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Selection Bias or Treatment Effect? A Re-Examination of Russell 1000/2000 Index Reconstitution

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ABSTRACT

A recent literature uses the annual reconstitution of the Russell 1000 and 2000 Indexes as a source of seemingly exogenous variation in institutional ownership to study the effect of institutional ownership on firm outcomes. We show that lagged institutional ownership measured prior to reconstitution exhibits very similar pre-existing differences at the 1000/2000 cutoff, and thus the results from the most common implementation of this setting (e.g. as in Bird and Karolyi (2019)) reflect selection bias instead of a treatment effect. Additional tests confirm that it is the use of rankings based on Russell's June index weights that leads to biased results. With an unbiased approach, there is no significant discontinuity in institutional ownership at the 1000/2000 cutoff despite the large difference in index weights.

Keywords: institutional ownership, regression discontinuity, Russell indexes, selection bias

JEL Codes: C36, G23, G30

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1 Introduction

Institutional investors own nearly two-thirds of U.S. public equities (Aguilar, 2013). Thus, it is important to understand whether institutional investors affect firm policies. However, because institutional ownership is not randomly assigned to firms, it can be difficult to interpret an association between institutional holdings and any given firm policy without a credible identification strategy.[1] A recent literature seeks to address this problem by using the annual reconstitution of the Russell 1000 and 2000 Indexes as a source of seemingly exogenous variation in total or quasi-indexer institutional ownership to study the effects of institutional investors on various firm policies, including for example management disclosure (Boone and White, 2015; Lin *et al.*, 2018), payout policy (Crane *et al.*, 2016), and tax avoidance (Khan *et al.*, 2017; Chen *et al.*, 2019; Bird and Karolyi, 2019).

The appeal of the Russell 1000/2000 Index reconstitution setting is that index assignment is rules-based. Prior to 2007, Russell determined 1000 and 2000 Index membership based on a descending order ranking of market capitalization at the end of May: firms ranked 1 to 1000 (1001 to 3000) were assigned to the Russell 1000 (2000). That is, there was an arbitrary threshold at the 1000$^{\text{th}}$ ranking such that firms close to, but on opposite sides of, the threshold should have been *ex-ante* similar at the end of May.

At the end of June, the two indexes are separately value weighted such that firms at the bottom of the Russell 1000 have the smallest weights in the Russell 1000, while firms at the top of the Russell 2000 have the largest weights in the Russell 2000. This discontinuity in index weights at the threshold apparently drives another discontinuity in institutional ownership at the threshold because institutions that track the Russell 2000 (1000) hold big (negligible) positions at the top (bottom) of the (respective) index.

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[1] For example, Ajinkya *et al.* (2005) summarize the difficulty in interpretation with disclosure: "Although ownership decisions could be influenced by a firm's disclosure policy, it is also reasonable that a firm's disclosure policy is influenced by its institutional ownership."

Thus, the variation in institutional ownership at the threshold is seemingly exogenous: firms that were similar at the end of May subsequently have different levels of institutional holdings for reasons unrelated to firm policies.

Our paper shows that the results from the most common implementation of the Russell 1000/2000 reconstitution setting reflect selection bias instead of a treatment effect. When measuring total and quasi-indexer institutional ownership before current year reconstitution, we find *pre-existing* discontinuities at the 1000/2000 cutoff that are almost identical to the discontinuities with institutional ownership measured after reconstitution. That is, firms close to, but on opposite sides of, the cutoff already had different levels of institutional holdings even before reconstitution takes place. As a result, the outcomes of the “control” firms represent a poor counterfactual for the outcomes of the “treatment” firms. Thus, papers that used this implementation of the reconstitution setting found descriptive differences between firms at the top (bottom) of the Russell 1000 (2000), but these differences have no causal interpretation.

We confirm in additional tests that it is the use of rankings based on Russell’s June index weights to determine distance to the Russell 1000/2000 cutoff that leads to biased results. Although papers that used these rankings claimed to be implementing regression discontinuity (RD) designs, or claimed an RD motivation, the Russell June rankings are not suitable for RD analysis because they are not the assignment variable that determines Russell 1000/2000 Index membership. Instead, Russell uses rankings measured at the end of May to determine Russell 1000/2000 Index assignment.

Then why not simply use Russell’s end-of-May rankings as the assignment variable? Unfortunately, Russell does not provide this variable to researchers. Nevertheless, if there is a threshold rule determining treatment (e.g. index assignment), but the true assignment variable is unobservable, the textbook solution is fuzzy RD. In this case, that amounts to constructing an end-of-May market capitalization ranking, using the constructed ranking to predict index membership, and then using predicted index membership as an instrumental variable for actual index membership.

With this fuzzy RD design, there is no significant discontinuity in total or quasi-indexer institutional ownership at the 1000/2000 cutoff, despite the large difference in index weights at the cutoff. The existing consensus in the literature is that the fuzzy RD design in the Russell 1000/2000 reconstitution setting suffers from a weak instrument problem, such that the estimation and inferences are not reliable. For example, although Crane *et al.* (2016) and Appel *et al.* (2016b) have each criticized the other's approach, both agree that the fuzzy RD first stage is weak. However, neither paper reports tables showing the first-stage estimation and the relevant test statistics, which is the standard approach to assessing instrument strength (Roberts and Whited, 2013, p. 516).

We estimate the fuzzy RD first-stage regression using a variety of bandwidths and polynomial orders following customary practices (Roberts and Whited, 2013, p. 546). We find that the first stage is very strong with the same bandwidths and polynomial orders used in Crane *et al.* (2016) and Appel *et al.* (2016a). Thus, the existing consensus in the literature is incorrect: the fuzzy RD first stage is not the reason why the fuzzy RD second stage produces a null result with total and quasi-indexer institutional ownership. It remains an open question why there is no second-stage discontinuity with these variables.

Our paper makes several contributions to the literature. First, we confirm that despite their appeal, the Russell June rankings should not be used to implement the Russell 1000/2000 reconstitution setting because they induce severe selection bias: the causal relationship between the outcome and treatment is not directly observable from the data since treatment and control groups are not comparable (Blundell and Costa Dias, 2009, p. 572).

We emphasize that our first contribution is to "confirm" because we are not the first authors to caution against using the Russell June rankings to implement the Russell 1000/2000 reconstitution setting. Warnings were clearly stated in Chang and Hong (2012, p. 10) as well as in the revised Chang *et al.* (2015, p. 222), the first published paper to introduce this setting in an RD context. Subsequent papers with different methodologies such as Appel *et al.* (2016a, p. 121), Gloßner (2018), and Coles *et al.* (2018) have also reinforced these warnings. However, given that numerous papers have continued to use the Russell June rankings, it is clear that the advice of these papers was ignored or unheeded. It is our hope that confirmation may help to redirect this literature moving forward.

Second, contrary to the existing consensus in the literature, we find that the fuzzy RD design is an appropriate implementation of the Russell 1000/2000 reconstitution setting. Researcher-constructed end-of-May rankings strongly predict actual Russell 1000 and 2000 Index membership, and covariates are also balanced at the threshold. Econometrically, we prove that the fuzzy RD design in the Russell 1000/2000 reconstitution setting can consistently estimate treatment effects at the threshold. Conceptually, it is unclear why the underlying assumptions of independence, exclusion, and monotonicity would be violated.

Third, some papers have claimed to find a discontinuity in total or quasi-indexer institutional ownership at the Russell 1000/2000 cutoff using researcher-constructed end-of-May rankings instead of the Russell June rankings (e.g. Chen *et al.* (2017) and Bird and Karolyi (2019)). With researcher-constructed rankings, we are unable to reproduce their findings. Instead, we find that results similar to theirs are obtained using the Russell June rankings. Our code and data is published to facilitate future discussion and reconciliation.

The remainder of the paper is organized as follows. Section 2 describes the empirical framework and data. Section 3 re-examines the most common current approach. Section 4 re-examines the fuzzy RD design. Section 5 addresses concerns about the fuzzy RD design that have been raised in the literature. Section 6 reconciles our findings with those of the existing literature. Section 7 concludes.

2 Empirical Framework

2.1 Data

Data on the Russell 1000 and 2000 Index constituents come from FTSE Russell. We use Thomson-Reuters Institutional Managers (13f) Holdings to compute total institutional ownership. We also use the institutional investor classification data from Brian Bushee to compute the quasi-indexer, dedicated, and transient subsets of total institutional ownership.[2] The sample period covers the annual reconstitutions from 1996 to 2006. Consistent with much of the literature, we end the sample period in 2006 because after that year, Russell began to use a "banding" rule intended to reduce turnover between the indexes.[3]

What we term "researcher-constructed rankings" are sorted based on market capitalization calculated from CRSP and Compustat at the end of May.[4] All firms in the Russell 1000 and 2000 are ranked each year based on publicly available prices and shares as of the last trading day in May. Prices are from CRSP. Shares are from Compustat Quarterly as of the quarterly earnings report date (variable RDQ). The firm with the largest (smallest) market capitalization on the last trading day in May is ranked 1 (3000).

What we term "Russell June rankings" are imputed from Russell's index weights published on the last Friday in June of each year. Within each index, we sort weights in descending order. The firm with the largest (smallest) weight in the Russell 1000 in year t is ranked 1 (1000). The firm with the largest (smallest) weight in the Russell 2000 in year t is ranked 1001 (3000) (Boone and White, 2015, p. 514).

