

Nominal Price Preferences, Investor Welfare and Market Quality*

Allaudeen Hameed[†] Zhenghui Ni[‡] Yuanyuan Pan[§]

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Abstract

We show that a preference for low-priced stocks is individually costly but collectively beneficial. Exploiting staggered reverse splits induced by a minimum trading price rule and account-level trading records, we find that retail investors reduce both buying and selling of affected stocks by 77% and migrate to other low-priced stocks. Following this reallocation, their low-price trades underperform their own high-price trades by 2.8% annually. As retail trading leaves affected stocks, liquidity deteriorates, price discovery slows, and valuations fall by 5%. A rule designed to protect retail investors thus relocates the private cost of their trading while eroding its public benefits.

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[†]National University of Singapore, Department of Finance, allaudeen@nus.edu.sg

[‡]Renmin University of China, Business School, nizhenghui@rmbs.ruc.edu.cn

[§]National University of Singapore, Department of Finance, yuanyuan@u.nus.edu

1 Introduction

Behavioral preferences that lead retail investors to misallocate their own wealth are usually treated as a private cost, so correcting them looks like an unambiguous gain. But a preference that harms the investors who hold it may also benefit the markets they trade in. Where it does, investor protection and market quality are in tension: a rule that removes the preference is meant to help its holders, and it collectively hurts the market. We study a regulation that set out to curb one such preference.

The preference we study is retail investors’ well-documented attraction to low-priced (“cheap”) stocks. A large literature establishes that retail investors gravitate toward low-nominal-price stocks even though the nominal price carries no fundamental information, and attributes this to nominal price illusion, lottery-like preferences, or non-proportional thinking (Birru and Wang, 2016; Green and Hwang, 2009; Hirshleifer, 2001; Kumar, 2009; Shue and Townsend, 2021). The preference is pervasive: as Figure 1 shows, median stock prices in Singapore, Hong Kong, China, and other retail-dominated markets have remained below US\$1 for decades, reflecting prevailing listing norms shaped by investor-driven demand.¹ What remains unknown is whether this preference is merely innocuous or instead generates a tradeoff: costly for investors themselves, yet supportive of market quality.

We study this tradeoff using a quasi-experimental shock to nominal prices: the Singapore Exchange (SGX)’s Minimum Trading Price (MTP) rule. In March 2015, SGX required all mainboard-listed firms to maintain a six-month volume-weighted average price of at least S\$0.20 (approximately US\$0.15) or face placement on a watchlist, heightened regulatory scrutiny, and the risk of delisting.² Around 40% of SGX-listed firms traded at or below S\$0.20 at the rule’s introduction (see Figure 2), so the compliance pressure to lift nominal prices mechanically via reverse splits applied to a large number of firms.

Three features of the policy make it well-suited for causal inference. First, the reverse splits were regulation-driven rather than manager-initiated, so the price jump is plausibly exogenous to firm fundamentals and managerial intent.³ Second, the

¹Globally, nominal prices are anchored at market-specific levels (Bae et al., 2019). In the U.S., with higher institutional participation, stocks have anchored around US\$25 since the 1920s, despite large shifts in inflation and market capitalization (Weld et al., 2009), and firms appear to manage prices toward investor preferences and industry norms (Baker, Greenwood, and Wurgler, 2009). Balasubramaniam et al. (2023) show that retail price clientele demands shape direct stockholding and influence returns in India. Table A.1 reports average prices across a broader list of markets.

²SGX introduced the MTP to curb excessive speculation and potential manipulation in low-priced stocks. The framework was refined in 2016 by adding a market-capitalization screen. SGX removed the MTP rule on 1 June 2020.

³Traditional splits and reverse splits can be motivated by signaling and catering considerations. The signaling hypothesis suggests stock splits convey private information about a firm’s future performance (Fama et al., 1969; Grinblatt, Masulis, and Titman, 1984; Ikenberry, Rankine, and Stice, 1996; Louis and Robinson, 2005; McNichols and Dravid, 1990). Managers with favorable private information have an incentive to conduct a stock split to attract analyst attention (Brennan and Hughes, 1991). The catering hypothesis suggests that stock splits restore prices to a “normal” range (Lakonishok

regulators granted a multi-year grace period, so compliance unfolded on staggered dates across otherwise similar low-price firms. Third, early- and late-treated firms are indistinguishable across a broad set of pre-treatment firm characteristics, which rules out the concern that managers with better private information about future firm quality timed compliance strategically.⁴

Our analysis links administrative, account-level trades from Singapore’s largest retail broker with exchange-level stock data and hand-collected corporate actions. At the investor level, we compare the same investor trading the same stock before versus after its reverse split, while tracking that investor’s activity in other low-price stocks that remain untreated. At the stock level, the staggered timing supports within-stock identification of changes in liquidity, price efficiency, return dynamics, and valuations. This combination allows us to trace individual behavioral responses and show how they scale up into market-wide consequences.

On the investor side, the reverse split produces a large and durable decline in retail trading activity. For the same investor trading the same stock, dollar volume falls by 77% and trade counts decline almost as much, with no pre-trends and trading volume remaining depressed for more than a year. This effect scales with the size of the price jump, consistent with the nominal price level itself driving investor behavior.

When a familiar “cheap” stock becomes “expensive”, we find that investors substitute into other sub-S\$0.20 stocks. Following the first reverse split in an investor’s portfolio, the proportion of their trading allocated to untreated low-priced stocks rises by about 13%, revealing a stable underlying preference for a low price label which they actively seek to restore elsewhere in their portfolios.

This preference is welfare-reducing. If retail investors target low-priced stocks because of informational advantages, such as firm familiarity or superior attention to small illiquid firms (Boehmer et al., 2021; Ivković and Weisbenner, 2005), their trades in that segment should outperform. We test this using a within-investor buy-minus-sell (BMS) portfolio that tracks the subsequent returns on each investor’s net purchases within a price tier, absorbing investor- and price-tier-level differences via granular fixed effects. Net retail investor purchases underperform in both low and high price tiers, and by more in low-priced stocks. Following the first reverse split in an investor’s portfolio, we find that the net-buy low-price portfolio underperforms the same investor’s net-buy high-price portfolio by 2.8% per annum. The underperformance occurs at the moment of reallocation, which is inconsistent with informed trading.⁵

and Lev, 1987) or optimize tick size as a percentage of stock price (Angel, 1997; Schultz, 2000). Neither motivation applies in our setting.

⁴The early- and late-treated firms share similar fundamental characteristics, including profitability (ROA, ROE), leverage, distress risk (Altman-Z), dividend policy, past return performance, volatility, liquidity, and retail trading activity (see Table A.2). This evidence supports a staggered difference-in-differences (DiD) design in which later-treated firms serve as credible counterfactuals for earlier-treated ones.

⁵We measure the return to active selection within a price tier, not the return on holdings; Welch (2022) finds the aggregate Robinhood holdings portfolio performed well and notes that holdings

Additional findings separate the salient price interpretation from alternative explanations. First, the volatility and skewness of treated stocks are unchanged after the split, so investors react to the salient nominal price label rather than any change in return distribution, consistent with the price illusion mechanism of Birru and Wang (2016). Second, capital constraints do not explain the retreat. SGX reduced the minimum round-lot size from 1,000 to 100 shares in January 2015, an order of magnitude drop in the capital required to establish a position. Yet investor-by-price-tier tests around the reform show no shift of retail flow toward higher-priced stocks and no attenuation of low-price tilts. Third, the pullback in trading following the reverse split is more pronounced among male, younger, and more active investors and margin traders, consistent with speculative demand driven by low price preference among retail investors (Barber and Odean, 2001; Kumar, 2009).

On the market side, the picture reverses. Treated stocks deteriorate across every dimension of market quality we examine. Retail trading volume collapses by 78% and is not replaced by institutional investors or other participants.⁶ Liquidity, measured by turnover, bid-ask spreads and zero-trade days (Lesmond, 2005), deteriorates sharply and persistently. This is in contrast to manager-initiated reverse splits which are endogenously timed when post-split conditions are favorable (Han, 1995). Price efficiency also suffers: using the delay measures of Hou and Moskowitz (2005), treated stocks become more sluggish in incorporating market-wide news.

Valuations decline by approximately 5% at the time of the split, with no subsequent drift or reversal and no change in earnings, dividends, volatility, or skewness. With fundamentals unchanged, the decline reflects the departure of the trading clientele rather than information revelation. Consistent with a non-informational interpretation, we also find that insiders (directors, CEOs, and substantial shareholders) do not sell ahead of the split announcements. The departure of the trading clientele can affect valuations through two channels that our setting does not separate. First, lower liquidity and slower price discovery are capitalized into a lower price. Second, a speculative preference for low-priced stocks had kept the price above its fundamental value, and the reverse split withdrew that trading demand (Cosemans and Frehen, 2021). Under either channel, the loss falls on the investors that the rule was written to protect.

Next, we trace the demand shift into return dynamics. First, short-term return reversals, which before the split are strongest in low-priced, retail-heavy stocks, attenuate sharply afterward, and the attenuation is larger where retail intensity was higher. This is consistent with a demand-for-immediacy channel (Foucault, Sraer, and Thesmar, 2011; Karolyi, Lee, and Van Dijk, 2012; Koch, Ruenzi, and Starks, 2016):

results are not comparable to trade-based ones. Barber, Lin, and Odean (2024) reconcile the positive predictive power of retail order imbalance with retail losses: the predictability is equal-weighted, while retail purchases concentrate in attention-grabbing stocks.

⁶Because the not-yet-treated control stocks are themselves low-priced, displaced retail volume could inflate this estimate, a violation of the Stable Unit Treatment Value Assumption. Section 5 bounds it by varying the price tier of the control group.

retail marketable orders create transitory price pressure that mean-reverts, so fewer such orders mean less pressure to unwind.⁷

Second, common returns can arise from correlated trading by shared investor clienteles, not just shared fundamentals, especially along a salient, non-fundamental category like nominal price (Green and Hwang, 2009; Kumar and Lee, 2006). In a difference-in-differences design with matched low-price controls, we find that the stock's beta on the low-price portfolio falls by 0.68 after the reverse split, while the beta on the high-price portfolio rises by 1.61. A non-informational price jump thus rewires return correlations by shifting the investor base, moving the stock from one demand-driven price tier to another.

Finally, we examine the impact of the announcement of an MTP rule on the low-price segment of the market as a whole. The question is topical: minimum-price rules have spread in recent years, most recently to Indonesia (2023), Saudi Arabia (2025), and Korea (2026). We use three MTP announcements that come closest to a clean test, in Poland (December 2013), Singapore (March 2015), and Korea (May 2026); elsewhere the rule is bundled with other regulatory changes. Stocks below the MTP lose 3% to 5% over the five days following the announcement, and lose more where retail ownership is higher. The announcement effect mirrors the split-month valuation decline: the market prices the departure of the low-price clientele in expectation when the rule arrives, and in realization when the firm complies.

Our paper contributes to three strands of the literature. The first strand documents that retail investors are drawn to low nominal prices, and that nominal price organizes trading and returns (Baker, Greenwood, and Wurgler, 2009; Birru and Wang, 2016; Dyl and Elliott, 2006; Green and Hwang, 2009; Kumar, 2009; Shue and Townsend, 2021; Weld et al., 2009). This work shows that the preference exists based on observed prices and revealed trading decisions. Recent literature argues that the preference is not behavioral: firms may choose prices to balance tick-size against lot-size frictions (Li and Ye, 2022), round-lot rules price small investors out of high-priced stocks in China (Du et al., 2025), and fractional trading draws them back into expensive US stocks (Da, Fang, and Lin, 2025). Neither setting separates a constraint from a preference, since the lot size does not vary in the first and the reform bundles constraint relief with a new product in the second. We exploit the MTP rule, which moves the nominal price while holding the firm fixed, and the 2015 lot-size reduction, which varies the capital constraint directly and leaves the low-price tilt intact, hence separating preferences from other factors. Our account-level data follow the same investor across price tiers, absorbing differences in skill and information to establish two new facts. The preference is a stable trait rather than an attachment to particular stocks: displaced investors rebuild it in other low-priced stocks. Green and Hwang

⁷A liquidity-supply account predicts the opposite. If retail investors were supplying immediacy, their withdrawal would raise the compensation to remaining suppliers and strengthen reversals, and strengthen them most where retail participation was highest (Kaniel, Saar, and Titman, 2008; Nagel, 2012; Pástor and Stambaugh, 2003). Foucault, Sraer, and Thesmar (2011) report the same direction as we do: return reversals weaken after a French reform that halved speculative retail trading.

(2009) conjecture that small investors drive price-based comovement, and we supply the clientele evidence their conjecture requires. Moreover, the preference is costly, as the same investor's low-price trades underperform their own high-price trades.

The second strand asks whether retail trading helps or harms the markets it occupies, and the evidence is divided. Foucault, Sraer, and Thesmar (2011) study a French reform that halved speculative retail trading and find that idiosyncratic volatility falls and reversals weaken; Peress and Schmidt (2020) find that liquidity deteriorates when news distracts noise traders. Eaton et al. (2022) reconcile the two with brokerage outages: liquidity improves when Robinhood investors cannot trade and worsens when investors at traditional brokers cannot, so the sign depends on whether the departing traders herd or follow contrarian strategies. Our setting differs on three margins this literature has not varied. The removal is permanent rather than a distraction or a half-hour outage; it applies to the sub-\$0.20 segment these studies screen out; and we observe the departing investors in their own accounts rather than inferring them from trade prices, an inference whose accuracy varies with the spread (Barber et al., 2024).

The third strand studies regulation aimed at protecting retail investors. Heimer and Imas (2022) show that leverage limits improve retail decisions by raising the cost of postponing the realization of a loss. Our result is the counterpart: a rule that constrains a behavior can discipline it, but a rule that removes a characteristic from one stock cannot, because the characteristic remains available elsewhere. The concern motivating such rules is real: brokers in emerging markets run pump-and-dump schemes that attract traders who buy on rising prices and then bear the subsequent decline (Khwaja and Mian, 2005), and firms use splits to inflate share prices while retail traders buy and insiders sell around the split (Titman, Wei, and Zhao, 2022). Retail investors nonetheless kept the preference and paid for it, while the stocks they left lost the liquidity, price efficiency, and value their presence had supported. Regulators designing MTP rules should therefore weigh protection benefits against market quality costs that may be substantial in retail-dominated markets. More broadly, nominal price emerges as a first-order state variable for retail demand: one that organizes participation, shapes trading frictions, and prices into valuations in ways that standard asset pricing models do not capture.

The remainder of the paper is organized as follows. Section 2 introduces the institutional background and data. Section 3 details the empirical design. Sections 4 and 5 present the main results. Section 5.3 discusses additional analysis, Section 5.3.3 examines MTP rules in other markets, and Section 6 concludes.

2 Institutional Background and Data

2.1 MTP Rule

To mitigate market manipulation and improve investor confidence, the Singapore Exchange (SGX) introduced an MTP rule for mainboard-listed stocks on March 2, 2015, requiring firms to maintain a six-month volume-weighted average price (VWAP) of at least S\$0.20 (approximately US\$0.15). The MTP rule had a broad impact, as approximately 40% of SGX-listed firms were trading below this threshold at the time of the rule’s introduction (see Figure 2). Firms failing to comply within the grace period were placed on a watchlist, faced heightened delisting risk, and became ineligible for the Central Provident Fund, Singapore’s defined contribution savings plan, and a significant source of retail capital.⁸

The primary way to satisfy the MTP was a reverse split, and SGX waived all administrative and legal fees to facilitate compliance. Compliance was mandatory, but firms retained discretion over when to implement within a multi-year grace period, and this timing variation is central to our identification. Firms typically announce a split several weeks ahead of the effective date, stating the ratio and rationale, with one to two months between the two dates.⁹

We collect data on all reverse-split announcement dates and effective dates for all mainboard reverse splits filed between March 2, 2015, and March 2, 2018: 126 firms across 95 distinct event dates. We define event month 0 as the month in which the reverse split becomes effective and the nominal price jumps. We use the announcement date as the event date separately in Section 5.3.2, when we test for informed trading and the timing of when information becomes public matters. For firms with multiple announcements, we use the earliest public disclosure of the compliance process as the announcement date.

The price adjustments were large. The median split ratio was 6:1, with an interquartile range of 4:1 to 10:1, and this cross-sectional variation determines how far above the threshold each firm lands, a variation in the size of the mechanical price jump that we exploit in Section 4. Compliance events were spread across 95 distinct dates, with over 90% occurring between March 2015 and March 2016. Figure 3 illustrates the staggered rollout, with events clustering in the days preceding each of SGX’s quarterly compliance reviews (held on the first trading day of March, June,

⁸The initial one-year grace period was subsequently extended. In 2016, the MTP requirement was subsequently expanded to require a six-month average market capitalization above S\$40m. Non-common stocks, such as exchange-traded funds (ETFs), investment trust funds, and global depository receipts, were exempted from the MTP requirement. While the MTP regulation was eventually removed in June 2020 following enhancements to SGX’s market surveillance system, it remained in full effect during our sample period.

⁹For example, Yongnam Holdings Limited (Ticker AXB.SI) was trading at S\$0.13 on March 13, 2015, when it announced that it would consolidate 4 existing shares into one new share, which translates to a price of S\$0.52 per share after the reverse split. Upon approval by SGX and shareholders, the effective date of the reverse split was May 20, 2015.

September, and December). This pattern is consistent with firms timing compliance to regulatory deadlines rather than to firm-specific news.¹⁰ Hence, the MTP rule creates a quasi-experimental setting, enabling us to identify the causal effect of nominal price changes on retail trading.

2.2 Data Description

Our main dataset is a proprietary equity trading dataset provided by a leading financial institution in Singapore with a large retail client base, offering a representative sample of retail investor activity. The dataset covers the period from 2014 to 2019 and includes daily transaction records (security identifier, transaction type, price, and quantity) linked to investor demographic profiles (gender, age, and income). We focus on common stocks listed on the SGX, converting foreign-currency transactions to Singapore dollars at daily Bloomberg spot rates, and aggregate to monthly frequency. We merge this retail trading data with daily SGX data, which includes stock price, trading volume, market capitalization, dividends, bid-ask spread, and firm-level financial variables. Information on reverse stock splits, including split ratios, announcement dates, and effective dates, is extracted from corporate announcements available on the SGX website.

To assess the representativeness of our retail trading dataset, we examine its coverage of total SGX trading activity. For each stock, we compute a monthly ratio of the total dollar trading volume in our sample relative to the stock's exchange-wide dollar volume. As shown in Figure 4, coverage is highest among low-priced stocks (below S\$0.20), where our sample captures 15% to 25% of total market volume.¹¹ Coverage remains meaningful even for higher-priced stocks (above S\$1), at 3% to 7% of exchange-wide volume. These figures are consistent with retail investors being disproportionately active in low-priced stocks (Birru and Wang, 2016; Kumar and Lee, 2006) and suggest that our data provide a representative sample of retail trading in the market segment most affected by the MTP rule.

We identify 374 firms with volume-weighted prices below S\$0.20 between April 2014 and March 2015, the year before the MTP rule took effect. Of these, 126 undertook a reverse split (treated) and 248 did not (never-treated).¹² Our analysis (Section 3) exploits the staggered timing of reverse splits among the 126 treated firms, with later-treated firms serving as controls for earlier-treated ones. The 248 never-treated firms supply matched control samples used as robustness checks in Section 5.

¹⁰We exclude voluntary reverse splits that occurred prior to March 2015, as these were not mandated by the MTP rule.

¹¹Even after accounting for potential double-counting of buy and sell legs within the broker, our sample captures over 10% of total market volume in this segment.

¹²A firm enters the treated sample if it ultimately undertook an MTP-compliance reverse split, regardless of whether its volume-weighted price was strictly below S\$0.20 throughout the pre-MTP window. This rule includes 18 firms whose pre-MTP volume-weighted prices were slightly above S\$0.20 but subsequently fell below the threshold and triggered compliance.

Table 1 compares treated firms’ characteristics before and after the reverse split. The mechanical increase in the nominal share price is large: the average price more than triples, from S\$0.14 pre-event to S\$0.50 post-event. Notably, the average post-split price remains below the S\$0.84 level implied by the median split ratio of 6:1, consistent with partial adjustment and post-event price drift. Average market capitalization declines from S\$113 million to S\$92 million in the year after the reverse splits, though this change is not statistically significant. Liquidity deteriorates sharply: total trading volume on SGX declines by over 60%, while retail trading volume falls by approximately 70%, with all illiquidity proxies (zero-trading days, bid-ask spreads, and turnover) corroborating. The univariate evidence indicates post-split changes in investor composition rather than changes in firm fundamentals.

Treatment timing does not appear selective. Table A.2 shows early- and later-treated firms are statistically indistinguishable across a wide range of observable characteristics, including price, fundamentals (e.g., ROE, leverage, distress, dividends), past performance, volatility, liquidity, and retail trading. Specifically, we find no differences in *fundamentals*, which directly addresses the concern that firms with stronger (or weaker) prospects self-selected into earlier or later compliance treatment. We also find no differences in *past return performance*: 3-month and 12-month pre-event returns are similar, so later-treated firms were not simply on a different price trajectory. The only exception is market capitalization, where later-treated firms are slightly larger (S\$78 vs. S\$141 million, $t \approx 1.7$). The broad absence of pre-treatment differences supports the quasi-random timing assumption for our identification strategy.

3 Empirical Strategy

Our primary goal is to estimate the causal effect of nominal stock price on investor trading behavior. Previous studies link nominal price changes to stock trading using manager-chosen stock splits (Baker, Greenwood, and Wurgler, 2009; Birru and Wang, 2016; Green and Hwang, 2009), which confound the price effect with managerial information and intent. We use the SGX MTP rule as a quasi-exogenous shock that sidesteps this problem: the exogenous cross-sectional and staggered compliance supports a staggered DiD design, with evidence that early and late compliers are observationally similar pre-treatment.

3.1 Investor-Level Analysis

We begin our analysis at the individual investor level to isolate the behavioral response to the mechanical price jump. Our identification comes from comparing the same investor’s trading in the same stock before and after its reverse split. By absorbing investor-by-stock fixed effects, the design nets out any baseline preference investor i has for stock j , so the estimated coefficients capture the change in trading attributable to the price jump. To estimate the average treatment effect on individual

trading behavior, we focus on all treated stocks that undergo reverse splits and estimate the following investor-stock specification within the event window spanning 15 months prior to and 24 months following the reverse split month:

$$Y_{i,j,t} = \beta_1 \times D_{j,(-5m,-1m)} + \beta_2 \times D_{j,(0m,12m)} + \beta_3 \times D_{j,(13m,24m)} + \gamma_{i,j} + \sigma_t + \varepsilon_{i,j,t} \quad (1)$$

where $Y_{i,j,t}$ measures the trading activity (e.g., trading volume and trading frequency) of individual i in stock j in month t . The individual-by-stock fixed effects $\gamma_{i,j}$ control for all time-invariant individual preferences for specific stocks and persistent characteristics of those stocks. Year-month fixed effects σ_t account for common macroeconomic and market-wide shocks that could influence trading activity across all investors and stocks at a given time. The omitted benchmark period spans 15 to 6 months prior to the reverse split month. The independent variables $D_{j,(-5m,-1m)}$, $D_{j,(0m,12m)}$ and $D_{j,(13m,24m)}$ are indicator variables equal to 1 if month t falls within five months before, months 0 to 12 following, or months 13 to 24 following a reverse split event for stock j , respectively.¹³ The standard errors are double-clustered at the individual and year-month levels to allow for cross-sectional and temporal correlation.

