The Economic Consequences of Lower Retail Trading Costs[*]

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Abstract

The economic benefits of lowering trading costs depend not only on the direct savings but also on how investors respond. This paper studies how cheaper retail trading affects individuals and the market through increased speculation. We exploit a 2017 reform in Taiwan that halved the transaction tax specifically for day trading. Using account-level transaction data, we find that investors' responses cost more than the tax cut saves them. Trading increases, but the additional trades come disproportionately from less sophisticated investors, whose trades lose the most. Moreover, day traders' gross returns per trade decline, suggesting that trade quality deteriorates. In the end, the tax cut reduces day traders' net portfolio returns, especially for the least sophisticated investors. To parsimoniously explain these responses, we develop a model of trading with costly attention that illustrates the disciplinary role of transaction costs. At the market level, however, the increased retail speculation improves intraday liquidity and lowers volatility. Cheaper retail trading therefore involves a trade-off: it can benefit markets while harming individual investors.

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1 Introduction

Trading costs for retail investors—most notably commission fees—have fallen substantially over the past several decades, and many brokers around the world now offer zero-commission trading.[1] Existing research shows that retail investors trade frequently but often incur losses, mostly because their trades do not earn enough to cover transaction costs (Barber and Odean 2000). They would therefore appear to benefit from lower costs. As former Charles Schwab CEO Walt Bettinger put it, "the ultimate winners in our decision to eliminate commissions were investors."[2] This reasoning, however, holds investor behavior fixed.

When trading becomes cheaper, investors may trade more, but it is unclear ex ante which investors will respond most strongly. This matters because not all investors trade equally well, and some lose money even before transaction costs (Barber et al. 2014; Coval, Hirshleifer, and Shumway 2021; Jones et al. 2025). If the additional trading comes disproportionately from these investors, the resulting losses may offset, or even exceed, the direct savings from lower costs. Moreover, for a given investor, there is no guarantee that the marginal trade will perform as well as the trades that came before. Whether investors ultimately benefit from lower costs is therefore an empirical question.

Answering this question has important implications. Regulators have long been concerned about retail speculation, and the rise of zero-commission trading has brought such concern back to the fore (Cook 2021; ESMA 2021; Gensler 2021). Policy tools aimed at retail speculation typically involve changing the cost of trading, whether by taxing transactions or by regulating how brokers charge for them. Yet little is known about how retail speculation responds when its cost changes, whether the response leaves investors better or worse off financially, and how it affects the market. Answering these questions causally requires an exogenous change in trading costs. The change must also apply specifically to speculative trades since trades made for different reasons need not react to costs in the same way. Such changes, however, are rare. In practice, commissions and transaction taxes may be adjusted in response to market conditions and for all trades at once.

In this paper, we overcome this challenge by exploiting a 2017 reform in Taiwan that halved the transaction tax on day trading from 30 to 15 basis points while leaving the tax on other trades unchanged. Day trading, defined as the purchase and sale of a stock within a day, is textbook speculation and accounts for a substantial share of retail trading volume worldwide.[3] The reform therefore provides within-market variation in the cost of a key form of retail speculation. This

1. Examples include Australia (Morningstar 2020), Canada (Bloomberg 2021), the US (CNBC 2019), and the UK (TechCrunch 2018).

2. Charles Schwab Corporation, 2019 Annual Report.

3. Day trading accounts for 43% of retail trading volume in Korea (Lee, Park, and Jang 2007; Chung, Choe, and Kho 2009), 64% in Finland (Linnainmaa 2005), 50% in Brazil (Chague, Silveira Bueno, and Giovannetti 2019; Chague et al. 2024; Chague, Giovannetti, and Paiva 2024), and 22% in Taiwan (Barber et al. 2009, 2014; Barber et al. 2020).

variation allows us to examine how retail speculators respond to lower costs while holding market conditions constant, and how those responses affect their own returns and market quality.

Throughout the paper, we use detailed account-level transaction data from one of the largest brokerage firms in Taiwan. The dataset tracks the trading activity of a large sample of individual investors, whose trading represents 4.5% of aggregate market volume. We observe details of the orders people place, for example, at what price and when they transact. The behavior of investors in our sample is representative of overall retail trading in Taiwan. Their demographic and account characteristics are also similar to those documented for retail investors in other markets (e.g., Barber and Odean 2000; Dorn and Huberman 2005), which supports the external validity of our findings.

To identify the effect of the reform on investor behavior, we use a difference-in-differences design that compares investors who regularly day trade with investors who share similar speculative motives but rarely do so. Since day trading is a persistent behavior, we assign investors to treatment and control groups based on their activity during a classification period, the year before our analysis period begins. The treatment group consists of day traders, defined as investors who executed at least one day trade during the classification period. The control group consists of active non-day traders, defined as investors who traded on at least 30 days but never day traded during the same period. We find that non-day traders continue to avoid day trading after the reform, yet the trading activity of the two groups is highly correlated. This suggests that they trade for similar reasons and respond comparably to changes in market conditions. The two groups also have similar observable characteristics and exhibit parallel pre-trends across a range of outcomes. Therefore, the behavior of non-day traders can serve as a counterfactual for that of day traders in the absence of the reform.

We start by examining the impact of the tax reform on day traders' behavior, focusing on changes in trading volume and per-trade performance. Together, these responses determine whether the reform leaves day traders better or worse off financially. Because we classify investors before the reform, our results speak to investors who were already day trading. This is precisely the group for which the financial consequences of a lower transaction cost are least clear. A tax cut raises the net return on the trades they would have made in any case, while also potentially inducing trades that need not be profitable.

We first document that day trading volume increases by 32% following the reform, while non-day trading volume remains virtually unchanged. This pattern suggests the increase is a response to the tax cut itself rather than to broader changes in trading conditions. Crucially, the effect of this increase on day traders' profits depends on who makes the additional trades. If the marginal trades are made by investors who tend to lose money, more trading would mean larger losses. We therefore examine how the response varies with investor sophistication, using wealth as a proxy. We find that the increase in day trading is concentrated among investors with lower wealth, the group that incurs the largest losses per trade on average. Standard explanations of excessive trading do not obviously predict this pattern. Investors with the lowest wealth already had the highest turnover before the reform. If their high turnover reflects overconfidence, a preference for gambling, or inattention to costs, a given reduction in costs should matter less to them, not more. We later propose a mechanism based on investor attention that accounts for this pattern.

We next show that day traders not only trade more but also trade worse. Their risk-adjusted gross returns per dollar traded fall by 5.5 basis points after the tax cut. Consistent with the tax cut driving this decline, the effect appears only in day trades but not in the non-day trades of the same investors. The economic magnitude is quite large. A 5.5-basis-point decline per day corresponds to roughly a 14-percentage-point reduction in returns at an annual frequency. This is striking because the stock market is generally efficient, and systematically making worse trades should be as difficult as systematically making better ones. Yet, these day traders managed to do so. We find that the same attention mechanism can also explain the performance deterioration, and trace the decline in per-trade returns to a decline in the attention that day traders devote to trading.

Whether the reform raises or lowers day traders' net portfolio returns is not obvious from these two results alone. The tax cut mechanically increases traders' net return per trade by 15 basis points, and this gain could outweigh the losses from additional trading and worse performance. We find, however, that the average day trader's annual net portfolio return falls by 2.1 percentage points after the reform. Moreover, these losses are unevenly distributed. Investors with the lowest wealth suffer the largest declines in portfolio returns. It is therefore how investors respond, not merely the size of the tax cut, that ultimately determines how much each investor gains or loses.

Given the importance of investor responses, we next explore why investors respond as they do. We propose a mechanism that formalizes a widespread regulatory concern: transaction costs serve a disciplinary role, and removing them creates an illusion of free trading that invites careless and inattentive behavior (Cook 2021; ESMA 2021; Gensler 2021).

We first document evidence consistent with this concern. Models of attention allocation (Gabaix 2019; Bastianello, Decaire, and Guenzel 2024) predict that when decision makers reduce cognitive effort, they disproportionately reduce the weight they place on low-salience information. If the tax cut lowers the effort investors bring to their trades, behaviors associated with limited attention should become more common. Consistent with this prediction, we show that three such behaviors, each previously shown to hurt performance, rise after the tax cut. Day traders rely more on cognitive shortcuts, monitor their limit orders less, and engage more frequently in salience-driven trading. These changes in behavior contribute to the observed decline in per-trade performance.

We then present a model of trading with costly attention that parsimoniously explains investors' responses to the tax cut. In the model, traders believe that the attention they pay affects the return they earn (Duraj et al. 2025). Because attention is costly, they trade off its benefit and cost (Sims 2003; Maćkowiak, Matějka, and Wiederholt 2023; Gabaix 2019). The model makes three predictions that match the observed investor behavior and responses to lower trading costs. First, since the benefit of attention increases with position size, low-wealth investors pay less attention and trade more in general, as they do before the reform. Second, paying less attention makes low-wealth investors' trading more sensitive to trading costs, which explains why the increase in day trading post-reform is concentrated among them. Finally, because lowering trading costs raises the expected return on every trade, it weakens the incentive to pay attention. This explains why trades get worse. The model connects the investor responses we documented through a single mechanism and highlights a disciplinary role of transaction costs.

Beyond investor welfare, increased retail speculation may have consequences for the market. Although retail investors are often characterized as noise traders (Barber, Odean, and Zhu 2009; Foucault, Sraer, and Thesmar 2011), the literature offers competing views on their market impact. On one side, retail trading may increase volatility by impounding incorrect beliefs into prices (Black 1986; De Long et al. 1990a; Campbell and Kyle 1993; Llorente et al. 2002) and may harm liquidity by raising market makers' inventory risk (Ho and Stoll 1981; Grossman and Miller 1988; Hendershott and Menkveld 2014). On the other side, retail investors can provide liquidity either directly (Kaniel, Saar, and Titman 2008; Barrot, Kaniel, and Sraer 2016) or indirectly (Kyle 1985; Glosten and Milgrom 1985), thereby reducing price impact and volatility (Ross 1989; Schwert and Seguin 1993; Song and Zhang 2005).

Prior studies of retail investors' market impact typically rely on variation other than transaction costs (Foucault, Sraer, and Thesmar 2011; Eaton et al. 2022). Since different sources of variation affect different traders, which channel dominates when the cost of retail speculation falls remains an open question. This matters for policies that operate through changing trading costs.

To answer this question, we exploit an institutional feature of the Taiwanese market: a subset of stocks was ineligible for day trading and therefore unaffected by the reform. These stocks provide a natural control group. Using a matched difference-in-differences design, we find that the surge in day trading, despite being driven mainly by less sophisticated investors, improves market quality. Following the reform, intraday liquidity increases, as measured by order book depth and quoted spreads, and intraday volatility decreases, as measured by the standard deviation of ten-minute price changes. To shed light on the mechanism, we show that day traders as a group exhibit contrarian behavior, consistent with liquidity provision. Together, these results suggest that when the cost of retail speculation falls, the liquidity-providing effect of the additional trading dominates its destabilizing effect. Moreover, they call into question the effectiveness of financial transaction taxes as a tool to curb short-term volatility, even when directly imposed on noise trading.

Overall, our paper shows that the consequences of a change in retail trading costs depend on how investors respond to it. Lower costs mechanically raise the net return on the trades investors would have made anyway. But they also induce additional trades from investors who perform poorly, and they reduce the quality of the trades each investor makes. In the end, a tax cut leaves investors worse off financially, especially the least wealthy. We propose an explanation reflecting the disciplinary role of transaction costs. At the market level, however, increased retail speculation improves liquidity and lowers volatility. Taken together, our results highlight a trade-off for policies affecting retail trading costs. These range from the design of transaction taxes to restrictions on payment for order flow, which may limit brokers' ability to offer low-fee trading.