2.2 Russell June Rankings

The most common implementation of the Russell 1000/2000 reconstitution setting uses the Russell June rankings.[5] In this approach, actual Russell 2000 Index membership is treated as an instrumental variable for institutional ownership such that the following can be used as a first-stage

[2] The classification data are available at http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html

[3] In the internet appendix, we explain the banding rule as well as how the indexes are constructed.

[4] Chang *et al.* (2015, p. 218) give the complete details on the construction of the May rankings.

[5] See for example Boone and White (2015, p. 514), Crane *et al.* (2016, p. 1389), Khan *et al.* (2017, p. 120), Lin *et al.* (2018, p. 287), Baghdadi *et al.* (2018, p. 24), Chen *et al.* (2019, p. 282), and Bird and Karolyi (2019). See also table 1 of Gloßner (2018).

regression, or as a reduced-form regression:

$$Y_{i,t} = \alpha_0 + \alpha_1 \text{R2000}_{i,t} + \sum_{n=1}^{k} \alpha_{2n} \text{June Rank}_{i,t}^{n} + \\ + \sum_{n=1}^{k} \alpha_{3n} \text{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n} + \nu_{i,t} \tag{1}$$

where

- $Y_{i,t}$ is a firm outcome such as institutional ownership.
- $\text{R2000}_{i,t}$ is an indicator variable for actual Russell 2000 Index membership.
- $\text{June Rank}_{i,t}$ is the Russell June ranking.
- k is the polynomial order (e.g. $k = 1$ is linear).

Eq. (1) combines two separate regressions, one on each side of the threshold. The interaction terms, $\text{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n}$, allow the slopes of the regression functions to differ on both sides. The coefficient on $\text{R2000}_{i,t}$ is intended to represent the treatment effect, and can visually be thought of as the difference between the two regressions' intercepts at the threshold (Roberts and Whited, 2013, p. 542-543).

2.2.1 Discussion

While eq. (1) looks like the equation for a sharp regression discontinuity (RD) design (cf. Lee and Lemieux (2010, p. 318)), the conditions for a valid sharp RD design are not satisfied. To be clear, in a sharp RD design, treatment is wholly determined by whether the value of a predictor, X_i, is on one side of a fixed threshold.[6] X_i is also "known not to have been affected by the treatment" (Imbens and Lemieux, 2008, p. 616).

In the Russell 1000/2000 reconstitution setting, the most common implementation sets X_i as the Russell June rankings. But these rankings fail to satisfy both of the above mentioned properties. First, Russell 2000 Index

[6] X_i is referred to as the assignment, forcing, selection, running, or ratings variable (Roberts and Whited, 2013, p. 533); we use the term "assignment variable" in this paper.

membership (treatment indicator) is determined by whether Russell's *ranking of market capitalization in May* crosses the threshold, **not** whether the *ranking of index weights in June* crosses the fixed threshold. That is, the Russell June rankings are not the variable that determines index membership (X_i).

More importantly, second, using the Russell June rankings **inverts** the principle behind regression discontinuity: treatment is supposed to be a function *of the assignment variable*, but in this case, the so-called "assignment variable" is a function *of treatment*. To see this, recall that the Russell June rankings are constructed in a process. For each year,

- Rank the Russell 1000 firms by their June index weights (1 has the highest weight).
- Separately rank the Russell 2000 firms by their June index weights.
- Add 1000 to each Russell 2000 June index weight ranking (so that the firm in the Russell 2000 with the highest weight has rank 1001).
- Combine the two rankings together.

Thus, the Russell June rankings cannot be the variable that determines index assignment because their calculation requires knowledge of *ex-post* index assignment! As a result, eq. (1) is not an implementation of a sharp RD design, and the attractive textbook properties of sharp RD do not necessarily apply.

2.3 Balance test

The question then becomes, does it matter in practice that the Russell June rankings are not the variable that determines index assignment? To answer this, we conduct a covariate balance test based on the following intuition: if firms at the bottom of the Russell 1000 by the June rankings truly could have been assigned to the top of the Russell 2000, then they should have been *ex-ante* similar to firms that will be at the top of the Russell 2000 (cf. Atanasov and Black (2016, p. 282)). More formally, covariate balance is a direct implication of the local continuity assumption that underlies all RD designs (Roberts and Whited, 2013, p. 547).

Some covariates may be imbalanced by chance (Lee and Lemieux, 2010, p. 330). Hence, imbalance is relevant only if the observed characteristic is related to the outcome of interest (Roberts and Whited, 2013, p. 548), which in this case is institutional ownership. For this reason, we focus on two key covariates, both measured *prior* to reconstitution in June of each year: market capitalization at the end of May; and lagged institutional ownership in March of the current year. (In our internet appendix, we extend the institutional ownership lags to December and September of the previous year.)

Market capitalization in May is relevant because index membership is based on a ranking of market capitalization at the end of May. Hence, while by construction, the smallest firm in the Russell 1000 is larger than the biggest firm in the Russell 2000 for any given year, the difference in size should not be significant at the cutoff.

Lagged institutional ownership is relevant because the literature argues that the process of Russell 1000/2000 Index reconstitution generates exogenous variation in institutional ownership. If index membership around the cutoff is "as good as randomly assigned" with the Russell June rankings, then we should not see any significant pre-existing discontinuities in lagged institutional ownership at the cutoff.[7]

3 Re-examination of the most common approach

3.1 Replication

To alleviate concerns that differences in coding or variable measurement may drive our results, we first reproduce the discontinuities in September-dated institutional ownership using graphs. Consistent with Boone and White (2015, p. 514), we use a bandwidth of 200 (i.e. number of firms on each side of the threshold), ten non-overlapping equally-spaced bins per side, and a third-order polynomial ($k = 3$) to plot institutional ownership

[7] Our logic is similar to that of Patatoukas and Thomas (2011), who argued that "last year's earnings cannot reflect news that will be subsequently revealed this year" and therefore last year's earnings should not be able to differentially reflect the good and bad components of this year's news.

against the Russell June rankings.

Figure 1 shows that there are a clear discontinuities in September-dated total and quasi-indexer institutional ownership, a smaller but still noticeable discontinuity in transient institutional ownership, and an insignificant discontinuity in dedicated institutional ownership.

While graphs are valuable and aid transparency through visualizing the data, some discretion remains in presentation (Roberts and Whited, 2013, p. 541). Hence, it is important to also show the results more formally. We estimate eq. (1) with OLS:

$$Y_{i,t} = \alpha_0 + \alpha_1 \mathrm{R2000}_{i,t} + \sum_{n=1}^{k} \alpha_{2n} \text{June Rank}_{i,t}^{n} + \sum_{n=1}^{k} \alpha_{3n} \mathrm{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n} + \nu_{i,t}$$

The standard errors are clustered by firm. We continue to use a third-order polynomial ($k = 3$) and a bandwidth of 200 to match the specification from the graphs in figure 1.

Table 1 presents the results. Consistent with the graphs, there are large, statistically significant discontinuities at the threshold in total and quasi-indexer institutional ownership, both at the 1% level; a smaller but still significant discontinuity in transient ownership at the 1% level; and an economically small, statistically insignificant discontinuity in dedicated ownership. The magnitudes of the estimates are very similar to those of the corresponding estimates in table 1, panel B of Boone and White (2015, p. 516). Thus, we are able to reproduce the first-stage results from the literature.

3.2 Covariate balance

We next test for covariate balance by replacing the dependent variables in eq. (1) with market capitalization at the end of May; and March-dated total, quasi-indexer, dedicated, and transient institutional ownership; all else equal. Table 2 presents the results.

Column 1 shows that there is a discontinuity in market capitalization of **\$726 million** at the threshold, significant at the 1% level. That is, on average, the largest firm in the Russell 2000 has a market capitalization of nearly \$750 million *less* than the smallest firm in the Russell 1000. This can also be seen in the upper left panel of figure 2.

Columns 2 through 5 of table 2 show that the discontinuities in institutional ownership documented in table 1 *already existed* in March *before* reconstitution takes place in June.[8] For completeness, we also conduct a joint test that the coefficients on $\text{R2000}_{i,t}$ in table 2 are all equal to 0 by estimating columns 1 through 5 as a system of equations with seemingly unrelated regression (Lee and Lemieux, 2010, p. 330-331). The χ^2 statistic is 191.8 with 5 degrees of freedom, corresponding to $p < 0.00001$; we thus reject the null hypothesis that there are no joint discontinuities in these covariates.

Not only are the pre-existing discontinuities in March-dated institutional ownership statistically nonzero, they are also similar in magnitude to the discontinuities in September-dated ownership. This similarity can also be seen when figure 3, which graphs March-dated institutional ownership, is compared with figure 1. Table 3 confirms that if we estimate eq. (1) with the *change* in institutional ownership from March to September as the dependent variable:

$$\Delta Y_{i,3\to 9,t} = \alpha_0 + \alpha_1 \text{R2000}_{i,t} + \sum_{n=1}^{k} \alpha_{2n} \text{June Rank}_{i,t}^{n} + \sum_{n=1}^{k} \alpha_{3n} \text{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n} + \nu_{i,t}$$

there is no significant discontinuity in the *change* in institutional ownership at the Russell 1000/2000 threshold. Together with the pre-existing discontinuities in the *level* of institutional ownership, the null results in *changes* imply that the process of reconstitution in June does not cause the discontinuity in the *level* of September-dated institutional ownership. (In this section, we merely document the pre-existing differences. We discuss why the differences occur later in section 6.1.)

[8] In our internet appendix, we show that pre-existing discontinuities remain with December and September-dated institutional ownership *of the previous year*.

