The Effect of Institutional Ownership on Payout Policy: A Regression Discontinuity Design Approach

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Abstract

We show that higher institutional ownership causes firms to pay more dividends and repurchase more shares. Our identification strategy relies on a discontinuity in ownership based on the annual composition of the Russell 1,000 and 2,000 indices. We also find evidence of a causal effect on proxy voting, corporate investment, R&D, and equity issuance. Overall, results support agency models where concentrated ownership lowers the marginal cost of delegated monitoring.

Institutional investors own the bulk of public equity in the US. Over the past 40 years, ownership by institutions has increased dramatically from 10% in the 1970s to more than 60% by 2006 (Aghion, Van Reenen, Zingales (2012)). Testing whether differences in institutional ownership influence corporate policy is important, but in practice it is very difficult to establish causality. Not only do existing theories suggest different possible causal relationships, but also the existing empirical evidence is mixed. While institutions may *cause* differences in corporate policies, they may also *choose* stocks because of differences in corporate policy.

In this paper, we break the endogeneity between corporate policies and ownership by showing that random ownership by institutions causes higher payout. Our identification of causality relies on forced institutional holdings around the Russell 1000 and Russell 2000 index cut-off to isolate a large discontinuity in institutional ownership. 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. The difference in size around the Russell 1000/2000 cut-off is less than 2% of a standard deviation of daily returns. Within some "bandwidth" around the cut-off, a firm's size ranking, and therefore index assignment, is random. Since the indices are value-weighted, a firm ranked 1,000th in size has a trivial weighting in the Russell 1000 while a firm ranked 1,001 is the largest stock in the Russell 2000 and must be held by any fund tracking or benchmarking against the index. Figure 1 shows the weights for the firms in the Russell 1000 and the Russell 2000. From a tracking error perspective, a firm's position just to the left or right of the threshold should have a significant impact on ownership.

This discontinuity in the index composition represents random exposure to higher institutional ownership that we exploit to test for causal effects on payout policy. Our main finding is that more ownership by institutions appears to cause an increase in payout and a net decrease in cash holdings. Specifically, we find that when randomly exposed to 9% higher institutional ownership, firms pay 13% more in dividends, repurchase 22% more of their shares, and on net pay out 5% more of their net cash flows. The effects we measure are consistent with monitoring activity by institutional investors. We find that proxy-voting participation increases by 55 percentage points for firms with higher institutional ownership. In the cross-section, our results on payout policy are stronger for firms more likely to have high agency costs.

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. Jensen (1986) argues that corporate managers are more likely to pay out free cash flow (rather than consume it) if the marginal cost of delegated monitoring is lower. Admati, Pfleiderer, and Zechner (1994) argue that as a firm's shares become more widely held by less informed investors, the marginal benefits of delegated monitoring decline and the costs increase. As a result, increased ownership by institutions should increase the net returns to monitoring.

Institutional investors have long been associated with shareholder activism (see e.g. Gillan and Starks, 2007), but even passive investors may help discipline managers and align incentives with shareholders. For example, Edmans (2009), Admati, and Pfleiderer (2010), and Edmans and Manso (2011) show passive investors may discipline managers through the threat of exit. Further, institutions are generally required to participate in proxy votes by law.[1] At a minimum, ownership by institutions reduces coordination costs (Grossman and Hart (1980), Shleifer and Vishny (1986)) and can lower agency costs through economies of scale in delegated monitoring. Indeed, Brickley, Lease, and Smith (1988) find that voting on governance increases with institutional ownership. Finally, Alexander et al. (2010) find that institutions often vote based on the recommendations of proxy advisers such as Institutional Shareholder Services (ISS).

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[1] 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.

Such behavior can increase coordinated voting, which can be pivotal in votes against management.

Beyond agency costs, taxes and information asymmetry may also matter for payout policy. Institutions are generally taxed less and better informed than individuals so they may prefer to hold firms with certain payout and investment policies causing segmented clienteles of ownership ((Barclay and Smith (1988), Brennan and Thakor (1990), Allen, Bernardo, and Welch (2000)). These models often predict opposite causal relationships compared to monitoring theories.

Consistent with the fundamental endogeneity between ownership structure and payout policy, the empirical evidence is mixed. Allen and Michaely (2003) provide a comprehensive survey of this large literature. They conclude that there is some limited evidence for tax-clientele effects, little evidence in favor of signaling theories, and mixed evidence on agency theories.

Some recent studies have made progress on identifying causal channels between ownership and payout policy.[2] Even so, the evidence is still decidedly mixed. For example, Grinstein and Michaely (2005) adopt a vector auto regression approach and find that institutions are attracted to firms with positive payout but find no evidence that ownership Granger-causes payouts. On the other hand, Gaspar, Massa, Matos, Patgiri and Rehman (2012) employ VARs in a different setting and find the opposite. Desai and Jin (2011) show that plausibly exogenous changes in payout policy cause changes in ownership by "dividend-averse" investors. Desai and Jin (2011) and Perez Gonzales (2003) argue that exogenous changes in tax policy cause firms to cater their payout policy to the tax preferences of their shareholders.[3]

[2] 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.

[3] In addition, Michaely, Thaler and Womack (1995) find no institutional ownership changes following dividend omissions while 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 yield and mutual funds' portfolio choice.

Our identification strategy has distinct advantages relative to prior studies. First, our setting has the advantage of not relying on institutional ownership changes for identification. Changes in ownership are not random, and it is therefore difficult to rule out the possibility that those changes are driven by unobservable determinants related to corporate policies (e.g. expectations of future policies). Our setting rules out these unobservable factors *causing* payout. As a result, while previous studies are limited only to Granger causality, we make a direct causal interpretation.

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 decision rules. For example, a stock may be included in the S&P 500 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&P 500 index are observed, the firms that are just outside are not. In contrast, our identification strategy relies on the (verifiable) assumption that firms' rankings around the Russell 1000/2000 threshold are random, and therefore so is the exposure to higher institutional ownership associated with Russell 2000 index assignment.[4]

Chang and Hong (2012) also exploit this discontinuity and find that the smaller firms that are just included in the more popular Russell 2000 index experience higher returns right after the reconstitution of the index, which the authors attribute to price pressure due to higher institutional demand for the Russell 2000 stocks.[5] Consistent with their results, we find a large and significant

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[4] While firms could try to manipulate index inclusion at the threshold, as long as firms do not have *precise* control over their assignment (which they don't since they cannot precisely control their rank relative to other firms), the discontinuity still identifies random assignment to the treatment, and the regression discontinuity design is well specified (see e.g. Lee (2008) for a formal proof.)

[5] They also document a significant increase in co-movement with other index stocks. In a previous version of their paper, Chang and Hong also document a large change in institutional ownership around the threshold. However, they also found a significant pre-treatment effect which violates the exogeneity of the selection. In contrast, our sample selection procedure (detailed in section I.C) is specifically designed to purge any pre-treatment effects.

exogenous discontinuity in a firm's ownership structure, with firms at the top of the Russell 2000 having institutional ownership that is 9% greater than firms at the bottom of the Russell 1000.

Exposure to higher institutional ownership has an effect on corporate payout policy. Firms at the top of the Russell 2000 pay $0.7M more in dividends over the next year and a cumulated $1.5M over the next 3 years, which represents a 13% increase over the median dividends for firms just in the Russell 1000. Share repurchases are also roughly $1M greater in the next year, a 22% increase relative to the median, and total payout is about 5% larger. An interesting aspect of our results is that they go against traditional size-based explanations: small firms typically pay out less cash to shareholders, but in our case, arbitrarily smaller firms at the discontinuity pay out more.

We also dig deeper into the cross-section to test whether our results are stronger for firms that may benefit more from an increase in external monitoring. When we split the sample, we find that the results are primarily driven by the firms with higher expected agency costs (low profitability, high CEO compensation, low growth options). While these measures are imperfect, they suggest payout is related to external monitoring. Furthermore, we find no differences based on analyst coverage, suggesting that the effect may not stem from a change in information asymmetry. The finding that proxy-voting participation increases by 55 percentage points around the threshold reinforces our agency costs interpretation.

In addition to measures of net payout, we also test for changes in other corporate policies. We find that higher institutional ownership causes an increase in net equity issues, total assets and R&D expense. We find no significant treatment effects for capital expenditures, executive compensation, profitability, or capital structure. While these results are mixed, they are generally consistent with the idea that lower agency costs may increase corporate investment and that long-run oriented institutional investors increase long-run R&D investment (Bushee (1998)).

We perform a battery of robustness tests. First, we replicate our results with alternative measures and scaling variables. Second, we test the sensitivity of our results to regression discontinuity methodology choices like bandwidth selection and kernel choice. Our results are robust to alternative methodologies. Third, we find that *lagged* corporate policies do not exhibit a discontinuity at the threshold, unlike policies *after* the change in index composition. This suggests that our results are not driven by pre-treatment effects caused by selection bias in the Russell index composition.[6]

Our paper makes several contributions. First, we provide evidence that institutional ownership *causes* firms to disgorge cash. We don't rule out that dividends cause changes in ownership as in Grinstein and Michaely (2005), but we show that, in our setting, ownership structure affects payout. Second, we show that institutions cause increases in investment and equity issuance. Finally, consistent with Bond, Edmans, and Goldstein (2011), and Grullon, Michenaud, and Weston (2012), we find evidence that capital market frictions, like random index inclusion, can have an important impact on the economic behavior of publicly listed firms.

The paper is organized as follows. Section I briefly discusses the empirical strategies, the data and the variables used in the tests. Section II presents the main empirical results, while Section III discusses alternative explanations, robustness checks, and additional tests. Section IV concludes.

I. Data and Methodology

A. Data

Our sample consists of the Russell 1000 and Russell 2000 index constituents from 1991 until 2008. 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 8,193 unique firms from 1991 to 2008. The average number of years for which a firm is in either the Russell 1000 or 2000 in our sample is about 11 years.