Related literature. This paper contributes to the extensive literature on behavioral biases of retail investors. Prior research documents that retail investors exhibit a range of biases that contribute to excessive trading and poor performance. These include overconfidence (Odean 1998b; Barber and Odean 2001; Dorn and Huberman 2005; Glaser and Weber 2007; Graham, Harvey, and Huang 2009; Liu et al. 2022), the disposition effect (Odean 1998a; Grinblatt and Keloharju 2001; Barber et al. 2007; Calvet, Campbell, and Sodini 2009; Jin and Peng 2023), sensation seeking (Dorn and Sengmueller 2009; Grinblatt and Keloharju 2009; Gao and Lin 2015), and attention-driven trading (Seasholes and Wu 2007; Barber and Odean 2008; Engelberg and Parsons 2011; Barber et al. 2022). In this literature, transaction costs are taken as given and generally viewed as a major source of retail investors' trading losses because their trading gains, if any, are insufficient to cover these costs. We contribute to this literature by looking at what happens when transaction costs change. We show that lowering trading costs can exacerbate biases related to limited attention. Moreover, retail investors, especially the least sophisticated ones, exhibit excessive sensitivity to reductions in these costs and increase their trading activity to their own detriment. We develop a model with costly attention that jointly explains these behaviors. More generally, our paper complements Heimer and Simsek (2019) and Heimer and Imas (2022), who study leverage constraints, and supports the idea that reducing trading friction can in fact harm retail investors.

Our paper also contributes to the literature on financial transaction taxes (FTTs). Proponents of FTTs argue that transaction taxes can disproportionately discourage noise trading, thereby improving market quality and reducing volatility (Tobin 1978, 1984; Stiglitz 1989; Summers and Summers 1989). In contrast, opponents argue that transaction taxes can indiscriminately affect both noise trading and fundamental trading. Therefore, any potential benefits from reducing noise trading may be offset by a decrease in fundamental trading (Grundfest and Shoven 1991; Kupiec 1996; Song and Zhang 2005). Moreover, noise trading provides liquidity benefits, and reducing it may, instead, worsen market quality (Ross 1989; Schwert and Seguin 1993). Mirroring the theoretical debate, empirical evidence on the market impact of transaction taxes—particularly with respect to volatility—is mixed. Several studies find that higher transaction taxes reduce volatility (Liu and Zhu 2009; Deng, Liu, and Wei 2018), while others find that they increase volatility (Umlauf 1993; Jones and Seguin 1997) or have no significant effect (Capelle-Blancard and Havrylchyk 2016;

Gomber, Haferkorn, and Zimmermann 2016; Hvozdyk and Rustanov 2016; Coelho 2016; Colliard and Hoffmann 2017). Because existing studies examine transaction cost changes that apply to all market participants, it is difficult to disentangle the source of these mixed results.[4]

Our contribution to this literature is threefold. First, we isolate the market impact of retail day traders, who are often considered noise traders, and show that their increased participation in fact improves market quality. Second, our use of individual-level data allows us to shed light on the mechanisms behind the observed market effects. Third, we document both the individual-level effects and the market-wide consequences of reduced transaction costs, which together show the trade-offs policymakers face when designing FTTs.

This paper also connects to the growing literature on retail investors in the zero-commission era. Several recent studies document their behavior in equity markets (Fedyk 2021; Barber et al. 2022; Welch 2022) and option markets (Bryzgalova, Pavlova, and Sikorskaya 2023; de Silva, Smith, and So 2023), and explore their broader market impact (Eaton et al. 2022; Ozik, Sadka, and Shen 2021; Glossner et al. 2025). While most papers take zero-commission trading as given, we contribute to this literature by providing causal evidence on how lowering transaction costs affects retail investors and market outcomes. Alongside Kalda et al. (2021), Barber et al. (2022), and Chapkovski, Khapko, and Zoican (2024), who study the effects of smartphone usage, user interface design, and gamification, respectively, on retail trading behavior, our work helps disentangle how different features of zero-commission platforms affect retail investors. Our findings also have implications for ongoing debates over payment for order flow (PFOF) regulation, which could influence brokers' ability to offer commission-free trading to retail investors (Reuters 2024).

Our paper complements Even-Tov et al. (2022), who examine the effect of a fee removal on investors' non-leveraged equity trades on an online brokerage platform. Unlike their analysis, our setting allows us to document both the individual-level financial impact and the market-level effects of lower retail trading costs. Moreover, our focus on day traders allows us to precisely measure changes in performance at the individual trade level, because day traders trade frequently and have a fixed investment horizon.

Outline. The rest of the paper proceeds as follows. Section 2 reviews institutional details of Taiwan's stock market, the transaction tax reform, and our data. Section 3 examines how lower transaction costs affect trading behavior. Section 4 explores the financial impact of the tax reform

4. For instance, differing results may stem from variation in market composition. Transaction taxes might reduce volatility only in markets with a higher share of noise trading. Alternatively, the effects could reflect differences in the relative sensitivity of informed and noise traders to transaction costs. In some markets, noise traders may be more sensitive, so imposing an FTT reduces their activity and lowers volatility. Finally, the ambiguity may arise from the dual role of noise trading itself: while destabilizing, it also contributes to liquidity. Understanding the source of heterogeneity has important policy implications. For instance, if mixed findings are primarily due to compositional differences, then targeted transaction taxes on short-term speculators may be more effective in improving market quality.

on investors. Section 5 proposes a mechanism based on costly attention and presents supporting evidence. Section 6 analyzes market-level effects of the tax reform. Section 7 concludes.

2 Background and Data

In this section, we first provide background on Taiwan's stock market and day trading (Section 2.1). Then, we discuss the natural experiment studied in this paper: the 2017 securities transaction tax reform (Section 2.2). Finally, we describe our data (Section 2.3).

2.1 Taiwan Stock Market, Day Trading and Transaction Costs

In 2017, Taiwan's stock market had approximately $1 trillion USD in market capitalization and 1,639 listed stocks, with an average daily dollar volume per stock of $2 million. Stocks trade on either the Taiwan Stock Exchange (TWSE) or the Taipei Exchange (TPEx), both of which operate consolidated limit order books that accept only limit orders. Regular trading sessions run from 9:00 a.m. to 1:30 p.m., during which orders are matched via a call-auction mechanism every 5 seconds. Although market orders are not permitted, traders can effectively achieve immediate execution by submitting aggressively priced limit orders.

Day traders are an important group of investors in Taiwan's stock market. Day trading—the purchase and sale of the same stock within a single trading day—accounts for approximately 15% of total daily trading volume. Importantly, retail investors account for 92% of day trading volume. Table F.1 summarizes the composition of day trading by investor type according to the Taiwan Ministry of Finance (2018). Day trading appeals to many retail investors because it offers high leverage and has essentially no margin requirements.[5]

The dominance of day trading by retail investors reflects a distinct feature of Taiwan's market: the absence of institutional market makers. This is also evident in the composition of aggregate trading volume. As Figure F.1 shows, securities dealers—who would typically serve as market makers—account for only 5% of total trading volume, while retail investors constitute nearly 60%. Given these features that distinguish Taiwan from some developed markets, we discuss their implications for the external validity of our findings in Section 6.

Despite its popularity among retail investors, day trading incurs substantial transaction costs in Taiwan. The government imposes a 0.3% (30 basis points) transaction tax on sales of all common stocks, regardless of holding period or investor type. Brokerage firms also charge commissions on both purchases and sales. Since we do not directly observe the discounts that brokers offer on commissions, we assume that traders pay a commission of 10 basis points per round trip, following

5. For example, a day trader who buys 1,000 shares at $10 and sells them at $11 on the same day earns $1,000 before transaction costs, without needing the full $10,000 in initial capital since positions are netted at the end of the day.

Barber et al. (2014), resulting in total round-trip transaction costs of 40 basis points. These high costs largely explain why most retail day traders in Taiwan consistently lose money (Barber et al. 2014). Notably, Taiwan levies no capital-gains tax on either realized or unrealized gains from stock trading.

2.2 2017 Securities Transaction Tax Reform

On April 28, 2017, Taiwan halved the securities transaction tax specifically for day trading of common stocks from 30 to 15 basis points, while maintaining the tax rate for other trades (Business Today Taiwan 2017). Under this reform, investors who complete a round-trip trade within a single day pay only 15 basis points in transaction tax per dollar sold, rather than the previous 30 basis points. The reduced tax rate was initially implemented as a one-year temporary measure to boost stock-market trading volume and liquidity. However, it has since been extended multiple times and is currently scheduled to remain in effect through December 31, 2027. The reform reduced total transaction costs (tax plus commissions) for day trading from 40 to 25 basis points.

Importantly, this tax cut initially applied exclusively to brokered transactions (primarily retail investors). Securities dealers' proprietary trading became eligible only one year later, on April 28, 2018.[6] Therefore, the 2017 reform represents an exogenous shock to trading costs primarily for retail day traders.

The tax cut leaves two groups of securities unaffected. First, Taiwan restricts day trading eligibility to approximately 85% of common stocks. This unique institutional feature allows us to identify the reform's market impact by comparing eligible and ineligible stocks. Eligibility is prescribed by the Financial Supervisory Commission and covers stocks that qualify for margin trading, serve as warrant underlyings, or are eligible for securities lending. These criteria include a minimum listing tenure, net book equity per share above par value, minimum market capitalization, trading volume, and profitability.[7] In general, eligible stocks tend to be larger, more liquid, and more profitable than ineligible ones. Second, the tax cut applies only to common stocks, so the cost of day trading exchange-traded funds (ETFs) is unchanged. The transaction tax on stock ETF sales remains at 10 basis points.[8] Our main analysis does not exploit this variation because common stocks account for 96.3% of day trading volume in Taiwan. However, day trading activity in ETFs serves as a useful comparison for ruling out alternative explanations for our results.

6. See Ministry of Finance Law Database, https://law-out.mof.gov.tw/LawContentHistory.aspx?hid=42432&id=FL006079.

7. The FSC order defining the scope of eligible securities is issued under Article 37-1 of the Regulations Governing Securities Firms (https://www.sfb.gov.tw/ch/home.jsp?id=88&parentpath=0,3&mcustomize=lawnews_view.jsp&dataserno=202511130002). Margin eligibility is governed by Article 2 of the Standards Governing Eligibility of Securities for Margin Purchase and Short Sale (https://law.moj.gov.tw/ENG/LawClass/LawAll.aspx?pcode=G0400009); warrant-underlying eligibility by Article 10 of the TWSE Rules Governing Review of Call (Put) Warrants for Listing (https://twse-regulation.twse.com.tw/m/LawContent.aspx?FID=FL007274); and securities-lending eligibility by the TWSE Securities Borrowing and Lending Rules (https://twse-regulation.twse.com.tw/m/LawContent.aspx?FID=FL007044).

8. Bond ETFs became tax-exempt on January 1, 2017.

2.3 Data and Sample

For our analysis of investor responses to the tax reform, we use comprehensive trading records for 120,577 retail investors from a major Taiwanese brokerage. This dataset accounts for approximately 7.5% of total retail volume and 4.5% of total volume in Taiwan. The data include both order-level and account-level information. At the order level, we observe submission and execution details, including order prices and quantities, execution prices, timestamps, trade direction, and whether orders were canceled or modified. At the account level, we observe basic demographics (age, gender, and zip code) and portfolio holdings. Panel A of Table F.2 presents summary statistics on the demographics and account characteristics of investors in our full brokerage sample.