To visualize monthly dynamics of individual investor response and to diagnose pre-trends, we estimate:

$$Y_{i,j,t} = \sum_{k=-5}^{24} \beta_k \times D_{j,t}^k + \gamma_{i,j} + \sigma_t + \varepsilon_{i,j,t} \quad (2)$$

where $D_{j,t}^k$ is a set of monthly indicators equal to 1 if the month t is k months relative to the reverse split of stock j for individual i , with $k = 0$ denoting the month of the reverse split. The coefficients β_k map the dynamic investor response from 5 months before to 24 months after the event. The pre-event coefficients (β_k for $k < 0$) serve as a diagnostic test for pre-trends.

3.2 Stock-Level Analysis

We next aggregate to the stock level to examine how individual micro choices scale into broader market outcomes. We restrict our estimation sample to the set of treated firms, utilizing the staggered timing of the reverse split to employ not-yet-treated firms as counterfactuals for already-treated firms.¹⁴ We aggregate individual trading

¹³Throughout our empirical specifications, event month 0 is the month in which the reverse split becomes effective, that is, the month the nominal price mechanically jumps. The only exception is Section 5.3.2, which tests for informed trading ahead of the reverse split and therefore anchors on the announcement date.

¹⁴The validity of using late-treated firms as a control group requires that treatment timing is uncorrelated with firm characteristics. As shown in Panel A of Table A.2, early- and late-treated firms

activity to the stock level and estimate the following DiD specification for stock j in month t :

$$Y_{j,t} = \beta_1 \times D_{j,(-5m,-1m)} + \beta_2 \times D_{j,(0m,12m)} + \beta_3 \times D_{j,(13m,24m)} + \delta_j + \sigma_t + \epsilon_{j,t} \quad (3)$$

where $Y_{j,t}$ denotes the aggregated stock-level outcome variables, such as total retail trading volume, liquidity, and stock return, for stock j in month t . The model includes stock fixed effects δ_j and year-month fixed effects σ_t . The indicator variables are the same as in Equation 1. Standard errors are double-clustered at the firm and year-month levels to account for potential correlation within firms and over time.

To further explore the monthly dynamics of response and validate the parallel trends assumption, we also estimate the following event-study specification,

$$Y_{j,t} = \sum_{k=-5}^{24} \beta_k \times D_{j,t}^k + \delta_j + \sigma_t + \epsilon_{j,t} \quad (4)$$

where the variable $D_{j,t}^k$ is the same as in Equation 2. The coefficients β_k represent the dynamic response of stock-level outcomes in reaction to the change in nominal price around the reverse split.

4 Nominal Prices and Retail Trading

In this section, we present our main findings on within-investor trading behavior around MTP rule-induced reverse stock splits. We estimate the effect of nominal price increases on retail trading for the same investor in the same stock, trace reallocations into other low-priced stocks, assess the welfare consequences using within-investor trading portfolio performance, and examine heterogeneity by investor demographics.

4.1 Individual Trading Summary Statistics

We start with a descriptive view of the individual trading activities. Table 2 presents the summary statistics of the trading activities of 37,730 individual accounts that ever traded a stock that underwent a reverse split (treated stocks) during the sample period. We measure their trading activities using the monthly dollar trading volume and number of trades, including both buy and sell trades. Trading volume is higher among male traders and higher-income investors. The average investor makes 1.72 trades with a total trading volume of S\$24,637 per month. Investors with Contracts

are comparable on observable characteristics, supporting this assumption. Separately, because two-way fixed-effects estimators can produce biased average treatment effects under staggered adoption with heterogeneous treatment effects, we complement the baseline TWFE specification with the heterogeneity-robust estimators that account for cohort-specific average treatment effects (ATTs) (Callaway and Sant’Anna, 2021) and group-time ATTs (Sun and Abraham, 2021).

for Difference (CFDs) accounts, which are margin trading accounts, exhibit nearly double the total average trading volume (S\$37,529 vs. S\$19,791) and frequency (2.50 vs. 1.43 trades), compared to non-CFD traders.¹⁵

4.2 Effect of Reverse Splits on Individual Trading

Table 3 reports the average effect on individual-stock dollar volume and trade counts. Columns (1)-(3) report the estimates for dollar trading volume for buy trades, sell trades, and the total buy and sell volume, respectively. The post-event estimates reveal a substantial and persistent decline in retail trading following a reverse split. On average, individual investors reduce monthly trading volume in the treated stock by S\$563 (see column (3)) over the following 12 months, 77% of their pre-event average volume. The effect persists in months 13-24. Columns (1) and (2) show comparable declines for both buy and sell trades, indicating a symmetric reduction in trading activities rather than exits. To address right skewness in the data, we re-estimate the model using log-transformed volume (Columns (4)-(6)) and find that the economic magnitudes remain robust, with declines ranging from 21%-30%. The log specification implies that the typical investor reduces total trading volume by approximately 30% ($e^{-0.36} - 1$), while the levels specification yields a 77% decline because it weights heavy traders more. We also observe a similar decline in trading frequency (the number of trades) in columns (7) - (9), with magnitudes ranging from 67%-75%.

To visualize our event study, we estimate Equation 2 and plot the coefficients in Figure 5. The event-time coefficients are flat pre-split, drop discretely at month 0, and remain depressed for months. This pattern indicates that the nominal-price jump durably curbs retail activity for the same investor in the same stock.

If retail investors are highly sensitive to price levels, we should observe a more pronounced trading activity reduction from stocks that undergo more aggressive reverse splits. Table A.3 presents heterogeneous responses of individual trading activity based on the split conversion rate. The results indicate that the drop in retail trading increases with reverse-split conversion rate across all measures of trading activity.

Finally, we also conduct a placebo test by randomly assigning pseudo-event dates within the sample period to treated stocks. Table A.4 shows no comparable effects, reinforcing that the trading declines are tied to the quasi-exogenous timing of the nominal price changes rather than generic time patterns.

¹⁵The Contracts for Difference (CFDs) are derivative contracts employed to speculate on the price movement of underlying assets such as equities, indices, commodities, and currencies. CFD trading is similar to margin trading, and investors are required to post a margin, typically around 10% of the underlying asset value.

4.3 Do Investors Migrate to Other Low-priced Stocks?

We next examine whether investors' preference for low nominal prices is behaviorally salient. Specifically, when a stock an investor holds is pushed out of the "cheap" tier by a reverse split, does the investor re-express their preference elsewhere by shifting activity toward other low-priced stocks that did not undergo a reverse split?

We implement a composition-focused event study that tracks how investors reallocate across price tiers before and after the first reverse split in their portfolio. The outcome variable $Y_{i,t}$ is the ratio of an individual's trading volume in untreated low-priced stocks (price < S\$0.20) relative to their total trading volume (excluding the treated stock). By excluding the treated stock from the total trading volume in the denominator, the ratio captures substitution into other low-priced stocks. We estimate:

$$Y_{i,t} = \beta_1 \times D_{i,(-5m,-1m)} + \beta_2 \times D_{i,(0m,12m)} + \beta_3 \times D_{i,(13m,24m)} + \gamma_i + \sigma_t + \epsilon_{i,t} \quad (5)$$

which follows Equation 1 with two changes. Event time is now measured from the first reverse split in investor i 's portfolio rather than from each stock's own split, and the fixed effects are investor-level (γ_i) rather than investor-by-stock, since the outcome is a trading share. We focus on β_2 and β_3 , which capture the change in the low-price share after the event. The sample is restricted to accounts with strictly positive holdings in treated stocks prior to the reverse-split date.¹⁶

The composition-based design isolates the preference itself rather than changes in overall trading intensity: an investor who simply trades less after the split leaves the ratio unchanged. A rise in the low-price share therefore indicates that displaced investors substitute toward the stocks that remain below the threshold, while a flat share would indicate that nominal price does not organize their allocation.

Table 4 reports the results. The coefficients for the months before the first reverse-split event in each investor's portfolio are not significantly different from zero, confirming the absence of pre-trends in the trading of untreated low-priced stocks. After the first reverse split in an investor's portfolio, the share of trading allocated to untreated low-priced stocks rises by 13%, relative to the pre-event share of trading volume. This magnitude increases to 16% in months 13 to 24. Estimates based on the number of trades produce similar magnitudes (Column 2). This economically meaningful rise in composition, measured relative to the investor's own baseline mix, provides direct evidence of stable nominal-price preference: when one "cheap" stock is pushed out of the tier, investors actively substitute into other low-priced stocks.¹⁷

¹⁶An account's holding position in a treated stock prior to the reverse-split date is computed as cumulative buy volume minus cumulative sell volume from account inception through three trading days before the reverse split.

¹⁷To examine whether this migration is also shaped by industry affiliation, Appendix Table A.5 decomposes post-split migration into untreated low-priced stocks in the same sector as the treated stock and those in different sectors. Although first-year same-sector migration is positive and marginally

4.4 Individual Trading Performance in Low-priced Stocks

We examine whether the retail preference for low-priced stocks is motivated by information. If investors target low-price stocks because they possess superior local knowledge or private information (Boehmer et al., 2021; Ivković and Weisbenner, 2005), their trades should on average earn positive subsequent returns. If instead the tilt is driven by price salience or lottery taste, the demand should hurt performance. We compare the relative returns on net-buy portfolios for low-priced (below S\$0.20, $P = L$), mid-priced (above S\$0.20 and below S\$1, $P = M$), and high-priced (above S\$1, $P = H$) stocks, restricting attention to investors who traded both low- and high-priced stocks during the sample period. We denote the net-buy portfolio return over k months as $R_{i,t,k,P}^{BmS}$, representing the difference between the returns on securities bought ($R_{i,t,k,P}^B$) and sold ($R_{i,t,k,P}^S$) by individual i in month t , where $k \in [1, 12]$ denotes the k^{th} month following the trade, and P denotes price levels. The returns for the buy ($R_{i,t,k,P}^B$) and sell ($R_{i,t,k,P}^S$) portfolios are the dollar-volume-weighted average of k -month-ahead returns for securities traded in month t :

$$R_{i,t,k,P} = \sum_{j,j \in P} \omega_{i,j,t} \times r_{j,t+k}, k \in [1, 12] \quad (6)$$

where $\omega_{i,j,t}$ is the dollar trading volume weight of security j bought (or sold) in month t by investor i . Figure 8 shows no evidence of informed trading: net-buy returns are significantly below zero at every horizon in low- and high-priced portfolios, and the underperformance is more pronounced in the low-priced tier, suggesting that the preference for low-priced stocks is not motivated by informational advantages. These findings are robust to risk-adjusted portfolio returns derived from either the CAPM or the Fama-French three-factor model, shown in Figure A.2 in Appendix A.

Next, we ask whether this underperformance worsens after the investor is nudged to re-express the low-price preference. We compare the returns on stocks an investor buys to the returns on stocks the same investor sells within a given price tier over a window from 24 months before to 12 months after each investor's first reverse split and estimate:

$$R_{i,t,k,P}^{BmS} = \alpha + \beta_0 Post_{i,t} + \beta_1 (Mid_P \times Post_{i,t}) + \beta_2 (Low_P \times Post_{i,t}) + \gamma_{i,P} + \delta_{t,P} + \epsilon_{i,t} \quad (7)$$

where $R_{i,t,k,P}^{BmS}$ is the k -month cumulative return of the net-buy portfolio for individual i in month t at price level $P \in \{Low, Mid, High\}$. $Post_{i,t}$ equals one after the investor's first reverse split. The high-priced stock portfolio serves as the reference group, so β_1 and β_2 capture the incremental performance for mid- and low-priced stock portfolios, respectively, relative to the high-priced stock portfolio. We include price-level by

significant, there is weak evidence that migration is affected by same-industry stocks. We therefore interpret the migration result as primarily about nominal-price preference.

individual fixed effects ($\gamma_{i,p}$) to control for time-invariant investor skills or preferences, and price-level by year-month fixed effects ($\delta_{t,p}$) to account for time-varying price-tier-specific market shocks. These granular fixed effects allow us to isolate the effect of the reverse split and the subsequent migration behavior on individual trading performance across portfolios of varying price levels.

Table 5 shows that individual investors underperform in their low-price investments after the reverse split. The *Low Price* $\times$ *Post* interaction is negative and significant at each horizon, an incremental loss of about 2.8% per annum. This evidence implies that investors' performance in a low-priced stock portfolio deteriorates significantly relative to their high-priced stock portfolio. These findings are robust to risk adjustment using either the CAPM or the Fama-French three-factor model, with the low-priced portfolio underperforming the high-priced portfolio by 2.4%–4.2% per annum in the post period. The result is not driven by trading in the treated stocks themselves in the post-period. Excluding all 126 MTP-treated stocks leaves the *Low Price* $\times$ *Post* coefficient negative and significant at -2.1% to -2.5% . That low-price performance deteriorates precisely when investors migrate into substitute sub- $\$0.20$ stocks is hard to reconcile with private information, and points to welfare-reducing price salience preferences. We find no offsetting improvement in high-price portfolio performance, so the reallocation does not merely reflect skill redeployment.

4.5 Speculative Demand or Capital Constraints

Having established that retail investors significantly reduce trading activity following a price jump, we now ask why the nominal price matters. The hypothesis of lottery-like preferences or price salience predicts larger effects for investors who, *ex ante*, are more prone to speculative demand.¹⁸ Meanwhile, the capital constraint channel would predict the retreat should concentrate among investors with tighter capital constraints.¹⁹ The two channels differ in who should respond, and in how trading should react when the constraint itself is relaxed. We test both channels.

Figure 7 shows participation in sub- $\$0.20$ stocks across demographics. Low-price participation is higher among male and high-activity investors, and is also elevated among investors with CFD accounts, a proxy for risk tolerance, while in-

¹⁸Prior research explores psychological mechanisms behind retail investors' trading preference based on nominal price levels. Kumar (2009) finds that retail investors seeking "cheap bets" on lottery-like stocks are more attracted to low-priced stocks. Birru and Wang (2016) suggest that investors overestimate the growth potential of low-priced stocks. Shue and Townsend (2021) show that investors think in absolute price changes rather than returns, so nominal price levels distort perceived movements. Relatedly, Roger, Roger, and Schatt (2018) show that financial analysts process small numbers and large numbers differently, issuing disproportionately more optimistic target prices for low-priced stocks.

¹⁹Recently, Du et al. (2025) show that round-lot rules price small investors out of high-priced stocks in China, Da, Fang, and Lin (2025) show that fractional trading draws them back into expensive U.S. stocks, and Li and Ye (2022) model price levels as a firm-side trade-off between tick-size and lot-size frictions, with no appeal to investor preferences.

come gradients are weak. We then interact the treatment indicators from Equation 1 with these characteristics. Trading activeness is measured by the percentage of months individuals trade over their entire trading history, split into terciles: “High Activeness”, “Median Activeness”, and “Low Activeness”. We also examine variations across investors’ age, accounting for potential non-linearities in investment decisions. Finally, risk tolerance is proxied by the ownership of a CFD (Contracts for Difference) account, a margin account used for leveraged derivative instruments. Table 2 presents the distribution of these variables.

The responses differ sharply across investor types. As shown in Column (1) of Table 6, male investors reduce their trading volume by approximately S\$115 more than female investors after a reverse split. In Column (2), we find that more active investors exhibit stronger reactions: medium-active investors decrease their trading volume by about S\$277 more than low-active investors, while highly active investors show an even larger reduction of S\$507. Finally, Columns (3) and (4) show that younger investors and those who have a CFD account reduce trading disproportionately.

This profile of male, younger, highly active, and CFD users matches the ex ante footprint of speculative demand rather than affordability.²⁰ The investor types who are more active in low-price stocks (see Table 2) are precisely those who reduce their trading most after the price is mechanically lifted out of the “cheap” range.

We examine the alternative explanation of capital constraints with the SGX round lot-size reduction from 1,000 to 100 shares in January 2015 as a falsification test. This exchange-level rule change sharply reduced the minimum capital required to establish a position. If affordability were first-order, lower entry costs should shift demand toward higher-priced stocks. We test this with a DiD specification that interacts a *High Price* indicator (price > S\$1) with a *Post* indicator for months after January 2015. Under the affordability hypothesis, the interaction coefficient should be positive and significant, indicating a shift in retail trading toward higher-priced stocks once the lot-size reduction made them more accessible. Table 7 shows that the *High Price* × *Post* coefficient is statistically indistinguishable from zero across all three trading proxies. A 100-share lot at the S\$0.20 floor implies a minimum outlay of just S\$20, far too small to bind for a typical retail investor. Together with the heterogeneity evidence, this points to behavioral preference rather than capital constraints as the key driver.

5 Implications for Market Outcomes

Our evidence shows that a jump in nominal price triggers a large, within-investor retreat and a reallocation toward other low-priced stocks. In this section, we examine

²⁰These results align with existing literature documenting that male and younger investors tend to exhibit stronger preferences for gambling-like investments (Barber and Odean, 2001; Dohmen et al., 2011; Dorn and Sengmueller, 2009; Dwyer, Gilkeson, and List, 2002; Grinblatt and Keloharju, 2009; Korniotis and Kumar, 2011; Kumar, 2009).

how these micro choices aggregate to the stock level. In doing so, we provide micro-foundations for market outcomes that the literature typically studies in the cross-section. We first show that losing the low nominal price retail clientele has a negative impact on treated stocks along several dimensions: trading activity collapses, liquidity dries up, price discovery slows, and valuations fall. We then trace the same demand shift into return dynamics, where short-term reversals weaken among low-priced stocks and returns co-move less with low-priced peers and more with high-priced ones. These patterns corroborate the clientele mechanism behind the market-quality results.

5.1 Effect on Market Quality

5.1.1 Stock Trading Activity

The within-investor decline documented in Section 4 need not aggregate into a stock-level decline: new retail entrants or institutional traders could in principle replace the trading volume lost. We test this directly by aggregating volume to the stock level.

An identification challenge in our stock-level staggered DiD analysis is that the not-yet-treated control stocks are themselves low-priced. When retail investors opt out of a treated stock following the reverse split, they may reallocate their trading toward those low-priced control stocks, inflating control-group trading volume and overestimating the true treatment effect, a violation of the Stable Unit Treatment Value Assumption (SUTVA). To assess the magnitude of overestimation, we exploit variation in how different price-tier control groups are exposed to such spillover. Substitution occurs among stocks in the same low-priced tier as the treated stocks, making low-priced controls the most contaminated counterfactual, while high-priced controls are a near-clean benchmark. If contamination is present, the estimate should decrease as the control price tier rises. If the estimates are similar across price tiers, SUTVA is not a concern. Comparing the staggered DiD estimates with the matched low- and high-priced control estimates therefore tells us whether migration into the not-yet-treated group meaningfully biases our design.

We match each treated stock using the coarsened exact matching (CEM) algorithm within two price tiers, low-priced (below S\$0.20, never treated) and high-priced (above S\$0.50), on past 3-month returns, average turnover, and zero-trade days measured over the year before the MTP, assigning each control the matched firm's split date as a pseudo-event date.²¹ Table 8 reports estimates for log-transformed aggregated dollar trading volume across all three control groups. Columns (1)–(3) use low-priced controls, columns (4)–(6) use high-priced controls, and columns (7)–(9) use the not-yet-treated sample. In all cases, the pre-period interaction ($Treated \times$

²¹The matching of treated firms with control firms is via the coarsened exact matching (CEM) algorithm. It uses monotonic imbalance bounding matching, where the balance between the treated and control groups is chosen so that adjusting the imbalance on one variable does not affect the maximum imbalance of any other.

$M[-5, -1]$) is statistically insignificant, supporting the parallel pre-trends assumption. Post-treatment effects are negative and significant in all matched samples. The first-year decline in volume is 26% ($e^{-0.30} - 1$) against low-priced controls versus 17% ($e^{-0.19} - 1$) against high-priced ones. The difference of 0.11 log-points implies that using low-priced controls inflates the estimate by approximately 10% ($e^{-0.11} - 1$), quantifying the SUTVA contamination. Crucially, the drop in volume survives against high-priced controls, where we do not expect any spillover, and the not-yet-treated estimate of -0.18 (column 9) matches closely, confirming the reliability of our staggered design. Table A.6 further supports our findings using two alternative measures of trading activities. The not-yet-treated estimate implies a decline of S\$0.82 million in volume and 0.07 thousand trades per month in the first post-split year (columns 3 & 6), equivalent to 78% and 88% of the respective pre-split means.

To capture the dynamic effects of reverse splits, we estimate Equation 4, which tracks the aggregated monthly evolution of retail trading activity from five months before to twenty-four months after the event. As shown in Figure 6, following the reverse split, retail dollar trading volume declines by 60% to 90% across post-event months and shows no recovery subsequently. This pattern holds across buy trades (Figure 6a), sell trades (Figure 6b), and total trades (Figure 6c). The validity of the parallel trends assumption is supported.

We address the concern that two-way fixed-effects estimators can be biased under staggered treatment with heterogeneous effects by re-estimating the baseline using heterogeneity-robust DiD estimators. Following Sun and Abraham (2021), we form cohort-specific comparisons using the last-treated firm as control and take all months up to month -5 as the reference period. For the Callaway and Sant’Anna (2021) estimator, we vary the reference period and apply the inverse probability weighting advocated in their study. As shown in Figure A.1, these robust methods not only confirm our main results but yield even more pronounced treatment effects, strengthening the evidence of a large, negative impact of reverse splits on retail trading.

5.1.2 Stock Liquidity

A collapse in order flow need not degrade market quality if the departing trades were a mix of informed and uninformed, and market makers adjust accordingly. Next, we examine whether the documented retreat translates into worse stock liquidity. We employ three proxies for monthly stock illiquidity. Our first measure is the percentage of days in a month with zero trading volume (zero-trade percentage), which is informative of infrequently traded stocks (Lesmond, 2005). The second measure is turnover, which is constructed as the stock’s monthly dollar volume divided by its market capitalization. The third measure is the bid-ask spread. Finally, we also construct a composite illiquidity index by averaging the standardized ranks of zero-trade percentage, negative turnover, and bid-ask spread, so that higher values

indicate worse liquidity. Detailed construction of these variables is provided in Appendix C.

We estimate the event study specification following Equation 3 using each illiquidity measure as the outcome variable. As shown in Table 9, we find that stocks experience a significant deterioration in stock liquidity following a reverse split, as evidenced by a significant decrease in turnover and significant increases in zero-trade days and bid-ask spreads. In standardized units relative to the pre-event distribution, turnover falls by 0.33 standard deviations, the zero-trade percentage rises by 0.39 standard deviations, the bid-ask spreads widen by 0.47 standard deviations, and the composite illiquidity index increases by 0.50 standard deviations in the first year following the reverse splits.²² The dynamic plot, shown in Figure 9, confirms the absence of pre-trends and an immediate step up in illiquidity at event time that remains persistent over the subsequent 24 months.

