8.18*** (2.99) | -3.62*** (-3.05) | 5.40*** (2.76) |
| $IO_{i,t}$ | 2.73 (1.07) | 3.68 (1.24) | 6.78** (1.97) | 2.79 (0.89) | 7.35*** (2.70) | 1.34 (0.87) |
| $XS_{i,t}$ | -2.14** (-2.00) | -0.52** (-2.10) | -3.99 (-1.35) | -4.45*** (-2.61) | 0.98* (1.65) | -2.62** (-2.23) |
| $(Rank_{it}^* - 1000)$ | -0.27*** (-12.41) | -0.24*** (-10.59) | -0.24*** (-5.88) | -0.18*** (-5.56) | -0.23*** (-10.41) | -0.27*** (-14.34) |
| $(Rank_{it}^* - 1000) \times R2000_{it}$ | 0.21*** (5.37) | 0.16*** (4.40) | 0.10 (1.48) | 0.08* (1.81) | 0.17*** (5.55) | 0.18*** (6.30) |
| $FloatAdj_{i,t}$ | -0.08 (-0.84) | -0.10 (-0.94) | -0.35* (-1.75) | -0.19 (-1.55) | -0.09 (-1.16) | -0.03 (-0.49) |
| Year effects | y | y | y | y | y | y |
| Observations | 13,053 | 8,787 | 5,361 | 10,937 | 17,349 | 14,582 |

This table presents an instrumental variable estimation based on Equations (1) and (2). The second-stage regression presents payout policy variables as a function of instrumented institutional ownership and its interaction with our proxies for agency costs or information asymmetry,

$$\begin{aligned}
 \text{Ln}(\text{Dividends})_{i,t} = & \theta_1 + \beta_1(\widehat{IO}_{it} \times XS_{it}) + \beta_2XS_{it} + \gamma_1(Rank_{it}^* - 1000) \\
 & + \gamma_2\text{Russell}2000_{it}(Rank_{it}^* - 1000) + \gamma_3\text{FloatAdj}_{it} + \eta_{it}.
 \end{aligned}$$

All results are estimated using ranks implied by the firm's market capitalization within the assigned index as of the index assignment date. Our cross-sectional split is represented by XS , which is a dummy variable equal to one if our measure of agency or information asymmetry is above the median for each of our continuous proxies. CEO/Chairman is equal to one if the CEO is also the Chairman in that year. We present estimates calculated over the large bandwidth (± 750 ranks). The estimation is performed using a two-stage least squares. First-stage results are suppressed for brevity. Coefficients are presented with t -statistics in parentheses. Standard errors are clustered by firm. Rank variable coefficients are reported per 100 ranks. Variables are defined in Table A1. *, **, and *** indicate significance of less than 10%, 5%, and 1% level, respectively.

Brav et al. 2005; Daniel, Denis, and Naveen 2008). However, this channel also seems less likely in our setting because such myopic behavior should cause a decline in firm value that is inconsistent with the permanent price increases associated with Russell 2000 index inclusion in Chang, Hong, and Liskovich (2015). While none of our tests can definitively rule out the tax-clientele or myopia channels, we provide further tests to establish the plausibility of the agency cost/free cash-flow hypotheses based on more direct proxies for expected agency costs and institutional monitoring.

In unreported tests we also split on proxies for information asymmetry. Allen, Bernardo, and Welch (2000) suggest that under asymmetric information, firms may alter their payout policy to attract better-informed institutional investors who prefer dividends for tax reasons. In their agency-based model, the marginal benefit of dividends depends on how effective dividends are in attracting institutional shareholding and on the effectiveness of institutions at value-enhancing monitoring. Since variation in institutional holdings are exogenous in our setting, the use of dividends to attract institutions is less relevant. However, the effect of institutions on dividends should be larger

when monitoring is cheaper and more effective. If institutions reveal hidden information in the firm (i.e., they solve the hidden action problem as opposed to a collective action problem), then the marginal benefit to monitoring should be higher for firms with more asymmetric information. To test the empirical relevance of this channel in our setting, we use total analyst coverage, R&D, and asset tangibility as proxies for asymmetric information.¹⁸ We argue that more analyst coverage and higher asset tangibility should be associated with less information asymmetry, while higher R&D should be associated with more.