Appendix A demonstrates the representativeness of our dataset. First, we show that the average trading behavior in our sample is comparable to that reported by Barber et al. (2014) and Barber et al. (2020), who observe the entire population of retail traders in Taiwan from 1992 to 2006. Second, Panel A of Figure A.1 plots the daily cross-sectional correlation across stocks between logged trading volumes in our sample and the corresponding logged aggregate retail volumes. The correlation remains consistently around 90% throughout the sample period, with no noticeable change around the tax reform. Moreover, since our study relates to day trading, Panel B shows that the biweekly day trading share (defined as the ratio of day-trade volume to total retail volume) measured in our sample closely tracks that of the aggregate market. Importantly, both series show a substantial increase in day trading activity after the tax reform.

We obtain stock-level information, including daily closing prices, trading volume, and quarterly firm fundamentals from the *Taiwan Economic Journal* (TEJ) database. To examine the market impact of the tax reform, we measure market quality using intraday order book data for all common stocks traded on the Taiwan Stock Exchange and Taipei Exchange. The data consist of second-by-second snapshots of the consolidated limit order book, including the five best bid and ask prices and their respective depths. We construct daily stock-level intraday liquidity measures using time-weighted order book depth and quoted spreads. Specifically, the variables are defined as follows:

- $ Depth (in USD) measures the average liquidity at the best bid and ask price:

$$\$\text{ Depth}_{st} = \sum_{\tau} w_{s\tau}(\text{Bid price}_{s\tau} \cdot \text{Bid depth}_{s\tau} + \text{Ask price}_{s\tau} \cdot \text{Ask depth}_{s\tau}) \tag{1}$$

  where s indexes stocks, t indexes trading days, and τ indexes quote updates within day t. Bid (Ask) $\text{price}_{s\tau}$ and Bid (Ask) $\text{depth}_{s\tau}$ denote the best bid (ask) price and corresponding depth for stock s at time τ. The weight $w_{s\tau}$ is the share of the trading day for which each quote is active, such that $\sum_{\tau} w_{s\tau} = 1$. This construction captures the average dollar liquidity available at the inside quotes over the course of a trading day.

- ▶ Quoted spread (in bps) measures the tightness of the order book and reflects the cost of immediate execution. For each stock and trading day, it is defined as:

$$\text{Quoted spread}_{st} = \sum_{\tau} w_{s\tau} \cdot 2 \cdot \left(\frac{\text{Ask}_{s\tau} - \text{Bid}_{s\tau}}{\text{Ask}_{s\tau} + \text{Bid}_{s\tau}} \right) \cdot 10000 \tag{2}$$

  where s indexes stocks, t indexes trading days, and τ indexes quote updates within day t. $\text{Bid}_{s\tau}$ and $\text{Ask}_{s\tau}$ denote the best bid and ask prices for stock s at time τ, and $w_{s\tau}$ is the share of the trading day for which the quote is active, such that $\sum_{\tau} w_{s\tau} = 1$. Multiplying by 10,000 converts the spread to basis points. This time-weighted measure captures the average quoted spread over the course of a trading day.

We use realized volatility to measure intraday volatility at the stock level. It is defined as the standard deviation of intraday returns calculated from midquote prices sampled every 10 minutes. This measure is annualized by multiplying by $\sqrt{27 * 252}$. Specifically,

$$\text{Realized volatility}_{st} = \sqrt{27 \cdot 252} \cdot \text{SD} \left(\log \left(\frac{m_{s\tau}}{m_{s,\tau-1}} \right) \right) \cdot 100 \tag{3}$$

where s indexes stocks, t indexes trading days, and τ indexes 10-minute intervals within a trading day. $m_{s\tau}$ denotes the midquote for stock s at time τ, calculated as the average of the best bid and ask prices. The log return $\log(m_{s\tau}/m_{s,\tau-1})$ measures price changes between adjacent 10-minute intervals. SD denotes the standard deviation across all intraday returns for a given stock-day, and the final value is annualized under the assumption of 27 ten-minute intervals per 4.5-hour trading day and 252 trading days per year. Multiplying by 100 converts the value to percentage points.

Throughout the paper, we restrict our main analysis to the sample period from November 2016 to October 2017, covering 6 months before (the "pre-period") and 6 months after the tax reform (the "post-period").

3 The Effect of the Transaction Tax Reform on Trading Behavior

In this section, we study the impact of the tax reform on day traders' behavior, focusing on trading volume and performance (measured by risk-adjusted gross returns). We begin by outlining our empirical strategy (Section 3.1). Then, we present the effect of the tax reform on day trading volume (Section 3.2) and document heterogeneity in trading volume responses (Section 3.3). Subsequently, we analyze changes in trading performance (Section 3.4).

3.1 Empirical Strategy

The main challenge in identifying the causal effect of the tax reform on day traders' behavior is isolating this effect from other influences on investor behavior, such as changes in market conditions.

To address this, we employ a difference-in-differences strategy that compares investors who regularly day trade (treatment) and investors who have similar speculative trading motives but rarely engage in day trading (control).

Specifically, we classify traders into treatment and control groups based on their day trading activity during a *classification* period—the year before the analysis period begins. The treatment group consists of day traders, defined as investors who executed at least one day trade during the classification period. The control group consists of active non-day traders who traded on at least 30 days but never executed a day trade during the same period. The 30-day restriction allows us to rule out investors with objectives fundamentally different from those of day traders, such as buy-and-hold investors.

This classification approach is motivated by the fact that day trading behavior is persistent. As Figure 1 shows, in the three years before the reform we examined, investors who executed at least one day trade in the previous calendar year continue to do so on roughly 23% of trading days the following year, while those who did not day trade maintain a much lower rate of about 3%. Figure 1 also confirms that the probability of day trading increases after the reform for the treatment group but remains low for the control group. It is worth noting that since trading behavior serves both as our classification criterion and an outcome of interest, we separate the classification and analysis periods to avoid biasing our estimates by conditioning on the outcome.

Our treatment and control groups are similar along several dimensions. Panel B of Table F.2 shows that their demographics and account characteristics are comparable during the pre-reform period. Beyond these characteristics, investors in the two groups tend to trade the same stocks at the same time. Figure F.2 plots the cross-sectional correlation across stocks between the log day trading volume of the treatment group and the log total trading volume of the control group, computed over windows of different lengths. The correlation averages 50% for daily windows and rises to 80% for two-week windows.

Together, this evidence suggests that, absent the reform, the two groups are likely to respond comparably to changes in market conditions. Crucially, since control-group members (identified by their classification-period behavior) have minimal day trading activity during the analysis period, the tax cut has little effect on their overall behavior.

Identification Assumption Formally, the validity of our results rests on the *parallel trends* (PT) assumption: the changes in behavior of active non-day traders (control) can serve as a valid counterfactual for day traders (treatment). Specifically, for trading volume, we assume that the percentage change in day trading volume among the treatment group would equal the percentage change in total trading volume among the control group absent the tax reform. Formally:

$$\underbrace{\frac{\mathbb{E}[\text{Volume}^{Day}_{i,\ post}(0)|D_i=1]}{\mathbb{E}[\text{Volume}^{Day}_{i,\ pre}(0)|D_i=1]}}_{\substack{\text{\% change in day-trade volume}\\ \text{by treatment}}} = \underbrace{\frac{\mathbb{E}[\text{Volume}^{Total}_{i,\ post}(0)|D_i=0]}{\mathbb{E}[\text{Volume}^{Total}_{i,\ pre}(0)|D_i=0]}}_{\substack{\text{\% change in total volume}\\ \text{by control}}} \tag{4}$$

where D_i indicates the treatment status of investor i, $\text{Volume}^{Day}_{i,\ pre}(0)$ and $\text{Volume}^{Day}_{i,\ post}(0)$ denote the day trading volume of investor i in the pre- and post-periods, absent the tax reform, and $\text{Volume}^{Total}_{i,\ pre}(0)$ and $\text{Volume}^{Total}_{i,\ post}(0)$ denote the total trading volume of investor i in the pre- and post-periods, absent the tax reform.

Specification Motivated by the PT assumption in multiplicative form (Equation 4) and accounting for zero trading volumes, we estimate the effect of the day trading tax cut on trading volume using Poisson quasi-maximum likelihood estimators (Wooldridge 2023; Chen and Roth 2024):

$$\text{Volume}_{it} = \exp(\beta \ \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t)\varepsilon_{it} \tag{5}$$

where i denotes investors and t denotes biweekly intervals. Volume_{it} represents day trading volume if investor i belongs to the treatment group and total trading volume if investor i belongs to the control group. Treat_i indicates whether investor i is in the treatment group. Post_t indicates whether interval t occurs after the tax reform implementation (April 28, 2017). The coefficient β captures the differential change in trading volume between the treatment and control groups after the tax reform. The expression $\exp(\beta)-1$ corresponds to the treatment effect in percentage terms. γ_i and δ_t represent individual and time fixed effects, respectively. We cluster standard errors at the investor level. To assess pre-trends, we also estimate a dynamic difference-in-differences specification.

For other trading outcomes, we employ a standard difference-in-differences design. The identification assumption here is analogous to the PT assumption in Equation 4, but expressed in additive rather than multiplicative form: absent the reform, the change in the outcome from day trades for the treatment group would equal the change in the outcome from all trades for the control group:

$$\underbrace{\mathbb{E}[\Delta Y^{Day}_i(0)|D_i=1]}_{\substack{\text{change in outcomes from day trades}\\ \text{by treatment}}} = \underbrace{\mathbb{E}[\Delta Y^{Total}_i(0)|D_i=0]}_{\substack{\text{change in outcomes from all trades}\\ \text{by control}}} \tag{6}$$

Specifically, we estimate:

$$y_{it} = \beta \ \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t + \varepsilon_{it} \tag{7}$$

where i indexes investors and t indexes days. y_{it} denotes the outcome of day trading if i belongs to the treatment group, and it denotes that of all trading if i belongs to the control group. Treat_i is an indicator for whether investor i is in the treatment group. Post_t is an indicator for whether day t

occurs after the tax reform implementation (April 28, 2017). The coefficient of interest, β, captures the differential change in the outcome for the treated group relative to the control group following the tax reform. γ_i, δ_t are individual and time fixed effects, respectively. We double-cluster standard errors at the investor and day levels for specifications involving returns, to account for the strong cross-sectional correlation in returns, and cluster at the investor level otherwise.

Threats to Identification Our empirical strategy involves several design choices and assumptions. Below we address five potential concerns.

First, a distinctive feature of our specifications (Equations 5 and 7) is that the outcome variables are defined differently for the treatment and control groups. We examine only day trades for the treatment group but all trades for the control group. This asymmetry is intentional. Since day trading accounts for only a fraction of total trading volume for the treatment group (around 20%, as shown in Panel (a) of Figure F.3), focusing exclusively on their day trades allows us to isolate the margin directly affected by the tax reform and avoid introducing noise into the estimated treatment effect.

Although this setup may appear unconventional, the difference-in-differences framework does not require the outcome to be defined identically across groups. Its validity depends instead on the parallel trends assumptions in Equations 4 and 6. These assumptions are supported by the two groups' similar speculative motives, observable characteristics and parallel pre-trends across different outcome variables in the following sections. Appendix B provides the theoretical basis for using different outcome variables in a difference-in-differences setting. It shows that our parallel trends assumptions follow from two stronger conditions with a clear economic interpretation. These conditions also imply that a conventional specification that compares the total trading volume of the two groups and rescales the resulting estimate by the day trading share of the treatment group should recover the same treatment effect as our main specification. In the main analysis, we confirm that the estimates from the two specifications are very similar.

