Identification using Russell 1000/2000 index assignments: A discussion of methodologies*

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Abstract

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(*JEL* D22, G23, G30, G34, G35)
Keywords: instrumental estimation, regression discontinuity, Russell indexes

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Abstract

This paper discusses empirical methods that rely on Russell 1000/2000 index assignments for identification. Using simulated data, the paper illustrates why the varying approaches reach conflicting conclusions about the effect of index assignment on a firm's ownership structure and corporate policies. Some estimators likely suffer from bias (e.g., those that employ a sharp regression discontinuity estimation); others do not (e.g., those that either use a fuzzy regression discontinuity or an instrumental variable estimation). The paper also discusses changes in Russell's index assignment methodology that began in 2007 and why these changes require modifications to the existing methodologies.

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Appel, Gormley, and Keim (2016) [AGK (2016) hereafter], Boone and White (2015), and Crane, Michenaud, and Weston (2016) (as well as others) use Russell index assignments as a source of exogenous variation in firms' ownership structures. The idea of this identification strategy is to exploit variation in ownership that occurs because of stocks' assignments to two widely-used market benchmarks— the Russell 1000 index and Russell 2000 index. Because the Russell 2000 is a relatively more popular benchmark than the Russell 1000 for index funds and exchange-traded funds (ETFs), the assignment of an individual stock to one or the other index can have a significant impact on a firm's ownership structure. For example, Appel, Gormley, and Keim (2019) [AGK (2019) hereafter] show that the proportion of a stock's total market cap held by all passively-managed mutual funds and ETFs is about 40% higher, on average, for stocks in the Russell 2000 relative to otherwise similar stocks in the Russell 1000.

However, there is little agreement about exactly how the Russell index assignments affect firms' ownership structures and other corporate outcomes. For example, Boone and White (2015), Crane, Michenaud, and Weston (2016), and others claim that inclusion in the Russell 2000 increases total institutional ownership by a large 10 to 40 percentage points and that both passive and active institutional ownership drive this increase. AGK (2016), however, finds that assignment to the Russell 2000 has no impact on total institutional ownership. Instead, AGK claim there is a one to two percentage point effect on ownership by passively managed mutual funds and no impact on actively managed fund holdings.[1] Moreover, in further contrast to other papers, AGK (2016) also fail to detect an effect of index assignment on CEO compensation or firm policies related to investments, acquisitions, and capital structure.[2]

We explain why these papers reach wildly different conclusions. In short, some estimators suffer from bias (e.g., those that employ a sharp regression discontinuity estimation); others do not (e.g., those that either use a fuzzy regression discontinuity or an instrumental variable estimation). Papers that use unbiased estimators find that Russell index assignments have little to no impact on *total* institutional ownership (e.g., AGK 2016, 2019; Wei and Young 2019) but do increase ownership by <u>index</u> funds (e.g., AGK 2016, 2019; Cao, Gustafson, and Velthuis, 2019; Ben-David, Franzoni, and Moussawi, 2019; Glossner, 2019). Only papers that use biased estimators (e.g., Boone and White, 2015; Crane, Michenaud, and Weston, 2016) find large effects on total institutional ownership and other corporate outcomes. Appendix Table 1 provides a complete list of papers that employ each of the above methodologies. Below, we also discuss papers that use yet other biased strategies (e.g., Schmidt and Fahlenrach, 2017).

The key challenge with using Russell index assignments as an identification strategy is that Russell determines index assignments using a stock's total market cap. Therefore, one cannot merely compare the

[1] After merging Thomson Reuters' S12 mutual fund holdings data to the CRSP Mutual Funds database, AGK (2016) defines passive ownership as ownership by any fund or ETF classified as an index product by the CRSP Mutual Fund database or where the name of the fund or ETF includes any of the following keywords: *Index*, *Idx*, *Indx*, *Ind_* (where _ indicates as space), *Russell*, *S & P*, *S and P*, *S&P*, *SandP*, *SP*, *DOW*, *Dow*, *DJ*, *MSCI*, *Bloomberg*, *KBW*, *NASDAQ*, *NYSE*, *STOXX*, *FTSE*, *Wilshire*, *Morningstar*, *100*, *400*, *500*, *600*, *900*, *1000*, *1500*, *2000*, and *5000*.

[2] See footnote #6 of AGK (2016). Both Wei and Young (2019) and Glossner (2018) also highlight conflicting findings among various papers that make use of the Russell 1000/2000 threshold for identification.

characteristics and policy choices of companies across the two Russell indexes and attribute observed differences to index inclusion. A stock's total market cap is a potential determinant of ownership structure and various corporate policies, and hence, such simple comparisons can suffer omitted variable biases. Unfortunately, controlling for this crucial determinant of index assignment is difficult as Russell uses a proprietary measure of total market cap that is unobservable to researchers.

This paper discusses the different methodological choices made in the Russell 1000/2000 literature to control for this unobserved determinant of index inclusion and explains why they yield different findings. To do this, we begin by simulating a dataset that contains the key features of the Russell setting and matches observed empirical patterns documented in the extant literature. To facilitate discussion and replication, we provide the code used to generate our baseline data. We then discuss each of the Russell 1000/2000 estimation strategies used in the literature and the code one would use to execute them on our simulated data. Because the structure of the dataset is known, this analysis illustrates why the various estimation strategies used in the literature can reach such different conclusions.

The primary reason for these differences stems from the different methods used to account for the endogenous weighting of stocks within indexes by Russell. In particular, to minimize the trading costs of funds seeking to track each index, Russell assigns smaller portfolio weights within each index to the less liquid, high-inside-ownership stocks. Failing to account for how Russell sorts stocks within each index results in some specifications comparing the least-liquid, highest-inside-ownership stocks at the bottom of the Russell 1000 to the most-liquid, lowest-inside-ownership stocks at the top of the Russell 2000 and incorrectly attributing observed differences to index assignment rather than to the underlying differences in liquidity and inside ownership. This comparison leads to faulty inferences about the impact of index assignment on firms' ownership structure and various other firm-level outcomes.[3]

This paper also discusses changes made by Russell starting in 2007 in how it constructs its indexes and why these changes further complicate attempts to use the Russell 1000/2000 setting for identification. This discussion is particularly important for researchers interested in using the Russell setting to study the effects of passive ownership in more recent years. Ownership of US companies by index funds and ETFs has increased sharply since 2006, making the post-2006 years a better setting for researchers interested in studying the effects of this growth on various outcomes. For example, passively managed funds' share of all mutual fund assets has increased from around 15% in 2006 to 50% by the end of 2019 (Lim, 2019). We discuss which estimation strategies will work in these later sample years.

Last, we briefly discuss two recent papers that also compare the various methodologies used in the Russell 1000/2000 setting—Wei and Young (2019) and Glossner (2018). A number of the points made in these papers, especially the inappropriate use of float-adjusted rankings in a regression discontinuity type

[3] Our discussion of this problem, now contained in this paper, first appeared in the working paper version of AGK (2016). We have since expanded the scope in this paper to include more recent estimation techniques used in this literature (e.g., Schmidt and Fahlenbrach 2017; Wei and Young 2019).

estimation, mirror those discussed in our paper and earlier papers (e.g., Chang, Hong, and Liskovich 2015; Mullins 2014; Appel, Gormley, and Keim 2016). That said, Wei and Young (2019) and Glossner (2018) do shed additional light on the challenges mentioned above and why some findings in the existing literature are misleading. In the last section, we briefly discuss a couple of disagreements we have with these papers. Most importantly, we also clarify that while Russell index assignments do not affect firms' *total* institutional ownership [the finding of Wei and Young (2019), which was shown earlier in AGK (2016)], they do affect *passive* institutional ownership [the finding of AGK (2016), which Glossner (2018) confirms using both the AGK (2016) and Wei and Young (2019) estimation approaches.]

The remainder of our paper proceeds as follows. In Section 1, we summarize how the Russell 1000 and 2000 indexes are constructed both before and after 2006. In Section 2, we discuss the underlying identification challenges one faces when attempting to use the Russell setting to analyze the effect of ownership structure on various outcomes. In Section 3, we simulate data that includes the key features of the pre-2007 Russell setting to evaluate the main types of estimation techniques employed in the literature and illustrate why some of them are highly problematic. For the techniques that yield unbiased estimates, we discuss their relative tradeoffs. In Section 4, we discuss how to use the Russell 1000/2000 setting as a source of identification after Russell changed its index assignment rules in 2007. In Section 5, we discuss Wei and Young (2019) and Glossner (2018). Section 6 concludes.

1. Russell index construction

Although many papers already discuss the construction of the Russell 1000 and 2000 indexes, it is worth explaining it here because the details are crucial for constructing a correct empirical specification. For example, the incorrect use of the end-of-May *total* market capitalization (used to determine index assignment for the next twelve months) and the *float-adjusted* market capitalization (used to determine a stock's ranking within the assigned index) can lead to spurious findings.

1.1. Construction of the Russell indexes, Pre-2007

Before 2007, the Russell 1000 included the 1,000 largest US stocks in terms of market capitalization, and the Russell 2000 included the next largest 2,000 stocks not included in the Russell 1000. To account for changes in stocks' market cap ranking, Russell reconstitutes its indexes each year at the end of June using a proprietary measure of stocks' total market capitalizations calculated as of the last trading day in May of that year. Specifically, Russell included stocks with an end-of-May market capitalization below (above) the market cap of the 1,000th (1,001st) largest market cap in the Russell 2000 (Russell 1000). Between the yearly end-of-June reconstitutions, index membership remained constant except for the occasional addition or removal due to delistings and IPOs.[4]

[4] Russell's method for setting the end-of-June reconstitution date has varied over time. Since 2013, reconstituted indexes take effect after market close on the last Friday in June unless the last Friday falls on the 29th or 30th, in which

Russell determines each stock's daily portfolio weight in the index using a <u>second</u> measure of market cap, the *float-adjusted* market cap. Unlike the total market cap used to determine index membership, Russell's float-adjusted market cap only includes the value of shares that Russell considers publicly available. Shares held by someone with more than 10% of shares outstanding, by another member of a Russell index, by an employee stock ownership plan (ESOP), or by a government are excluded from a firm's float-adjusted market cap, as are any shares not listed on an exchange. Therefore, a stock that was the 1,000th largest stock in total market cap need not be the stock with the smallest portfolio weight in the Russell 1000 index. Russell recalculates the float-adjusted market caps and resulting portfolio weights daily, after market close, using the most recent price and the float-adjusted number of shares.

Russell weights stocks using its float-adjusted market cap to minimize the trading costs of index funds that track each benchmark. By determining portfolio weights using the float-adjusted market cap, Russell tends to shift the less-liquid, high-inside-ownership stocks toward the bottom of each index, and the most-liquid, low-inside-ownership stocks toward the top of each index. Failure to account for this weighting and the resulting imbalances in stock characteristics at the top and bottom of the two indexes is a common flaw of empirical methodologies that make use of the Russell 1000/2000 setting.

1.2. Construction of the indexes, 2007 and later

Beginning with the reconstitution of the Russell 1000 and 2000 indexes in June of 2007, Russell changed its methodology. The Russell 1000 no longer comprises the 1,000 largest stocks in terms of market cap, and the Russell 2000 no longer comprises the next 2,000 largest stocks. Russell changed its methodology to reduce the number of stocks switching indexes each June.[5]

During each yearly reconstitution, Russell now uses three factors to determine stocks' index assignments: (1) the stock's index assignment in the previous reconstitution year, (2) the stock's end-of-May market capitalization ranking, and (3) whether the stock's end-of-May market cap falls within a specific range of the cutoff between 1,000th and 1,001st largest end-of-May market caps. Specifically, a stock with an end-of-May market cap below (above) the market cap of the 1,000th (1,001st) largest market cap will be included in the Russell 2000 (Russell 1000) *unless* that stock was in the Russell 1000 (Russell 2000) last year *and* its market cap is below (above) the market cap of the 1,000th (1,001st) largest market

case, the newly reconstituted indexes take effect after market close on the preceding Friday. Russell announces the schedule for reconstitution in early spring and publishes preliminary lists of each index's constituents in the first or second week of June. See Appendix A of Ben-David, Franzoni, and Moussawi (2019) for a complete list of reconstitution and preliminary announcement dates going back to 1996. Between reconstitutions, Russell removes delisted stocks from the index immediately and adds newly-listed IPO stocks quarterly using the most recent end-of-June reconstitution market capitalization cutoff and the newly-listed stock's market capitalization at the time of its IPO. Because of these additions and deletions, the number of stocks can deviate from 1000/2000 during the year. For more details regarding eligibility for inclusion in the Russell indexes, see Russell Investments (2013, 2020).

5 It is unclear when Russell announced the change in methodology, but looking back at analyst reports, it is clear that market participants were aware of the change by at least early May 2007.

cap by less than 2.5% of the cumulative market cap of the Russell 3000E Index.[6]

This policy, which Russell refers to as "banding," means that stocks previously in the Russell 2000 are only moved to the Russell 1000 index during the annual reconstitution if their end-of-May Russell market cap ranking increased significantly. For example, a Russell 2000 stock that moved from a market cap ranking of 1,050 at the end of May last year to a market cap ranking of 950 this year will remain in the Russell 2000 even though it is now among the 1,000 largest stocks. Given the bandwidth size (2.5% of the cumulative market cap of the Russell 3000E Index), a Russell 2000 stock typically would need to increase its Russell-calculated market cap to higher than that of the 800th largest market cap before being reassigned to the Russell 1000 at reconstitution. Likewise, a Russell 1000 stock would need to fall below an end-of-May market cap ranking of about 1,200 before being moved to the Russell 2000. This banding policy also causes the number of stocks in the two indexes to deviate from 1,000 and 2,000.

The implementation of the banding policy in 2007 poses an added challenge to using the Russell 1000/2000 cutoff as a source of identification. In particular, the Russell 1000 will be "over-weighted" in stocks with a lower-than-average stock performance during the prior year, and the Russell 2000 will be "over-weighted" in stocks with a higher-than-average stock performance during the prior year. These differences occur because the banding procedure constrains the best-performing stocks at the top of the Russell 2000 from naturally moving to the Russell 1000, and conversely for the worst performers at the bottom of the Russell 1000, and will confound comparisons of stocks at the bottom of the Russell 1000 with stocks at the top of the Russell 2000. We discuss this challenge further in Section 4.

Beginning in 2016, Russell also switched to using a "rank day" that need not coincide with the last trading day of May. For example, in the year 2020, the total market cap ranking is based on market caps calculated as of May 8, 2020. Russell preannounces the "rank day" in early spring. Appendix A of Ben-David, Franzoni, and Moussawi (2019) provides a complete list of historical rank days.

2. Challenges with using Russell index membership for identification

In finance, there is an extensive literature that seeks to understand the effect of firms' ownership structures on various outcomes [e.g., see Edmans and Holderness (2017) for a survey of the literature on block holders and governance]. However, identifying the causal effect of ownership structure on corporate policies is difficult as firms' ownership structures are inherently endogenous. In particular, researchers cannot merely regress corporate outcomes onto measures of ownership structure and interpret the findings as causal. Owners may choose to own companies with particular policies (i.e., there could be a reverse causality bias). Moreover, ownership structures might correlate with other company characteristics that affect the outcome of interest (i.e., there could be an omitted variable bias).

[6] The Russell 3000E Index covers the largest 4,000 US stocks; it included 3,428 stocks as of March 31, 2020.

Russell index membership provides a potentially useful source of exogenous variation in firms' ownership structures. If some investors tend to hold stocks from one index but not the other, Russell's index assignments could generate variation in firms' ownership structures that is not driven by reverse causality. This variation might also be orthogonal to other determinants of the outcome of interest.

One particular type of ownership thought to be affected by index inclusion is that of passive investors. Passive mutual funds and ETFs attempt to match the performance of a market index by holding a basket of representative securities in the particular market index in proportion to their weights in the index. Using industry estimates of the number of assets passively tracking the two indexes in 2010, as provided in Chang, Hong, and Liskovich (2015), AGK (2019) estimates that assignment to the Russell 2000, rather than to the Russell 1000, would increase a stock's passive institutional ownership by 3.76 percentage points.[7] This increase occurs because the indexes are value-weighted and the Russell 2000 is a relatively more popular benchmark among index funds and ETFs. Consistent with this, AGK document a similarly-sized difference in passive institutional ownership across the two indexes in that year and others.

Unfortunately, using the Russell setting to uncover the causal effect of such passive institutional ownership (or institutional ownership more broadly) is easier said than done.

2.1. Why one cannot rely on simple comparisons of outcomes across indexes or from index switchers

An econometrician cannot just compare the policy choices of firms assigned to the Russell 2000 to that of stocks in the Russell 1000 and attribute any differences to the observed differences in passive ownership. Because Russell determines index assignment using a stock's total market cap ranking, average differences in market cap across the two indexes could drive the observed differences in policy outcomes rather than the observed difference in passive ownership. In other words, because a stock's index assignment correlates with market cap, a failure to control for the market cap in a regression of outcomes onto index assignment may result in an omitted variable bias.

Likewise, one cannot attribute observed changes in policy outcomes among firms that switch indexes to the coinciding change in passive ownership. Firms that switch index are those that experienced a change in their relative market cap ranking over the past year. Hence, omitted factors related to the relative change in a stock's market cap ranking could drive any observed policy changes rather than the index switch. Using changes in policy outcomes of all non-switchers as a counterfactual does not help address the omitted variable concern as the non-switchers likely exhibit a different change in market cap ranking than switchers, and hence, do not represent an appropriate control group.

2.2. Controlling for the determinant of index assignment is critical

[7] In particular, estimates suggest that \$56.8 billion in assets were passively tracking the Russell 2000 in 2010, which accounts for about 4.93% of the index's total market cap of \$1,115 billion, while there was \$137.1 billion of assets passively tracking the Russell 1000, accounting for just 1.17% of the index's total market cap of \$11,740 billion.

To isolate the potentially exogenous variation created by index assignment, one must control for the determinants of index assignment (or index switching) that could correlate with the outcome of interest, and hence, cause an omitted variable bias. In other words, one must control for the end-of-May market cap rankings (or changes in these rankings) in cross-sectional comparisons across indexes (or pre- versus post-switch comparisons). Alternatively, one could control for market caps (or changes in market cap).[8]

The primary challenge is that researchers are unable to observe the end-of-May market cap and resulting rankings used by Russell to determine index assignments. Russell uses a proprietary market capitalization value that does not perfectly match the market caps reported in publicly available databases like CRSP. For example, using 2006 CRSP end-of-May market caps to construct the rankings, one finds that only 68% of the firms ranked between 950 and 1,000 were included in the Russell 1000 after the reconstitution in June 2006. The remaining 32% were assigned to the Russell 2000 even though they were among the 1,000 largest market caps, according to CRSP.

A recent analysis by Ben-David, Franzoni, and Moussawi (2019) finds that a combination of factors likely explains why the market caps calculated from CRSP do not match the unobserved market caps being used by Russell to determine rankings. One factor is that Russell appears to include non-publicly traded shares sometimes not captured by CRSP in its calculation of total end-of-May market cap. Another factor is that Russell often appears to combine different stock issuances of the same issuer when calculating the total market cap, but in other instances, it does not. Russell's mapping of issuances to particular companies also seems to differ from CRSP's in some cases, and whether or not Russell includes stock issuances related to a recent spinoff in the calculation of total market cap does not seem to follow a systematic pattern.

Russell's unwillingness to share the historical market caps used to determine index assignments likely reflects a desire to make it difficult for investors to predict which stocks will switch indexes. Given the stock price effects of such switches, being able to predict index switches represents a profitable trading opportunity,[9] and Russell may want to avoid facilitating such trades. Disclosing the historical market caps might undermine this goal by making it easier to reverse engineer how Russell calculated end-of-May market caps for each company, and hence, how it is likely to do so in the future.

Various attempts by researchers to recover Russell's unobserved end-of-May market caps have failed. For example, some papers have found one can improve the ability to predict index assignments by

[8] In practice, whether one controls for market cap, market cap rankings, or both will not matter. The market cap rankings are just a monotonic function of the underlying market cap. So, as long as one is allowing the functional form of the association between outcome and market cap (or market cap rankings) to take many possible forms, one will obtain similar findings when controlling for either measure of market cap.

[9] Using publicly available pricing and share data, Chang, Hong, and Liskovich (2015) report an average 5% price increase during June for stocks predicted to switch from the Russell 1000 to the Russell 2000 at reconstitution and a 5.4% decrease for stocks predicted to switch indexes in the other direction. There is also an extensive literature on the price effect of index additions and deletions to the S&P 500 index; see, e.g., Patel and Welch (2017).

combining pricing data from CRSP with the number of shares in the Compustat quarterly filings (e.g., see Chang, Hong, and Liskovich 2015). More recently, Ben-David, Franzoni, and Moussawi (2019) show that one can do even better using a complicated combination of CRSP issuer-level market caps and company-level market caps from the Compustat Securities Daily and Compustat Quarterly datasets. However, even these improved methods are unable to perfectly predict Russell's index assignments, which means these market caps are still a noisy measure of the actual market cap Russell uses.

This inability to observe (and control for) the critical determinant of index assignment, Russell's proprietary end-of-May market cap, makes it challenging to use the Russell index assignments as a source of exogenous variation. While researchers have proposed a variety of potential ways to overcome this limitation, many of the proposed solutions are problematic.

3. Pre-2007 specification choices when using the Russell 1000/2000 threshold for identification

To facilitate our discussion of specifications in the literature, we begin by generating a dataset in Stata that captures what we believe to be the key features of the Russell setting before 2007. The generated data will match a number of the underlying empirical patterns documented in the existing literature and allow us to illustrate why some specification choices yield biased estimates, while others do not. The appendix provides a version of the same code using R.

3.1 Generating a baseline dataset that matches observed patterns

For our analysis, we generate a dataset with yearly market caps and index assignments for 3,000 stocks each year between 1998 and 2006, which coincides with the sample years used in AGK (2016). We generate three market caps in the data: (1) the unobservable end-of-May total market cap used by Russell to determine index assignments (*russell_mc*), (2) the observable float-adjusted market cap used by Russell to determine within-index portfolio weights (*float_mc*), and (3) the observable CRSP end-of-May market caps (*crsp_mc*) that are a noisy approximation of Russell's total end-of-May market caps.