[6] Given the nature of the index inclusion rule, there is no selection bias for the inclusion in the indices. We discuss selection issues related to the index weights assigned by Russell to firms in the index to adjust for the level of investible shares in section I.C.

{Insert Table I about here}

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 discontinuity, firms that are just in the smaller index - the Russell 2000 - pay out more of their cash flows and hold less cash than the firms that are just in the larger index - the Russell 1000. Therefore our results go against a purely size-based explanation.[7]

B. Regression Discontinuity Methodology

To measure the effect of the Russell index assignment on various firm policies, we implement a regression discontinuity methodology similar to Imbens and Lemieux (2008) and Lee and Lemieux (2010). The basic idea is that we have an exogenous discontinuous variable that drives selection of observations into a treatment or control group around the discontinuity. Assignment of observations to either the left or right of the discontinuity is random, at least near the discontinuity. If this is the case, then we can measure a treatment effect by comparing data from one side of the break-point to the other side. As long as assignment around the discontinuity is not caused by any variable of interest prior to the assignment, then we can make causal inference. Comparison of the data on either side of the discontinuity typically proceeds by some form of local regression, on either side of the break-point, within some reasonably close proximity.

[7] We present a detailed discussion of pre- and post-treatment firm characteristics around the discontinuity in section I.C.

In our setting, we argue that Russell index assignment is random close to the 1000/2000 threshold. Close to the 1,000th ranking, differences in market capitalization are very small (within 2% of a standard deviation of intraday return standard deviation) and so assignment to the left or right of the break-point is essentially random. Since assignment is based on very small differences in market capitalization rankings, it should be independent of firm characteristics prior to the assignment. Given this setup, we can measure firm financial policies on both sides of the threshold and test for any differences in those policies.

There are good reasons to expect differences around the 1000/2000 threshold. 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 and Hong (2012) report that in 2008 the amount of institutional assets benchmarked to the Russell 2000 index was $264bn while only $169bn was benchmarked to 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 only 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. On the other hand, funds tracking the Russell 1000 could hold none of the smallest firms in the index with no real impact on performance metrics. The combination of the total benchmarked dollars and the difference in the relative index weights motivates 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.

Figure 1 shows the non-linear properties of the index weights around the threshold. As a result, there are two main methodological choices we have to make in implementing the RDD approach: how to define the neighborhood or bandwidth around the threshold and how to model the local behavior of the data around the break-point. Our approach follows a standard sharp regression discontinuity design format as described in Imbens and Lemieux (2008) and Roberts and Whited (2012). As a starting point, we define the neighborhood around the discontinuity using the Rule of Thumb (ROT) plug-in estimator (Fan and Gijbels (1996)). We then re-calculate results over a continuum of bandwidths around this "optimal" neighborhood to check the sensitivity of our estimates. In order to measure the treatment effect, we fit the data using semi-nonparametric local polynomial regressions on each side of the threshold. Once we have estimates for the expected value just-to the left and just-to-the-right of the discontinuity, we can test for differences and estimate the treatment effect. The details of our procedures, including our optimal bandwidth selection, semi-nonparametric regression estimation, and standard errors are described in Appendix 1.

C. Randomness of the Index Assignment and Float Adjustment

Our research design relies on the conditions of regression discontinuity being well specified in our setting. However, there are number of adjustments made by Russell related to the selection into the index and the subsequent determination of index weighting. These adjustments have the potential to induce pre-treatment effects in our sample and violate our assumption of randomness around the threshold. In this section, we describe the sample selection procedure we use to mitigate any confounding effects of the adjustments made by Russell.

The Russell indices are constituted once every year using data as of May 31st each year, and announced at the end of June. The Russell 1000 represents the 1,000 largest firms by market value of equity and the Russell 2000 is the next 2,000 largest firms. 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 shares outstanding.

Our identification relies on the notion that assignment to an index is random around the 1,000th rank threshold. In fact, the assignment to the index is based solely on the total market capitalization of the firms at the end of May. As a result, a firm's rank on May 31st, within a certain range, should be orthogonal to firm policies. For example, it is possible for a firm to be ranked 999 on May 30th, and 1001 on the 31st. This would lead to a different index assignment, but is unlikely to be based on future expectations of financial policy. We can replicate the actual Russell index assignment using only CRSP market capitalizations with 98% accuracy. While this seems to satisfy our identification requirements, when we actually implement our regression tests we must look at firms very close to the Russell 1000/2000 threshold, so any adjustment made by Russell that affects firms close to the threshold may violate our assumption of randomness.

The first adjustment that Russell makes is 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 has not been applied systematically prior to 2007. In our replication of the index assignments, we find that this affects less than 1% of our sample. Nevertheless, our results do not change if we exclude data after 2007. This adjustment is easy to identify and does not appear to cause significant problems for our discontinuity design.

The second adjustment made by Russell relates to the public float. Once each firm is assigned to an index, Russell then assigns index weights based on market capitalization adjusted for investible shares (e.g. treasury stock, block holders etc.). However, 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, float adjustment by Russell may change the ranks of firms relative to the threshold decision made based on unadjusted shares. For example, if a Russell 1000 firm ranked very highly in terms of market capitalization has a large float adjustment, the adjustment may push it near the threshold. If this happens, the largest firms in the

Russell 2000 may be different from those firms with the lowest weights in the Russell 1000. In essence, we could find ourselves comparing firms that were not "neighbors" when the index break-point was determined.

To address this issue, we first examine the nature of the float adjustment. While we cannot directly observe the adjustment, we can easily proxy for it because we observe both the adjusted and unadjusted weights. We calculate the percent difference between the unadjusted weight and the adjusted weight used by Russell. We call this the weight prediction error and large values indicate a large float adjustment. We find that firms with the lowest index weight in the Russell 1000 have large float adjustments, and are persistently ranked at the lowest ranks of the Russell 1000 index. This obviously introduces a problem of non-random proximity to the threshold.

However, it is important to note that these adjustments are not related to our financial policy variables of interest. Below, we report the results of a regression where we model the weight prediction error (WPE) using lagged values of the error, stock returns (R), current and past market values (MV), dummy variables for past inclusion in the index (I) and our corporate variables of interest. We estimate the following relationship with T-statistics reported underneath OLS point estimates:

$$\begin{aligned} WPE_{i,t} = & -\underset{[-4.1]}{0.045} + \underset{[20.6]}{0.83} WPE_{i,t-1} - \underset{[-0.3]}{0.20} R_{i,April} + \underset{[1.7]}{4.0} MV_{i,April_t} - \underset{[-2.9]}{6.0} MV_{i,April_{t-1}} \\ & + \underset{[3.2]}{0.04} I_{Russell2000,t-1} - \underset{[-2.9]}{0.03} I_{Russell2000,t-2} \\ & + \underset{[0.43]}{0.4} Volatility_{t-1} + \underset{[0.92]}{0.3} \ln(Dividends)_{t-1} \\ & + \underset{[0.16]}{0.1} \ln(Repurchase)_{t-1} + \underset{[0.78]}{0.5} \ln(Cash)_{t-1} \end{aligned}$$

We find that the weight prediction error is persistent and that the previous year's adjustment has the largest effect on the current year. Beyond that, the only variables that contribute significantly to this error are the size of the firm and the index assignment (1000 vs. 2000). These are mechanical determinants in that larger firms within each index have large ranking errors. We also see that the economic impact of these is small. A one standard deviation change in market capitalization predicts a 0.004 standard deviation increase in the ranking error. More importantly, we see that these ranking errors are not a significant function of any corporate policy variable.

While this adjustment by Russell should not invalidate our causal inferences (since it is orthogonal to firm characteristics), we want to ensure there are no lingering selections biases that might drive unknown pre-treatment effects for our sample. As a result, we simply drop observations in the top 5% of squared ranking errors. The basic idea here is to identify the firms that Russell has made a large share adjustment to, and remove them from the sample, while still using the information in the actual index weights assigned by Russell, since it's the actual index weights that drive institutional holdings[8]. Since we can identify the non-randomly selected firms and remove them, we can hopefully ensure random characteristics around the threshold for the remaining 95% of the sample.

To test whether the adjustments made by Russell leave any remaining selection bias after our filter, we compare firm characteristics at the threshold before the index inclusion and rank allocation by Russell. After excluding the 5% of observations with large adjustments, we use the regression discontinuity methods described above to measure a variety of firm characteristics to the right and the left of the threshold in the year *prior* to the index assignment. If our filtered sample is random, we expect no pre-treatment effects in firm characteristics around the threshold.

{Insert Table II about here}

Table II 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 selection bias around the threshold.

[8] Chang and Hong (2012) deal with this issue by using the unadjusted market weights. This makes sense given the questions they address. In our setting, because the index weights are the underlying cause of the institutional ownership and given the longer term nature of our tests, we feel our adjustment is appropriate. However our payout results are qualitatively similar when using an unadjusted market weight ranking.

Importantly, there are no significant differences in institutional ownership, dividend policy, or share repurchases. Our process of identifying and removing observations contaminated by the Russell adjustment appears to preserve randomness around the threshold and ensure the conditions for a regression discontinuity approach.

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 and alter our causal inferences. 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. In our case, assignment to an index is related to their size *ranking* at the threshold. It is unlikely firms could self-select on one side of the threshold, and the regression discontinuity design should be well specified. Lee (2008) formally shows that even in the presence of manipulation, an exogenous discontinuity still allows for random assignment to the treatment as long as firms do not have precise control over their assignment.

II. Main Results

A. Higher Exposure to Institutional Ownership

In this section we test whether the discontinuity in index weights around the Russell 1000/2000 threshold leads to a discontinuity in institutional ownership. This result is central to our identification strategy because a discontinuity in institutional ownership enables us to identify a causal impact of institutional ownership on payout policy.