Second, our estimates would be affected if investors in the control group also increased their day trading in response to the tax cut. However, any such response would raise the total trading volume of the control group and bias our estimates toward zero. Therefore, if anything, our results understate the true effect. Moreover, Figure 1 shows that the probability that a control-group investor day trades remains low both before and after the reform, so the effect of any day trading response on their total trading volume is minimal.

Third, our definition of "active" traders in the control group, trading on at least 30 days during the classification period, may appear arbitrary. In the following sections, we address this concern by showing our main results remain robust when the control group is defined using alternative thresholds for activity (10, 20, or 40 days). This suggests our findings are not driven by this particular choice.

Fourth, by defining treatment and control groups based on day trading activity during the classification period, we do not include investors who only started day trading after the classification period in the treatment group. We make this choice deliberately to avoid the overlap between the periods used for classification and analysis. However, to gauge the impact of this choice, we repeat our main analysis with treatment and control groups defined using the year immediately preceding the reform. The results in the following sections suggest that, if anything, using our original group definition slightly understates the treatment effect of the tax reform.

Fifth, we observe only completed day trades—positions opened and closed within the same trading day. However, investors may open multiple positions intending to day trade but close only some within the day, carrying others overnight due to, for instance, unfavorable price movements. This creates a potential bias: the tax reform might affect which positions traders close rather than their actual day trading activity. We address this by examining intended day trades, defined as all positions opened on days when an investor completes at least one day trade, following Barber et al. (2014) and Barber et al. (2020). Table F.3 shows that while the realization rate of intended day trades increases by 1.5 percentage points post-reform, the magnitude is economically small relative to the 77% baseline realization rate in the pre-reform period. Moreover, our main results prove robust to using this expanded definition, as we demonstrate in subsequent sections.

3.2 Trading Volume Responses

To assess the financial impact of the tax reform for day traders, we start by examining how day trading volume changes. In theory, traders account for transaction costs and should trade more when these costs fall. Whether they do so in practice, and by how much, remains an empirical question.

Using Equation 5, we find that the tax reform leads to a substantial increase in day trading activity. This suggests day traders in Taiwan are aware of the transaction tax and respond to changes in it. Table 1 reports the estimates of Equation 5 using different volume measures as outcomes for the treatment group: total, day trading, and non-day trading volume. Column (1) shows that total trading volume of investors in the treatment group increased by 5% $(\exp(0.05) - 1)$ relative to the control group, but the increase is not significant at the 5% level. This is due to the effect being driven by day trading, which is only 20% of total trading volume. Columns (2) and (3) show day trading volume rises by 32% $(\exp(0.28) - 1)$, while non-day trading volume shows no significant change ($\approx -2\%$ but statistically insignificant). In dollar terms, the 32% increase in day trading volume translates to roughly $8,000 per month. These estimates are internally consistent. Given the treatment group's baseline day trading share of approximately 20%, a 32% increase in day trading volume implies a 6% increase in total trading volume if non-day trading is unaffected, which is close to the 5% increase in column (1). This consistency is what the two conditions in Appendix B predict, and it further validates our use of different outcome variables across groups.

Panel (a) of Figure 2 plots biweekly day trading volume for the treatment group and total trading volume for the control group, with each series scaled by its own pre-reform mean. Panel (b) reports the coefficients from our dynamic difference-in-differences specification at a biweekly frequency, expressed in percentage terms for ease of interpretation. Consistent with our identifying assumption in Equation 4, treatment-group day trading volume and control-group total trading volume follow parallel trends throughout the pre-reform period. After the reform, the two series diverge sharply: day trading volume in the treatment group increases substantially, while total trading volume in the control group declines. Panel (b) captures this divergence as a sharp increase in the difference-in-differences coefficient.

The widening gap between the trading volume of the treatment and control groups could, in principle, reflect a reform-induced decline in the control group's trading instead of an increase in day trading by the treatment group. To distinguish between these explanations, we use the treatment group's non-day trading volume as a placebo outcome. Figure 3 plots this volume against the control group's total trading volume. The two series follow each other closely before and after the reform.

This pattern has two implications. First, the post-reform decline in the control group's volume is unlikely to be caused by the reform. Non-day trading by the treatment group is not directly affected by the reform, yet it falls in step with the control group's volume. Therefore, the decline in both series reflects broader market conditions, such as seasonality or changes in volatility, that affect the two groups in similar ways. These common factors are exactly why a difference-in-differences design is needed. Second, the reform's effect is specific to day trading, and the additional day trades are new trades rather than trades shifted out of non-day trading. Substitution would have reduced the treatment group's non-day trading volume relative to the control group, but no such relative decline appears.

We provide suggestive evidence that seasonality may contribute to the apparent post-reform decline in the control group's trading volume. Figure F.5 plots monthly non-day trading volume for our brokerage sample, expressed in percentage terms relative to April, the final pre-reform month. We plot the series separately for 2017 and for the pooled 2013–2016 period. Non-day trading volume declines after April in both periods. Because a similar decline appears even in years without the reform, this suggests part of the control group's pre- to post-reform decline may reflect seasonality.

In terms of economic significance, our estimate implies an elasticity of day trading volume with respect to total explicit transaction costs of roughly -0.85.[9] This aligns with elasticities documented across various markets summarized by Matheson (2011), as well as more recent evidence from Deng, Liu, and Wei (2018). Figure F.6 visualizes these estimates from the literature. While our setting is unique—examining a targeted day trading tax cut rather than broad-based transaction tax changes studied elsewhere—finding a similar elasticity enhances the external validity of our results.[10]

Robustness Our result is robust to several design choices as discussed in Section 3.1. Table F.4 shows that our estimated volume response is robust to alternative cutoffs for defining the control group. Column (1) of Table F.5 reports that redefining treatment and control groups based on the year immediately preceding the reform, rather than our original classification, yields a larger estimated day trading response. This suggests that using our original group definition, if anything, slightly understates the treatment effect of the tax reform. Column (1) of Table F.6 shows that our result is robust to expanding our definition of day trading to intended day trades (i.e. new positions opened on days when an investor completes at least one day trade).

Beyond these design choices, a remaining concern is that our estimate captures a shock other than the tax cut that differentially affected day trading. While the parallel pre-trends in Figure 2 make such a shock unlikely, we cannot completely rule it out. To further address this concern, we exploit the fact that the tax cut applies only to common stocks, leaving the cost of day trading ETFs unchanged (Section 2.2). Therefore, if the increase in day trading is driven by the tax cut, it should appear only in common stocks. If instead a shock other than the reform differentially affects day trading, its effect should appear in both common stocks and ETFs. Panel (b) of Figure F.3 plots the ratio of day trading volume to total trading volume for the treatment group, separately for common stocks and ETFs. The ratio for common stocks rises sharply right after the reform, from about 22% to nearly 30%. In contrast, the ratio for ETFs does not increase after the reform and, if anything, declines. This pattern provides further evidence against a concurrent shock that differentially affected day trading and supports our interpretation that the increase is driven by the tax cut.[11]

9. The tax cut of 15 basis points represents a 37.5% reduction in total explicit transaction costs (from 40 to 25 basis points, including both tax and commission). The 32% increase in day trading volume divided by this 37.5% cost reduction yields an elasticity of approximately -0.85.

10. For instance, Jackson and O'Donnell (1985) study UK stamp duty changes in 1974 and 1984; Liu (2007) examines changes in Japanese securities transaction taxes from 1978–1999; Deng, Liu, and Wei (2018) study Chinese stamp duty changes from 1997–2008.

11. We use this comparison as supporting evidence rather than exploiting this variation in our main analysis because the choice of which securities to day trade is itself likely affected by the tax cut. Moreover, ETFs account for less than 4% of day trading volume, which makes the ETF series noisy.

3.3 Heterogeneity in Trading Volume Responses

Having documented that day trading volume increases following the tax reform, we now examine whether this response varies with investor sophistication. The financial consequences of the reform depend on which investors are induced to trade more. If the additional trading comes mainly from sophisticated investors who persistently earn higher returns on their trades, the reform is more likely to leave day traders better off.

Motivated by evidence of persistent return heterogeneity across the wealth distribution (Fagereng et al. 2020; Bach, Calvet, and Sodini 2020; Daminato and Pistaferri 2024), we proxy for investor sophistication with predicted wealth.[12] As a robustness check, we also look at trading experience as an alternative measure of investor sophistication (Dhar and Zhu 2006; Seru, Shumway, and Stoffman 2010; Barrot, Kaniel, and Sraer 2016).

Figure 4 presents the heterogeneous effects of the transaction tax reform on day trading volume by investor wealth terciles. Traders in the bottom wealth tercile increase their day trading volume the most in percentage terms ($\approx$ 49%). An alternative sophistication proxy—trading experience, measured by the number of trades placed in the two years before the reform—produces a similar pattern (see Figure F.8 in the appendix). Taken together, these findings indicate that lower day trading costs disproportionately stimulate activity among less sophisticated investors.

These same investors, however, are also the worst day traders. Figure 5 plots day traders' alpha by wealth over the six months preceding the reform, where alpha is the intercept from regressing day traders' returns on the intraday market excess return.[13] Panel (a) and Panel (b) plot alpha before and after accounting for transaction costs, respectively. After adjusting for market risk and netting out all transaction costs — including the transaction tax and brokerage commissions — the average day trade by a bottom-tercile investor loses roughly 40 basis points per dollar. Even if these traders fully captured the 15-basis-point tax reduction, they would still lose money on the average trade after the reform. Yet this is precisely the group whose trading expands the most. Because the volume response is negatively correlated with investor sophistication, the reform's stimulus to trading magnifies the losses borne by those least equipped to profit from it.

The stronger volume response of low-wealth investors is at odds with standard explanations of their ex ante trading behavior. Before the reform, low-wealth investors have the highest turnover

12. We observe the wealth of every Taiwanese resident by combining several administrative registers maintained by the Fiscal Information Agency of the Ministry of Finance. We then predict the wealth of each investor in our brokerage sample as the average wealth of residents in the same postal code, age, and gender cell who participated in the stock market. Chu, Lin, and Nian (2024) describe the construction of the wealth data in detail.

13. To accurately measure day trading performance in levels, we follow Barber et al. (2014) and Barber et al. (2020) and compute their intraday returns using *intended day trades*, which include all completed round-trip day trades together with any position-opening trades left open at the close of the trading day. Restricting attention to round-trips alone would bias the performance measure if traders tend to realize winners and ride losers — that is, if they exhibit the disposition effect. As noted in Sections 3.2 and 3.4, our estimates of day traders' response to the tax cut are robust to using this broader definition in place of the round-trip definition.

(Figure F.7). The literature often attributes frequent trading by retail investors to behavioral biases such as overconfidence (Odean 1998b; Glaser and Weber 2007; Graham, Harvey, and Huang 2009), sensation seeking (Grinblatt and Keloharju 2009; Dorn and Sengmueller 2009; Gao and Lin 2015), or inattention to trading costs (Barber, Odean, and Zheng 2005). However, if low-wealth investors are more prone to these biases, a reduction in trading costs would likely affect their trading less, not more. A cost reduction encourages trades that were previously marginal, that is, trades whose expected return fell just short of the cost of making them. For overconfident investors, for example, few trades are likely to be marginal. Because they overestimate expected returns, most trading opportunities already appear profitable before the cost is reduced, leaving few that a lower cost could convert into trades. Therefore, reconciling the explanations for retail investors' high turnover with the response we document requires either a specific assumption about how these investors perceive the distribution of returns or an additional mechanism. We return to this pattern in Section 5.2, where we propose a mechanism that accounts for it.