3.3 Discussion

Appel *et al.* (2018) contest the validity of using covariate balance tests with lagged institutional ownership in the Russell 1000/2000 reconstitution setting. They argue that if index assignment is persistent, and if index assignment affects institutional ownership, then pre-reconstitution institutional ownership is not truly predetermined. Hence, in their view, covariate *imbalance* in institutional ownership does not necessarily invalidate the research design choice of using the Russell June rankings, as such imbalance may be expected.

A review of the applied econometrics literature indicates that such an argument is incorrect. The practice of using lagged dependent variables to assess RD validity is standard, *especially* when the dependent variable is persistent over time. For example, in their widely cited survey of the RD design, Lee and Lemieux (2010, p. 338) state,

> ...we can treat the lagged dependent variable Y_{it-1} as **simply another baseline covariate** in period t. In cases where Y_{it} is highly persistent over time, Y_{it-1} may well be a very good predictor and has a very good chance of reducing the sampling error. As we have also discussed earlier, looking at possible discontinuities in baseline covariates is an important test of the validity of the RD design. In this particular case, since Y_{it} can be highly correlated with Y_{it-1}, finding a discontinuity in Y_{it} **but not in** Y_{it-1} would be a strong piece of evidence supporting the validity of the RD design.

Furthermore, this practice is theoretically justified. Under a fairly mild assumption, it follows that "all predetermined characteristics should have the same distribution just below and above the threshold" in an RD design (Lee, 2008, p. 679). Importantly, Lee (2008) emphasizes that this proposition is robust to **arbitrary correlation** between the assignment variable and any predetermined variable. Hence, covariate balance in a valid RD design is a theorem; and in this setting, the theorem **does not require** zero correlation between this year's Russell 1000/2000 index assignment and last year's assignment.

The intuition behind the theorem applied to this case is that while Russell

1000/2000 Index assignment may be persistent overall, the RD design does not compare Russell 1000 and Russell 2000 firms overall. Instead, the focus is on the firms *around* the cutoff, and it is at the 1000^{th} ranking cutoff that we would expect index membership to resemble a coin toss. The argument that covariate imbalance may be natural and expected implies the opposite, that a firm ranked 999 (1001) is *not likely* to be ranked 1001 (999), which would indeed invalidate the research design choice of using the Russell June rankings.

We reiterate that covariate balance is a direct implication of the local continuity assumption underlying *any* RD design:

> **Roberts and Whited (2013, p. 547)** - Recall the implication of the local continuity assumption. Agents close to but on different sides of the threshold should have similar potential outcomes. Equivalently, these agents should be comparable both in terms of observable and unobservable characteristics.

Stated differently, if the local continuity assumption holds, then we should observe covariate balance. Logically, this statement is equivalent to its contrapositive: covariate *imbalance* is evidence that the local continuity assumption *does not* hold; firms around the cutoff (based on the Russell June rankings) *were not* comparable before reconstitution.

4 Re-examination of Fuzzy RD

4.1 Fuzzy RD Design

We now re-examine the Russell 1000/2000 reconstitution setting from a principles standpoint. Without observing the true assignment variable, it is not possible to implement a sharp RD design (Roberts and Whited, 2013, p. 533). However, as long as Russell 1000/2000 Index assignment is still based on a threshold rule, we can implement a fuzzy RD design.

Fuzzy RD designs apply to nonexperimental settings where treatment is *partially* determined by whether an assignment variable crosses a threshold, possibly because the true assignment variable may be unobservable to the researcher (Roberts and Whited, 2013, p. 536). In these cases, the treatment effect can be recovered using instrumental variables (Roberts and Whited, 2013, p. 544):

- In the first stage, regress the *actual* treatment indicator on *predicted* treatment, controlling for the fuzzy assignment variable (X). To clarify, predicted treatment is an indicator variable for whether X crosses the threshold (c), and it serves as the instrumental variable for actual treatment:

$$\text{Predicted Treatment} = \begin{cases} 1 & \text{if } X > c \\ 0 & \text{if } X \leq c \end{cases}$$

- In the second stage, regress the outcome on instrumented actual treatment, again controlling for the fuzzy assignment variable, X.

In the Russell 2000 setting,

- treatment is actual Russell 2000 Index membership:

$$\text{R2000}_{i,t} = \begin{cases} 1 & \text{if unobservable end-of-May ranking} > 1000 \\ 0 & \text{if unobservable end-of-May ranking} \leq 1000 \end{cases}$$

- the instrument is predicted membership:

$$\tau_{i,t} = \begin{cases} 1 & \text{if researcher-constructed May ranking} > 1000 \\ 0 & \text{if researcher-constructed May ranking} \leq 1000 \end{cases}$$

- the assignment variable for the fuzzy RD is the researcher-constructed May ranking, $\text{Rank}_{i,t}$ (Chang *et al.*, 2015, p. 228).

The treatment effect can then be estimated with 2SLS:[9]

$$\text{R2000}_{i,t} = \alpha_0 + \alpha_1 \tau_{i,t} + \sum_{n=1}^{k} \alpha_{2n} \text{Rank}_{i,t}^n + \sum_{n=1}^{k} \alpha_{3n} \tau_{i,t} \times \text{Rank}_{i,t}^n + \nu_{i,t}$$

$$\begin{aligned} Y_{i,t} &= \beta_0 + \beta_1 \widehat{\text{R2000}}_{i,t} + \sum_{n=1}^{k} \beta_{2n} \text{Rank}_{i,t}^n \\ &\quad + \sum_{n=1}^{k} \beta_{3n} \widehat{\text{R2000}}_{i,t} \times \text{Rank}_{i,t}^n + \varepsilon_{i,t} \end{aligned} \tag{2}$$

[9]Crane *et al.* (2016, p. 1403) state that their use of actual index membership rather than predicted (or forecasted) membership distinguishes their design from the fuzzy RD. However, as shown in eq. (2), the fuzzy RD design **also uses** actual index membership; actual membership is the endogenous regressor to be instrumented in the first stage.

Similar to before, each stage in eq. (2) combines two separate regressions, one for each side of the threshold. The interaction terms once again allow the slopes of the regression functions to differ on both sides.

Different from before, the coefficient on $\widehat{\text{R2000}}_{i,t}$ now represents the local average treatment effect (LATE): the effect of Russell 2000 Index membership on firm outcomes for the subgroup of firms whose Russell 2000 Index membership switches from 0 to 1 if their researcher-constructed May rankings cross the 1000$^{\text{th}}$ rank threshold. The coefficient can be thought of as the discontinuity in the outcome at the threshold *divided by* the discontinuity in the probability of being assigned to the Russell 2000 at the threshold (Roberts and Whited, 2013, p. 538-539).

Note that based on our discussion, actual index membership is an endogenous variable and therefore *cannot* be a valid instrumental variable for institutional ownership. Since researchers do not have the actual rankings used by Russell to determine index assignments, the omitted actual rankings become part of the error term, leading to correlation between actual membership and the error term.

To see this, let Russell Rank$_{i,t}$ denote the true-but-unobservable end-of-May rankings from Russell. $\text{R2000}_{i,t} \equiv f(\text{Russell Rank}_{i,t})$ because

$$\text{R2000}_{i,t} = \begin{cases} 1 & \text{if Russell Rank}_{i,t} > 1000 \\ 0 & \text{if Russell Rank}_{i,t} \leq 1000 \end{cases}$$

Without controlling for Russell Rank$_{i,t}$, we have

$$\begin{aligned} Y_{i,t} &= \beta_0 + \beta_1 \text{R2000}_{i,t} + \xi_{i,t} \\ &= \beta_0 + \beta_1 f\left(\text{Russell Rank}_{i,t}\right) + \underbrace{\left\{\beta_2 \text{Russell Rank}_{i,t} + \varepsilon_{i,t}\right\}}_{\xi_{i,t}} \end{aligned}$$

$\text{R2000}_{i,t}$ is correlated with $\xi_{i,t}$ because $\text{R2000}_{i,t} \equiv f(\text{Russell Rank}_{i,t})$ is a deterministic function of the Russell Rank$_{i,t}$ component of $\xi_{i,t}$. Hence, in practice, actual index membership is an endogenous variable, is not conditionally exogenous, and thus cannot be a valid instrumental variable.[10]

[10]Appel *et al.* (2018) disagree with our explanation of why actual index membership cannot be conditionally exogenous in practice. We respond in our internet appendix.

4.2 Fuzzy RD Results

We estimate eq. (2) with 2SLS. Table 4 presents the results with a third-order polynomial and bandwidth of 200, consistent with the specification used previously in tables 1 and 2. Across all four columns, the coefficient on $\text{R2000}_{i,t}$ is economically small, ranging from -5.5% to -0.2%, and not significantly different from zero at conventional levels. Similarly, figure 4 shows that there do not appear to be any visible discontinuities at the threshold.

Our results are consistent with Chang *et al.* (2015, p. 234) and Schmidt and Fahlenbrach (2017, p. 304), who use slightly different specifications and also report that the fuzzy RD design does not produce a significant discontinuity in institutional ownership at the threshold.[11] This consistent null result contradicts the strong prior belief that the Russell 1000/2000 reconstitution setting should generate a discontinuity in institutional ownership. Consequently, the fuzzy RD design is seldom used to implement the Russell 1000/2000 reconstitution setting.

The consensus in the literature is that the null result is caused by a weak first stage in the fuzzy RD. For example,

> **Crane *et al.* (2016, p. 1402)** - "The standard fuzzy RD implementation has a subtle but serious problem in the Russell setting ... creating a <u>weak instrument</u> problem."