Figure 2 presents our RDD analysis for *Institutional Ownership*. We follow the standard approach of presenting our results primarily with figures (Imbens and Lemieux (2008)). In Panel A we plot average institutional ownership (in 10 rank "bins" for smoothness) 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 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 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 few institutions tracking this benchmark have little need to hold these firms, on average.

{Insert Figure 2 about here}

The magnitude of the discontinuity is large. The largest firms in the Russell 2000 have an institutional ownership that is 9.2% percentage points higher than the smallest firms in the Russell 1000. This is roughly a 16% increase relative to average ownership just to the left of the threshold.[9] This difference is also statistically significant. In Figure 2, Panel B, we report our local polynomial estimates and confidence intervals (see Appendix 1 for details). The discontinuity is represented graphically by the difference in the polynomial estimates at the threshold. The lack in overlap of the confidence bands from the two polynomial estimates demonstrates the statistical significance of the effect. In order to see the effect closer to the threshold, we also present Panels C and D of Figure 2, which simply repeat Panels A and B for ranks close to the threshold.

Our results are not generally sensitive to our methodology choices. In panel E, we replicate the analysis for different bandwidths around the threshold. Even over smaller and larger bandwidths the discontinuity is consistent in both economic and statistical significance. Finally, in panel F, we report discontinuity estimates over 1,000 placebo thresholds. Placebo thresholds are formed by randomly selecting a different market capitalization rank and re-estimating the

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[9] We find that about 3/4 of the increase is generated by an increase in “transient institutional investors”, investors who selectively pick stocks and may benchmark but not necessarily track an index (Bushee (1998)). Further discussion of this result is presented in section II.D.

treatment effect with the regression discontinuity design. The vertical line represents the discontinuity at the true threshold. It is clear from the histogram that the effect we measure is unlikely to be driven by chance: the 1000/2000 threshold is the one that matters.

B. Institutional Ownership and Payout Policy

Since we have identified exogenous variation in institutional ownership, we test whether this difference in ownership has an effect on payout policy. Our analysis follows the same regression discontinuity design we employ for institutional ownership. We first plot the mean *Total Dividends* across all years over 10 rank intervals in Figure 3, panel A, for 100 bins to the left of the threshold (firms in the Russell 1000) and for 200 bins to the right of the threshold (firms in the Russell 2000). We focus on the log levels because firm size is very similar around the threshold.[10] The graph shows a clear discontinuity in *Total Dividends* at the threshold. Firms that are at the top of the Russell 2000, the index of smaller firms, have higher *Total Dividends* than the firms that are at the bottom of the Russell 1000 index. This is all the more surprising given that smaller firms typically have lower dividends (as evidenced by the generally downward sloping nature of the data). Figure 3 Panel B adds the fitted third degree polynomial estimate to the right and to the left of the threshold with the 90% confidence interval around the fitted values. Both panels point to a significant discontinuity around the threshold. Figure 3 Panel C and D present the same results as Figure 3 Panel A and B with a zoom in around the threshold. These graphs point to a causal effect of institutional ownership on dividend payout.

{Insert Figure 3 about here}

The discontinuity estimates at the threshold for all the payout policies variables are presented in Table III. The difference in dividends at the discontinuity is equal to $0.73M and is statistically significant at the 1% level. This represents an increase in dividends of approximately 13% relative to the average dividends of firms just in the Russell 1000. The economic

[10] Our results are robust to using yields as well as levels.

significance of these results is quite large and supports the idea that the almost 16% increase in institutional ownership (relative to the ownership to the left of the threshold) causes an 13% increase in dividends paid relative to the dividends of those same Russell 1000 firms. Assuming that the causal impact of an increase in ownership on dividends paid is linear, this suggests that every 1% increase in institutional ownership causes an increase in dividends of 0.8% over the original dividend amount.

The discontinuity estimate is not sensitive to the size of the bandwidths to the left and to the right of the threshold. In Figure 3, Panel E, we plot the value of the discontinuity estimates and the 90% confidence interval as a function of the increase (or decrease) in percentage of the original rule of thumb optimal bandwidth. Except for very small optimal bandwidths (bandwidths more than 50% smaller than the original optimal ROT bandwidth), discontinuity estimates are stable around the point estimate of 0.5 (approximately $73M) and statistically significant at the 10% level using bootstrapped standard errors.[11] Again, our analysis of 1,000 placebo thresholds presented in Panel F suggests that our results are unlikely to be driven by chance.

We conduct the same discontinuity analysis around the Russell 1000 threshold for *Share Repurchases* and *Total Payout* using log transformations of these variables. We present the results of the analyses in Table III.

{Insert Table III about here}

{Insert Figure 4 about here}

Figure 4 shows a clear discontinuity in *Total Payout*. Firms just included in the Russell 2000 have higher *Total Payout* than firms just included in the Russell 1000. Panels A-D point to a visibly significant discontinuity around the threshold. The dollar increase in total payout at the discontinuity is $0.75M and is statistically significant at the 1% level. This represents an increase in total payout of 4.7% relative to the median total payout of ($16M) at the bottom of the Russell

[11] See the discussion in Appendix 1 for more information on the bootstrap procedure.

1000. For brevity we only tabulate the results for *Share Repurchases*. Table III shows a dollar increase in share repurchases at the discontinuity is $0.93M and is statistically significant at the 1% level. This represents an increase in share repurchases of 22% relative to the median share repurchases of $4M for firms at the bottom of the Russell 1000.

Finally, Figure 5 presents results related to *Cash Holdings*. Consistent with the increased payout evidence described above, we find that firms with higher institutional ownership hold less cash. Panels A and B show that those firms just to the left of the threshold (the smallest firms in the Russell 1000) have higher cash holdings. The point estimate of the discontinuity suggests that firms to the right of the discontinuity hold $0.32M less in cash than those firms to the left. This represents an approximately 0.3% decrease relative to the median holdings at the bottom of the Russell 1000.

{Insert Figure 5 about here}

Taken together these results point to a causal effect of institutional ownership on dividend payment, share repurchases, and total payout. Institutional shareholders force managers to disgorge more cash 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)). In the next section, we explore the channel through which institutional investors affect payout.

C. 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. We further test whether payout is more sensitive to institutional ownership for firms with bigger agency problems. Generally, our predictions stem from the idea that monitoring by institutions forces managers on net to disgorge cash.

To measure monitoring 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. Using the RDD, we find that the proxy-voting participation is 55 percentage points larger for firms that are just included in the Russell 2000. The results are presented in Figure 6.[12] This result is consistent with past studies that find institutions vote more actively and often oppose management proposals (Brickley, Lease, and Smith (1988)), are more successful at getting shareholders proposals supported (Gillan and Starks (2000)), and act as substitutes for other governance mechanisms (Gillan and Starks (2005)). More generally, our result is consistent with institutional investors reducing agency costs of free cash flows (Jensen (1986)) and provides evidence of a direct monitoring channel.

{Insert Figure 6 about here}

To test whether there are differences for firms with high expected agency costs, we dig deeper into the cross section of our results. We sort firms based on the probability of being subject to high or low agency costs ex-ante. Stable, cash rich firms without many growth opportunities, with powerful rent extracting CEOs are typically expected to suffer more from agency costs of free cash flow.

We rely on four proxies for agency costs. The first proxy is the market-to-book ratio. Firms with low market-to-book ratios have lower investment opportunities and are typically more likely to suffer from agency costs (see e.g. Lang and Litzenberger (1989), Yoon and Starks (1995)). Second, we compare firms with high cash flows and low market-to-book ratio to firms with low cash flows and high market-to-book. Third, we use profitability, as measured by return on assets. Firms that have low ex-ante profitability are expected to suffer more from agency costs than highly profitable firms. And finally, we use total CEO compensation under the hypothesis that CEO compensation reflects the bargaining power of the CEO (see e.g. Bebchuk and Grinstein (2005), or Bebchuk and Fried (2006)).

[12] Our results are based on a logit transformation of voting participation percentage in order to bound our predictions between 0 and 1 for clarity. Results are qualitatively unchanged for the non-transformed data.

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 RDD analysis for high vs. low agency costs and test for significant differences in the discontinuity estimates between the two groups of firms. We present our results for *Total Payout* but our results are qualitatively similar for *Dividends* and for *Share Repurchases*.[13]

In Table IV, we find that firms with low market-to-book ratio, low market-to-book and high cash flow firms, low profitability, high CEO compensation seem to drive the effect we observe in the overall sample.[14] The economic magnitude of the difference between the high agency costs firms and the low agency costs firms is large. However, these tests have low power relative to our full sample estimates, and as a result not all differences are statistically significant at the conventional levels. Jointly however, our results are meaningfully different for firms we believe to suffer from higher ex-ante agency costs (p-value<.01) and are at least suggestive that agency may underlie our findings.

Finally, we also find that firms with higher analyst coverage have slightly lower discontinuity estimates, and firms with high response to earnings surprises have lower higher discontinuity estimates. However, the differences are statistically insignificant. 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 and exit.

{Insert Table IV about here}

[13] These results are not tabulated in the interest of space but are available from the authors upon request.
[14] We also use measures of corporate governance by Gompers, Ishii, and Metrick (2003), the G index, and by Bebchuk, Cohen, and Ferrell (2009), the E index in untabulated analyses. The results with these measures are directionally similar but not significant due to the low number of observations at the threshold.

D. Institution type and monitoring

In unreported analysis, we find that three quarters of the difference in institutional ownership (7% out of the 9.2%) comes from institutional investors that Bushee (1998) defines as "transient institutional shareholders". These institutions actively manage their portfolios, thus inducing high turnover in their stock holdings. While this result may seem surprising at first, many of these institutions benchmark their performance against a popular index - such as the Russell 2000 - and therefore have to manage tracking error relative to that benchmark. So, even these active managers have a large incentive to hold the biggest stock in their benchmark while at the same time having very little incentive to hold the smallest. About a quarter of the difference in institutional ownership is also coming from institutions Bushee (1998) defines as "quasi-indexers" (most index funds would fall into this category). These are institutions that obviously have considerable tracking error incentives as well.