3.4 Performance Responses

We now examine how the tax reform affects day traders' performance, specifically their risk-adjusted gross returns per dollar traded. Even though we demonstrate that day trading volume increases post-reform, whether the additional trades of a given investor perform as well as their previous trades is not obvious. If day traders increase trading while largely maintaining their existing strategies on average, their risk-adjusted gross returns should remain constant.

To evaluate the effect of the reform on day trading performance, we estimate Equation 7, comparing per-dollar gross returns between the treatment group's day trades and the control group's total trades on the same day. Importantly, to interpret our result on a risk-adjusted basis, we control for the intraday market excess return: we interact the market excess return with the treatment and post indicators, allowing market beta to differ between day traders and the control group and to shift after the reform. The treatment-by-post coefficient then isolates the change in risk-adjusted return—the change in alpha—purged of any change in market exposure.

Specifically, we estimate

$$y_{it} = \beta \text{ Treat}_i \times \text{Post}_t + \lambda_1 \text{ Treat}_i \times R_t^m + \lambda_2 \text{ Treat}_i \times \text{Post}_t \times R_t^m + \gamma_i + \delta_t + \varepsilon_{it}, \quad (8)$$

where y_{it} is the gross return per dollar traded by investor i on day t, in basis points. R_t^m is the intraday market return in excess of the risk-free rate on day t. The market return is common to all investors on a given day and is therefore absorbed by the day fixed effects δ_t, so R_t^m enters Equation 8 only through its interactions with Treat_i. These interactions allow the treatment group's market beta to differ from the control group's (λ_1) and to change following the reform (λ_2). Once the return variation attributable to this (possibly changing) market exposure is removed, the coefficient β isolates the differential change in the treated group's *alpha* relative to the control group.

Surprisingly, we find that day traders' gross alpha declines by 5.5 basis points following the tax reform. Table 2 reports the estimates from Equation 8, where we calculate gross returns per dollar traded using different subsets of trades for the treatment group: all trades, day trades only, and non-day trades only. For the control group, we consistently use all trades. Column (1) shows that when all trades are included, the treatment group's alpha decreases by 2.4 basis points after the tax cut, although this estimate is only marginally significant. This is consistent with the decline being concentrated in day trades, as shown in columns (2) and (3). Day-trade gross alpha falls by 5.5 basis points, while non-day-trade alpha shows no significant change. This pattern confirms that the tax reform specifically affected day trading behavior.

The result suggests not only do day traders trade more after the reform, but they also trade differently. This substantial decline in per-dollar gross returns following the tax reform is quite surprising. Since stock markets are relatively efficient, consistently selecting either losing or winning trades would be notoriously difficult. Yet these traders manage to systematically worsen their per-trade performance following the tax cut. We therefore explore a potential explanation in Section 5.

We also examine whether the decline in day-trade alpha varies by investor wealth. Table F.11 reports the coefficient estimates from Equation 8 separately for each wealth tercile. Although the estimates for the middle and top terciles are not statistically significant at the 5% level, we cannot reject the null that the performance decline is equal between the top and bottom terciles.

Robustness Our result is robust to several design choices as discussed in Section 3.1. Table F.7 shows that our finding on investor performance response is robust to alternative cutoffs for control-group definition. The coefficient estimates remain negative and statistically significant across specifications using different trading frequency thresholds for defining the control group. Similarly, when we redefine treatment and control groups based on trading behavior in the year immediately preceding the reform rather than our original classification, we observe a comparable decline in day trading performance (Table F.5, column 2). Column (2) of Table F.6 shows that our estimate is similar when we expand our definition of day trading to intended day trades (i.e. new positions opened on days when an investor completes at least one day trade).

We also examine whether the decline in day traders' performance is robust to alternative counterfactual measures. Table F.8 demonstrates that using longer holding periods for the control group, either 10-day or 30-day returns normalized to daily, instead of first-day gross returns yields similar results. These robustness checks confirm that our findings are not driven by the particular choice of control-group performance measure.

4 The Financial Impact of the Transaction Tax Reform on Individuals

In this section, we assess the financial impact of the tax reform on individual investors. We measure this financial impact as the change in the risk-adjusted contribution of day trading to net portfolio returns. The risk adjustment removes the impact attributable to market exposure. We restrict the analysis to day trading because Sections 3.2 and 3.4 show that the reform has minimal effects on non-day trading volume and performance.

Whether the reform raises or lowers day traders' net portfolio returns is not obvious from the responses documented in Sections 3.2 and 3.4 alone. Day traders mechanically gain 15 basis points per trade from the tax cut. In theory, they could still come out ahead if this gain on their existing trades exceeds the losses driven by additional trading and deteriorated performance. It is worth noting that, because of our empirical design, our results speak to investors who were already day trading before the reform. This is precisely the group for which the financial consequences of the reform are least clear—a tax cut raises the net return on the trades they would have made in any case, while also inducing trades that need not be profitable.

We examine the financial impact of the tax reform on day traders through a simple decomposition exercise. Because of the investor heterogeneity documented in Section 3.3, we start by computing the impact on portfolio returns within each wealth tercile, g. Specifically, we approximate the impact as

$$\begin{aligned}
\mathbb{E}_g[\Delta\text{Risk-Adj. PR}_i] &= \mathbb{E}_g\left[\frac{\Delta\text{Risk-Adj. Net Profit}_i}{\text{Portfolio Size}_{i,\text{pre}}}\right] = \mathbb{E}_g\left[\frac{\Delta\left(\text{Day-Trade Volume}_i \times \text{Risk-Adj. Net Return}_i\right)}{\text{Portfolio Size}_{i,\text{pre}}}\right] \\
&\approx \mathbb{E}_g\left[\frac{\text{Day-Trade Volume}_{i,\text{pre}}}{\text{Portfolio Size}_{i,\text{pre}}}\right] \times \left(\underbrace{\Delta\text{Tax}}_{\text{Tax cut}} + \underbrace{\Delta\text{Risk-Adj. Gross Return}_g}_{\text{Performance response}}\right) \\
&\quad + \mathbb{E}_g\left[\frac{\text{Risk-Adj. Net Return}_{i,\text{post}}}{\text{Portfolio Size}_{i,\text{pre}}}\right] \times \underbrace{\mathbb{E}_g[\Delta\text{Day-Trade Volume}_i]}_{\text{Volume response in \$}}
\end{aligned} \tag{9}$$

In this expression, $\mathbb{E}_g[\cdot]$ denotes the average across investors in group g, where groups are formed based on terciles of investor wealth. ΔRisk-Adj. Net Profit$_i$ denotes the change in monthly risk-adjusted net profits from intended day trades.[14] Portfolio Size$_{i,\text{pre}}$ is each investor's portfolio size in the month prior to the reform. Day-Trade Volume$_{i,\text{pre}}$ represents each investor's monthly intended day trading volume pre-reform. The change in the group's risk-adjusted net return per dollar day-traded, ΔRisk-Adj. Net Return$_g$, is decomposed into two components: ΔTax, the mechanical increase in net returns from the tax cut (15 bps for all investors), and ΔRisk-Adj. Gross Return$_g$, the causal effect of the reform on day-trade alpha, estimated by wealth tercile as in Section 3.4. The term

14. Intended day trades are defined following Barber et al. (2014) and Barber et al. (2020). Focusing on intended day trades allows us to measure day traders' trading volume and performance accurately in levels. As shown in the robustness checks in Sections 3.2 and 3.4, using this expanded definition of day trading does not affect our estimates of investor responses.

$\mathbb{E}_g\left[\frac{\text{Risk-Adj. Net Return}_{i,\text{post}}}{\text{Portfolio Size}_{i,\text{pre}}}\right]$ is the average across investors in group g of the post-reform risk-adjusted net return per dollar day-traded divided by pre-reform portfolio size.[15] $\mathbb{E}_g[\Delta\text{Day-Trade Volume}_i]$ represents the causal effect of the reform on intended day trading volume in dollar terms, obtained by multiplying the volume responses of intended day trades in column (5) of Table F.12 by the average pre-reform monthly intended day trading volume for each group g.

The first component of Equation 9 (i.e., $\mathbb{E}_g\left[\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times \Delta\text{Tax}$) captures the effect of the tax reform on portfolio returns in the absence of any investor responses. The second term ($\mathbb{E}_g\left[\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times \Delta\text{Risk-Adj. Gross Return}_g$) reflects the financial impact of changes in trading performance regardless of the market return. In our setting, this term is negative, indicating that the performance response partially offsets the gains from the tax cut. Finally, the third term, $\mathbb{E}_g\left[\frac{\text{Risk-Adj. Net Return}_{i,\text{post}}}{\text{Portfolio Size}_{i,\text{pre}}}\right] \times \mathbb{E}_g[\Delta\text{Day-Trade Volume}_i]$, isolates the change in portfolio returns generated by the additional day trading, valued at the post-reform risk-adjusted net return of day traders.

To compute the financial impact of the tax reform on the average day trader, we take a weighted sum across groups:

$$\mathbb{E}\left[\Delta\text{Risk-Adj. PR}_i\right] = \sum_g s_g\, \mathbb{E}_g\left[\Delta\text{Risk-Adj. PR}_i\right] \tag{10}$$

where s_g is the population share of group g.

Figure 7 presents the decomposition of the financial impact of the transaction tax reform across wealth terciles, based on Equation 9. The blue bars show that, absent any investor responses, bottom-tercile investors would have gained the most from the tax cut—a 1.89 pp boost to annualized portfolio returns versus 1.64 pp and 1.40 pp for middle and top terciles—because they day-trade most intensively relative to portfolio size. The green bars indicate the negative impact of reduced per-dollar risk-adjusted gross returns from day trading for all groups following the tax reform. The red bars represent the impact from increased trading volume, which proves most damaging. Bottom-tercile investors generate volume-induced losses of -5.46 pp by increasing trading aggressively while earning negative net returns even post-reform (see Section 3.3). In contrast, top-tercile investors, who trade more moderately and perform better, incur comparatively smaller volume-induced losses of -1.65 pp.

Overall, all wealth terciles are left worse off by the reform, with the losses heavily concentrated among low-wealth investors. Bottom-tercile investors lose 4.54 pp in annualized portfolio returns,

15. The post-reform risk-adjusted net return per dollar day-traded for investor i is constructed as

$$\text{Risk-Adj. Net Return}_{i,\text{post}} = \text{Risk-Adj. Net Return}_{i,\text{pre}} + \underbrace{\Delta\text{Tax}}_{15\text{ bps}} + \Delta\text{Risk-Adj. Gross Return}_g$$

far more than the middle and top terciles, which incur similar losses of 1.07 pp and 0.62 pp, respectively. Our result underscores the importance of investor responses to lower trading costs. Absent any change in behavior, smaller investors would have benefited the most from the reform. Instead, they respond most aggressively to the tax cut while earning the worst returns, leaving them substantially worse off. Table F.12 reports the population-weighted average impact using Equation 10. Column (10) shows that the average day trader's annualized portfolio return decreases by 2.08 percentage points following the tax cut.

Statistical Uncertainty The decomposition combines several estimated inputs so we bootstrap the entire procedure to gauge the statistical uncertainty. We resample in 1,000 draws along both the day and the investor dimensions because there are two sources of dependence in the data. Returns are correlated across investors within a trading day, while trading volume and the pre-reform averages are investor-level quantities. Table F.13 reports the results. The net impact for bottom-tercile investors, -4.54 pp, is significant at the 5% level, and the population-weighted net impact of -2.08 pp is also significant at the 5% level. The net impacts for the middle and top terciles are not statistically distinguishable from zero. The losses of low-wealth day traders and of the average day trader are therefore not due to sampling variation.

External Validity Given that our paper is based on a setting in Taiwan, it is important to consider the extent to which our findings generalize beyond this context. Below we discuss three factors that affect the financial impact of the reform and show that our results are unlikely to be specific to Taiwan.

