Quasi-Indexer Ownership and Insider Trading: Evidence from Russell Index Reconstitutions*

Stephen A. Hillegeist† and Liwei Weng‡

Abstract: Understanding the association between quasi-indexer ownership and insider trading is important given the externalities that insider trading can impose on shareholders, the importance of quasi-indexers in the capital markets, and their mixed monitoring incentives. The prior literature has produced an inconsistent set of results regarding this association. These results are difficult to interpret because the association between them is likely endogenous and prior studies have not employed effective identification strategies to address this issue. In this study, we examine the effects of quasi-indexer institutional ownership on insider trading using the plausibly exogenous discontinuity in quasi-indexer ownership around the Russell 1000/2000 index cutoff. Using both regression discontinuity and instrumental variable research designs, we find higher quasi-indexer ownership leads to less insider trading (both buys and sells) and less profitable sell trades. The effects for sells are concentrated among insider trades that, *ex ante*, are more likely to be based on private information. Our evidence on the profitability of buys is mixed. In addition, we find firms with higher quasi-indexer ownership are more likely to have and/or more strictly enforce blackout policies. Overall, our results suggest that quasi-indexers can reduce the agency costs associated with insider trading through their direct and indirect monitoring activities.

JEL Classification: G14, G23, G34, D82

Keywords: quasi-indexers; insider trading; blackout policies; causal effect; regression discontinuity

Please see supporting information, "Internet Appendix for Quasi-Indexer Ownership and Insider Trading: Evidence from Russell Index Reconstitutions," as an addition to the online article.

* Accepted by Hai Lu. We acknowledge helpful comments from Hai Lu (editor), Alan Webb (editor-in-chief), two anonymous reviewers, Jennifer Brown, Lyungmae Choi, Lucile Faurel, Steve Kaplan, Michal Matejka, Terry Walter, seminar participants at Arizona State University, the University of Auckland, the University of Tasmania, and the University of Western Australia, and conference participants at the University of Technology Sydney 2017 Summer Accounting Conference and the 2017 University of Arizona-Arizona State University Joint Research Conference. We would like to thank Russell Investments for providing the Russell index data. We are grateful to Jason Chao of Russell Investments for insightful discussions on the Russell index methodology. The authors appreciate financial support from Arizona State University and the Hong Kong Polytechnic University.

† Corresponding author: W. P. Carey School of Business, Arizona State University. Address: 300 East Lemon Street, Tempe, AZ, USA, 85287. Office: (480) 965-6614. Email: Stephen.Hillegeist@ASU.edu.

‡ School of Accounting and Finance, The Hong Kong Polytechnic University. Address: Li Ka Shing Tower, The Hong Kong Polytechnic University, Hung Hom, Kowloon, Hong Kong. Office: (+852) 2766-7053. Email: Liwei.Weng@PolyU.edu.hk.

1. Introduction

Quasi-indexers follow more passive investment styles that frequently seek to mimic the performance of stock indexes.[1] While quasi-indexers cannot easily "vote with their feet" by selling their holdings, they have fiduciary duties and fee-based incentives to actively monitor and influence managerial actions (Lewellen and Lewellen 2020). Recent studies using the Russell index setting conclude quasi-indexers are active monitors who have a positive effect on certain governance and corporate practices (e.g., Appel et al. 2016; Boone and White 2015; Crane et al. 2016). However, the evidence in Schmidt and Fahlenbrach (2017) indicates that while quasi-indexers may be effective at relatively simple monitoring activities, they are unlikely to be effective in more complex and nuanced settings. Thus, it is unclear to what extent quasi-indexers can effectively monitor a relatively nuanced and complex activity such as insider trading. In this study, we examine how quasi-indexer ownership affects insider trading in a setting that allows us to more precisely identify causal effects.

Despite legal prohibitions, prior literature consistently finds insiders' trades are often based on private information as evidenced by the abnormal profits their trades earn on average. When insiders trade on the basis of their private information, their activities have important implications for shareholders. On the costs side, informed insider trading is associated with higher litigation risk (Jagolinzer and Roulstone 2009; Johnson et al. 2000) and higher information asymmetry (Billings and Cedergren 2015). On the benefits side, insider trading can provide stronger incentives for managers to increase firm value (Carlton and Fischel 1983; Leland 1992). It can also allow firms to reduce CEO compensation (Henderson 2011; Roulstone

[1] Quasi-indexers are a type of institutional investors that have investment styles characterized by low portfolio turnover and highly diversified holdings. This designation includes both pure index funds and actively managed diversified funds that benchmark indices. Roughly 67% of all institutional shareholdings are held by quasi-indexers.

2003). Quasi-indexers will weigh the expected costs and benefits when determining whether and to what degree they should monitor or restrict insider trading. Based on the limited anecdotal evidence and the weight of extant empirical evidence, we expect shareholders in general, and quasi-indexers in particular, prefer less information-based insider trading. Thus, we hypothesize that higher levels of quasi-indexer ownership will lead to less trading by insiders.

In order to more precisely identify the causal effects of quasi-indexer ownership on insider trading, we analyze plausibly exogenous differences in quasi-indexer ownership caused by annual reconstitutions of the Russell 1000 (R1000) and Russell 2000 (R2000) stock indexes.[2] The R1000 includes the 1000 largest US-listed firms by market capitalization while the R2000 tracks the next 2000 largest firms. During the reconstitution, firms in a narrow bandwidth around the R1000/R2000 threshold are quasi-randomly assigned to an index based on their relative market capitalizations. Index tracking and benchmarking behavior generates large and plausibly exogenous variation in quasi-indexer ownership across the threshold.

We first investigate whether exogenous differences in quasi-indexer ownership around the index threshold lead to differences in the frequency and profitability of insider trading. Using regression discontinuity (RD) and instrumental variables (IV) research designs, we find that around the index threshold, higher quasi-indexer ownership for R2000 firms results in fewer insider trades (both sells and buys). The effects are both statistically and economically significant. We also categorize insider trades as routine or non-routine using the methodology in Cohen et al. (2012). The reductions in sell frequency are concentrated among non-routine trades,

[2] The Russell index setting has been used in several recent finance and accounting studies. Some of these studies focus on total institutional ownership, others on quasi-indexer ownership, and still others on mutual funds that specifically benchmark the R2000 or R1000. For expositional ease, we generically refer to these studies as pertaining to quasi-indexers because differences in institutional ownership around the R1000/R2000 threshold are mostly due to differences in quasi-indexer ownership. Wei and Young (2017) and Young (2018) question the validity of a subset of these studies due to certain methodological choices. We note that their primary concern does not apply to our study.

i.e., those most likely to be based on private information. Our evidence indicates higher quasi-indexer ownership results in insiders making fewer trades based on private information.

We expect monitoring by quasi-indexers affects average trade profitability in two ways. First, it decreases average trade profitability because it reduces the likelihood a trade is based on private information. Second, more intensive monitoring increases the average profitability of privately-informed insider trades because it increases the required expected return in order for insiders to trade on their private information. Given the conflicting forces, we do not make a directional prediction. We find higher quasi-indexer ownership leads to lower average sell profitability. The RD analyses indicate the results are concentrated among non-routine sells. The lower abnormal returns indicate fewer sell transactions are based on private information when quasi-indexer monitoring is higher, and hence, the first effect dominates. Our RD analyses provide some evidence that higher quasi-indexer ownership increases buy profitability, especially among non-routine trades. In contrast, our IV results indicate that buy profitability is not significantly affected by quasi-indexer ownership.

Bettis et al. (2000) find most firms have formal "blackout" policies that prohibit trading by insiders during periods leading up to and including quarterly earnings announcements. However, these policies are frequently ineffective as about 24% of all insider trades occur within restricted trade windows (Jagolinzer et al. 2011). Thus, firms often fail to enforce their policies or frequently grant exemptions to them. Firms can reduce informed insider trading by adopting new blackout policies or more strictly enforcing existing ones. In either case, we expect fewer insider trades will occur during blackout periods when quasi-indexer ownership is higher. We find both the fraction of insider trades made during the non-restricted "safe" periods and the likelihood of having an inferred blackout policy are higher when quasi-indexer ownership is

higher. Thus, our evidence indicates one way in which quasi-indexers affect insider trading is through firms adopting and/or more strictly enforcing policies that restrict insider trading.

Finally, we examine whether the effects of quasi-indexer ownership on insider trading are driven by quasi-indexers directly monitoring insider trading or due to indirect effects via one or more mediating variables. As discussed in Section 2, there is only limited anecdotal evidence that quasi-indexers directly lobby firms about insider trading. We think this is reasonable given the complexity involved in determining optimal insider trading policies. Instead, it is more likely that quasi-indexers indirectly influence insider trading. For example, quasi-indexers could push for better governance practices (Appel et al. 2016), and one way in which firms respond to these efforts is by instituting policies that reduce informed insider trading.

To identify the underlying mechanisms, we examine several corporate governance, information environment, and incentive variables that are both affected by quasi-indexer ownership and plausibly related to insider trading. Our results indicate that the percentage of independent directors and four incentive variables act as mediators for at least two insider trading variables. Thus, differences in quasi-indexer ownership around the index threshold cause changes in these variables, which in turn affect insider trading. With the exception of the frequency of insider sells, one or more of these variables totally mediate the effect of quasi-indexer ownership on the insider trading variables.

Prior literature has examined the association between institutional ownership and insider trading, including Bricker and Markarian (2015), Huddart and Ke (2007), and Skaife et al. (2013). As discussed in Section 2, these studies have produced a mixed set of results. Institutional ownership is likely endogenous with other factors that directly affect insider trading. For example, institutional investors are likely attracted to firms with better governance

characteristics or other factors that directly or indirectly affect the extent to which insiders trade on private information (Chung and Zhang 2011). As prior studies have not effectively addressed this issue, one reason why their results are mixed is likely due to unresolved endogeneity issues. We contribute to this literature by producing more reliable causal inferences of how quasi-indexer ownership influences insider trading. We also extend this literature by analyzing how quasi-indexer ownership affects the likelihood that firms have effective blackout policies.

We also contribute to the literature by examining whether quasi-indexers affect insider trading directly or indirectly. These analyses provide a deeper and more nuanced understanding of the forces that can effectively constrain informed insider trading. This understanding is important because such trading can adversely affect shareholders and the capital markets.

2. Prior literature and hypothesis development

Quasi-indexers' preferences regarding privately-informed insider trading

Whether privately-informed insider trading is something quasi-indexers would choose to monitor and/or restrict is not immediately clear (see Bhattacharya (2014) for a review). Allowing insiders to trade on private information can be beneficial to shareholders for several reasons. Trading impounds insiders' private information in share prices, and hence, increases stock price efficiency (Manne 1966). More efficient stock prices benefit shareholders by reducing information asymmetry and improving liquidity. Insider trading also improves incentive alignment by allowing insiders to profit from their efforts to increase shareholder wealth (Carlton and Fischel 1983; Leland 1992). Shareholders also benefit from insider trading because they can reduce executive compensation (Henderson 2011; Roulstone 2003).

In contrast, there are important negative externalities for shareholders that quasi-indexers should consider when deciding whether to monitor insider trading. Insider trading is positively

associated with the cost of equity capital (Beny 2005; Bhattacharya and Daouk 2002; Masson and Madhavan 1991). Insider trading is associated with more earnings management, which has negative implications for shareholders (Beneish et al. 2012; Elitzur and Yaari 1995; Park and Park 2004). Insider trading incentives also induce managers to manipulate voluntary disclosures (Billings and Cedergren 2015; Cheng and Lo 2006; Rogers 2008). These actions can reduce liquidity and increase information asymmetry and the cost of capital. Insider trading is positively associated with class-action litigation risk (Jagolinzer and Roulstone 2009; Johnson et al. 2000).

Given the mixed arguments, we search for anecdotal evidence of institutional investor preferences (not necessarily quasi-indexers) regarding insider trading. Among the few examples is the California Public Employees' Retirement System (CalPERS). CalPERS admonishes in its 2016 Global Governance Principles that "Individuals should not benefit directly or indirectly from knowledge which is not generally available to the market." Positions taken by proxy advisors may indirectly reflect the views of quasi-indexers (Larcker et al. 2015; Malenko and Shen 2016). Both Institutional Shareholder Services and Glass Lewis advocate against allowing insider trading based on material non-public information in their voting guidelines. Thus, the limited anecdotal evidence indicates quasi-indexers are opposed to informed insider trading.

While the lack of public statements could indicate most institutions do not have strong preferences regarding insider trading, most institutions prefer to express their opinions through informal, private discussions with management and directors (Appel et al. 2016; McCahery et al. 2016). Large institutions such as BlackRock, Fidelity, and Vanguard emphasize their informal engagement and frequent meetings with firms' management. Thus, the lack of public statements may reflect their preference for private communications.

Prior empirical research on institutional ownership and insider trading is mixed and

difficult to interpret due to endogeneity concerns.[3] Huddart and Ke (2007) find a negative association between institutional ownership and abnormal returns following insider trades. However, opposite to their prediction, they find institutional ownership is positively associated with the dollar magnitude of insider trading profits. Bricker and Markarian (2015) find institutional ownership is positively associated with the profitability of buy, opportunistic buy, and opportunistic sell transactions but is negatively associated with the profitability of sell transactions. In addition, they find institutional ownership is positively associated with the likelihood that an insider buy is based on private information. In contrast, Skaife et al. (2013) find no significant associations between institutional ownership and trade profitability.

Overall, we expect shareholders in general, and quasi-indexers in particular, prefer less information-based insider trading for two reasons. First, the limited anecdotal evidence available supports this view. Second, while many of the negative implications of insider trading have been documented empirically, most of the beneficial aspects have only been shown theoretically.