Prior studies, such as Bushee (1998) and Gaspar, Massa, Matos, Patgiri, and Rehman (2012) show that "transient" institutional ownership is associated with increased myopic behavior on the part of managers. They suggest that short-term oriented institutional investors are less effective monitors relative to those with longer horizons, *ceteris paribus*. Our results point to the effects of the *level* of institutional ownership. We show that the higher levels of overall institutional ownership more than offset any reduction in monitoring related to the ownership composition in the largest Russell 2000 firms. This is not entirely surprising. Institutions are generally required to vote their proxies as a result of rules established in the Employment Retirement Income Security Act (ERISA) of 1974 and SEC's Proxy-Voting by Investment Advisers rule (2003). Proxy advisory services, such as ISS, provide services to help coordinate voting across institutions (Alexander et al. (2010)). These features are independent of institution investment horizon, so we expect more ownership to improve monitoring even though the composition tilts toward "transient" owners. Additionally, several papers have shown that firms have an incentive to respond to passive institutions due to the threat of exit (Edmans (2009), Admati, and Pfleiderer (2010), and Edmans and Manso (2011)).

E. Institutional Ownership and Other Corporate Policies

In the previous sections, we show that higher institutional ownership causes firms to payout more cash to shareholders and we provide evidence suggestive of an agency explanation. In this section, we further test whether institutional ownership has a causal effect on investment policy and security issuance behavior.

In Table V, we show that increased institutional ownership causes an increase in financing activity, specifically *Net Equity Issuance.* Table V shows that just to the right of the threshold, *Net Equity Issuance* is higher by approximately $1.1M dollars. This is a more than 19% increase relative to the median issuance activity for the smallest firms of the Russell 1000. This result is, again, consistent with better monitoring by institutions enabling firms to reduce the adverse selection costs of issuing new equity. It is also consistent with an information asymmetry interpretation of the results that we discarded in the previous section. [15]

{Insert Table V about here}

We also show results consistent with Bushee (1998). The reduction in agency costs associated with higher institutional ownership appears to cause a higher investment in research and development. Due to the large number of firms with no R&D spending, we focus on comparing firms across the threshold that have some R&D costs. This allows us to test the hypothesis that firms increase *R&D Expenses*, rather than focusing on initiation of R&D.[16] Economically, this represents a discontinuity of approximately $0.5M. This is almost a 2%

[15] It should be noted that an alternative interpretation of the results is possible. Firms just to the right of the threshold opportunistically take advantage of the short-run price increase caused by the Russell 2000 inclusion (Chang and Hong, 2012). However opportunistic equity issuance following such a short-run price run-up seems implausible given the timing constraints to issue new equity.

[16] We see very little evidence of R&D initiation across the threshold. As a result, when we include all the non R&D firms, our estimates are substantially smaller.

increase for R&D firms just to the right of the threshold relative to the median positive R&D expense of firms to the left.

Table V also reports results consistent with Bertrand and Mullainathan (2003). The reduction in agency costs associated with higher institutional ownership appears to cause an increase in total asset growth consistent with firms investing more when managers become more closely monitored and can no longer enjoy the "quiet life".

Overall, these results suggest that institutional ownership causes an increase in R&D investment, corporate investment, as well as equity financing. Taken in light of the results on payout policy presented above, these results are consistent with institutional investors reducing agency costs in firms. We want to interpret these results with some degree of caution relative to the payout results described in Section II.B. In this case, the size effect in the Russell index weights assignment works in the same direction as the measured effects. We would expect firms to the right of the Russell 2000 threshold to be growing.

III. Alternative Explanations and Robustness tests

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

First, we examine several different regression discontinuity estimates. In untabulated results, we calculate discontinuity estimates using two alternative kernel functions, the rectangular kernel and the triangular kernel, in addition to our main specification (the Epanechnikov kernel). While the Epanechnikov kernel was chosen for its asymptotic properties, we find that the kernel choice matters very little in this case, and our results are not sensitive to our choice of kernel. We also use alternative specifications for estimation of the semi-nonparametric regressions. Using different odd-ordered polynomials (Fan and Gijbels (1996)) we find little difference between our primary specification and higher or lower order estimates. Robustness tests with respect to bandwidth choices are presented throughout the figures. The sensitivity range presented captures other common bandwidth choices (specifically the Cross Validation bandwidths) and our results are generally robust to those sensitivity analyses (see Panel E of Figures 2-6).

More importantly, we conduct falsification tests on our dependent variables around the threshold. We run our RDD analysis using three, two, and one year(s) lags of all independent variables relative to the contemporary index assignments. We find no evidence of a discontinuity for the lagged variables around the contemporaneous Russell 1000/2000 threshold. Results for the one-year lags are shown in Table II. This (along with the WPE results presented in section I.C) confirms the exclusion restriction: index assignment does not appear to be a function of past firm characteristics.

Finally, we use a number of alternative definitions of dividend, share repurchases, total payout, corporate investment, and financing variables. Our results are robust to these alternative definitions.

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 don't find such conditions plausible. First, we test whether there are differences in the effect for years in which the dividend catering premium of Baker and Wurgler (2004) is high vs. low and find no difference in the treatment effect. Second, Hoberg and Prabhala (2009) find that the dividend catering premium is closely associated with firm risk, and we find no pre-treatment or treatment effects related to firm risk.

IV. 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 observed discontinuity at the threshold to identify a causal impact of institutional ownership on dividends, share repurchases, total payout policy, and other corporate policy variables. We find that higher institutional ownership causes an increase in the redistribution of cash to shareholders. In addition, we find that institutional ownership increases proxy-voting participation, and suggestive evidence that institutional ownership increases R&D expenses, corporate investment, and equity financing. Firms that are prone to agency conflicts primarily drive our results, suggesting that institutional investors play an important role in reducing manager/shareholder conflicts. Our results speak to the impact of delegated monitoring (i) on the firms' propensity to disgorge cash to shareholders (Jensen (1986)), and (ii) on the improvement in the financing ability and in the capital budgeting policy of firms. Our results also suggest that stock markets have a large causal impact on the real economy: a random inclusion in a stock market index can have a significant influence on the managerial behavior and corporate policy.

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Appendix 1

Regression Discontinuity Design Methodology

Regression Discontinuity Design (RDD) is an empirical technique designed to evaluate the effects of a treatment when treatment is a discontinuous function of an underlying continuous forcing variable. In the context of this paper, treatment is exposure to higher levels of institutional ownership as a function of the continuous forcing variable, market capitalization. Treatment is determined when firms are above (or below) a known threshold (e.g. rank 1001 in market capitalization as of May 31 of each year) and economic agents must not be able to self-select in -or out of- treatment. The intuition is that, around the threshold firms will be similar on average because around the cutoff point, assignment is essentially random. In order to evaluate the treatment effect, we must measure the difference in the outcome variable for the average treated firms that lie just on either side of the threshold. In practice, measuring differences in outcome just to the left vs. just to the right of the threshold requires the researcher to make a number of estimation decisions. Following Imbens and Lemieux (2008), we note *Y(0)* the outcome without exposure to the treatment and *Y(1)* the outcome given exposure to the treatment. We also note c the cutoff point at which the forcing variable X causes treatment, $u_r(c)$ the predicted value of the outcome variable at the threshold from the right, and $u_l(c)$ the predicted value of the outcome at the threshold from the left. The average treatment effect, τ, of being included in the Russell 2000 at the discontinuity point is given by the difference in these two predicted values:

$$\tau_{Russell2000} = u_r(c) - u_l(c) \qquad (1)$$

where

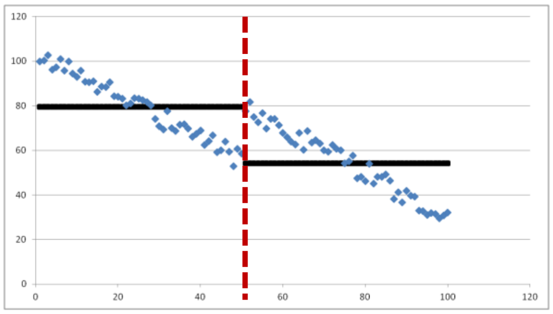
$$u_l(c) = \lim_{x \uparrow c} \mathrm{E}[Y(0)|X = x]$$

and

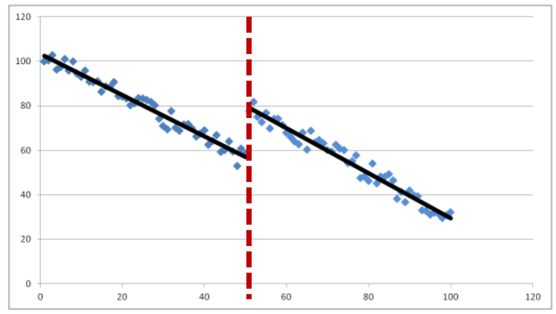
$$u_r(c) = \lim_{x \downarrow c} \mathrm{E}[Y(1)|X = x]$$

τ can be interpreted as the average causal estimate of the treatment at the Russell 2000 cutoff point. In principle, $u_r(c)$ or $u_l(c)$ can take any functional form estimated using parametric or nonparametric techniques. In early applications of the regression discontinuity approach $u_l(c)$ was merely the sample mean estimated over some bandwidth to the left of the threshold, compared to $u_r(c)$ the sample mean estimated over some bandwidth to the right of the threshold (Thistlethwaite and Campbell (1960)). However, in practice, using the sample mean potentially exposes the researcher to wrong inference if the forcing variable and the outcome variable are correlated. For instance, if dividend payment is an increasing function of size, and if the bandwidth is large enough, one may infer that the discontinuity is negative, when it actually is positive. We illustrate this potential shortcoming with simulated data around a fictitious threshold. Using a sample mean functional form for $u_r(c)$ and $u_l(c)$, the discontinuity estimate is -25. In contrast, using a linear functional form, the discontinuity estimate is +25.