> **Appel *et al.* (2016b)** - "A problem with using the end-of-May CRSP market capitalization as an instrument in a fuzzy RD, however, is that they are a <u>weak predictor</u> of index assignment near the threshold."[12]

However, the process for testing weak instruments has become standardized and involves reporting the first-stage regression (Roberts and Whited, 2013,

[11] Chen *et al.* (2017) claim to use CRSP-constructed May rankings and claim to find a discontinuity in institutional ownership at the threshold. We discuss this in the internet appendix.

[12] In email correspondence with Todd Gormley (dated January 23, 2018), Gormley acknowledged that researcher-constructed May rankings can be used to create a strong instrumental variable for being assigned to the Russell 2000 in a fuzzy RD design. We thank him for his comments and discussion on this issue.

p. 516-517). Since neither Crane *et al.* (2016) nor Appel *et al.* (2016b) reports any first-stage regressions of the fuzzy RD design, we report the first-stage regressions and formally test the null hypothesis of weak instruments.

5 Addressing concerns about fuzzy RD

5.1 First-Stage Regressions

Table 5 presents the first-stage results of estimating the following with OLS:

$$\text{R2000}_{i,t} = \alpha_0 + \alpha_1 \tau_{i,t} + \sum_{n=1}^{k} \alpha_{2n} \text{Rank}_{i,t}^n + \sum_{n=1}^{k} \alpha_{3n} \tau_{i,t} \times \text{Rank}_{i,t}^n + \nu_{i,t}$$

In column 1, we use a bandwidth of 200 and a third-order polynomial as before. We find that there is a 54.9 percentage point discontinuity in the probability of Russell 2000 Index assignment at the cutoff. To clarify, this *does not* mean a coin flip could have decided assignment at the threshold. The intercept estimate of 21.1% refers to the probability of being in the Russell 2000 for a firm predicted to be at the bottom of the Russell 1000, compared with the $21.1 + 54.9 = 76\%$ probability of being in the Russell 2000 for a firm predicted to be at the top of the Russell 2000.

The adjusted R^2 in column 1 is 90.3%. The Kleibergen and Paap (2006) F-statistic testing the hypothesis that $\alpha_1 = \cdots = \alpha_{3k} = 0$ is 17.53. The third-order polynomial specification has 4 endogenous and 4 instrumental variables. While Stock and Yogo (2005, p. 101) do not report critical values for this combination, extrapolation suggests that the critical value would be less than 16.87.

In column 2, we use a bandwidth of 100 and a linear specification (Crane *et al.*, 2016, p. 1387). There is a 71.5 percentage point discontinuity in the probability of Russell 2000 Index assignment at the cutoff. The adjusted R^2 is 82.2%. The Kleibergen and Paap (2006) F-statistic is **299.96**, compared to a critical value of 7.03 for 2 endogenous and 2 instrumental variables in Stock and Yogo (2005, p. 101).

Column 3 uses a bandwidth of 250 and a third-order polynomial (Appel *et al.*, 2016a, p. 121). There is a 60.4 percentage point discontinuity in the probability of Russell 2000 Index assignment at the cutoff. The adjusted R^2 is 91.7%. The Kleibergen and Paap (2006) F-statistic is 29.31. As in column 1, the relevant Stock and Yogo (2005, p. 101) critical value is likely less than 16.87.

A limitation of these critical values is that they are derived assuming homoskedasticity (Stock and Yogo, 2005, p. 106). When standard errors are clustered by firm as in our case, Baum *et al.* (2007, p. 490) suggest using either the Stock and Yogo (2005) critical values with caution, or the traditional Staiger and Stock (1997, p. 557) "rule of thumb" value of 10. In each column of table 5, the Kleibergen and Paap (2006) F-statistic exceeds 10. Lastly, as suggested by Roberts and Whited (2013, p. 541), we present graphs of the first-stage discontinuity in figure 5. It is clear that there is a "jump" in the probability of being assigned to the Russell 2000 Index at the threshold with the researcher-constructed rankings.

Thus, the null result in the fuzzy RD design with institutional ownership is not caused by a weak instrument problem. To the contrary, the first-stage regression is very strong: there are large discontinuities in the probability of being assigned to the Russell 2000 at the threshold; and in the linear specification, the Kleibergen and Paap (2006) F-statistic vastly exceeds the corresponding critical value in Stock and Yogo (2005, p. 101). As another point of reference, Roberts and Whited (2013, p. 517) remark that an R^2 over 40% indicates a strong instrument. The smallest adjusted R^2 we report in table 5 is 71.2%.

5.2 Discussion

Since fuzzy RD is a special case of instrumental variables (Angrist and Pischke, 2009, p. 259), it inherits all of the underlying assumptions:

- The instrument is as good as randomly assigned (i.e. independent of potential outcomes and potential treatment assignments).
- The instrument affects the outcome only through its effect on the endogenous regressor (the exclusion restriction).
- The first stage is nonzero.

- The instrument may have no effect on some firms, but all firms who are affected are affected in the same way (monotonicity/no defiers) (Angrist and Pischke, 2009, p. 155).

Only the nonzero first stage assumption can be tested in the data, and we have shown that the first stage is very strong. We note that Chang *et al.* (2015, p. 229-230) also reported the first-stage regressions for the Russell 1000/2000 fuzzy RD design and found evidence that the instruments were extremely strong. However, their design was criticized by Crane *et al.* (2016, p. 1403), who argued that Chang *et al.* (2015)'s choice of splitting the sample based on last year's index assignment was problematic. We emphasize that we did not split the sample based on last year's index assignment in our results.

In addition, whereas Chang *et al.* (2015, p. 215) used a sample period of 1996 to 2012, which includes the "banding" years after 2006, we ended our sample period in 2006 to be consistent with Crane *et al.* (2016, p. 1385). Thus, the sample splits and the "banding" years used in Chang *et al.* (2015) do not drive the strength of the fuzzy RD first stage in the Russell 1000/2000 reconstitution setting.

Beyond the first stage assumption, the remaining assumptions must also be satisfied for an instrument to be valid (Roberts and Whited, 2013, p. 519). Of these, it is unclear why an indicator for predicted Russell 2000 Index membership, conditional on the researcher-constructed rankings, would be correlated with potential outcomes or potential treatment assignments; why it would affect institutional ownership other than through its effect on actual Russell 2000 Index membership; and why a researcher-constructed indicator variable would cause any defiant behavior on the part of firms.

Lastly, Crane *et al.* (2016, p. 8-10, internet appendix) present a toy model and claim to prove that the fuzzy RD design in the Russell 1000/2000 reconstitution setting *inconsistently* estimates the treatment effect at the threshold. In our internet appendix, we show they did not complete the final step of the proof. Completing the final step demonstrates that the fuzzy RD design consistently estimates the treatment effect at the threshold in this setting.

5.3 Covariate balance

For completeness, we test covariate balance in the fuzzy RD design by replacing the dependent variables in eq. (2) with the same variables as before: market capitalization at the end of May; and March-dated total, quasi-indexer, dedicated, and transient institutional ownership; all else equal. Table 6 presents the results.

In column 1, the estimated discontinuity in end-of-May market capitalization at the threshold is \$7 million, insignificant at conventional levels. The estimate is also economically insignificant given that the estimated intercept at the threshold on the Russell 1000 side is \$1.439 billion, compared with the intercept on the 2000 side of $\$1.439 - 0.007 = \$1.432$ billion. This can be seen in the top right panel of figure 2, where market capitalization is smoothly and monotonically decreasing as the researcher-constructed rankings cross the threshold.

The estimated discontinuities in March-dated institutional ownership in columns 2 through 5 are likewise economically small and insignificant at conventional levels. Figure 6 visually confirms the small discontinuities at the threshold.

Thus, unlike the Russell June rankings approach, the fuzzy RD design exhibits balance with key covariates. That is, under the fuzzy RD design, we are comparing *ex-ante* similar firms around the Russell 1000/2000 threshold such that by using instrumental variables, we can credibly claim to estimate the effect of Russell 2000 Index membership on outcomes such as institutional ownership. While by definition we cannot test balance in unobservables, we have no reason to expect significant differences on average at the threshold with the researcher-constructed end-of-May rankings.

6 Reconciliation

6.1 Use of Russell's Float Adjustment Mechanically Influences IO

The Russell June index weights are based on a float adjusted market capitalization that removes publicly unavailable shares (FTSE Russell, 2017, p. 25). Consequently, firms at the bottom of the Russell 1000 by index weights are not necessarily the firms with the smallest market capitalizations in the Russell 1000. As we saw in the upper left panel of figure 2, firms at the bottom of the Russell 1000 by index weights may *also* consist of relatively larger firms that have proportionally many publicly unavailable shares.

Tautologically, publicly unavailable shares and institutional ownership are negatively related: the more publicly unavailable shares, the less can be held by any outside investors. Hence, institutional ownership at the bottom of the Russell 1000 by Russell June rankings is low *not only* because the index weights are low, but also because

- the publicly unavailable shares may be high, and
- the institutional ownership measure is calculated as shares held by institutions divided by total shares outstanding, which include the publicly unavailable shares.

Stated differently, using the Russell June rankings confounds the research design because it is not obvious

- whether the discontinuity in institutional ownership at the threshold is driven by the discontinuity in index weights (as claimed in the literature and shown in the bottom left panel of figure 2), or
- whether the discontinuity mechanically reflects Russell's float adjustments for publicly unavailable shares.