Quasi-indexers' ability to influence insider trading

Recent studies (e.g., Appel et al. 2016; Bird and Karolyi 2016; Boone and White 2015; Crane et al. 2016) show quasi-indexers are active monitors who have a positive effect on voluntary disclosures, dividend payouts, and certain governance practices, including board independence, poison pills, and voting rights. A common characteristic of these outcomes is that they result from relatively low-cost, one-size-fits-all monitoring activities that can be uniformly applied to all firms. Thus, we expect monitoring efforts by quasi-indexers are mostly focused on (and most effective in affecting) low-cost standardized activities and practices.

[3] Both Huddart and Ke (2007) and Bricker and Markarian (2015) acknowledge the possible endogeneity between institutional ownership and insider trading and its potential to affect their results but do not employ identification strategies designed to address this issue.

Schmidt and Fahlenbrach (2017) examine more idiosyncratic settings such as board appointments and mergers and acquisitions. These actions are relatively costly for institutions to monitor because they require more nuanced and customized analyses. They find announcement returns to these events are negatively associated with passive institutional ownership. Thus, quasi-indexers are less likely to be effective monitors in more complex and nuanced settings.

We expect directly monitoring insider trading is relatively difficult and costly for quasi-indexers. Quasi-indexers need to weigh time-varying, firm-specific costs and benefits to determine how tightly insider trading should be restricted. Moreover, restricting insider trading directly affects insiders' utility. These managerial-specific incentive issues make it more difficult to directly monitor and curtail information-based insider trading. Thus, monitoring insider trading is a relatively difficult, complex, and hence, costly activity for quasi-indexers.

However, quasi-indexers can indirectly influence insider trading. Many quasi-indexers privately pressure firms to adopt policies to increase their value, but do not advocate for specific policies (Appel et al. 2016; McCahery et al. 2016). Firms could respond to this pressure by reducing informed insider trading.[4] Higher quasi-indexer ownership causes certain corporate governance and disclosure improvements (Appel et al. 2016), which could lead to less frequent insider trading. Finally, higher ownership by quasi-indexers could lead firms to increase insiders' equity-based incentives (Chen et al. 2019). Higher incentives cause insiders to internalize the shareholder costs associated with insider trading, which in turn, reduce its frequency.

In summary, while it may be relatively difficult for quasi-indexers to monitor insider trading directly, we expect their monitoring efforts will indirectly reduce the frequency of insider

[4] Prior literature indicates that at the country level, the cost of capital decreases after insiders are first prosecuted for violating insider trading laws (Bhattacharya and Daouk 2002). Xu (2008) finds positive abnormal returns for stocks that previously had high levels of insider trading around the passage of the Insider Trading Sanctions Act of 1984.

trading based on private information. Accordingly, we make the following hypothesis:

HYPOTHESIS 1. Higher levels of quasi-indexer ownership reduce the frequency of insider trades.

We expect quasi-indexers prefer that fewer insider trades are motivated by private information as such trades are costlier for shareholders. Quasi-indexers are not expected to be (as) concerned with uninformed trades made for liquidity or diversification purposes because they are unlikely to impose substantial costs on shareholders. By definition, informed (uninformed) trades are (not) expected to earn abnormal returns, on average. If their monitoring efforts are successful, then higher quasi-indexer ownership will reduce the likelihood a trade is based on private information. Thus, monitoring reduces the average profitability of all insider trades because a smaller proportion of them will be based on private information.

This monitoring effect will be relatively stronger for sells because litigation risk is higher when insiders sell before stock price declines than when they buy before increases (Cheng and Lo 2006; Huddart et al. 2007; Johnson et al. 2007; Rogers 2008). For example, Billings and Cedergren (2015) find litigation risk is higher when insiders sell before negative earnings news. In contrast, there is little evidence suggesting informed buying increases litigation risk. The asymmetry in litigation risk suggests the reduction in the proportion of informed sells will be greater than that of informed buys, and hence, so will the reduction in average trade profitability.

While decreasing the frequency of privately informed trades, higher levels of quasi-indexer ownership will also increase the average profitability of the remaining information-based trades. Insiders face expected costs from both internal sources (e.g., sanctions for violating the firm's insider trading policy) and external sources (e.g., unfavorable publicity, civil liability, and criminal prosecution). For insiders to be willing to trade, the expected financial gains from trading must exceed the expected costs. More intensive monitoring increases the expected costs

of informed insider trading. Therefore, the expected trading profits must be higher in order to offset the higher costs. Accordingly, we expect the average profitability of insider trades based on private information will increase with quasi-indexer ownership. As above, we expect this required return effect will be stronger for sells compared to buys because quasi-indexers focus their monitoring efforts on privately-informed sells due to asymmetric litigation costs.

In summary, we expect monitoring by quasi-indexers causes two opposing effects on insider trading profitability. First, monitoring reduces the proportion of information-based trades, which reduces average profitability. Second, the average profitability of privately-informed trades increases as insiders require higher expected returns in order to offset the higher monitoring costs. Given the opposing forces, we make the following non-directional hypothesis:

> *HYPOTHESIS 2. Higher levels of quasi-indexer ownership change the profitability of insider trades.*

Firms can reduce informed trading by insiders by adopting, strengthening, or more strictly enforcing insider trading policies. Bettis et al. (2000) find 73% of firms have explicit insider trading policies that prohibit insiders from trading during "blackout" periods before earnings announcements. Blackout policies are effective in reducing informed insider trading (Bettis et al. 2000). However, Jagolinzer et al. (2011) find enforcement of these policies is inconsistent as 24% of all insider trades occur within blackout periods. Thus, firms could respond to quasi-indexer pressure by either adopting new blackout policies or more strictly enforcing existing policies. In either case, fewer insider trades will occur during blackout periods when quasi-indexer ownership is higher. Accordingly, we hypothesize:

> *HYPOTHESIS 3. Higher quasi-indexer ownership increases the likelihood a firm has an effective blackout policy.*

3. Empirical identification strategy and methodology

Reconstitutions of the Russell 1000 and 2000 Indexes

Our identification strategy uses Russell index inclusion as a source of plausibly exogenous differences in quasi-indexer ownership to more reliably identify its causal effect on insider trading. From 1985 through 2006, assignment in the R1000 and R2000 was based solely on the ranking of market capitalization on the last trading day of May.[5] Russell uses its own proprietary measure of shares outstanding to compute market capitalization. The 1000 largest firms are placed in the R1000 and the next 2000 largest firms are placed in the R2000. Subsequently, Russell uses a different float-adjusted market capitalization measured as of the end of June to calculate the new index weights. Value weighting causes the smallest R1000 firms to have substantially smaller index weights compared to the largest R2000 firms. In 2006, for example, the combined index weight of the 10 largest R2000 firms is about 44 times the combined index weight of the 10 smallest R1000 firms. Following the reconstitution, quasi-indexers rebalance their portfolios based on index membership and the new weights. Thus, index assignment causes discontinuous and exogenous variation in quasi-indexer ownership.

Regression discontinuity design

We employ a sharp regression discontinuity (RD) design, consistent with Boone and White (2015), Chen et al. (2019), and Khan et al. (2017). The RD design compares insider trading activity of R2000 firms to otherwise similar (counterfactual) R1000 firms near the threshold. The key to our RD identification strategy is to show the discontinuous difference in quasi-indexer ownership at the threshold is followed by a discontinuous difference in insider trading activity at the threshold. Focusing on firms near the threshold is important because it

[5] Starting in 2007, Russell instituted a "banding" policy where firms were not placed in a different index based on only small differences in market capitalization. This policy potentially reduces the local continuity of firm assignment around the threshold, and hence, violates the underlying assumption of our identification strategy.

ensures that sample firms are similar enough so any variation in quasi-indexer ownership is exogenous to firm characteristics that are likely associated with insider trading activities.

Let $ITV_i(1)$ [$ITV_i(0)$] denote the potential level of *ITV* for firm *i* in the R2000 [R1000], where *ITV* is one of our insider trading variables. We set the index threshold to equal 0 and define R_i as the end-of-May market capitalization rank of firm *i* minus 1000. Thus, firms with negative (positive) ranks are in the R1000 (R2000). The observed outcome for *ITV* is:

$$ITV_i = \begin{cases} ITV_i(0), \ if \ R_i \leq 0 \\ ITV_i(1), \ if \ R_i > 0 \end{cases} \quad (1)$$

The average treatment effect is given by $\tau = E[ITV_i(1) - \ ITV_i(0)|R_i = 0]$. The RD treatment estimate is interpreted as the difference in the insider trading variable between a firm at the top of the R2000 and a firm at the bottom of R1000, i.e., at the index threshold.

To estimate the treatment effect, we implement local polynomial-based and bandwidth-robust inference procedures developed in Calonico et al. (2014) and Calonico et al. (2019). The procedures developed in Calonico et al. (2019) represent an important innovation because they allow us to include additional covariates, which leads to improved estimates and inferences.

The RD estimate of the treatment effect τ is $\hat{\tau} = \alpha_0 - \beta_0$, where α_0 and β_0 are the intercepts (at the threshold) of a weighted second or third-order polynomial regression estimated separately for R2000 and R1000 firms. The polynomials are based on the ranked distance from the index threshold. Specifically, α_0 (along with $\alpha_1, \dots, \alpha_p, g_1, \dots, g_q$) is non-parametrically determined such that equation 2 attains a minimum value when $R_i > 0$; β_0 (along with $\beta_1, \dots, \beta_p, g_1, \dots, g_q$) is determined analogously when $R_i \leq 0$.

$$\sum_{i=1}^{n} \left\{ K(R_i) \left[ITV_i - \alpha_0 - \alpha_1(R_i) - \cdots - \alpha_p(R_i)^p - \sum_{q=1}^{Q} g_q z_q \right]^2 \right\} \quad (2)$$

In Equation 2, z_q represents three additional explanatory variables. Following Crane et al. (2016), we include $FloatAdj_{it}$ to control for inference problems caused by the float adjustments and to improve the precision of the estimates (Imbens and Lemieux 2008). $FloatAdj_{it}$ is the difference between the end-of-May and end-of-June market capitalization ranks. We include *TransOwn* and *DedicOwn*, the fraction of shares owned by transient and dedicated institutional owners, respectively, to better isolate the effects of quasi-indexer ownership on insider trading. $K(R_i)$ is a triangular kernel function representing the weight that is placed on each observation, with more (less) weight placed on observations closer to (farther from) the threshold.[6] The RD estimate is corrected for the bias induced by the choice of the bandwidth, which is the size of the "window" of observations on each side of the threshold included in the regression. We follow Calonico et al. (2014) and employ the optimal bandwidth. Accordingly, the sample size differs for each RD analysis. Standard errors are based on plug-in residuals.

Instrumental variable design

We also employ a two-stage instrumental variables (IV) design where index membership is used to isolate exogenous variation in quasi-indexer ownership in the first stage (Appel et al. 2016; Bird and Karolyi 2016; Crane et al. 2016). The identifying assumption is that index assignment is exogenous to insider trading activity except through its effect on quasi-indexer ownership. Our IV design employs a sharp RD design for the Russell treatment assignment in the first stage to estimate the exogenous level of quasi-indexer ownership. We build off the specification in Boone and White (2015) and Crane et al. (2016) and estimate equation 3:

$$QuasiOwn_{it} = \delta_0 + \delta_1 R2000_{it} + \delta_2 Rank_{it} + \delta_3 R2000_{it} \times Rank_{it} + \delta_4 FloatAdj_{it} + \delta_5 TransOwn + \delta_6 DedicOwn + \boldsymbol{B}Controls + YFE + IFE + \varepsilon_{it} \quad (3)$$

[6] Placing more weight on observations near the threshold better conforms to the underlying economics of the setting. In contrast, OLS and IV designs can lead to inferences being driven by observations far from the index threshold.

where $QuasiOwn_{it}$ is the fraction of shares owned by quasi-indexers; $R2000_{it}$ is an indicator variable equal to one for R2000 firms, and zero otherwise; $Rank_{it}$ is the end-of-May market capitalization ranking minus 1000*; Controls* is a vector of control variables that are included in both the first- and second-stage regressions (equation 4 below).[7] Specifically, we include *B/M*, the book-to-market ratio (Piotroski and Roulstone 2005; Skaife et al. 2013), *LagReturn*, lagged stock returns (Huddart and Ke 2007; Lakonishok and Lee 2001), *Leverage*, the ratio of debt to equity (Bricker and Markarian 2015), *Volatility*, the variance of daily stock returns (Frankel and Li 2004; Skaife et al. 2013), *ROA*, the return-on-assets ratio (Bricker and Markarian 2015), *R&D*, a research and development indicator variable (Aboody and Lev 2000; Skaife et al. 2013), *Loss*, a negative net income indicator variable (Skaife et al. 2013), and *Sales Growth*, annual sales growth (Gao et al. 2014). *YFE* (*IFE*) is a vector of year (industry) indicator variables.