The first factor is the baseline profitability of retail day traders. The reform makes day traders worse off because those who increase trading tend to incur the largest losses. Whether day traders lose money depends on their gross per-trade performance and on the transaction costs they pay. We show that neither drives our results.

In terms of gross per-trade performance, day traders in our sample earn returns comparable to those documented in other markets. Table F.18 reports the average gross day-trading return in our sample computed with the method of Linnainmaa (2005). Domestic individual day traders earn -0.04% per day before the reform, compared with -0.21% per day for Finnish day traders. While there is no comparable study for US retail day traders, Table F.17 shows that the average trade by individual investors in our sample earns market-adjusted returns similar to those documented for US retail investors by Odean (1999). Retail trades in Taiwan therefore do not appear unusually unprofitable relative to these international benchmarks.

In terms of transaction costs, day traders in Taiwan continue to pay 25 basis points per round trip after the reform, which is substantially higher than the cost of trading in markets where commissions have fallen to zero. Because these remaining costs contribute to the losses that day traders incur, a natural concern is that the same behavioral response would not be harmful in a market where transaction costs become essentially zero. To assess this concern, we repeat the calculation in Equation 9 assuming the total transaction cost is reduced from 15 to 0 basis points, while holding fixed the estimated volume and performance responses. Figure F.12 shows that the effect on low-wealth investors is similar. These investors, who respond most strongly to the reform, lose 1.26 percentage points per year. The average day trader is marginally better off under this assumption because for larger investors the mechanical tax saving exceeds the losses on the additional trades. This calculation, however, holds each group's baseline performance fixed at the level observed in Taiwan's high-cost environment. If lower trading costs also worsen baseline performance, as the decline in gross return documented in Section 3.4 suggests, the average impact would easily become negative.

The second factor is how strongly day trading volume responds to transaction costs. No prior study estimates this elasticity for retail speculators specifically, so we compare our estimate with the literature on broad-based changes in securities transaction taxes.[16] As discussed in Section 3.2, our estimated elasticity of day trading volume with respect to total explicit transaction costs of -0.85 lies within the range documented in this literature.

The third factor is the decline in the per-trade performance of day traders after the reform. No prior study has examined how the per-trade performance of retail speculators changes as transaction costs fall so we cannot benchmark this component directly. We nevertheless expect it to generalize beyond Taiwan for two reasons. First, retail investors behave in remarkably similar ways across countries. Table F.15 shows that three well-documented behavioral patterns, namely excessive trading, the disposition effect, and gambling preferences, appear in the United States, Taiwan, and many other markets. Second, the investors in our brokerage sample resemble those studied in the seminal work of Barber and Odean (2000) and Dorn and Huberman (2005). Table F.16 shows that the average characteristics of investors in our sample, including age and account size, are similar to those reported in these studies.

5 Mechanism

The previous section shows that investor responses more than offset the tax savings from the reform and leave investors worse off financially. Given the importance of these responses, we now explore what explains them. We develop a model, motivated by the disciplinary role of transaction costs, in which a single mechanism can account for both why cheaper trading leads investors to trade worse and why the volume response is concentrated among investors with less wealth.

16. This literature typically reports the elasticity of overall trading volume with respect to transaction costs.

5.1 Motivating Evidence

Our model formalizes a widespread regulatory concern that transaction costs serve a disciplinary role, and removing them creates an illusion of free trading that invites careless and inattentive behavior. (Cook 2021; ESMA 2021; Gensler 2021) The European Securities and Markets Authority (ESMA), for example, writes that "the marketing of the service as 'cost-free' [...] could incentivise retail investors' gaming or speculative behaviour due to the incorrect perception that trading is free" (ESMA 2021).

We first document evidence consistent with this concern. Models of attention allocation, such as Gabaix (2019) and Bastianello, Decaire, and Guenzel (2024), predict that when decision makers reduce cognitive effort, they disproportionately reduce the weight they place on low-salience information. Therefore, if lower trading costs reduce the cognitive effort investors bring to their trades, as regulators worried, trading behaviors associated with limited attention should become more common after the tax cut. Appendix C derives this prediction in a simple model of attention allocation.

We show that three inattentive behaviors that the literature has identified as detrimental to trading performance all increase following the tax reform, contributing to the decline in day traders' gross alpha. Specifically, we document (i) greater reliance on cognitive shortcuts, (ii) reduced monitoring of limit orders, and (iii) increased salience-driven trading.

First, we present evidence that day traders' reliance on cognitive shortcuts when placing orders increases following the tax reform. Cognitive shortcuts, such as left-digit bias or round-number bias, are often adopted by decision makers to reduce cognitive load and save psychic costs (Gilovich, Griffin, and Kahneman 2002). Following Kuo, Lin, and Zhao (2015), we proxy the use of cognitive shortcuts with the placement of limit orders at round-number prices. While this heuristic simplifies decision-making for the trader, it is associated with worse performance when day trading as shown in Table F.9. If investors devote less mental effort after the reform, we would expect this behavior to intensify because placing limit orders at round numbers can be viewed as investors focusing on salient features of prices.

Figure F.9 confirms that traders in our sample exhibit a notable use of cognitive shortcuts: limit orders are significantly more likely to be at round-number prices ending in .00 or .50. We measure the reliance on cognitive shortcuts for an investor as the share of round-number limit orders submitted:

$$\text{Share of round-number orders}_{i,t} = \frac{\text{\# Limit orders at round-number prices}_{i,t}}{\text{\# Limit orders}_{i,t}}$$

where # Limit orders at round-number prices$_{i,t}$ refers to limit orders placed at prices ending in .00

or .50 by investor i on trading day t, and # Limit orders$_{i,t}$ is the total number of limit orders submitted by investor i on trading day t. Importantly, this measure is correlated with investor sophistication: during the pre-reform period, traders with lower wealth and less trading experience are significantly more likely to submit round-number orders (see Figure F.10).

Table 3 reports the effect of the transaction tax reform on the reliance on cognitive shortcuts, estimated using Equation 7. The table indicates that treatment-group investors' share of round-number limit orders associated with day trades rises by 2 percentage points following the reform (7% of the pre-reform mean). This result is consistent with our prediction that lower trading costs may encourage traders to act with less attention. We now turn to additional evidence supporting this mechanism.

Second, we show that day traders' monitoring of their limit orders decreases following the tax reform. Linnainmaa (2010) shows that retail investors' poor performance can be traced to their limit orders being picked off by informed traders. Moreover, the paper shows Finnish individual investors do not always actively monitor their limit orders, significantly increasing their risk of adverse selection. For instance, they often place orders before the trading session even starts and leave 25% of their limit orders outstanding for more than one day. To measure traders' monitoring efforts of limit orders in our sample, we use two measures: (i) limit order idle time and (ii) the probability of order modification.

Limit order idle time is defined as the time until order execution, cancellation, or market close, in seconds. That is,

$$\text{Order idle time}_{i,o,t} = \begin{cases} \text{Execution time}_{i,o,t} - \text{Submission time}_{i,o,t}, & \text{if executed} \\ \text{Cancellation time}_{i,o,t} - \text{Submission time}_{i,o,t}, & \text{if canceled} \\ \text{Market close (1:30 p.m.)} - \text{Submission time}_{i,o,t}, & \text{otherwise} \end{cases}$$

where Execution time$_{i,o,t}$ is the time at which investor i's limit order o is executed on trading day t, Cancellation time$_{i,o,t}$ is the time at which investor i cancels limit order o on trading day t, and Submission time$_{i,o,t}$ is the time at which investor i submits limit order o on trading day t. Market close (1:30 p.m.) refers to the market closing time, which is 1:30 p.m. in Taiwan.

The probability of order modification is the share of limit orders modified or canceled by investor i on day t:

$$\text{Probability of modification}_{i,t} = \frac{\text{\# Limit orders modified or canceled}}{\text{\# Limit orders}}$$

Table 4 presents the estimates from Equation 7 with these two measures as dependent variables. Column (1) reports the effect of the transaction tax reform on the log of mean order idle time. The estimate indicates that the duration a day-trade order from the treatment group remains outstanding increases by 4% following the tax reform. Additionally, column (2) shows that the probability of a day-trade order being modified or canceled decreases by 0.73 percentage points (4% of the pre-reform mean). Taken together, the longer idle times and lower modification rates point to less frequent monitoring of limit orders among day traders. This is consistent with investors devoting less mental effort and may have contributed to the observed decline in trading performance due to increased adverse selection risk.

Finally, we document greater salience-driven trading among day traders following the tax reform. Retail investors face a formidable search problem: there are thousands of potential stocks to invest in, but they have limited cognitive resources to evaluate them. Seasholes and Wu (2007) and Barber and Odean (2008) show that retail investors tend to trade attention-grabbing stocks, suggesting they use salience as a heuristic to navigate this complexity. According to our stylized attention allocation model, we hypothesize that if day traders exert less mental effort after the reform, they will concentrate more on trading stocks that are salient and grab their attention.

To test this hypothesis, we define salient events for a stock as days when that stock's absolute overnight return ranks in the top 5% across all stocks.[17] To measure the extent to which investors' trades are influenced by salient events, we compute the share of their trading volume on a given trading day that is in stocks with extreme returns X days prior, where X represents any chosen time interval (e.g., 2 days or 10 days). This measure captures investors' propensity to trade stocks a certain number of days before or after significant price movements, with smaller and positive X values indicating more immediate reactions to salient events.

$$\begin{array}{c}\text{Share of volume in stocks}\\ \text{w/ extreme overnight returns X days ago}_{i,t}\end{array} = \frac{\text{Volume in stocks with extreme overnight returns X days ago}_{i,t}}{\text{Trading Volume}_{i,t}}$$

where Trading $\text{Volume}_{i,t}$ is the total trading volume of investor i on trading day t, and Volume in stocks with extreme overnight returns X days $\text{ago}_{i,t}$ is investor i's trading volume in stocks that experienced extreme overnight returns X days prior to day t.

If day traders increase their salience-driven trading following the tax reform, we would expect their share of trading in stocks with recent extreme overnight returns to increase. Importantly, if those trades are indeed driven by salience, such a pattern should be particularly prominent for stocks with very *recent* extreme returns and more muted for stocks that experienced extreme returns a long time ago (say, 10 days) or have yet to experience those returns (say, -5 days). Therefore, we estimate multiple regressions varying the definition of salient stocks as placebo tests.

17. Using past returns to define salient events is common in the literature (e.g., Barber and Odean (2008), Fedyk (2021), and Barber et al. (2022)).

Figure 6 plots the coefficient estimates from estimating Equation 7, where the dependent variable measures the share of trading volume in stocks that experienced an extreme overnight return between -12 and 20 days ago. The figure shows that, following the tax reform, day traders' propensity to trade stocks with extreme overnight returns in the prior two trading days increases by nearly 1 percentage point (4% of the pre-reform mean). Crucially, we observe no similar change for stocks that experienced extreme returns three or more days ago, nor do traders preemptively trade stocks ahead of future extreme returns. Moreover, Table F.10 shows that day traders, on average, earn 2 basis points lower returns from stocks with extreme overnight returns in the prior two days, suggesting these trades are driven more by salience than by information. The increased propensity to trade stocks with very recent extreme returns following the tax reform is therefore consistent with increased inattentive trading, where the visibility of recent extreme returns drives trading decisions.

5.2 Model with Costly Attention

The previous section documents evidence consistent with investors paying less attention to their trades, which contributes to worse per-trade performance, after day trading becomes cheaper. In this section, we present a stylized model of trading with costly attention. The model rationalizes two investor responses that result in the decline in day traders' net portfolio returns following the reform. First, the percentage increase in trading volume after the reform is higher among investors with lower wealth. Second, investor attention falls when trading costs fall.