Overall, these results align with our micro evidence: when a stable retail clientele reduces trading in response to a price jump and is not fully replaced, trading becomes patchier, effective trading costs rise, and activity scales down. In this segment of the market, reverse splits dampen retail participation and make trading conditions persistently worse. The contrast with Han (1995), where manager-initiated reverse splits improved liquidity, follows directly from the difference in selection: managerial splits are timed when post-split conditions are favorable, while regulation-induced splits are not.

5.1.3 Price Efficiency

If the liquidity drain documented reflects a thinner, less attentive investor base, treated stocks may become less efficient in incorporating information. Following Hou and Moskowitz (2005), we examine the speed at which stock prices incorporate market-wide information and construct three rolling measures of price delay from weekly returns over a 26-week window. $D1$ is the fraction of return variation explained by lagged market returns. $D2$ is the weighted average of the absolute lagged coefficients, capturing the magnitude of the delayed response; and $D3$ precision-weights those coefficients by their standard errors to adjust for estimation noise. Higher values indicate slower incorporation of market-wide information. Appendix C gives the exact definitions.

To test the effect of the price jump induced by the MTP policy on stock price efficiency, we estimate the following event-study specification:

$$Delay_{j,t} = \alpha + \beta_1 W_j[-20, -1] + \beta_2 W_j[26, 52] + \beta_3 W_j[53, 104] + \mu_j + \lambda_t + \epsilon_{j,t} \quad (8)$$

²²The pre-event standard deviations for turnover, percentage of zero-trade days, and bid-ask spreads are 0.09, 0.25, and 0.09, respectively, as shown in Table 1. For composite illiquidity, the standard deviation is 0.78.

Table 10 presents the regression of these delay measures around the event window. To prevent mechanical transition noise inherent in the 26-week rolling calculation, we exclude weeks 0 to 25 following the reverse split.²³ The coefficients for the pre-period 20 weeks ($W_j[-20, -1]$) are small and statistically insignificant across all three specifications, confirming the absence of pre-existing trends in price delay. However, in the post-period ($W_j[26, 52]$), we observe a sharp and statistically significant increase in price delay, that is, slower adjustment to market-wide information: D1 increases by 0.154, while D2 and D3 rise by 0.314 and 0.313, respectively, representing approximately an 18% to 24% increase relative to the benchmark period. This price delay persists into the second year ($W_j[53, 104]$), remaining economically meaningful and statistically significant. These results indicate that a stock’s price becomes more sluggish in response to market news following the price jump, consistent with the interpretation that the reduction in retail trading activity not only drains market liquidity but also impairs price discovery.

5.1.4 Changes in Valuation

To examine whether the clientele shift around reverse splits shows up in firm valuations, we estimate Equation 4 with monthly stock returns as the outcome. Figure 10 shows a sharp negative return effect concentrated in the split month: -5.45% ($t = -2.51$) for raw returns, -5.65% ($t = -2.79$) for market-adjusted returns, and -5.70% ($t = -2.76$) for small-cap-adjusted returns. There is no evidence of subsequent drift or rebound, ruling out under- or overreaction. We argue that the one-time decline reflects the market pricing the departure of trading by retail investors. Collectively, with low-price retail trading gone and not fully replaced, increased trading costs, and slower price discovery, the price settles at a lower equilibrium price that does not revert.²⁴

5.2 Shifts in Retail Trading Demand

We now ask whether the departure is visible in the distribution of returns themselves. If nominal price organizes who trades a stock, then removing a stock from the low-price tier should change both the transitory price pressure its order flow generates and the set of stocks with which it co-moves. The two tests that follow examine each.

²³Observations dated in weeks 0–25 after the split would be estimated from a mix of pre- and post-split returns. Week 26 is the first observation computed entirely from post-split data.

²⁴We use the reverse split effective month as event time 0, the month in which the nominal price mechanically jumps and the retail clientele is displaced. Demand driven by the salience of the low nominal price label can support trading at a price above fundamental value, and the reverse split removes the label and the demand with it (Cosemans and Frehen, 2021). These channels share a common cause, the clientele’s departure, and our setting does not separate them. The paper’s central claim is independent of this decomposition, since the liquidity and price-discovery losses obtain under either reading.

5.2.1 Short-Term Reversals

Short-term return reversals arise when correlated marketable orders generate transitory price pressure that subsequently mean-reverts. When a clientele trades together, their demand for immediacy moves prices away from fundamentals and creates predictable reversals as imbalances unwind (e.g., Karolyi, Lee, and Van Dijk, 2012; Koch, Ruenzi, and Starks, 2016). Our earlier findings establish three consequences of reverse splits: a sharp reduction in trading among retail investors, deteriorated liquidity, and slower price discovery. These shifts predict an attenuation of short-term reversals through the demand-side channel, where retail demand for immediacy that pushes prices away from fundamentals is reduced. Wider spreads and thinner liquidity further discourage the immediacy-demanding trading that generates transitory pressure. Furthermore, as prices become less efficient in incorporating information, the mean-reversion of returns should also shrink.

We first establish that monthly reversals are concentrated in low-priced, retail-heavy stocks. In Panel A of Table 11, a one-percentage-point increase in month- t returns predicts a decrease of 15 bps in month- $t+1$ returns for low-priced stocks, while high-price stocks show no significant reversal. Column (4) confirms this pattern in the full sample: the interaction $Ret \times Low\ Price$ is -0.134^{***} , indicating that reversals are significantly stronger among low-priced stocks. This concentration of reversals in the low-priced, retail-heavy segment is consistent with the demand-side view, where retail activity is heavier and short-run price pressure more pronounced.

We then exploit the mechanical drop in retail trading induced by reverse splits. For stock i in month t , we run the following regressions:

$$r_{i,t+1} = \alpha + \beta_1 r_{i,t} + \beta_2 Post_{i,t} + \beta_3 r_{i,t} \times Post_{i,t} + \varepsilon_{i,t} \quad (9)$$

where $r_{i,t+1}$ refers to stock returns for each firm in month $t+1$, and $Post_{i,t}$ equals one for months at or after the effective split month. As shown in Table 11 Panel B, $\beta_3 = 0.084$ (Column 2, significant at 10%) indicates post-split attenuation, reducing the pre-split reversal by more than half. This attenuation effect is consistent with the demand-side prediction: when retail investors reduce trading in response to the price jump, there is less correlated marketable order flow to generate transitory pressure, and hence smaller reversals.

To connect the attenuation directly to retail activity, we add retail trading intensity (RTI), defined as retail buy plus sell volume scaled by market capitalization, standardized to a mean of zero and a unit standard deviation. We interact RTI with Ret and $Post$. The triple interaction $Ret \times Post \times RTI$ is 0.048 (Column 3, significant at 5%). This cross-sectional pattern is not a mechanical artifact of extreme reversals reverting toward the average: such an artifact would require high- RTI stocks to exhibit the strongest reversals before the split, that is, a negative coefficient on

$Ret \times RTI$, whereas the estimated coefficient is positive (0.02).²⁵ The finding reinforces the demand-side view: reverse splits remove a key source of marketable orders, lowering demand for immediacy, so there is less transitory pressure to unwind.²⁶

A natural concern is that the attenuation of reversals is mechanical. Treated stocks trade less frequently after the split, so a larger share of months record zero returns, and a stock that does not trade cannot reverse. This rise in stale, non-trading prices would by itself dampen measured reversals—without any reduction in retail demand for immediacy. Column (4) addresses this concern by augmenting the specification with the change in the proportion of zero-trading days ($\Delta Zero\text{-}Trade$) and its full set of interactions with Ret and $Post$. The triple interaction $Ret \times Post \times RTI$ remains positive and significant (0.046, at the 5% level). The decline in reversals therefore reflects reduced retail participation and demand for immediacy, rather than stale prices in thinly traded stocks.

5.2.2 Return Co-movement

A natural implication of a price-tier clientele is a habitat in returns: stocks that share the same nominal tier load on common order-flow and attention shocks, and thus co-move more with one another.²⁷ Our setting allows us to focus on low-priced shares, where retail investors participate extensively and which institutional investors tend to avoid.

We use the MTP shock to test whether a stock lifted out of the low-price tier co-moves less with low-priced peers and more with higher-priced ones. To identify this shift, we implement a DiD design with matched controls using the coarsened exact matching (CEM) algorithm. We match each reverse-split stock with a control (untreated low-price stock) based on three pre-event (−3 to −1 months) characteristics: volume-weighted price, past return, and illiquidity (percent zero-trade days, PZT). The final sample comprises 98 matched pairs with no significant pre-treatment

²⁵Within the uniformly low-priced treated sample, retail turnover plausibly proxies for trading activity and depth, which dampen the price impact of any given order imbalance; this is consistent with weaker unconditional reversals among high-RTI stocks, while reversal strength across price tiers (Panel A) tracks the concentration of the retail clientele.

²⁶Our findings are inconsistent with the supply-side hypothesis, which would predict that reversals *strengthen* in the post-split period: as retail activity withdraws, the cost of liquidity provision rises, and compensation to remaining suppliers should increase rather than decline (Hameed and Mian, 2015; Nagel, 2012; Pástor and Stambaugh, 2003). Foucault, Sraer, and Thesmar (2011) report the same direction we do: reversals weaken after a French reform that halved speculative retail trading.

²⁷Investors allocate across categories rather than individual securities, so that correlated flows into a category generate common return variation beyond fundamentals (Barberis and Shleifer, 2003; Wahal and Yavuz, 2013). Nominal price is a salient and easily observed category of exactly this kind. Kumar and Lee (2006) find that systematic retail trading explains return co-movement for stocks with high retail concentration, typically small-cap and low-priced stocks, as these stocks are costly to arbitrage. Conversely, Green and Hwang (2009) document that the price-based return co-movement primarily concentrates among large firms in the U.S., attributing it to institutional investors, rather than retail investors.

differences (see Tables A.7 and A.8 in Appendix A). For each firm we estimate betas on low- and high-priced portfolio returns separately before and after the split,

$$r_{i,t} = \alpha_i + \beta_{i,LP} \times R_{LP,t} + \beta_{i,HP} \times R_{HP,t} + \epsilon_{i,t} \quad (10)$$

where $r_{i,t}$ is the return of the firm i in month t , $R_{LP,t}$ and $R_{HP,t}$ are value-weighted returns on stocks priced below S\$0.20 and between S\$0.20 and S\$1.50 respectively, each excluding firm i , and we require at least 12 months of returns.²⁸ For each firm i , $\beta_{i,LP,before}$ represents firm i 's return co-movement with a low-priced stock portfolio before its reverse split, while $\beta_{i,LP,after}$ shows the return co-movement with a low-priced stock portfolio after its reverse split. $\beta_{i,HP,before}$ and $\beta_{i,HP,after}$ are defined analogously.²⁹ We also estimate a lagged version by adding $R_{LP,t-1}$ and $R_{HP,t-1}$, and sum the contemporaneous and lagged coefficients. These betas capture the sensitivity of firm returns to the low- and high-priced stock portfolios both at time t and $t - 1$.

We then test whether treated betas shift after the split:

$$\hat{\beta}_{i,t} = \alpha + \beta_0 Treated_i + \beta_1 Post_{i,t} + \beta_2 \times Treated_i \times Post_{i,t} + \epsilon_{i,t} \quad (11)$$

where $\hat{\beta}_{i,t}$ denotes the estimated beta with respect to either the high-priced or low-priced portfolio, calculated as the sum of the contemporaneous and lagged beta coefficients. $Treated_i$ equals one for reverse-split firms and $Post_{i,t}$ for months at or after the split.

Table 12 shows the predicted re-sorting: the beta on the low-priced portfolio falls by 0.68, and the beta on the high-priced portfolio rises by 1.61 (Columns 1 and 2), with similar magnitudes once lagged adjustment is allowed (Columns 3 and 4). These patterns are consistent with what a nominal price habitat predicts: once the stock jumps from “cheap” to “expensive,” its investor base and correlated order flow decouple from low-priced stocks and re-couple with higher-priced peers, which is causal evidence that nominal price tiers organize correlated trading and, through it, the structure of return co-movement, independent of fundamentals.

5.3 Additional Analysis

5.3.1 Concurrent Changes in Firm Characteristics

A natural concern is that our results, particularly the persistent illiquidity increase and the 5% valuation drop, could reflect post-split changes in firm characteristics

²⁸The cutoffs follow the SGX policy threshold and the maximum post-split price in the treated sample; results are similar using S\$1.00 as the upper bound.

²⁹Chen, Singal, and Whitelaw (2016) point out a multicollinearity issue in bivariate regressions measuring co-movement betas, which we address in two ways. First, we regress the returns of high-priced stocks on the returns of low-priced stocks and use the residuals as orthogonalized high-priced stock returns. Second, we run univariate regressions by including only the high-priced or low-priced portfolio in each regression. Our results remain qualitatively similar.

rather than the clientele change. To assess this possibility, we examine two proxies for cash-flow fundamentals: ROA as a measure of profitability and dividend yield as a proxy for expected payouts. If reverse splits coincide with a deterioration in profitability or expected cash flows, we should observe declines in both ROA and dividend yield (DeAngelo, DeAngelo, Skinner, et al., 2009). Estimating an annual-frequency version of Equation 4, columns (1)–(2) of Table 13 show no significant post-split change, with point estimates economically small and indistinguishable from zero, indicating that the reverse splits did not coincide with any deterioration in profitability or payouts.

A second concern is that low-priced shares are associated with higher volatility and right-skewness, both features of lottery-like payoffs (Bali, Cakici, and Whitelaw, 2011). We test whether these return characteristics change following a reverse split by calculating the standard deviation and skewness of 10-day cumulative returns for each firm-year and estimating annual panel regressions with firm and year fixed effects. Columns (3) and (4) of Table 13 show no significant changes in either volatility or skewness.³⁰ Univariate comparisons in Table 1 suggest declines in both, but these differences are absorbed by firm and year fixed effects, implying that any apparent change reflects compositional or time-period differences rather than a treatment effect. The results are robust to adjusting returns for exposure to market returns (i.e., idiosyncratic return volatility) and using returns based on daily or monthly holding periods. This finding rules out a “less lottery-like” interpretation of the trading response in Section 4: the underlying return distribution is essentially the same, only the price label has changed.

Hence, across profitability (ROA), payout proxies (dividend yield), volatility, and skewness, we find no systematic post-split shifts. An interpretation of our main results due to the changes in firm characteristics is therefore difficult to sustain.

5.3.2 Insider Trading

Another valid concern is that the split is timed on information rather than regulation, and the valuation decline around the reverse split might reflect adverse private information. We check if insiders, including directors, the CEO, and substantial shareholders, read the split as bad news and sell before the public announcement. The SGX grace period gives them that opportunity: compliance timing is discretionary, so an insider who viewed the split as value-destroying had both the information and the window to act on it. Pre-announcement selling would therefore support an informational channel.

³⁰Two differences from Foucault, Sraer, and Thesmar (2011) may explain the null finding. First, their French reform raised the cost of speculative trading directly, whereas the MTP rule sorts investors only on a price label. Second, two effects offset in our setting: less retail order flow generates less transitory price pressure (consistent with the attenuated reversals in Section 5.2.1), while lower liquidity raises the price impact of a given order.

We construct the sample of insider trades from the SGX corporate action announcements that insiders and substantial shareholders are legally required to file within two business days of a change in holdings via “Changes in the Interests of Directors/CEO” (SGX Form 1) and “Changes in Substantial Shareholdings” (SGX Forms 3 and 5). From each filing, we identify holdings before and after the transaction and sign the trade as a buy or a sell from the change in shares held, discarding non-transactional filings and share-count changes that are mechanical consequences of the reverse split itself.³¹ We trace insider trading around each treated firm’s reverse split *announcement* date, yielding 138 insider transactions across 35 treated firms in the $[-120, +30]$ trading-day window around the announcement. Based on the sample, we build a balanced firm $\times$ event-day panel and estimate:

$$Y_{j,d} = \beta_{\text{pre}} \text{Pre}_{j,d} + \beta_0 \text{M0}_{j,d} + \beta_{\text{post}} \text{Post}_{j,d} + \alpha_j + \tau_{m(j,d)} + \varepsilon_{j,d}, \quad (12)$$

where $Y_{j,d}$ denotes insider trading outcomes for firm j on event-day d : an indicator and a count for insider buys, sells, and net sells (sells minus buys). Pre, M0, and Post mark the windows $[-30, -1]$, $[0, +5]$, and $[+6, +30]$, relative to an omitted baseline of $[-120, -31]$. All specifications absorb firm and calendar-year-month fixed effects, with standard errors clustered by firm.

Table A.9 in Appendix A shows no pre-announcement selling. In the thirty days before the announcement, neither the probability of insider selling nor the number of sells differs from its baseline period (columns 2 and 5). Selling emerges only once the split is public. Figure 11 re-estimates Equation 12 weekly and confirms the pattern: every pre-announcement coefficient is near zero. Because mandatory disclosure surfaces trades within two business days, genuine front-running would have appeared as selling *before* the announcement. The evidence reflects a response to public information rather than anticipation of it.

5.3.3 Announcement Effects of MTP Regulation

MTP regulation is not unique to Singapore. Several stock exchanges have introduced the MTP rule in recent years.³² While the specific implementation of the MTP rule varies across markets, a common motivation is to provide retail investors “a safeguard against certain market activity associated with low-priced securities” and to “increase investor confidence” (U.S. Securities and Exchange Commission, 1997).

³¹SGX reports the insider’s total holdings immediately before and after the disclosed change. We sign the transaction based on the change in the share count. A firm’s *net selling* on a given day is the count of that firm’s sell filings minus its buy filings that day ($n_{\text{sell}} - n_{\text{buy}}$). Two types of filings are excluded. First, non-transactional filings in which holdings are unchanged, e.g. a change in percentage interest arising from the firm’s own issuance rather than a trade by the insider. Second, we drop mechanical share-count changes produced by the reverse split itself. We identify these and exclude them, so that the measured net selling reflects only discretionary trades.

³²These markets include Nasdaq (1991/1997), NYSE (1999), Poland (2013), Brazil (2014), Singapore (2015), China (2020), Indonesia (2023), Saudi Arabia (2025), and Korea (2026). Appendix B provides details.

Our staggered design cannot measure what such a rule does to the low-price segment as a whole: the not-yet-treated controls are themselves low-priced, so any segment-wide repricing at the rule's arrival is differenced out, and the sample conditions on firms that eventually comply. We therefore ask a complementary question: how do MTP announcements affect the market valuation of the stocks they expose? A clean announcement test requires a policy that (i) affects a broad set of firms; (ii) permits reverse splits as a compliance mechanism, so the policy plausibly induces higher nominal prices; (iii) is not bundled with contemporaneous changes likely to dominate the price-floor effect; and (iv) has a discrete, verifiable announcement date. Singapore meets all four conditions. Poland and Korea are the closest available, though imperfect, benchmarks.³³ We study Poland (2 December 2013), Singapore (2 March 2015), and Korea (13 May 2026).³⁴

We classify a stock as exposed if its closing price fell below the local floor at any point in the sixty trading days before the announcement. Because exposure selects stocks whose prices have already fallen, we compare them against two portfolios of stocks that never breached the floor but traded close to it, with minimum pre-announcement prices between one and two times the floor and between two and three times. We then measure the cumulative abnormal returns (CAR) estimated over days $[-120, -21]$ against the value-weighted local-market return, and we aggregate firm-level abnormal returns into equal-weighted portfolios before computing event-window precision, since stocks within a market share an announcement and their abnormal returns are correlated (Kolari and Pynnönen, 2010).

Panel A of Table 14 reports six-day cumulative abnormal returns $CAR[0, 5]$ by market. Exposed stocks lose 5.0% in Poland, 3.2% in Singapore, and 4.3% in Korea, with portfolio t -statistics of -1.89 , -2.65 , and -1.71 . In Poland and Singapore, the two control portfolio returns are close to zero, suggesting that the decline is confined to the stocks the policy actually exposes rather than reflecting market-wide news. In Korea, the CARs of stock portfolios above the floor also fall by 3.3% and 2.4%, though neither estimate is statistically significant.

We next ask whether that decline varies with the level of a stock's retail ownership. Below-floor stocks have an average of 62.5% retail ownership against 43.8% above the floor, a paired difference of 18.7 percentage points across nine market means, so these rules hit the segment retail investors occupy. If the policy removes the trading a retail clientele supplies, the loss should be larger where that clientele is larger. We sort exposed stocks on pre-announcement retail ownership, measured over the twelve months before the announcement as the share not held by institutions,

³³The Polish rule also changed the trading protocol, while Korea's reform simultaneously tightened market-capitalization thresholds; Appendix B explains these caveats and the other exclusions in detail.

³⁴We use the formal policy dates: Poland's resolution on 2 December 2013, SGX's introduction on 2 March 2015, and Korea's approval on 13 May 2026. Earlier disclosures may have generated anticipation, so the formal-date estimates may understate the full response.

strategic entities, or government.³⁵

Panel B reports the result. The losses line up with ownership. The low, middle, and high-retail portfolios lose 2.0%, 4.5%, and 6.6%, respectively. The high-retail estimate is statistically significant ($t = -2.97$), and the High – Low portfolio is –4.6 percentage points ($t = -1.77$). This monotonic economic pattern is consistent with the clientele composition documented in Singapore, though the spread is only marginally significant and the comparison remains associational.³⁶

The announcement decline mirrors the split-month valuation decline in Section 5.1.4: the market prices the departure of the low-price clientele in expectation when the rule is announced, and in realization when the firm complies. Because the timing and form of compliance remain uncertain at announcement, the two responses are complements rather than alternatives. In all three markets, however, the price floor carries an escalating delisting threat, so the announcement return bundles the expected loss of the clientele with a change in delisting risk. Given this, and given three event dates, we place more weight on the within-market ownership gradient in Panel B than on the levels in Panel A.

6 Conclusion

We show that a normatively irrelevant but psychologically salient attribute, nominal price, has first-order consequences for both households and markets and that these consequences point in opposite directions. Exploiting regulation-induced, staggered reverse splits on the Singapore Exchange and account-level retail trades, we isolate mechanical jumps in nominal price from fundamentals and managerial intent. The design delivers within-investor and within-stock identification and links a micro demand shock to market-level outcomes, revealing a fundamental tension between investor protection and market quality.

When a regulatory reverse split pushes a stock out of the low-price tier, the same investor reduces trading in it, the more so the larger the price jump, and relocates the preference into other low-priced stocks. Those substitute trades lose money: within-investor buy-minus-sell portfolios underperform the investor’s own high-price trades, and this welfare loss deepens precisely after the reverse-split shock. Yet on the market side, the clientele these investors form is what sustains the stocks it inhabits. Once

³⁵Monthly investor-category ownership comes from Refinitiv, matched to daily prices from CRSP-linked Compustat for the United States and Compustat Global elsewhere. Retail ownership is the residual after subtracting institutional, strategic, and government holdings, clipped to $[0, 100]$. The vendor’s strategic category subsumes insider holdings, so no separate insider term is subtracted. We average the retail ownership over months $[-12, -1]$ and split the sample into terciles within each market, so the sort compares stocks to others facing the same announcement rather than across markets with different ownership levels. This results in 156 stocks: 12 in Poland, 56 in Singapore, and 88 in Korea. Coverage of ownership data differs across markets and is not random, thus we read Panel B as suggestive. Appendix B describes the construction and the firm-exclusion screens in full.