In the second-stage regression, we use the fitted value of quasi-indexer ownership, $\widehat{QuasiOwn}$, from the first stage. We then estimate equation 4 where the dependent variable is one of the insider trading variables, *ITV*. In order to estimate the standard errors correctly, we follow the approach suggested in Angrist and Pischke (2009), where the two stages are estimated simultaneously. Other variables are defined as above.

$$ITV_{it} = \gamma_0 + \gamma_1 \widehat{QuasiOwn}_{it} + \gamma_2 Rank_{it} + \gamma_3 R2000_{it} \times Rank_{it} + \gamma_4 FloatAdj_{it} + \gamma_5 TransOwn + \gamma_6 DedicOwn + \boldsymbol{B}Controls + YFE + IFE + \varepsilon_{it} \quad (4)$$

Insider trading variables

The main dependent variables are the frequency of insider trades, the profitability of

[7] The RD methodology assumes firms around the treatment threshold are otherwise similar before the treatment. Under this maintained assumption, control variables are unnecessary as they are similar across the two groups of firms (Imbens and Lemieux 2008; Imbens and Wager 2019). As we discuss below, the results in Table 2 indicate that this assumption is correct. Thus, we exclude the control variables from equation 2. Nonetheless, we replicate our primary RD analyses in Tables 3, 4, and 5 where we include all of the control variables. The untabulated results are qualitatively similar to the tabulated results and our inferences remain unchanged.

insider trades, and whether the firm has policies restricting insider trading. Data on insider trades come from the Thomson Financial Insider Filing database and include all insiders' transactions filed on Form 4. We use open market insider transactions (TRANCODE=P or S) and exclude problematic insider trading records (CLEANSE=A or S). The frequency of insider sells (buys), *Sell Frequency*$_{it}$ (*Buy Frequency*$_{it}$) is the number of insider sell (buy) transactions at firm i from July in year *t* through June in year *t+1*.

Cohen et al. (2012) use an individual insider's past trading behavior to categorize insider trades as either routine, which are less likely to be based on private information, or non-routine, which are more likely to be based on private information. In order to be classified as a routine trader, an insider must have made at least one trade in the same calendar month in each of the three prior years. For a routine trader, any trades made in that same calendar month (i.e., the "routine month") are classified as routine trades. Trades made by a routine insider in a non-routine month are classified as non-routine trades. All other insiders are classified as non-routine traders, and hence, all of their trades are classified as non-routine trades.[8]

We calculate the profitability of insider trading, *Profitability*, as the average Carhart (1997) four-factor adjusted daily return, α_{it}, for each trade-day over the 180 calendar days following the transaction date from ordinary least squares (OLS) regressions of daily returns over the 180 days after the trade (Carhart 1997; Fama and French 1993; Jagolinzer et al. 2011). For buys (sells), *Profitability*$_{it}$ = α_{it} (-α_{it}); α_{it} captures the profitability of insider buys and -α_{it} measures the losses avoided by selling before price declines.

[8] The method in Cohen et al. (2012) only categorizes insiders who trade in each of the prior three years. This requirement results in a large loss in sample size (roughly 67%). Instead, we include trades by non-classified traders as Cohen et al. (2012) find their trades are similar to those made by non-routine insiders. Our results are qualitatively similar, but statistically weaker in some cases, when we exclude non-classified trades.

We use two variables to infer whether a firm has a blackout policy and to what degree it is enforced. *Safe Trades* equals the natural logarithm of the fraction of insider trading that occurs within the 30-day "safe" period following the quarterly earnings announcement (Jeng 1999). We use two measures of insider trading activity: *Share Volume* is the number of shares traded by insiders; *Dollar Volume* is the total value of shares traded by insiders (i.e., for an insider transaction, it is the number of shares traded multiplied by the transaction price). Observations with no insider trades are excluded. Following Roulstone (2003), we infer there is an effective firm-imposed blackout policy if at least 75% of insider trading (shares or dollar volume) from July in year *t* to June in year *t+1* occurs within the 30-day safe periods. *Restriction* equals one if the firm has an inferred blackout policy, and zero otherwise. While noisy and somewhat *ad hoc*, this classification rule divides firms into groups that are more (less) likely to have such policies.

Sample

Our sample starts in 1995 as most of the insider trading data are widely available then and ends in 2006 due to the banding policy instituted by Russell. In addition to insider trading data from Thomson Reuters, we use institutional ownership data from the Thomson Reuters Institutional (13f) Holdings database, stock market data from the Center for Research in Security Prices (CRSP) database, corporate governance data from Institutional Shareholder Services, compensation data from Execucomp, management forecast data from the Thomson Reuters I/B/E/S database, analyst data from the Thomson Reuters I/B/E/S database, 8K filings from SEC EDGAR, and Russell index data including constituent lists and float-adjusted market weights from Russell Investments. We classify institutional investors into three categories following Bushee (2001): Quasi-indexers (~67% of all institutional holdings), Dedicated institutions (~8% of institutional holdings), and Transient institutions (~24% of institutional holdings).

Table 1 reports descriptive statistics separately for the R1000 and R2000 firms using a bandwidth of +/- 300 around the threshold. Market capitalization, market ranks, and shares outstanding are calculated as of the end of May. The mean (median) level of quasi-indexer ownership is 41.5% (42.2%) for the R1000 firms and 41.0% (41.7%) for the R2000 firms. Consistent with prior literature, insider sells are much more common than buys. The mean (median) annual number of sells is 56.21 (13) for R1000 firms while the mean (median) number of buys is 7.39 (1). For R2000 firms, the mean number of sells is 41.96 and the mean number of buys is 6.51. In addition, the standard deviations for trading frequency are several times the mean values for both groups, indicating large differences in insider trading among firms. For both groups, mean and median sell (buy) profitability is negative (positive), while the absolute values are lower for R2000 firms. Overall, 60.6% of all insider trades occur within a safe period and 43.5% of sample firms have an inferred blackout policy.

{Place Table 1 about here.}

Pre-assignment differences

The validity of our research design rests on variation in *ITV* following the index reconstitution being attributable to differences in quasi-indexer ownership rather than to pre-existing differences in firm attributes that are potentially correlated with insider trading. We test whether there are any pre-assignment discontinuities in quasi-indexer ownership, the insider trading variables, and firm characteristics (e.g., Boone and White 2015; Crane et al. 2016). Pre-assignment quasi-indexer ownership is measured at the end of the first calendar quarter in year *t*, *ITV* are measured for the period between July in year *t-1* and June in year *t*, and firm attributes

are measured at the end of the fiscal quarter that ends during the first calendar quarter.[9]

Table 2 presents the RD estimates based on the optimal bandwidth for the continuous pre-assignment variables around the reconstitution threshold and *t*-tests of differences for the dichotomous variables.[10] The results show there is no significant discontinuity in quasi-indexer ownership, the insider trading variables, or the firm characteristics around the index threshold prior to the index reconstitution. Thus, there is no obvious selection bias near the threshold. These findings support our use of the RD methodology as the only significant difference between the two groups is the index assignment that exogenously affects quasi-indexer ownership levels.

{Place Table 2 about here.}

4. Empirical results

We first provide evidence on the discontinuous differences in quasi-indexer ownership around the index threshold. We then present the results of our main empirical tests.[11]

Discontinuity in quasi-indexer ownership

Our research design relies on there being discontinuous differences in quasi-indexer ownership around the index threshold. Figure 1 illustrates these discontinuities using a bandwidth of +/-300. The x-axis represents the distance from the threshold using the actual end-

[9] The treated (controlled) firms in year *t* are R2000 (R1000) firms post-assignment. Thus, when firm characteristics are measured prior to the treatment, a firm in the year *t* treated (controlled) group can be either be in R1000 or R2000 in year *t-1*. Therefore, the effect of previous Russell assignment and weights is offset within each group. This cancels out any difference within each group and there should be no discontinuity in firm characteristics.

[10] We are unable to use our RD methodology when the outcome variable is binary because the bias corrector and robust standard error methodology developed by Calonico et al. (2014) is no longer applicable. Accordingly, we use a simple *t*-test of the differences in sample means. For similar reasons, we use a two-stage IV approach when the binary variable *Restriction* is the dependent variable in Table 5, panel B (Appel et al. 2016; Crane et al. 2016).

[11] We discuss the results of supplementary analyses in the online Appendix, including switching analyses, alternative measures of trade profitability, and analyses that address potential alternative explanations related to short selling and litigation risk. The results of these analyses are consistent with our tabulated results and our inferences remain unchanged. Please see "Online Appendix for Quasi-Indexer Ownership and Insider Trading: Evidence from Russell Index Reconstitutions" as an addition to the online article.

of-June ranking.[12] Negative (Positive) values represent firms in the R1000 (R2000) and zero represents the smallest R1000 firm. Each dot represents the average of the 72 observations within each bin. Each RD plot employs a second-order polynomial to approximate the regression function with local sample averages of ownership (see Calonico et al. (2015) for details).

{Place Figure 1 about here.}

Figure 1 shows there is a striking decrease in the level of quasi-indexer ownership for smaller R1000 firms. This steep drop-off is consistent with their very small index weights. In contrast, the largest R2000 firms have much higher levels of quasi-indexer ownership. Consistent with prior Russell index studies, this evidence indicates Russell index membership leads to large, discontinuous differences in quasi-indexer ownership around the index threshold.

Figure 1 shows that the difference in quasi-indexer ownership levels around the index threshold is roughly 25 percentage points. While large, back-of-the-envelope calculations indicate the magnitude is plausible. During 2006, there was $221.1 billion across 273 investment products indexed to the R2000 and $146.1 billion across 52 products indexed to the R1000 (Russell Investments 2008). In addition, the collective index weight for the top 10 R2000 firms (bottom 10 R1000 firms) was 1.63% (0.037%). These amounts imply that average quasi-indexer ownership was higher by about $355 million for top 10 R2000 firms compared to bottom 10 R1000 firms. This difference represents 20% of their average market capitalization.

Effect of quasi-indexer ownership on the frequency of insider trades

In this section, we provide evidence on Hypothesis 1 by analyzing whether insiders of R2000 firms trade less frequently than insiders of R1000 firms near the index threshold. Figures

[12] Similar to Crane et al. (2016), we use the actual end-of-June rankings for our figures while we use the end-of-May rankings for our regression analyses. We follow this convention because we cannot additionally control for the float adjustment in two-dimensional figures. As the end-of-June market capitalizations determine the index weights, this convention more clearly shows the relation between market capitalization rankings and insider trading behavior.

2A and 2B are constructed similarly to Figure 1. They provide graphical evidence of the discontinuity in trading frequency using a fixed bandwidth of +/-300. The *y*-axis represents the annual number of sells and buys, respectively.

In Figure 2A, the sell frequency for R1000 firms generally increases as firm size decreases. In contrast, the average sell frequency slightly declines as firm size decreases for R2000 firms. The fitted regression line shows a large discontinuity in sell frequency. The fitted R1000 regression line intercepts the index threshold at about 78 annual insider sells while the corresponding R2000 intercept is at about 45 annual insider sells. Thus, the implied discontinuity is 33 sells per year, which is an economically large difference.

{Place Figure 2 about here.}

Figure 2B presents a similar graph of insider buy frequency. For R1000 firms, insider buy frequency is relatively constant until it starts to increase near the index threshold. In contrast, buy frequency is relatively constant across R2000 firms. The fitted regression line for R1000 (R2000) intercepts the index threshold at approximately 19 (7) buys per firm per year. The implied discontinuity of 12 buys per year is economically significant. Thus, Figures 2A and 2B are consistent with Hypothesis 1.

In panel A of Table 3, we report RD estimates for the frequency of insider sells. The results for all sell trades in Column 1 (2) are based on second- (third-) order polynomials. Both RD estimates are significantly negative at the 1% level or better. These estimates represent the difference in the sell frequencies between R1000 and R2000 firms at the index threshold. The estimated effects are economically large. For example, the RD estimate in Column 1 indicates that at the index threshold, there are almost 55 fewer sell trades per year attributable to differences in quasi-indexer ownership. The magnitude of the estimated effect is large compared

to the sample average annual sell frequency of 49.14. However, comparing the RD estimates to the observed averages is somewhat misleading as the RD estimate only applies at the threshold, where the effects of differences in quasi-indexer ownership are expected to be strongest.

{Place Table 3 about here.}

Our primary measure of insider trading activity is the frequency of insider trades. In addition, we examine two alternative measures: *Dollar Volume*$_{it}$ (*Share Volume*$_{it}$) is the total value (number) of shares traded in millions by insiders at firm *i* in year *t*. Consistent with prior literature (Badertscher et al. 2011; Bricker and Markarian 2015; Skaife et al. 2013; Veenman 2012), we winsorize the observations at the 1%/99% level to reduce the influence of outliers. The results with *Dollar Volume* and *Share Volume* are presented in Columns 3 and 4, respectively. The RD estimates for both alternative measures are negative and significant at the 1% level.[13] Thus, our results are robust to these alternative measures of trading activity.

Next, we examine whether inclusion in the R2000 affects the frequency of routine and non-routine sells, where routine (non-routine) trades are less (more) likely to be based on private information (Cohen et al. 2012). To the extent institutions are concerned with insiders trading on private information, we expect the effects of their monitoring will be concentrated on non-routine trades. The RD estimate for routine sells in Column 5 is insignificant. In contrast, Column 6 shows the RD estimate for non-routine sells is significantly negative at the 1% level. Together, these findings indicate the effects of quasi-indexer ownership on insider sell frequency

[13] In Columns 3 – 6, we only tabulate the results for the third-order polynomial estimates. The estimates using a second-order polynomial are qualitatively similar and our inferences remain unchanged. In addition, the results in Column 3 (4) are qualitatively similar if we scale *Dollar Volume* (*Share Volume*) by the firm's market capitalization (total shares outstanding). The results in Column 3 contrast with those in Huddart and Ke (2007), who find no significant association between institutional ownership and a variable similar to *Dollar Volume.*

are concentrated among sells that are more likely to be based on private information. Thus, they provide evidence supporting Hypothesis 1.