Mean functional form

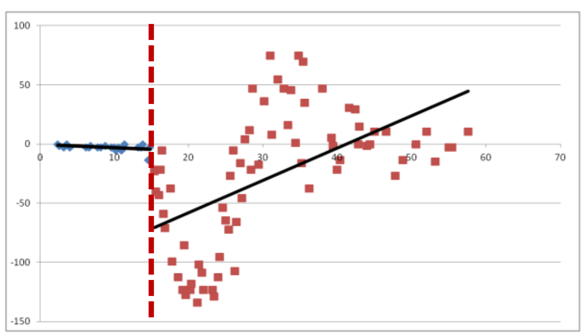


Linear functional form



Likewise, linear estimation can be misleading. It may force identification of a discontinuity when there is none. The data presented below illustrates this issue using data from Silverman (1985) and a fictitious discontinuity.

Linear functional form



To abstract from the issues illustrated above, we measure the effect of the threshold on financial policies using a local polynomial regression to estimate $u_l(c)$ and $u_r(c)$ (Fan and Gijbels (1996), Imbens and Lemieux (2008)) using data just to the left of the threshold and just to the right of the threshold. This semi-nonparametric approach allows for a nonlinear effect on either side of the threshold.

Our results present a local polynomial specification using a third-degree polynomial with an Epanechnikov kernel. We base these choices on the results of Fan and Gijbels (1996) who suggest the use of an odd-degree polynomial as these perform better at boundary points. They also show that the Epanechnikov kernel minimizes the mean squared errors and mean squared integrated errors of the estimates. Our results are robust to other kernels (rectangular and triangular) and the use of lower order polynomials (first degree (linear estimation), or degree 0 (sample mean estimation)) and higher order polynomials (fifth degree).

In order to fit the local polynomial regressions, we must determine a bandwidth over which the estimates are calculated. It is well understood that the bandwidth is a critical component to the estimation of the regression discontinuity effects (e.g. Bakke and Whited, 2011). The bandwidth choice trades off power (more data allows for better estimates) against bias (the farther away from the threshold, the more likely we are to achieve biased estimates because the treatment is less random in nature.) We start by using the Rule of Thumb (ROT) plug-in estimator, shown by Fan and Gijbels (1996) to be the most efficient in terms of mean squared integrated errors. We then calculate results over a continuum of bandwidths around this "optimal" bandwidth to check the sensitivity of the discontinuity estimates. The ROT estimator is given by:

$$\hat{h} = C_{0,p}(K)\left[\frac{\hat{\sigma}^2\int w_0(x)dx}{\sum_{i=1}^{n}\{\hat{m}^{(p+1)}(X_i)\}^2 w_0(X_i)}\right]^{\frac{1}{2p+3}} \tag{2}$$

where $C_{0,p}(K)$ represents a constant function of the kernel chosen for the estimation given by:

$$C_{v,p}(K) = \left[\frac{(p+1)!^2(2v+1)\int K_v^{*2}(t)dt}{2(p+1-v)\{\int t^{p+1}K_v^*(t)dt\}^2}\right]^{\frac{1}{(2p+3)}} \tag{3}$$

where $K_v^*$is the equivalent kernel for the values of v and p (Fan and Gijbels (1996)).

The estimate in (2) is calculated by starting from an asymptotically optimal pilot bandwidth and fitting a polynomial of order $p + 3$ globally to $m(x)$ to estimate the unknown quantities $\sigma^2(.)$, $m^{(p+1)}(.)$, and $f(.)$.

The shape of the data to the left of the threshold may differ substantially from the data to the right. As a result, we may have very different optimal bandwidths to the right and left. In examining the sensitivity of estimates for smaller and larger bandwidths, we do so proportionally to the original ROT bandwidth estimates.

After choosing the optimal bandwidth, we estimate the local polynomial regressions to the left and the right of the threshold. We then measure the distance between these two estimates at the threshold point. In order to test the difference between these two estimates, we bootstrap the standard errors, sampling with replacement from the data, and impose a restriction that ensures sampling from each decile of ranks in the Russell 3000 (to ensure some data around the threshold). These results are robust to an unrestricted bootstrap sample.

To further demonstrate the significance of the threshold effect we report, we use a falsification exercise in which we use the above estimation technique over a large number of randomly selected placebo thresholds and report the distribution of measured discontinuities. These results demonstrate that not only is the effect statistically significant at the true threshold, it is larger than the effects measured at random thresholds. This provides for a robustness check in case the methodology over rejects the null at any threshold by comparing the magnitudes of the estimates at the true threshold against the estimates over 1,000 placebo threshold tests (see section III for further discussion.)

Valid causal identification of the regression discontinuity design relies on the randomness of the allocation around the threshold. This assumption can be tested empirically by looking at firms' characteristics in the year prior to the treatment, and ensuring that these firms are similar around the threshold. We discuss issues related to randomness in section I.C.

The randomness argument is also key to understanding why the study of "switchers", firms that move from one index to the other, is not helpful in identifying a causal treatment effect in our case. When looking at all "switchers" the causal inference is invalidated by the fact these firms changed index for a reason that may be related to the corporate policy of interest (e.g. firms may increase dividend payment if they become smaller and move from the Russell 1000 index to the Russell 2000 index because their investment opportunities shrink.) To avoid this selection issue, we would need to restrict ourselves to firms that have moved from one index to the other with only minor changes in their relative market capitalization ranking. In our case this would require that firms be on different sides of the threshold in consecutive years but be within a small bandwidth of the threshold in both. This constraint severely restricts the size of the sample over which we can base our inference. We only find around 10 switchers each year on either side of a 100 rank bandwidth. Like most regression discontinuity design studies, we do not look at switchers in this paper for this reason.

Appendix 2
Definition of Main Variables

Book Leverage	Compustat Total Debt (DLC + DLTT) scaled by total assets (AT)
Cash Holdings	Cash and Short Term Investments (CHE) scaled by total assets (AT)
Institutional Ownership	Thomson 13F Shares Held summed across all institutions scaled by CRSP shares outstanding (SHROUT)
Ln (Cash Holdings)	Ln (Cash and Short Term Investment (*CHE*))
Ln (Dividends)	Ln (Compustat Dividends (DVC+DVP))
Ln (Net Equity Issuance)	Ln (Sale of Common and Preferred Shares (*SSTK*) -Share Repurchases (PRSTKC))
Ln (Net Financing)	Ln (Net Equity Issuance+Change in Total Debt [(DD1+DLTT)-(DD1+DLTT)$_{t-1}$])
Ln (Repurchases)	Ln(Purchase of Common and Preferred Shares (PRSTKC))
Ln(R&D Expense)	Ln(Research and Development Expenses (*XRD*))
Ln (Total Payout)	Ln (Compustat Dividends (DVC+DVP) plus Purchase of Common and Preferred Shares (PRSTKC))
Market-to-Book ratio	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*).
Market Leverage	Compustat Total Debt (DLC + DLTT) scaled by Market value of equity (*PRCC* x *CSHPRI*)
Market Value	CRSP Price (PRC) multiplied by shares outstanding (SHROUT)
ROA	Operating Income Before Depreciation (OIBDP) scaled by lagged total assets (AT)

Figure 1 - Russell Index Weights Around the Threshold

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.

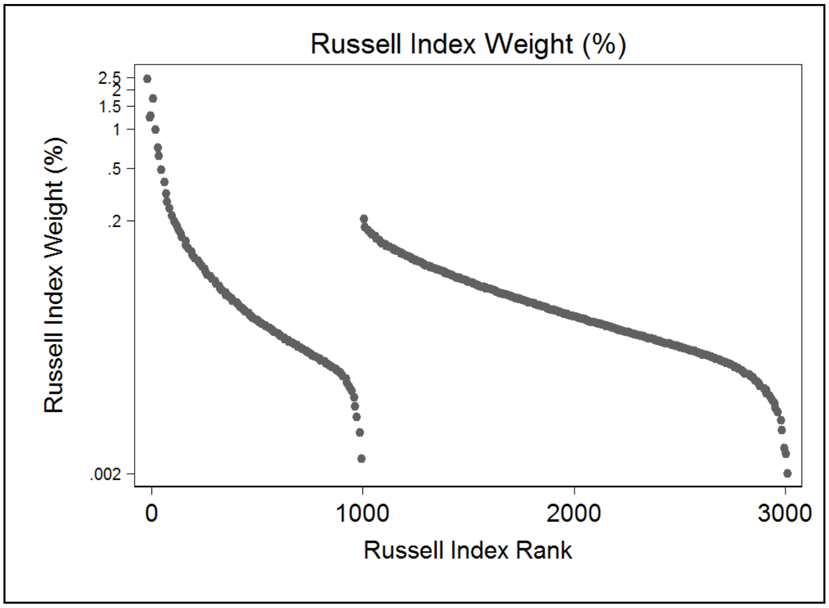


Figure 2 – Institutional Ownership Discontinuity

Panel A, B, C, and D 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, 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 local polynomial regression estimates and the associated 90% confidence bands using the Rule of Thumb (ROT) optimal plug-in bandwidth estimate of Fan and Gijbels (1996). Panel E shows the estimate of the Total Institutional Ownership discontinuity for the same period. The X axis represents the size of the bandwidth used in the estimation as a percentage of the optimal ROT bandwidth, with 0 representing the estimate at the ROT choice. The solid line is the point estimate of the discontinuity and the dashed line represent the 90% confidence bands from bootstrapped standard errors. Panel F shows estimates of the Total Institutional Ownership discontinuity for simulations of 1000 random placebo thresholds. The vertical dashed line represents the estimate at the true threshold.