These two possibilities can be distinguished by examining whether there is a discontinuity in the float adjustment at the 1000/2000 threshold. We first discuss how we measure the float adjustment. As we have the Russell-provided June index weights but not the underlying float-adjusted market capitalizations, we compute weights within each index based on the firm's June market capitalization from CRSP:

$$\begin{aligned}\text{CRSP weight}_{i,j,t} &= \frac{\text{June market capitalization}_{i,j,t}}{\sum_{i=1}^{n}\text{June market capitalization}_{i,j,t}}\\ &= \frac{(\text{PRC}_{i,j,t}/\text{CFACPR}_{i,j,t})\times(\text{SHROUT}_{i,j,t}\times\text{CFACSHR}_{i,j,t})}{\sum_{i=1}^{n}\text{June market capitalization}_{i,j,t}}\end{aligned}$$

for firm i in index j in year t. CRSP variable names for price, shares outstanding, and cumulative adjustment factors are in parentheses. Since

Chang *et al.* (2015, p. 230) document a 5% addition (deletion) effect for firms added to (deleted from) the Russell 2000, we use CRSP data from June to match the dates and to ensure that the addition or deletion effects do not bias our estimate of the float adjustment.[13]

We then calculate the percentage change from the CRSP weights to the Russell June weights:

$$\text{Float adjustment}_{i,j,t} = \frac{\text{Russell June weight}_{i,j,t} - \text{CRSP weight}_{i,j,t}}{\text{CRSP weight}_{i,j,t}}$$

Thus, the float adjustment for firm i in index j and year t is the percentage change between the two weight measures. More negative (positive) values indicate larger (smaller) float adjustments. (We provide an illustration in the internet appendix as to why more positive values indicate smaller adjustments.)

We re-estimate eq. (1) with the float adjustment as the dependent variable:

$$\begin{aligned}\text{Float adjustment}_{i,j,t} = \alpha_0 + \alpha_1 \text{R2000}_{i,t} + \sum_{n=1}^{k} \alpha_{2n} \text{June Rank}_{i,t}^{n} \\ + \sum_{n=1}^{k} \alpha_{3n} \text{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n} + \nu_{i,t}\end{aligned}$$

Table 7 presents the results. In column 1, we use a bandwidth of 200 and a third-order polynomial as before. There is a 104.7 percentage point discontinuity in the float adjustment at the Russell 1000/2000 threshold, significant at the 1% level. While this estimate may seem very large, recall that the float adjustment is expressed in decimals; the estimated intercept of -0.752 corresponds to a -75.2% float adjustment at the threshold on the Russell 1000 side. The bottom right panel of figure 2 confirms the magnitude of the discontinuity at the threshold.

Columns 2 and 3 vary the polynomial order, while holding the bandwidth constant at 200. Column 4 uses a bandwidth of 100 and a linear specification as in Crane *et al.* (2016). Across all columns, the estimates are

[13] We reiterate that we do not split the sample based on last year's index assignment in our results.

statistically significant at the 1% level and are around 100 percentage points in magnitude. Thus, proportionally, firms at the bottom of the Russell 1000 by the June rankings have significantly more publicly unavailable shares than firms at the top of the Russell 2000.

It would seem that a natural solution to any confound arising from the float adjustment would be to simply include it as a control variable. Intuitively, to control for the float adjustment is to hold it constant and then examine how institutional ownership varies with Russell 2000 Index membership. However, based on the bottom right panel of figure 2, if we were to draw horizontal lines throughout the panel (i.e. hold constant the float adjustment for a given value), the horizontal lines would not intersect *both* curves. That is, for a given value of the float adjustment, we have either Russell 1000 firms, or Russell 2000 firms, *but not both*. Without an overlap, controlling for the float adjustment may not be meaningful.

Table 8 confirms that when we estimate the following with March-dated institutional ownership as the dependent variable:

$$
\begin{aligned}
Y_{i,t} = \alpha_0 + \alpha_1 \text{R2000}_{i,t} + \sum_{n=1}^{k} \alpha_{2n} \text{June Rank}_{i,t}^{n} \\
+ \sum_{n=1}^{k} \alpha_{3n} \text{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n} + \alpha_4 \text{Float Adj.}_{i,t} + \nu_{i,t}
\end{aligned}
$$

we continue to find large pre-existing discontinuities at the threshold. Hence, the confound from the float adjustment cannot be easily addressed by including the float adjustment as a control. Coupled together with the covariate imbalance reported in table 2 and figure 3, our results indicate that the discontinuity in September-dated institutional ownership is partly a mechanical artifact of the discontinuity in publicly unavailable shares at the Russell 1000/2000 threshold.

6.2 Interpretation of Prior Literature's Results

Conceptually, selection bias implies that "the causal relationship between [the outcome] and [treatment] is not directly observable from the data *since participants and nonparticipants are not comparable*" (Blundell and Costa Dias, 2009, p. 572). The motivation behind using Russell 1000/2000 Index reconstitution as a natural experiment is that the rules-based methodology seemingly allows for a credible argument that Russell 1000 and 2000 firms close to the cutoff were "comparable" prior to reconstitution but became different afterward in terms of institutional ownership.

However, this argument is based on the true rankings Russell uses in May, which are unobservable. The argument does not carry over to the Russell June rankings: we have shown that the discontinuity in September-dated institutional ownership at the Russell 1000/2000 cutoff ranked by Russell-provided June index weights already exists in March, prior to Russell Index reconstitution in June; and that this discontinuity does not exist when using researcher-constructed rankings of market capitalization. That is, due to Russell's float adjustment for publicly unavailable shares, firms on either side of the cutoff ranked by Russell's June index weights were already different and thus *not comparable* prior to reconstitution.

Hence, papers that rely on these discontinuities documented differences in outcomes between Russell 1000 and 2000 firms close to the Russell 1000/2000 cutoff, but these differences are only descriptive: they cannot be causally attributed to institutional investors' responding to Russell 1000/2000 Index reconstitution.[14]

7 Concluding Remarks

A recent literature uses the annual reconstitution of the Russell 1000 and 2000 Indexes as a source of seemingly exogenous variation in institutional ownership (e.g. Boone and White (2015), Crane *et al.* (2016), Khan *et al.* (2017), Lin *et al.* (2018), Chen *et al.* (2019), and Bird and Karolyi (2019)). The reasoning is that assignment to the Russell 1000 and 2000 is based on a threshold rule with market capitalization such that index membership is as good as randomly assigned close to the cutoff. However, as each index is separately value weighted, firms at the top of the Russell 2000 should have significantly larger institutional ownership than firms at the bottom of the Russell 1000.

[14] We provide a simplified model to illustrate why the differences are descriptive given selection bias in the internet appendix.

Our evidence indicates that the findings of this literature are driven by a research design choice that results in selection bias. Specifically, the discontinuities in September-dated total and quasi-indexer institutional ownership at the Russell 1000/2000 cutoff occur when firms are ranked on their Russell-provided June index weights. These index weights are based on a *different* measure of market capitalization than what Russell uses to rank firms and determine index membership. In particular, the index weights are based on a float-adjusted market capitalization that removes shares that Russell considers to be publicly unavailable.

Using a placebo test with institutional ownership measured prior to reconstitution, we find significant pre-existing differences in institutional ownership between firms that will be at the bottom of the Russell 1000 and firms that will be at the top of the Russell 2000 in June by index weights. Hence, Russell 1000/2000 Index reconstitution in June cannot cause the discontinuities in September-dated total and quasi-indexer institutional ownership at the cutoff, as these already exist prior to June.[15] Instead, these discontinuities are driven by the float adjustments: tautologically, the more publicly unavailable shares, the less can be held by any outside investors.

The failure of the placebo tests with the Russell June rankings implies that the literature using this approach did not establish any causal relations between institutional ownership and the outcome variables. In the end, we are back to the beginning, before this literature began. It is not clear that higher total or quasi-indexer institutional ownership causes greater management disclosure (Boone and White, 2015; Lin *et al.*, 2018), higher dividend payout (Crane *et al.*, 2016), greater tax avoidance (Khan *et al.*, 2017; Chen *et al.*, 2019; Bird and Karolyi, 2019), and so forth. These relations need to be re-established using an unbiased approach.

To use the Russell 1000/2000 reconstitution setting to properly establish causal relationships, we need to use rankings that are independent of Russell's float adjustments. We verify that the researcher-constructed rankings

[15] Similarly, Snyder and Welch (2017) find that lagging the date of new chair appointments to the United States Senate Finance Committee produces "effects" on Senator home-state firm capital expenditures comparable to those from using the actual chair appointment date.

of end-of-May market capitalization based on CRSP and Compustat data strongly predict actual Russell 1000/2000 Index assignment. Using these researcher-constructed rankings, we show that there are no discontinuities in September-dated total or quasi-indexer institutional ownership at the Russell 1000/2000 cutoff, consistent with Chang *et al.* (2015, p. 234) and Schmidt and Fahlenbrach (2017, p. 304).

The null results suggest that we need to rethink and reconsider the details about Russell 1000/2000 Index reconstitution. Although it is possible that different types of institutional investors trade with each other such that there is no change in total institutional ownership (Chang *et al.*, 2015, p. 234), the lack of discontinuities in quasi-indexer institutional ownership is nevertheless surprising. Perhaps the first question we need to answer is why we do not observe a sizable response in institutional ownership to Russell Index reconstitution, given the large difference in index weights around the Russell 1000/2000 cutoff?