The analogous results for the frequency of insider buys are presented in panel B. The RD estimates for all buy trades in Columns 1 and 2 are both negative and significant at the 5% level. The estimated effects are economically large. For example, the RD estimate in Column 1 indicates there are roughly 31 fewer buy trades per year attributable to differences in quasi-indexer ownership at the index threshold. The RD estimates in Columns 3 and 4 for *Dollar Volume* and *Share Volume* are both negative and significant at the 5% and 1% levels, respectively. These results provide further support for Hypothesis 1.

The RD estimates in Columns 5 and 6 for *Routine Buys* and *Non-Routine Buys* are both negative and significant at the 5% level. Thus, the results indicate that near the threshold, R2000 insiders engage in significantly fewer routine and non-routine buys compared to R1000 insiders.[14] Somewhat surprisingly, the absolute magnitude of the estimate for routine buys is larger than the estimate for non-routine buys (-58.72 vs. -31.0). Given insiders' highly undiversified portfolios, insider purchases are often thought to be primarily motivated by private information; in other words, routine purchases are uncommon. Thus, our findings could indicate quasi-indexers monitor all buys, and not just non-routine buys.

Next, we conduct trade frequency analyses using the IV design. In the first-stage, we estimate equation 3 where *QuasiOwn* is the dependent variable. Using a bandwidth of +/-300, the untabulated *R2000* coefficient is positive and significant (*t*-statistic = 8.19). The coefficient estimate indicates quasi-indexer ownership levels are higher by 7.8 percentage points for R2000 firms. The model fit is reasonably high (adjusted R^2 = 43.7%) and the partial R^2 for *R2000* is

[14] This result contrasts with Bricker and Markarian (2015). Their evidence indicates that institutional ownership is positively associated with the frequency of non-routine buys.

1.8%. The Kleibergen-Paap rk LM statistic equals 9.94 and the Cragg-Donald Wald F statistic equals 98.4. These tests for weak-identification indicate *R2000* meets the relevance condition. We cannot test the exclusion condition because we only have one instrumental variable.

In the second stage, we estimate equation 4 where the main independent variable is the fitted value of quasi-indexer ownership, $\widehat{QuasiOwn}$, from the first stage. The results for the second stage are presented in Table 3, panel C. For the sake of brevity both here and below, we do not tabulate the results for the control variables. In Columns 1 - 3, the $\widehat{QuasiOwn}$ coefficients are negative and significant (*t*-statistics range between -2.20 and -2.64). These results indicate quasi-indexer ownership reduces the frequency of insider sells regardless of which measure of sell frequency we use. The results in Columns 4 and 5 show that while both routine and non-routine sells are significantly lower when quasi-indexer ownership is higher, the absolute magnitude of the effect is substantially larger for non-routine sells (-408.1 vs. -28.2). Overall, these results are consistent with the corresponding results in panel A.

Columns 6 – 10 present the IV results for buy frequency. For each measure of buy frequency, the $\widehat{QuasiOwn}$ coefficient is negative and significant. Thus, the results are generally consistent with the corresponding RD results and indicate higher levels of quasi-indexer ownership result in fewer buys by insiders. In addition, the results indicate the $\widehat{QuasiOwn}$ coefficients are significantly negative for both routine and non-routine buys. Consistent with the results in panel B, the absolute magnitude of the estimate for routine buys is larger than that for non-routine buys (-108.8 vs. -76.5).

Overall, the results in Table 3 provide evidence in support of our hypothesis that higher levels of quasi-indexer ownership cause insiders to significantly reduce their insider trading.

Effect of quasi-indexer ownership on the profitability of insider trades

In this section, we provide evidence on Hypothesis 2 by analyzing whether the profitability of insider trades is different for R2000 insiders compared to R1000 insiders near the threshold. Figures 2C and 2D present graphical evidence on how sell and buy profitability varies around the index threshold. In both figures, average trade profitability is highly variable around the fitted regression lines. Figure 2C shows a discontinuity at the threshold where the average daily abnormal return for sells made by R1000 insiders is about 2 basis points higher than that for R2000 insiders. In contrast, Figure 2D shows the opposite result for buys. The average profitability of buys is about 2.5 basis points higher for R2000 insiders compared to R1000 insiders. Together, this evidence suggests the influence of quasi-indexer ownership on how likely insider trades are based on private information depends on the direction of the trade.

The RD results for insider sell profitability are presented in panel A of Table 4. The sample sizes are much larger than those in Table 3 because each firm-trade date pair constitutes an observation. The RD estimates for all sell trades in Columns 1 and 2 are negative and significant at the 1% level. The significant reductions in trading profits indicate sell transactions are less likely to be based on private information when quasi-indexer ownership is higher. The results are also economically significant. The magnitudes of the estimates indicate average abnormal daily returns are lower by between 0.076% and 0.093% for R2000 insiders at the index threshold. The results in Columns 3 and 4 indicate the reduction in trade profitability is concentrated among non-routine sells as the RD estimate is only significant (at the 1% level) in Column 4. Together, the results indicate that a smaller proportion of insider sells are motivated by private information when quasi-indexer ownership is higher.

{Place Table 4 about here.}

The RD results for the profitability of insider buys are presented in Table 4, panel B. In Column 1, the RD estimate is positive but only marginally significant at the 10% level. The RD estimate in Column 2 is not significant. The RD estimate for routine buys in Column 3 is insignificant while the Column 4 estimate for non-routine buys is positive and significant at the 1% level. Thus, insider buy profitability is only higher for trades that are most likely to be based on private information. Together, the results indicate that while the insiders at R2000 firms make fewer purchases, when they do decide to buy, their trades are more likely based on more valuable private information compared to insiders at R1000 firms near the threshold.[15]

For the IV profitability analyses, we use smaller bandwidths (+/-100 or +/-200) compared to trade frequency IV analyses (300). The much larger number of trade-based observations compared to firm-year observations allows us to maintain sufficient power while the smaller bandwidth allows for stronger inferences. We estimate the first stage separately for sells and buys as the transaction-based samples are different. The untabulated results show the *R2000* coefficients are both positive (0.102 and 0.150, respectively) and significant (*t*-statistics = 21.8 and 15.0, respectively). The partial R^2s are 2.4% and 4.7%, and the tests for weak-identification indicate *R2000* meets the relevance condition in both regressions.

The second stage results for sells are presented in Columns 1 – 3 of Table 4, panel C. The $\widehat{QuasiOwn}$ coefficient in Column 1 is negative and significant (*t*-statistic = -2.06). This result indicates quasi-indexer ownership is negatively associated with the profitability of insider sells and is consistent with the RD results in Table 4, panel A. When we separately analyze *Routine*

[15] Bricker and Markarian (2015) find a positive association between total institutional ownership and non-routine buy profitability. They attribute their results to institutions encouraging insiders to make privately informed buys in order to provide additional incentives. The combination of lower buy frequency and higher non-routine buy profitability is more consistent with our explanation that insiders require higher expected returns before trading on private information when there is more monitoring. In contrast, Huddart and Ke (2007) find total institutional ownership is negatively associated with buy profitability.

Sells and *Non-Routine Sells*, the results in Columns 2 and 3 show neither IV estimate is significant (although the *t*-statistic (1.50) for the *Non-Routine Sells* approaches marginal significance). Thus, we are unable to draw inferences regarding the effect of quasi-indexer monitoring on the relative profitability of *Routine Sells* and *Non-Routine Sells* using the IV design.

The second stage results for buys show the $\widehat{QuasiOwn}$ coefficient in Column 4 is negative but insignificant (*t*-statistic = -1.25). This result is consistent with the insignificant RD estimate in Column 2 of Table 4, panel B. The $\widehat{QuasiOwn}$ coefficients for *Routine Buys* and *Non-Routine Buys* are both negative but insignificant.[16] The insignificant coefficient in Column 6 contrasts with the significant RD estimate in Column 4 of Table 4, panel B.

Overall, the results provide consistent evidence that higher levels of quasi-indexer ownership result in lower average sell profitability. The RD results (but not the IV results) show the reductions are concentrated among *Non-Routine Sells*. These results indicate that monitoring by quasi-indexer owners reduces the likelihood that sells are based on private information. However, the results regarding the effect of quasi-indexer ownership on buy profitability depend on the estimation methodology. The RD analyses indicate quasi-indexers have a positive or insignificant effect on buy profitability while the IV analyses indicate an insignificant effect.

Effect of quasi-indexer ownership on insider trading restrictions

In this section, we provide evidence on Hypothesis 3 by conducting two sets of analyses. First, we examine whether *Safe Trades*, the fraction of insider trading that occurs during the "safe" period following earnings announcements, is higher for R2000 firms using the RD design.

[16] We also use a bandwidth of +/-300. The untabulated results show that the $\widehat{QuasiOwn}$ coefficients for *Buys* and *Non-Routine Buys* are positive but insignificant (*t*-statistics = 0.63 and 1.16, respectively). The $\widehat{QuasiOwn}$ coefficient for *Routine Buys* remains negative and insignificant (*t*-statistic = -1.19).

Second, we use the IV design to analyze whether quasi-indexer ownership increases the likelihood that a firm has an implied blackout policy (i.e., when *Restriction* = 1).[17] We first present graphical evidence on how *Safe Trades* varies around the index threshold in Figure 3. The fitted regression lines show a discontinuity in *Safe Trades*. At the threshold, the percentage of trades made by R2000 insiders during safe periods is five percentage points higher than that for R1000 insiders. Thus, the graphical evidence is consistent with Hypothesis 3.

{Place Figure 3 about here.}

The results for the RD analyses where *Safe Trades* is the dependent variable are presented in Table 5, panel A. In Columns 1 and 2 (3 and 4), insider trading is measured using *Share Volume* (*Dollar Volume*). In Columns 1 and 3 (2 and 4), we present the results using second- (third-) order polynomials, respectively. In all four columns, the RD estimate is positive and significant (at the 5% level or better).[18] The magnitudes of the estimates indicate that around the index threshold, the percentage of insider trades taking place during safe periods is roughly 4 to 5 percentage points higher for R2000 firms. These results are consistent with firms more strictly enforcing insider trading policies when quasi-indexer ownership levels are higher.

{Place Table 5 about here.}

The results for the IV analyses are presented in Table 5, panel B. The untabulated results for the first-stage regression show the *R2000* coefficient is positive and significant (*t*-statistic = 9.09). In the second stage, we use a probit model to regress *Restriction* on the fitted values of

[17] Untabulated analyses show the mean abnormal return for insider sells during blackout periods is larger than that for sells made during safe periods (*t*-statistic = 1.92). However, the difference in mean abnormal returns for insider buys is not significant (*t*-statistic = -1.19). Thus, sells are more likely to be based on private information when they are made during blackout periods, which is consistent with the development of Hypothesis 3.

[18] Untabulated analyses show that when the number of trades is used to calculate *Safe Trades*, the RD estimates remain positive but are no longer significant at conventional levels. Furthermore, using the IV approach, the $\widehat{Quasi Own}$ coefficients are positive and significant at the 10% level.

quasi-indexer ownership from the first-stage regression ($\widehat{QuasiOwn}$), as well as the additional control variables. The results show that when using either measure of insider trading, the $\widehat{QuasiOwn}$ coefficient is positive and significant (*t*-statistics = 3.71 and 2.92. respectively). The magnitudes of the $\widehat{QuasiOwn}$ coefficients indicate the effect is economically significant as well. For example, the coefficient estimate in Column 1 indicates that a one standard deviation increase in quasi-indexer ownership (StdDev = 0.18) leads to a 6.67% increase in the probability of having an insider trading restriction (0.18*0.37=6.67%, where 0.37 is the untabulated marginal effect).

The results in Table 5 provide support for Hypothesis 3 and indicate that one way in which firms respond to monitoring by quasi-indexer owners is either by instituting new blackout policies and/or more strictly enforcing existing policies. Our findings are not mechanically related to the previously documented reductions in insider trading frequency. Our inferences rely on the distribution of trades, and thus, are not mechanically driven by the number of trades.

Mediation of the effect of quasi-indexers on insider trading

As discussed above, the effects on insider trading could result from direct monitoring efforts by quasi-indexers aimed explicitly at pressuring firms to reduce insider trading activity. Another possibility is that quasi-indexers indirectly affect insider trading by first directly affecting one or more mediating variables. In turn, the mediating variable affects insider trading behavior. There could, of course, be both direct and indirect effects.[19] In this section, we examine whether certain variables mediate the effect of quasi-indexers on insider trading. Identifying mediators increases our understanding of the underlying mechanisms or paths through which

[19] The evidence in Chen et al. (2019) indicates quasi-indexer ownership has both direct and indirect effects on corporate tax planning while Khan et al. (2017) only find evidence of an indirect effect. Lin et al. (2018) find the total effect of R2000 inclusion on peer firms' forecast frequency consists entirely of an indirect effect.

quasi-indexers affect insider trading (Baron and Kenny 1986).

In order for a variable to be a mediator, it must satisfy two conditions. First, it must exhibit a significant discontinuity around the index threshold. Second, it must affect insider trading behavior. Prior literature (e.g., Appel et al. 2016; Bird and Karolyi 2016; Boone and White 2015; Schmidt and Fahlenbrach 2017) has identified several corporate governance and information environment variables that exhibit significant discontinuities at the threshold that are plausibly related to insider trading. Dai et al. (2016), Gao et al. (2014), Jagolinzer et al. (2011), and Skaife et al. (2013) find that stronger governance is negatively associated with trading profitability. Thus, governance attributes are plausible mediators. Stronger governance mechanisms could make insiders more responsive to the preferences of quasi-indexers when making trading decisions. In addition, information attributes are also plausible mediators as Aboody and Lev (2000) and Frankel and Li (2004) find more transparent information environments are associated with less profitable and/or less frequent trading.