³⁶Repeating Table 14 using $CAR[0, 7]$ yields the same qualitative pattern.

expelled, treated stocks lose liquidity, incorporate market-wide news more slowly, and fall in value, with no concurrent change in fundamentals. The deterioration is not offset by institutional investors or other participants stepping in. Nominal price organizes trading habitats, confirming that the retail low-price clientele provides externalities that are both real and irreplaceable in this market segment.

The central policy implication is a tension that standard investor-protection frameworks do not easily resolve. The MTP rule targeted the symptoms of a behavioral preference that was measurably costly for retail investors. But in doing so, it eliminated a source of liquidity and price efficiency that benefited all market participants. Regulators designing MTP rules, position limits, or other interventions that mechanically alter the nominal price landscape in retail-dominated markets should weigh these market-quality costs, as they may be large, persistent, and not offset by other participants.

In the language of demand-based asset pricing (Kojien and Yogo, 2019), nominal price operates as a characteristic in the retail sector's demand function, and our setting supplies what such estimation typically lacks: variation in a characteristic that is uncorrelated with fundamentals. The valuation, co-movement, and migration results are the level, covariance, and substitution responses that a demand system predicts when a sector's characteristic-based demand is switched off. How exchanges and intermediaries design price environments for retail investors is thus a market design problem with measurable welfare consequences on both sides.

Declaration of Generative AI Use

During the preparation of this manuscript, the authors used OpenAI Codex to assist with language editing. The authors are fully responsible for the content of the manuscript.

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Table 1: Firm Characteristics Before and After MTP-Induced Reverse Splits

Variable	1 Year Before Reverse Split				1 Year After Reverse Split				Difference (b-a)	t_value
	No of Obs	Mean (a)	Median	Sd Dev	No of Obs	Mean (b)	Median	Sd Dev		
Raw Price	126	0.143	0.112	0.200	126	0.495	0.412	0.349	0.351	9.773
Adjusted Price	126	0.105	0.095	0.081	126	0.082	0.063	0.084	-0.022	-2.146
Market Cap (S\$ millions)	126	113.284	55.664	225.735	126	91.500	36.731	216.745	-21.784	-0.778
ROA	96	-0.094	0.020	0.389	116	-0.046	0.015	0.292	0.048	0.985
Dividend Yield	126	0.013	0.000	0.032	126	0.030	0.000	0.049	0.017	3.258
Percentage of Zero Trade Days	126	0.300	0.267	0.251	126	0.481	0.502	0.274	0.181	5.445
Bid Ask Spread	126	0.085	0.059	0.086	124	0.110	0.090	0.113	0.025	1.986
Turnover	126	0.040	0.007	0.091	126	0.013	0.004	0.036	-0.027	-3.080
Volatility	126	0.044	0.035	0.028	126	0.037	0.035	0.017	-0.007	-2.260
Skewness	126	0.136	0.161	0.382	124	-0.019	0.040	0.598	-0.155	-2.431
Retail Dollar Volume (S\$ millions)	126	0.543	0.012	1.696	126	0.167	0.022	0.632	-0.376	-2.323
Retail Buy Volume (S\$ millions)	126	0.267	0.005	0.829	126	0.078	0.007	0.299	-0.189	-2.400
Retail Sell Volume (S\$ millions)	126	0.276	0.006	0.869	126	0.089	0.012	0.334	-0.187	-2.247
Total Traded Volume (S\$ millions)	126	4.447	0.369	14.924	126	1.655	0.155	9.030	-2.791	-1.789

Notes: This table reports characteristics of the 126 treated firms over the one year before (a) and one year after (b) their MTP-induced reverse split, and tests whether each characteristic changes across the event. The last two columns report the post-minus-pre difference in means and its *t*-statistics based on heteroskedasticity-robust standard errors. Market capitalization and trading volumes are in millions of Singapore dollars. Variable definitions provided in Appendix C.

Table 2: Summary Statistics for Retail Accounts

	No. Accounts	Trading Volume			No. Trades		
		Total	Treated	Untreated	Total	Treated	Untreated
	37,730	24,637	3,014	5,014	1.72	0.21	0.50
Gender							
Female	12,359	19,136	2,207	3,583	1.42	0.17	0.39
Male	25,307	27,355	3,413	5,724	1.87	0.22	0.56
Annual Income (SGD)							
Below \$30 K	15,435	22,255	2,737	4,719	1.73	0.21	0.52
\$30 K TO \$60 K	11,007	17,962	2,131	3,998	1.52	0.18	0.46
\$60 K TO \$100 K	5,870	26,406	3,217	5,004	1.84	0.22	0.52
\$100 K TO \$300 K	5,299	43,797	5,466	8,096	2.00	0.23	0.51
Age Bracket							
18-29	71	3,980	534	663	0.62	0.08	0.19
30-39	2,587	14,896	1,745	3,731	1.25	0.16	0.41
40-49	7,950	19,510	2,784	4,751	1.51	0.19	0.49
50-59	9,958	27,888	3,558	5,807	1.76	0.21	0.51
60-69	8,853	29,368	3,614	5,960	1.97	0.24	0.58
70+	7,112	24,546	2,429	3,934	1.83	0.20	0.47
Activeness							
Low	12,579	2,275	507	803	0.19	0.04	0.07
Mid	12,743	10,856	1,851	2,680	0.76	0.12	0.25
High	12,408	61,582	6,763	11,703	4.26	0.46	1.20
Has CFD Account							
No	27,401	19,791	2,379	3,747	1.43	0.17	0.40
Yes	10,329	37,529	4,704	8,384	2.50	0.30	0.77

Notes: This table describes the 37,730 retail accounts that ever traded a stock later subject to an MTP-induced reverse split, and shows how their trading is distributed across price segments and demographic groups. It motivates the heterogeneity analysis by documenting which investor types are most active in the low-price segment before any treatment occurs. Columns report monthly trading in all SGX stocks (Total), in low-priced (below S\$0.20) stocks that undergo a reverse split (Treated), and in other low-priced stocks that do not (Untreated), during the sample period from January 2014 to December 2019. Trading activities are measured via total monthly dollar trading volume and the number of trades for each individual. Activeness terciles are formed on the fraction of months in which an investor trades; CFD accounts are margin accounts used to trade leveraged derivatives and proxy for risk tolerance.

Table 3: Effects of Reverse Splits on Retail Trading in Treated Stocks

	Dollar Trading Volume			Log of Dollar Volume			Number of Trades		
	Buy (1)	Sell (2)	Buy&Sell (3)	Buy (4)	Sell (5)	Buy&Sell (6)	Buy (7)	Sell (8)	Buy&Sell (9)
M[-5,-1]	-71.18 (59.86)	-75.83 (61.00)	-156.91 (120.87)	-0.08 (0.05)	-0.09 (0.05)	-0.12* (0.06)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
M[0,12]	-261.30*** (90.65)	-267.20*** (64.72)	-563.25*** (154.84)	-0.23*** (0.07)	-0.24*** (0.06)	-0.36*** (0.09)	-0.02** (0.01)	-0.03*** (0.01)	-0.04*** (0.01)
M[13,24]	-278.17** (116.62)	-278.84*** (88.26)	-577.27*** (205.79)	-0.22** (0.09)	-0.23*** (0.08)	-0.32*** (0.12)	-0.02* (0.01)	-0.02** (0.01)	-0.03* (0.02)
Individual-Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,261,058	3,261,058	3,261,058	3,261,058	3,261,058	3,261,058	3,261,058	3,261,058	3,261,058
Number of Months	65	65	65	65	65	65	65	65	65
Number of Accounts	37,730	37,730	37,730	37,730	37,730	37,730	37,730	37,730	37,730
Mean of DV	339.67	339.94	725.85	0.30	0.34	0.50	0.03	0.04	0.06
R ²	0.10	0.09	0.11	0.09	0.08	0.09	0.08	0.07	0.09

Notes: This table presents the average response of individual trading activities on 126 stocks that underwent a reverse split before and after the reverse split event. Following Equation 1, we regress the outcome variables on a dummy variable $M[-5, -1]$ that equals 1 in months 1 to 5 before the reverse stock split, a dummy variable $M[0, 12]$ that equals 1 if it is 0 to 12 months after, and a dummy variable $M[13, 24]$ that equals 1 in months 13 to 24 after the reverse stock split. The dependent variables of interest are dollar trading volume, the logarithm of one plus dollar trading volume, and number of trades for buy trades, sell trades, and buy plus sell trades in Columns 1-9, respectively. The Mean of DV represents the mean value of outcome variables during the benchmark period. The sample period is from January 2014 to December 2019. All specifications include individual $\times$ stock fixed effects and year-month fixed effects. Standard errors are double-clustered at the individual level and the year-month level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Migration of Retail Trading to Other Low-Priced Stocks

	Trading Migration	
	% Dollar Volume (1)	% Number of Trades (2)
M[-5,-1]	0.013 (0.013)	0.011 (0.012)
M[0,12]	0.069*** (0.015)	0.069*** (0.013)
M[13,24]	0.087*** (0.021)	0.091*** (0.019)
Individual FE	Yes	Yes
Year-Month FE	Yes	Yes
Observations	42,086	42,086
R^2	0.462	0.479
Mean of DV	0.545	0.592
Number of Months	65	65
Number of Accounts	5,352	5,352

Notes: This table examines account-level migration behavior to untreated low-priced stocks following the first reverse split experienced by a retail account. The outcome variable is the ratio of an individual's trading volume (column (1)) or number of trades (column (2)) in untreated low-priced stocks (price < \$0.20) relative to their total trading volume (excluding the treated stock). Following Equation 5, we regress the outcome variables on three key dummies: $M[-5, -1]$, which equals 1 for months 1 to 5 prior to the reverse split; $M[0, 12]$, which equals 1 for months 0 to 12 after the split; and $M[13, 24]$, which equals 1 for months 13 to 24 after the reverse split. In Column 1, the dependent variable is the ratio of trading volume in untreated low-priced firms (with prices below 20 cents) to the total trading volume (excluding the treated stock). Column 2 reports the analogous ratio for the number of trades. The sample covers January 2014 to December 2019. All specifications include individual and year-month fixed effects. Standard errors are double-clustered at the individual and year-month levels. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Retail Net Buy Portfolio Trading Performance After Reverse Split

	3-m Cumulative Return (1)	6-m Cumulative Return (2)	9-m Cumulative Return (3)	12-m Cumulative Return (4)
Post	-0.07 (0.16)	-0.62** (0.26)	-0.62** (0.29)	-0.82** (0.37)
Mid Price $\times$ Post	0.44 (0.53)	0.53 (0.58)	-0.08 (0.58)	-0.31 (0.65)
Low Price $\times$ Post	-1.32** (0.62)	-1.83** (0.82)	-2.63** (1.00)	-2.83** (1.08)
Price Level $\times$ Individual FE	Yes	Yes	Yes	Yes
Price Level $\times$ Year-Month FE	Yes	Yes	Yes	Yes
Observations	447,646	447,646	447,646	447,646
Number of Months	53	53	53	53
Number of Accounts	22,141	22,141	22,141	22,141
Mean of DV	-1.05	-1.66	-2.21	-2.48
R^2	0.16	0.16	0.16	0.16

Notes: This table presents the estimates of net buy (Buy-Sell) portfolio performance following a reverse split. The sample includes individuals who traded both high-priced and low-priced stocks during the 2014–2019 period and have ever experienced a stock reverse split induced by MTP policy. The dependent variables in Columns (1)–(4) are the 3-, 6-, 9-, and 12-month cumulative returns $R_{i,k,P}^{BmS}$, where $k = 3, 6, 9, 12$, $P \in L, M, H$, following Equation 6. The analysis tracks the cumulative return of their net buy trades across different time horizons and compares the performance of trades conducted before and after the very first stock reverse split in each account, following Equation 7. *Post* is an indicator variable equal to one for one year after an account's first reverse split, and zero otherwise. *Price Level = Low* is an indicator variable equal to one for low-priced stocks (below S\$0.20), *Price Level = Mid* is an indicator variable equal to one for mid-priced stocks (above S\$0.20 and below S\$1), and the reference group in this regression is high-priced stocks (above S\$1). All regressions include Price Level $\times$ Individual, and Price Level $\times$ Year-Month fixed effects. Mean of DV is the mean of the dependent variable over the estimation sample. Standard errors are double-clustered at the individual-year-month level and are reported in parentheses. **, *, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Heterogeneity in the Retail Trading Response to Reverse Splits

	Dollar Trading Volume – Buy& Sell			
	Gender (1)	Activeness (2)	Age (3)	Has CFD Account (4)
constant	632.20*** (109.77)	635.69*** (110.35)	640.54*** (110.52)	631.93*** (109.84)
M[-5,-1]	-134.21 (94.76)	-121.37 (64.08)	-118.46 (108.07)	-108.77 (95.23)
M[0,12]	-387.47** (133.21)	-129.68 (121.15)	-445.63** (144.39)	-372.32** (137.87)
Male × M[-5,-1]	2.37 (31.03)			
Male × M[0,12]	-114.69*** (27.54)			
Medium × M[-5,-1]		-92.59 (55.97)		
Medium × M[0,12]		-276.98*** (40.98)		
High × M[-5,-1]		21.83 (137.78)		
High × M[0,12]		-507.20*** (74.96)		
Age<50 × M[-5,-1]			-68.29 (41.97)	
Age<50 × M[0,12]			-106.02** (37.38)	
Has CFD Account × M[-5,-1]				-74.83 (67.38)
Has CFD Account × M[0,12]				-296.41*** (54.14)
Individual-Stock FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Observations	3,256,498	3,261,058	3,157,578	3,261,058
Number of Months	65	65	65	65
Number of Accounts	37,666	37,730	36,531	37,730
Mean of DV	317.638	317.548	320.428	317.548
R ²	0.10	0.10	0.10	0.10

Notes: This table presents effect heterogeneity. We regress the buy and sell dollar trading volume on pre-period dummy variable $M[-5, -1]$ and post-period dummy variable $M[0, 12]$ and their interactions with variables representing the individual characteristics, including an individual's gender, trading activeness, age, and risk profile. *Male* in Column (1) is a dummy variable equal to one for male investors. Trading activeness is measured by the fraction of months in which individuals trade over their entire trading history. In Column (2), we classify investors into terciles: *Low Activeness*, *Median Activeness*, and *High Activeness*. In Column (3), we classify investors into the younger group $Age < 50$ and those who are age 50 or above. In Column (4), risk tolerance is proxied by having a CFD (Contract for Difference) account, which is used for trading leveraged derivative instruments. The Mean of DV represents the mean value of outcome variables for the sample with base characteristics during the benchmark period, which is 6 to 15 months before the reverse stock split. The sample period is from January 2014 to December 2019. All specifications include individual × stock fixed effects and year-month fixed effects. Standard errors are double-clustered at the individual level and the year-month level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Effects of the SGX Lot Size Reduction on Retail Trading by Price Tier

	Pr(Trades) (1)	Number of Trades (2)	Log(Dollar Volume) (3)
High Price	0.06*** (0.01)	-0.07 (0.05)	0.70*** (0.07)
High Price $\times$ Post	0.00 (0.01)	0.04 (0.06)	-0.01 (0.09)
Individual FE	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes
Observations	3,575,820	3,575,820	3,575,820
Number of Months	49	49	49
Number of Accounts	37,730	37,730	37,730
Mean of DV	0.14	0.52	1.35
R^2	0.19	0.26	0.21

Notes: This table presents the effect of SGX lot size change from 1000 shares to 100 shares on retail trading shift in high-priced stocks (above S\$1.0), compared to trading in low-priced stocks (below S\$0.20). *Post* dummy equals 1 if it is after January 2015, when the SGX lot size change policy was implemented. The dependent variables are the probability of trading, the number of trades, and the logarithm of one plus dollar trading volume for low-priced and high-priced stocks, measured monthly for each individual. We regress on *High Price* dummy, which equals 1 if the stock price is above S\$1.0, and its interaction terms with *Post* dummy. The sample period is from January 2013 to January 2017, a four-year window spanning the policy change. Our sample consists of individuals who are exposed to the reverse split induced by the MTP policy, which is the same sample as Table 3. All specifications include individual fixed effects and year-month fixed effects. Standard errors are double-clustered at the individual-year-month level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Effects of Reverse Splits on Stock Trading Volume

	log of Dollar Volume								
	<i>DiD with matched control</i>						<i>Staggered DiD</i>		
	Low-priced control			High-priced control			Not-yet treated control		
	Buy (1)	Sell (2)	Buy&Sell (3)	Buy (4)	Sell (5)	Buy&Sell (6)	Buy (7)	Sell (8)	Buy&Sell (9)
M[-5,-1]	-0.01 (0.04)	-0.01 (0.04)	-0.01 (0.06)	-0.06 (0.05)	-0.08* (0.05)	-0.09 (0.06)	-0.05 (0.04)	-0.05 (0.04)	-0.06 (0.06)
M[0,12]	-0.02 (0.07)	-0.02 (0.07)	-0.02 (0.09)	-0.08 (0.07)	-0.09 (0.07)	-0.11 (0.09)	-0.14** (0.07)	-0.14** (0.07)	-0.18* (0.09)
M[13,24]	0.01 (0.07)	0.01 (0.06)	0.02 (0.09)	-0.04 (0.07)	-0.05 (0.07)	-0.06 (0.09)	-0.11 (0.07)	-0.10 (0.07)	-0.14 (0.10)
Treated $\times$ M[-5,-1]	-0.06 (0.04)	-0.06 (0.04)	-0.08 (0.05)	0.02 (0.05)	0.03 (0.06)	0.03 (0.07)			
Treated $\times$ M[0,12]	-0.21*** (0.06)	-0.22*** (0.06)	-0.30*** (0.08)	-0.14* (0.07)	-0.14* (0.07)	-0.19** (0.09)			
Treated $\times$ M[13,24]	-0.22*** (0.08)	-0.23*** (0.08)	-0.32*** (0.10)	-0.14 (0.09)	-0.14* (0.08)	-0.18 (0.11)			
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,618	5,618	5,618	5,875	5,875	5,875	5,406	5,406	5,406
Number of Months	58	58	58	56	56	56	65	65	65
Number of Stocks	190	190	190	194	194	194	126	126	126
Mean of DV	0.25	0.26	0.38	0.39	0.40	0.56	0.22	0.22	0.32
R ²	0.49	0.48	0.51	0.74	0.74	0.76	0.52	0.52	0.51

Notes: This table presents difference-in-differences estimates of the effect of reverse splits on retail trading volume in treated stocks, aggregated from the brokerage account data to the stock-month level, comparing treated stocks against a different control sample. The outcome variables are the logarithm of one plus retail dollar trading volume for buy trades, sell trades, and buy plus sell trades, respectively. Columns (1)–(3) compare treated stocks against a matched control sample of low-priced stocks ($\leq$ \$50.20, never treated). Columns (4)–(6) compare treated stocks against a matched control sample of high-priced stocks (above \$50.5). Columns (7)–(9) report estimates from a not-yet-treated control design. For the matched control columns, we use coarsened exact matching (CEM) to find the best match sample for treated stocks in each price level group, based on the past 3-month returns, average turnover, and zero-trade days based on 1-year data before the MTP introduction. We then assign a pseudo-event date as the matched treated firm's reverse split date. Following Equation 3, we regress the outcome variables on a pre-period dummy variable $M[-5, -1]$, post-period dummy variables $M[0, 12]$, $M[13, 24]$, and their interaction terms with dummy variable $Treated$, which equals 1 if it is a treated stock that underwent an MTP-induced reverse split and zero for matched control stocks in Columns 1-6. Columns 7-9 employ staggered DiD and use the not-yet-treated sample as control, following Equation 3. The Mean of the DV represents the mean value of outcome variables during the benchmark period for both treated and control samples. The sample period is from January 2014 to December 2019. All specifications include stock fixed effects and year-month fixed effects. Standard errors are double-clustered at the firm level and the year-month level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Effects of Reverse Splits on Stock Liquidity

	Turnover (1)	Zero-Trade% (2)	Bid-Ask Spread (3)	Composite Illiq. (4)
M[-5,-1]	-0.008 (0.008)	-0.009 (0.021)	0.017 (0.012)	0.081 (0.081)
M[0,12]	-0.030** (0.014)	0.097** (0.044)	0.042* (0.022)	0.387** (0.152)
M[13,24]	-0.034** (0.014)	0.094* (0.051)	0.021 (0.028)	0.361** (0.171)
M[25+]	-0.035** (0.015)	0.071 (0.054)	0.008 (0.033)	0.306* (0.181)
Stock FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Observations	6,458	6,458	6,458	6,458
R ²	0.348	0.655	0.301	0.507
Mean of DV	0.032	0.293	0.080	-0.266
Number of Months	64	64	64	64
Number of Stocks	126	126	126	126

Notes: This table reports dynamic liquidity effects of MTP-induced reverse splits in the sample with 126 treated stocks. Event time is measured in months relative to the reverse split date. The benchmark period ($M[-15, -6]$) is the omitted reference. *Turnover* is monthly dollar volume divided by market capitalization. *Zero-Trade%* is the fraction of trading days in the month with zero volume. *Bid-Ask Spread* is the monthly average of the daily ratio of the quoted bid-ask spread to the nominal closing price, consistent with Table 1. *Composite Illiq.* is the average of the standardized ranks of zero-trade percentage, negative turnover, and bid-ask spread. The Mean of DV represents the mean value of outcome variables during the benchmark period. Every regression requires all outcomes to be non-missing and includes stock and year-month fixed effects. Standard errors are double-clustered at the stock and year-month levels. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 10: Effects of Reverse Splits on Price Delay

	Price Delay Measures		
	D1 (1)	D2 (2)	D3 (3)
constant	0.653*** (0.044)	1.759*** (0.075)	1.759*** (0.076)
W[-20,-1]	0.034 (0.028)	0.080 (0.050)	0.081 (0.050)
W[26,52]	0.154** (0.068)	0.314*** (0.112)	0.313*** (0.114)
W[53, 104]	0.120 (0.077)	0.252* (0.132)	0.249* (0.133)
Stock FE	Yes	Yes	Yes
Year-Month-Week FE	Yes	Yes	Yes
Observations	16,687	16,349	16,349
Number of Weeks	219	219	219
Number of Stocks	126	126	126
Mean of DV	0.734	1.927	1.927
R ²	0.203	0.133	0.133