We begin by constructing *russell_mc*. Because we do not observe these market caps or their underlying distribution, we set the initial distribution of Ln(*russell_mc*) in 1998 using a normal distribution that matches the observed mean and standard deviation of the observable Ln(CRSP market caps (millions)) for 1998 (mean=7, sd=1.3). We use the value-weighted average and standard deviation of annual stock returns observed in CRSP from 1998-2006 (mean=0.08, sd=0.35) to generate *russell_mc* in later years.

```
	set obs 27000
	egen year = seq(), f(1998) t(2006)
	bys year: gen stock_id=_n
	gen log_russell_mc=rnormal(7,1.3) if year==1998
	sort stock_id year
	by stock_id: replace log_russell_mc=log_russell_mc[_n-
1]+(.08+(.35^2)/2)+.35*rnormal(0,1) if log_russell_mc==.
	gen russell_mc=exp(log_russell_mc)
```

Next, we construct the *float_mc* used by Russell to sort stocks within indexes. To do this, we generate a time-invariant, stock-specific float adjustment factor (*traded*) that captures the proportion of a firm's shares that Russell considers publicly-traded. Because we do not observe the distribution of adjustment factors Russell uses to construct *float_mc*, we assume a truncated skewed normal distribution for *traded* that is similar to the distribution of adjustment factors one obtains by dividing Russell's float-adjusted market cap (which is observable) by CRSP market cap (mean=0.85, var=0.05, skewness=-1.5, kurtosis=6).

```
sknor 27000 123 0.85 0.05 -1.5 6
replace skewnormal=. if year!=1998
replace skewnormal=0 if skewnormal < 0
replace skewnormal=1 if skewnormal>1 & skewnormal!=.
bys stock_id: egen traded=mean(skewnormal)
gen float_mc= traded*russell_mc
```

We then follow Russell's assignment process to allocate every stock to an index each year and construct Russell's two ranking variables. We construct Russell's unobserved total market cap ranking (*russell_rank*) using *russell_mc*, and for every year, we assign the 1000 stocks with the largest *russell_mc* to the Russell 1000 index (*index*=1) and the remaining 2000 stocks to the Russell 2000 (*index*=2). We then sort stocks within each index using *float_mc* to construct the ranking variable that captures Russell's float-adjusted ranks (*float_rank*). In particular, we assign the Russell 1000 stock with the smallest *float_mc* a *float_rank* of 1,000, the Russell 1000 stock with the second smallest *float_mc* a *float_rank* of 999, and so on. We assign the Russell 2000 stock with the largest *float_mc* a *float_rank* of 1,001, the Russell 2000 stock with the second-largest *float_mc* a *float_rank* of 1,002, and so on.

```
gsort year -russell_mc
by year: gen russell_rank=_n
gen index=1 if russell_rank>=1 & russell_rank<=1000
replace index=2 if russell_rank>=1001
gsort year +index -float_mc
by year: gen float_rank=_n
```

Next, we construct our last market cap measure, *crsp_mc*, which is the observable CRSP total market cap, and the corresponding ranking, *crsp_rank*. We start by assuming that for most stocks, the CRSP market caps are only a slightly noisy measure of the unobserved market cap used by Russell. We do this by assuming that the CRSP market cap equals the Russell market cap multiplied by $1 + x$, where we select x from a normal distribution with mean=0 and sd=0.01.[10] Because Russell's total market cap appears to

[10] We choose this particular level of noise as it yields simulated data that closely matches the actual data. Increasing the assumed amount of noise (e.g., assuming sd=0.1) does not affect the relative performance of the different

include non-publicly traded shares not captured in CRSP, resulting in about 7-10 percent of CRSP market caps to be seemingly much smaller than those used by Russell (Ben-David, Franzoni, and Moussawi, 2019), we then reduce the CRSP market cap by 50 percent for a random 20 percent of the observations with a float-adjustment factor less than 75 percent. Moreover, because Russell appears to occasionally use a different mapping of issuances to companies resulting in smaller market caps than reported in CRSP, we increase the CRSP market cap by 50 percent for a random one percent of the sample.[11]

```
gen temp=runiform(0, 1)
gen russell_mc_adj=russell_mc
replace russell_mc_adj= russell_mc*.50 if traded<.75 & temp<.20
replace russell_mc_adj= russell_mc*1.5 if temp>0.99
gen crsp_mc= russell_mc_adj*(1+rnormal(0,.01))

gsort year -crsp_mc
by year: gen crsp_rank=_n
```

In terms of ownership structure, we assume that Russell's index assignment has *no* effect on the proportion of a stock's shares held by institutional investors, but that institutional investors prefer to hold more liquid stocks and stocks with less inside ownership. To accomplish this, we approximate the extent of illiquidity and inside ownership using the ratio of a stock's float-adjusted market cap to its CRSP market cap (*float_adj*), where larger values represent more liquid stocks with less inside ownership. We then generate the proportion of institutional ownership (*IO*) such that its mean and standard deviation matches the observed distribution of ownership shares and is, on average, increasing in *float_adj* by the same amount observed in the data [regressing *IO* onto *float_adj,* one finds a slope coefficient of 0.37 with a T-stat=55].[12]

```
gen float_adj = float_mc/crsp_mc
gen IO = rnormal(0.35, 0.24) + float_adj*rnormal(0.37, 0.02)
replace IO=0 if IO<0
replace IO=1 if IO>1
```

Finally, while we assume no effect of index assignment on total institutional ownership, we do assume that assignment to the Russell 2000 increases passive ownership by one percentage point. To do this, we construct the baseline proportion of total market cap held by passive investors (*passive*) by

specifications discussed in Section 3.2, except that the fuzzy RD using *crsp_rank* performs even more poorly. This occurs because a noisier CRSP weakens the first stage of that particular estimation; see Section 3.2.4 for more details.

[11] Admittedly, our parameter choices here are a bit ad hoc as we do not observe the actual mapping from Russell's unobserved total market caps to the total market caps we observe in CRSP. Because of this, we choose parameters that provide a reasonable match of the observed data patterns. The exact parameter choices we make here, however, do not qualitatively affect the performance of the different estimation strategies.

[12] We model *float_adj* and *IO* as a function of *crsp_mc* rather than *russell_mc* because researchers do not use the unobservable *russell_mc* when calculating *IO*, which is typically constructed by dividing the number of shares held by institutional investors (as reported in the 13F data) by the total observable outstanding shares reported in CRSP.

multiplying *IO* by the average share of institutional ownership held in passively managed mutual funds and ETFs during the AGK (2016) sample period (0.046). Then, for stocks assigned to the Russell 2000, we increase the proportion of ownership accounted for by passive investors by one percentage point. Combined, our assumptions of an effect on passive ownership but not total institutional ownership allows us to analyze the ability of different estimators to avoid both Type 1 and Type 2 errors.

```
gen passive = 0.046*IO
replace passive=passive+.01 if index==2
```

Finally, we construct an indicator for inclusion in the Russell 2000, *R2000*, and change our measures of ownership, *IO* and *passive*, into percents.

```
gen R2000=0
replace R2000=1 if index==2

replace passive=passive*100
replace IO=IO*100
```

Using these parameter choices, we can generate patterns that closely resemble the actual data. To illustrate this, we start by sorting the *actual* data using end-of-May CRSP market cap rankings and plotting the share of stocks in the Russell 2000 at the start of each reconstitution year along with the average institutional and passive ownership in September of that year (see Figure 1). To sharpen the analysis, we limit the sample to stocks with a CRSP end-of-May market cap ranking of 750 to 1,250. Doing this, we confirm that CRSP market cap rankings do not correctly predict all index assignments. For example, the probability of being assigned to the Russell 2000 increases sharply for stocks with CRSP rankings exceeding 970 rather than 1,000. There is no evidence of a corresponding increase in institutional ownership (middle panel of Figure 1), suggesting that index assignment does not affect overall institutional ownership. There is evidence, however, of a corresponding increase in passive ownership (bottom panel), suggesting that index assignment does affect such ownership. The observed jumps in index assignment and passive ownership are similar to those shown in Figure 3 of AGK (2016). In Figure 2, we create the same plots using our *simulated* data. The simulated data exhibit similar patterns of a jump in the likelihood of being assigned to the Russell 2000 and of having higher passive ownership around the *crsp_rank* of 970 (top and bottom panels, respectively), but no corresponding jump in institutional ownership (middle panel).[13]

[Figures 1 and 2 here]

The generated data also match observed patterns when we instead plot the same outcomes as a

[13] Our assumption that CRSP market caps are 50 percent of the unobserved Russell market cap for some stocks with a large proportion of non-publicly traded shares causes the jump to occur at the ranking of 970 rather than 1000. As noted earlier, we make this assumption to reflect the observed pattern that CRSP market caps frequently seem to exclude some share classes used by Russell (see Ben-David, Franzoni, and Moussawi (2019)).

function of the *within-index* float-adjusted rankings (i.e., *float_rank*). Figure 3 (actual data) and Figure 4 (simulated data) illustrate this similarity. In both figures, we see that the within-index ranking mechanically predicts index assignment (top panel). We also see that this alternative way of sorting leads to a significant increase in *both* institutional and passive ownership at the ranking of 1000 (middle and bottom panels).

[Figures 3 and 4 here]

The observed increase in institutional ownership when using the within-index rankings to sort the data highlights a vital problem of some identification strategies used in the literature. Looking at these figures, one might conclude that index assignment affects *both* overall institutional ownership and passive ownership. However, we know that this is not true in the generated data. Instead, this drop in institutional ownership occurs because of how Russell sorts and weights stocks within each index. When Russell sorts on float-adjusted market cap, stocks with a more significant proportion of shares not publicly held (and hence, not held by institutional investors because these tend to be the less liquid stocks with higher inside ownership) drop toward the bottom of the index. A comparison that ignores this can lead to faulty inferences about the impact of index assignment on firms' ownership structure and other outcomes.

Resorting the simulated data using rankings based on Russell's unobserved total market cap further illustrate this problem. Figure 5 does this resorting. As can be seen, there is no evidence of an impact on institutional ownership but clear evidence of an impact on passive ownership. The pattern is similar to that seen in Figures 1 and 2, which sort on the observable CRSP market caps, but less noisy because we are now sorting on the actual variable used to determine index assignments rather than a noisy version of it.

[Figure 5 here]

3.2 Comparing the performance of specifications used in the literature

Russell 1000/2000 specifications in the literature range from using regression discontinuity to instrumental variable estimation. While each specification is trying to use Russell's index assignment as a source of exogenous variation in ownership structures, how they each attempt to control for the key, unobserved determinant of index assignment, Russell's total market cap, varies considerably. Although the differences between the specifications and how they control for the total market cap are subtle, the differences in implementations can lead to significant differences in inferences. We now illustrate this using the simulated data where a correctly specified model will show that passive institutional ownership is one percentage point higher in the Russell 2000 Index relative to the Russell 1000 index, while there is no difference in total institutional ownership across the two indexes. For completeness, Appendix Table 1 provides a list of papers that employ each of the below methodologies along with additional details on how each paper's approach varies from the baseline estimations we describe below.

3.2.1. Sharp regression discontinuity using Russell's unobservable end-of-May market cap rankings

If the total market cap rankings used by Russell were observable, the ideal specification for the

Russell 1000/2000 setting would be a sharp regression discontinuity (RD) estimation. Intuitively, this estimation exploits the discontinuity in index assignment at the unobserved *Rank*=1000 threshold (see top panel of Figure 5) and tests for discontinuities in other outcomes around this threshold. Specifically, researchers interested in estimating the effect of Russell 2000 assignment on some outcome *Y* before 2007 could have estimated the following pooled, cross-sectional sharp RD estimation,

$$Y_{it} = \alpha + \gamma R2000_{it} + \sum_{n=1}^{N} \phi_n (Rank_{it} - 1000)^n + \mu_{it}, \tag{1}$$

where *Y* is the outcome of interest for firm *i* in reconstitution year *t*, *Rank* is the ranking of firm *i* in reconstitution year *t* when using Russell's unobserved end-of-May market capitalization (e.g., the 995th largest firm would have a rank of 995), and *R2000* is an indicator that equals one for firms assigned to the Russell 2000 that reconstitution year. One could also include year fixed effects in the specification to control for a time trend, but because this will not affect our simulated regressions, we do not include them.

The sharp RD estimation controls for the key determinant of index assignment, end-of-May market caps, in a couple of ways. First, it directly controls for Russell's end-of-May market cap ranking using a flexible functional form, where the polynomial order of controls, *N*, can be varied.[14] Second, one would typically restrict the sample to observations close to the cutoff threshold of *Rank* = 1000 as a way to nonparametrically control for market cap and reduce concerns about the functional form being used to control for market cap. For example, one might restrict the sample to stocks with a *Rank* between 750 and 1,250. By limiting the sample in this way and centering the *Rank* controls around the threshold of *Rank*=1000, the sharp RD estimate γ identifies the effect of being assigned to Russell 2000 on outcome *Y* by testing for a discontinuity in *Y* between the 1,000th and 1,001st largest firms.

Using the data we constructed earlier, we can confirm that the sharp RD estimation recovers the correct estimates. For example, when using a bandwidth of 250 stocks around the *Rank*=1000 threshold to select the sample and a third-order polynomial control (*N*=3), we run the following code to estimate the impact of index assignment on passive ownership:

```
gen rank=russell_rank

gen rank1=(rank-1000)
gen rank2=(rank-1000)^2
gen rank3=(rank-1000)^3

gen band=250

reg passive R2000 rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-band,
```

[14] One could also add a set of controls, $R2000_{it} \times \sum_{n=1}^{N}(Rank_{it} - 1000)^n$, to allow the functional form of the relation between *Rank* and outcome *Y* to vary above and below the cutoff. See Angrist and Pischke (2009), Lee and Lemieux (2010), and Roberts and Whited (2013) for more details regarding regression discontinuity estimations. We do not do that here and in our regressions using the simulated data as we know that the functional form for how *Rank* relates to our outcomes is the same above and below the threshold.

```
cluster(stock_id)
```

In Table 1, we report the results of the sharp RD estimation for bandwidths of 50, 100, 250, and 500 and polynomial orders of N=1, 2, and 3. As can be seen, the estimation uncovers about a one percentage point increase in passive ownership for stocks assigned to the Russell 2000 (Table 1, top panel) but no evidence of an impact of index assignment on overall institutional ownership (bottom panel). In essence, the sharp RD estimation is just formally showing what could already be seen visually in Figure 5; there is a discontinuity in passive ownership, but not institutional ownership, at *Rank*=1000.

[Table 1 here]

To estimate the effect of index assignment using stocks that switch indexes, one simply adds stock-level fixed effects to the sharp RD estimation, thus converting it from a pooled, cross-sectional estimation to a panel estimation. The coefficients of the estimation would now be estimated using within-stock variation in index assignment, *R2000*, after conditioning on within-stock variation in market cap rankings. In essence, this approach identifies index assignment's effect by comparing the change in outcomes of a stock that changes market cap rankings by X spots and *does* switch indexes to the change in outcomes for another stock that changes market cap rankings by X spots but *does not* switch indexes.

Because there are relatively few stocks that switch indexes each year, an estimation that relies on switchers will tend to be noisier and less reliable in practice. Table 2, which provides estimates from this 'switcher' sharp RD estimation, illustrates this weakness. At smaller bandwidths, the estimates are very noisy and often fail to identify the correct effect of index assignment on ownership (Table 2, Columns 1-6). At larger bandwidths, however, the sharp RD estimates begin to recover the correct point estimates as there is a sufficient number of switchers to obtain more precise estimates (Columns 7-12).

[Table 2 here]

As specified, the sharp RD identifies the effect of index assignment on outcome Y. However, if researchers are instead interested in using index assignment as a source of exogenous variation in passive ownership, *Passive%*, they can use the sharp RD as the first stage of an IV estimation. Specifically, one would use *Passive%* as the outcome in Equation (1), and the second stage of the IV would be

$$Y_{it} = \alpha + \chi \widehat{Passive}\%_{it} + \sum_{n=1}^{N} \phi_n (Rank_{it} - 1000)^n + \varepsilon_{it}. \tag{2}$$

This IV also uses the discontinuity in index assignments for identification and makes the standard exclusion assumption. In particular, this IV regression identifies the effect of passive ownership on outcome Y by taking the ratio of two discontinuities. It takes the discontinuity in outcome Y between the 1,000th and 1,001st largest firms (which is the reduced form coefficient given by the sharp RD) and scales it by the discontinuity in *Passive%* between the 1,000th and 1,001st largest firms (which is the first stage coefficient of the estimation). The exclusion assumption of this IV estimation would be that, after controlling the determinants of index assignment (i.e., total end-of-May market cap), index assignment only affects the outcome of interest *Y* through its effect on passive ownership.

3.2.2. Sharp regression discontinuity using Russell's observable June float-adjusted rankings

Because Russell's total end-of-May market cap is unobservable, some papers (e.g., Boone and White, 2015; Khan, Srinivasan, and Tan, 2017) construct *Rank* using the float-adjusted <u>within-index</u> rankings that are assigned by Russell (see Appendix Table 1 for a list of papers that use this approach). By making this change, the modified sharp RD estimation no longer tests for discontinuities using Russell's unobserved threshold between the 1,000th and 1,001st largest stocks. Instead, the estimation tests for discontinuities in outcomes between stocks at the bottom of the Russell 1000 and stocks at the top of the Russell 2000 after sorting the data based on Russell's float-adjusted portfolio weights.[15] The regression code is the same as before, except that the *Rank* variable is defined differently.

```
gen rank=float_rank

gen rank1=(rank-1000)
gen rank2=(rank-1000)^2
gen rank3=(rank-1000)^3

gen band=250

reg passive R2000 rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-band,
cluster(stock_id)
```

This modified sharp RD, however, is problematic in a couple of ways. First, it no longer controls for the critical determinant of index assignment, end-of-May total market caps, thus exposing the estimation to potential omitted variable biases when total market cap is also a determinant of the outcome *Y*. While the estimation does control for float-adjusted market caps, these are not the market caps used to determine index assignment, *R2000*, and can deviate from total market caps considerably.

Second, constructing *Rank* in this way ensures that other variables will no longer be continuous at the threshold between *Rank*=1,000 and *Rank*=1,001, which violates the fundamental identification assumption of regression discontinuity (Angrist and Pischke (2009), Lee and Lemieux (2010), Roberts and

---

[15] Typically, researchers will use what they call the "June float-adjusted market cap," which is the float-adjusted market cap on the last day of June, to construct these within-index rankings. The FTSE/Russell "Russell – Monthly Index Holdings US" data, available on Wharton's Research Data Services (WRDS), reports this market cap as the "Ending Market Value." This variable description is a common source of confusion for users of the WRDS data. The FTSE/Russell data do <u>not</u> include the proprietary total market cap used by Russell to determine index assignments; it only reports the float-adjusted market cap. Moreover, WRDS sources its data (as of March 2020) from Russell's monthly files which report index assignments, portfolio weights, and float-adjusted market caps as of the last trading day each month. However, this trading day in June need not be the first day of the newly reconstituted indexes. For example, the first trading day of the newly reconstituted indexes in 2012 was Monday, June 25th, and the first trading day of the newly reconstituted indexes in 2013 was Monday, July 1. Therefore, a researcher that seeks to know the new index assignments in 2013 must look at the July data, not the June data. In our analysis here, we use the *daily* Russell files, which are available to Wharton faculty and allow one to get the float-adjusted market caps on the first day of the new reconstitution year. The daily files also seem to be of higher quality than the monthly files.

Whited (2013)). In particular, there will be a discontinuity in stocks' float-adjusted market cap because Russell resorts stocks within each index using their float-adjusted market cap. Stocks at the bottom of the Russell 1000 will have a much smaller float-adjusted market cap than stocks at the top of the Russell 2000. Figure 6, which plots the average *ln*(float-adjusted market cap) from the first trading day of the new reconstitution year as a function of this new version of *Rank,* shows this discontinuity. On average, the stock with a ranking of 1,000 (i.e., the bottom firm in the Russell 1000) has a float-adjusted market cap that is three log points smaller than the stock with a ranking of 1,001 (i.e., the top firm in the Russell 2000).[16] The simulated data show a similar discontinuity (Figure 6, bottom panel).

[Figure 6 here]

This difference in Russell's float-adjusted market cap between firms at the bottom of the Russell 1000 and the top of the Russell 2000 will cause this modified sharp RD methodology to generate spurious findings. In particular, because firms' float-adjusted market cap correlates with liquidity, this sharp RD compares the least liquid stocks of the Russell 1000 index against the most liquid stocks of the Russell 2000. As one might expect, these two sets of stocks will differ in other dimensions for reasons that have nothing to do with index assignment or ownership structure. Moreover, stocks with a smaller float-adjusted market cap relative to their total market cap (i.e., stocks at the bottom of the Russell 1000)), are also stocks where insiders and non-financial companies hold a larger proportion of the firm's equity. Again, this difference will confound any comparisons using this sharp RD estimation.

Table 3, which reports estimates from the modified sharp RD using the simulated data, illustrates this problem. Because our data-generating process assumes institutions endogenously hold smaller ownership stakes in stocks with larger float-adjustments, there is a discontinuity in institutional ownership at the float-adjusted ranking of 1000 (see Figure 4, middle panel) that the modified sharp RD estimation incorrectly attributes as an effect of index assignment. In particular, the estimation incorrectly finds that index assignment causes a 13 to 31 percentage point increase in total institutional ownership (Table 3, bottom panel). The estimation also uncovers an effect on passive ownership that is about 0.6 to 1.5 percentage points too large for the same reason (Table 3, top panel).

[Table 3 here]

The biased estimates obtained in the simulated data are similar in magnitude to those reported in papers using this estimation as a source of variation in institutional ownership (e.g., Boone and White, 2015; Khan, Srinivasan, and Tan, 2017). However, the simulation estimates (Table 3) and the lack of any such jump in institutional ownership when sorting on CRSP market caps (see Figure 1) illustrate that it is the underlying difference in float-adjusted market cap around this threshold, and not index assignment, that

[16] Some of the observed discontinuity in the float-adjusted market cap might also reflect the impact of index assignment on June stock prices, as documented in Chang, Hong, and Liskovich (2015).

matters. Less liquid stocks with high inside ownership are stocks that institutions endogenously avoid, and this endogenous choice conflates the modified sharp RD estimates.

Earlier papers, including Chang, Hong, and Liskovich (2015, Section 1.5), Mullins (2014, footnote #9), AGK (2016, last paragraph of Section 3.2), and AGK (2019, Section 3.2) also highlighted the problems with this estimation approach. More recent papers (e.g., see Wei and Young, 2019; Glossner 2018) restate these same problems. Despite these warnings, numerous papers continue to use this flawed estimation strategy. See Appendix Table 1 for a listing of papers using this problematic strategy.