We also examine equity-based incentive variables. While the effects of equity-based incentive variables on insider trading have not received much attention in the prior literature, these incentives ameliorate agency costs in other contexts. Hence, if informed insider trading is a manifestation of agency costs, then incentive variables are plausible mediators as they cause insiders to internalize the costs of their trades to shareholders.[20]

We consider governance attributes [(1) percentage of independent directors (*%IndeDirectors*); (2) dual CEO and board chairperson (*CEO Duality*); (3) dual class share structure (*Dual Class*); (4) existence of a poison pill (*Poison Pill*); and (5) limited ability to call a special meeting (*Special Meeting*)], information environment variables [(6) bid-ask spread

[20] Consistent with this idea, Khan et al. (2017) find that including CEO Delta and Vega in their tax avoidance regressions causes their *R2000* indicator variable to become insignificant.

(*Spread*); (7) number of 8-Ks (*#8Ks*); (8) number of management forecasts (*#MFs*); and (9) analyst following (*#Analysts*)], and CEO or Top 5 equity-based incentive variables [(10) wealth-performance sensitivity (*WPS*); (11) incentives to increase shareholder wealth (*Delta*); (12) incentives to increase stock volatility (*Vega*); and (13) percentage of shares owned (*Ownership*)].

We sequentially include each variable as the dependent variable in equation 2 in order to assess whether there is a significant discontinuity at the index threshold. The results are tabulated in Table 6. We find significant discontinuities in all five governance attributes. Consistent with quasi-indexer ownership leading to stronger governance attributes, we find that at the threshold, R2000 firms have more independent directors and are less likely to use dual class shares (Appel et al. 2016; Chen et al. 2019). However, R2000 firms at the threshold are also more likely to combine the CEO and chairman positions, to use poison pills, and limit special meetings. These latter findings of weaker governance attributes are consistent with Schmidt and Fahlenbrach (2017) and indicate CEOs at R2000 firms are relatively more powerful than their R1000 counterparts. Consistent with Boone and White (2015), R2000 firms have higher analyst coverage and lower bid-ask spreads. However, contrary to Boone and White (2015) and Bird and Karolyi (2016), our estimates for *#8Ks* and *#MFs* are insignificant. We find discontinuities in *CEO Ownership*, *CEO Vega*, *Top 5 Ownership*, and *Top 5 Delta*. CEOs of R2000 firms have relatively higher incentives to increase volatility but smaller ownership stakes while Top 5 executives have lower incentives to increase firm value in addition to smaller ownership stakes.[21] Overall, these analyses identify 11 potential mediators.

{Place Table 6 about here.}

[21] Overall, the prior literature indicates that while quasi-indexer ownership has a positive effect on relatively standardized governance and corporate practices, quasi-indexers are unlikely to be effective monitors in more complex and nuanced settings. Thus, it is not surprising to see that quasi-indexer ownership has mixed effects on different governance, disclosure, and incentive compensation practices.

Next, we individually include each potential mediator as an additional explanatory variable in equation 2. Given the weakness of the original results, we do not examine potential mediators for *Buy Profitability*. We use the same samples from the corresponding analyses in Table 3 – 5 to ensure that any differences in the results are solely due to the inclusion of the potential mediator. We classify a variable as a "Total" mediator if including it in equation 2 eliminates the significance of both the second- and third-order polynomial RD estimates at the 10% level or better. We classify a variable as a "Partial" mediator if including it results in a reduction in the absolute magnitude of the RD estimate of at least 20% and both RD estimates remain significant at the 10% level or better, or if one of the estimates becomes insignificant.[22] Otherwise, a potential mediator variable is not classified as a mediator and is designated as "No."

The results summarized in Table 7 indicate that *%IndeDirectors* acts as a total or partial mediator for three of the four *ITV* variables (all except *Sell Frequency*). Thus, the results suggest higher quasi-indexer ownership first strengthens board independence, which in turn influences insiders' trading behavior. In contrast, including *Dual Class* only partially mediates the effect on *Sell Profitability*, and otherwise, does not act as a mediator. In the three cases where higher quasi-indexer ownership leads to weaker governance attributes (*CEO Duality*, *Poison Pill*, and *Special Meeting*), there is limited evidence that these variables act as mediators. One reason why these governance attributes play such a limited mediating role is that they affect the ability of shareholders to directly affect firm decisions. As discussed above, we find limited evidence that shareholders directly try to influence insider trading, and thus, it is not surprising that these attributes have limited ability to explain the effects of quasi-indexer ownership on insider

[22] We are not able to assess whether the magnitude of one RD estimate is significantly smaller than another. Thus, we impose the 20% cutoff rule to identify economically significant reductions. While arbitrary, we think it is reasonable for our purposes. The Partial classification is mostly unaffected when using cutoff thresholds between 15% and 30%.

trading. In contrast, independent directors can directly monitor the trading activities of insiders.

{Place Table 7 about here.}

The results for the two information environment variables, *Spread* and *#Analysts*, indicate that when either variable is added to equation 2, the RD estimates remain significant and their absolute magnitudes are similar to their counterparts in Tables 3 – 5. Thus, we find no evidence that either variable mediates the effects of quasi-indexer ownership on insider trading. These results suggest that insiders' decisions about whether to trade based on private information are not affected by these information environment variables.

The results in Table 6 indicate that executives at R2000 firms near the threshold have lower wealth-increasing incentives (*CEO Ownership*, *Top 5 Delta*, and *Top 5 Ownership*). While *CEO Ownership* acts as a mediator for each *ITV*, *Top 5 Delta* and *Top 5 Ownership* partially or fully mediate two or three insider trading variables. Thus, lower equity-based incentives appear to be associated with less privately-informed trading by insiders. One possible explanation is that lower equity-based holdings indicate relatively less powerful CEOs and executives (Bebchuk et al. 2011), and less powerful executives are more responsive to monitoring efforts by quasi-indexers. In addition, we find *CEO Vega* totally mediates the effect of quasi-indexer ownership on the frequency of insider buys and on the proportion of trades that take place during safe periods. In this respect, our results are similar in spirit to those in Khan et al. (2017), who find *CEO Vega* fully mediates the relation between institutional ownership and tax avoidance. Overall, these results indicate that equity-based incentives influence insiders' trading decisions.

We argue in Section 2 that quasi-indexers are unlikely to directly monitor insider trading because it is relatively difficult, complex, and costly. Instead, we expect quasi-indexers indirectly influence insider trading through one or more mediating paths. Overall, the evidence in Table 7

is consistent with our expectations. For three of the *ITV* variables, one or more variables totally mediate the effect of quasi-indexer ownership on insider trading. For *Sell Frequency*, only *CEO Ownership* acts as a partial mediator. Thus, it appears that either quasi-indexers directly affect sell frequency or indirectly do so through some other mechanism that we did not consider.

5. Conclusion

We examine the effects of quasi-indexer ownership on insider trading. Our identification strategy relies on exogenous differences in quasi-indexer ownership following Russell index reconstitutions. Using both regression discontinuity and instrumental variable research designs, we find higher quasi-indexer ownership causes significant reductions in the frequency of insider sells and buys, in addition to reductions in average sell profitability. The results are both statistically and economically significant. The evidence on the profitability of buys is somewhat mixed and depends on the specification. Many of our findings regarding sells are concentrated among insider trades that are most likely to be based on private information. In addition, we find firms with higher levels of quasi-indexer ownership are significantly more likely to have and/or more strictly enforce blackout policies, which limit insider trades to certain periods immediately following earnings announcements. Overall, our evidence indicates that when quasi-indexer ownership is higher, insiders are less likely to trade based on private information.

Appendix: Variable definitions

Insider Trading Variables (ITV):	
Frequency	The total number of insider trades from $July_t$ to $June_{t+1}$.
Profitability	The alpha from the Fama and French (1993) and Carhart (1997) four-factor model estimated over the 180 calendar days following insider trade *j*. For sells, alpha is multiplied by (-1). *Profitability* is in percentages.
Restriction	An indicator variable that equals one if 75 percent or more of insider trading from $July_t$ to $June_{t+1}$ occur within 30 days after a quarterly earnings announcement, and zero otherwise. Insider trading is alternatively measured using the number of shares traded or the dollar value of shares traded.
Safe Trades	The natural logarithm of the fraction of insider trading from $July_t$ to $June_{t+1}$ made within the 30-day window following an earnings announcement. Insider trading is alternatively measured using the number of shares traded or the dollar value of shares traded.
Dollar Volume	The total value of shares traded in millions by insiders from $July_t$ to $June_{t+1}$.
Share Volume	The total number of shares traded in millions by insiders from $July_t$ to $June_{t+1}$.
Institutional Ownership and Index Variables:	
QuasiOwn	The total number of shares held by quasi-indexer institutions divided by total shares outstanding as of the end of September in year *t*, as in Bushee (2001).
TransOwn	The total number of shares held by transient institutions divided by total shares outstanding as of the end of September in year *t*, as in Bushee (2001).
DedicOwn	The total number of shares held by dedicated institutions divided by total shares outstanding as of the end of September in year *t*, as in Bushee (2001).
R2000	An indicator variable that equals one if the firm is in R2000 in year *t*, and zero otherwise.
Rank	Firm *i*'s market capitalization rank in year *t* within the R1000 or R2000 based on CRSP data as of the last trading day of May. The last rank in R1000 is 1000 and the first rank in R2000 is 1001.
Russell Float-Adjusted Cap Rank	Firm *i*'s market capitalization rank during year *t* within the R1000 or R2000 based on the float-adjusted market capitalization at the end of June provided by Russell Investments.
FloatAdj	The difference between CRSP-based market capitalization rank and Russell float-adjusted market capitalization rank in year *t*.
Control Variables:	
B/M	Book-to-market ratio for the fiscal year ending after June of year *t*.
LagReturn	Past stock returns, measured as stock returns of last 12 months using CRSP daily stock returns from $July_{t-1}$ to $June_t$.
Leverage	The sum of long-term debt and short-term debt, divided by stockholders' equity, for the fiscal year ending after June of year *t*.
Volatility	The variance of daily stock returns from July of year *t*-1 to June of year *t*.
ROA	Net income divided by assets for the fiscal year ending after June of year *t*.
R&D	An indicator variable that equals one if a firm has positive R&D expenses for the fiscal year ending after June of year *t*, and zero otherwise.

Loss	An indicator variable that equals one if a firm reports negative earnings for the fiscal year ending after June of year *t*, and zero otherwise.
Sales Growth	The change in sales from fiscal year *t* to *t+1*, divided by sales in year *t-1*, where the fiscal year *t* is the fiscal year ending after June of year *t*.

Potential Mediators:

%IndeDirectors	The number of independent directors divided by the number of all directors.
CEO Duality	An indicator variable that equals one if the CEO also holds the position of the chairman of the board, and zero otherwise.
Dual Class	An indicator variable that equals one if the firm has a dual class stock structure, and zero otherwise.
Poison Pill	An indicator variable that equals one if the firm uses a poison pill strategy, and zero otherwise.
Special Meeting	An indicator variable that equals one if the firm limits shareholders' ability to call a special meeting, and zero otherwise.
Spread	The average of the monthly closing bid-ask spread from $July_t$ to $June_{t+1}$, measured as the difference between bid and ask price divided by their average.
#8Ks	Number of 8-Ks filed between $July_t$ and $June_{t+1}$.
#MFs	Number of management earnings forecasts issued between $July_t$ and $June_{t+1}$.
#Analysts	Number of analysts following the firm in year *t*.
CEO Vega	The dollar change in the value of the CEO's option holdings for a 1% change in stock volatility, measured after the reconstitution.
Top 5 Vega	The dollar change in the value of the top 5 executives' option holdings for a 1% change in stock volatility, measured after the reconstitution.
CEO Delta	The dollar change in the CEO's wealth (stock holdings and option holdings) for a 1% change in stock price, measured after the reconstitution.
Top 5 Delta	The dollar change in the top 5 executives' wealth (stock holdings and option holdings) for a 1% change in stock price, measured after the reconstitution.
CEO Ownership	The shares held by the CEO divided by the total shares outstanding, measured after the reconstitution.
Top 5 Ownership	The shares held by the top 5 executives divided by the total shares outstanding, measured after the reconstitution.
CEO WPS	Scaled Wealth-Performance Sensitivity from Edmans et al. (2009). WPS is the dollar change in CEO wealth for a one percentage point change in firm value, divided by annual pay, measured after the reconstitution.

Routine and Non-Routine Trades:

Routine	An insider is designated as a routine trader if she makes at least one trade in the same calendar month for at least three consecutive years. After an insider is defined as a routine trader, all trades made in the same month as the month that established her as routine are classified as routine trades.
Non-Routine	All trades that are not classified as routine trades are classified as non-routine.

References

Aboody, D., and B. Lev. 2000. Information asymmetry, R&D, and insider gains. *Journal of Finance* 55 (6): 2747–66.

Angrist, J. D., and J.-S. Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ: Princeton University Press.

Appel, I. R., T. A. Gormley, and D. B. Keim. 2016. Passive investors, not passive owners. *Journal of Financial Economics* 121 (1): 111–41.

Badertscher, B. A., S. P. Hribar, and N. T. Jenkins. 2011. Informed trading and the market reaction to accounting restatements. *The Accounting Review* 86 (5): 1519–47.

Baron, R. M., and D. A. Kenny. 1986. The moderator-mediator variable distinction in social psychological research: Conceptual, strategic, and statistical considerations. *Journal of Personality and Social Psychology* 51 (6): 1173–82.