We find that institutional ownership has a larger effect on dividends for firms with higher analyst coverage and low R&D, but no relation to asset tangibility. These results suggest that, in equilibrium, the effect of institutions on dividend policy is stronger for firms with low information asymmetry. While firms might lower the level of asymmetric information by raising dividends to attract institutions, the marginal return to monitoring should not be larger for firms with less asymmetric information. These results are not inconsistent with an agency interpretation, but suggest that institutions may help reduce the costs of coordinated monitoring as opposed to reducing an information opacity cost.

Our tests in Sections 4 and 5 suggest that institutions have an effect on payout policy through their role as external monitors. However, it is possible there are other channels by which Russell index inclusion drives differences in payout policy. One potential channel is through a firm's visibility to investors. If firms just included in the Russell 1000 index receive differential media coverage, it may have an effect on corporate policy. For example, Liu and McConnell (2013) and Dyck and Zingales (2002) show that the media can play a governance role aligning managers' and shareholders' interests. Managers may be better monitored by the investment community in general, as opposed to institutional investors specifically. As a result, media exposure might help mitigate agency costs and confound our analysis of institutional holdings.

To test this hypothesis directly, we collect data on news coverage for all of the firms in our sample near the threshold. We measure the total number of news stories for each firm in the Factiva database in the year of Russell index inclusion. We then use this measure of media coverage in the first stage of an IV estimation as in Section 2 and test whether instrumented media exposure drives firm payout decisions. Test results presented in our Internet Appendix

¹⁸ Our use of these measures follows, for example, Titman and Wessels (1988), Hong, Lim, and Stein (2000), Aboody and Lev (2000), Barth and Hutton (2004), and Chang, Dasgupta, and Hilary (2006). As with our agency measures, these variables are not perfect measures of information asymmetry. Analyst coverage could be a form of monitoring in which higher analyst coverage would predict a weaker effect of institutions on payout. Similarly, R&D could proxy for growth opportunities that might suggest lower dividend payout to finance internal growth. Again, our predictions are consistent with past studies that view analyst coverage and R&D as measures of information opacity, but these predictions should be taken with appropriate caveats.

show no significant effect of instrumented news coverage on any form of payout, suggesting that differences in visibility do not confound the effect of institutional ownership.

6. Robustness

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

6.1 Comparison with other approaches

Our main results are based on a simple implementation of 2SLS, where our instrumental variable comes from exogenous variation in IO near the index threshold, which we isolate with a first-stage RD. The basic assumption is that we identify a source of exogenous variation in IO. However, there are a number of challenges in exploiting this discontinuity. Discontinuity methods are relatively new in finance, and this fact partly explains why a number of different studies have relied on distinct empirical techniques all within the same basic setting. In this section we explore the econometric assumptions behind our analysis and compare our approach with other recent papers in the literature.

The basic problem is that Russell uses its own market capitalization to decide index inclusion. As a result, the researcher does not observe the true forcing variable and must use an estimate (e.g., CRSP market capitalization). We assume that any differences in market capitalization estimates across sources are random with respect to future outcome variables. Russell's own description of their methodology yields no obvious reason to suspect otherwise.

Small positive differences between CRSP and Russell's market capitalization around the threshold could lead a researcher to predict that a Russell 2000 firm was actually in the Russell 1000. Since we keep firms in the index they are actually assigned to, there will be a small discontinuity in the observed market capitalizations as noted above. Appel, Gormley, and Keim (Forthcoming) suggest that this may violate the assumption of local continuity in potential outcomes. However, this is only true if the proxy for market capitalization differs from Russell in a way related to dividend policy, payout, etc., since that would create a selection bias.¹⁹ Importantly, if this were true, all studies that make use of this Russell setting without access to Russell's market values would be subject to the same selection bias, regardless of the specification.

¹⁹ Our identifying assumption depends on local continuity in potential outcomes and not observed market capitalization. Section III of our Internet Appendix explores this issue in greater detail. We present simulations that maintain the assumption of local continuity but show a small discontinuity in observed market capitalization that arises mechanically from the noise in index assignment. The effect is small and is not likely to affect statistical inference in our tests.