The same underlying problem also applies to papers that use the sharp RD with Russell's float-adjusted rankings as the first stage in an IV estimation (e.g., for total institutional ownership). In particular, these papers use Equation (1) as the first stage of their IV estimation, where *Rank* is defined using the problematic float-adjusted <u>within-index</u> rankings, and the threshold between *Rank* = 1,000 and *Rank* = 1,001 is used to instrument for *Y*. The instrument, however, violates the exclusion restriction because it based on threshold variation in ownership that is driven by endogenous differences in liquidity and inside ownership caused by the within-index resorting of stocks. These papers are also listed in Appendix Table 1 under "sharp regression discontinuity using Russell's observable June float-adjusted rankings."

3.2.3. Sharp regression discontinuity using observable within-index, end-of-May rankings

The modified sharp RD estimation of Crane, Michenaud, and Weston (2016) is also problematic for similar reasons. Crane, Michenaud, and Weston use the above sharp RD as the first stage of an IV estimation. However, they instead use end-of-May market caps, as calculated from CRSP, to calculate *Rank* <u>after</u> sorting stocks into the two indexes. Specifically, the regression code for the first stage of their IV is the same as before, except that the *Rank* variable is defined differently.

```
gsort year +index -crsp_mc
by year: gen rank=_n

gen rank1=(rank-1000)
gen rank2=(rank-1000)^2
gen rank3=(rank-1000)^3

gen band=250

reg passive R2000 rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-band,
cluster(stock_id)
```

While this approach to constructing *Rank* avoids using the problematic float-adjusted portfolio weights of Russell and avoids a spurious jump in institutional ownership (as illustrated in Figure 7), it instead leads to a mechanical discontinuity in end-of-May CRSP market caps. This discontinuity occurs because end-of-May CRSP market caps do not perfectly predict index assignments. The Russell 1000 includes many stocks with end-of-May CRSP market caps that are smaller than the end-of-May CRSP market caps of some stocks in the Russell 2000. Therefore, a sorting of stocks within an index based on end-of-May CRSP market caps creates a discontinuity in end-of-May CRSP market caps at the threshold. The top panel of Figure 8, where we plot the average end-of-May CRSP market caps as a function of ranks constructed using the approach of Crane, Michenaud, and Weston (2016), illustrates this problem. As can be seen, there is a jump in end-of-May CRSP market caps at the threshold of about three log points. A similar pattern occurs in the simulated data, as shown in the bottom panel of Figure 8.

[Figures 7 and 8 here]

Crane, Michenaud, and Weston (2016, p. 1386) acknowledge this jump in market cap at the threshold they use for identification, but argue it is not problematic for their estimation. We disagree. While the switch in the variable used to construct the within-index rankings does eliminate the spurious jump in institutional ownership at the index threshold (illustrated in Figure 7), the remaining jump in market caps is problematic (see Figure 8). If end-of-May CRSP market caps correlate with the outcome of interest (payout policy in their case), then the threshold jump in end-of-May CRSP market caps invalidates their RD identification strategy. Specifically, differences in end-of-May CRSP market cap at the threshold, rather than index assignment, could drive the estimates from this modified sharp RD.

Furthermore, the large 6 to 9 percentage discontinuity in institutional ownership that Crane, Michenaud, and Weston (2016) detect using their approach confirms that this mechanical discontinuity in CRSP market cap is problematic. Estimation strategies that do not suffer from this potential bias, which we discuss in the next two subsections, detect no effect of index assignment on institutional ownership. Moreover, the absence of a discontinuity in institutional ownership when using their definition of *Rank* to sort stocks (see top panel of Figure 7) immediately raises questions on how their RD estimation detects an increase in institutional ownership and suggests a deeper problem with their approach.[17] A list of papers that employ this estimation strategy is provided in Appendix Table 1.

3.2.4. Fuzzy regression discontinuity

Because one cannot estimate the ideal sharp RD discussed in Section 3.2.1, some propose switching to a fuzzy regression discontinuity (e.g., Mullins, 2014; Wei and Young, 2019). The key idea is to sort the data using the observable market caps, which are a noisy proxy for the unobserved Russell market caps, and test for a discontinuity. For example, one could use market caps as reported by CRSP. So long as this

[17] Crane, Michenaud, and Weston (2016) do not report a version of Figure 7 to support their sharp RD estimation. Instead, they report a version of Figure 3, which instead sorts using the float-adjusted rankings (see Figure 2 of their paper). Moreover, in unreported tests, we find that likely culprit for why their estimation detects a discontinuity in instititutional ownership is their inclusion of an additional control, *FloatAdj*, in their sharp RD estimation. They define *FloatAdj* as the difference between their version of *Rank* and the *Rank* one finds using the float-adjusted market caps. Without this control, we find their estimation correctly detects no discontinuity in institutional ownership, but with this control, it sporadically detects a large discontinuity in institutional ownership similar to what they report.

sorting on the CRSP market cap causes a discontinuity in the *probability* of being assigned to the Russell 2000 when moving from an end-of-May CRSP market cap ranking of 1,000 to 1,001, one can still recover the effect of index assignment on the outcome *Y*. Intuitively, the fuzzy RD tests for a discontinuity in outcome *Y* at this new threshold and rescales it by the observed jump in probability of treatment (i.e., being assigned to the Russell 2000) to recover the effect of being assigned to the Russell 2000. An IV estimation, which was unnecessary in the sharp RD estimation because the likelihood of treatment (i.e., being in the Russell 2000) moved from zero to one, accomplishes this rescaling.[18]

To estimate the fuzzy RD, the specification uses *Treatment* as an instrument for *R2000*, where *Treatment* is an indicator that equals one for firms with a *Rank* greater than 1,000, and *Rank* is now defined using end-of-May CRSP market capitalization. Specifically, the first stage of the estimation is

$$R2000_{it} = \beta + \lambda Treatment_{it} + \sum_{n=1}^{N} \varphi_n (Rank_{it} - 1000)^n + \mu_{it}, \tag{3}$$

and the second stage of the instrumental variable estimation is

$$Y_{it} = \alpha + \chi \widehat{R2000}_{it} + \sum_{n=1}^{N} \phi_n (Rank_{it} - 1000)^n + \varepsilon_{it}, \tag{4}$$

where $\widehat{R2000}$ is the fitted value of *R2000* from the first-stage estimation.[19] Similar to the sharp RD, the fuzzy RD controls for the key determinant of index assignment, end-of-May total market caps, by restricting the sample to observations close to the cutoff threshold of *Rank* = 1000 and controlling for CRSP's end-of-May market cap ranking using a flexible functional form. The key difference is that the fuzzy RD controls for the key determinant of index assignment using an observable, but noisy, proxy for Russell's market cap. A list of Russell papers that employ the fuzzy RD estimation strategy is provided in Appendix Table 1.

To estimate the two stages of the fuzzy RD estimation in the simulated data using a bandwidth of 250 and polynomial order *N*=3, we use the following code:

```
gen rank=crsp_rank

gen treatment = 0
replace treatment = 1 if rank > 1000 & rank !=.
```

[18] For example, suppose Russell 2000 inclusion causes a two-percentage-point increase in passive ownership but having a CRSP market cap ranking greater than 1,000 only increases the probability of being assigned to the Russell 2000 by 0.2. In this case, one would only detect a 2×0.2=0.4 percentage point jump in passive ownership at the CRSP rank = 1000 threshold when using a sharp RD estimation that sorts using the CRSP market cap. To recover the causal effect of index assignment on passive ownership, the fuzzy RD estimation takes the observed jump in passive ownership (0.4 percentage points) [i.e., the reduced form estimate] and divides it by the observed jump in the probability of being assigned to the Russell 2000 (0.2) [i.e., the first stage estimate].

[19] Similar to the sharp RD, one could include an additional set of controls *Treatment*$\times \sum_{n=1}^{N} (Rank_{it} - 1000)^n$ in the first stage of the fuzzy RD estimation to allow the functional form of the relationship between *Rank* and outcome *Y* to vary across the two indexes. In the second stage of the fuzzy RD, one would then include $\widehat{R2000} \times \sum_{n=1}^{N} (Rank_{it} - 1000)^n$ as additional controls. In other words, the instrumental variable estimation remains just-identified, and each term of *Treatment*$\times \sum_{n=1}^{N} (Rank_{it} - 1000)^n$ instruments for the corresponding term of $R2000 \times \sum_{n=1}^{N} (Rank_{it} - 1000)^n$. See Angrist and Pischke (2009) for more details.

```
gen rank1=(rank-1000)
gen rank2=(rank-1000)^2
gen rank3=(rank-1000)^3

gen band=250

reg R2000 treatment rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-band,
cluster(stock_id)

ivregress 2sls passive (R2000 = treatment) rank1 rank2 rank3 if (rank-1000)<=band
& (rank-1000)>-band, cluster(stock_id)
```

A problem with using the fuzzy RD, however, is that there is no jump in the probability of assignment to the Russell 2000 when moving from an end-of-May CRSP market cap ranking of 1,000 to 1,001. The top panels of Figures 1 and 2, where we plot the proportion of stocks in the Russell 2000 as a function of end-of-May CRSP market cap ranks using the actual and simulated data, respectively, show the absence of a discontinuity. The lack of a discontinuity at the *Rank*=1000 threshold is more readily apparent when one narrows the bandwidth to 50 stocks (see Figure 9).

[Figure 9 here]

Absent a significant jump in the probability of index assignment, the first stage of the fuzzy RD estimation can fail, and the IV estimation will not uncover the actual effect of index assignment on passive ownership. Table 4, where we estimate both the first and second stages of the fuzzy RD using the simulated data over various bandwidths, illustrates this problem. The first stage of the estimation correctly detects no jump in assignments to the Russell 2000 in the smaller bandwidths, and the IV estimation fails to recover the correct effect on passive ownership (e.g., Table 4, Columns 1-3). The fuzzy RD, however, does perform better when using wider bandwidths (e.g., Columns 7-12).

[Table 4 here]

The estimates provided in Table 5 confirm that the CRSP rankings often fail to predict index assignment in the actual data. In Table 5, we estimate the above first stage of the fuzzy RD estimation, but following Wei and Young (2019), we include the additional controls *Treatment*$\times \sum_{n=1}^{N}(Rank_{it} - 1000)^n$ that they recommend. As shown in Table 5, we fail to find robust evidence of an increase in the likelihood of being in the Russell 2000 for stocks with an end-of-May CRSP market cap ranking above 1,000 when using the 50-stock bandwidth [see columns (1) to (3)] or when using a broader bandwidth of 100 stocks [see columns (4) to (6)]. We actually find a negative and statistically significant coefficient in some cases, which is the opposite of what we should find because *Treatment* should positively predict *R2000*. While the predictive ability of the instrument, *Treatment*, does increase when one uses an even broader bandwidth, like 250 stocks, the magnitude of the coefficient can vary significantly depending on the polynomial order, *N,* one uses as a control for the forcing variable, *Rank* [see columns (7) to (12)]. The sensitivity to the polynomial order choice reflects the lack of a real discontinuity near the threshold, as shown in the top panel of Figures 1 and 9, which use 250 and 50 bandwidths, respectively.

[Table 5 here]

While the predictive power of end-of-May CRSP market caps is better further from this threshold, this is not necessarily helpful in that fuzzy RD estimations rely on a discontinuity in probability of treatment at the threshold, not at points further away from the threshold (Angrist and Pischke (2009), Lee and Lemieux (2010), Roberts and Whited (2013)). Absent such a discontinuity, the fuzzy RD estimation can suffer from a weak instrument problem and hard-to-predict biases.

The lack of a reliable first stage when using end-of-May CRSP market caps (and the associated problems) likely explains the absence of papers using this estimation strategy in the Russell setting.

More recent papers, however, provide evidence that constructing end-of-May market caps using a combination of pricing data from CRSP and the number of shares data from Compustat can improve the first stage (e.g., see Wei and Young 2019; Glossner 2018). For example, using broad bandwidths, Wei and Young (2019) report evidence of a first stage that is not sensitive to the polynomial order used to control for the forcing variable (e.g., see the two left panels of Figure 5 in Wei and Young (2019)). These findings suggest the Russell 1000/2000 setting is amenable to using a fuzzy RD in wider bandwidths when using a combination of CRSP and Compustat to calculate the end-of-May market cap rankings.[20,21]

However, at smaller bandwidths, the robustness of the fuzzy RD remains questionable, even using this alternative way to calculate end-of-May market cap rankings. The right two panels of Figure 5 in Wei and Young (2019) illustrate the potential weakness of the first stage when using narrower bandwidths. In both cases (especially in the bottom right panel), one can see that the probability of being in the Russell 2000 is converging to 50-50 as you near the threshold. While only controlling for a linear trend on either side of the threshold might suggest a discontinuity, the use of a second- or third-order polynomial, which would be better able to control for the nonlinear index assignment pattern shown in their Figure 5, would likely not detect a discontinuity in the smaller bandwidths. Consistent with this possibility, Wei and Young note that the Kleibergen and Paap (2016) F-stat for the first stage in the 100 bandwidth is only 2.962 when introducing a third-order polynomial. Given this, it is probably not advisable to rely solely on a fuzzy RD estimation in the Russell 1000/2000 setting when using only observations very near the threshold.

Putting aside the potential need to use wider bandwidths, which also is a limitation of the AGK (2016) IV estimation discussed in the next section, there are two additional tradeoffs one must consider when using the fuzzy RD in this setting. The first tradeoff is that it is not possible to use it for sample years

[20] The methodology for calculating end-of-May market caps proposed by Ben-David, Franzoni, and Moussawi (2019) appears to offer yet another improvement in the first stage of the fuzzy RD estimation.

[21] However, it might still be necessary to account for potential biases that can occur when one observes the assignment variable in the RD estimation with error, as is the case in the Russell 1000/2000 setting. See Pei and Shen (2017) for more details on why the fuzzy RD estimates might still be biased because of this remaining measurement error and the approaches one can use to recover the unbiased RD coefficient.

after 2006 when Russell changed its index assignment methodology. With the implementation of banding in 2007, there is no longer a discontinuity in index assignment between the 1,000th and 1,001st end-of-May market cap rankings. Instead, index assignment now depends on a combination of factors, including last year's index assignment and the distance of the stock from the 1,000/1,001 ranking cutoff. Section 4 discusses methodologies designed to overcome this post-banding challenge.

A second tradeoff of the fuzzy RD estimation is that it does not provide a direct way to quantify the importance of firms' ownership structure on other outcomes, which is a critical motivating factor for using the Russell 1000/2000 setting for identification purposes. The first stage of the fuzzy RD estimation provides an instrument for index inclusion, not ownership structure. Therefore, when one uses fuzzy RD with index inclusion as the outcome of the first stage, one is estimating the impact of index inclusion, not ownership structure, on outcomes in the second stage of the IV estimation. However, one can implicitly quantify the impact of ownership structure on some outcome of interest, *Y*, by separately estimating the fuzzy RD for ownership structure and *Y* and then taking the ratio of the two coefficients. For example, suppose the fuzzy RD finds a two-percentage point increase in passive ownership and a five-percentage point increase in the share of a firm's independent directors. Using the standard exclusion assumption of an IV estimation (i.e., the only reason index inclusion matters for board independence is through its effect on passive ownership), one can infer that a one percentage point increase in passive ownership leads to a 5/2 = 2.5 percentage point increase in the share of independent directors.

If one were interested in more directly studying the effect of ownership on other outcomes, one would instead need to use ownership as the outcome variable in the first stage. Doing this, however, means one is no longer estimating a fuzzy RD estimation but instead estimating an IV regression where one instruments for passive ownership using predicted index inclusion. This approach is very similar to the baseline estimation of AGK (2016), except that they use actual index inclusion as the IV for passive ownership, and they robustly control for *ln*(market cap) rather than market cap rankings.

3.2.5. IV estimation of Appel, Gormley, and Keim (2016)

The IV estimation of AGK (2016) overcomes the shortcomings of the previous methodologies. In particular, to avoid the weak IV problem of fuzzy RD, AGK uses index assignment, *R2000*, as their instrument, rather than *Treatment*. They, however, continue to robustly control for the critical determinant of index assignment, end-of-May total market cap, similar to what one would do in an RD estimation, by restricting the sample to firms of similar market cap and including a robust set of controls for total CRSP market cap. In contrast to the empirical strategies above, however, their IV approach does not attempt to directly exploit variation in ownership or index assignment at the threshold between the Russell 1000 and 2000 indexes. Additionally, they also include a control for float-adjusted market cap to account for its importance for institutional ownership and its possible correlation with their instrument, *R2000*.

To implement this IV strategy and use Russell 2000 inclusion as a source of exogenous variation in passive institutional ownership, the AGK (2016) first stage estimation is

$$Passive\%_{it} = \eta + \lambda R2000_{it} + \sum_{n=1}^{N} \chi_n (Ln(Mktcap_{it})^n + \sigma Ln(Float_{it}) + \delta_t + u_{it}. \quad (5)$$

and the second stage of the instrumental variable estimation is

$$Y_{it} = \alpha + \beta \widehat{Passive}\%_{it} + \sum_{n=1}^{N} \theta_n (Ln(Mktcap_{it})^n + \gamma Ln(Float_{it}) + \delta_t + \varepsilon_{it} , \quad (6)$$

where Y_{it} is the outcome of interest for firm *i* in reconstitution year *t*; $Passive\%_{it}$ is the percent of a firm's shares held by passively managed mutual funds and ETFs at the end of the first quarter of the reconstitution year *t*; $\widehat{Passive}\%_{it}$ is the fitted value of $Passive\%_{it}$ from the first stage of the IV estimation; $Mktcap_{it}$ is the end-of-May CRSP market capitalization of stock *i* in year *t*; and $Float_{it}$ is the float-adjusted market capitalization calculated by Russell that determines index weights.[22]

The identifying assumption for the AGK (2016) framework is that after conditioning on stocks' end-of-May CRSP market cap, inclusion in the Russell 2000 is associated with an increase in *Passive%* (relevance condition) but does not directly affect their outcomes of interest except through its impact on ownership by passive investors (exclusion assumption). AGK (2016) tests the relevance condition in Tables 2 and 3 of their paper. They find that assignment to the Russell 2000 is associated with a 1.1 percentage point increase in passive mutual fund and ETF ownership between 1998 and 2006 (*p*-value < 1%), an increase of about 0.5 standard deviations in passive ownership (Table 2, Column 2 of their paper). They do not find an effect of index assignment on total institutional ownership (Table 11, Column 1) or active mutual fund ownership (Table 2, Column 3), suggesting that one cannot use the Russell IV as a means to study the effects of total institutional ownership or active mutual fund ownership on outcomes of interest.

As with any IV estimation, the validity of AGK's estimation hinges on the exclusion assumption. AGK (2016) argues the exclusion assumption is plausible in this setting because it is unclear why index inclusion would otherwise affect their outcomes of interest after robustly controlling for firms' end-of-May market capitalization (as calculated using either CRSP, Compustat, or Russell's noisy measure they obtained). Moreover, as an additional non-parametric control for size, their baseline estimation restricts their sample to the 250 stocks at the bottom of the Russell 1000 and top 250 stocks of the Russell 2000. In later estimates, they also show the robustness of their IV point estimates to varying the number of stocks they include from each index from anywhere between 100 and 500 (see Appendix Figure 1 of that paper).

[22] The baseline specification of AGK (2016) uses end-of-May market caps, as calculated by CRSP. However, in Appendix Tables A.4 and A.5, they show that their findings are not sensitive to instead calculating end-of-May market caps either using Compustat or using the end-of-May market caps provided to them by Russell for the years 2002 through 2006. AGK (2016) calculates the float-adjusted market cap using the last trading day of June, as obtained from the monthly files provided by Russell Investments. AGK (2019) refines this approach by using float-adjusted market caps from the first day of the reconstitution year, as obtained from the daily files provided by Russell.

One concern with the baseline approach of AGK (2016) is their use of Russell within-index rankings when selecting their sample. Because of how Russell ranks stocks within each index (i.e., shifting stocks with the lowest float-adjusted market cap toward the bottom of each index), AGK's sample selection might result in a correlation between their instrument, *R2000*, and the float-adjusted market cap of stocks. This correlation, if not accounted for, could result in a bias similar to the one discussed in Section 3.2.2.

They mitigate this concern in three ways. First, their baseline IV specification always includes a control for firms' float-adjusted market capitalization, as provided by Russell. In other words, they directly control for the variable that might differ across the two indexes because of how they select the sample. Second, they select their sample using wider bandwidths of 250 or more stocks, where this potentially problematic difference in float-adjusted market caps across indexes is likely smaller than in the narrower bandwidths. Third, they show that their findings are similar if they instead choose their sample using only end-of-May CRSP market cap rankings (see Section 7.3 and Appendix Table A.9), similar to the fuzzy RD estimation of Section 3.2.4. This last modification instead uses the following code:

```
gen mc=crsp_mc
gen rank = crsp_rank

gen size1 = log(mc)
gen size2 = size1^2
gen size3 = size1^3
gen float1 = ln(float_mc)

gen band=250

xi: reg passive R2000 float1 size1 size2 size3 i.year if (rank-1000)<=band &
(rank-1000)>-band, cluster(stock_id)
```

Tables 6 and 7, where we estimate the first stage of the AGK estimator over a range of bandwidths and polynomial orders, *N* using the simulated data, illustrates the importance of these choices.

[Tables 6 and 7 here]

Using the AGK (2016) specification and their first method for selecting the sample, one correctly recovers the effect (non-effect) of index assignment on passive (total institutional) ownership, but only for the broader bandwidths. In the 250 and 500 stock bandwidths, the first stage AGK estimates detect a one percentage point increase in passive ownership and no impact on total institutional ownership (Table 6, Columns 7-12). By avoiding the use of the noisy IV *Treatment*, the AGK estimates for passive ownership are also more precise than those obtained in a fuzzy RD estimation (see the bottom panel of Table 4, Column 7-12). However, similar to the fuzzy RD, the performance of the baseline AGK estimator is less compelling in smaller bandwidths. In those bandwidths, it incorrectly reports a positive impact of index assignment on institutional ownership and an impact on passive ownership that is too large (Table 6, Columns 1-6).

While both the fuzzy RD and baseline AGK estimation exhibit weaker performance in smaller bandwidths, the reason for this is different. Fuzzy RD can fail in smaller bandwidths because of a weak first stage of the IV estimation, but with the baseline AGK estimation, the weakness occurs because of how the sample is selected. When using float-adjusted ranks to select the sample, the instrument, *R2000*, can be correlated with float-adjusted market caps, leading to violation of the exclusion restriction. The risk of this is greater in smaller sample bandwidths because there will be larger differences in the average float adjustment of stocks across the two indexes, and the included Ln(*Float*) control can be insufficient to control for these differences, as shown in Table 6, Columns 1-6. While including polynomials of the float-adjusted market cap can better control for these differences, it might not eliminate the problem.