Notes: This table reports regression results on the average effect of the stock reverse split on price delay measures following Hou & Moskowitz (2005). The dependent variables are three rolling measures of price delay calculated using weekly returns over a 26-week window. $D1 = 1 - \frac{R^2_{restricted}}{R^2_{unrestricted}}$, where the Restricted Model: $r_{j,t} = \alpha_j + \beta_j R_{m,t} + \varepsilon_{j,t}$ and Unrestricted Model: $r_{j,t} = \alpha_j + \beta_j R_{m,t} + \sum_{n=1}^4 \delta_j^{(-n)} R_{m,t-n} + \varepsilon_{j,t}$. D2: A weighted average of the lagged coefficients. To account for signed coefficients canceling out, this measure is redefined using the absolute values of the coefficients: $D2 = \frac{\sum_{n=1}^4 n |\delta_n|}{|\beta| + \sum_{n=1}^4 |\delta_n|}$. D3: Similar to D2 but weights the lagged coefficients by their precision (dividing each by its standard error) to minimize the impact of noisy estimates. $D3 = \frac{\sum_{n=1}^4 n \frac{|\delta^{(-n)}|}{se(\delta^{(-n)})}}{\frac{|\beta|}{se(\beta)} + \sum_{n=1}^4 \frac{|\delta^{(-n)}|}{se(\delta^{(-n)})}}$. $W[-20, -1]$ is a dummy variable equal

to one for the 20 weeks preceding the reverse split. $W[26, 52]$ and $W[53, 104]$ are dummy variables representing the post-split periods from week 26 to 52 and week 53 to 104, respectively (weeks 0–25 are excluded to avoid transition noise as the dependent variables are calculated based on past 26-week window). The benchmark period is from week 52 to week 21 prior to the reverse split. All regressions include Firm Fixed Effects and Year-Month-Week Fixed Effects to control for time-invariant firm characteristics and common time trends. Standard errors, clustered by year-month-week and stock, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 11: Short-term Return Reversal and Retail Trading

Panel A: Return Reversal and Low-Priced Stocks				
Dependent variable:	Ret_{t+1}			
	Full Sample (1)	Low Price (2)	High Price (3)	Full Sample (4)
Ret	-0.103*** (0.013)	-0.151*** (0.015)	-0.017 (0.016)	-0.017 (0.016)
Low Price				0.004 (0.005)
Ret $\times$ Low Price				-0.134*** (0.019)
Constant	0.002 (0.002)	0.004 (0.004)	0.001 (0.003)	0.001 (0.003)
Observations	44,354	16,914	27,440	44,354
R ²	0.022	0.037	0.019	0.058
Panel B: Effects of Reverse Splits on Return Reversals				
Dependent variable:	Ret_{t+1}			
	(1)	(2)	(3)	(4)
Ret	-0.093*** (0.023)	-0.149*** (0.043)	-0.170*** (0.043)	-0.185*** (0.037)
Ret $\times$ Post		0.084* (0.051)	0.051 (0.049)	0.056 (0.046)
Ret $\times$ Post $\times$ RTI			0.048** (0.021)	0.046** (0.021)
Ret $\times$ RTI			0.020** (0.009)	0.021** (0.010)
Post $\times$ RTI			-0.001 (0.006)	-0.002 (0.006)
Post		0.017** (0.008)	0.021 (0.013)	0.022 (0.014)
RTI			-0.006* (0.004)	-0.007* (0.004)
Constant	-0.007 (0.005)	-0.017** (0.008)		
Δ Zero-Trade controls	-	-	-	Yes
Stock FE	-	-	Yes	Yes
Year-Month FE	-	-	Yes	Yes
Observations	7,918	7,918	7,918	7,918
R ²	0.009	0.014	0.108	0.111

Notes: Panel A reports the results of Fama-MacBeth regressions on short-term reversals. The regression specifications are as follows: $r_{i,t+1} = \beta_1 r_{i,t} + \varepsilon_{i,t}$, $r_{i,t+1} = \beta_1 r_{i,t} + \beta_2 r_{i,t} \times \text{Low Price}_{i,t} + \beta_3 \text{Low Price}_{i,t} + \varepsilon_{i,t}$, where $r_{i,t+1}$ and $r_{i,t}$ refer to stock returns for firm i in month $t + 1$ and month t , respectively, and $\text{Low Price}_{i,t}$ equals 1 if the nominal price for firm i is at or below S\$0.20, and 0 otherwise. We present the results of the first regression specification for all firms listed in SGX in Column (1), for low-priced ($\leq$ S\$0.20) firms in Column (2), and for high-priced ($>$ S\$0.20) firms in Column (3). In Column (4), we show the results of the second regression specification. Monthly returns are winsorized at the 1st and 99th percentiles each month. Panel B presents the effects of reverse splits on short-term reversal for treated firms following Equation 9. We report unconditional results in Column (1) and estimates of Equation 9 in Column (2). Columns (3) and (4) interact reversal strength with retail trading intensity (RTI), where RTI is retail turnover, defined as retail trading volume (retail buys plus retail sells) scaled by market capitalization and standardized to unit standard deviation. Column (4) additionally controls for the change in the proportion of zero-trading days (Δ Zero-Trade) and its full set of interactions with *Ret* and *Post* (including the triple interaction $\text{Ret} \times \text{Post} \times \Delta\text{Zero-Trade}$); coefficients on these controls are suppressed for brevity and indicated by “Yes”. Standard errors are double-clustered at the stock and month-by-year levels. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 12: Effects of Reverse Splits on Return Co-movement

	β_{LP} (1)	β_{HP} (2)	$\beta_{LP,0} + \beta_{LP,-1}$ (3)	$\beta_{HP,0} + \beta_{HP,-1}$ (4)
Treated	0.178 (0.159)	-1.546*** (0.523)	0.342* (0.205)	-1.634*** (0.574)
Post	0.771*** (0.174)	-1.992*** (0.571)	1.256*** (0.224)	-2.409*** (0.624)
Treated $\times$ Post	-0.676*** (0.225)	1.610** (0.739)	-0.685** (0.289)	1.729** (0.808)
Constant	0.287** (0.123)	2.417*** (0.404)	-0.046 (0.159)	2.804*** (0.443)
Observations	328	328	328	328
Mean of DV	0.465	0.871	0.296	1.170
R^2	0.064	0.049	0.113	0.059

Notes: This table reports regression results on the average treatment effect of the share reverse split on return co-movement. We first estimate return betas, using a firm-year-month panel sample from January 2014 to December 2019. The sample includes both treated firms and matched untreated firms. We estimate co-movement betas using the following regression with monthly data for each firm before or after the reverse split: $r_{i,t} = \beta_{LP}R_{LP,t} + \beta_{HP}R_{HP,t} + \varepsilon_{i,t}$, where $r_{i,t}$ is the return of the treated firm in month t , $R_{LP,t}$ is the value-weighted portfolio return of shares with month-end prices below 20 cents in month t excluding firm i itself, and $R_{HP,t}$ is the value-weighted portfolio return of shares with month-end prices above 20 cents and below 1 dollar, excluding firm i itself. For each regression, we only include $R_{LP,t}$ or $R_{HP,t}$. $\beta_{LP,t}$ is used in Column (1) and $\beta_{HP,t}$ is used in Column (2). We also estimate co-movement betas using the following regression, controlling for lagged returns: $r_{i,t} = \beta_{LP,0}R_{LP,t} + \beta_{HP,0}R_{HP,t} + \beta_{LP,-1}R_{LP,t-1} + \beta_{HP,-1}R_{HP,t-1} + \varepsilon_{i,t}$, where $R_{LP,t-1}$ and $R_{HP,t-1}$ are lagged returns of low-priced and high-priced portfolios. We use $\beta_{LP,0} + \beta_{LP,-1}$ in Column (3) and $\beta_{HP,0} + \beta_{HP,-1}$ in Column (4). With the estimated betas, we further run the DiD analysis following Equation 11. The Mean of DV represents the mean value of outcome variables in the pre-period. Standard errors are corrected based on the Newey-West adjustment. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 13: Effects of Reverse Splits on Other Firm Characteristics

Dependent Variable:	ROA (1)	Dividend Yield (2)	Volatility (3)	Skewness (4)
Year 0	−0.014 (0.025)	−0.003 (0.007)	−0.007 (0.016)	−0.150 (0.265)
Year 1	0.041 (0.043)	0.001 (0.006)	−0.003 (0.016)	0.069 (0.329)
Long Term	0.083 (0.060)	0.119 (0.141)	−0.013 (0.020)	−0.272 (0.412)
Stock FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	680	640	741	741
R^2	0.410	0.386	0.454	0.215

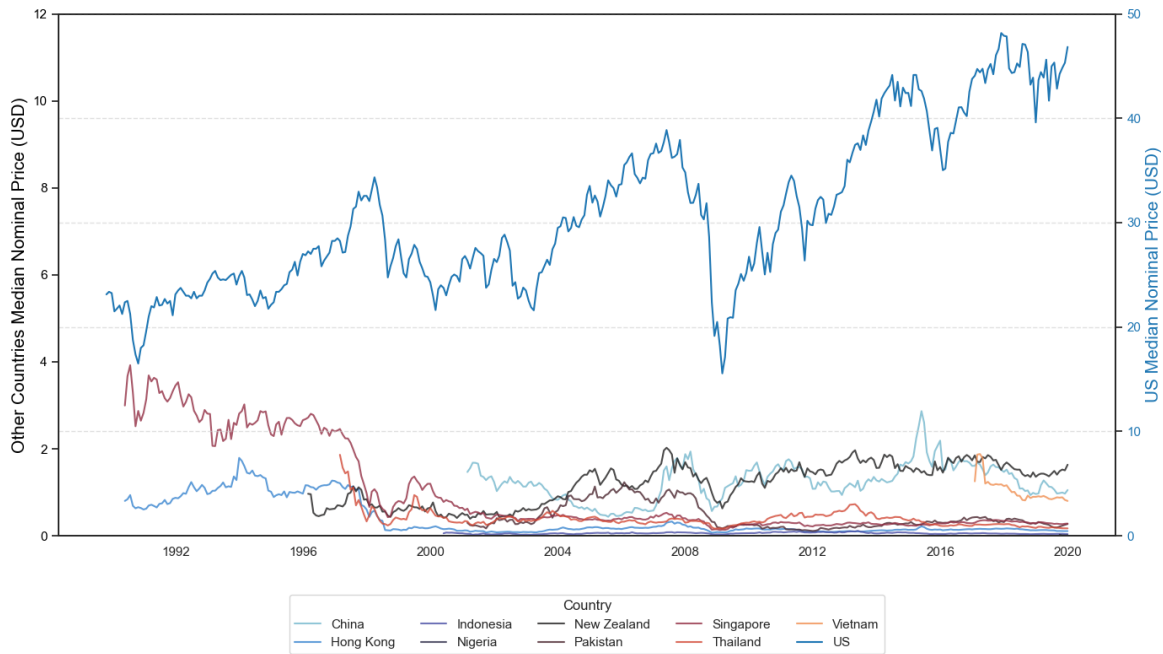
Notes: This table reports regression results on the dynamic effect of the share reverse-split on annual ROA, dividend yield, volatility, and skewness, using a firm-year panel sample. We estimate the following model using annual data: $Y_{i,t} = \alpha_i + \delta_t + \beta_0 \text{Year } 0_{i,t} + \beta_1 \text{Year } 1_{i,t} + \beta_2 \text{LongTerm}_{i,t} + \epsilon_{i,t}$, where α_i and δ_t are stock and year fixed effects. $\text{Year } 0_{i,t}$ equals one in the year in which firm i reverse-splits its shares, $\text{Year } 1_{i,t}$ equals one in the following year, and $\text{LongTerm}_{i,t}$ equals one in the second and later years after the split; the omitted category is the years before firm i 's reverse split. The dependent variables are annual ROA, annual dividend yield, annual skewness, and volatility. Variable definitions are provided in Appendix C. Standard errors are clustered by stock and displayed in parentheses. For brevity, the coefficient estimates on stock and year fixed effects are not reported. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 14: Announcement Returns Around the Adoption of Minimum Trading Price Rules

Panel A: CAR_[0, +5] by pre-announcement price				
Market	Below floor	1–2× floor	2–3× floor	
Poland	-0.050*	-0.000	-0.003	
	(-1.89)	(-0.01)	(-0.17)	
<i>N</i>	118	55	35	
Singapore	-0.032***	-0.006	-0.002	
	(-2.65)	(-0.64)	(-0.18)	
<i>N</i>	232	101	49	
Korea	-0.043*	-0.033	-0.024	
	(-1.71)	(-1.41)	(-0.95)	
<i>N</i>	160	257	196	
Panel B: CAR_[0, +5] by pre-announcement retail ownership				
	Retail ownership tercile			
	Low	Middle	High	High – Low
CAR _[0,+5]	-0.020	-0.045*	-0.066***	-0.046*
	(-1.03)	(-1.97)	(-2.97)	(-1.77)
<i>N</i>	51	52	53	104

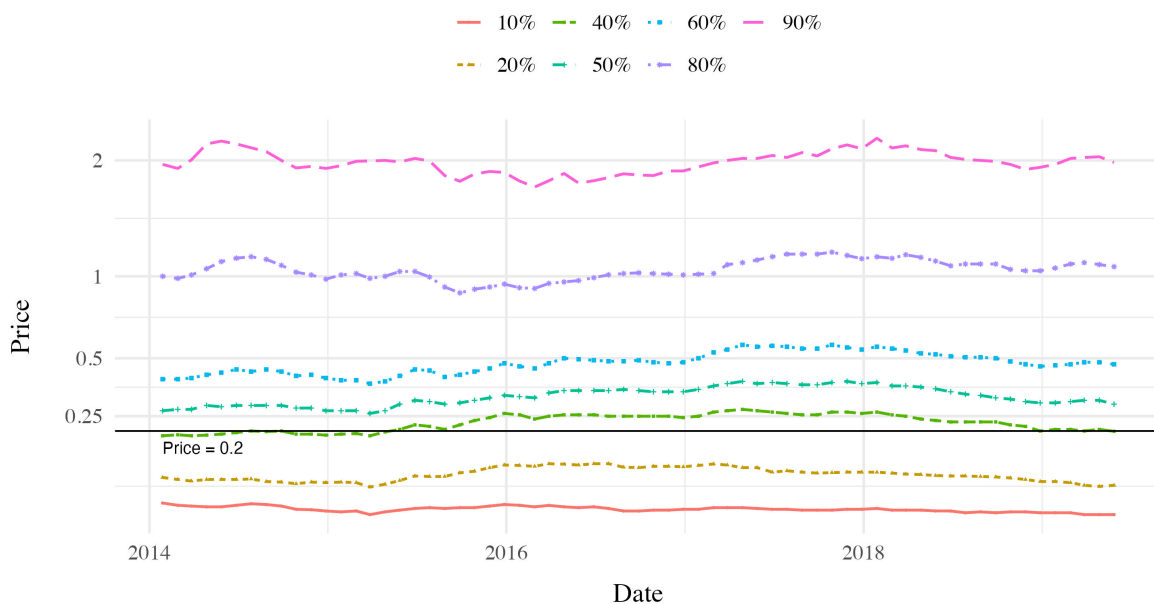
Notes: This table reports market-model cumulative abnormal returns around the announcements in Poland (2 December 2013), Singapore (2 March 2015), and Korea (13 May 2026). A stock enters the Below-floor portfolio if its closing price fell below the local minimum-price threshold at some point during days $[-60, -1]$. The two comparison portfolios contain stocks that never breached the threshold, grouped by their minimum price over the same window: between one and two times the threshold, and between two and three times. At least twenty price observations are required. Korean stocks must also have at least ten observed market-capitalization values in days $[-30, -1]$, none below KRW30 billion. Firm-level market models estimate α and β over days $[-120, -21]$ against the value-weighted local-market return. Panel A forms an equal-weighted daily portfolio separately for each market and price band. Panel B averages retail ownership over months $[-12, -1]$, applies the firm-exclusion screens in Appendix B, and sorts the 156 remaining below-floor stocks into terciles within each market. It then forms equal-weighted daily abnormal-return portfolios after aligning the three announcements in event time. The High – Low column is the daily long–short portfolio. Numbers in parentheses are portfolio *t*-statistics based on the corresponding estimation-window portfolio variance and therefore allow firms exposed to the same announcement to move together. Estimates in Panel B are associational rather than causal. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Figure 1: Median Nominal Stock Prices Across Countries



Notes: This figure shows the median nominal prices of all stocks listed in selected countries' main-board exchange, over the period from January 1989 to December 2019. The plots of median nominal price of other countries refer to the y-axis on the left, and the US median nominal price plot is on the right y-axis. All prices are converted to USD based on the spot exchange rate.

Figure 2: Cross-Sectional Distribution of Nominal Stock Prices on Singapore Exchange



Notes: This figure shows the time-series of cross-sectional distribution of the nominal prices of all stocks in SGX over the period from Jan 2014 to December 2019. The figure plots the prices at the 10th, 20th, 40th, 50th, 60th, 80th, and 90th percentiles of the cross-sectional distribution across all stocks, each month. The black horizontal line indicates S\$0.20. The y-axis scale follows a square root transformation of the price to make distances between higher values compressed and lower values expanded.

Figure 3: Timing of MTP-Induced Reverse Splits

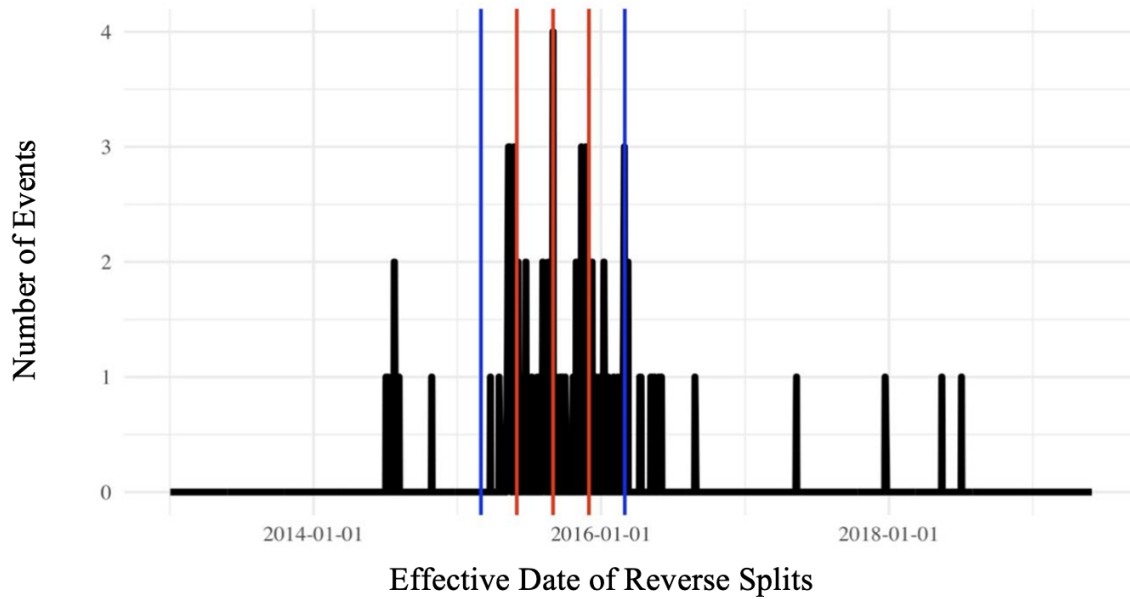
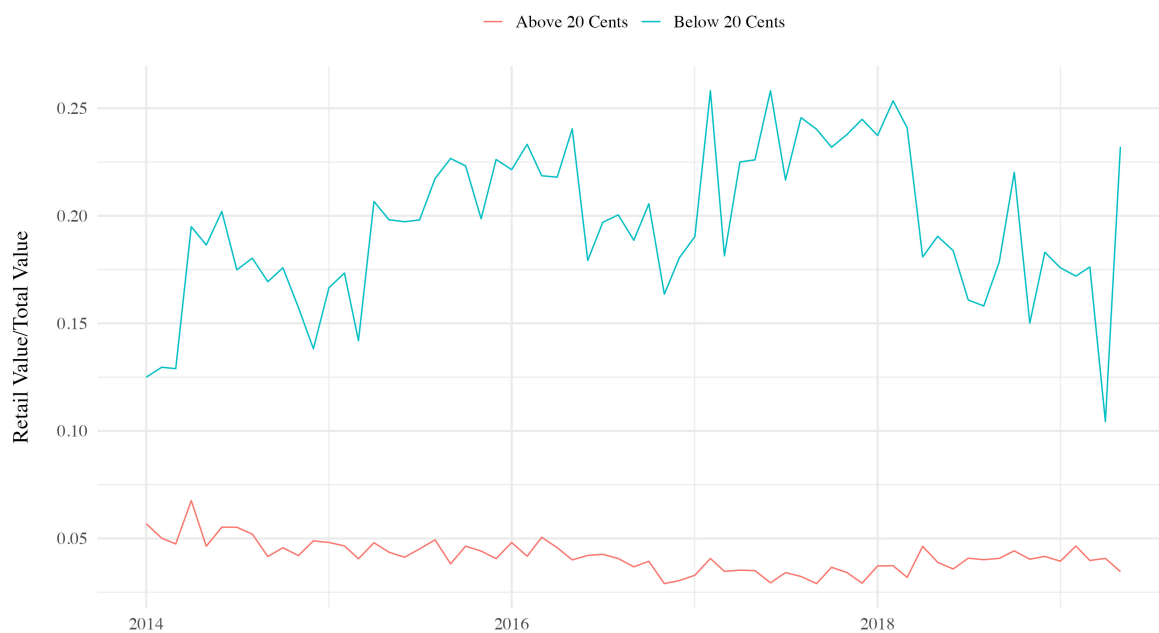
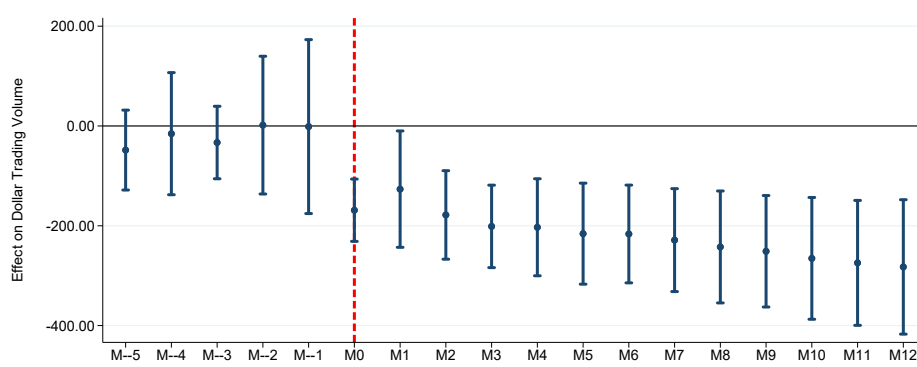


Figure 4: Retail Share of Trading Volume on the Singapore Exchange by Price Tier

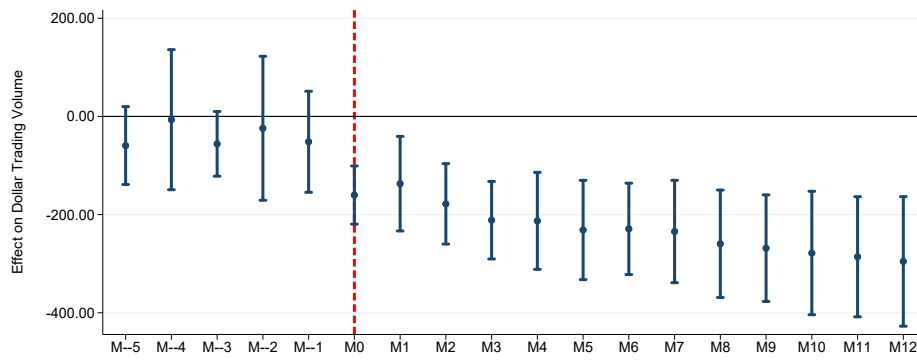


Notes: This figure displays the monthly time-series of retail trading volume as a proportion of total trading volume on the stock exchange from 2014 to 2019. The green line represents the group of stocks with nominal prices below S\$0.20 and the red line indicates the group of stocks with nominal prices above S\$1. To ensure data quality, we require each stock to have non-zero retail trading in the month to be included.