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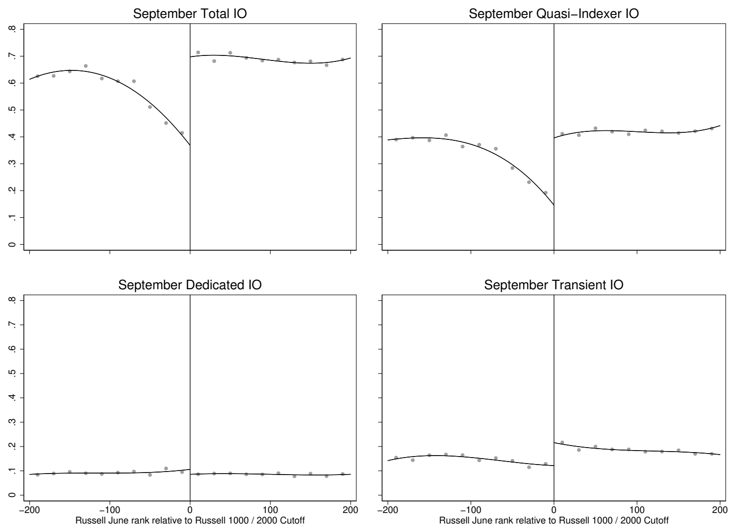

Figure 1: End-of-September total and subtotal institutional ownership, June rankings

Description: The top left (right) plots September-dated total (quasi-indexer) institutional ownership against rankings based on Russell's index weights in June centered at the 1000$^{\text{th}}$ ranking. The bottom left (right) plots September-dated dedicated (transient) institutional ownership against rankings based on Russell's index weights in June centered at the 1000$^{\text{th}}$ ranking. Firms on the left (right) side of the vertical line are in the Russell 1000 (Russell 2000) in June of year t. Each bin represents the average of the y-axis variable over 20 ranks through the sample period. The curves fit the data using local polynomial (cubic) regression. The sample period is 1996–2006.

Interpretation: With the Russell June rankings, firms at the top of the Russell 2000 have significantly higher September-dated total, quasi-indexer, and transient institutional ownership than firms at the bottom of the Russell 1000. The figure is nearly identical to figure 3 in Boone and White (2015, p. 515), is very similar to figure 2 in Khan *et al.* (2017, p. 108), and the top left panel is similar to figure 2 in Crane *et al.* (2016, p. 1389). Hence, we have successfully replicated these papers' results with comparable data.

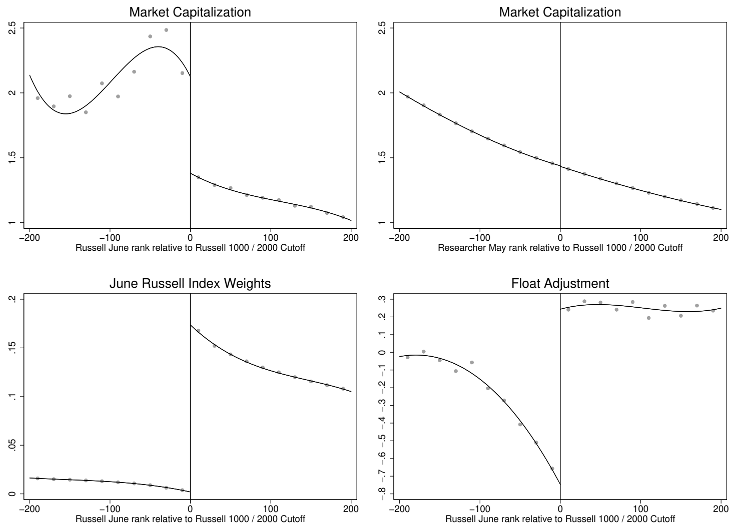

Figure 2: End-of-May market capitalization, June index weights, float adjustment

Description: The top left (right) plots market capitalization from CRSP and Compustat in billions against rankings based on Russell's June index weights (researcher-constructed market capitalization in May) centered at the 1000^{th} ranking. The bottom left (right) plots index weights (the float adjustment) against rankings based on Russell's June index weights centered at the 1000^{th} ranking. The bandwidth is 200. For the top left, bottom left, and bottom right panels, firms on the left (right) side of the vertical line are *actually* in the Russell 1000 (Russell 2000) in June of year t by Russell June rankings; for the top right panel, firms on the left (right) side of the vertical line are *predicted to be* in the Russell 1000 (Russell 2000) in June of year t by the researcher-constructed May rankings. Each bin represents the average of the y-axis variable over 20 ranks through the sample period. The curves fit the data using local polynomial (cubic) regression. The sample period is 1996–2006.

Interpretation: With the Russell June rankings, firms at the top of the Russell 2000 were significantly smaller at the end of May (top left), have significantly larger June index weights (bottom left), and have significantly more publicly available shares (bottom right) compared to firms at the bottom of the Russell 1000. With the researcher-constructed May rankings, end-of-May market capitalization is smooth and continuously decreasing across the threshold (top right). Hence, the Russell June rankings lead to firms that are significantly different in size at the cutoff, while the researcher-constructed May rankings lead to firms that are similar in size at the cutoff.

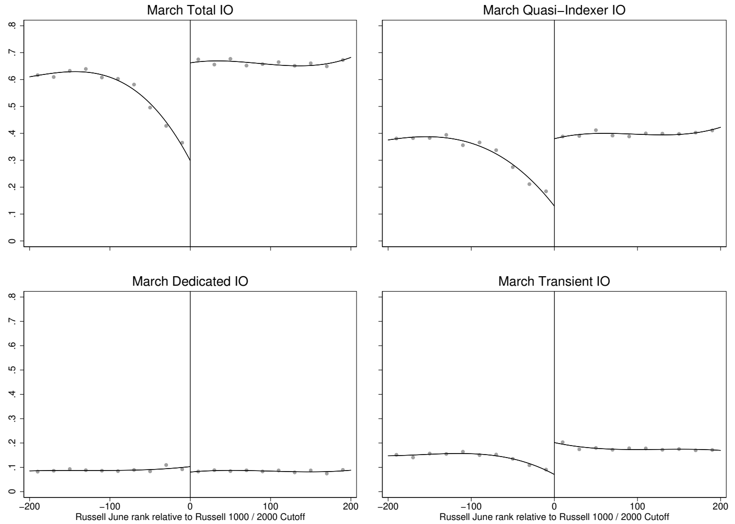

Figure 3: End-of-March total and subtotal institutional ownership, June rankings

Description: The top left (right) plots March-dated total (quasi-indexer) institutional ownership against rankings based on Russell's index weights in June centered at the 1000$^{\text{th}}$ ranking. The bottom left (right) plots March-dated dedicated (transient) institutional ownership against rankings based on Russell's index weights in June centered at the 1000$^{\text{th}}$ ranking. Firms on the left (right) side of the vertical line will be in the Russell 1000 (Russell 2000) in June of year t. Each bin represents the average of the y-axis variable over 20 ranks through the sample period. The curves fit the data using local polynomial (cubic) regression. The sample period is 1996–2006.

Interpretation: With the Russell June rankings, firms that will be at the top of the Russell 2000 already had significantly higher March-dated total, quasi-indexer, and transient institutional ownership compared to firms that will be at the bottom of the Russell 1000 *even before* reconstitution takes place. Hence, the Russell June rankings result in covariate imbalance at the cutoff.

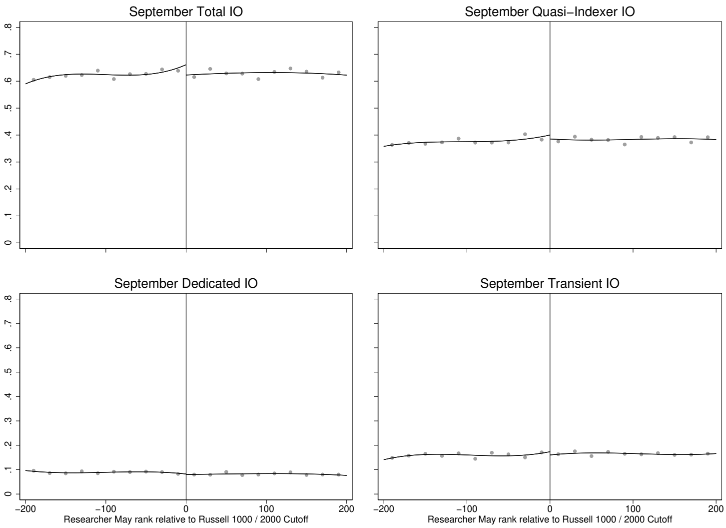

Figure 4: End-of-September total and subtotal institutional ownership, May rankings

Description: The top left (right) plots September-dated total (quasi-indexer) institutional ownership against rankings based on researcher-constructed market capitalization in May centered at the 1000$^{\text{th}}$ ranking. The bottom left (right) plots September-dated dedicated (transient) institutional ownership against rankings based on researcher-constructed market capitalization in May centered at the 1000$^{\text{th}}$ ranking. Firms on the left (right) side of the vertical line are predicted to be in the Russell 1000 (Russell 2000) in June of year t. Each bin represents the average of the y-axis variable over 20 ranks through the sample period. The curves fit the data using local polynomial (cubic) regression. The sample period is 1996–2006.

Interpretation: With the researcher-constructed May rankings, firms predicted to be at the top of the Russell 2000 have similar September-dated total, quasi-indexer, dedicated, and transient institutional ownership compared to firms predicted to be at the bottom of the Russell 1000. Hence, with an unbiased ranking, there are no significant discontinuities in any of these ownership measures after reconstitution.