This weak performance of AGK's estimator in smaller bandwidth, however, disappears when one uses AGK's alternative sample selection technique. Table 7, where we instead select the sample based on CRSP end-of-May market cap rankings, shows this improvement. The AGK estimator now correctly recovers the impact of index assignment across all bandwidths. The change in coefficients between Tables 6 and 7 highlight a potential bias with AGK's first sampling technique.

The sampling choice boils down to the classic tradeoff between noise and potential bias. An advantage of using end-of-May CRSP market cap rankings in selecting the sample is that it eliminates the risk of estimation bias coming from Russell's float-adjusted reweighting of stocks. A disadvantage of this alternative sampling approach is that the IV estimation is noisier and can isolate a smaller shift in passive ownership when index assignment does not uniformly affect passive ownership as assumed in the simulated data. Because passive investors focus on minimizing tracking error by closely tracking the stocks with larger portfolio weights, the expected difference in passive ownership occurring because of Russell index assignments, in practice, should be more significant when comparing stocks at the bottom of the R1000 to stocks at the top of the R2000. If true, then using end-of-May CRSP market caps to select the sample is going to yield a smaller and weaker first stage because it is no longer compares stocks with the most substantial differences in portfolio weights.[23] AGK (2016) discusses this tradeoff in Section 7.3. They note that their first stage point estimate for passive ownership falls, as expected, by 25.2% when using this alternative approach (compare their coefficient in Table 3, Column 2 to the reported point estimate in Section 7.3). In AGK (2019), the first stage estimate falls by about 31.1% (compare their coefficient in

[23] This tradeoff is not visible in the simulated data because it assumes a uniform increase in passive ownership for all Russell 2000 stocks. With small changes to this assumption, however, the tradeoff becomes more apparent. For example, suppose we instead assume that the one percentage point increase in passive ownership only occurs for the top 250 stocks of the Russell 2000. Specifically, we use "`replace passive=passive+.01 if index==2 & float_rank <= 1250`" to simulate the effect of index assignment instead of "`replace passive=passive+.01 if index==2`", as currently done. With this alternative data structure, the AGK estimator still correctly detects a one percentage point increase in passive ownership when using the 250 bandwidth and selecting the sample using float-adjusted ranks. Selecting the sample using CRSP market cap rankings, however, recovers a 15 to 20 percent smaller increase in passive ownership and the standard errors of the estimation are larger by about 25 to 40 percent.

Table 3, Column 2 to that first stage point estimate of 0.83 reported in Section 6.1).[24]

Given the inherent tradeoff of the two sampling techniques, there is not a clear winner between the baseline approach in AGK (2016) and the alternative one they propose. However, given the potential for bias in the baseline approach of AGK (2016), it is advisable to run the estimation using both sampling techniques to ensure each approach yields similar IV estimates. Should the IV estimates remain mostly unchanged using the alternative sampling approach, this would give confidence that the less noisy estimates obtained using the baseline approach of AGK (2016) are unbiased.

Comparing the two sampling approaches, AGK (2016) provides evidence that the choice seems to have almost no impact on their IV point estimates. The lack of an impact suggests that the potential bias from choosing a sample using within-index weights is negligible in the actual data and for the outcomes they study.[25] IV coefficients are the reduced form coefficient (i.e., where you regress the outcome of interest directly on to the instrument) scaled by the coefficient from the first stage. Because the IV coefficients do not change in a meaningful way when AGK (2016) uses the alternative sampling approach despite the change in first stage estimates, the reduced form coefficients must be changing by the same proportion. This proportional change is intuitive. If the instrument isolates an increase in passive ownership that is 50% less, then assuming the original IV estimates were unbiased, the outcomes affected by passive ownership in the alternative sample will also change by 50% less, resulting in no change in the IV estimates.

Similar to the above sharp RD, the AGK estimator can also be modified to make use of only stocks that switch indexes. One accomplishes this by adding stock-level fixed effects to the estimation, thus converting it from a pooled, cross-sectional estimation to a panel estimation. By adding such fixed effects, the estimation uses only within-stock variation in index assignment, *R2000*, after conditioning on within-stock variation in the control variables, which includes *ln*(*Mktcap*).

3.2.6. A warning about estimation strategies that rely on index switchers

While the above estimation strategies, including AGK (2016), can be modified to make use of only index switchers, in practice, this estimation strategy will often be less useful. First, the estimation is noisier because it relies on a much smaller set of firms for identification. Most stocks remain in the same Russell index from one year to the next, resulting in relatively few stocks with changes in index assignment. Second,

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[24] Making a similar comparison, Glossner (2018) finds about a 33.6% to 56% drop in the first stage estimate for passive ownership when using the alternative sample (compare panels B and D of Table 6 in that paper). However, because Glossner (2018) changes both the sample selection method and market cap construction, it is hard to know how much of the drop comes from the sample selection change.

[25] To see the similarity of the IV point estimates under the alternative sampling choices, compare the coefficients reported in the AGK (2016) Appendix Table A.9 [which only used end-of-May market cap rankings to select the sample] to the corresponding estimates using their baseline estimation [which selects the sample using the actual top and bottom stocks of the Russell 2000 and 1000, respectively], as reported in Table 4 (Column 3), Table 6 (Columns 3 and 6), Table 7 (Column 3), Table 8 (Columns 3 and 6), Table 9 (Column 3), and Table 10 (Column 6).

this identification strategy might not capture the relevant variation. Specifically, this estimation relies on more transitory changes in ownership structure, which may not be the type of variation in ownership that is important for driving corporate outcomes. For example, consider a stock that drops into the Russell 2000 in one year and experiences an increase in passive ownership, but then jumps back up to the Russell 1000 the very next year and experiences a reversal in passive ownership. Should we expect that the one-year change in ownership for that stock to have a meaningful impact on persistent firm-level outcomes, like corporate governance? Instead, the relevant variation might be the sustained differences in ownership and index assignment that occurs among stocks that do not switch indexes. This type of problem is similar to the concern about fixed effects raised in McKinnish (2008) and Gormley and Matsa (2014, Section 4.2).

One must also be careful when attempting to exploit variation from index switchers using strategies that differ from those discussed above. For example, in an attempt to study the effects of passive ownership on corporate governance, Heath, Macciocchi, Michaely, and Ringengberg (2020) uses a difference-in-differences type estimation that compares the post-switch change in outcomes of switchers vs. non-switchers. However, by failing to control for the changes in end-of-May CRSP market caps that led one stock to switch but not the other, the estimation ignores that switchers and non-switchers are inherently different at the time of the switch. In essence, their difference-in-differences estimation likely suffers from an omitted variable bias because it fails to control for the critical determinant of index switches. A similar concern applies to Coles, Heath, and Ringgenberg (2020).

Schmidt and Fahlenbrach (2017) provides a subtler example of a problematic switcher estimation. In the first stage of their IV estimation, the authors are careful to control for changes in end-of-May market cap rankings (see Equation (2) in their paper). However, the second stage of the IV estimation does not include this control (see Equation (1) in their paper). Thus, their estimation uses the change in ranks over the last year as an instrument, not as a control, which they explicitly acknowledge on page 292 of their paper. Using the change in market cap as an IV is inappropriate because it may directly drive many outcomes of interest or correlate with other factors that drive changes in ownership and other outcomes of interest. In other words, one would be hard-pressed to argue that changes in market cap rankings are a valid instrument of ownership, as this estimation strategy assumes.[26]

4. Post-2006 specification choices when using the Russell 1000/2000 threshold for identification

Because of the growing importance of index funds and ETFs for US stock ownership, empirical researchers are increasingly interested in using the Russell index assignments as a source of exogenous variation for passive institutional ownership. Passively managed funds' share of mutual fund assets and

[26] Another potential concern with the specification of Schmidt and Fahlenbrach (2017) is that it does not attempt to robustly control for changes in market cap by testing the robustness of the findings to using a second- or third-order polynomial set of controls like other papers in this literature.

the average share of a US company's outstanding equity held in passively managed mutual funds and ETFs have both more than tripled since 2006. These trends raise important questions on how this shift in ownership might affect the functioning of financial markets and corporate policies.

Unfortunately, Russell's implementation of a banding policy beginning with the 2007 index reconstitution makes all of the above estimation techniques unsuitable for isolating exogenous variation in passive ownership after 2006. Following this change in the methodology, index assignment was no longer just a function of Russell's proprietary end-of-May market capitalization. Instead, index assignment became a function of (1) past index assignment (2) Russell's end-of-May market capitalization rank, and (3) whether the firm's end-of-May Russell market capitalization falls within a specific range of the 1,000th largest firm. Because of this change, empirical methodologies used for the Russell setting before 2007 will not work for later years. For example, this policy change eliminates any potential discontinuity in index assignment at the cutoff between the 1,000th and 1,001st largest firms, precluding the use of a fuzzy RD as proposed by Wei and Young (2019). The regression specification used by AGK (2016) is also inappropriate because it does not control for the additional factors that contribute to index assignment after 2006, which could cause an omitted variable bias if these factors correlate with the outcome of interest.

The implementation of banding also causes additional systematic differences in the type of stocks at the bottom of the Russell 1000 versus stocks at the top of the Russell 2000. In particular, stocks in the Russell 1000 with negative changes in end-of-May market cap rankings from year $t-1$ to year t are more likely to remain in the Russell 1000 because banding prevents them from moving down to the Russell 2000. On the other hand, stocks in the Russell 2000 with positive changes in end-of-May market cap rankings are more likely to remain in the Russell 2000 because banding prevents them from moving to the Russell 1000.

Table 8, where we tabulate the average change in end-of-May CRSP market cap rankings in the year before reconstitution, shows these differences. We find no difference in the average change in CRSP market cap rankings across the two indexes in the pre-banding years but see significant differences post-banding. In pre-banding years, the average change in end-of-May CRSP market cap rankings was -90.6 for the bottom 250 stocks of the Russell 1000 and -106.2 for the top 250 stocks of the Russell 2000, and the difference 15.6 is not statistically significant at conventional levels (p-value = 0.145). In post-banding years, the average stock at the bottom of the Russell 1000 experienced a change in rankings of 47.3 (i.e., they became smaller relative to other stocks). The average stock at the top of the Russell 2000, however, experienced a change of rankings of -134.1 (i.e., they moved up in terms of their end-of-May CRSP market cap ranking). The post-banding difference in past market cap movements across the two Russell indexes is statistically significant at the 1% level.

[Table 8 here]

Failure to control for these post-banding differences in the past market cap changes of stocks at the top of the Russell 2000 and bottom of the Russell 1000 could result in misleading inferences. For example, stocks that increase their relative market cap ranking are potentially companies exhibiting greater sales and profitability growth than the average firm. If true, then estimations that compare stocks across the two indexes while failing to control for this difference in past growth could incorrectly attribute observed differences in outcomes to passive ownership rather than the underlying differences in growth.

AGK (2019) proposes a modification of AGK (2016) that allows researchers to continue using a stock's Russell index assignment as a source of exogenous variation in passive ownership in years after 2006. Specifically, they continue to use assignment to the Russell 2000 as an instrument for passive mutual fund ownership, but they add three additional controls to account for the new factors that determine index assignment for firm *i* at time *t* starting in 2007. These additional controls are: 1) an indicator for having an end-of-May CRSP market capitalization that ensures firm *i* will be "banded" by Russell and not switch indexes in reconstitution year *t* because the distance between its market cap and the Russell 1000/2000 cutoff is less than 2.5% of the Russell 3000E Index cumulative market cap, $band_{it}$; (2) an indicator for being in the Russell 2000 in the last reconstitution year t–1, $R2000_{it-1}$; and (3) the interaction of these two indicators. These three additional controls capture the additional criteria used by Russell beginning in 2007 when determining each firm's index assignment at the annual end-of-June reconstitution for year *t.*

To implement this IV empirical strategy, the AGK (2019) first stage is

$$Passive\%_{it} = \eta + \lambda R2000_{it} + \sum_{n=1}^{N} \chi_n (Ln(Mktcap_{it})^n + \sigma Ln(Float_{it})$$
$$+\phi_1 band_{it} + \phi_2 R2000_{it-1} + \phi_3(band_{it} \times R2000_{it-1}) + \delta_t + u_{it}, \quad (6)$$

and the second stage of the instrumental variable estimation is

$$Y_{it} = \alpha + \beta \widehat{Passive}\%_{it} + \sum_{n=1}^{N} \theta_n (Ln(Mktcap_{it})^n + \gamma Ln(Float_{it})$$
$$+\mu_1 band_{it} + \mu_2 R2000_{it-1} + \mu_3(band_{it} \times R2000_{it-1}) + \delta_t + \varepsilon_{it}, \quad (7)$$

where $R2000_{it}$ is still the instrument for *Passive%.* Furthermore, researchers interested in only using variation in ownership caused by index switchers can accomplish this by simply adding firm fixed effects to both stages of the above IV estimation. The potential downsides of using such a panel estimation in the Russell setting (see Section 3.2.6), however, still apply in the post-banding period.

5. Discussion of Wei and Young (2019) and Glossner (2018)

Similar to our paper, both Wei and Young (2019) and Glossner (2018) discuss the various estimation techniques used in Russell 1000/2000 settings. A number of the points made in these two papers, especially the inappropriate use of float-adjusted rankings in an RD type estimation, mirror those discussed above and in earlier papers (e.g., Chang, Hong, and Liskovich 2015; Mullins 2014; Appel, Gormley, and

Keim 2016). The warning of these earlier papers, including earlier drafts of this paper, has not been heeded as many subsequent papers (e.g., Bird and Karolyi 2016; Chen, Huang, Li, and Shevlin 2018; Lin, Mao, and Wang 2017; Schmidt and Fahlenbrach 2017; Heath, Macciocchi, Michaely, and Ringengberg 2019) continue to use the Russell 1000/2000 setting in inappropriate ways. Moreover, readers not familiar with the Russell 1000/2000 setting continue to have difficulty determining which approach is correct.

While we mostly agree with many points made in both Wei and Young (2019) and Glossner (2018), we have a couple of disagreements and one clarification.

5.1. A clarification on when to use the Russell 1000/2000 setting for studying ownership

Wei and Young (2019) argues that the Russell 1000/2000 setting is not useful for studying the importance of *total* institutional ownership. We agree. The IV estimations of AGK (2016) also find no effect of index assignment on total institutional ownership.

What Wei and Young (2019) do not speak to, however, is the ability to use the Russell 1000/2000 setting to study the importance of *passive* institutional ownership, as measured using the holdings of ETFs and index mutual funds. This is something their paper never directly tests. While they do analyze "quasi-index" holdings, as classified in Bushee (2001), this is not a test of passive ownership. As noted in Gilje, Gormley, and Levit (2020), the Bushee (2001) classification of "quasi-indexer" is not a meaningful measure of passive ownership; nearly two-thirds of institutions are classified as "quasi-indexers," while some of the biggest indexers, including BlackRock, are not classified as such.

In our view, the setting does provide a significant source of variation in passive institutional ownership. This is shown in AGK (2016), Schmidt and Fahlenbrach (2017), and AGK (2019) using fund-level classifications that more precisely capture passive ownership. Moreover, this is also shown in Glossner (2018) using the fuzzy RD estimation recommended by Wei and Young (2019).

5.2. The modified AGK (2016) IV estimation of Glossner (2018)

Glossner (2018) argues that when using the IV estimation of AGK (2016), one should not use the actual Russell rankings to select the sample but should instead use the end-of-May market cap rankings, as calculated using CRSP, to create the sample. The concern is that using the actual rankings to select the sample may introduce an imbalance in the sample because of the way Russell resorts stocks within indexes. Specifically, AGK's sample of Russell 1000 stocks might have more substantial float adjustments, on average, than their sample of Russell 2000 stocks. This difference in average float adjustments across the two samples might cause bias because the AGK instrument (inclusion in the Russell 2000) would then correlate with float adjustments, which are, in turn, related to a stock's liquidity and inside ownership.

As shown in Table 6, this concern is reasonable. It is the reason why AGK [see AGK (2016, page 120) and Section 3.2.5 above] include the float-adjusted market cap as an additional control in their baseline IV specification. *Moreover*, AGK proposes the same modification to the sample selection process as an alternative way to deal with this concern and do that test in both of their papers [see AGK (2016, Section 7.3 and Appendix Table A.9) and AGK (2019, Section 5.1 and Appendix Table 4)].

As discussed in Section 3.2.5, we agree that researchers should test the robustness of their findings to using the alternative sampling approach. If the two approaches yield different estimates, then one should consider the potential for bias in AGK's baseline sampling approach. However, if the two sampling techniques yield similar point estimates (as in AGK), then one could use this as evidence that the more precisely estimated coefficients obtained using AGK's baseline approach are unbiased. This assessment of the tradeoff is similar to the approach recommended in Chapter 6 of Angrist and Pishke (2009) when dealing with the noise versus bias trade-off one encounters when selecting the sample in an RD estimation.[27]

5.3. Using Russell 2000 index inclusion as an IV

We disagree with Wei and Young (2019)'s claim that because actual index assignment is an outcome of Russell's end-of-May market cap rankings, it cannot be used as an instrument when Russell's end-of-May market cap rankings are unobserved (e.g., see Section 4.1 of their paper). In other words, they argue that it is impossible to isolate exogenous variation in firms' ownership structures using index assignment as an IV unless one can control for Russell's unobserved end-of-May market cap rankings.

This claim is clearly incorrect, as illustrated by the prior estimations using simulated data (e.g., see Table 7). Estimators that use index assignment as an IV can recover the correct coefficients even when they are unable to control for Russell's unobserved total end-of-May market caps.

To further understand why their claim is incorrect, consider the scenario where Russell's end-of-May market cap ranking only correlates with a researcher's outcomes of interest (besides its effect on index assignment and a firm's ownership structure) because those unobserved rankings correlate with firm size. In this scenario, a more direct route to isolate exogenous variation in ownership would be to control for firm size in the IV estimation rather than Russell's unobserved end-of-May market cap ranking. While robustly controlling for Russell's unobserved market cap ranking can also be used to isolate exogenous variation in ownership through an IV estimation, it is not the only way to do it.

In other words, the inability to observe Russell's end-of-May market cap rankings need not imply that the AGK estimation fails to satisfy the exclusion assumption. Instead, the AGK IV estimation assumes that the unobservable component of Russell's total market cap does not directly matter for the IV estimation's outcome of interest after robustly controlling for the observable end-of-May market cap. While one can never rule out a possible violation of the IV estimation's exclusion condition, it is unclear

[27] As Angrist and Pischke (2009) notes, the smaller the bandwidth one chooses around the discontinuity when selecting the sample, the less risk one faces of potential bias from inadequate controls for the forcing variable. The tradeoff, however, is that the number of observations will decline in smaller bandwidths, reducing the precision of the RD estimate. Because of this, Angrist and Pischke recommend checking whether the RD estimate one obtains in a broader bandwidth (which will yield more precise estimates because of more observations) change in a meaningful way as you reduce the size of the bandwidth. A lack of such a change would be evidence that the more precisely estimated RD coefficient obtained using wider bandwidths is unbiased.

(at least to us) what the potential violation would be, especially given the idiosyncratic reasons for why Russell's total market cap calculations appear differ from those found in standard databases like CRSP and Compustat (see Ben-David, Franzoni, and Moussawi 2019).[28]

6. Concluding remarks

The Russell 1000/2000 index assignments can provide a powerful identification tool for helping researchers understand the relation between passive institutional ownership and outcomes related to corporate policy and governance. That said, the literature frequently misuses the setting, and multiple estimation methodologies have often led to competing findings. For example, some papers claim that the setting provides exogenous variation in total institutional ownership (e.g., Boone and White 2015; Crane, Michenaud, and Weston 2016), while other papers disagree (e.g., AGK 2016; Glossner 2018; Wei and Young 2019). The root cause of these disagreements is the use of inappropriate estimation methods by some authors. We believe that index assignment does affect firms' ownership structures, but only for ownership by passive mutual funds and ETFs. This conclusion is similar to that of Glossner (2018), which shows an impact of index assignment on passive ownership using both his preferred version of the AGK IV specification and the fuzzy RD specification recommended by Wei and Young (2019).

In our paper, we attempt to provide clarity to these issues by discussing the various methodologies used in the Russell 1000/2000 setting along with their relative weaknesses and strengths. Our objective is to provide guidance to future researchers who wish to use the Russell 1000/2000 setting for identification and hopefully help them avoid making incorrect inferences.

[28] To put this into the context of Wei and Young (2019), the AGK IV estimation assumes that if one adds a robust set of controls for observable market cap to the last equation in Section 4.1 of their paper, then the coefficient on the unobserved *Russell Rank* in that equation, β_2, is zero.