Setup Consider a trader with wealth W. Each day, one trading opportunity arrives.[18] The trader can act on the opportunity or pass on it. Acting on it costs a fraction κ of the amount traded for a round-trip trade, which captures the transaction cost that a trader pays. For simplicity, we assume that the trader is risk-neutral and that the position size is a fixed fraction θ of wealth, i.e., θW dollars. In Appendix E, we show that our main predictions hold for a trader with CRRA utility who chooses the position size under an additional assumption.

To decide whether to act on an opportunity, the trader must assess its return. The trader starts from a prior, denoted μ^d, about the expected return of any opportunity. The prior represents the trader's ex ante assessment of what a typical opportunity delivers and reflects their confidence in their own skill. We assume $\mu^d > \kappa$. This means that a trader who relied only on the prior would trade every opportunity, since each one appears profitable net of trading costs. Since the goal of our model is to explain day traders' behavior, one way to rationalize this prior is that those who already self-selected into day trading are overconfident (Dorn and Huberman 2005; Glaser and Weber 2007; Liu et al. 2022).

18. Because our goal is to isolate the role of attention, we keep the arrival of opportunities deliberately simple and abstract from variation in how many opportunities a trader receives.

When evaluating the return of a trading opportunity, the trader does not rely on their prior alone. The trader believes that paying attention affects the return they can earn. This is consistent with Duraj et al. (2025), who find that people see successful investing as requiring continuous effort. With full attention, an opportunity could deliver an expected return α, drawn from a distribution F on $[0, \infty)$ with density f and mean μ^d. Learning about α and earning it require attention, $a \geq 0$. Each unit of attention has a cognitive cost, c. This is the cost of the trader's effort, such as reading news and price charts, timing the order, and monitoring the position once it is open. Laarits and Wurgler (2025) show that retail investors do incur this cost in practice.

The trader chooses how much attention to pay before an opportunity arrives. Hence, they trade off the expected profit from paying attention a against its cost, $c \cdot a$ (Sims 2003; Maćkowiak, Matějka, and Wiederholt 2023). Given attention a, the expected profit from an opportunity is

$$\theta W \underbrace{\Pr(\hat{\mu} > \kappa)}_{\text{Prob. of trading}} \underbrace{\mathbb{E}[\hat{\mu} - \kappa \mid \hat{\mu} > \kappa]}_{\text{Net return if trade}} \tag{11}$$

where $\hat{\mu}$ is the expected return of an opportunity, obtained by

$$\hat{\mu} = \mu^d + q(a)\,(\alpha - \mu^d) \tag{12}$$

and $q(a) \in [0, 1]$ is increasing and concave with $q(0) = 0$. The function q measures how far attention moves the expected return from the prior toward the full-attention return. This reduced-form specification follows Gabaix (2014, 2019). It can also be read as the belief of a Bayesian trader who receives a signal, α, that is informative with a probability that rises with attention, in the spirit of Gervais and Odean (2001). The trader chooses the optimal attention by solving

$$\max_{a \geq 0} \ \theta W \ \Pr(\hat{\mu} > \kappa)\,\mathbb{E}[\hat{\mu} - \kappa \mid \hat{\mu} > \kappa] \ - \ c\,a \tag{13}$$

Attention Choice Writing $V(q, \kappa) \equiv \Pr(\hat{\mu} > \kappa)\,\mathbb{E}[\hat{\mu} - \kappa \mid \hat{\mu} > \kappa] = \mathbb{E}[(\hat{\mu} - \kappa)^+]$ for the expected net return per dollar, an interior optimum a^* satisfies the first-order condition

$$\theta W\, q'(a^*)\,\frac{\partial V}{\partial q} = c \tag{14}$$

The left-hand side is the marginal benefit of attention, the money at stake times the marginal value of attention per dollar, and the right-hand side is the marginal cost. Because the benefit of attention is proportional to the position θW while the cost is not, wealth does not cancel from the attention decision. This is the only place in the model where wealth matters, and it is the source of the cross-sectional predictions. The marginal value of attention per dollar is

$$\frac{\partial V}{\partial q} = \int_{\tau}^{\infty} (\alpha - \mu^d)\, dF(\alpha) \tag{15}$$

where $\tau \equiv \mu^d - (\mu^d - \kappa)/q(a)$. The trader trades if and only if $\alpha > \tau$, so τ is the threshold that separates the opportunities the trader takes from those the trader passes on in terms of full-attention return, α. Paying more attention has two effects. First, it moves the trader's expected return on every opportunity taken toward its full-attention return. As a result, the trader's average expected return over the opportunities taken rises. We interpret this as the trader capitalizing on good opportunities. Second, it raises the threshold τ. The intuition is that paying attention allows the trader to avoid the opportunities with the lowest full-attention returns (i.e., bad opportunities).

Attention and Turnover The trader trades if and only if $\hat{\mu} > \kappa$ or, equivalently, $\alpha > \tau$. Therefore, expected daily turnover, defined as expected trading volume divided by wealth, is

$$T = \frac{\mathbb{E}[\text{Volume}]}{W} = \frac{\theta W \cdot \Pr(\hat{\mu} > \kappa)}{W} = \theta \cdot \Pr(\alpha > \tau) = \theta\left[1 - F(\tau)\right] \tag{16}$$

Because the threshold τ rises with attention, turnover falls with attention. Figure 8 illustrates how attention shapes trading. It plots the distribution of the expected return $\hat{\mu}$ for a trader who pays high attention and for one who pays low attention. The vertical line marks the trading cost. A trader trades whenever the expected return is above the trading cost. With high attention, the trader's expected return closely tracks the full-attention return α, so only good opportunities are acted on. In contrast, with low attention, the trader's expected return stays close to the prior. Since the prior exceeds the trading cost, most opportunities look profitable, and the trader trades a lot. However, most of these trades are viewed as only slightly profitable since the prior is only a little above the cost. Low attention therefore produces many trades, but each has an expected return only marginally above the trading cost. This pattern is what allows the model to generate high turnover and a large response to a cost cut at the same time.

The model yields three predictions that match the pattern of investor responses we documented. Below, we describe the intuition and leave the derivations to Appendix D.

Prediction 1 (Lower-wealth traders pay less attention and have higher turnover) In the model, wealth affects how much attention a trader pays. Consider two traders who differ only in wealth. They face the same cost of attention c but different stakes θW. In Equation 14, a lower θW scales down the marginal benefit of attention at every level of a, while the marginal cost is unchanged, so

$$\frac{da^*}{dW} > 0 \tag{17}$$

The intuition is that paying attention improves the return on each dollar traded, so a larger position turns the same improvement into a larger dollar gain. A trader with less wealth has less to gain from paying attention but faces the same cost. As a result, a low-wealth trader optimally pays less attention. This prediction is consistent with three sets of evidence. Experimental subjects acquire more information when a correct decision pays more (Dean and Neligh 2023; Khaw, Li, and Woodford 2026). Investors with lower financial wealth pay less attention (Sicherman et al. 2016; Gargano and Rossi 2018; Dierick et al. 2019; Guiso and Jappelli 2020). Our own evidence in Figure F.10 shows that lower-wealth investors use more cognitive shortcuts.

Lower attention in turn raises turnover. By assumption, $q(a)$ is increasing in attention a. By Equation 16 and the definition of the threshold τ, turnover is decreasing in q and, therefore, in attention:

$$\frac{\partial T}{\partial q} = -\,\theta\, f(\tau)\, \frac{\partial \tau}{\partial q} = -\,\theta\, f(\tau)\, \frac{\mu^d - \tau}{q} < 0 \tag{18}$$

As discussed earlier, a trader who pays less attention trades more because their expected return stays close to a prior that exceeds the cost and they see most opportunities as profitable. Taken together, because of costly attention, lower-wealth traders choose to pay less attention when trading, which contributes to their higher turnover. This prediction is consistent with the evidence in Figure F.7 that lower-wealth investors have higher turnover before the reform.

Prediction 2 (Lower-wealth traders have higher trading sensitivity to transaction costs) We now show that traders with different attention respond differently to a change in trading costs. A decline in trading costs converts opportunities whose expected return falls just short of the cost into trades. By Equation 16, the percentage change in trading volume per unit change in the trading cost is

$$-\frac{d \ln T}{d\kappa} = \underbrace{\frac{h(\tau)}{q}}_{\text{fixed attention}} + \underbrace{h(\tau)\, \frac{\mu^d - \kappa}{q^2}\, q'(a^*)\, \frac{da^*}{d\kappa}}_{\text{through attention}} \tag{19}$$

where $h \equiv f/(1-F)$ is the hazard rate of F (i.e., the distribution of the full-attention return α). The hazard rate can be interpreted as the number of opportunities a small cost cut turns into trades, relative to the number of opportunities the trader would have taken anyway.

Our model predicts that the trading sensitivity to transaction costs decreases with attention. We derive this formally in Appendix D. The intuition is as follows. The first term of Equation 19 is the trading volume response to a cost change, holding attention fixed. It is higher for lower-wealth traders who pay less attention. A low-attention trader relies mostly on the prior when forming the expected return of an opportunity, but the prior is only slightly above the trading cost. As a result, this trader views many opportunities as marginally unprofitable, and a small cost cut turns them into trades. The second term is the trading volume response through the change in attention. As Prediction 3 later shows, a lower trading cost reduces the incentive to pay attention, which further increases turnover. This term is also larger for a low-attention trader.

This prediction matches the stronger volume response to the tax reform among lower-wealth investors documented in Figure 4. Together with Prediction 1, our model explains why these investors both trade the most before the reform and respond the most to a cost cut. This combination is hard to generate with typical explanations for high retail turnover alone. In our model, all traders can be equally overconfident, but their volume responses still differ because they differ in wealth and therefore in attention.

Prediction 3 (A cost cut lowers attention) To understand how optimal attention changes with the trading cost, we differentiate Equation 15 with respect to the cost and get

$$\frac{\partial^2 V}{\partial q\,\partial \kappa} = \frac{(\mu^d - \tau)\, f(\tau)}{q} > 0 \tag{20}$$

so the marginal benefit of attention increases with the trading cost. By Equation 14, a lower trading cost reduces the marginal benefit of attention while the marginal cost is unchanged. Therefore, optimal attention falls. The intuition is as follows. Attention is valuable because it lets the trader capitalize on high-potential trades and drop low-potential ones. When the trading cost falls, $\mu^d - \kappa$ rises, so every trade becomes more profitable even without effort, reducing the incentive to pay attention. This prediction is the model's counterpart to the regulatory concern discussed in Section 5.1. It rationalizes why inattentive trading behaviors become more common after the reform. Moreover, because those behaviors are associated with worse per-trade performance, day traders' gross alpha declines.

Taken together, the three predictions account for the two investor responses that lead to the decline in day traders' net portfolio returns after the reform. When trading becomes cheaper, investors with less wealth, who already trade the most and perform the worst, expand their trading the most. At the same time, every trader pays less attention to each trade, which contributes to lower per-trade performance.

While our model explains why investors respond to the reform the way they do, a natural follow-up question is why they respond so persistently that their responses end up more than offsetting the tax savings. In other words, why do investors not change their behavior with experience? In the model, this is because the trader's beliefs, namely the prior μ^d and the rule in Equation 12, stay fixed. We believe beliefs can be hard to change for several reasons. First, traders might have self-attribution bias. That is, they might credit gains to skill and losses to luck, which prevents beliefs from updating in the right direction (Daniel, Hirshleifer, and Subrahmanyam 1998; Gervais and Odean 2001). Second, daily returns might be too noisy for traders to infer their skill since telling a small average loss from no loss at all takes many trades (Merton 1980; Linnainmaa 2011). Third, traders might attend to their own performance selectively since people tend to look at their finances less after losses (Sicherman et al. 2016; Olafsson and Pagel 2017). In general, slow learning is consistent with Barber et al. (2020) and Chague, Silveira Bueno, and Giovannetti (2019), who find that many day traders in Taiwan and Brazil continue to trade despite a long record of losses.