Appel, Gormley, and Keim (Forthcoming) follow up on our study to test whether index funds improve other evidence of monitoring. They use an approach similar to ours but with a different first-stage regression and different outcome variables in the second stage. Appel, Gormley, and Keim (Forthcoming) focus on a wider bandwidth, while we identify variation closer to the threshold. They also focus on variation in *Quasi indexers*, while we focus on total institutional ownership. Our tests have lower power, but are less subject to misspecification. This reflects the basic trade-off of robust specification against power. For example, there is a strong correlation between firm size and IO. The validity of causal inference depends on properly accounting for any relationship between the forcing variable (e.g., size ranking) and the outcome variable (e.g., IO). Otherwise, what looks like a discontinuity may be a misspecification in the conditional mean of the counterfactual. In our Internet Appendix, we contrast our approach with that of Appel, Gormley, and Keim (Forthcoming) and argue that our specification yields more robust inference, though we note that the basic result in our paper (higher IO and payout) is robust to either specification.

Some other recent studies take a different approach and adopt a traditional fuzzy RD estimation. In these studies, the threshold forecasts treatment with some probability. For example, Mullins (2014) uses this approach and forecasts index assignment using his own market capitalization calculation.²⁰ Here, IO is a second-stage outcome, where the first stage predicts index inclusion. Mullins (2014) finds that IO is actually higher for firms at the bottom of the Russell 1000. This result is opposite in sign from our estimate and has the opposite economic interpretation for our instrumental variable. As such, it is important to highlight the econometric issues that underlie this approach.

The standard fuzzy RD implementation has a subtle but serious problem in the Russell setting. 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 a mean zero noise term, this reverses the sign of the treatment effect for that pair of observations and induces a correlation in the errors. These prediction errors are large and drive the discontinuity in the probability of treatment toward zero, creating a weak instrument problem (e.g., see Bound, Jaeger, and Baker 1995; Angrist, Imbens, and Rubin 1996; Bartels 1991; Shea 1997). In our Internet Appendix, we present evidence from Monte Carlo simulations and show that a fuzzy RD provides unreliable estimates in the 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 simulations also suggest that the IV estimation we employ results in an unbiased estimate of

²⁰ The econometric specifications for the alternative approaches are detailed in our Internet Appendix.

the treatment effect. As a result, we have chosen to focus on actual rather than forecasted inclusion in our identification strategy.²¹

Chang, Hong, and Liskovich (2015) study price pressure around the threshold. They focus on additions and deletions to the index by splitting the sample based on the prior year's index assignment and use the fuzzy RD design. This approach is subject to the same weak instrument problem as in Mullins (2014). Moreover, the assumption of local continuity is more problematic when firms move from one index to the other. Firms that move in or out of the Russell 2000 did so because of a potentially large movement in their market capitalization ranking. Relative to counterfactual firms just around the threshold, there may be an endogenous reason for the large swing in market capitalization. Perhaps the firms just added or deleted became smaller or larger because of changes in profitability, payout, capital structure, or governance. The results for IO in Chang, Hong, and Liskovich (2015) are also inconsistent with ours. They show a 3.1-percentage-point difference in IO for firms in the Russell 2000 conditioning on firms that shrunk from the Russell 1000 and a -6.3 percentage points in IO for firms in the Russell 2000 that stay in the index versus firms that move to the Russell 1000.

6.2 Pretreatment effects

While it is impossible to test the assumption of local continuity (or the exclusion restriction), certain diagnostics are useful. Angrist and Pischke (2009) emphasize that the analysis of pretreatment variables provides an important robustness check in a discontinuity setting, regardless of whether estimates are formed based on RD or IV. Since pretreatment observations have not received the treatment, there should not be any jumps around the discontinuity for pretreatment outcome variables. The existence of significant pretreatment effects might reflect nonrandom selection into the treatment group, which could signal omitted variable or sample selection biases.

To test whether the index assignment made by Russell creates any obvious selection bias around the threshold, we compare firm characteristics (market capitalization, institutional ownership, and payout variables) at the index threshold prior to assignment by Russell using rankings based on the May 31st observed market capitalizations from CRSP. To test for differences, we measure characteristics to the right and the left of the threshold in the year prior to the index assignment. We present the results of these tests in our Internet Appendix. Firms are very similar on both sides of the threshold. The discontinuity tests show no significant differences in size, ownership, or payout around the threshold in the prior year, suggesting there is no obvious selection bias near the threshold.

²¹ Mullins (2014) uses additional information from Russell to improve market capitalization estimates, but we find that this does not meaningfully improve forecasts of index inclusion.