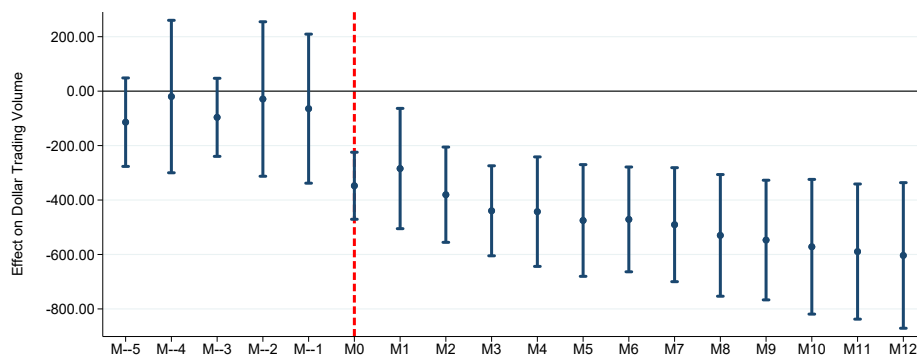
Figure 5: Dynamic Effects of Reverse Splits on Retail Trading



(a) Buy



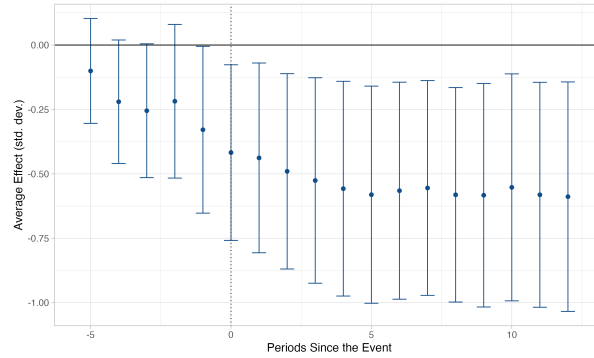
(b) Sell



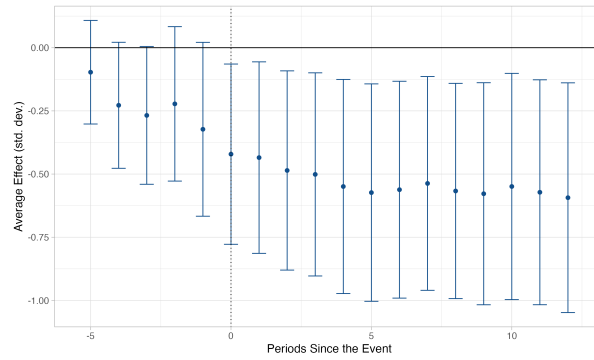
(c) Buy&Sell

Notes: This figure shows the dynamic response of the dollar trading volume on stocks that underwent a reverse split (treated) for each individual. We examine the changes in dollar trading volume of buy and sell trades in treated stocks before and after their reverse split event within each individual account following Equation 2. The regression includes individual $\times$ stock fixed effects and year-month fixed effects. Standard errors are double-clustered at both the individual level and the year-month level. The confidence intervals of coefficients are plotted at the 95% level.

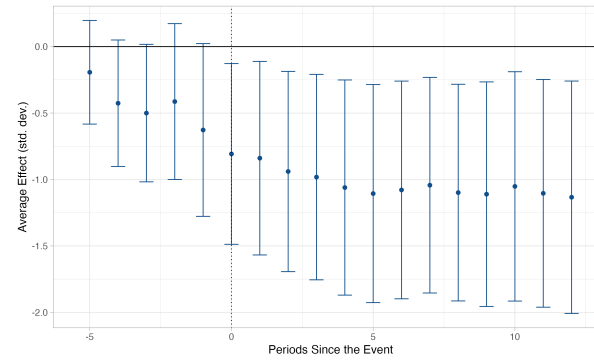
Figure 6: Dynamic Effects of Reverse Splits on Stock Trading Volume



(a) Buy



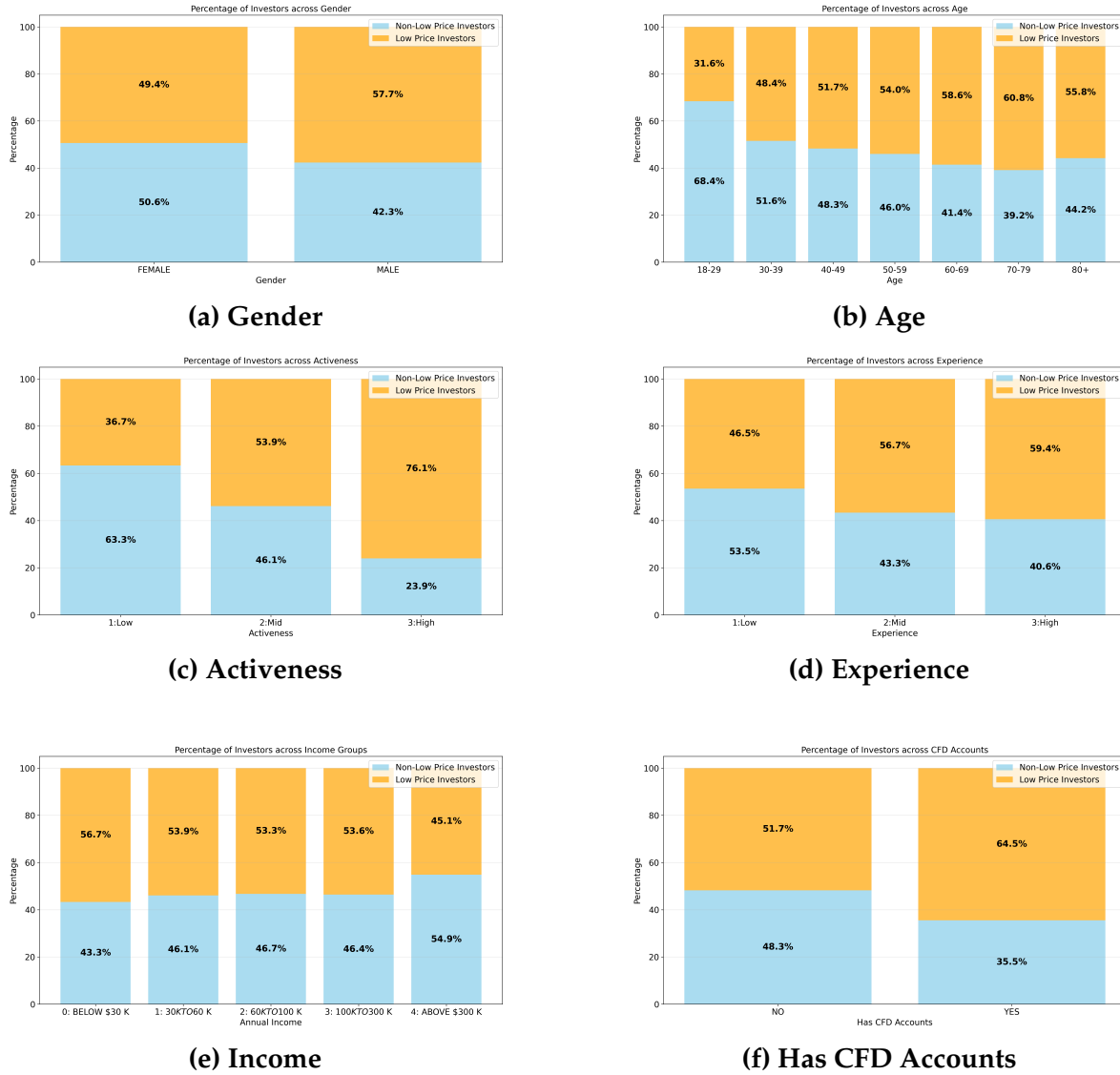
(b) Sell



(c) Buy&Sell

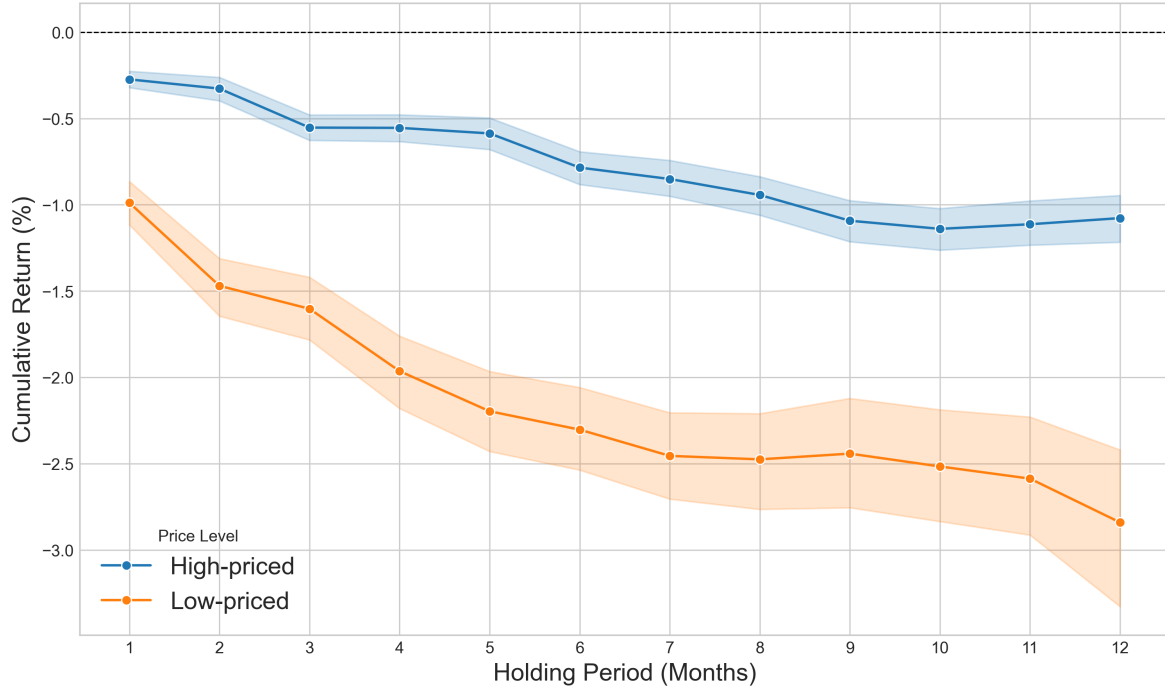
Notes: This figure shows the dynamic response of the aggregated dollar trading volume on stocks that underwent a reverse split (treated). We examine the changes in aggregated dollar trading volumes of buy, sell, and buy plus sell trades in treated stocks before and after their reverse split event, following Equation 4. The regression includes stock fixed effects and year-month fixed effects. Standard errors are double-clustered at both the firm-level and the year-month level. The confidence intervals of coefficients are plotted at the 95% level.

Figure 7: Characteristics of Retail Investors in Low-priced Stocks



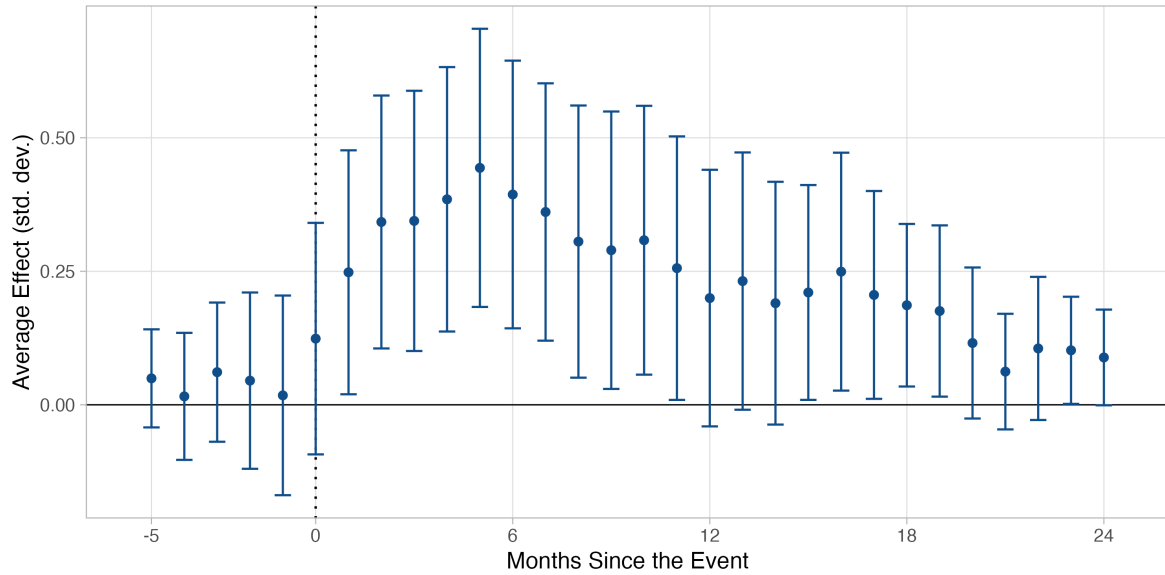
Notes: This figure documents which retail investors participate in the low-price segment, providing the descriptive basis for the heterogeneity tests in Section 4. Within each demographic group, investors are classified as *Low Price Investors* if they ever traded a stock priced below S\$0.20 during the sample period, and as *Non-Low Price Investors* otherwise; each bar therefore sums to 100% within the group and reports the share of that group falling into each category. Panels (a)–(f) split investors by gender, age bracket, trading activeness (terciles of the fraction of months in which an investor trades), trading experience, annual income bracket, and whether the investor holds a Contract for Difference (CFD) margin account, a proxy for risk tolerance. Group definitions match those in Table 2. The sample covers all retail accounts with demographic information that traded on SGX between January 2014 and December 2019. It is broader than the sample in Table 2, which is restricted to accounts that ever traded a stock later subject to an MTP-induced reverse split.

Figure 8: Net Buy Portfolio Returns: Low-Priced vs. High-Priced Stocks



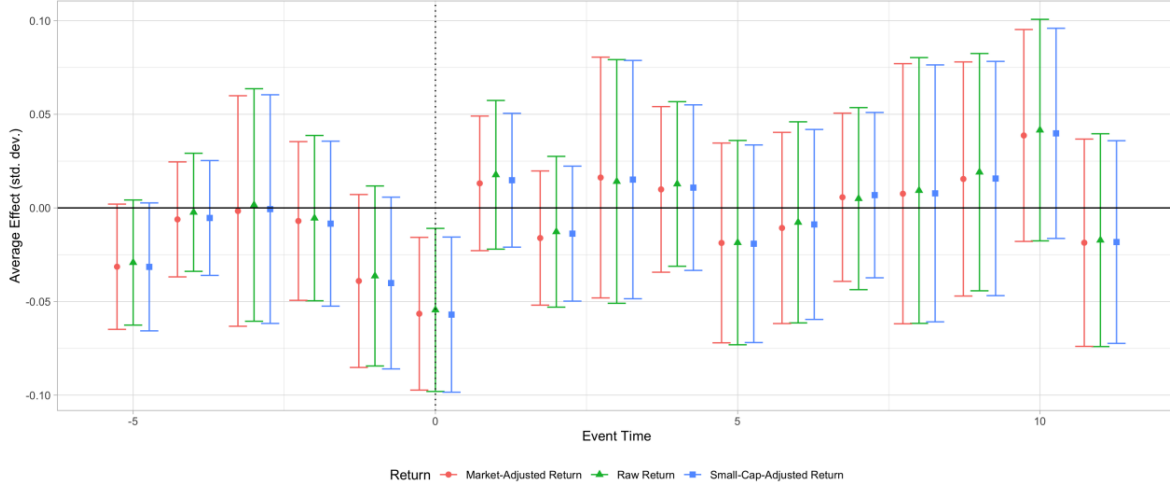
Notes: This figure compares the subsequent 12-month returns of net buy portfolios for low-priced (below S\$0.20) versus high-priced (above S\$1) stocks traded by individual i in month t . The analysis focuses on individuals who traded both stock types during the sample period (2014–2019). We denote the net buy portfolio return as $R_{i,k,P}^{BmS}$, representing the difference between the returns on securities bought ($R_{i,k,P}^B$) and sold ($R_{i,k,P}^S$) by individual i in month t and held for k months ($k = 1...12$). The portfolio P denotes either Low-priced (L) or High-priced (H) stocks. We calculate $R_{i,k,P}^B$ ($R_{i,k,P}^S$) by taking the dollar trading volume weighted average returns on the securities bought (sold) in month t and held for the next k months. The confidence intervals of cumulative return at each horizon for individuals are plotted at the 95% level.

Figure 9: Dynamic Effects of Reverse Splits on Stock Illiquidity



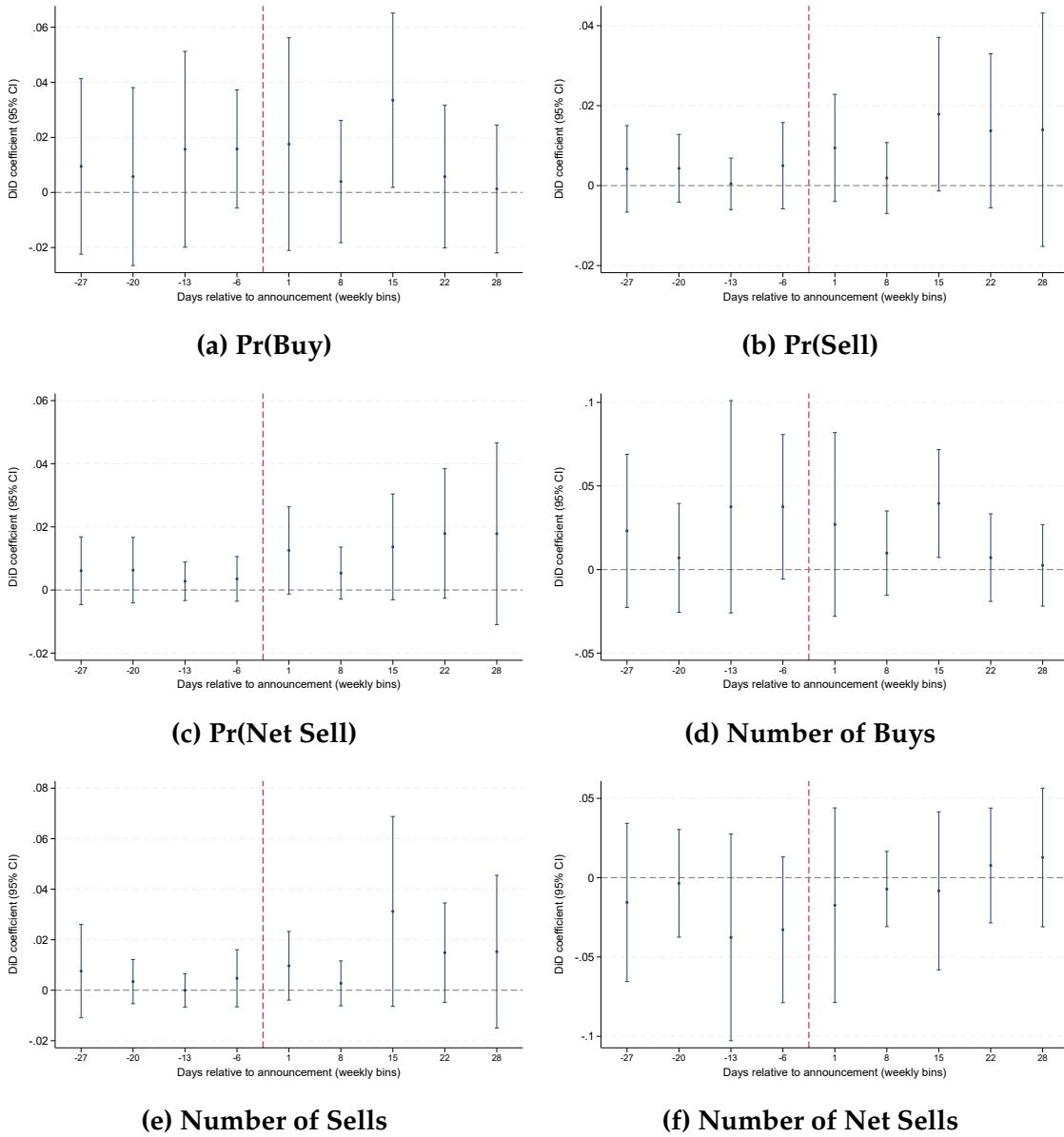
Notes: This figure shows the dynamic effects on illiquidity for stocks that underwent a reverse split (treated). We examine the composite illiquidity index. The regression includes stock fixed effects and year-month fixed effects. Standard errors are double-clustered at both the firm-level and the year-month level. The confidence intervals of coefficients are plotted at the 95% level.

Figure 10: Dynamic Effects of Reverse Splits on Stock Returns



Notes: This figure overlays the event-study plots constructed using three measures of stock return: raw return, market-adjusted return, and small-cap-adjusted return. Results are based on a dynamic version of the TWFE model following Equation 4. We calculate market-adjusted return as the monthly regression residual of individual share price returns on the market return. The estimation methodology is based on the following model: $R_{i,t} = \alpha_i + \beta_{i,MKT} R_t^{MKT} + \epsilon_{i,t}$, where $R_{i,t}$ is the monthly raw return of the share i at time t and R_t^{MKT} is monthly value-weighted return of all shares in our sample. For small-cap-adjusted return, we replace R_t^{MKT} with R_t^{Small} , which is the monthly value-weighted return of all shares with adjusted price below 1 dollar in our sample. The time variable is month-by-year, and the treatment group variable is given by the month in which the firms reverse-split their shares because of MTP. The figure displays only twelve post-periods, but post-periods beyond twelve are insignificant. The bars represent 95% confidence intervals. Standard errors are clustered at the firm and month-by-year levels.

Figure 11: Insider Trading Around Reverse-Split Announcements (Weekly)



Notes: This figure plots event-time coefficients from Equation 12 with weekly frequency, for each of the six insider-trading outcomes. The sample comprises 138 genuine insider transactions across 35 treated firms, drawn from SGXNet “Changes in the Interests of Directors/CEO” (Form 1) and “Changes in Substantial Shareholdings” (Forms 3 and 5) filings. Buys and Sells are identified from the change in shares held, and share-count changes mechanically induced by the reverse split are excluded. Event-time is measured relative to the reverse-split *announcement* date; the omitted baseline is the event window $[-120, -31]$ trading days. The x -axis reports the bin midpoint in trading days relative to the announcement, and the vertical red dashed line marks the announcement date. Each specification absorbs firm and calendar-year-month fixed effects, with standard errors clustered by firm. The figure plots 95% confidence intervals.