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Appendix

Stata code used to generate tables and figures in this paper, and corresponding R code

---

Construct *russell_mc (section 3.1)*:

Stata

```
set obs 27000
egen year = seq(), f(1998) t(2006)
bys year: gen stock_id=_n
gen log_russell_mc=rnormal(7,1.3) if year==1998
sort stock_id year
by stock_id: replace log_russell_mc=log_russell_mc[_n-1]+(.08+(.35^2)/2)+.35*rnormal(0,1) if
log_russell_mc==.
gen russell_mc=exp(log_russell_mc)
```

R

```
df <- data.frame("year" = rep(seq(from = 1998, to = 2006), each = 3000), "stock_id" =
         rep(seq(from = 1, to = 3000), time = 9))
df[df["year"]==1998,"log_russell_mc"]=rnorm(3000,7,1.3)
df <- df[order(df$stock_id, df$year),]
for (i in seq(1,3000)){
      for (j in seq(1999,2006)){
        df[(df$stock_id==i&df$year==j),]["log_russell_mc"]=df[(df$stock_id==i&df$year==(j-
        1)),]["log_russell_mc"]+(.08+(.35^2)/2)+.35*rnorm(1,0,1)
      }
}
df["russell_mc"]=exp(df$log_russell_mc)
```

Construct *float_ mc (section 3.1)*:

Stata

```
sknor 27000 123 0.85 0.05 -1.5 6
replace skewnormal=. if year!=1998
replace skewnormal=0 if skewnormal < 0
replace skewnormal=1 if skewnormal>1 & skewnormal!=.
bys stock_id: egen traded=mean(skewnormal)
gen float_mc= traded*russell_mc
```

R

```
install.packages("rugarch")
library("rugarch")
df["skewnormal"]=rdist(distribution = "snorm", 27000, mu=0.85, sigma=0.233607, skew=-1.5)
df[df$year!=1998,"skewnormal"]=NA
df[(df$skewnormal<0)&(is.na(df$skewnormal)==FALSE),"skewnormal"]=0
df[(df$skewnormal>1)&(is.na(df$skewnormal)==FALSE),"skewnormal"]=1
df<-within(df, {traded = ave(skewnormal,stock_id,FUN=function(x) mean(x, na.rm=T))})
df<-df[,colnames(df)!="skewnormal"]
df["float_mc"]=df["traded"]*df["russell_mc"]
```

Construct Russell's rank variables *(section 3.1)*:

Stata

```
gsort year -russell_mc
by year: gen russell_rank=_n
gen index=1 if russell_rank>=1 & russell_rank<=1000
```

```
replace index=2 if russell_rank>=1001
gsort year +index -float_mc
by year: gen float_rank=_n
```

R

```
df <- df[order(df$year,-df$russell_mc),]
df["russell_rank"]=rep(seq(from = 1, to = 3000),9)
df["index"]=NA
df["index"]=ifelse((df$russell_rank>=1 & df$russell_rank<=1000), df["index"]<-1, df["index"]<-2)
df <- df[order(df$year,df$index,-df$float_mc),]
df["float_rank"]=rep(seq(from = 1, to = 3000),9)
```

Construct *crsp_ mc (section 3.1)*:

Stata

```
gen temp=runiform(0, 1)
gen russell_mc_adj=russell_mc
replace russell_mc_adj= russell_mc*.50 if traded<.75 & temp<.20
replace russell_mc_adj= russell_mc*1.5 if temp>0.99
gen crsp_mc= russell_mc_adj*(1+rnormal(0,.01)}
gsort year -crsp_mc
by year: gen crsp_rank=_n
```

R

```
df["temp"]=runif(27000,0,1)
df["russell_mc_adj"]=df["russell_mc"]
df[df$traded<0.75&df$temp<0.20,"russell_mc_adj"]=df[df$traded<0.75&df$temp<0.20,"russell_mc"]*0.5
df[df$temp>0.99,"russell_mc_adj"]=df[df$temp>0.99,"russell_mc"]*1.5
df["crsp_mc"]=df$russell_mc_adj*(1+rnorm(27000,0,.01))
df <- df[order(df$year,-df$crsp_mc),]
df["crsp_rank"]=rep(seq(from = 1, to = 3000),9)
```

Generate the Proportion of Institutional Ownership *(section 3.1)*:

Stata

```
gen float_adj = float_mc/crsp_mc
gen IO = rnormal(0.35, 0.24) + float_adj*rnormal(0.37, 0.02)
replace IO=0 if IO<0
replace IO=1 if IO>1
```

R

```
df["float_adj"]=df["float_mc"]/df["crsp_mc"]
df["IO"]=rnorm(27000, 0.35, 0.24) + df$float_adj*rnorm(27000, 0.37, 0.02)
df[df$IO<0,"IO"]=0
df[df$IO>1,"IO"]=1
```

Adjust the Proportion of Passive Institutional Ownership *(section 3.1)*:

Stata

```
gen passive = 0.046*IO
replace passive=passive+.01 if index==2
```

R

```
df["passive"]=df["IO"]*0.046
df[df$index==2,"passive"]=df[df$index==2,"passive"]+0.01
```

Construct Indicator for inclusion in *Russell 2000 (section 3.1)*:

Stata
```
gen R2000=0
replace R2000=1 if index==2

replace passive=passive*100
replace IO=IO*100
```

R
```
df["R2000"]=0
df[df$index==2,"R2000"]=1

df$passive=df$passive*100
df$IO=df$IO*100
```

Sharp RD using Russell's unobservable end-of-May market cap rankings *(section 3.2.1)*:

Stata
```
gen rank=russell_rank

gen rank1=(rank-1000)
gen rank2=(rank-1000)^2
gen rank3=(rank-1000)^3

gen band=250

reg passive R2000 rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-band,
cluster(stock_id)
```

R
```
install.packages("estimatr")
library("estimatr")

df["rank"]=df$russell_rank

df["rank1"]=df$rank-1000
df["rank2"]=(df$rank-1000)^2
df["rank3"]=(df$rank-1000)^3

df["band"]=250

datareg=df[((df$rank-1000)<=df$band) & ((df$rank-1000)>(-df$band)),]
lm_robust(data=datareg,formula=passive~R2000+rank1+rank2+rank3,clusters=stock_id)
```

Sharp RD using Russell's observable June float-adjusted rankings *(section 3.2.2)*:

Stata
```
gen rank=float_rank

gen rank1=(rank-1000)
gen rank2=(rank-1000)^2
gen rank3=(rank-1000)^3

gen band=250

reg passive R2000 rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-band,
cluster(stock_id)
```

R
```
install.packages("estimatr")
library("estimatr")

df["rank"]=df$float_rank
df["rank1"]=df$rank-1000
df["rank2"]=(df$rank-1000)^2
df["rank3"]=(df$rank-1000)^3

df["band"]=250

datareg=df[((df$rank-1000)<=df$band) & ((df$rank-1000)>(-df$band)),]
lm_robust(data=datareg,formula=passive~R2000+rank1+rank2+rank3,clusters=stock_id)
```

Sharp RD using observable within-index end-of-May rankings *(section 3.2.3)*:

Stata
```
gsort year +index -crsp_mc
by year: gen rank=_n

gen rank1=(rank-1000)
gen rank2=(rank-1000)^2
gen rank3=(rank-1000)^3

gen band=250

reg passive R2000 rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-band, cluster(stock_id)
```

R
```
df <- df[order(df$year,df$index,-df$crsp_mc),]
df["rank"]=rep(seq(from = 1, to = 3000),9)

df["rank1"]=df$rank-1000
df["rank2"]=(df$rank-1000)^2
df["rank3"]=(df$rank-1000)^3

df["band"]=250

datareg=df[((df$rank-1000)<=df$band) & ((df$rank-1000)>(-df$band)),]
lm_robust(data=datareg,formula=passive~R2000+rank1+rank2+rank3,clusters=stock_id)
```

Fuzzy Regression Discontinuity *(section 3.2.4)*:

Stata
```
gen rank=crsp_rank

gen treatment = 0
replace treatment = 1 if rank > 1000 & rank !=.

gen rank1=(rank-1000)
gen rank2=(rank-1000)^2
gen rank3=(rank-1000)^3

gen band=250

reg R2000 treatment rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-band, cluster(stock_id)

ivregress 2sls passive (R2000 = treatment) rank1 rank2 rank3 if (rank-1000)<=band & (rank-1000)>-
band, cluster(stock_id)
```

R

```
install.packages("estimatr")
library("estimatr")

df["rank"]=df$crsp_rank

df["treatment"]=0
df[(df$rank>1000)&(is.na(df$rank)==FALSE),"treatment"]=1

df["rank1"]=df$rank-1000
df["rank2"]=(df$rank-1000)^2
df["rank3"]=(df$rank-1000)^3

df["band"]=250

datareg=df[((df$rank-1000)<=df$band) & ((df$rank-1000)>(-df$band)),]
lm_robust(data=datareg,formula=R2000~treatment+rank1+rank2+rank3,clusters=stock_id)

iv_robust(data=datareg,formula=passive~R2000+rank1+rank2+rank3 |
                  treatment+rank1+rank2+rank3, se_type="stata", clusters=stock_id)
```

IV Estimation of Appel, Gormley, and Keim (2016) *(section 3.2.5)*:

Stata

```
gen mc=crsp_mc
gen rank = crsp_rank

gen size1 = log(mc)
gen size2 = size1^2
gen size3 = size1^3
gen float1 = ln(float_mc)

gen band=250

xi: reg passive R2000 float1 size1 size2 size3 i.year if (rank-1000)<=band & (rank-1000)>-band,
cluster(stock_id)
```

R

```
install.packages("estimatr")
library("estimatr")

df["mc"]=df$crsp_mc
df["rank"]=df$crsp_rank
df["size1"]=log(df$mc)
df["size2"]=df["size1"]^2
df["size3"]=df["size1"]^3
df["float1"]=log(df$float_mc)
df["band"]=250
df$year=factor(df$year)
datareg=df[((df$rank-1000)<=df$band) & ((df$rank-1000)>(-df$band)),]
lm_robust(data=datareg,formula=passive~R2000+float1+size1+size2+size3+year,
                    clusters=stock_id)
```

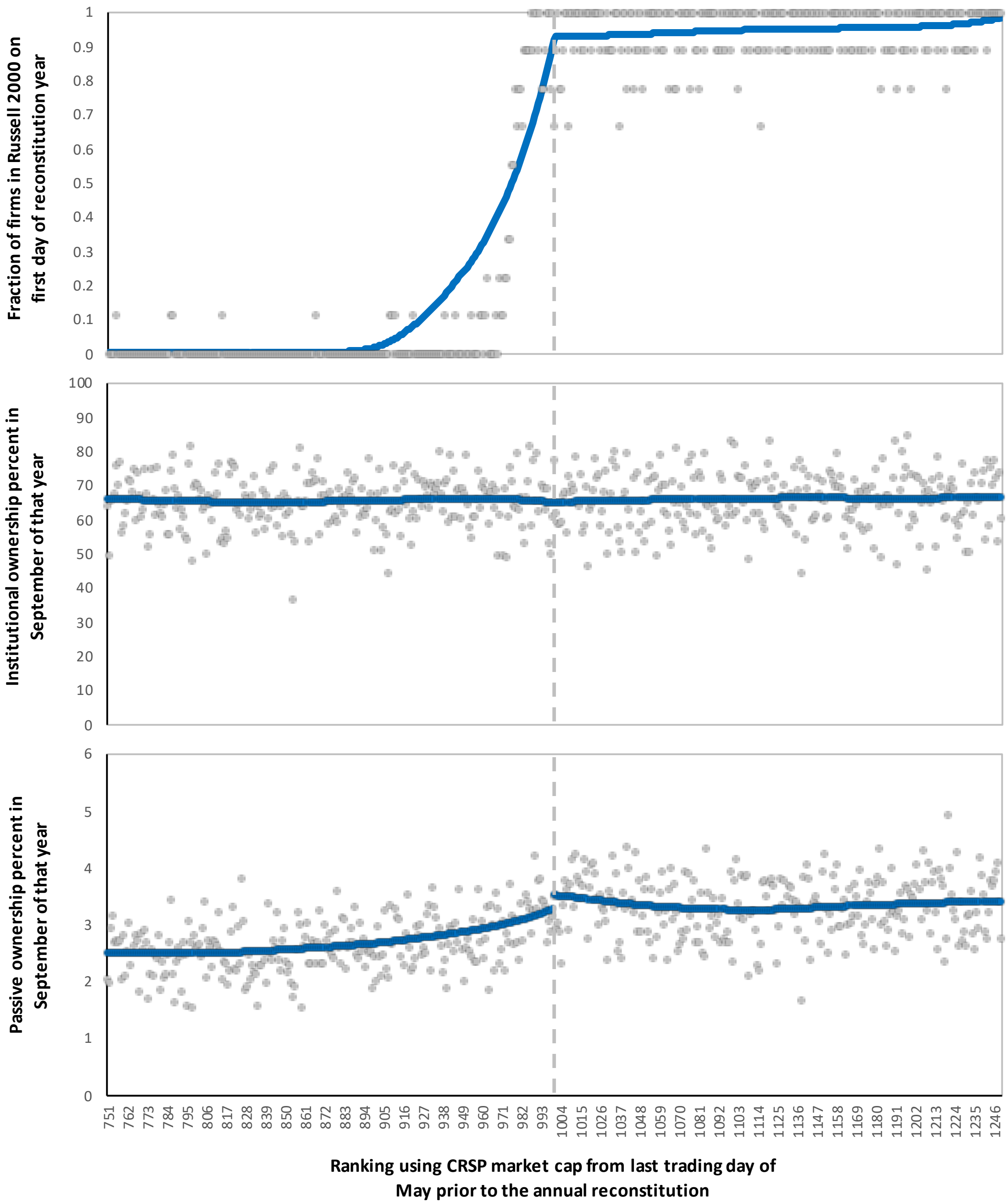

Figure 1
Average fraction of firms in Russell 2000, institutional ownership, and passive ownership by ranking, using CRSP market caps from the last trading day in May prior to the annual reconstitution to construct the ranking

Description: This figure plots the average fraction of firm-year observations in the Russell 2000 after the June reconstitution (top panel), average percent of stock held by institutional investors in September of that year (middle panel), and average percent of stock held by passive investors in September of that year (bottom panel) by size ranking for the 751st to 1,250th largest firms for the years 1998-2006, where ranking is determined using CRSP total market capitalization numbers from the last trading day in May prior to Russell's annual reconstitution. Averages are calculated per ranking using the reconstitution years 1998-2006. The plotted lines reflect locally weighted scatterplot smoothings estimated separately for rankings above and below the 1,000th/1,001st ranking threshold using the default settings of Stata's lowess command.

Interpretation: The unobserved market cap used by Russell to assign stocks to its indexes is different than the market cap computed with CRSP data. As a result, many stocks that Russell assigns to the bottom of the R1000 are predicted to be in the R2000 when stocks are ranked using end-of-May CRSP market cap, and there is no discontinuity at the ranking of 1000. And, while there is no evidence that index assignment is connected to institutional ownership, it is connected to passive ownership.

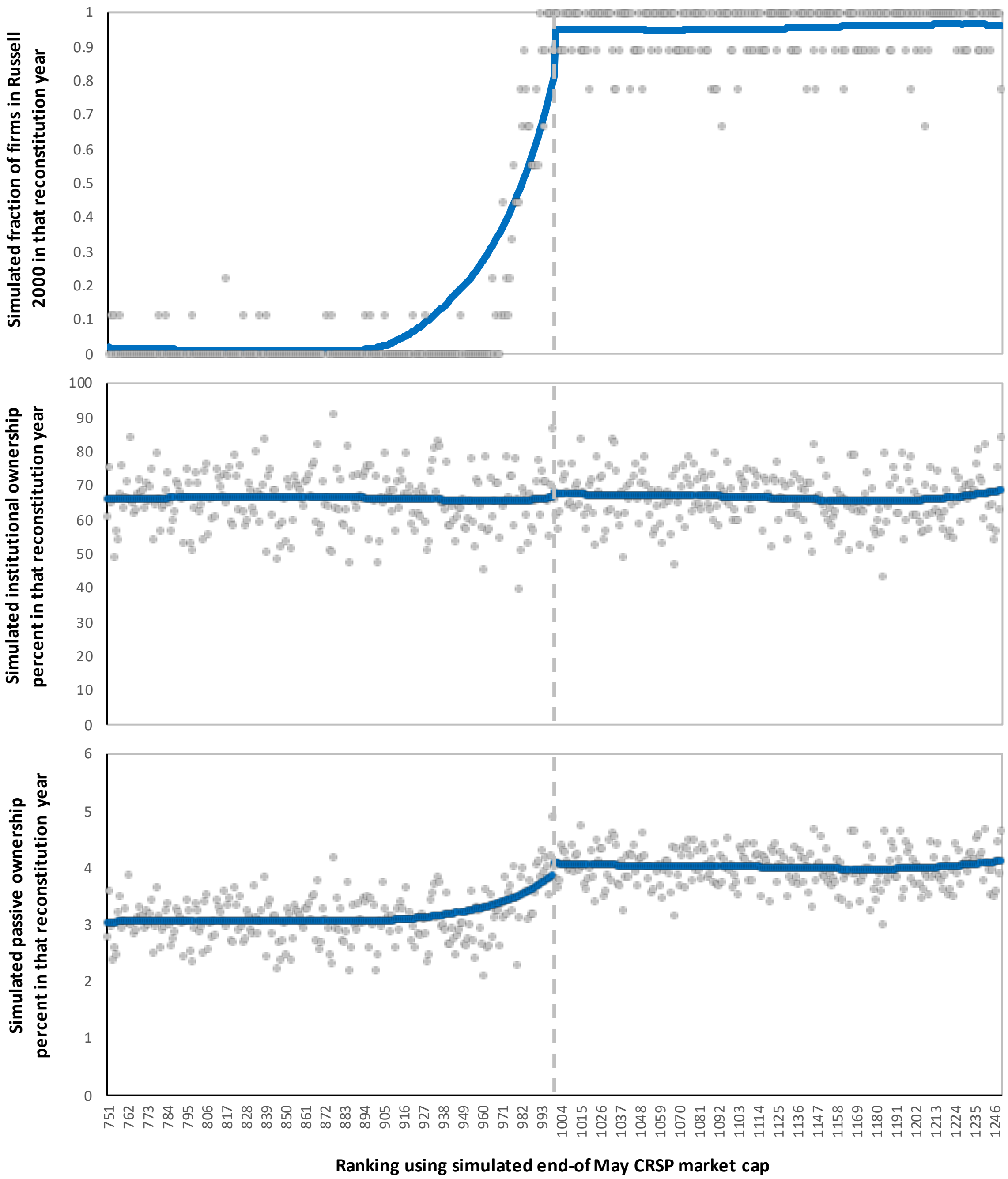

Figure 2
Reconstruction of Figure 1 using simulated data

Description: This figure plots the average simulated fraction of firm-year observations in the Russell 2000 that reconstitution year (top panel), average simulated percent of stock held by institutional investors (*IO*) in that reconstitution year (middle panel), and average simulated percent of stock held by passive investors (*passive*) in that reconstitution year (bottom panel) by size ranking for the 751st to 1,250th largest firms for the years 1998-2006, where ranking is determined using the simulated end-of-May CRSP total market capitalization numbers (*crsp_mc*). Averages are calculated per ranking using the simulated reconstitution years 1998-2006. The plotted lines reflect locally weighted scatterplot smoothings estimated separately for rankings above and below the 1,000th/1,001st ranking threshold using the default settings of Stata's lowess command.

Interpretation: The simulated data, which assumes no effect of index assignment on institutional ownership but an effect on passive ownership, exhibits patterns similar to those observed in the actual data plotted in Figure 1.

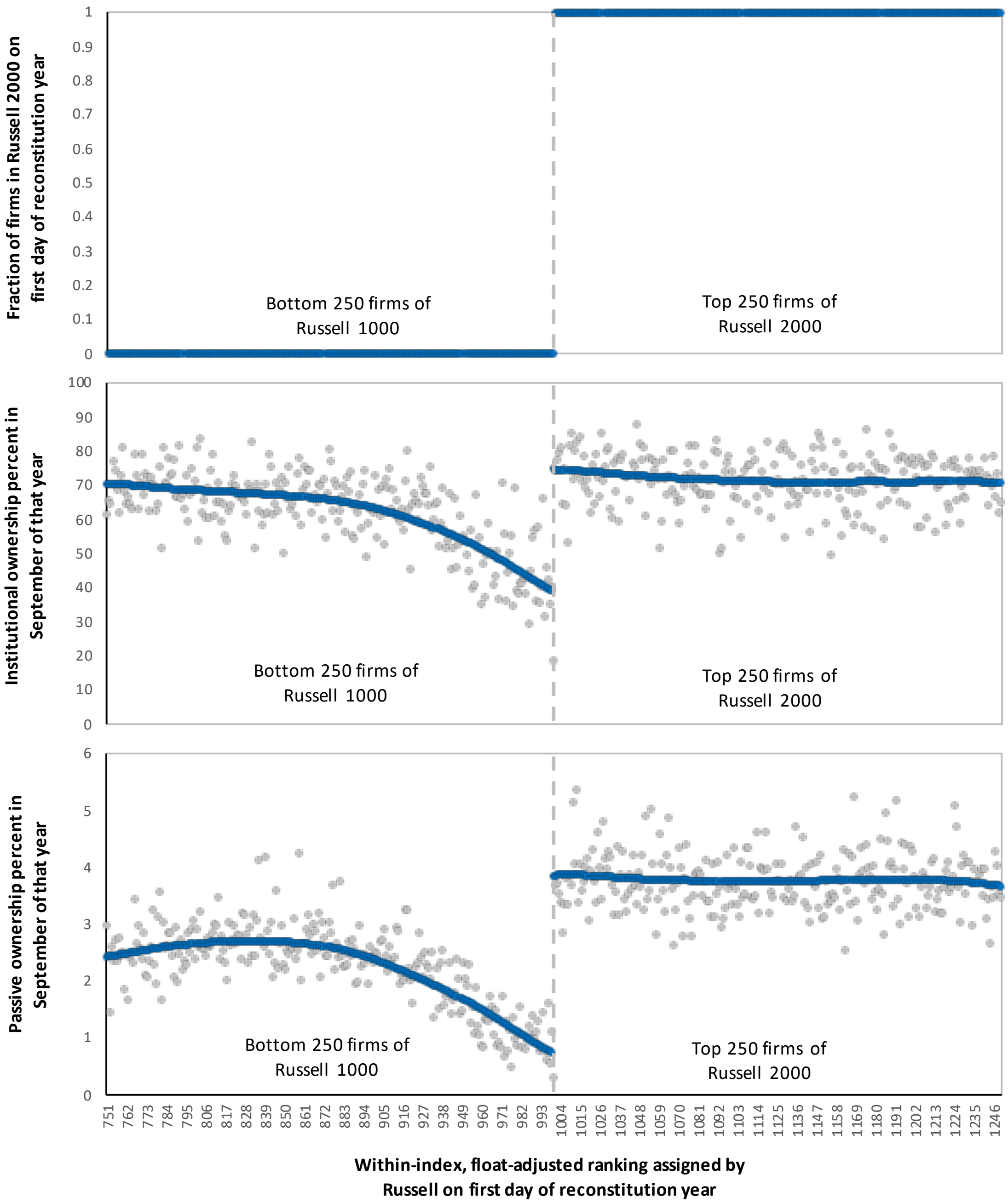

Figure 3
Average fraction of firms in Russell 2000, institutional ownership, and passive ownership by ranking, where ranking is calculated using the initial within-index, float-adjusted portfolio weights assigned by Russell

Description: This figure plots the average fraction of firm-year observations in the Russell 2000 after the June reconstitution (top panel), average percent of stock held by institutional investors in September of that year (middle panel), and average percent of stock held by passive investors in September of that year (bottom panel) by size ranking for the bottom 250 firms in the Russell 1000 index and the top 250 firms in the Russell 2000 index for the years 1998-2006, where rankings are determined using Russell's within-index, float-adjusted market cap rankings, as determined on the first trading day after the annual reconstitution that year. A ranking of 1000 reflects the firm with the smallest initial portfolio weight in the Russell 1000 index, while a ranking of 1001 reflects the firm with the largest initial portfolio weight in the Russell 2000 index. Averages are calculated per ranking using the reconstitution years 1998-2006. The plotted lines reflect locally weighted scatterplot smoothings estimated separately for rankings above and below the 1,000th/1,001st ranking threshold using the default settings of Stata's lowess command.