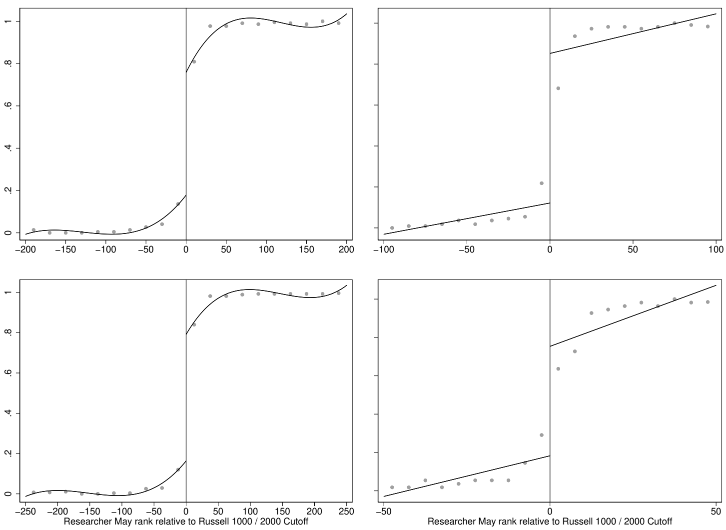

Figure 5: Probability of being in the Russell 2000 Index

Description: Each panel plots an indicator for actual Russell 2000 Index membership against rankings based on researcher-constructed market capitalization in May centered at the 1000th ranking. Firms on the left (right) side of the vertical line are predicted to be in the Russell 1000 (Russell 2000) in June of year t. Clockwise from the top left, each bin represents the average of the y-axis variable over 20, 10, 5, and 25 ranks through the sample period. In the two left (right) panels, the curves fit the data using local cubic (linear) regression. The sample period is 1996–2006.

Interpretation: This figure is a visual supplement to table 5 and shows that with the researcher-constructed rankings, a firm predicted to be at the top of the Russell 2000 is significantly more likely to actually be in the Russell 2000 than a firm predicted to be at the bottom of the Russell 1000. That is, the first stage of the fuzzy RD design is strong.

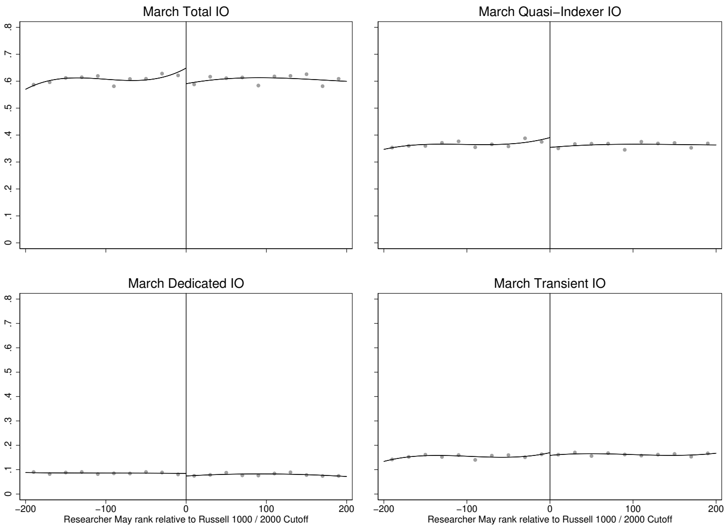

Figure 6: End-of-March total and subtotal institutional ownership, May rankings

Description: The top left (right) plots March-dated total (quasi-indexer) institutional ownership against rankings based on researcher-constructed market capitalization in May centered at the 1000$^{\text{th}}$ ranking. The bottom left (right) plots March-dated dedicated (transient) institutional ownership against rankings based on researcher-constructed market capitalization in May centered at the 1000$^{\text{th}}$ ranking. Firms on the left (right) side of the vertical line are predicted to be in the Russell 1000 (Russell 2000) in June of year t. Each bin represents the average of the y-axis variable over 20 ranks through the sample period. The curves fit the data using local polynomial (cubic) regression. The sample period is 1996–2006.

Interpretation: With the researcher-constructed May rankings, firms predicted to be at the top of the Russell 2000 had similar March-dated total, quasi-indexer, dedicated, and transient institutional ownership compared to firms predicted to be at the bottom of the Russell 1000. Hence, the researcher-constructed May rankings exhibit covariate balance in these ownership measures.

Table 1: Discontinuity in institutional ownership, Russell June rankings

	(1) IO 9	(2) QIX 9	(3) DED 9	(4) TRA 9
R2000	0.365***	0.268***	-0.016	0.105***
	(0.049)	(0.026)	(0.038)	(0.025)
Constant	0.359***	0.144***	0.104***	0.119***
	(0.043)	(0.021)	(0.037)	(0.021)
Third-Order	Yes	Yes	Yes	Yes
Observations	4048	4037	3989	4036

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Description: This table reports the results of estimating the following equation with OLS. The outcome variables are September-dated total, quasi-indexer, dedicated, and transient institutional ownership. R2000 is an indicator variable for membership in the Russell 2000 Index. June Rank is the within-index ranking of the Russell June index weights for stock i in year t centered at the cutoff for Russell 2000 membership. The interaction terms are suppressed for brevity. The bandwidth is 200 (Boone and White, 2015). Standard errors are clustered by firm. The sample period is 1996–2006.

$$Y_{i,q,t} = \beta_0 + \beta_1 \mathrm{R2000}_{i,t} + \sum_{n=1}^{3} \gamma_n \text{June Rank}_{i,t}^{n} + \sum_{n=1}^{3} \delta_n \mathrm{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n} + \varepsilon_{i,t}$$

Interpretation: With the Russell June rankings, firms at the top of the Russell 2000 have higher total, quasi-indexer, and transient institutional ownership at the end of September than firms at the bottom of the Russell 1000. Hence, we have successfully replicated the prior results in the literature.

Table 2: Covariate imbalance, Russell June rankings

	(1) Market Cap.	(2) IO 3	(3) QIX 3	(4) DED 3	(5) TRA 3
R2000	-0.726***	0.399***	0.269***	-0.017	0.141***
	(0.107)	(0.048)	(0.027)	(0.041)	(0.017)
Constant	2.099***	0.290***	0.127***	0.100**	0.069***
	(0.104)	(0.041)	(0.022)	(0.040)	(0.012)
Third-Order	Yes	Yes	Yes	Yes	Yes
Observations	4395	4079	4079	4029	4078

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Description: This table reports the results of estimating the following equation with OLS. The "outcome" variables are end-of-May market capitalization and March-dated total, quasi-indexer, dedicated, and transient institutional ownership. R2000 is an indicator variable for membership in the Russell 2000 Index. June Rank is the within-index ranking of the Russell June index weights for stock i in year t centered at the cutoff for Russell 2000 membership. The interaction terms are suppressed for brevity. The bandwidth is 200 (Boone and White, 2015). Standard errors are clustered by firm. The sample period is 1996–2006.

$$Y_{i,q,t} = \beta_0 + \beta_1 \text{R2000}_{i,t} + \sum_{n=1}^{3} \gamma_n \text{June Rank}_{i,t}^{n} + \sum_{n=1}^{3} \delta_n \text{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n} + \varepsilon_{i,t}$$

Interpretation: With the Russell June rankings, firms that will be at the top of the Russell 2000 were already much smaller at the end of May and already had higher total, quasi-indexer, and transient institutional ownership at the end of March compared to firms that will be at the bottom of the Russell 1000 *even before* reconstitution takes place in June. Hence, the approach with the Russell June Rankings fails the balance tests.

Table 3: No discontinuity in the change in ownership, Russell June rankings

	(1) $\Delta IO_{3\to9}$	(2) $\Delta QIX_{3\to9}$	(3) $\Delta DED_{3\to9}$	(4) $\Delta TRA_{3\to9}$
R2000	-0.028	0.000	0.002	-0.033
	(0.023)	(0.010)	(0.006)	(0.020)
Constant	0.062***	0.017**	0.003	0.044**
	(0.020)	(0.007)	(0.003)	(0.019)
Third-Order	Yes	Yes	Yes	Yes
Observations	4000	3989	3915	3987

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Description: This table reports the results of estimating the following equation with OLS. The outcome variables are the changes in total, quasi-indexer, dedicated, and transient institutional ownership (from March to September). R2000 is an indicator variable for membership in the Russell 2000 Index. June Rank is the within-index ranking of the Russell June index weights for stock i in year t centered at the cutoff for Russell 2000 membership. The interaction terms are suppressed for brevity. The bandwidth is 200 (Boone and White, 2015). Standard errors are clustered by firm. The sample period is 1996–2006.

$$\Delta Y_{i,3\to9,t} = \beta_0 + \beta_1 \text{R2000}_{i,t} + \sum_{n=1}^{3} \gamma_n \text{June Rank}_{i,t}^{n} + \sum_{n=1}^{3} \delta_n \text{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n} + \varepsilon_{i,t}$$

Interpretation: With the Russell June rankings, there is no discontinuity in the change in institutional ownership from March to September at the threshold. Therefore, the pre-existing discontinuities in March-dated institutional ownership are very similar to the post-reconstitution discontinuities in September-dated institutional ownership.