7. Conclusion

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

Firms just included in the Russell 2000 have more shareholder engagement through the proxy process. The finding that institutional ownership increases payout is driven by firms with higher agency costs. These cross-sectional results, along with our evidence on shareholder engagement, suggest that institutional investors play an important role in reducing manager/shareholder conflicts. Overall, our results are consistent with those of Easterbrook (1984) and Jensen (1986), who suggest that distributing cash to shareholders can discipline managers and reduce agency costs. We find that firms subject to higher levels of ownership by institutions with benchmarking incentives have different voting participation and voting outcomes. Our findings indicate that institutions, even if they are not traditionally activist investors, monitor and influence the payout policy of the firms they own.

Appendix

Table A1
Variable definitions

| Variable | Definition |
|----------------------------|--|
| <i>Analyst coverage</i> | Number of analysts from IBES that estimate EPS for the fiscal year |
| <i>Asset tangibility</i> | Net property, plant, and equipment (PPENT), scaled by total assets (AT) |
| <i>Board size</i> | Number of board members from the Corporate Library |
| <i>CEO ownership</i> | Equal to one if Execucomp stock CEO ownership > median, and zero otherwise |
| <i>CEO/Chairman</i> | Equal to one if the CEO is the Chairman, and zero otherwise. <i>Source:</i> Execucomp. |
| <i>CF</i> | Cash flow from operations |
| <i>Dedicated ownership</i> | Institutions with low portfolio turnover and a small number of positions. Institutions are classified based on data and methods from Bushee 2001 |
| <i>Dividend yield</i> | Compustat dividends (DVC+DVP), divided by market capitalization |
| <i>Dividends</i> | Compustat dividends (DVC+DVP) |
| <i>Float adj.</i> | The difference between rank implied by the observed market capitalization and the rank assigned by Russell in June |
| <i>GIM index</i> | GIM index from ISS-RiskMetrics |
| <i>Gov. proposals</i> | The number of governance proposals from ISS shareholder proposals database |
| <i>High CF/Low MTB</i> | Equal to one if ROA > median and MTB < median, and zero otherwise |
| <i>IO</i> | Thomson 13F shares held across all institutions/shares outstanding (SHROUT) |
| <i>Market value</i> | CRSP price (PRC), multiplied by shares outstanding (SHROUT) |
| <i>Mgmt. proposals</i> | The number of management proposals from ISS shareholder proposals database |

(continued)

Table A1
Continued

| Variable | Definition |
|-----------------------------------|---|
| <i>MTB</i> | Market equity (PRCC x CSHPRI) + debt (DLC+DLTT) + preferred stock (PSTKL), minus deferred taxes, all scaled by book value of total assets (AT) |
| <i>Payout</i> | Compustat dividends (DVC+DVP), plus purchase of shares (PRSTKC) |
| <i>Pr(gov.) proposals</i> | Equal to one if at least one governance proposal, and zero otherwise |
| <i>Pr(SRI) proposals</i> | Equal to one if at least one social responsibility proposal, and zero otherwise |
| <i>Proxy votes against</i> | Votes against divided by total votes. From the ISS voting database |
| <i>Proxy voting participation</i> | The percent of ballots cast from the ISS voting database |
| <i>Proxy voting Pr(pass)</i> | Equal to one if a proposal passed, and zero otherwise. From the ISS voting database |
| <i>Quasi indexer</i> | Institutions with low portfolio turnover and a large number of positions. Institutions are classified based on data and methods from Bushee 2001 |
| <i>R&D</i> | Compustat research and development expense (XRD) |
| <i>R2000</i> | Dummy variable equal to one if the firm is in the Russell 2000 index |
| <i>Rank</i> | Rank order with a Russell index based on market capitalization |
| <i>Repurchases</i> | Purchase of common and preferred shares (PRSTKC) |
| <i>ROA</i> | Operating income before depreciation (OIBDP), scaled by lagged total assets (AT) |
| <i>Shldr. proposals</i> | The number of shareholder proposals from the ISS shareholder proposals database |
| <i>SRI proposals</i> | The number of social responsibility proposals from the ISS shareholder proposals database |
| <i>Total assets</i> | Compustat total assets (AT) |
| <i>Transient ownership</i> | Institutions with high portfolio turnover and a large number of positions. Institutions are classified based on data and methods from Bushee 2001 |

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