Interpretation: Ranking stocks based on Russell's actual within-index portfolio weights creates a discontinuity in institutional ownership at the threshold between the R1000 and R2000 indexes, in contrast to the lack of any such discontinuity one finds when instead using end-of-May CRSP market caps to create the ranking, as done in Figure 1.

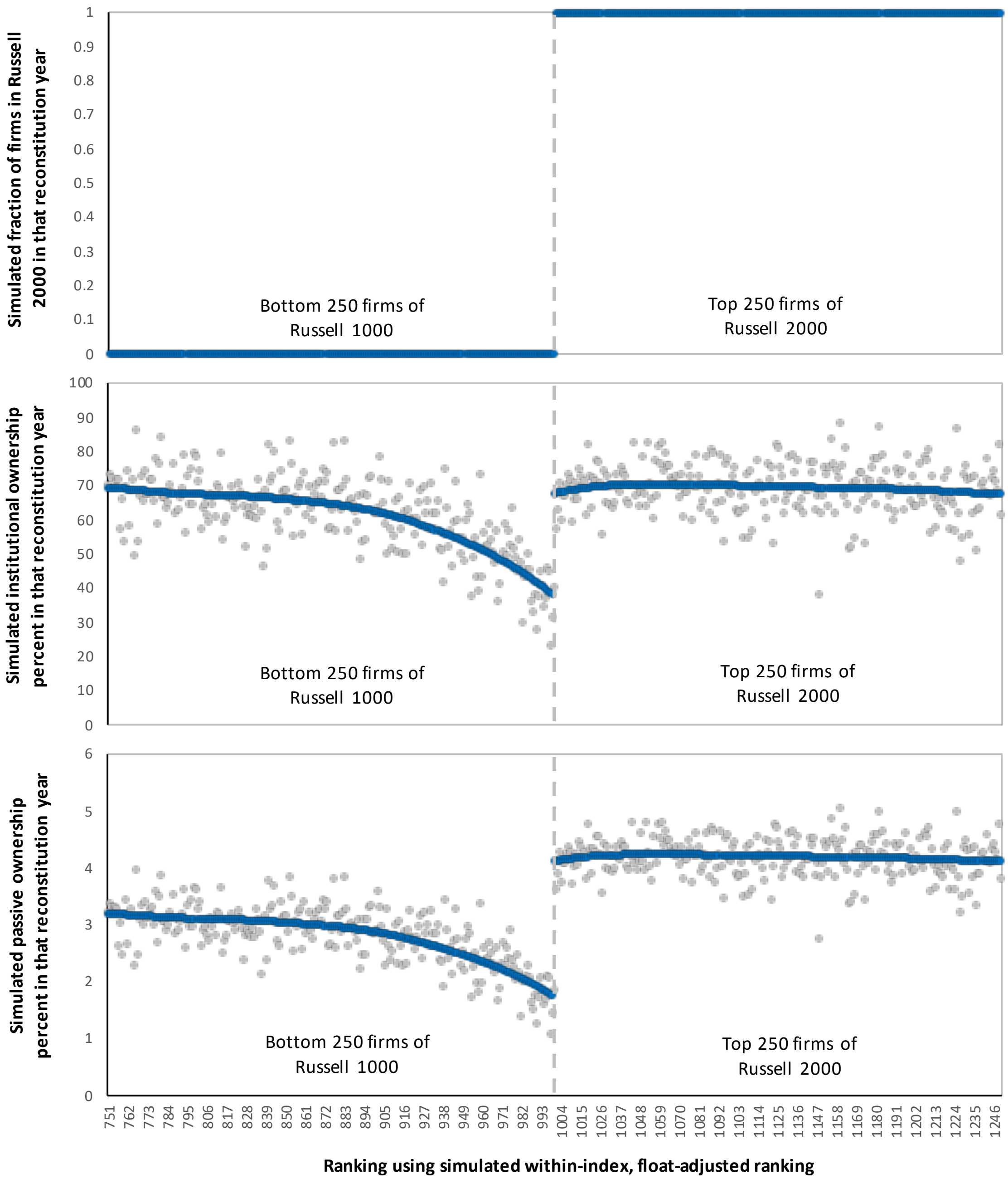

Figure 4
Reconstruction of Figure 3 using simulated data

Description: This figure plots the average simulated fraction of firm-year observations in the Russell 2000 that reconstitution year (top panel), average simulated percent of stock held by institutional investors (*IO*) in that reconstitution year (middle panel), and average simulated percent of stock held by passive investors (*passive*) in that reconstitution year (bottom panel) by size ranking for the simulated bottom 250 firms in the Russell 1000 index and the simulated top 250 firms in the Russell 2000 index for the years 1998-2006, where ranking is determined using the simulated within-index float-adjusted rankings (*float_rank*). Averages are calculated per ranking using the simulated reconstitution years 1998-2006. The plotted lines reflect locally weighted scatterplot smoothings estimated separately for rankings above and below the 1,000th/1,001st ranking threshold using the default settings of Stata's lowess command.

Interpretation: The simulated data, which assumes no effect of index assignment on institutional ownership but an effect on passive ownership, exhibits patterns similar to those observed in the actual data plotted in Figure 3, and shows that sorting the data using the within-index, float-adjusted rankings leads to a spurious jump in institutional ownership.

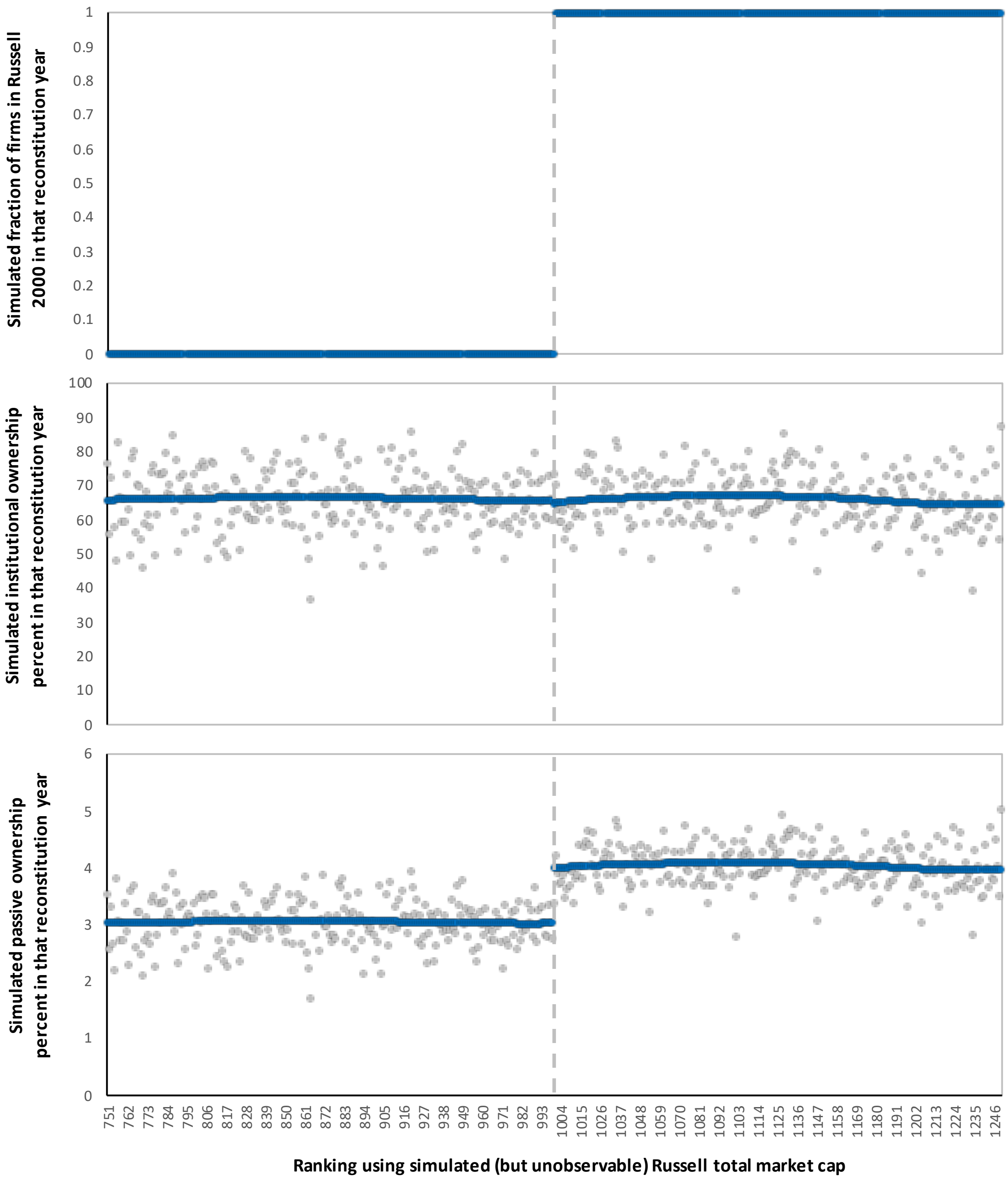

Figure 5
Average fraction of firms in Russell 2000, institutional ownership, and passive ownership by ranking in the simulated data, using the simulated (but unobservable) Russell market caps to construct the ranking

Description: This figure plots the average simulated fraction of firm-year observations in the Russell 2000 that reconstitution year (top panel), average simulated percent of stock held by institutional investors (*IO*) in that reconstitution year (middle panel), and average simulated percent of stock held by passive investors (*passive*) in that reconstitution year (bottom panel) by size ranking for the 751st to 1,250th largest firms for the years 1998-2006, where ranking is determined using the simulated Russell total market capitalization numbers (*russell_mc*). Averages are calculated per ranking using the simulated reconstitution years 1998-2006. The plotted lines reflect locally weighted scatterplot smoothings estimated separately for rankings above and below the 1,000th/1,001st ranking threshold using the default settings of Stata's lowess command.

Interpretation: If one could observe and sort the data based on Russell's total market cap, one would more clearly see that index assignment has no effect on institutional ownership but an effect on passive ownership. The patterns for ownership are similar to those observed when sorting the data on the observable CRSP market caps, but less noisy now that the ranking variable perfectly predicts index assignments each year.

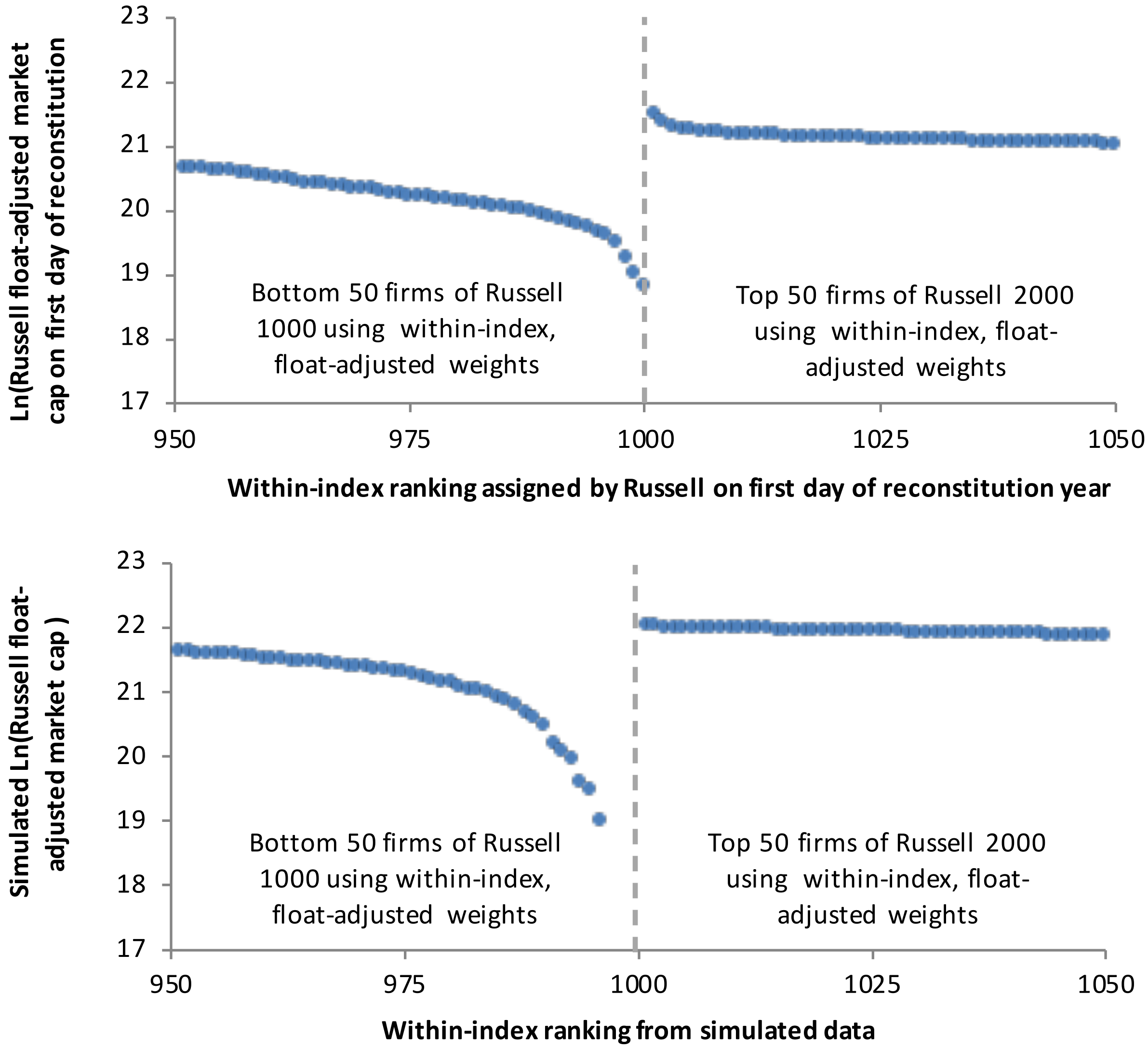

Figure 6
Average Ln(Russell's float-adjusted market cap) by ranking using both actual and simulated data, where ranking is calculated using the initial within-index, float-adjusted market caps reported by Russell

Description: The top panel of this figure plots the average Ln(float-adjusted market cap) used by Russell to determine within-index rankings on the first trading day of the reconstitution year. The average is reported by ranking for the years 1998-2006 for the bottom 50 firms in the Russell 1000 index and the top 50 firms in the Russell 2000 index. The bottom panel does the same calculation using the simulated data. A ranking of 1000 reflects the firm with the smallest float-adjusted market cap in the Russell 1000 index, while a ranking of 1001 reflects the firm with the largest float-adjusted market cap in the Russell 2000 index.

Interpretation: Ranking stocks based on within-index portfolio weights creates a discontinuity in float-adjusted market caps at the threshold between the R1000 and R2000 indexes that will bias any sharp RD estimation that uses this method to control for market cap.

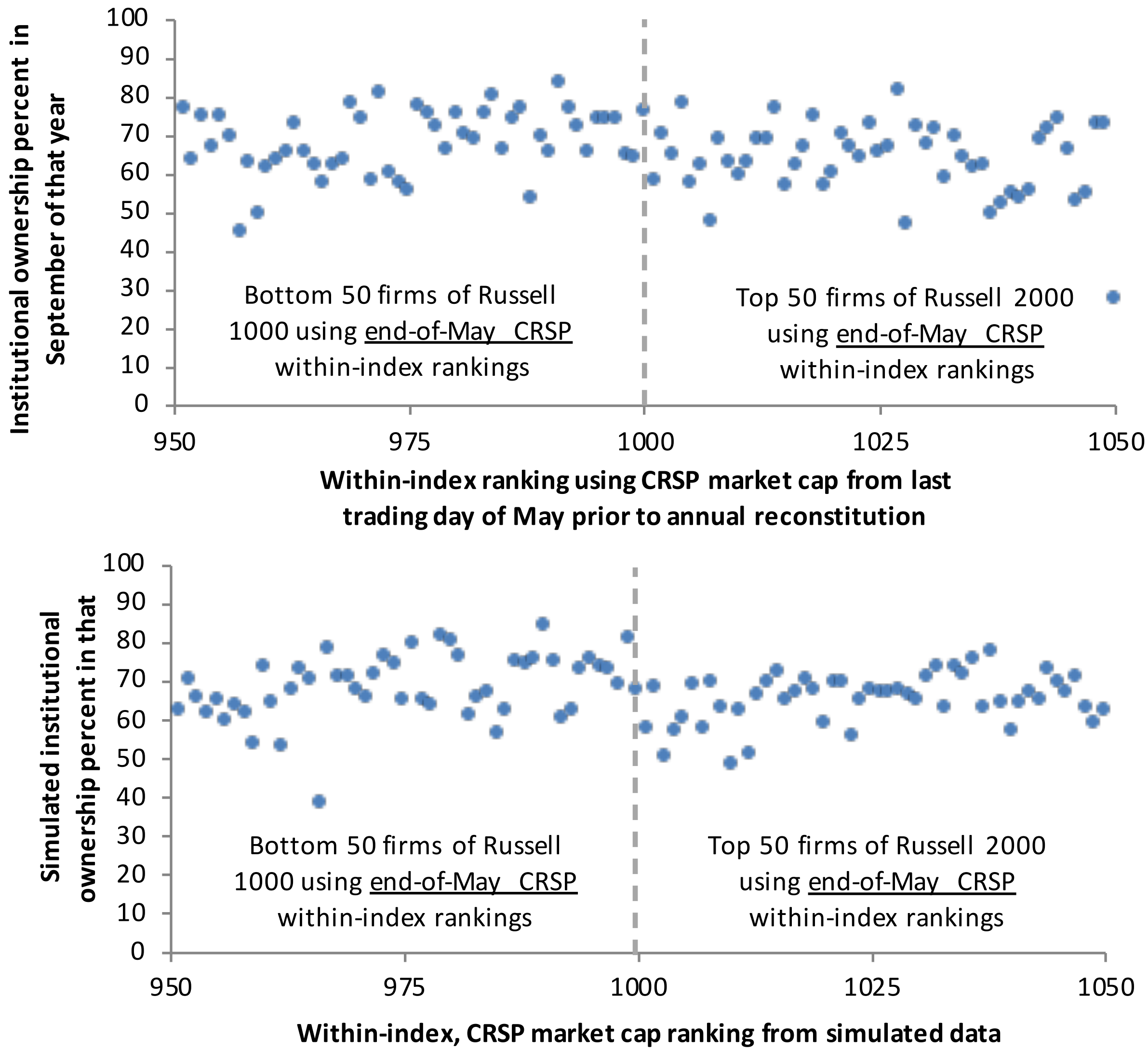

Figure 7
Average institutional ownership by ranking using both actual and simulated data, where ranking is calculated using within-index rankings based on CRSP market caps from the last trading day in May prior to the annual reconstitution

Description: The top panel of this figure plots the average percent of stock held by institutional investors in September of that year by within-index ranking, as determined using CRSP market caps from the last trading day in May prior to the reconstitution. A ranking of 1000 reflects the firm with the lowest end-of-May market cap in the Russell 1000 index, while a ranking of 1001 reflects the firm with the highest end-of-May market cap in the Russell 2000 index. The average is reported by ranking for the years 1998-2006 for the bottom 50 firms in the Russell 1000 index and the top 50 firms in the Russell 2000 index after ranking in this way. The bottom panel does the same calculation using the simulated data.

Interpretation: The Crane et al. (2016) method of ranking stocks within index based on end-of-May CRSP market caps eliminates the spurious jump in institutional ownership of Figures 3-4.

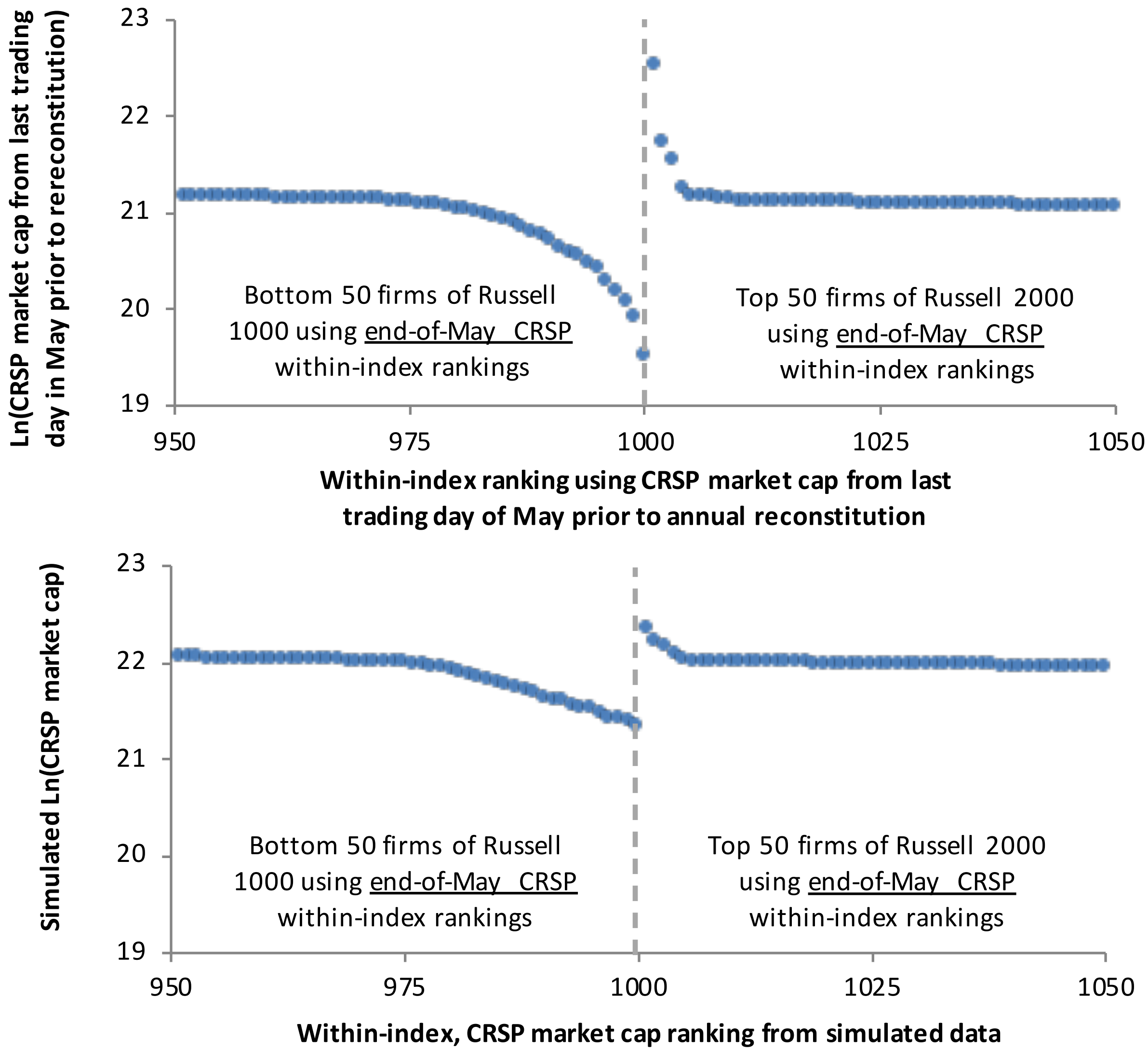

Figure 8
Average Ln(CRSP end-of-May market cap) by ranking using both actual and simulated data, where ranking is calculated using within-index rankings based on CRSP market caps from the last trading day in May prior to the annual reconstitution

Description: The top panel of this figure plots the average Ln(CRSP market cap) from the last trading day in May prior to the reconstitution by within-index ranking, as determined using CRSP market caps from the last trading day in May prior to the reconstitution. A ranking of 1000 reflects the firm with the lowest end-of-May market cap in the Russell 1000 index, while a ranking of 1001 reflects the firm with the highest end-of-May market cap in the Russell 2000 index. The average is reported by ranking for the years 1998-2006 for the bottom 50 firms in the Russell 1000 index and the top 50 firms in the Russell 2000 index after ranking in this way. The bottom panel does the same calculation using the simulated data.