Bebchuk, L. A., K. J. M. Cremers, and U. C. Peyer. 2011. The CEO pay slice. *Journal of Financial Economics* 102 (1): 199–221.

Beneish, M. D., E. Press, and M. E. Vargus. 2012. Insider trading and earnings management in distressed firms. *Contemporary Accounting Research* 29 (1): 191–220.

Beny, L. N. 2005. Do insider trading laws matter? Some preliminary comparative evidence. *American Law and Economics Review* 7 (1): 144–83.

Bettis, J. C., J. L. Coles, and M. L. Lemmon. 2000. Corporate policies restricting trading by insiders. *Journal of Financial Economics* 57 (2): 191–220.

Bhattacharya, U., and H. Daouk. 2002. The world price of insider trading. *Journal of Finance* 57 (1): 75–108.

Bhattacharya, Utpal. 2014. Insider trading controversies: A literature review. *Annual Review of Financial Economics* 6: 385–403.

Billings, M. B., and M. C. Cedergren. 2015. Strategic silence, insider selling and litigation risk. *Journal of Accounting and Economics* 59 (2–3): 119–42.

Bird, A., and S. A. Karolyi. 2016. Do institutional investors demand public disclosure? *Review of Financial Studies* 29 (12): 3245–77.

Boone, A. L., and J. T. White. 2015. The effect of institutional ownership on firm transparency and information production. *Journal of Financial Economics* 117 (3): 508–33.

Bricker, R., and G. Markarian. 2015. Institutional investors and insider trading profitability. *European Accounting Review* 24 (3): 495–518.

Bushee, B. J. 2001. Do institutional investors prefer near term earnings over long-run value? *Contemporary Accounting Research* 18: 207–46.

Calonico, S., M. D. Cattaneo, M. H. Farrell, and R. Titiunik. 2019. Regression discontinuity designs using covariates. *Review of Economics and Statistics* 101 (3): 442–51.

Calonico, S., M. D. Cattaneo, and R. Titiunik. 2014. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica* 82 (6): 2295–2326.

Calonico, S., M. D. Cattaneo, and R. Titiunik. 2015. Optimal data-driven regression discontinuity plots. *Journal of the American Statistical Association* 110: 1753–69.

Carhart, M. M. 1997. On persistence in mutual fund performance. *Journal of Finance* 52 (1): 57–82.

Carlton, D. W., and D. R. Fischel. 1983. The regulation of insider trading. *Stanford Law Review* 35 (5): 857–95.

Chen, S., Y. Huang, N. Li, and T. Shevlin. 2019. How does quasi-indexer ownership affect corporate tax planning? *Journal of Accounting and Economics* 67: 278–96.

Cheng, and K. Lo. 2006. Insider trading and voluntary disclosures. *Journal of Accounting Research* 44 (5): 815–48.

Chung, K. H., and H. Zhang. 2011. Corporate governance and institutional ownership. *Journal of Financial and Quantitative Analysis* 46 (1): 247–73.

Cohen, L., C. Malloy, and L. Pomorski. 2012. Decoding inside information. *Journal of Finance* 67 (3): 1009–43.

Crane, A. D., S. Michenaud, and J. P. Weston. 2016. The effect of institutional ownership on payout policy: Evidence from index thresholds. *Review of Financial Studies* 29(6): 1377–1408.

Dai, L., R. Fu, J.-K. Kang, and I. Lee. 2016. Corporate governance and the profitability of insider trading. *Journal of Corporate Finance* 40: 235–53.

Edmans, A., X. Gabaix, and A. Landier. 2009. A multiplicative model of optimal CEO incentives in market equilibrium. *Review of Financial Studies* 22 (12): 4881–917.

Elitzur, R. R., and V. Yaari. 1995. Executive incentive compensation and earnings manipulation in a multi-period setting. *Journal of Economic Behavior and Organization* 26 (2): 201–19.

Fama, E. F., and K. R. French. 1993. Common risk factors in the returns on stocks and bonds. *Journal of Financial Economics* 33 (1): 3–56.

Frankel, R., and X. Li. 2004. Characteristics of a firm's information environment and the information asymmetry between insiders and outsiders. *Journal of Accounting and Economics* 37 (2): 229–59.

Gao, F., L. L. Lisic, and I. X. Zhang. 2014. Commitment to social good and insider trading. *Journal of Accounting and Economics* 57 (2–3): 149–75.

Henderson, M. T. 2011. Insider trading and CEO pay. *Vanderbilt Law Review* 64 (2): 503–56.

Huddart, S. J., and B. Ke. 2007. Information asymmetry and cross-sectional variation in insider trading. *Contemporary Accounting Research* 24 (1): 195–232.

Huddart, S., B. Ke, and C. Shi. 2007. Jeopardy, non-public information, and insider trading around SEC 10-K and 10-Q filings. *Journal of Accounting and Economics* 43 (1): 3–36.

Imbens, G. W., and T. Lemieux. 2008. Regression discontinuity designs: A guide to practice. *Journal of Econometrics* 142 (2): 615–35.

Imbens, G. W., and S. Wager. 2019. Optimized regression discontinuity designs. *The Review of Economics and Statistics* 101 (2): 264–78.

Jagolinzer, A., D. F. Larcker, and D. J. Taylor. 2011. Corporate governance and the information content of insider trades. *Journal of Accounting Research* 49 (5): 1249–74.

Jagolinzer, A., and D. T. Roulstone. 2009. The effects of insider trading regulation on trade timing, litigation risk, and profitability. *Stanford University working paper.*

Jeng, L. A. 1999. Corporate insiders and the window of opportunity. *Boston University working paper*.

Johnson, M. F., R. Kasznik, and K. K. Nelson. 2000. Shareholder wealth effects of the Private Securities Litigation Reform Act of 1995. *Review of Accounting Studies* 5: 217–33.

Johnson, M. F., K. K. Nelson, and A. C. Pritchard. 2007. Do the merits matter more? The impact of the Private Securities Litigation Reform Act. *Journal of Law, Economics and Organization* 23 (3): 627–52.

Khan, M., S. Srinivasan, and L. Tan. 2017. Institutional Ownership and Corporate Tax Avoidance: New Evidence. *The Accounting Review* 92 (2): 101–22.

Lakonishok, J., and I. Lee. 2001. Are insider trades informative? *Review of Financial Studies* 14 (1): 79–111.

Larcker, D. F., A. L. McCall, and G. Ormazabal. 2015. Outsourcing shareholder voting to proxy advisory firms. *The Journal of Law and Economics* 58 (1): 173–204.

Leland, H. E. 1992. Insider trading: Should it be prohibited? *Journal of Political Economy* 100 (4): 859–87.

Lewellen, J., and K. Lewellen. 2020. Institutional investors and corporate governance: The incentive to be engaged. *Journal of Finance*, forthcoming.

Lin, Y., Y. Mao, and Z. Wang. 2018. Institutional ownership, peer pressure, and voluntary disclosures. *The Accounting Review* 93 (4): 283–308.

Malenko, N., and Y. Shen. 2016. The role of proxy advisory firms: Evidence from a regression-discontinuity design. *Review of Financial Studies* 29 (12): 3394–427.

Manne, H. G. 1966. *Insider Trading and the Stock Market*. New York: Free Press.

Masson, R. T., and A. Madhavan. 1991. Insider trading and the value of thefirm. *Journal of Industrial Economics* 39 (4).

McCahery, J. A., Z. Sautner, and L. T. Starks. 2016. Behind the scenes: The corporate governance preferences of institutional investors. *Journal of Finance* 71 (6): 2905–32.

Park, M. S., and T. Park. 2004. Insider sales and earnings management. *Journal of Accounting and Public Policy* 23 (5): 381–411.

Piotroski, J., and D. Roulstone. 2005. Do insider trades reflect both contrarian beliefs and superior knowledge about future cash flow realizations? *Journal of Accounting & Economics* 39 (1): 55–81.

Rogers, J. L. 2008. Disclosure quality and management trading incentives. *Journal of Accounting Research* 46 (5): 1265–96.

Roulstone, D. T. 2003. The relation between insider-trading restrictions and executive compensation. *Journal of Accounting Research* 41 (3): 525–51.

Russell Investments. 2008. US equity indexes: Institutional benchmark survey. Annual Benchmark Survey.

Schmidt, C., and R. Fahlenbrach. 2017. Do exogenous changes in passive institutional ownership affect corporate governance and firm value? *Journal of Financial Economics* 124(2): 285–306.

Skaife, H. A., D. Veenman, and D. Wangerin. 2013. Internal control over financial reporting and managerial rent extraction: Evidence from the profitability of insider trading. *Journal of Accounting and Economics* 55 (1): 91–110.

Veenman, D. 2012. Disclosures of insider purchases and the valuation implications of past earnings signals. *The Accounting Review* 87 (1): 313–42.

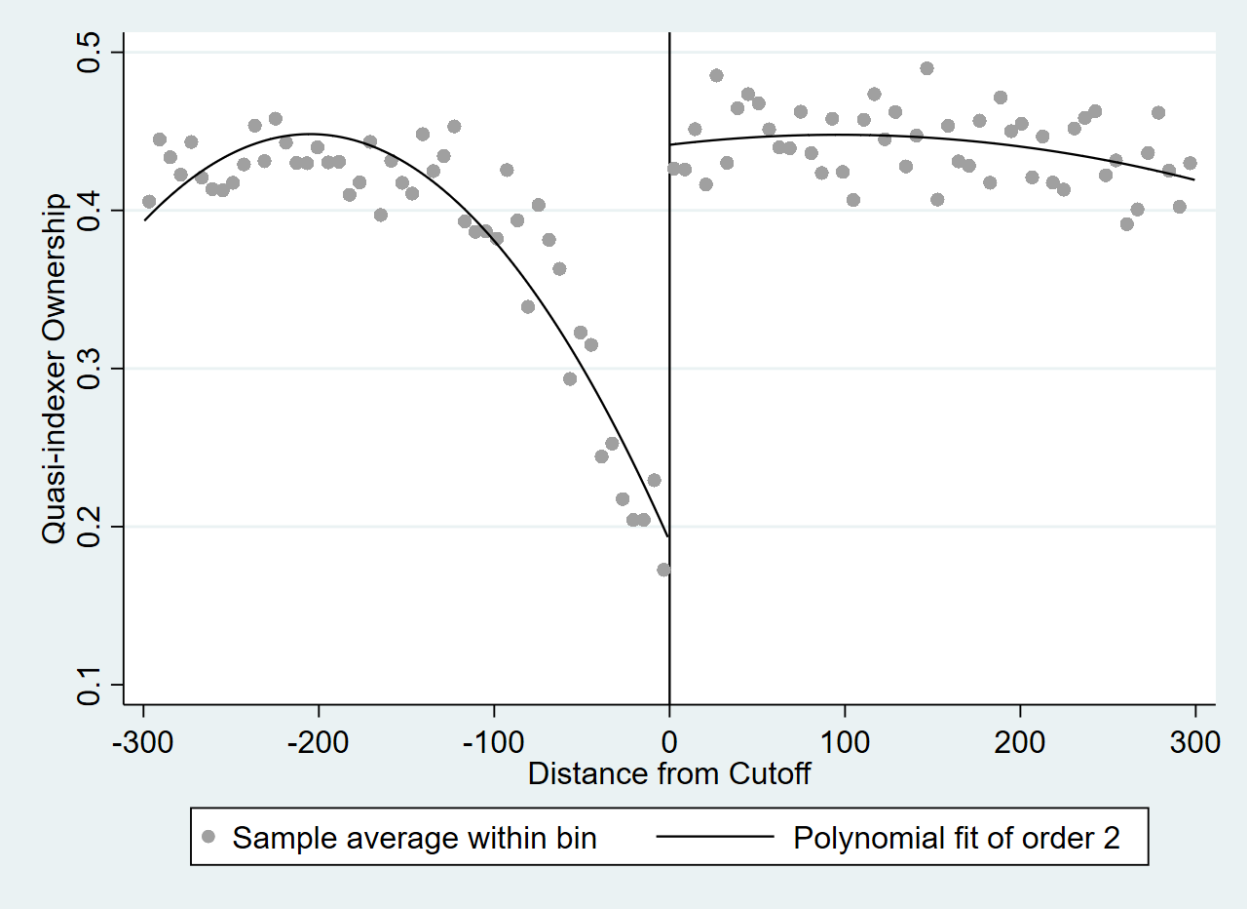
Wei, W., and A. Young. 2017. Selection bias or treatment effect? A re-examination of Russell 1000/2000 Index reconstitution. *University of Calgary working paper*.

Wooldridge, J. M. 2015. *Introductory econometrics: A modern approach*. Nelson Education.

Xu, J. 2008. New evidence on the effects of the Insider Trading Sanctions Act of 1984. *Purdue University working paper*.

Young, A. 2018. Will the real specification please stand up? A comment on Andrew Bird and Stephen Karolyi. *Econ Journal Watch* 15 (1): 35–48.

Figure 1 Quasi-indexer ownership around the index threshold



Notes: This figure displays a regression discontinuity plot theoretically developed by Calonico et al. (2015) using second order polynomials to approximate the regression function (excluding any control variables) with local sample averages of quasi-indexer institutional ownership within bins of the market capitalization ranking. Following Boone and White (2015) and Crane et al. (2016), among others, the x-axis represents the distance from the thresholds using the actual Russell ranking based on the float-adjusted, end-of-June market capitalization. Negative (Positive) values represent firms in the R1000 (R2000) and zero represents the smallest firm in the R1000. Each side has 50 bins. The bandwidth is 300. The sample period is 1995–2006.