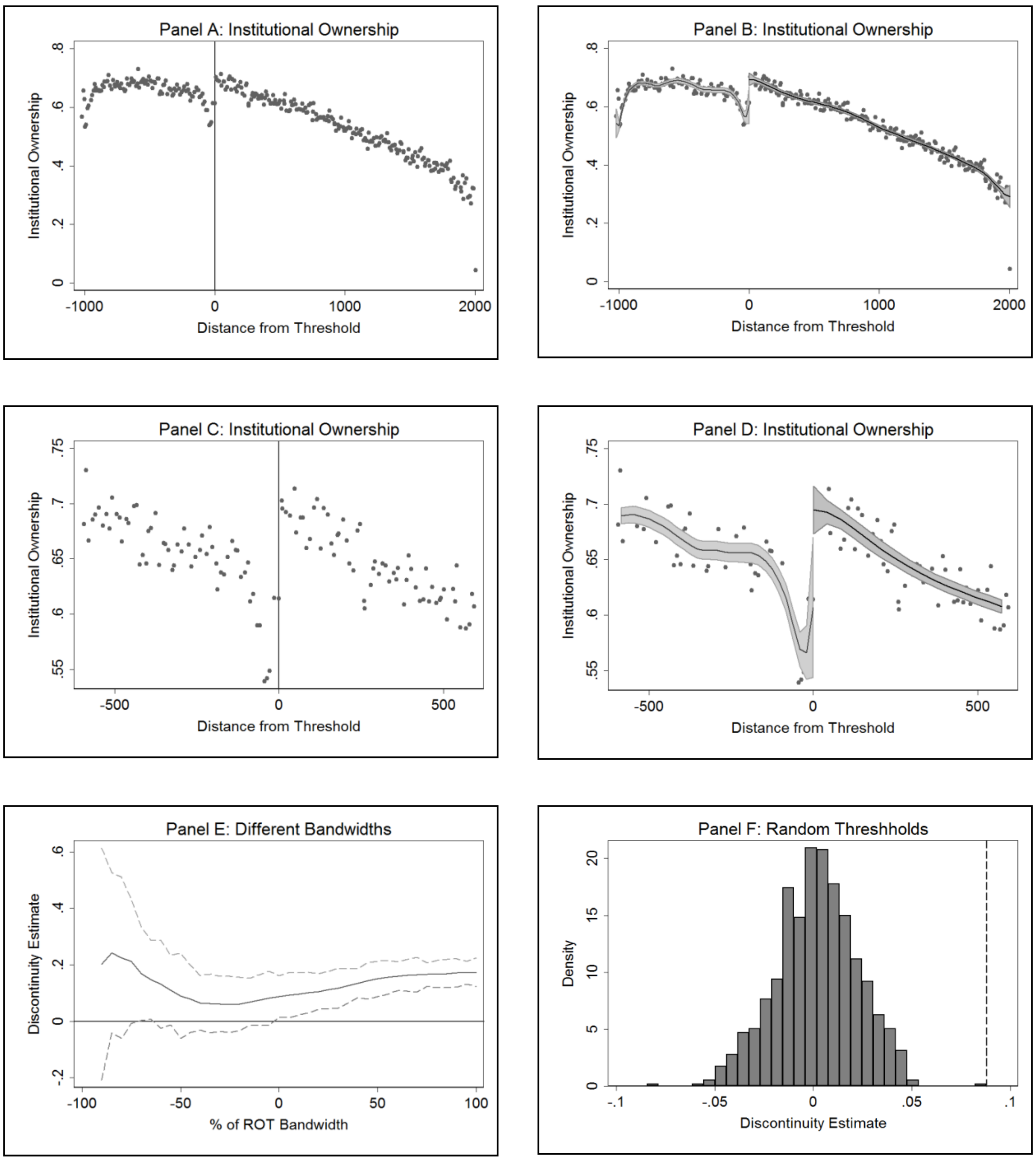


Figure 3 – Dividends Discontinuity

Panel A, B, C, and D show the Ln(Dividend) for the first fiscal year ending at least 12 months 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, with 0 representing the last firm in the Russell 1000. Panel A plots the average Ln(Dividend) over 10 ranks across all years. Panel B adds local polynomial regression estimates and the associated 90% confidence bands using the Rule of Thumb (ROT) optimal plug-in bandwidth estimate of Fan and Gijbels (1996). Panel E shows the estimate of the Ln(Dividend) discontinuity for the same period. The X axis represents the size of the bandwidth used in the estimation as a percentage of the optimal ROT bandwidth, with 0 representing the estimate at the ROT choice. The solid line is the point estimate of the discontinuity and the dashed line represent the 90% confidence bands from bootstrapped standard errors. Panel F shows estimates of the Ln(Dividend) discontinuity for simulations of 1000 random placebo thresholds. The vertical dashed line represents the estimate at the true threshold.

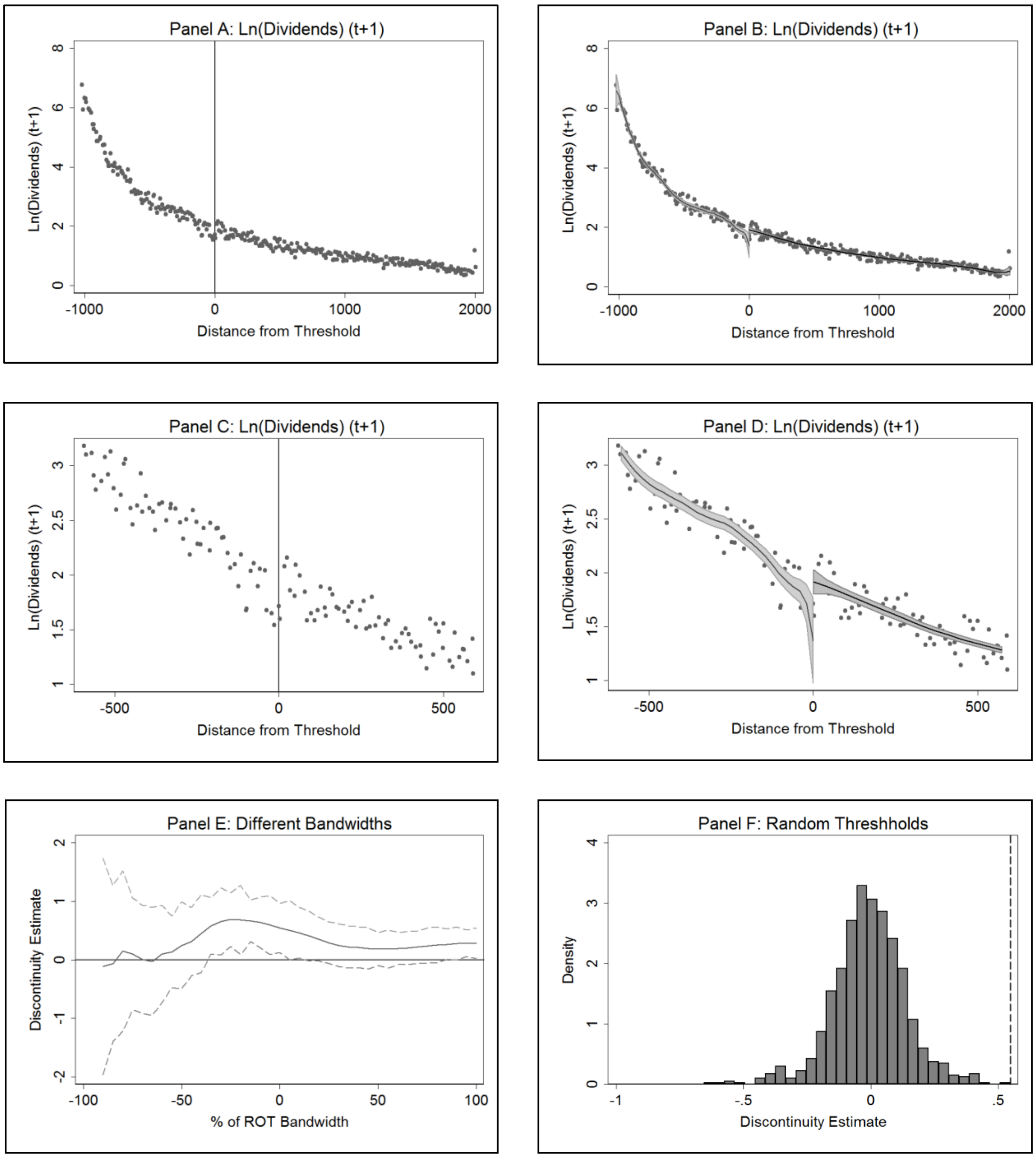


Figure 4 – Total Payout Discontinuity

Panel A, B, C, and D show the Ln(Total Payout) for the first fiscal year ending at least 12 months 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, with 0 representing the last firm in the Russell 1000. Panel A plots the average Ln(Total Payout) over 10 ranks across all years. Panel B adds local polynomial regression estimates and the associated 90% confidence bands using the Rule of Thumb (ROT) optimal plug-in bandwidth estimate of Fan and Gijbels (1996). Panel E shows the estimate of the Ln(Total Payout) discontinuity for the same period. The X axis represents the size of the bandwidth used in the estimation as a percentage of the optimal ROT bandwidth, with 0 representing the estimate at the ROT choice. The solid line is the point estimate of the discontinuity and the dashed line represent the 90% confidence bands from bootstrapped standard errors. Panel F shows estimates of the Ln(Total Payout) discontinuity for simulations of 1000 random placebo thresholds. The vertical dashed line represents the estimate at the true threshold.