Interpretation: The Crane et al. (2016) method of ranking stocks within index using end-of-May CRSP market caps creates a discontinuity in CRSP market caps at the R1000/R2000 threshold that will bias any sharp RD estimation that uses this method to control for market cap.

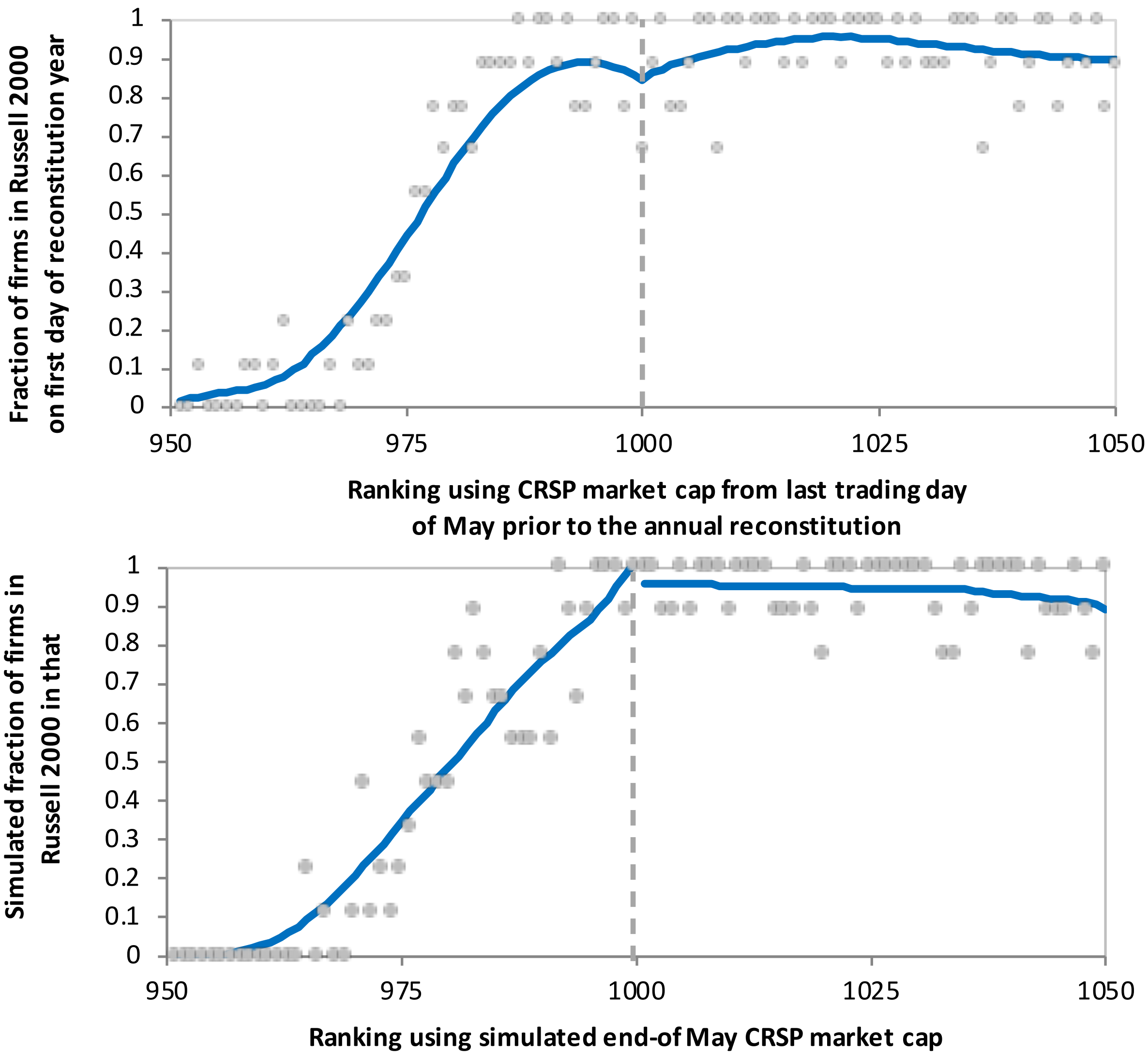

Figure 9
Probability of being included in the Russell 2000 by ranking near the Russell 1000/2000 threshold using CRSP end-of-May market capitalizations to construct the ranking

Description: This figure plots the average fraction of firm-year observations in the Russell 2000 by size ranking for the 950th to 1,050th largest firms, where ranking is determined using end-of-May market capitalization numbers as calculated for firms in the Russell 1000/2000 indices between 1998-2006 using both the actual data (top panel) and simulated data (bottom panel). Averages are calculated per ranking using the reconstitution years 1998-2006. The plotted lines reflect locally weighted scatterplot smoothings estimated separately for rankings above and below the 1,000th/1,001st ranking threshold using the default settings of Stata's lowess command.

Interpretation: The unobserved market cap used by Russell to assign stocks to its indexes is different than the market cap computed with CRSP data. As a result, many stocks that Russell assigns to the bottom of the R1000 are predicted to be in the R2000 when stocks are instead ranked using end-of-May CRSP market cap, and there is no discontinuity at the ranking of 1000.

Table 1
Sharp RD estimates using simulated data and Russell's unobserved end-of-May market cap rankings

Description: This table uses the simulated data to estimate the ideal sharp RD estimation that uses Russell's unobserved end-of-May CRSP market cap (*russell_mc*) to construct the rankings. Specifically, we estimate

$$Y_{it} = \alpha + \gamma R2000_{it} + \sum_{n=1}^{N} \varphi_n (Rank_{it} - 1000)^n + \mu_{it}$$

where Y is the simulated outcome of interest for stock i in reconstitution year t, *R2000* is an indicator for inclusion in the simulated Russell 2000 index for firm i after the annual reconstitution in June of year t, *Rank* is the firm's market cap ranking when using the simulated, but unobservable, Russell end-of-May CRSP market caps to construct the ranking (*russell_rank*), and N is the polynomial order we use to control for (*Rank* - 1000). The model is estimated over the nine simulated annual reconstitutions that occur from 1998 to 2006 using a polynomial order control of N=1, 2, and 3. Two dependent variables, Y, are used. The top panel uses the percent of a stock's ownership held by passive investors in that reconstitution year (*passive*), while the bottom panel uses the simulated percent of a stock's ownership held by institutional investors (*IO*). Columns (1)-(3) use a sample bandwidth of 50 stocks (i.e., $Rank \in [951,1050]$); Columns (4)-(6) use a sample bandwidth of 100 stocks (i.e., $Rank \in [901,1100]$); Columns (7)-(9) use a sample bandwidth of 250 stocks (i.e., $Rank \in [751,1250]$), and Columns (10)-(12) use a sample bandwidth of 500 stocks (i.e., $Rank \in [501,1501]$). Standard errors, μ, are clustered at the stock level and reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Interpretation: If Russell's end-of-May total market caps were observable, the sharp RD would uncover the correct effect (non-effect) of index assignment on passive ownership (institutional ownership) under the assumed data generating process.

Dependent variable **= Simulated percent of stock *i* held by passive investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	+0.83***	+0.83***	+0.63***	+1.02***	+1.02***	+0.93***	+1.06***	+1.06***	+1.02***	+1.03***	+1.03***	+1.00***
(1 if Russell Rank > 1000)	(0.13)	(0.13)	(0.17)	(0.10)	(0.10)	(0.12)	(0.06)	(0.06)	(0.08)	(0.05)	(0.05)	(0.06)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.21	0.21	0.21	0.19	0.19	0.19	0.18	0.18	0.18	0.19	0.19	0.19

Dependent variable **= Simulated percent of stock *i* held by institutional investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	−3.71	−3.71	−8.15**	+0.51	+0.51	−1.59	+1.27	+1.27	+0.44	+0.55	+0.55	−0.11
(1 if Russell Rank > 1000)	(2.78)	(2.78)	(3.75)	(2.06)	(2.06)	(2.58)	(1.38)	(1.38)	(1.73)	(1.00)	(1.00)	(1.28)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Table 2
Sharp RD estimates using index switchers, simulated data, and Russell's unobserved end-of-May market cap rankings

Description: This table uses the simulated data to estimate the ideal sharp RD estimation that uses Russell's unobserved end-of-May CRSP market cap (*russell_mc*) to construct the rankings and, by including stock-level fixed effects, makes use of index switchers. Specifically, we estimate

$$Y_{it} = \gamma R2000_{it} + \sum_{n=1} \varphi_n (Rank_{it} - 1000)^n + \alpha_i + \delta_t + \mu_{it}$$

where Y is the simulated outcome of interest for stock i in reconstitution year t, *R2000* is an indicator for inclusion in the simulated Russell 2000 index for firm i after the annual reconstitution in June of year t, *Rank* is the firm's market cap ranking when using the simulated, but unobservable, Russell end-of-May CRSP market caps to construct the ranking (*russell_rank*), N is the polynomial order we use to control for (*Rank* - 1000), α_i are stock-level fixed effects, and δ_t are year fixed effects. The model is estimated over the nine simulated annual reconstitutions that occur from 1998 to 2006 using a polynomial order control of N=1, 2, and 3. Two dependent variables, Y, are used. The top panel uses the percent of a stock's ownership held by passive investors in that reconstitution year (*passive*), while the bottom panel uses the simulated percent of a stock's ownership held by institutional investors (*IO*). Columns (1)-(3) use a sample bandwidth of 50 stocks (i.e., $Rank \in [951,1050]$); Columns (4)-(6) use a sample bandwidth of 100 stocks (i.e., $Rank \in [901,1100]$); Columns (7)-(9) use a sample bandwidth of 250 stocks (i.e., $Rank \in [751,1250]$), and Columns (10)-(12) use a sample bandwidth of 500 stocks (i.e., $Rank \in [501,1501]$). Standard errors, μ, are clustered at the stock level and reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Interpretation: If Russell's end-of-May total market caps were observable, the sharp RD with stock-level fixed effects would also uncover the correct effect (non-effect) of index assignment on passive ownership (institutional ownership), but the estimates are noisier as there are not a lot of index switchers, particularly in smaller bandwidths.

Dependent variable **= Simulated percent of stock *i* held by passive investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	+0.27	+0.27	+0.09	+0.91***	+0.91***	+0.68***	+1.08***	+1.08***	+1.05***	+1.02***	+1.02***	+0.97***
(1 if Russell Rank > 1000)	(0.23)	(0.23)	(0.33)	(0.13)	(0.13)	(0.17)	(0.07)	(0.07)	(0.09)	(0.05)	(0.05)	(0.06)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.22	0.22	0.22	0.20	0.20	0.20	0.18	0.18	0.18	0.14	0.14	0.14

Dependent variable **= Simulated percent of stock *i* held by institutional investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	−15.8***	−15.8***	−19.7***	−1.99	−1.98	−7.05*	+1.83	+1.82	+0.99	+0.40	+0.40	−0.60
(1 if Russell Rank > 1000)	(5.03)	(5.05)	(7.18)	(2.82)	(2.82)	(3.63)	(1.56)	(1.56)	(2.02)	(1.06)	(1.06)	(1.36)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.05	0.05	0.05	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.00	0.00

Table 3
Sharp RD estimates using simulated data and Russell's within-index, float-adjusted market cap rankings

Description: This table uses the simulated data to estimate the same sharp RD estimation as Table 1, but now uses Russell's within-index, float-adjusted market cap (*float_mc*) to construct the rankings. Specifically, we estimate

$$Y_{it} = \alpha + \gamma R2000_{it} + \sum_{n=1}^{N} \varphi_n (Rank_{it} - 1000)^n + \mu_{it}$$

where Y is the simulated outcome of interest for stock i in reconstitution year t, $R2000$ is an indicator for inclusion in the simulated Russell 2000 index for firm i after the annual reconstitution in June of year t, $Rank$ is the firm's market cap ranking when using the simulated within-index, float-adjusted market caps to construct the ranking (*float_rank*), and N is the polynomial order we use to control for ($Rank$ - 1000). The model is estimated over the nine simulated annual reconstitutions that occur from 1998 to 2006 using a polynomial order control of N=1, 2, and 3. Two dependent variables, Y, are used. The top panel uses the percent of a stock's ownership held by passive investors in that reconstitution year (*passive*), while the bottom panel uses the simulated percent of a stock's ownership held by institutional investors (IO). Columns (1)-(3) use a sample bandwidth of 50 stocks (i.e., $Rank \in [951,1050]$); Columns (4)-(6) use a sample bandwidth of 100 stocks (i.e., $Rank \in [901,1100]$); Columns (7)-(9) use a sample bandwidth of 250 stocks (i.e., $Rank \in [751,1250]$), and Columns (10)-(12) use a sample bandwidth of 500 stocks (i.e., $Rank \in [501,1501]$). Standard errors, μ, are clustered at the stock level and reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Interpretation: Using Russell's within-index, float-adjusted rankings to estimate the sharp RD results in biased estimates, and researchers using this estimation will incorrectly infer that assignment of a stock to the Russell 2000 increases both institutional ownership and passive ownership by large amounts.

Dependent variable* = Simulated percent of stock *i* held by passive investors in reconstitution year *t

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	+2.43***	+2.43***	+2.47***	+2.32***	+2.32***	+2.39***	+1.98***	+1.98***	+2.23***	+1.61***	+1.61***	+1.88***
(1 if Russell Rank > 1000)	(0.13)	(0.13)	(0.17)	(0.10)	(0.10)	(0.12)	(0.06)	(0.06)	(0.08)	(0.05)	(0.05)	(0.06)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.21	0.21	0.21	0.19	0.19	0.19	0.18	0.18	0.18	0.19	0.19	0.19

Dependent variable* = Simulated percent of stock *i* held by institutional investors in reconstitution year *t

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	+31.0***	+31.0***	+32.0***	+28.8***	+28.8***	+30.3***	+21.3***	+21.3***	+26.8***	+13.3***	+13.3***	+19.2***
(1 if Russell Rank > 1000)	(2.78)	(2.78)	(3.75)	(2.06)	(2.06)	(2.58)	(1.38)	(1.38)	(1.73)	(1.00)	(1.00)	(1.28)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.21	0.23	0.23	0.14	0.16	0.16	0.06	0.07	0.07	0.02	0.02	0.03

Table 4
Fuzzy RD estimates using simulated data

Description: This table uses the simulated data to estimate the fuzzy RD estimation described in Equations (3) and (4). Specifically, we estimate an instrumental variable estimation where the first and second stage estimations are given by

$$R2000_{it} = \beta + \lambda Treatment_{it} + \sum_{n=1}^{N} \varphi_n (Rank_{it} - 1000)^n + \mu_{it} \text{ and } Passive\%_{it} = \alpha + \gamma \widehat{R2000}_{it} + \sum_{n=1}^{N} \phi_n (Rank_{it} - 1000)^n + \varepsilon_{it},$$

respectively, where *Passive%* is the simulated the percent of a stock *i*'s ownership held by passive investors (*passive*) in that reconstitution year *t*, *Treatment* is an indicator equal to 1 if a firm's simulated CRSP market cap ranking exceeds 1000, *R2000* is an indicator for inclusion in the simulated Russell 2000 index for firm *i* after the annual reconstitution in year *t*, *Rank* is the firm's market cap ranking when using the simulated CRSP market caps to construct the ranking (*crsp_rank*), and *N* is the polynomial order we use to control for (*Rank* - 1000). The model is estimated over the nine simulated annual reconstitutions that occur from 1998 to 2006 using a polynomial order control of *N*=1, 2, and 3. The top panel reports the first stage estimates, while the bottom panel reports the second state IV estimates for passive ownership. Columns (1)-(3) use a sample bandwidth of 50 stocks (i.e., $Rank \in [951,1050]$); Columns (4)-(6) use a sample bandwidth of 100 stocks (i.e., $Rank \in [901,1100]$); Columns (7)-(9) use a sample bandwidth of 250 stocks (i.e., $Rank \in [751,1250]$), and Columns (10)-(12) use a sample bandwidth of 500 stocks (i.e., $Rank \in [501,1501]$). Standard errors are clustered at the stock level and reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Interpretation: Because there is no discontinuity in the probability of being assigned to the Russell 2000 when using a noisy measure of the true ranking variable, the first stage of the fuzzy RD estimation can fail, particularly at smaller bandwidths, and the IV estimation will not uncover the true effect of index assignment on passive ownership.

First stage: ***Dependent variable*** **= Indicator for stock *i's* inclusion in the Russell 2000 index in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	–0.02	–0.02	–0.12***	+0.32***	+0.32***	+0.03	+0.65***	+0.65***	+0.47***	+0.79***	+0.79***	+0.69***
(1 if CRSP Rank > 1000)	(0.05)	(0.04)	(0.05)	(0.04)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.47	0.59	0.60	0.63	0.68	0.70	0.77	0.78	0.79	0.84	0.84	0.84

IV Estimation: ***Dependent variable*** **= Simulated percent of stock *i* held by passive investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	+4.84	+4.84	+1.32	+1.23***	+1.23***	+3.28	+1.11***	+1.11***	+1.30***	+1.06***	+1.06***	+1.02***
(1 if Russell Rank > 1000)	(15.52)	(14.09)	(1.50)	(0.31)	(0.31)	(5.72)	(0.10)	(0.10)	(0.19)	(0.06)	(0.06)	(0.09)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.21	0.23	0.23	0.14	0.16	0.16	0.06	0.07	0.07	0.02	0.02	0.03

Table 5
First stage of fuzzy RD estimates using actual data

Description: This table uses the actual data to estimate the first stage of the fuzzy RD estimation described in Equations (3) using the additional controls recommended by Wei and Young. Specifically, we estimate an instrumental variable estimation where the first and second stages are given by

$$R2000_{it} = \beta + \lambda Treatment_{it} + \sum_{n=1}^{N}[\varphi_n(Rank_{it} - 1000)^n + \theta_n Treatment_{it}(Rank_{it} - 1000)^n] + \mu_{it}$$

where *R2000* is an indicator for inclusion in the Russell 2000 index for firm *i* after the annual reconstitution in year *t*, *Treatment* is an indicator equal to 1 if a firm's CRSP market cap ranking exceeds 1000 on the last trading day in May prior the the annual reconstitution, *Rank* is the firm's market cap ranking when using the end-of-May CRSP market caps, and *N* is the polynomial order we use to control for (*Rank* - 1000) and *Treatment*×(*Rank* - 1000), similar to that of Wei and Young (2018). The model is estimated over the nine annual reconstitutions that occur from 1998 to 2006 using a polynomial order control of *N*=1, 2, and 3. Columns (1)-(3) use a sample bandwidth of 50 stocks (i.e., $Rank \in [951,1050]$); Columns (4)-(6) use a sample bandwidth of 100 stocks (i.e., $Rank \in [901,1100]$); Columns (7)-(9) use a sample bandwidth of 250 stocks (i.e., $Rank \in [751,1250]$), and Columns (10)-(12) use a sample bandwidth of 500 stocks (i.e., $Rank \in [501,1501]$). Standard errors, μ, are clustered at the stock level and reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Interpretation: The fuzzy RD's first stage also often fails to detect a discontinuity in index assignments when using actual data and end-of-May CRSP market cap rankings, especially at smaller bandwidths where it sometimes finds a negative impact of *Treatment* on being assigned to the Russell 2000, which is the opposite of what we should expect. Because of this, the fuzzy RD estimation can suffer from a weak instrument problem and hard to predict biases.