Figure 2 Discontinuities in insider trading frequency and profitability

Figure 2A: Sell frequency

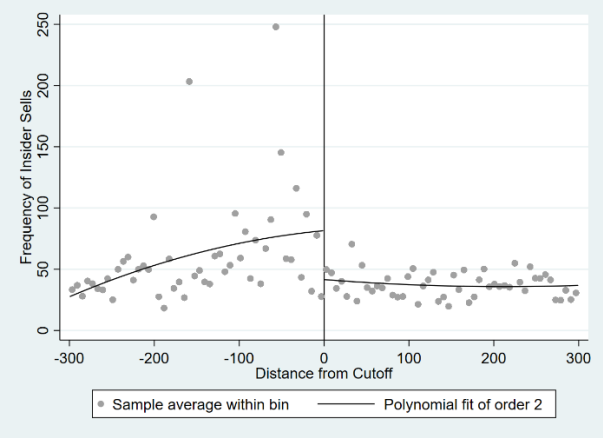


Figure 2B: Buy frequency

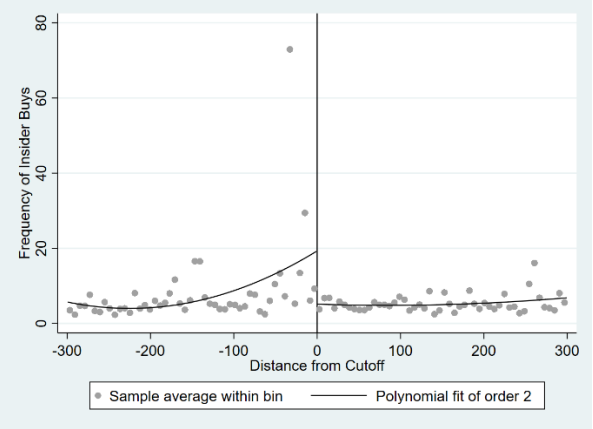


Figure 2C: Sell profitability

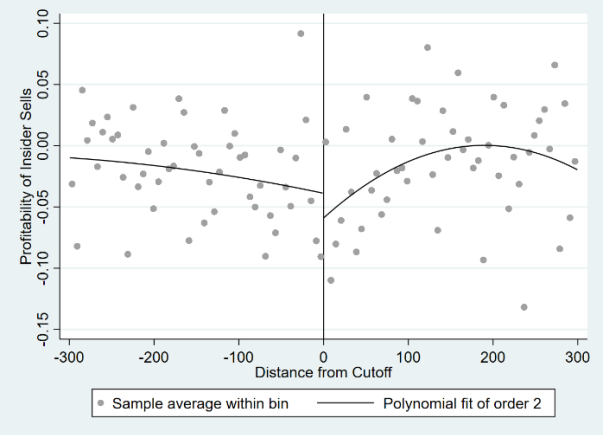
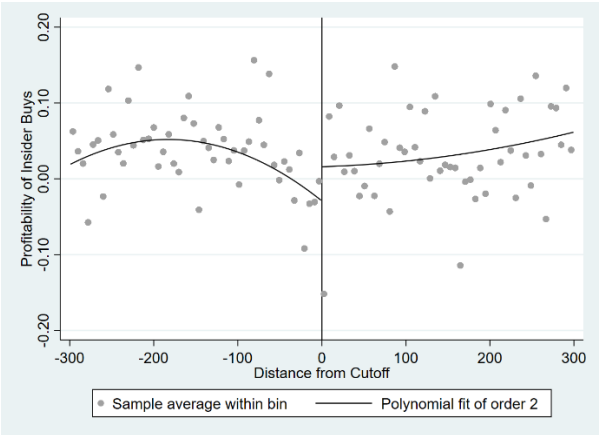
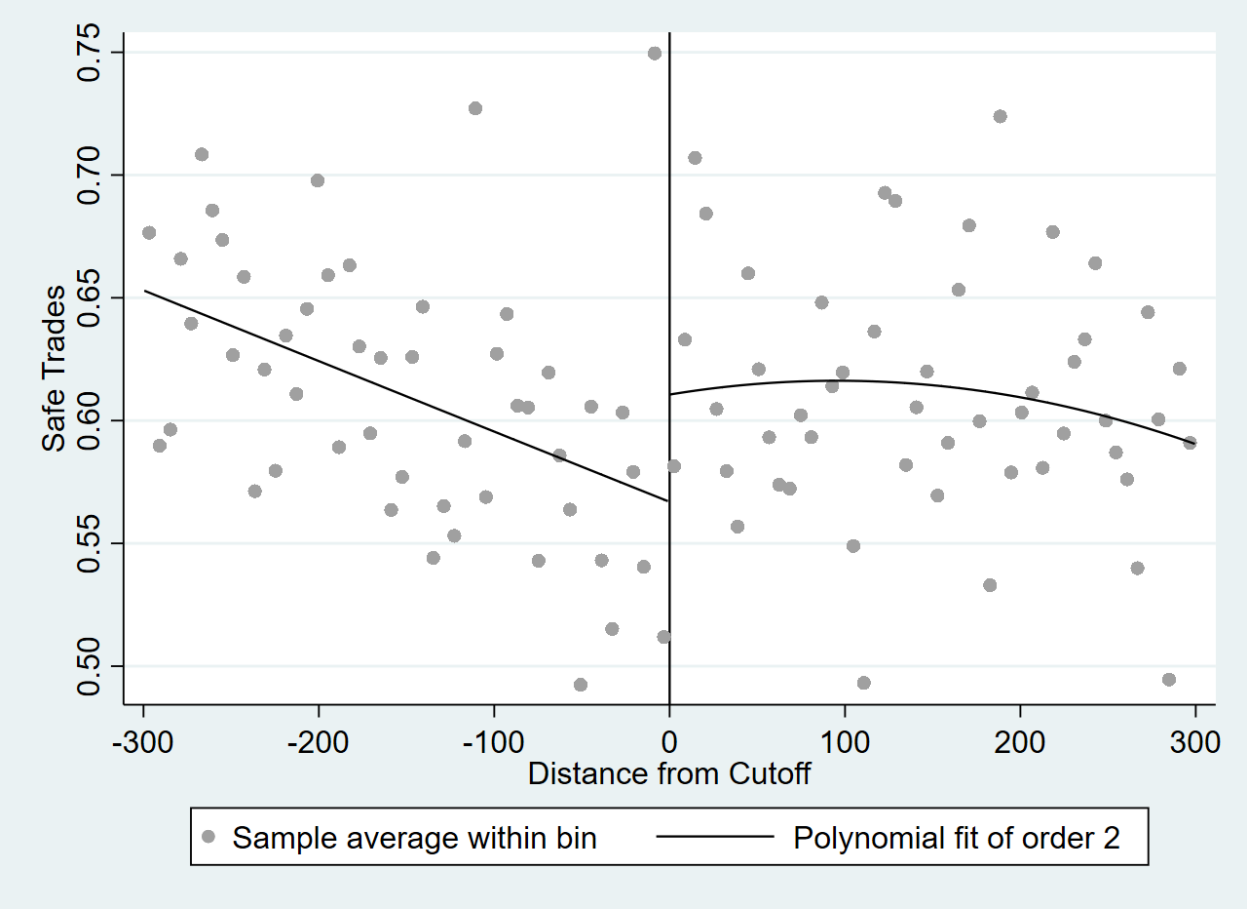


Figure 2D: Buy profitability



Notes: These figures display regression discontinuity plots using a bandwidth of +/-300. See the notes to Figure 1 for the details on how the plots are computed. Negative (Positive) values represent firms in the R1000 (R2000). The sample period is 1995–2006.

Figure 3 Discontinuity in Safe Trades



Notes: This figure displays a regression discontinuity plot using a bandwidth of +/-300. *Safe Trades* is based on the dollar volume of shares traded. For illustrative purposes, we use the unlogged values of *Safe Trades*. See the notes to Figure 1 for the details on how the plot is computed. Negative (Positive) values represent firms in the R1000 (R2000). The sample period is 1995–2006.

TABLE 1
Descriptive statistics

	Russell 1000					Russell 2000					All
	Mean	Median	SD	P25	P75	Mean	Median	SD	P25	P75	Mean
QuasiOwn	0.415	0.422	0.173	0.295	0.538	0.410	0.417	0.185	0.275	0.540	0.412
Sell Frequency	56.21	13.00	287.3	3.00	39.00	41.96	12.00	147.2	3.00	35.00	49.14
Buy Frequency	7.390	1.000	62.93	0.000	4.000	6.511	1.000	24.62	0.000	5.000	6.954
Sell Profitability	-0.022	-0.020	0.234	-0.133	0.087	-0.009	-0.008	0.251	-0.136	0.116	-0.016
Buy Profitability	0.028	0.022	0.213	-0.082	0.137	0.021	0.011	0.237	-0.088	0.132	0.025
Restriction	0.450	0.000	0.498	0.000	1.000	0.423	0.000	0.494	0.000	1.000	0.435
Safe Trades	0.616	0.689	0.343	0.336	0.952	0.559	0.653	0.343	0.317	0.939	0.606
B/M	0.437	0.394	0.398	0.243	0.580	0.443	0.409	0.446	0.249	0.594	0.440
LagReturn	0.437	0.213	1.331	-0.046	0.544	0.543	0.267	1.440	-0.021	0.712	0.489
Leverage	1.221	0.529	22.470	0.124	1.123	1.074	0.488	9.054	0.059	1.092	1.148
Volatility	0.001	0.001	0.001	0.000	0.001	0.001	0.001	0.002	0.000	0.001	0.001
ROA	0.034	0.042	0.171	0.013	0.085	0.028	0.044	0.212	0.012	0.085	0.031
R&D	0.367	0.000	0.482	0.000	1.000	0.383	0.000	0.486	0.000	1.000	0.375
Loss	0.154	0.000	0.361	0.000	0.000	0.159	0.000	0.366	0.000	0.000	0.156
Sales Growth	0.251	0.115	0.979	0.027	0.250	0.351	0.138	2.602	0.039	0.316	0.301

Notes: This table reports the descriptive statistics separately for all constituent stocks in the Russell 1000 and Russell 2000 indexes within a bandwidth of +/-300 around the index threshold. The sample period is 1995–2006. See the Appendix for variable definitions. *Restriction* and *Safe Trades* are based on the dollar volume of shares traded. For illustrative purposes, we use the unlogged values of *Safe Trades* here and in Table 2.

TABLE 2
Pre-assignment insider trading variables and firm characteristics

	Russell 1000			Russell 2000			RDD		
	Mean	Median	SD	Mean	Median	SD	RD Estimate	*z*-statistic	Optimal Bandwidth
QuasiOwn	0.413	0.418	0.171	0.384	0.383	0.186	0.021	0.95	500
Pre-assignment dependent variables:									
Sell Frequency	35.11	11.00	115.09	31.22	9.000	111.2	1.58	0.18	446
Buy Frequency	6.135	1.000	33.821	8.127	1.000	88.43	-23.14	-1.49	516
Sell Profitability	-0.018	-0.011	0.188	-0.012	-0.005	0.204	-0.034	-1.44	396
Buy Profitability	0.066	0.043	0.211	0.082	0.051	0.241	0.006	0.18	521
Safe Trades	0.597	0.654	0.341	0.586	0.635	0.344	0.042	0.84	396
Pre-assignment continuous firm characteristics:									
B/M	0.418	0.382	0.267	0.435	0.400	0.293	-0.034	-0.74	396
LagReturn	0.413	0.241	0.887	0.445	0.243	0.972	0.153	1.43	400
Leverage	1.014	0.533	2.026	0.852	0.451	1.661	-0.327	-0.92	368
Volatility	0.001	0.000	0.001	0.001	0.001	0.001	0.000	-0.82	488
ROA	0.046	0.046	0.086	0.033	0.043	0.117	0.002	0.16	497
Sales Growth	0.252	0.122	0.579	0.316	0.144	0.819	-0.072	-1.09	484
Pre-assignment dichotomous firm characteristics:									
	Mean	Median	SD	Mean	Median	SD	Difference in Means	*t*-statistic	Fixed Bandwidth
Restriction	0.418	0.000	0.493	0.417	0.000	0.493	-0.028	-1.15	100
R&D	0.370	0.000	0.483	0.393	0.000	0.488	-0.030	-1.37	100
Loss	0.144	0.000	0.352	0.165	0.000	0.371	-0.016	-0.93	100

Notes: This table reports the descriptive statistics and regression discontinuity nonparametric estimates for the discontinuities in pre-assignment variables around the R1000/2000 threshold. The RD design implements local-polynomial based inference procedures theoretically developed by Calonico et al. (2014) and Calonico et al. (2019). The table reports bias-corrected RD estimates with bandwidth-robust and cluster-robust standard errors. The regressions include the float adjustment variable, *FloatAdj*. The triangular kernel function is used to construct the local third-order polynomial estimators. Standard errors are adjusted for clusters at the firm level. The bandwidth selection is the optimal bandwidth, which is covariate-adjusted and cluster-robust. The optimal bandwidth is also used to determine the descriptive statistics for each variable. All variables in this table are measured before the index reconstitution. See the Appendix for variable definitions. *Restriction* and *Safe Trades* are based on the dollar volume of shares traded. For illustrative purposes, we use the unlogged values of *Safe Trades*.