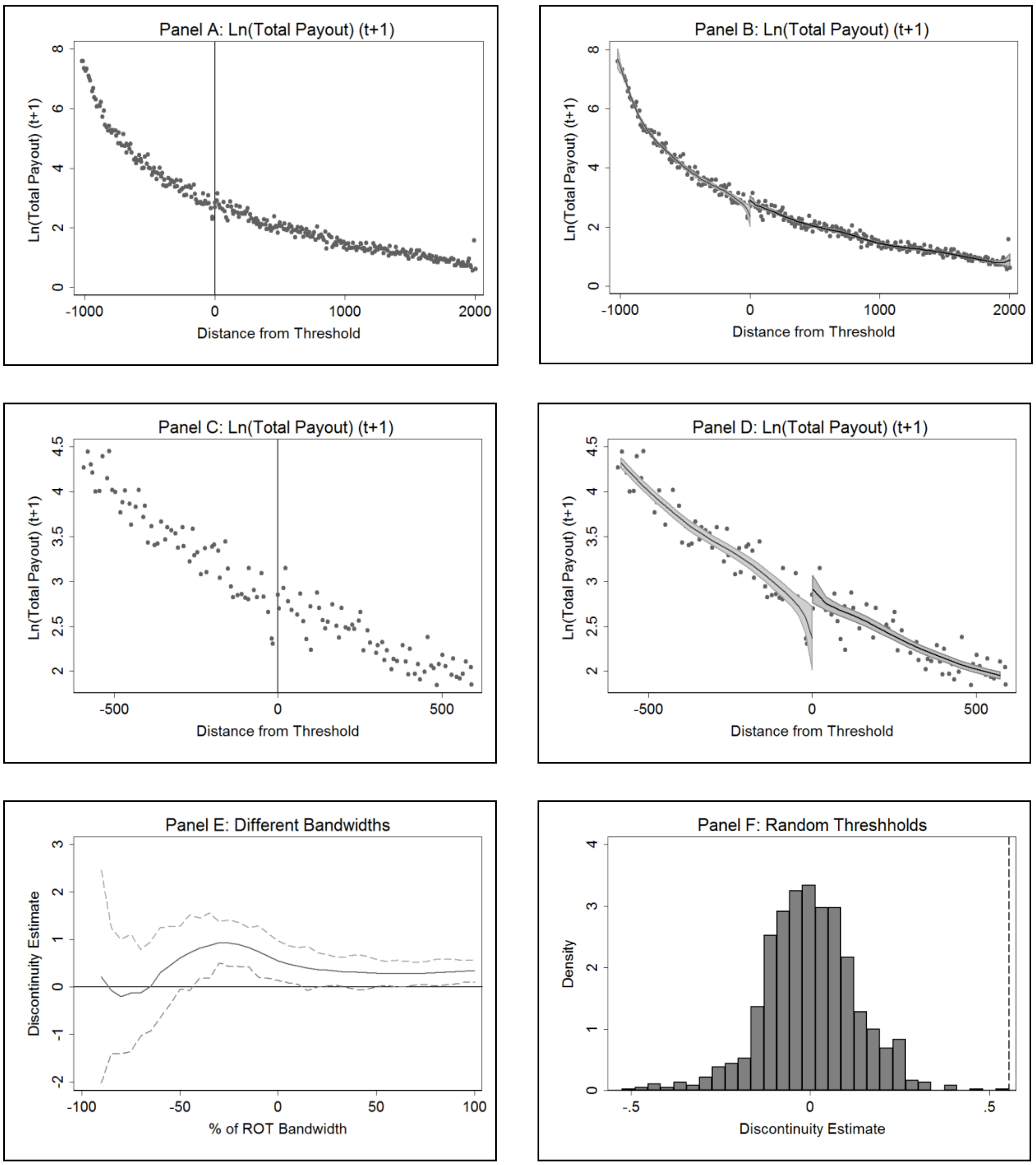


Figure 5 – Cash Holdings Discontinuity

Panel A, B, C, and D show the Ln (Cash Holdings) for the first fiscal year ending at least 12 months 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, with 0 representing the last firm in the Russell 1000. Panel A plots the average in Ln (Cash Holdings) over 10 ranks across all years. Panel B adds local polynomial regression estimates and the associated 90% confidence bands using the Rule of Thumb (ROT) optimal plug-in bandwidth estimate of Fan and Gijbels (1996). Panel E shows the estimate of the Ln (Cash Holdings) discontinuity for the same period. The X axis represents the size of the bandwidth used in the estimation as a percentage of the optimal ROT bandwidth, with 0 representing the estimate at the ROT choice. The solid line is the point estimate of the discontinuity and the dashed line represent the 90% confidence bands from bootstrapped standard errors. Panel F shows estimates of the Ln (Cash Holdings) discontinuity for simulations of 1000 random placebo thresholds. The vertical dashed line represents the estimate at the true threshold.

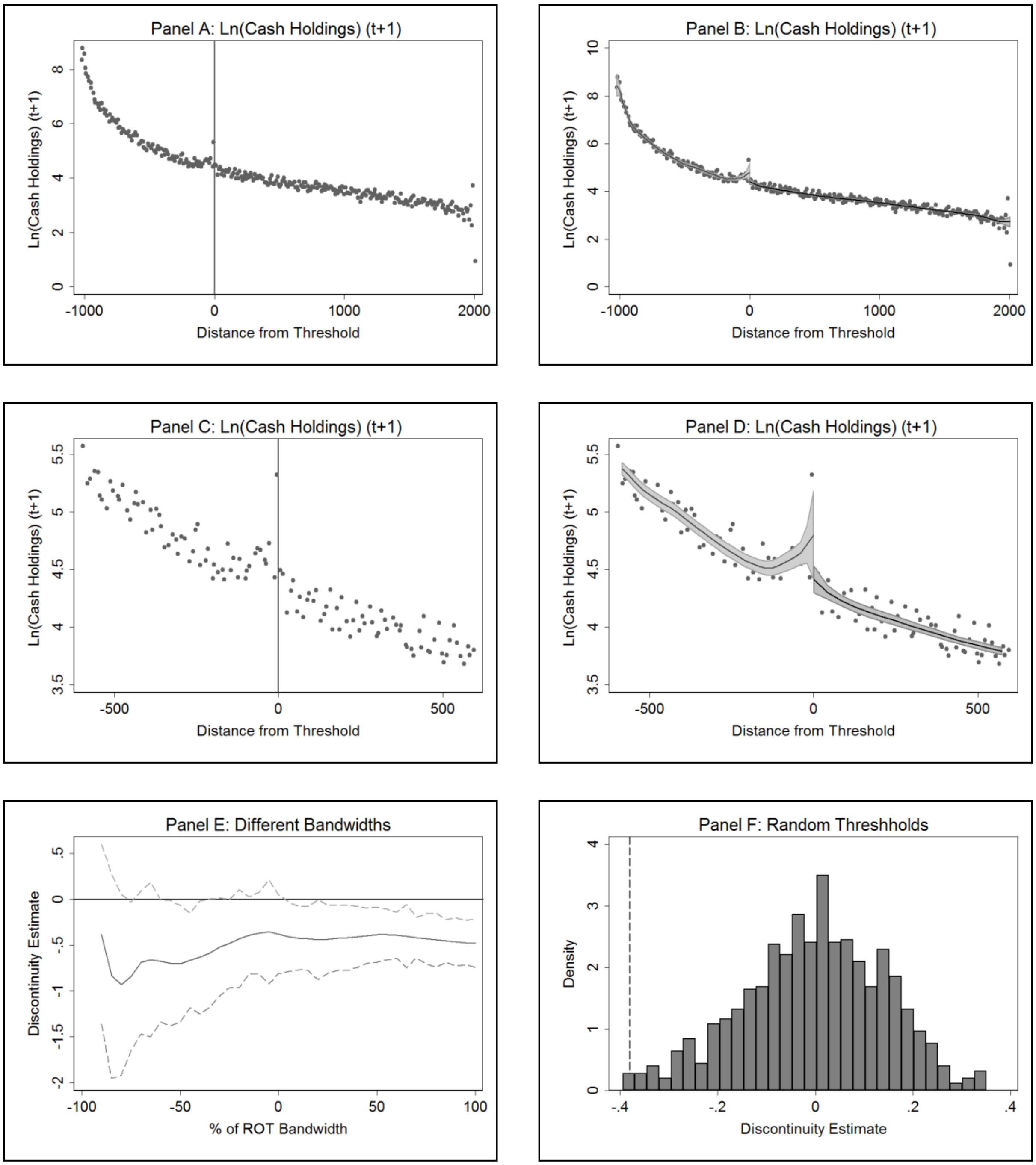


Figure 6 – Proxy-voting Participation

Panel A, B, C, and D show the proxy-voting participation for the first fiscal year ending at least 12 months 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, with 0 representing the last firm in the Russell 1000. Panel A plots the average change in Ln (Voting Percentage/(1-Voting Percentage)) over 10 ranks across all years. Panel B adds local polynomial regression estimates and the associated 90% confidence bands using the Rule of Thumb (ROT) optimal plug-in bandwidth estimate of Fan and Gijbels (1996). Panel E shows the estimate of the voting participation discontinuity for the same period. The X axis represents the size of the bandwidth used in the estimation as a percentage of the optimal ROT bandwidth, with 0 representing the estimate at the ROT choice. The solid line is the point estimate of the discontinuity and the dashed line represent the 90% confidence bands from bootstrapped standard errors. Panel F shows estimates of the voting participation discontinuity for simulations of 1000 random placebo thresholds. The vertical dashed line represents the estimate at the true threshold.