Dependent variable **= Indicator for stock *i's* inclusion in the Russell 2000 index in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	–0.04	–0.25***	–0.034	+0.33***	–0.07*	–0.26***	+0.66***	+0.38***	+0.11***	+0.79***	+0.63***	+0.44***
(1 if CRSP Rank > 1000)	(0.04)	(0.06)	(0.08)	(0.03)	(0.04)	(0.06)	(0.02)	(0.03)	(0.04)	(0.01)	(0.02)	(0.03)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.65	0.67	0.69	0.70	0.77	0.78	0.79	0.82	0.85	0.87	0.88	0.89

Table 6
AGK first stage estimates using simulated data

Description: This table uses the simulated data to estimate the first stage of the AGK (2016) IV estimation in Equation (5). Specifically, we estimate

$$Y_{it} = \eta + \lambda R2000_{it} + \sum_{n=1}^{N} \chi_n (Ln(Mktcap_{it})^n + \sigma Ln(Float_{it}) + \delta_t + u_{it}$$

where *Y* is the simulated outcome of interest for stock *i* in reconstitution year *t*, *R2000* is an indicator for inclusion in the simulated Russell 2000 index for firm *i* after the annual reconstitution in June of year *t*, *Mktcap* is the firm's simulated CRSP market cap (*crsp_mc*), Float is the firm's simulated float-adjusted market cap (*float_mc*), and *N* is the polynomial order we use to control for *Ln*(*Mktcap*). The model is estimated over the nine simulated annual reconstitutions that occur from 1998 to 2006 using a polynomial order control of *N*=1, 2, and 3. Two dependent variables, *Y*, are used. The top panel uses the percent of a stock's ownership held by passive investors in that reconstitution year (*passive*), while the bottom panel uses the simulated percent of a stock's ownership held by institutional investors (*IO*). Columns (1)-(3) use a sample bandwidth of 50 stocks (i.e., *Rank* ∈ [951,1050]); Columns (4)-(6) use a sample bandwidth of 100 stocks (i.e., *Rank* ∈ [901,1100]); Columns (7)-(9) use a sample bandwidth of 250 stocks (i.e., *Rank* ∈ [751,1250]), and Columns (10)-(12) use a sample bandwidth of 500 stocks (i.e., *Rank* ∈ [501,1501]), where *Rank* is the firm's market cap ranking when using the simulated within-index, float-adjusted market caps to construct the ranking (*float_rank*). Standard errors, *u*, are clustered at the stock level and reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Interpretation: Using the baseline specification of AGK (2016), one succesfully recovers the correct impact (non-impact) of index assignment on passive ownership (total institutional ownership) when using the wider bandwidths they use as their baseline specification. The AGK (2016) estimates, however, can be biased in smaller bandwidths because of how they select their sample using the float-adjusted market cap rankings of Russell.

Dependent variable **= Simulated percent of stock *i* held by passive investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	+1.53***	+1.55***	+1.54***	+1.31***	+1.29***	+1.28***	+1.05***	+1.03***	+1.03***	+1.01***	+1.00***	+0.99***
(1 if Russell Rank > 1000)	(0.12)	(0.12)	(0.12)	(0.07)	(0.07)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.53	0.53	0.53	0.46	0.47	0.47	0.35	0.35	0.35	0.30	0.30	0.30

Dependent variable **= Simulated percent of stock *i* held by institutional investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	+11.4***	+11.9***	+'11.8***	+6.68***	+6.36***	+6.12***	+1.11	+0.60	+0.57	+0.22	−0.10	−0.15
(1 if Russell Rank > 1000)	(2.59)	(2.53)	(2.50)	(1.44)	(1.41)	(1.41)	(0.92)	(0.90)	(0.92)	(0.83)	(0.83)	(0.84)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.25	0.26	0.26	0.19	0.20	0.20	0.12	0.12	0.12	0.08	0.08	0.08

Table 7
AGK first stage estimates using simulated data and their alternative sampling approach

Description: This table uses the simulated data to estimate the same first stage of the AGK (2016) IV estimation as done in Table 6, but instead selects the sample using CRSP market cap rankings rather than float-adjusted rankings. Specifically, we estimate

$$Y_{it} = \eta + \lambda R2000_{it} + \sum_{n=1}^{N} \chi_n (Ln(Mktcap_{it})^n + \sigma Ln(Float_{it}) + \delta_t + u_{it}$$

where Y is the simulated outcome of interest for stock i in reconstitution year t, $R2000$ is an indicator for inclusion in the simulated Russell 2000 index for firm i after the annual reconstitution in June of year t, *Mktcap* is the firm's simulated CRSP market cap (*crsp_mc*), Float is the firm's simulated float-adjusted market cap (*float_mc*), and N is the polynomial order we use to control for *Ln*(*Mktcap*). The model is estimated over the nine simulated annual reconstitutions that occur from 1998 to 2006 using a polynomial order control of N=1, 2, and 3. Two dependent variables, Y, are used. The top panel uses the percent of a stock's ownership held by passive investors in that reconstitution year (*passive*), while the bottom panel uses the simulated percent of a stock's ownership held by institutional investors (*IO*). Columns (1)-(3) use a sample bandwidth of 50 stocks (i.e., $Rank \in [951{,}1050]$); Columns (4)-(6) use a sample bandwidth of 100 stocks (i.e., $Rank \in [901{,}1100]$); Columns (7)-(9) use a sample bandwidth of 250 stocks (i.e., $Rank \in [751{,}1250]$), and Columns (10)-(12) use a sample bandwidth of 500 stocks (i.e., $Rank \in [501{,}1501]$), where *Rank* is the firm's market cap ranking when using the simulated CRSP market caps to construct the ranking (*crsp_rank*). Standard errors, u, are clustered at the stock level and reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Interpretation: Using the AGK (2016) estimation with their alternative sampling approach, one succesfully recovers the correct impact (non-impact) of index assignment on passive ownership (total institutional ownership) for all bandwidths.

Dependent variable **= Simulated percent of stock *i* held by passive investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	+0.86***	+0.86***	+0.86***	+1.00***	+1.00***	+1.00***	+0.98***	+0.98***	+0.98***	+0.99***	+1.00***	+1.00***
(1 if Russell Rank > 1000)	(0.08)	(0.09)	(0.09)	(0.07)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.28	0.28	0.28	0.28	0.28	0.28	0.24	0.24	0.24	0.25	0.25	0.25

Dependent variable **= Simulated percent of stock *i* held by institutional investors in reconstitution year *t***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
R2000	−3.05*	−2.98	−3.05*	+0.01	+0.01	+0.03	−0.35	−0.39	−0.39	−0.14	−0.09	−0.02
(1 if Russell Rank > 1000)	(1.83)	(1.84)	(1.85)	(1.52)	(1.52)	(1.52)	(1.09)	(1.09)	(1.09)	(0.83)	(0.84)	(0.84)
Sample bandwidth	50	50	50	100	100	100	250	250	250	500	500	500
Polynomial order, *N*	1	2	3	1	2	3	1	2	3	1	2	3
# of observations	100×9	100×9	100×9	200×9	200×9	200×9	500×9	500×9	500×9	1000×9	1000×9	1000×9
R-squared	0.13	0.13	0.13	0.11	0.11	0.11	0.09	0.09	0.09	0.08	0.08	0.08

Table 8
Average change in end-of-May CRSP market cap ranking by sample period and index

Description: This table reports the average change in end-of-May CRSP market cap rankings prior to reconstitution for both the pre- and post-banding periods (1998-2006 versus 2007-2014) for the bottom 250 stocks of the Russell 1000 index and top 250 stocks of the Russell 2000. Standard deviations are reported below the averages in parantheses. The change in end-of-May CRSP market cap ranking is calculated using the change in ranking from end-of-May in year *t-1* to end-of-May year *t* for each reconstitution year *t*. A ranking of 1 is assigned to the stock with the largest end-of-May CRSP market cap, a ranking of 2 is assigned to the stock with the second largest end-of-May CRSP market cap, and so on. A stock that moves from a ranking of 1,050 in year *t* to a ranking of 950 in year *t* would have a change in end-of-May market cap of -100. In other words, negative changes in rankings reflect stocks that became relatively larger in market cap over the last year. The *p*-values for the difference in means across the two indexes by sample period are also reported.

Interpretation: After the Russell banding policy was implemented (after 2006), the average change in market cap in the year before reconstitution increased for stocks at the top of the R2000, but decreased for stocks at the bottom of the R1000; i.e., fewer stocks on either side the boundary switched index membership because of the prior year's change in market cap. This is in contrast to the years before the banding policy (before 2007).

Time period =	Pre-banding period [1998-2006]	Post-banding period [2007-2014]
Bottom 250 stocks of Russell 1000	–90.6	+47.3
	(300.1)	(207.6)
Top 250 stocks of Russell 2000	–106.2	–134.1
	(345.4)	(260.3)
Difference =	15.6	181.4
Difference *p* -value =	0.145	0.000

Appendix Table 1

List of papers that use the Russell setting for identification by type of estimation strategy

Description: This table categorizes the numerous papers that use Russell index assignments as an identification strategy. The categories reflect the different identification strategies discussed in Sections 3.2.2-3.2.6 of the paper. Because many papers use a combination of strategies, we categorize each paper based on their main approach. Additionally, because many papers make small (but sometimes important) changes to the methodology described in Sections 3.2.2-3.2.6, we also provide a brief explanation of how their estimation differs and why these differences are important. Finally, because authors of working papers occasionally change their estimations before publication, we provide the publication status of each paper, and for unpublished papers, we provide the date to indicate the version we used. [For example, earlier versions of Crane, Michenaud, and Weston (2016) used the problematic sharp regression discontinuity described in Section 3.2.2, but the published version of that paper instead used the problematic sharp RD described in Section 3.2.3.]

Interpretation: Many estimation strategies are used in the literature, and most of them are problematic.

Papers that employ a "sharp regression discontinuity using Russell's observable June float-adjusted rankings"
[see Section 3.2.2 for an explanation on why this estimation is problematic]

Boone and White (2015) *Journal of Financial Economics*	This appears to be the first paper to use this problematic estimation strategy as all the other papers in this category cite this paper's proposed methodology. As a robustness check, the authors also use the same problematic sharp RD as the first stage of an IV estimation [see Table 6 of their paper], which will suffer from the same biases discussed in Section 3.2.2. Additionally, the second stage of their IV estimation appears to exclude the *Rank* controls [see Eqn. (2) of their paper], indicating that their IV estimation is also using the June float-adjusted rankings as an instrumental variable, which is clearly problematic as well. As an additional robustness check, the authors also do a pre- versus post-switch comparison of stocks that change index, but these comparisons are problematic as well because they fail to control for the change in market cap that drove these switches [see Section 3.2.6 for a discussion of switcher estimations.]
Chen, Huang, Li, and Shevlin (2019) *Journal of Accounting and Economics*	Consistent with the paper's main estimates being biased because of the issues discussed in Section 3.2.2, the paper's internet appendix shows that the paper's findings are not robust to using the IV approach of AGK (2016) and "much weaker" when using an estimation that is similar to that of a fuzzy RD.
Cheung, Im, and Zhang (2017) Unpublished manuscript [April 2017]	Paper also uses the problematic sharp RD as the first stage of an IV estimation, which will be problematic for the same reasons.
Fang (2018a) Unpublished manuscript [July 2018]	The paper also claims it findings are robust to using the sharp RD estimation of Crane et al. (2016) [see Section 3.2.3. for explanation on why this estimation is also problematic], but it actually appears that the paper instead uses the same sharp RD with June float-adjusted ranks as the first stage of its IV estimation, which will be problematic for the same reasons.

Fang (2018b) Unpublished manuscript [July 2018]	Same comment as for Fang (2018a).
Khan, Srinivasan, and Tan (2017) *The Accounting Review*	The paper also reports a robustness test that uses a before versus after comparison of index switchers, but this comparison is problematic because there is no control group (i.e., counterfactual) or attempt to control for the change in market cap ranking that led to this switch in index [see Section 3.2.6 for an explanation of why such switcher comparisons are problematic].
Lin, Mao, and Wang (2018) *The Accounting Review*	In addition to using the sharp RD with June float-adjusted ranks, the paper also does an IV estimation similar to that of AGK (2016) but fails to control for the the differences in end-of-May market caps, which will likely lead to a violation of the exclusion restriction [see Section 3.2.5 for details]. The paper also does a problematic comparison of switchers to non-switchers that fails to control for the change in market cap that drives these switches [see Section 3.2.6 for details on why such an estimation is problematic].
Wong and Yi (2017) Unpublished manuscript [September 2017]	Paper also uses the problematic sharp RD as the first stage of an IV estimation, which will be problematic for the same reasons. Moreover, it seems the IV specification also fails to control for *Rank* in the second stage (which will cause additional problems). The paper also later does a comparison of switchers to non-switchers, but fails to control for the change in market cap causing these index switches [see Section 3.2.6 for an explanation of why this is problematic].

Papers that employ a "sharp regression discontinuity using observable within-index, end-of-May rankings"
[see Section 3.2.3 for an explanation on why this estimation is problematic]

Baghdadi, Bhatti, Nguyen, and Podolski (2018) *Journal of Banking & Finance*	While the paper claims to follow sharp RD estimation of Crane et al. (2016) [see Section 3.2.3. for an explanation on why that estimation is biased], the paper's first stage estimates suggest the paper is actually using a sharp RD with June float-adjusted rankings [see Section 3.2.2 for an explanation on why that estimation is biased.] The figures used to justify the sharp RD also use June float-adjusted ranks. As robustness check, paper also uses the problematic switcher IV estimation of Schmidt and Fahlenbrach (2017) [see Section 3.2.6 for explanation on why this estimation is biased].
Chen, Dong, and Lin (2019) Unpublished manuscript [Oct. 2019]	Their main estimation follows Crane et al. (2016) and is problematic for the same reasons (see Section 3.2.3 for details on why that estimation is biased). They claim, however, that their results are also robust to using the approach of AGK (2016), but it is unclear how that could be possible when AGK (2016) estimation fails to detect an affect on institutional ownership (which is what Chen, Dong, and Lin (2020) claim is driving their findings).
Chen, Dong, and Lin (2020) *Journal of Financial Economics*	Same comment as Chen, Dong, and Lin (2019).

Crane, Michenaud, and Weston (2016) *Review of Financial Studies*	This appears to be the first paper to use this problematic estimation strategy as all the other papers in this category cite this paper's proposed methodology. See Section 3.2.3 for an explanation for why this paper's approach is problematic.

Papers that employ a "fuzzy regression discontinuity"
[see Section 3.2.4 for a description of this estimation method and its advantages and disadvantages]

Ben-David, Franzoni, and Moussawi (2019) Unpublished manuscript [Oct. 2019]	This appears to be a properly estimated fuzzy RD. The paper also provides details on how one can better approximate the unobserved market caps being used by Russell to determine index assignments, which is necessary to avoid a weak IV in the first stage of the fuzzy RD estimation [see Section 3.2.4 for details].
Cao, Gustafson, and Velthuis (2019) *Management Science*	This appears to be a properly estimated fuzzy RD. Moreover, rather than use the cutoff between the Russell 1000 and Russell 2000 indexes for identification (as done by all other papers in this literature), this paper instead uses the cutoff between the Russell 2000 and Russell Micro indexes. To our knowledge, this is the only paper to make use of this alternative threshold.
Mullins (2014) Unpublished manuscript [Jan. 2014]	While the fuzzy RD estimation is executed properly, it appears to suffer from a weak instrumental variable bias. For example, the paper claims that assignment to the Russell 2000 index causes a counterintuitive reduction in institutional ownership. This appears to be driven by the paper's use of Russell-provided market caps to construct *Rank* . These market caps, which are not used in other fuzzy RD papers, are particularly noisy predictor of index assignment near the threshold (e.g., see Figure 2 of that paper), which results in a weak IV and biases [see Section 3.2.4 for a discussion of this fuzzy RD weakness].

Papers that employ an "instrumental variable estimation following AGK (2016)"
[see Section 3.2.5 for a description of this estimation method and its advantages and disadvantages]

Appel, Gormley, and Keim (2016) *Journal of Financial Economics*	This is the first paper to use this estimation strategy as all the other papers in this category cite this paper's proposed methodology.
Appel, Gormley, and Keim (2019) *Review of Financial Studies*	Uses same approach as AGK (2016), but because it is using a post-banding sample period (i.e., years 2007-2014), it adds additional controls to the AGK (2016) IV estimation. See Section 4 for a discussion of why those additional controls are required when using the AGK approach in the post-banding period.
Palia and Sokolinkski (2019) Unpublished manuscript [Aug. 2019]	Uses same approach as AGK (2019), but it is unclear if the authors have tested the robustness of their findings to the altenative sampling approach proposed in AGK (2016) and Glossner (2018). See Section 3.2.5 for of discussion of why this sampling approach can be important.

Papers that employ "estimations that make use of index switchers"
[See Section 3.2.6 for a warning about these type of estimation strategies]

Ben-David, Franzoni, and Moussawi (2018) *Journal of Finance*	This paper follows the approach of Chang, Hong, and Liskovich (2015) and hence, is subject to the same identification concerns as that approach [see the below entry for Chang, Hong, and Liskovich (2015)]. However, in a later unpublished note to their published article, the authors show that their findings are robust to a properly estimated fuzzy RD. See the above entry for Ben-David, Franzoni, and Moussawi (2019) for details about that paper's approach.
Chang, Hong, and Liskovich (2015) *Review of Financial Studies*	This paper is using a fuzzy RD estimation that attempts to identify the effect of index assignment using only variation from stocks that switch indexes. However, rather than insert firm fixed effects into the fuzzy RD estimation, which would be one way to properly isolate variation from index switchers [see Section 3.2.4 for details], the authors instead condition their sample each year based on the pre-reconstitution index assignments. Specifically, they separately estimate a fuzzy RD on two samples: (1) stocks in the Russell 1000 before reconstitution [which they call the "addition effect" estimation since the impact of Russell 2000 inclusion will be estimated by comparing Russell 1000 stocks that switched to the Russell 2000 versus those that remained in the Russell 1000], and (2) stocks in the Russell 2000 before reconstitution [which they call the "deletion effect" estimation since it will be estimated by comparing stocks that were kept in the Russell 2000 versus those that jumped to the Russell 1000]. Hence, their use of ex ante index assignment to select the sample implicitly turns the fuzzy RD estimation into a switcher-type estimation. A problem with this modification of the fuzzy RD, however, is that it does not control for the determinant of index switches, the change in market cap ranking over the last year. For example, Russell 1000 stocks that switch to the Russell 2000 must have, on average, exhibited a larger relative drop in market cap ranking over the past year than Russell 1000 stocks that did not switch indexes. This, however, is not controlled for in their estimation, resulting in a possible violation of the exclusion restriction since their the fuzzy RD's instrument, *Treatment* , is mechanically driven by these past movements on market cap ranking.

Coles, Heath, and Ringgenberg (2020) Unpublished manuscript [April 2020]	This paper uses a difference-in-differences estimation that compares the post-switch change in outcomes of switchers to that of non-switchers. The estimation's identifying assumption is that the outcome for switchers would have trended the same as that of non-switchers in the post-period absent the index switch. A concern with this assumption, however, is that switchers are, on average, inherently different than non-switchers. Index assignment is determined by market cap, and switches are determined by past market cap changes. Therefore, switchers will be different in terms of their total market cap and past movement in their relative market cap ranking. If these differences are potential drivers of the outcome in the post-switch period, the underlying parallel trends assumption would be violated. While the differences in market cap are implicitly controlled for in the authors' choice to only includes stocks near the threshold, the past differences in market cap movements are not. To do this, their estimation should include robust controls for each stock's past change in market cap ranking (or market cap) interacted with the post-assignment dummy. Doing so would ensure the estimation is comparing stocks with similar past movements in market caps as claimed. Similar controls for pre-assignment end-of-May market cap interacted with the post-assignment dummy should also be included since the market caps of switchers and non-switchers are systematically different even when focusing on stocks near the threshold for switching. The difference-in-differences approach does not eliminate the need to control for the determinant of index assignments (or switches) as claimed in Section III of their paper. Treatment in the Russell 1000/2000 setting is known to not be exogenous in ways that could be problematic, and therefore, its determinants should be controlled for (see Section 2 for a discussion of this issue). Other potential weaknesses of such switcher estimations are discussed in Section 3.2.6.
Heath, Macciocchi, Michaely, Ringgenberg (2020) Unpublished manuscript [March 2020]	This paper uses a difference-in-differences type estimation similar to that of Coles, Heath, and Ringgengberg (2020), and therefore, Tables 8 & 10 suffer from the same concerns [see above].
Schmidt and Fahlenbrach (2017) *Journal of Financial Economics*	Rather than add firm fixed effects to either the fuzzy RD estimation or AGK (2016) IV estimation, which would be two alternative ways to properly implement an estimation that is identified only using only index switchers [see Sections 3.2.4 and 3.2.5 for details], this paper employs a first differences IV approach. While there is nothing wrong conceptually with this approach, the paper's estimator is problematic because the second stage of the IV estimation does not control for the change in market cap ranks. Therefore, these changes in market cap rankings are being used as an instrument, not as a control, which the authors acknowledge on page 292 of their paper. Using the change in market cap as an IV is inappropriate because it may directly drive many outcomes of interest or correlate with other factors that drive changes in ownership and other outcomes of interest. In other words, one would be hard-pressed to argue that changes in market cap rankings are a valid instrument of ownership, as this estimation strategy assumes. Another potential concern with the specification of Schmidt and Fahlenbrach (2017) is that it does not attempt to robustly control for changes in market cap by testing the robustness of the findings to using a second- or third-order polynomial set of controls like other papers in this literature.

Papers that employ problematic estimations that do not directly follow any of the above approaches

Bird and Karolyi (2016) *Review of Financial Studies* [Correction Issued]	Published version of paper claimed to be doing something that is a mixture of the fuzzy RD and AGK (2016). Specifically, the paper claims to use an IV estimation similar to that of AGK (2016), except that the *Ln(mktcap)* controls are replaced with controls for (*Rank* -1000), where the *Rank* is calculated using end-of-May CRSP market caps. In true, this would have been a reasonable estimation approach [see last paragraph of Section 3.2.4 and Section 3.2.5 for details]. However, the reported first stage estimates and figures instead seem to follow the problematic approach of Crane et al. (2016) when calculating *Rank* [see Section 3.2.3 for an explanation on why that estimation is problematic]. Furthermore, a correction issued by the publishing journal in January 2020 noted that an investigation by the journal revealed that the authors misrepresented the actual estimation being used in another way. According to the correction, the authors excluded the market cap rankings from the second stage of their IV estimation. I.e., they used market cap rankings as an instrumental variable for instititutional ownership, which is clearly problematic (as acknowledged in the published correction). The correction further states that the paper's findings were not robust when using the specifications reported in the paper.
Bird and Karolyi (2017) *The Accounting Review* [Retracted]	Paper claims to follow the problematic approach of Crane et al. (2016) [see Section 3.2.3 for an explanation on why that estimation is problematic], but a careful look at the reported estimation reveals that the authors are excluding the *Rank* controls from the second stage of the estimation (see Eqn. (2) of their paper), meaning, if true, they were actually using market cap rankings as an instrument for institutional ownership, which is clearly problematic and similar to the problem identified in the journal investigation of Bird and Karolyi (2016) [see above note for details]. Furthermore, the article was later retracted following an investigation by the publishing journal where the authors acknowledged to instead using the problematic sharp RD with June float-adjusted rankings as their specification [see Section 3.2.2 for details on why that estimation is problematic] while simultaneously failing to provide the code that could reproduce their findings even using that problematic specification.