A Internet Appendix

Table A.1: Nominal Stock Prices Across Countries

Country	From	To	Securities	Currency	Mean(Local)	Median(Local)	Mean(USD)	Median(USD)
Argentina	1998	2019	67	Argentine peso	27.12	14.22	2.29	1.21
Australia	1990	2019	1,428	Australian dollar	2.26	0.27	1.78	0.21
Austria	1990	2019	102	Euro	64.98	37.21	77.30	46.16
Bahrain	2014	2019	16	Bahrain dinar	0.42	0.27	1.11	0.72
Bangladesh	2002	2019	235	Bangladesh taka	280.76	57.11	3.85	0.70
Belgium	1990	2019	187	Euro	197.14	59.82	241.33	74.06
Brazil	2000	2019	153	Brazil real	29.04	14.66	10.53	4.15
Bulgaria	2008	2019	134	Bulgaria lev	11.67	3.01	6.88	1.76
Canada	1990	2019	1,348	Canada dollar	11.67	4.35	9.38	3.49
Chile	1999	2019	130	Chile peso	252,459.74	425.53	384.10	0.74
China	2001	2019	690	Chinese yuan	11.78	8.55	1.77	1.30
Colombia	2002	2019	27	Colombian peso	14,466.54	7,892.27	5.74	2.88
Croatia	2006	2019	48	Euro	161.03	42.27	182.15	47.63
Cyprus	2003	2019	56	Euro	0.29	0.12	0.36	0.15
Denmark	1990	2019	207	Danish krone	1,354.74	272.35	211.45	42.78
Egypt	2005	2011	61	Egyptian pound	51.76	30.44	9.24	5.47
Estonia	2006	2019	18	Euro	3.80	1.89	4.82	2.47
Finland	1997	2019	143	Euro	12.38	7.11	14.86	8.94
France	1990	2019	854	Euro	104.33	30.47	122.56	36.86
Germany	1990	2019	596	Euro	40.21	9.99	46.81	12.06
Ghana	2018	2019	17	Ghana cedi	3.74	1.75	0.68	0.32
Greece	1998	2015	281	Euro	4.36	1.36	5.24	1.79
Hong Kong	1990	2019	1,146	Hong Kong dollar	5.16	1.08	0.66	0.14
Hungary	2003	2019	35	Hungary forint	4,331.36	1,071.63	19.68	4.58
India	1994	2019	1,420	Indian rupee	332.56	105.53	5.47	1.85
Indonesia	2000	2019	311	Rupiah	3,452.10	683.52	0.31	0.06
Ireland	1990	2019	57	Euro	4.91	2.51	5.78	3.24
Israel	1996	2019	397	New shekel	55.99	14.08	14.82	3.83
Italy	1990	2019	313	Euro	6.58	3.19	7.74	3.70
Japan	1990	2019	3,670	Yen	4,997.55	779.01	48.07	7.17
Jordan	2015	2019	145	Jordan dinar	1.67	0.91	2.35	1.28
Kenya	2001	2019	46	Kenya shilling	77.09	44.93	0.91	0.55
Kuwait	2013	2019	100	Kuwait dinar	0.26	0.12	0.87	0.40
Latvia	2007	2019	20	Euro	3.77	2.60	4.56	3.23
Lithuania	2008	2019	27	Euro	1.56	0.66	1.93	0.85
Malaysia	1990	2019	916	Ringgit	2.00	1.06	0.56	0.29
Mauritius	2018	2019	43	Mauritius rupee	110.01	39.85	3.12	1.14
Mexico	1998	2019	87	Mexican peso	51.26	30.31	3.38	2.13
Morocco	2003	2019	68	Morocco dirham	849.41	531.89	95.69	59.16

Notes: This table shows the mean and median of nominal stock prices in local currency at the end of each year from the inception of data availability in the Datastream database. It also shows the mean and median of nominal prices in U.S. dollars. To be included in the sample, firms are required to have at least 10 consecutive yearly observations.

Table A.1: Nominal Stock Prices Across Countries (Continued)

Country	From	To	Securities	Currency	Mean(Local)	Median(Local)	Mean(USD)	Median(USD)
Netherlands	1990	2019	195	Euro	143.04	22.33	167.71	27.07
New Zealand	1996	2019	131	New Zealand dollar	2.46	1.54	1.67	1.01
Nigeria	2019	2019	82	Nigeria naira	28.30	4.12	0.09	0.01
Norway	1990	2019	216	Norwegian krone	89.48	55.05	13.00	7.86
Oman	2015	2019	41	Oman rial	0.42	0.21	1.09	0.54
Pakistan	2001	2019	303	Pakistani rupee	189.87	34.92	1.50	0.33
Peru	2001	2019	42	Nuevo sol	10.59	4.15	3.38	1.30
Philippines	1998	2019	212	Philippine peso	49.80	3.08	1.04	0.06
Poland	2001	2019	318	Poland zloty	50.26	6.44	14.63	1.81
Portugal	1998	2019	80	Euro	7.44	4.15	8.03	4.83
Qatar	2014	2019	41	Qatar riyal	31.25	23.00	8.58	6.31
Romania	2005	2019	44	Romania leu	12.50	0.60	3.14	0.19
Russia	2005	2019	145	Russia ruble	1,133.60	37.31	18.87	0.65
Saudi Arabia	2009	2019	126	Saudi riyal	31.23	22.36	8.33	5.96
Singapore	1990	2019	518	Singapore dollar	1.20	0.42	0.84	0.30
Slovenia	2006	2019	32	Euro	67.22	24.10	81.38	28.68
South Africa	1990	2019	413	Rand	35.66	7.85	4.29	1.00
South Korea	1993	2019	892	Won	30,170.48	9,017.00	27.77	8.72
Spain	1996	2019	155	Euro	16.92	10.44	20.08	12.63
Sri Lanka	1998	2019	210	Sri Lanka rupee	145.23	58.51	0.95	0.39
Sweden	1992	2019	371	Krona	72.92	44.38	9.26	5.60
Switzerland	1990	2019	297	Swiss franc	835.43	337.42	684.28	291.54
Taiwan	1997	2019	822	Taiwan dollar	36.69	20.33	1.19	0.66
Thailand	1997	2019	546	Baht	33.16	9.74	0.99	0.28
Tunisia	2015	2019	44	Tunisia dinar	20.72	8.83	8.14	3.43
Turkey	1998	2019	312	Turkish lira	14.68	3.56	15.78	1.96
United Arab Emirates	2014	2019	43	Dirham	2.73	1.67	0.74	0.46
United Kingdom	1989	2019	2,225	British pound	2.65	1.13	4.14	1.80
United States	1989	2019	2,550	US dollar	75.54	26.31	75.54	26.31
Vietnam	2017	2019	151	Vietnam dong	28,430.90	22,191.38	1.23	0.97

Notes: This table shows the mean and median of nominal stock prices in local currency at the end of each year from the inception of data availability in the Datastream database. It also shows the mean and median of nominal prices in U.S. dollars. To be included in the sample, firms are required to have at least 10 consecutive yearly observations.

Table A.2: Pre-Split Characteristics of Early- and Later-Treated Firms

Variable	Early Treated (a)				Later Treated (b)				Diff in Mean (b-a)	T-value
	No of Obs	Mean (a)	Median	Sd Dev	No of Obs	Mean (b)	Median	Sd Dev		
Panel A: Price & Valuation										
Raw Price	63	0.122	0.112	0.081	63	0.135	0.117	0.084	0.013	0.871
Adjusted Price	63	0.093	0.079	0.070	63	0.114	0.108	0.077	0.021	1.578
Market Cap (Millions)	63	78.615	48.360	103.331	63	141.615	67.903	267.776	63.000	1.728*
Ever above S\$0.50	63	0.048	0.000	0.216	63	0.111	0.000	0.317	0.063	1.284
Panel B: Fundamentals										
ROA	45	-0.120	0.020	0.434	49	-0.080	0.022	0.369	0.039	0.467
ROE	46	-0.433	0.017	1.726	50	-0.143	0.011	1.704	0.291	0.821
Leverage	46	6.459	0.363	11.983	50	6.987	0.670	12.073	0.529	0.213
Altman-Z	45	2.637	1.386	8.766	49	4.678	2.975	10.129	2.042	1.036
Dividend Yield	63	0.011	0.000	0.025	63	0.013	0.000	0.024	0.002	0.449
Panel C: Past Performance & Risk										
3-mo Past Return	63	-0.064	-0.104	0.257	63	-0.116	-0.113	0.192	-0.052	-1.270
12-mo Past Return	62	-0.226	-0.235	0.336	62	-0.275	-0.304	0.302	-0.049	-0.853
Raw Return	63	-0.019	-0.018	0.037	63	-0.018	-0.020	0.044	0.001	0.203
Volatility	63	0.043	0.035	0.028	63	0.046	0.035	0.034	0.003	0.518
Skewness	63	0.135	0.168	0.336	63	0.151	0.161	0.355	0.016	0.263
Panel D: Liquidity & Trading										
Bid Ask Spread	63	0.100	0.073	0.089	63	0.104	0.054	0.126	0.004	0.199
Percentage of Zero Trades	63	0.330	0.310	0.246	63	0.268	0.166	0.253	-0.062	-1.380
Turnover	63	0.031	0.006	0.082	63	0.046	0.010	0.095	0.015	0.955
Total Traded Volume (Millions)	63	3.151	0.302	12.712	63	6.382	0.590	20.154	3.231	1.068
Panel E: Retail Trading										
Retail Dollar Volume (Millions)	63	0.472	0.004	2.439	63	0.947	0.032	2.960	0.475	0.974
Retail Buy Volume (Millions)	63	0.236	0.002	1.234	63	0.473	0.017	1.484	0.237	0.968
Retail Sell Volume (Millions)	63	0.236	0.002	1.205	63	0.473	0.015	1.478	0.237	0.979

Notes: This table reports pre-treatment summary statistics and compares early- (a) and later-treated (b) firms, split at the median reverse split effective date, over the twelve months before each firm's reverse split. Variables are grouped into five panels: price and valuation; fundamentals; past performance and risk; liquidity and trading; and retail trading. Market cap and trading volumes are in millions of Singapore dollars; ROA, ROE, leverage, and Altman-Z are computed from the nearest available annual report. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. T-statistics are robust to heteroskedasticity (HC3). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.3: Effects of Reverse Splits on Retail Trading by Split Conversion Rate

	Dollar Trading Volume (1)	Log of Dollar Volume (2)	Number of Trades (3)
M[-5,-1]	83.81 (128.89)	0.03 (0.08)	0.01 (0.01)
M[0,12]	-135.21 (112.50)	-0.10 (0.09)	-0.01 (0.01)
M[13,24]	-213.63 (187.13)	-0.13 (0.13)	-0.01 (0.02)
High Conversion Rate $\times$ M[-5,-1]	-349.72 (225.94)	-0.22* (0.11)	-0.03 (0.02)
High Conversion Rate $\times$ M[0,12]	-456.06** (223.75)	-0.35** (0.16)	-0.04* (0.02)
High Conversion Rate $\times$ M[13,24]	-444.16 (282.24)	-0.29 (0.23)	-0.03 (0.03)
Individual-Stock FE	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes
High Conversion Rate $\times$ Year-Month FE	Yes	Yes	Yes
High Conversion Rate $\times$ Individual FE	Yes	Yes	Yes
Observations	3,261,058	3,261,058	3,261,058
Number of Months	65	65	65
Number of Accounts	37,730	37,730	37,730
Mean of DV	578.09	0.46	0.06
R ²	0.11	0.10	0.09

Notes: This table presents the heterogeneous responses of individual trading activity to the reverse split of treated stocks across split conversion rate levels. Following Equation 1, we regress the outcome variables on a pre-period dummy variable $M[-5, -1]$, post-period dummy variables $M[0, 12]$, $M[13, 24]$, and their interaction terms with dummy variable *High Conversion Rate*, which equals 1 if a treated stock's reverse split conversion rate exceeds the median. The dependent variables of interest are dollar trading volume, the logarithm of one plus dollar trading volume, and number of trades for buy plus sell trades in Columns 1-3, respectively. The Mean of DV represents the mean value of outcome variables during the benchmark period. The sample period is from January 2014 to December 2019. All specifications include individual $\times$ stock fixed effects, year-month fixed effects, conversion rate level $\times$ year-month fixed effects, and conversion rate level $\times$ individual fixed effects. Standard errors are double-clustered at the stock level and the year-month level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.4: Placebo Test: Retail Trading Around Randomly Assigned Reverse-Split Dates

	Dollar Trading Volume			Log of Dollar Volume			Number of Trades		
	Buy (1)	Sell (2)	Buy&Sell (3)	Buy (4)	Sell (5)	Buy&Sell (6)	Buy (7)	Sell (8)	Buy&Sell (9)
M[-5-1]	-71.40 (44.22)	-28.85 (39.74)	-99.75 (83.11)	-0.04 (0.04)	0.01 (0.04)	-0.02 (0.05)	-0.01 (0.01)	-0.00 (0.01)	-0.01 (0.01)
M[0-12]	-61.87 (56.63)	-24.04 (51.66)	-83.90 (108.37)	-0.03 (0.05)	0.01 (0.05)	-0.01 (0.07)	-0.00 (0.01)	0.00 (0.01)	-0.00 (0.01)
Year 2	-127.83 (98.67)	-96.84 (94.00)	-247.50 (194.52)	-0.12 (0.09)	-0.07 (0.08)	-0.14 (0.11)	-0.01 (0.01)	-0.01 (0.01)	-0.02 (0.02)
Individual-Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,274,324	3,274,324	3,274,324	3,274,324	3,274,324	3,274,324	3,274,324	3,274,324	3,274,324
Number of Months	65	65	65	65	65	65	65	65	65
Number of Accounts	37,659	37,659	37,659	37,659	37,659	37,659	37,659	37,659	37,659
Mean of DV	150.25	134.18	322.84	0.19	0.19	0.30	0.02	0.02	0.05
R ²	0.11	0.10	0.13	0.11	0.09	0.11	0.11	0.09	0.12

Notes: This table presents the average response of individual trading activity to pseudo reverse split dates assigned to treated stocks that underwent a reverse split during our sample. We randomly sample a date from the sample period for each stock. Following Equation 1, we regress the outcome variables on a dummy variable $M[-5, -1]$ that equals 1 in months 1 to 5 before the reverse stock split, a dummy variable $M[0, 12]$ that equals 1 if it is 0 to 12 months after, and a dummy variable $M[13, 24]$ that equals 1 in months 13 to 24 after the reverse stock split. The dependent variables of interest are dollar trading volume, the logarithm of one plus dollar trading volume, and number of trades for buy trades, sell trades, and buy plus sell trades in Columns 1-9, respectively. The Mean of DV represents the mean value of outcome variables during the benchmark period. The sample period is from January 2014 to December 2019. All specifications include individual $\times$ stock fixed effects, year-month fixed effects, Treated $\times$ year-month fixed effects, and Treated $\times$ individual fixed effects. Standard errors are double-clustered at the individual level and the year-month level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.5: Migration of Retail Trading Within and Across Industry Sectors

	Same-Sector Migration		Different-Sector Migration	
	% Dollar Volume (1)	% Number of Trades (2)	% Dollar Volume (3)	% Number of Trades (4)
M[-5,-1]	-0.005 (0.010)	-0.006 (0.010)	0.010 (0.011)	0.008 (0.012)
M[0,12]	0.024* (0.013)	0.022* (0.013)	0.015 (0.017)	0.013 (0.017)
M[13,24]	0.007 (0.018)	0.010 (0.018)	0.027 (0.022)	0.021 (0.022)
<i>Coefficient-difference tests for M[0,12]</i>				
Col. (1) – Col. (3)		0.009 (0.027), $p = 0.755$		
Col. (2) – Col. (4)		0.009 (0.027), $p = 0.743$		
Individual FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Observations	40,171	40,171	40,171	40,171
R ²	0.285	0.301	0.313	0.319
Mean of DV	0.064	0.068	0.227	0.241
Number of Months	65	65	65	65
Number of Accounts	3,833	3,833	3,833	3,833

Notes: This table decomposes trading migration into same-sector and different-sector untreated low-priced stocks, following the account-level specification in Table 4. The outcome variable is the ratio of an individual's trading volume (Columns 1 and 3) or number of trades (Columns 2 and 4) in untreated low-priced stocks (price < \$0.20) relative to their total trading volume, excluding the treated stock. Columns 1–2 report migration ratios for untreated low-priced stocks in the same broad sector as the treated stock; Columns 3–4 report ratios for stocks in different broad sectors. Sectors are defined by first cleaning raw sector labels and collapsing them into eight broad groups: Oil & Gas & Utilities, Industrials, Consumer Goods, Consumer Services, Health Care, Telecommunications, Financials, and Technology (results are similar if we use raw sector labels). Firms with missing sectors are not reassigned to any other sectors. The coefficient-difference rows test equality of the M[0,12] coefficients between Columns 1 and 3, and between Columns 2 and 4, by regressing the same-sector minus different-sector outcome on the same event indicators, individual fixed effects, year-month fixed effects, and two-way clustered standard errors. Because the paired equations use the same sample and regressors, this difference-outcome test is equivalent to the corresponding SUR/Wald coefficient-equality test (Zellner, 1962). In odd-numbered columns the dependent variable is the dollar-volume ratio; in even-numbered columns it is the trade-count ratio. The sample covers January 2014 to December 2019. All specifications include individual and year-month fixed effects. Standard errors are double-clustered at the individual and year-month levels.

***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.6: Effects of Reverse Splits on Stock Trading: Alternative Trading Measures

	Dollar Volume (in millions)			Number of Trades (in thousands)		
	Low-priced control (1)	High-priced control (2)	Not-yet treated (3)	Low-priced control (4)	High-priced control (5)	Not-yet treated (6)
M[-5,-1]	-0.14 (0.28)	-0.70* (0.38)	-0.36 (0.25)	-0.01 (0.02)	-0.03 (0.02)	-0.03 (0.02)
M[0,12]	-0.32 (0.46)	-0.76 (0.54)	-0.82** (0.35)	-0.02 (0.03)	-0.04 (0.03)	-0.07** (0.03)
M[13,24]	-0.08 (0.38)	-0.43 (0.52)	-0.56 (0.42)	-0.01 (0.03)	-0.02 (0.03)	-0.06* (0.03)
Treated $\times$ M[-5,-1]	-0.39* (0.23)	0.16 (0.40)		-0.03 (0.02)	-0.00 (0.02)	
Treated $\times$ M[0,12]	-1.19** (0.47)	-0.87 (0.52)		-0.09*** (0.03)	-0.08** (0.03)	
Treated $\times$ M[13,24]	-1.32** (0.54)	-1.09* (0.61)		-0.10*** (0.03)	-0.08** (0.04)	
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,618	5,875	5,406	5,618	5,875	5,406
Number of Months	58	56	65	58	56	65
Number of Stocks	190	194	126	190	194	126
Mean of DV	1.32	2.28	1.05	0.11	0.16	0.08
R ²	0.45	0.70	0.55	0.47	0.69	0.54

Notes: This table presents difference-in-differences estimates of the effect of reverse splits on stock trading activity using alternative dependent variables, dollar trading volume and number of trades for buy plus sell trades. Columns (1) and (4) compare treated stocks against a matched control sample of low-priced stocks ($\leq$ S\$0.20, never treated). Columns (2) and (5) compare treated stocks against a matched control sample of high-priced stocks (above S\$0.50). Columns (3) and (6) report estimates from a not-yet-treated control design. For the matched control columns, we use coarsened exact matching (CEM) to find the best match sample for treated stocks in each price level group, based on the past 3-month returns, average turnover, and zero-trade days based on 1-year data before the MTP introduction. We then assign a pseudo-event date as the matched treated firm's reverse split date. Following Equation 3, we regress the outcome variables on a pre-period dummy variable $M[-5, -1]$, post-period dummy variables $M[0, 12]$, $M[13, 24]$, and their interaction terms with dummy variable $Treated$, which equals 1 if it is a treated stock that underwent an MTP-induced reverse split and zero for matched control stocks. The Mean of DV represents the mean value of outcome variables during the benchmark period. The sample period is from January 2014 to December 2019. All specifications include stock fixed effects and year-month fixed effects. Standard errors are double-clustered at the firm level and the year-month level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.7: Covariate Balance in the Matched Sample

	Reverse Split Group (a)	Control Group (b)	SMD (c)
Number of Firms	98	98	
Return	-0.03 (0.11)	-0.05 (0.10)	0.118
VWAP	0.09 (0.06)	0.09 (0.07)	0.002
Percentage Zero trade	0.29 (0.28)	0.29 (0.29)	0.007

Notes: This table reports the matching accuracy for the one-to-one coarsened exact matching. We report the mean value for each variable as well as its standard deviation in parentheses. All characteristics are measured based on the third month prior to the share reverse-split. A firm is classified into the treatment group if it reverse-splits its shares during the MTP program and into the control group if it is matched to a treated firm but did not undergo a stock reverse split. Variable definitions are provided in Appendix C. SMD is the standardized mean difference. An absolute SMD below 0.2 indicates a small standardized difference; it does not establish statistical insignificance.