Table 4: No discontinuity in institutional ownership, researcher-constructed rankings

	(1) IO 9	(2) QIX 9	(3) DED 9	(4) TRA 9
R2000	-0.055	-0.017	-0.002	-0.021
	(0.054)	(0.035)	(0.017)	(0.024)
Constant	0.665***	0.400***	0.083***	0.176***
	(0.030)	(0.020)	(0.009)	(0.014)
Third-Order	Yes	Yes	Yes	Yes
Observations	4041	4030	3975	4030

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Description: This table reports the results of estimating the following equation with 2SLS. The outcome variables are September-dated total, quasi-indexer, dedicated, and transient institutional ownership. R2000 is an indicator variable for actual membership in the Russell 2000 Index. τ is an indicator variable for predicted membership in the Russell 2000 Index. Rank is the researcher-constructed ranking of end-of-May market capitalization for stock i in year t centered at the cutoff for Russell 2000 membership. The interaction terms are suppressed for brevity. The bandwidth is 200 (Boone and White, 2015). Standard errors are clustered by firm. The sample period is 1996–2006.

$$\mathrm{R2000}_{i,t} = \alpha_0 + \alpha_1 \tau_{i,t} + \sum_{n=1}^{3} \alpha_{2n} \mathrm{Rank}_{i,t}^{n} + \sum_{n=1}^{3} \alpha_{3n} \tau_{i,t} \times \mathrm{Rank}_{i,t}^{n} + \nu_{i,t}$$

$$Y_{i,t} = \beta_0 + \beta_1 \widehat{\mathrm{R2000}}_{i,t} + \sum_{n=1}^{3} \beta_{2n} \mathrm{Rank}_{i,t}^{n} + \sum_{n=1}^{3} \beta_{3n} \widehat{\mathrm{R2000}}_{i,t} \times \mathrm{Rank}_{i,t}^{n} + \varepsilon_{i,t}$$

Interpretation: With the researcher-constructed rankings, firms at the top of the Russell 2000 have slightly lower September-dated total and subtotal institutional ownership than firms at the bottom of the Russell 1000, but the differences are statistically and economically insignificant.

Table 5: First-stage regression in fuzzy RD

	(1) R2000	(2) R2000	(3) R2000	(4) R2000
τ	0.549***	0.715***	0.604***	0.545***
	(0.048)	(0.029)	(0.041)	(0.048)
Constant	0.211***	0.142***	0.192***	0.212***
	(0.032)	(0.020)	(0.028)	(0.033)
Polynomial Order	Third	First	Third	First
Bandwidth	200	100	250	50
Observations	4041	2026	5053	1023
Adjusted R^2	0.903	0.822	0.917	0.712

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Description: This table reports the results of estimating the following equation with OLS. The outcome variable is an indicator variable for actual membership in the Russell 2000 Index. τ is an indicator variable for predicted membership in the Russell 2000 Index. Rank is the researcher-constructed ranking of end-of-May market capitalization for stock i in year t centered at the cutoff for Russell 2000 membership. The interaction terms are suppressed for brevity. The bandwidth is 200 in column 1 (Boone and White, 2015), 100 in column 2 (Crane *et al.*, 2016), 250 in column 3 (Appel *et al.*, 2016a), and 50 in column 4 (Appel *et al.*, 2018). Standard errors are clustered by firm. The sample period is 1996–2006.

$$\text{R2000}_{i,t} = \alpha_0 + \alpha_1 \tau_{i,t} + \sum_{n=1}^{k} \alpha_{2n} \text{Rank}_{i,t}^{n} + \sum_{n=1}^{k} \alpha_{3n} \tau_{i,t} \times \text{Rank}_{i,t}^{n} + \nu_{i,t}$$

Interpretation: With the researcher-constructed rankings, a firm predicted to be at the top of the Russell 2000 is 54.5 to 71.5 percentage points more likely to actually be in the Russell 2000 than a firm predicted to be at the bottom of the Russell 1000. Hence, the first stage of the fuzzy RD design is strong.

Table 6: Covariate balance, researcher-constructed rankings

	(1) Market Cap.	(2) IO 3	(3) QIX 3	(4) DED 3	(5) TRA 3
R2000	-0.007	-0.072	-0.052	-0.009	-0.010
	(0.054)	(0.055)	(0.035)	(0.016)	(0.024)
Constant	1.439***	0.651***	0.396***	0.084***	0.168***
	(0.030)	(0.030)	(0.020)	(0.009)	(0.013)
Third-Order	Yes	Yes	Yes	Yes	Yes
Observations	4409	4077	4077	4024	4076

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Description: This table reports the results of estimating the following equation with 2SLS. The "outcome" variables are end-of-May market capitalization and March-dated total, quasi-indexer, dedicated, and transient institutional ownership. R2000 is an indicator variable for actual membership in the Russell 2000 Index. τ is an indicator variable for predicted membership in the Russell 2000 Index. Rank is the researcher-constructed ranking of end-of-May market capitalization for stock i in year t centered at the cutoff for Russell 2000 membership. The interaction terms are suppressed for brevity. The bandwidth is 200 (Boone and White, 2015). Standard errors are clustered by firm. The sample period is 1996–2006.

$$\mathrm{R2000}_{i,t} = \alpha_0 + \alpha_1 \tau_{i,t} + \sum_{n=1}^{3} \alpha_{2n} \mathrm{Rank}_{i,t}^n + \sum_{n=1}^{3} \alpha_{3n} \tau_{i,t} \times \mathrm{Rank}_{i,t}^n + \nu_{i,t}$$

$$Y_{i,t} = \beta_0 + \beta_1 \widehat{\mathrm{R2000}}_{i,t} + \sum_{n=1}^{3} \beta_{2n} \mathrm{Rank}_{i,t}^n + \sum_{n=1}^{3} \beta_{3n} \widehat{\mathrm{R2000}}_{i,t} \times \mathrm{Rank}_{i,t}^n + \varepsilon_{i,t}$$

Interpretation: With the researcher-constructed rankings, firms that will be at the top of the Russell 2000 had similar market capitalizations at the end of May and had similar levels of total and subtotal institutional ownership at the end of March compared to firms that will be at the bottom of the Russell 1000. Hence, the fuzzy RD design with researcher-constructed May rankings is balanced in these variables.

Table 7: Float adjustment discontinuity, Russell June rankings

	(1) Float Adj.	(2) Float Adj.	(3) Float Adj.	(4) Float Adj.
R2000	1.047***	1.040***	0.869***	0.987***
	(0.056)	(0.051)	(0.037)	(0.046)
Constant	-0.752***	-0.745***	-0.588***	-0.703***
	(0.031)	(0.031)	(0.023)	(0.027)
Polynomial Order	Third	Second	First	First
Bandwidth	200	200	200	100
Observations	4394	4394	4394	2193

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Description: This table reports the results of estimating the following equation with OLS. The outcome variable is the float adjustment, estimated as the percentage difference between the Russell June index weight and the CRSP-calculated June index weight. R2000 is an indicator variable for membership in the Russell 2000 Index. June Rank is the within-index ranking of the Russell June index weights for stock i in year t centered at the cutoff for Russell 2000 membership. The interaction terms are suppressed for brevity. The bandwidth is 200 in columns 1 through 3 (Boone and White, 2015) and 100 in column 4 (Crane *et al.*, 2016). Standard errors are clustered by firm. The sample period is 1996–2006.

$$Y_{i,q,t} = \beta_0 + \beta_1 \text{R2000}_{i,t} + \sum_{n=1}^{k} \gamma_n \text{June Rank}_{i,t}^n + \sum_{n=1}^{k} \delta_n \text{R2000}_{i,t} \times \text{June Rank}_{i,t}^n + \varepsilon_{i,t}$$

Interpretation: The significant positive coefficients on *R2000* indicate that firms at the top of the Russell 2000 with the Russell June rankings have significantly smaller float adjustments (i.e. fewer publicly unavailable shares) than than firms at the bottom of the Russell 1000.

Table 8: Lagged ownership, controlling for the float adjustment, Russell June rankings

	(1) IO 3	(2) QIX 3	(3) DED 3	(4) TRA 3
R2000	0.352***	0.242***	-0.017	0.132***
	(0.052)	(0.029)	(0.039)	(0.017)
Float Adj.	0.044**	0.025**	-0.000	0.008**
	(0.020)	(0.011)	(0.005)	(0.004)
Constant	0.324***	0.146***	0.100**	0.076***
	(0.043)	(0.023)	(0.039)	(0.013)
Third-Order	Yes	Yes	Yes	Yes
Observations	4078	4078	4028	4077

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Description: This table reports the results of estimating the following equation with OLS. The "outcome" variables are March-dated total, quasi-indexer, dedicated, and transient institutional ownership. The float adjustment is estimated as the percentage difference between the Russell June index weight and the CRSP-calculated June index weight. R2000 is an indicator variable for membership in the Russell 2000 Index. June Rank is the within-index ranking of the Russell June index weights for stock i in year t centered at the cutoff for Russell 2000 membership. The interaction terms are suppressed for brevity. The bandwidth is 200 (Boone and White, 2015). Standard errors are clustered by firm. The sample period is 1996–2006.

$$Y_{i,q,t} = \beta_0 + \beta_1 \text{R2000}_{i,t} + \sum_{n=1}^{3} \gamma_n \text{June Rank}_{i,t}^{n} + \sum_{n=1}^{3} \delta_n \text{R2000}_{i,t} \times \text{June Rank}_{i,t}^{n}$$
$$+ \beta_2 \text{Float Adj.}_{i,t} + \varepsilon_{i,t}$$

Interpretation: With the Russell June rankings, controlling for the float adjustment does not eliminate the pre-existing discontinuities in March-dated total and subtotal institutional ownership at the Russell 1000/2000 threshold.