TABLE 3
Quasi-indexer ownership and insider trading frequency

Panel A: Frequency of insider sells using a regression discontinuity design

	Sell Trades (1)	*Sell Trades* (2)	*Sell Dollar Volume* (3)	*Sell Share Volume* (4)	*Routine Sells* (5)	*Non-Routine Sells* (6)
RD Estimate	**-54.54*** **	**-81.17*** **	**-32.12*** **	**-1.371*** **	-0.70	**-90.96*** **
	(17.62)	(21.09)	(10.77)	(0.352)	(2.78)	(23.76)
Optimal Bandwidth	326	302	380	340	313	341
Polynomial Order	2	3	3	3	3	3
Float Adjust	Yes	Yes	Yes	Yes	Yes	Yes
DedicOwn & TransOwn	Yes	Yes	Yes	Yes	Yes	Yes
Bias-Correction	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth-Robust SE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster-Robust SE	Yes	Yes	Yes	Yes	Yes	Yes
Effective No. of Obs.	6,255	5,792	7,293	6,532	2,706	5,673

Panel B: Frequency of insider buys using a regression discontinuity design

	Buy Trades (1)	*Buy Trades* (2)	*Buy Dollar Volume* (3)	*Buy Share Volume* (4)	*Routine Buys* (5)	*Non-Routine Buys* (6)
RD Estimate	**-31.13** **	**-40.66** **	**-2.576** **	**-0.190*** **	**-58.72** **	**-31.00** **
	(12.60)	(15.94)	(1.118)	(0.064)	(22.81)	(13.63)
Optimal Bandwidth	280	328	442	433	313	341
Polynomial Order	2	3	3	3	3	3
Float Adjust	Yes	Yes	Yes	Yes	Yes	Yes
DedicOwn & TransOwn	Yes	Yes	Yes	Yes	Yes	Yes
Bias-Correction	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth-Robust SE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster-Robust SE	Yes	Yes	Yes	Yes	Yes	Yes
Effective No. of Obs.	5,379	6,290	8,507	8,311	1,242	6,384

TABLE 3 (cont'd)

Panel C: Insider trading frequency using an instrumental variable design

	Dependent Variable: Frequency of Insider Trades									
	Sell Trades (1)	*Sell Dollar Volume* (2)	*Sell Share Volume* (3)	*Routine Sell Trades* (4)	*Non-Routine Sell Trades* (5)	*Buy Trades* (6)	*Buy Dollar Volume* (7)	*Buy Share Volume* (8)	*Routine Buy Trades* (9)	*Non-Routine Buy Trades* (10)
$\widehat{QuasiOwn}$	**-377.7****	**-137.1****	**-5.34*****	**-28.2***	**-408.1****	**-112.1****	**-18.4*****	**-0.92*****	**-108.8*****	**-76.5***
	(-2.20)	(-2.31)	(-2.64)	(-1.65)	(-2.32)	(-2.12)	(-2.58)	(-2.93)	(-2.63)	(-1.81)
Bandwidth	300	300	300	300	300	300	300	300	300	300
Float Adjust	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DedicOwn &TransOwn	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year & Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm & Year Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,565	5,565	5,565	2,520	5,417	5,565	5,565	5,565	1,164	5,417
Adjusted R^2	0.042	0.035	0.023	0.035	0.060	0.009	0.001	0.010	-0.043	0.003

Notes: Panel A (B) reports the regression discontinuity nonparametric estimates for the discontinuity in insider sell (buy) frequency around the R1000/R2000 threshold. Standard errors are provided below each estimate in parentheses. The RD design implements local-polynomial based inference procedures theoretically developed by Calonico et al. (2014) and Calonico et al. (2019). The table reports bias-corrected RD estimates with a bandwidth-robust variance estimator using triangular kernel and clustering with plug-in residuals at running variable level (market capitalization rank). The regressions include *FloatAdj*, *TransOwn*, and *DedicOwn*. The optimal bandwidth selection is based on a second-generation plug-in bandwidth selection approach and is covariate-adjusted and cluster-robust. Panel C reports the second-stage results from a two-stage instrumental variable (IV) design. The untabulated first-stage results are from estimating equation 3 and the tabulated second-stage results are from estimating equation 4 where the dependent variable $Frequency_{it}$ is based on various measures for insider selling activity (Columns 1 to 5) and insider buying activity (Columns 6 to 10) from July of year *t* to the following June. Standard errors are corrected for two-way clustering at the firm and year level. The corresponding *t*-statistics are reported in parentheses below each estimate. ***, **, and * indicate significance at the 1%, 5% and 10% levels, respectively. See the Appendix for variable definitions.

TABLE 4
Quasi-indexer ownership and insider trading profits

Panel A: Profitability of insider sells using a regression discontinuity design

	Sell Trades (1)	*Sell Trades* (2)	*Routine Sells* (3)	*Non-Routine Sells* (4)
RD Estimate	**-0.076*** **	**-0.093*** **	-0.057	**-0.096*** **
	(0.01)	(0.012)	(0.050)	(0.012)
Optimal Bandwidth	158	200	394	197
Polynomial Order	2	3	3	3
Float Adjust	Yes	Yes	Yes	Yes
DedicOwn & TransOwn	Yes	Yes	Yes	Yes
Bias-Correction	Yes	Yes	Yes	Yes
Bandwidth-Robust SE	Yes	Yes	Yes	Yes
Cluster-Robust SE	Yes	Yes	Yes	Yes
Effective No. of Obs.	34,762	43,850	4,035	42,971

Panel B: Profitability of insider buys using a regression discontinuity design

	Buy Trades (1)	*Buy Trades* (2)	*Routine Buys* (3)	*Non-Routine Buys* (4)
RD Estimate	**0.025***	0.013	0.018	**0.078*** **
	(0.013)	(0.013)	(0.024)	(0.017)
Optimal Bandwidth	255	397	499	285
Polynomial Order	2	3	3	3
Float Adjust	Yes	Yes	Yes	Yes
DedicOwn & TransOwn	Yes	Yes	Yes	Yes
Bias-Correction	Yes	Yes	Yes	Yes
Bandwidth-Robust SE	Yes	Yes	Yes	Yes
Cluster-Robust SE	Yes	Yes	Yes	Yes
Effective No. of Obs.	13,978	20,937	2,255	15,046

TABLE 4 (cont'd)

Panel C: Profitability of insider trades using an instrumental variable design

	Dependent Variable: Profitability of Insider Trades					
	Insider Sells (1)	*Routine Insider Sells* (2)	*Non-Routine Insider Sells* (3)	*Insider Buys* (4)	*Routine Insider Buys* (5)	*Non-Routine Insider Buys* (6)
$\widehat{QuasiOwn}$	**-0.163****	-0.012	-0.130	-0.130	-0.246	-0.022
	(-2.06)	(-0.06)	(-1.50)	(-1.25)	(-1.19)	(-0.15)
Bandwidth	100	200	100	100	200	100
Float Adjust	Yes	Yes	Yes	Yes	Yes	Yes
DedicOwn &TransOwn	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Year & Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Transaction Date Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,902	1,938	20,742	5,828	1,274	5,589
Adjusted R^2	0.095	0.195	0.093	0.131	0.391	0.145

Notes: Panel A (B) reports the regression discontinuity nonparametric estimates for the discontinuity in insider sell (buy) profitability around the R1000/R2000 threshold. Standard errors are provided below the estimates in parentheses. Panel C reports the second-stage results from a two-stage instrumental variable (IV) design. The untabulated first-stage results are from estimating equation 3 and the tabulated second-stage results are from estimating equation 4 where the dependent variable $Profitability_{jt}$ is the insider trade profitability over the 180 calendar days following insider trade *j* made between July in year *t* and the following June. Standard errors are corrected for clustering at the transaction date level. The corresponding *t*-statistics are reported below the estimate in parentheses. See the notes to Table 3 for details of the estimation procedures. ***, **, and * indicate significance at the 1%, 5% and 10% levels, respectively. See the Appendix for variable definitions.

TABLE 5
Quasi-indexer ownership and insider trading restrictions

Panel A: Fraction of insider trading that takes place during safe periods

	Dependent Variable: *Safe Trades*			
	Share Volume (1)	*Share Volume* (2)	*Dollar Volume* (3)	*Dollar Volume* (4)
RD Estimate	**0.042****	**0.054*****	**0.043****	**0.051****
	(0.020)	(0.021)	(0.020)	(0.022)
Optimal Bandwidth	352	548	334	473
Polynomial Order	2	3	2	3
Float Adjust	Yes	Yes	Yes	Yes
DedicOwn & TransOwn	Yes	Yes	Yes	Yes
Bias-Correction	Yes	Yes	Yes	Yes
Bandwidth-Robust SE	Yes	Yes	Yes	Yes
Cluster-Robust SE	Yes	Yes	Yes	Yes
Effective No. of Obs.	5,951	9,345	5,400	7,676

TABLE 5 (cont'd)

Panel B: Likelihood of having an inferred insider trading blackout policy

	Dependent Variable: *Restriction*	
	Share Volume	*Dollar Volume*
	(1)	(2)
$\widehat{QuasiOwn}$	**0.948*** **	**0.753*** **
	(3.71)	(2.92)
Bandwidth	300	300
Float Adjust	Yes	Yes
DedicOwn & TransOwn	Yes	Yes
Control Variables	Yes	Yes
Firm & Year Clustered SE	Yes	Yes
Observations	5,441	5,200

Notes: Panel A reports the regression discontinuity nonparametric estimates for the discontinuities in *Safe Trades* around the R1000/R2000 threshold. Standard errors are provided below each estimate in parentheses. *Safe Trades* is the natural logarithm of the fraction of shares traded by insiders (Columns 1 and 2) or the fraction of the dollar volume traded by insiders (Columns 3 and 4) from $July_t$ to $June_{t+1}$ that occur within the 30-day window following the quarterly earnings announcement. See the notes to Table 3 for details of the estimation procedures. Panel B employs a two-stage instrumental variable (IV) design. The first stage results are from estimating equation 3 and the second stage results are from estimating a probit regression version of equation 4 where *Restriction* is the dependent variable. *Restriction* equals one if 75% or more of shares traded by insiders (Column 1) or dollar volume traded by insiders (Column 2) from $July_t$ to $June_{t+1}$ occur within 30 days after a quarterly earnings announcement, and zero otherwise. We use bootstrapping with 1,000 replications to obtain valid standard errors in the second stage (Wooldridge 2015). Bootstrapped standard errors are used in the construction of the *t*-statistics that are reported in parentheses. *** and ** indicate significance at the 1% and 5% levels, respectively.

TABLE 6
Discontinuity in potential mediator variables

	Second-order polynomial		Third-order polynomial		
	RD Estimate	**Optimal Bandwidth**	**RD Estimate**	**Optimal Bandwidth**	**Discontinuity?**
Corporate governance variables					
%IndeDirectors	0.310***	186	0.420***	226	Yes
CEO Duality	0.118***	395	0.262***	303	Yes
Dual Class	-0.407***	161	-0.408***	241	Yes
Poison Pill	0.562***	182	0.675***	142	Yes
Special Meeting	0.159***	333	0.186***	418	Yes
Information environment variables					
Spread	-0.001**	315	-0.002***	346	Yes
#8Ks	0.658	385	0.640	308	No
#MFs	0.205	345	0.271	395	No
#Analysts	1.958***	277	2.628***	337	Yes
Equity incentive variables					
CEO Vega	25.73*	223	27.06*	334	Yes
Top 5 Vega	22.53	501	-65.94	666	No
CEO Delta	-605.9	282	-688.1	357	No
Top 5 Delta	-1,119**	318	-1,488**	380	Yes
CEO Ownership	-12.70***	177	-16.04***	233	Yes
Top 5 Ownership	-12.18***	178	-15.19***	242	Yes
CEO WPS	-46.23	489	-131.4	417	No

Notes: This table reports the regression discontinuity nonparametric estimates for the discontinuities in potential mediator variables around the R1000/R2000 threshold. The RD design implements local-polynomial based inference procedures theoretically developed by Calonico et al. (2014) and Calonico et al. (2019), and reports bias-corrected RD estimates with a bandwidth-robust variance estimator using triangular kernel and clustering with plug-in residuals at the market capitalization rank level. The optimal bandwidth selection is based on a second-generation plug-in bandwidth selection approach and is covariate-adjusted and cluster-robust. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. See the Appendix for variable definitions.

TABLE 7
Mediators between quasi-indexer ownership and insider trading

	Is the variable a mediator?			
	Sell Frequency	***Buy Frequency***	***Sell Profitability***	***Safe Trades***
Corporate Governance Variables				
%IndeDirectors (S)	No	Partial	Partial	Total
CEO Duality (W)	No	No	No	No
Dual Class (S)	No	No	Partial	No
Poison Pill (W)	No	No	No	Total
Special Meeting (W)	No	No	No	No
Information Environment Variables				
Spread (S)	No	No	No	No
#Analysts (S)	No	No	No	No
Equity Incentive Variables				
CEO Vega (S)	No	Total	No	Total
Top 5 Delta (W)	No	Total	Partial	Total
CEO Ownership (W)	Partial	Total	Total	Partial
Top 5 Ownership (W)	No	Total	No	Total

Notes: This table reports on the potential mediating variables between quasi-indexer ownership and insider trading that are identified in Table 6. A potential mediating variable is classified as a Total mediator if including it in equation 2 eliminates the significance of both the second- and third-order polynomial RD estimates at the 10% level or better. A potential mediating variable is classified as a Partial mediator if including it in equation 2 results in a reduction in the absolute magnitude of the RD estimate of at least 20% and both RD estimates remain significant at the 10% level or better, or if one of the estimates becomes insignificant. Otherwise, a potential mediator variable is not classified as a mediator and is classified as No. (S) [(W)] means that the corresponding RD estimate in Table 6 implies that higher quasi-indexer ownership around the index threshold results in stronger [weaker] corporate governance attributes and equity-based incentives and more information production. See the Appendix for variable definitions.