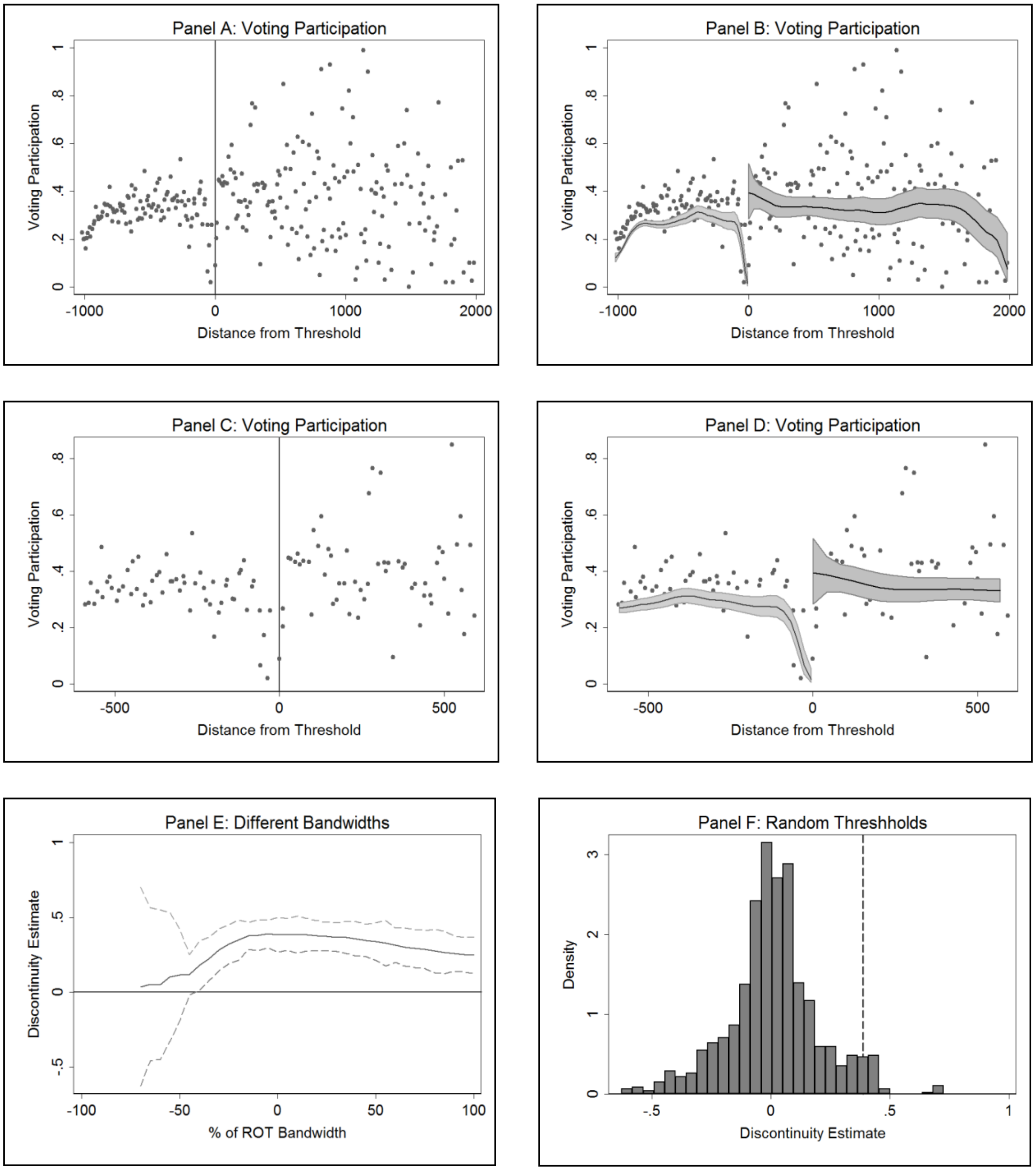


Table I – Summary Statistics

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 the Appendix 2.

Panel A: Russell 1000	Mean	StdDev	p25	Median	p75
Institutional Ownership	0.65	0.22	0.50	0.67	0.81
ROA	0.16	0.12	0.08	0.14	0.21
Dividend Yield	0.02	0.02	0.00	0.01	0.03
Book Leverage	0.25	0.19	0.10	0.24	0.37
Payout/Assets	0.05	0.06	0.01	0.02	0.06
Repurchases/Assets	0.03	0.05	0.00	0.00	0.03
Cash/Assets	0.14	0.21	0.02	0.06	0.17
Total Assets (B$ 2005)	1.06	1.61	0.15	0.39	1.12
Market Value (B$ 2005)	7.29	11.40	1.55	3.00	6.99
Cash Holdings (M$ 2005)	798	1,409	58	218	753
Payout (M$ 2005)	284	475	17	89	285
Repurchases (M$ 2005)	148	295	0.00	9.64	129
Dividends (M$ 2005)	119	201	0.00	34.59	127

Panel B: Russell 2000	Mean	StdDev	p25	Median	p75
Institutional Ownership	0.52	0.28	0.28	0.50	0.73
ROA	0.10	0.19	0.03	0.11	0.19
Dividend Yield	0.01	0.02	0.00	0.00	0.02
Book Leverage	0.22	0.22	0.02	0.17	0.35
Payout/Assets	0.03	0.05	0.00	0.01	0.03
Repurchases/Assets	0.01	0.04	0.00	0.00	0.01
Cash/Assets	0.22	0.30	0.03	0.09	0.29
Total Assets (B$ 2005)	0.97	1.94	0.16	0.39	1.02
Market Value (B$ 2005)	0.46	0.41	0.177	0.320	0.607
Cash Holdings (M$ 2005)	83	182	10.17	34.4	92
Payout (M$ 2005)	15.20	41	0.00	2.19	14
Repurchases (M$ 2005)	7.83	28	0.00	0.00	2.65
Dividends (M$ 2005)	7.42	24	0.00	0.00	6.21

Table II - Pre-treatment Sample Differences at the Russell 1000/2000 Threshold

This table presents the local polynomial values 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 a third degree polynomial estimate to the left and to the right within the given bandwidth. We report estimates of τ and the bootstrapped z-stats. Variables are defined in detailed in Appendix 2. Superscript a, b, and c indicate a significance level of less than 10%, 5%, and 1% respectively.

Variable	Just Included (Left)	Just Excluded (Right)	Treatment (τ)	z-stat	ROT Bandwidth	
Institutional Ownership$_{t+1}$	0.66	0.65	-0.01	-0.1	73	184
Ln(Market Value)$_{t-1}$	14.17	14.04	-0.03	-1.05	78	188
Annual Return$_{t-1}$	0.22	0.20	-0.02	-0.19	121	177
ROA$_{t-1}$	0.12	0.14	0.02	1.09	92	254
Market to Book$_{t-1}$	1.58	1.77	0.19	0.79	90	242
Market Leverage$_{t-1}$	0.32	0.28	-0.05	-1.24	94	146
Ln(Dividends)$_{t-1}$	1.90	1.87	-0.03	-0.1	72	288
Ln(Total Payout)$_{t-1}$	2.67	2.64	-0.11	-0.11	87	176
Ln(Repurchases)$_{t-1}$	1.30	1.35	0.05	0.18	91	189
Ln(Net Equity Issuance)$_{t-1}$	2.19	2.19	0.02	0.02	101	222
Ln(Net Financing)$_{t-1}$	-3.64	-2.77	0.04	1.32	85	192
Ln(Cash Holdings)$_{t-1}$	4.37	4.13	-0.24	-1.36	85	215

Table III– Discontinuity Statistics for Payout Policy

This table presents the regression discontinuity test results where τ is estimated by fitting a third degree polynomial estimate to the left and to the right of the Russell 1000/2000 threshold. We report estimates of τ and the z-stats in parentheses. Variables are defined in detailed in Appendix 2. Superscript a, b, and c indicate a significance level of less than 10%, 5%, and 1% respectively.

Dependent Variable	Treatment (τ)	ROT Bandwidth	
Institutional Ownership$_{t+1}$	9.18% (2.15)[b]	70	190
Ln(Dividends)$_{t+1}$	0.73M (2.25)[b]	86	245
Ln(Total Payout)$_{t+1}$	0.75M (2.31)[b]	110	181
Ln(Repurchases)$_{t+1}$	0.93M (2.22)[b]	97	213

Table IV– Cross-Sectional Results:
The Effects of Ex-Ante Agency Costs

This table presents the regression discontinuity test results where τ is estimated by fitting a third degree polynomial estimate to the left and to the right of the Russell 1000/2000 threshold. We report estimates of τ and the t-stats in parentheses separately for High ROA firms (firms with ROA above the median), low ROA firms (firms with ROA below the median), high growth option firms (market to book above median) and low growth option firms (market to book below median), high and low free cash flow (high cash flow and low growth opportunities vs. low cash flow and high growth opportunities), high and low total compensation (total direct compensation per Execucomp), and high and low analyst coverage (number of analysts above and below median.) Variables are defined in detailed in Appendix 2. Superscript a, b, and c indicate a significance level of less than 10%, 5%, and 1% respectively.

Treatment effects for Total Payout

	ROA	**Market-to-book**	**Cash/ Investment**	**Total Comp.**	**Analyst Coverage**
High group treatment effect	-0.44 (1.29)	0.001 (0.00)	1.16 (2.38)[a]	1.51 (2.14)[b]	0.3 (0.62)
Low group treatment effect	0.55 (1.53)	0.53 (1.87)[c]	0.45 (1.13)	-1.05 (-1.63)	-0.51 (-1.41)
Difference (High-Low)	-0.99 (-1.66)[c]	-0.53 (-1.14)	0.72 -1.48	2.56 (2.15)[b]	0.81 (1.39)

Table V– Discontinuity Statistics for Other Corporate Policies

This table presents the regression discontinuity test results where τ is estimated by fitting a third degree polynomial estimate to the left and to the right of the Russell 1000/2000 threshold. We report estimates of τ and the z-stats in parentheses. Variables are defined in detailed in Appendix 2. Superscript a, b, and c indicate a significance level of less than 10%, 5%, and 1% respectively.

Dependent Variable	**Treatment (τ)**	**ROT Bandwidth**	
Ln(Cash Holdings)$_{t+1}$	-0.32M (-1.68)[c]	78	210
Ln(Net Equity Issuance)$_{t+1}$	1.08M (2.52)[a]	122	185
Asset Growth	0.34 (4.91)[a]	97	176
Ln(R&D Expense)$_{t+1}$	0.48M (1.78)[c]	91	164