Table A.8: Characteristics of Treated and Matched Control Stocks

Variable	Treated Group				Control Group				Diff in Mean (2) - (6)	t-value
	No of Obs (1)	Mean (2)	Median (3)	Std Dev (4)	No of Obs (5)	Mean (6)	Median (7)	Std Dev (8)		
Adjusted Price	98	0.089	0.080	0.059	98	0.091	0.058	0.072	-0.002	-0.165
Volume-weighted Average Price	98	0.002	0.000	0.009	98	0.003	0.000	0.034	-0.002	-0.222
Relative Tick Size	98	0.037	0.014	0.113	98	0.044	0.018	0.109	-0.007	-0.440
Dividend Yield	98	0.010	0.000	0.039	98	0.022	0.000	0.022	-0.013	-0.549
Percentage Zero Trade	98	0.261	0.199	0.249	98	0.296	0.183	0.291	-0.035	-0.895
Market Cap	98	130.016	51.949	338.534	98	79.945	34.553	129.817	50.071	1.367
Turnover	98	0.111	0.024	0.249	98	0.155	0.034	0.464	-0.044	-0.820
Modified Bid-Ask Spread	98	0.010	0.001	0.028	98	0.007	0.001	0.042	0.003	0.554
Composite Illiquidity	98	-0.141	-0.157	0.328	98	-0.139	-0.191	0.421	-0.002	-0.031

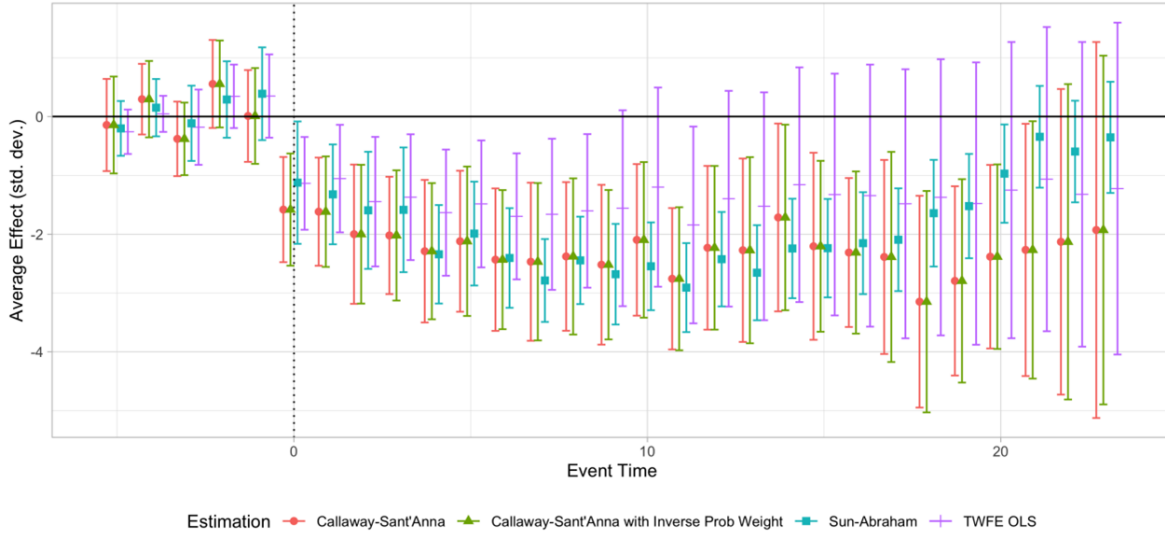
Notes: This table reports summary statistics of the firm characteristics for the matched sample of the share reverse-split (treatment group) and control groups. All characteristics are measured based on months -3 to -1 relative to the stock reverse split. A firm is classified into the treatment group if it conducts a share reverse split during the MTP program and into the control group if it is matched to a treated firm. Variable definitions are provided in Appendix C. Market cap is in millions of Singapore dollars. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.9: Insider Trading Around Reverse-Split Announcements

Dependent Variable:	Pr(Buy) (1)	Pr(Sell) (2)	Pr(Net Sell) (3)	# Buy (4)	# Sell (5)	# Net Sell (6)
Pre [-30,-1]	0.012 (0.013)	0.003 (0.003)	0.004 (0.003)	0.026 (0.016)	0.004 (0.004)	-0.022 (0.017)
Announcement [0,+5]	0.016 (0.016)	0.011 (0.007)	0.014* (0.007)	0.021 (0.020)	0.012 (0.007)	-0.009 (0.024)
Post [+6,+30]	0.011 (0.012)	0.012* (0.006)	0.013** (0.006)	0.014 (0.012)	0.017* (0.008)	0.003 (0.014)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,285	5,285	5,285	5,285	5,285	5,285
Firms	35	35	35	35	35	35
Mean of DV	0.0127	0.0029	0.0022	0.0143	0.0029	-0.0114
R ²	0.037	0.022	0.021	0.029	0.018	0.027

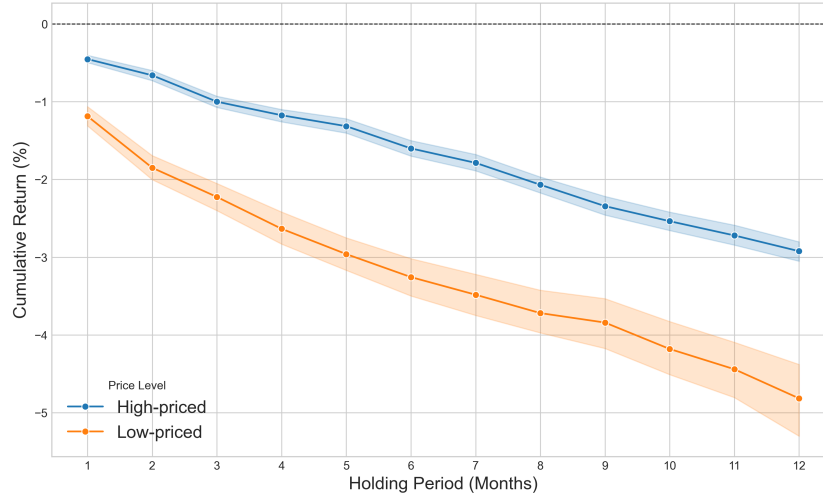
Notes: This table reports estimates of Equation 12 on a balanced firm $\times$ event-day panel of insider and substantial-shareholder trades around each treated firm's reverse-split *announcement* date. The sample comprises 138 genuine insider transactions across 35 treated firms, drawn from SGXNet "Changes in the Interests of Directors/CEO" (Form 1) and "Changes in Substantial Shareholdings" (Forms 3 and 5) filings. Buys and Sells are identified from the change in shares held, and share-count changes mechanically induced by the reverse split are excluded. The dependent variables are, respectively, an indicator that an insider buys (1), sells (2), or is a net seller (3) on that event day, and the corresponding number of buys (4), sells (5), and net sells (6). Pre, *Announcement*, and Post are indicators for the event windows $[-30, -1]$, $[0, +5]$, and $[+6, +30]$ trading days relative to the announcement; the omitted baseline is the far-from-event window $[-120, -31]$. All specifications include firm and calendar-year-month fixed effects. Standard errors, clustered by firm, are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Figure A.1: Dynamic Effects of Reverse Splits on Stock Trading Volume

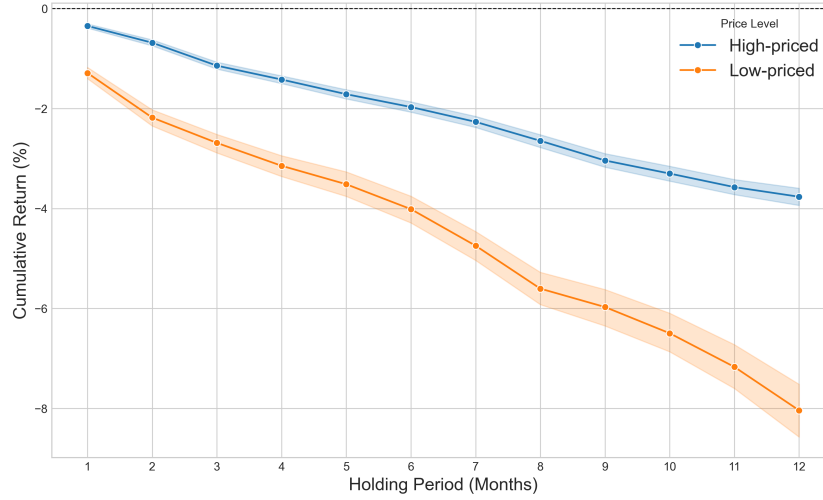


Notes: This figure overlays the event-study plots constructed using four different estimators: a dynamic version of the TWFE model - Equation (4) estimated using OLS (in pink with cross markers), Sun and Abraham (2021) (in blue with square markers), Callaway and Sant'Anna (2021) (in red with circle markers), and Callaway and Sant'Anna (2021) with Inverse Probability Weighting (in green with triangle markers). The outcome variable is the logarithm of one plus total retail trading volume (buy plus sell). The time variable is month-by-year, and the treatment group variable is given by the month in which the firms reverse-split shares due to MTP. The figure displays only twenty-four post-periods because the estimation of additional post-periods would require employing already-treated units as controls for newly treated units. In the presence of heterogeneous dynamic treatment effects, such comparisons would bias the estimation and, therefore, are shut down by all the newly introduced robust estimators. As a result, the maximum number of post-periods that can be estimated robustly is twenty-four. For the Sun and Abraham (2021) estimator, we use the last-treated firm as the control and all periods before the pre-period -5 as the reference period. For the Callaway and Sant'Anna (2021) estimator, we use a varying reference period and try both the doubly robust approach and inverse probability weighting as estimation methods. Results are robust if we require the sample to be a balanced panel. The bars represent 95% confidence intervals. Standard errors are clustered at the firm and month-by-year levels.

Figure A.2: Risk-Adjusted Net Buy Portfolio Returns: Low-Priced vs. High-Priced Stocks



(a) Risk-Adjusted Returns Based on CAPM Model



(b) Risk-Adjusted Returns Based on Fama-French 3-Factor Model

Notes: This figure compares the subsequent 12-month returns of net buy portfolios for low-priced (below S\$0.20) versus high-priced (above S\$1) stocks traded by individual i in month t . The analysis focuses on individuals who traded both stock types during the sample period (2014–2019). We denote the net buy portfolio risk-adjusted return as $R_{i,k,P}^{BmS}$, either based on CAPM model (Panel (a)) or Fama-French three-factor model (Panel (b)), representing the difference between the risk-adjusted return on securities bought ($R_{i,k,P}^B$) and sold ($R_{i,k,P}^S$) by individual i in month t and held for k months ($k = 1 \dots 12$). The portfolio P denotes either Low-priced (L) or High-priced (H) stocks. We calculate $R_{i,k,P}^B$ ($R_{i,k,P}^S$) by taking the dollar trading volume weighted average returns on the securities bought (sold) in month t and held for the next k months. The confidence intervals of cumulative return at each horizon for individuals are plotted at the 95% level.

B MTP Rules in Other Markets and Data Construction

B.1 MTP Rules

Table [B.1](#) lists the nine continued-listing regimes, in eight countries, that condition on an absolute nominal per-share price. We searched the continued-listing rulebooks of the major exchanges and excluded rules keyed to par or book value, aggregate market capitalization, or price relative to the IPO price, as well as admission-only thresholds and daily price limits. Three other price-based rules do not qualify. The rule on Japan's JASDAQ/Hercules market combines a price threshold with net-deficit conditions, and Korea's pre-2026 KOSPI rule sets the threshold relative to par value; the Korea entry in the table is the KRW 1,000 rule adopted in 2026. Hong Kong proposed delisting stocks trading below HK\$0.50 in July 2002 but withdrew the proposal within days after the affected stocks fell sharply, so no rule was adopted. Greece's EUR 0.30 rule drops out of the sample rather than the search: no Greek firm-month in our data trades below the threshold, so Greece fails the thirty-observation screen described below.

Table B.1: MTP Rules Across the Nine Markets

Market	Rule mechanics	Event date evaluated	Remedy	Announcement sample
US Nasdaq	Bid < \$1.00 for 30 days	9 Apr 1997	Public-float/capital alternative until 1997; price recovery or reverse split thereafter	No. 1997 amendment revised other maintenance standards for all issuers
US NYSE	Average close < \$1.00 over 30 days	26 Jul 1999	Price recovery or reverse split within 6 months	No. Fewer than 50 exposed firms
Poland GPW	Three-month average < PLN 0.50	2 Dec 2013	Price recovery or reverse split	Yes. Alert List also changes the trading protocol
Brazil B3	Close < R\$1.00 for 30 sessions	14 Feb 2014	Price recovery or reverse split within 6 months	No. Fewer than 50 exposed firms
Singapore SGX	Six-month VWAP < S\$0.20	2 Mar 2015	Price recovery or reverse split within 36 months	Yes
China SSE/SZSE	Close < RMB 1.00 for 20 days	31 Dec 2020	None; automatic delisting	No. Fewer than 50 exposed firms; no consolidation remedy
Indonesia IDX	Six-month average < Rp 51, one of 11 criteria	5 Jun 2023	Price recovery only	No. No consolidation remedy
Saudi Tadawul	Average close < SAR 1.00 for 15 days	30 Apr 2025	Reverse split to at least SAR 3.00 within 3 months	No. Two exposed firms
Korea KRX	Close < KRW 1,000 for 30 days	13 May 2026	Price recovery or reverse split within 90 days	Yes. Market-capitalization threshold raised at the same time; see below

Notes: Rows are ordered by event date. Each market name links to the official regulator or exchange source. The event date is the policy announcement evaluated for the announcement test; for Nasdaq it is the 1997 amendment that removed the public-float and capital alternative, and for China it is the 2020 rule that replaced the par-value trigger in place since 2012. Failure to comply leads to delisting, except in Poland, where exposed stocks move to the Alert List and single-price auction, and Indonesia, where they move to a call-auction monitoring board; Singapore also allows a transfer to Catalyst, and Korea caps consolidation ratios. The final column states whether the market enters the announcement analysis in Table 14 under the screens described in this appendix. The SGX rule is shown in bold.

B.2 Why the Remaining Markets Do Not Enter the Announcement Analysis

The announcement analysis in Section 5.3.3 requires (i) more than 50 exposed firms, so that portfolio returns are not driven by firm-specific news; (ii) share consolidation as a remedy, so that the rule forces a nominal-price response rather than only delisting risk; and (iii) a discrete announcement date that defines a common event window. Where the announcement bundles other changes, we require that the bundled component be separable at the firm level or we treat the market as a benchmark rather than a replication of the Singapore design.

Screen (i) removes the NYSE, Brazil, Saudi Arabia, and China, each with fewer than 50 exposed firms in our pre-announcement data (two in Saudi Arabia). Screen (ii) removes Indonesia and, again, China: neither offers a consolidation remedy, and Indonesia’s price test is one of eleven criteria for a call-auction monitoring board.

Nasdaq has enough exposed firms but no usable event. Its original rule, proposed by the NASD on 9 April 1990 and effective in September 1991 (Rhee and Wu, 2012), let issuers satisfy the standard through a public-float and capital alternative, so it did not require a nominal-price response and fails screen (ii). The 1997 amendment removed the alternative but formed part of a broader revision of Nasdaq’s maintenance standards that applied to all issuers (U.S. Securities and Exchange Commission, 1997), so its effect cannot be separated from the price-floor effect at the firm level.

Poland and Korea pass all three screens but bundle other changes with the price rule. Poland’s Alert List also moves exposed stocks to single-price auction and removes them from index eligibility, so the Polish reaction combines delisting risk with a change in trading protocol. Korea raised the KOSPI market-capitalization threshold from KRW 20 billion to KRW 30 billion at the same time; we therefore retain only Korean stocks whose market capitalization is observed on at least ten days in $[-30, -1]$ and never falls below KRW 30 billion, so that the retained stocks are exposed to the price rule alone. The Poland and Korea results in Table 14 are accordingly external-validity benchmarks rather than replications of the Singapore design.

B.3 Data Construction

Ownership. Refinitiv does not report household holdings directly, so we proxy retail ownership by the share not held by identifiable non-retail categories,

$$Retail_{i,t} = 100 - Institutional_{i,t} - Strategic_{i,t} - Government_{i,t}, \quad (13)$$

setting missing strategic or government ownership to zero, treating retail ownership as missing when institutional ownership is unavailable, and clipping the measure to $[0, 100]$. Refinitiv’s strategic category includes insider holdings, so no separate insider term is subtracted. The ownership panel is survivorship-corrected and retains

delisted issues. After removing placeholder observations, months after an issue's retirement, and observations with internally inconsistent ownership shares, the merged ownership and price data contain 1,120,347 firm-months for 19,919 firms. A market enters the ownership comparison only if it has at least thirty firm-months on each side of its floor, which leaves nine markets, 585,206 firm-months, and 12,212 firms.

Prices and returns. Daily prices, volume, and adjustment factors come from CRSP-linked Compustat for the two US markets and from Compustat Global elsewhere. Daily returns follow the Compustat convention,

$$r_{i,d} = \frac{(P_{i,d} / A_{i,d}) F_{i,d}}{(P_{i,d-1} / A_{i,d-1}) F_{i,d-1}} - 1,$$

where P is the closing price, A the cumulative adjustment factor, and F the daily total return factor. Floor screens use unadjusted prices, since split-adjusted series would remove the nominal-price variation of interest.

Announcement sample. A stock is exposed if its closing price fell below the local floor at some point in the sixty trading days before the announcement, with at least twenty daily observations in that window. Adjacent bands contain stocks whose price is between one and two times the floor, $[1, 2)$, and between two and three times the floor, $[2, 3)$, over the same window. Table 14 contains 510 exposed stocks (118 in Poland, 232 in Singapore, and 160 in Korea), 413 stocks in the $[1, 2)$ band, and 280 in the $[2, 3)$ band, after the Korean market-capitalization screen described above. The value-weighted market return in the market model weights each firm by its prior-month market capitalization, which is predetermined relative to the event window.

For the retail-ownership heterogeneity test in Panel B, we average retail ownership over months $[-12, -1]$ and require at least one nonmissing observation. This leaves 156 exposed stocks with usable ownership (12 in Poland, 56 in Singapore, and 88 in Korea), sorted into retail-ownership terciles within each market, with the security identifier breaking exact ties. Ownership coverage differs across markets and is not random, which is why we interpret Panel B as suggestive.

C Variable Definitions

Table C.1: Variable Definitions

Variable	Definition / Description
Panel A: Measures of Retail Trading (Monthly)	
Dollar Trading Volume	The total monetary value of all buy and sell transactions, calculated for specific stocks, or aggregated at the individual investor/individual-stock level.
Number of Trades	The total count of all buy and sell transactions, calculated for specific stocks, aggregated at the individual investor/individual-stock level.
Trading Migration Volume	The ratio of an individual's trading volume in untreated low-priced firms (firms with prices below 20 cents that did not undergo a reverse split) to their total trading volume, excluding the treated stock.
Trading Migration Number of Trades	The ratio of an individual's total count of trades in untreated low-priced firms (firms with prices below 20 cents that did not undergo a reverse split) to their total number of trades, excluding the treated stock.
Retail Trading Intensity (RTI)	Retail trading intensity of stock i in month t , defined as retail turnover, $\frac{Retail_Buy_{i,t} + Retail_Sell_{i,t}}{Market\ Cap_{i,t}}$, where the numerator is the sum of retail buy and sell dollar volume. It is standardized to mean zero and unit standard deviation.
Panel B: Primary Measures of Stock Prices and Stock Returns (Monthly)	
Raw Price	Raw price is the closing price at the end of each month.
Adjusted Price	Adjusted price is the closing price at the end of each month, adjusted for stock splits and dividends.
Volume-weighted Average Price (VWAP)	Volume-weighted average price is the monthly total trading volume divided by the monthly total traded shares.
Raw Returns	Raw return is the adjusted closing price at the end of each month divided by the adjusted closing price at the end of the previous month, minus one.
Market-adjusted Returns	We calculate market-adjusted returns as the monthly regression residual of individual stock returns on the market return. The estimation methodology is based on the CAPM model: $R_{i,t} = \alpha_i + \beta_{MRKT} R_t^{MRKT} + \varepsilon_{i,t}$, where $R_{i,t}$ is the monthly raw return of the stock i at time t and R_t^{MRKT} is the monthly value-weighted return of all stocks in our sample.
Small-cap-adjusted Returns	We calculate small-cap-adjusted return as the monthly regression residual of individual stock returns on the small-cap portfolio return. The estimation methodology is based on the following model: $R_{i,t} = \alpha_i + \beta_{Small} R_t^{Small} + \varepsilon_{i,t}$, where $R_{i,t}$ is the monthly raw return of the stock i at time t and R_t^{Small} is the monthly value-weighted return of all stocks with adjusted price below \$51 in our sample.

Table C.1: Variable Definitions (Continued)

Variable	Definition / Description
Panel C: Liquidity Measures (Monthly)	
Percentage of zero trades	Percentage of zero trades is the number of zero-trading days divided by the total number of trading days.
Turnover	Turnover is the stock's monthly dollar trading volume divided by its market capitalization.
Bid-ask spread	Bid-ask spread is the monthly average of daily bid-ask spreads, which is the ask price minus the bid price, divided by the closing price.
Composite Illiquidity	Composite illiquidity is a monthly composite liquidity measure calculated as the average of the individual z-scores of the following three liquidity measures: zero-trade percentage, negative turnover, and bid-ask spread. Each month for each stock, each of the three liquidity measures is first converted into its rank and then standardized to obtain its z-score: $z = (r - \mu_r) / \sigma_r$, where r is the rank of this measure, and μ_r and σ_r are the cross-sectional mean and standard deviation of r . The composite z-score is the average of these z-scores. We require all three non-missing z-scores to compute this measure.
Panel D: Price Delay Measures (Weekly)	
Price Delay (D1)	Following Hou & Moskowitz (2005), D1 is a rolling measure of price delay calculated using past weekly returns over a 26-week window. It captures the fraction of return variation explained by lagged market returns. It is defined as: $D1 = 1 - \frac{R^2_{\text{restricted}}}{R^2_{\text{unrestricted}}}$, where $R^2_{\text{restricted}}$ is derived from the baseline restricted market model, $r_{j,t} = \alpha_j + \beta_j R_{m,t} + \varepsilon_{j,t}$, and $R^2_{\text{unrestricted}}$ is derived from an unrestricted model that includes four weeks of lagged market returns: $r_{j,t} = \alpha_j + \beta_j R_{m,t} + \sum_{n=1}^4 \delta_j^{(-n)} R_{m,t-n} + \varepsilon_{j,t}$.
Price Delay (D2)	Following Hou & Moskowitz (2005), D2 is a rolling measure of price delay calculated using past weekly returns over a 26-week window and represents the weighted average of the absolute lagged coefficients in the above unrestricted model: $D2 = \frac{\sum_{n=1}^4 n \delta_j^{(-n)} }{ \beta_j + \sum_{n=1}^4 \delta_j^{(-n)} }$.
Price Delay (D3)	D3 is a rolling measure of price delay calculated using past weekly returns over a 26-week window, while adjusting for estimation noise by precision-weighting these coefficients using their standard errors in the above unrestricted model: $D3 = \frac{\sum_{n=1}^4 n \frac{ \delta_j^{(-n)} }{se(\delta_j^{(-n)})}}{\frac{ \beta_j }{se(\beta_j)} + \sum_{n=1}^4 \frac{ \delta_j^{(-n)} }{se(\delta_j^{(-n)})}}$.

Table C.1: Variable Definitions (Continued)

Panel E: Other Firm Characteristics	
Variable	Definition / Description
Dividend Yield	Dividend yield is an annual measure. It's defined as the yearly dividend amount divided by the closing price at the beginning of the same year.
ROA	ROA is the annual return on assets.
ROE	ROE is the annual return on equity.
Leverage	Leverage is long-term debt as a percentage of total assets, from the nearest available annual report prior to the reverse split.
Altman-Z	A distress-risk index following Altman (1968), in the emerging-market variant $Z = 3.25 + 6.56 X_1 + 3.26 X_2 + 6.72 X_3 + 1.05 X_4$. Because working capital is not consistently available for our sample, we report the three-term index $3.26 X_2 + 6.72 X_3 + 1.05 X_4$, where X_2 is retained earnings over total assets, X_3 is EBIT over total assets, and X_4 is book equity over total liabilities. Higher values indicate lower distress risk.
Market Cap	Market cap is the monthly market value of a firm in millions. It's defined as the total shares outstanding times the closing price at the end of the same month.
Total Trading Volume	Total monthly dollar trading volume of the stocks, including both retail and institutional investors.
3-mo Past Return	The stock's cumulative total return over the three months preceding its reverse split, computed as the adjusted closing price on the last trading day before the split divided by the adjusted closing price on the first trading day of the three-month window, minus one.
12-mo Past Return	The stock's cumulative total return over the twelve months preceding its reverse split, computed analogously to the 3-month past return. Firms without a full twelve months of pre-split price history are excluded from this measure.
Ever above S\$0.50	An indicator equal to one if the stock's raw closing price ever reached or exceeded S\$0.50 at any point in its available price history before the reverse split, and zero otherwise.
Volatility	Annual volatility is measured as the standard deviation, computed over the calendar year, of 10-day cumulative returns. To be consistent with skewness, volatility is coded as missing if 10-day returns are exclusively zero.
Skewness	Annual skewness is measured as the third moment, computed over the calendar year, of 10-day cumulative returns.