6 The Market Impact of the Transaction Tax Reform

Beyond investor financial welfare, another key debate over lower retail trading costs concerns the potential market effects of increased speculation. Retail investors are typically characterized as noise traders (Barber, Odean, and Zhu 2009; Foucault, Sraer, and Thesmar 2011). Yet the market impact of noise trading has long been debated in the literature. Noise traders may increase volatility by trading on noise they mistake for information (Black 1986; Shleifer and Summers 1990; De Long et al. 1990a, 1990b; Campbell and Kyle 1993; Llorente et al. 2002) and harm liquidity by amplifying inventory risks for market makers (Ho and Stoll 1981; Grossman and Miller 1988; Hendershott and Menkveld 2014). However, they also introduce a countervailing effect that reduces volatility by providing liquidity either directly or indirectly (Kyle 1985; Glosten and Milgrom 1985; Ross 1989; Schwert and Seguin 1993; Song and Zhang 2005; Kaniel, Saar, and Titman 2008; Barrot, Kaniel, and Sraer 2016). Existing evidence on which effect dominates typically comes from variation other than transaction costs (Foucault, Sraer, and Thesmar 2011; Eaton et al. 2022). Because different sources of variation affect different traders, that evidence need not carry over to the trading that a cost change induces. Which effect dominates when the cost of retail speculation falls is therefore an open question.

To answer this question, we examine the impact of the day-trading tax cut on market quality. We first outline our empirical strategy for identifying the market impact (Section 6.1), and then present the results (Section 6.2).

6.1 Empirical Strategy

Our empirical strategy exploits an institutional feature of Taiwan's stock market: a subset of stocks was ineligible for day trading and therefore unaffected by the tax reform. Day trading eligibility is updated quarterly by the stock exchanges using predetermined rules based primarily on firms' listing tenure, market value, and financial status (e.g. profitability, book value). As a result, eligibility status is not influenced by the reform. Stocks eligible for day trading account for approximately 85% of all listed stocks.

We construct our sample by excluding penny stocks (prices below 1 NTD) and stocks experiencing eligibility changes during the pre-reform period, though such changes are rare. We classify common stocks into the treatment and control groups based on their eligibility status before the reform. Our treatment group includes stocks that were eligible for day trading, while the control group consists of stocks that were ineligible. As shown in Table 5, Panel A, control stocks are typically smaller, less liquid, and less profitable than treatment stocks.

Because the treatment and control groups differ substantially in observable characteristics, we adopt a matched difference-in-differences design to strengthen the plausibility of the parallel trends assumption—that changes in market quality for the two groups would have evolved similarly in the absence of the reform. Specifically, we apply entropy balancing based on three pre-period market-quality measures: order book depth, quoted spreads, and realized volatility (Hainmueller 2012; Hainmueller and Xu 2013). Since there are significantly more treatment stocks than control stocks, we reweight treatment stocks so that their pre-period means of these variables match those of the control group. To assess the robustness of our results to the matching procedure, we also conduct analyses using propensity score matching. In this alternative approach, we match each control stock to five treated stocks based on propensity scores estimated from a logistic regression.

Table 5, Panel B, reports summary statistics for the entropy-balanced sample. While we target balancing order book depth, quoted spreads, and realized volatility, the table shows that the balance of other stock characteristics also improves significantly, lending credibility to our matching approach.

Specification We estimate the effect of the tax reform using a standard difference-in-differences specification:

$$y_{st} = \beta \text{ Treat}_s \times \text{Post}_t + \gamma_s + \delta_t + \varepsilon_{st} \tag{21}$$

where s indexes stocks and t indexes trading days. y_{st} denotes the market quality measure for stock s on day t. Treat_s is an indicator equal to one if stock s was eligible for day trading at the time of the tax reform (April 28, 2017). Post_t is an indicator equal to one for days after the tax reform implementation. γ_s and δ_t denote stock and day fixed effects, respectively. Standard errors are clustered at the stock level. We test the parallel trends assumption using a dynamic difference-in-differences specification.

6.2 Results

We find that intraday liquidity increases and volatility decreases following the tax reform. Table 6 reports estimates of β from Equation 21. Relative to the control group, treated stocks' order book depth increases by 13% (column 1), and quoted spreads narrow by 11% (column 3), indicating enhanced liquidity and lower transaction costs. Importantly, realized volatility declines by 3.1 percentage points (column 5), representing an 8.3% reduction from the pre-reform mean. Columns (2), (4), and (6) present results based on our alternative matching procedure. The estimates are similar in magnitude and confirm the robustness of our findings to the matching approach. The dynamic difference-in-differences estimates for the entropy-balanced sample are shown in Figure 9. The absence of differential pre-trends across the outcome variables supports the parallel trends assumption.

These findings indicate that reducing day trading costs improves market quality and suggest the liquidity benefits of retail day trading, which is often considered noise trading, can outweigh its destabilizing effects. In other words, although day traders may increase the volatility of the aggregate demand curve, they simultaneously make it more elastic, which reduces price impact and volatility.

To test whether day traders indeed provide liquidity, we examine whether they exhibit contrarian behavior (i.e., buy when prices fall and sell when prices rise) in aggregate. Following Barrot, Kaniel, and Sraer (2016), we analyze how day traders' aggregate order imbalance within a day relates to realized intraday price changes. We divide a trading day into 10-minute intervals and compute the aggregate order imbalance among day traders as

$$\text{Order Imbalance}_{s,t,\tau} = \frac{\text{Shares Bought}_{s,t,\tau} - \text{Shares Sold}_{s,t,\tau}}{\text{Shares Bought}_{s,t,\tau} + \text{Shares Sold}_{s,t,\tau}} \tag{22}$$

where s indexes stocks, t indexes trading days, and τ indexes 10-minute intervals within day t. $\text{Shares Bought}_{s,t,\tau}$ and $\text{Shares Sold}_{s,t,\tau}$ denote the total number of shares purchased and sold, respectively, by all day traders in stock s during interval τ of day t.

Then, we estimate the following regression

$$\text{Order Imbalance}_{s,t,\tau} = \sum_{k=0}^{8} \beta_k \, \Delta \log(\text{Midquote})_{s,t,\tau-k} + \gamma_s + \delta_t + \varepsilon_{s,t,\tau} \tag{23}$$

where $\Delta \log(\text{Midquote})_{s,t,\tau-k}$ is the k-lagged 10-minute log change in the midquote price for stock s on day t (i.e., $\log(\text{Midquote})_{s,t,\tau-k} - \log(\text{Midquote})_{s,t,\tau-k-1}$), γ_s are stock fixed effects, and δ_t are day fixed effects. The coefficients β_k capture the relationship between past price changes and current order imbalance, with negative values indicating contrarian trading behavior.

Figure 10 plots the coefficient estimates β_k from Equation 23 for the post-reform period. Each point is the estimated response of order imbalance to the return over one 10-minute interval, and the error bars show 95% confidence intervals. The x-axis indexes the interval by its distance from the current period, from the return 90 to 80 minutes earlier on the left to the most recent interval, $[-10, 0]$, on the right.

The result suggests aggregate day traders in Taiwan are indeed contrarians. The coefficient estimates are predominantly negative, suggesting that past price decreases are associated with increases in current order imbalance (i.e. more net buying). The estimate for the [-10,0] interval implies that a 1-percentage-point price decrease over the past 10 minutes is associated with a 12.5-percentage-point increase in day traders' current order imbalance. This result provides empirical support for the interpretation that the increase in day trading activity reduces volatility through liquidity provision.[19]

Robustness While Panel B of Table 5 shows that most observable stock characteristics are balanced in our matched sample, one difference remains: profitability (measured by return on assets, ROA). To ensure our results are not driven by trends differentially affecting high-profitability firms, we conduct a placebo test by comparing high- and low-profitability firms within the treatment and control groups. Table F.14 shows that within the control group, there are no significant differential effects between high- and low-profitability firms. Within the treatment group, stocks with higher profitability actually experience a slight decrease in order book depth relative to lower-profitability stocks—opposite to what we would expect if profitability differences were driving our main results. These patterns confirm that the profitability imbalance between treatment and control stocks does not confound our findings.

Another potential threat to our identification is that stocks ineligible for day trading might be indirectly affected by the reform through trader substitution. Specifically, as the tax cut increases trading volume in eligible stocks, traders who originally traded ineligible stocks might be attracted to eligible stocks because of their higher trading activity. If this occurs, the control group might experience a decline in trading and market quality, biasing our estimates away from zero. While this substitution channel is not directly testable, we examine the time series of market-quality measures for treatment and control stocks after matching. Figure F.11 shows that the relative improvement in market quality of eligible stocks is not driven by a deterioration in the quality of ineligible stocks. This mitigates the concern that our results are driven by indirect effects of the reform.

External Validity Even though Taiwan's market is unusual in its retail-dominated trading and its lack of designated market makers, these features are unlikely to be the main driver of our market-level results. Evidence from the literature suggests that retail investors' trading behavior might be more relevant for their market impact. Eaton et al. (2022) show that retail trades from traditional brokerages in the US tend to be contrarian. Using brokerage outages as a natural experiment, the paper shows these retail trades can increase liquidity and decrease intraday volatility even in a market where institutional investors and professional market makers dominate. In contrast, trades from Robinhood, an app-based brokerage, tend to chase past returns and have the opposite effect. The day traders in our sample closely resemble the clients of traditional brokerages in demographics and account size. As shown earlier, they also trade in a contrarian fashion, a pattern documented in many countries (Kaniel, Saar, and Titman 2008; Kaniel et al. 2012; Kelley and Tetlock 2013; Barrot, Kaniel, and Sraer 2016; Boehmer et al. 2021). For these reasons, we believe that our findings could apply beyond the Taiwanese setting and speak to the effect of lowering trading costs.

19. We report only the post-reform estimates because the degree of contrarian trading is similar before and after the reform. Therefore, the effect of the reform comes from changing the amount of day trading, not how day traders respond to past returns.

7 Conclusion

This paper uses a 2017 reform in Taiwan that halved the transaction tax on day trading to show that cheaper speculation harms investors because of their responses while improving market quality. The harm to investors comes from two responses. First, day traders trade more. The increase is concentrated among investors with lower wealth, who lose the most on their trades. Second, day traders trade worse. Their gross returns fall after the tax cut. Together, these responses more than offset the savings from the lower tax. The average day trader ends up with lower net portfolio returns, and the smallest investors lose the most. To explain these responses, we propose a mechanism in which transaction costs serve a disciplinary role. We start by showing evidence consistent with investors paying less attention after day trading becomes cheaper, contributing to worse per-trade performance. We then develop a model of trading with costly attention that jointly explains the decline in attention after the tax cut and the concentration of the volume response among investors with lower wealth. At the market level, the additional day trading improves liquidity and reduces intraday volatility.

These findings highlight a central trade-off for policymakers: while reducing trading costs for retail investors can enhance market-level outcomes such as liquidity, it may also encourage excessive trading that harms individual investors, even though lower costs might appear to be a benefit on paper. This tension is directly relevant to policies that could affect retail investors' trading costs. One such policy is the design of financial transaction taxes. Many countries have adopted or revised these taxes over the past decade. However, most policy discussions and empirical research focus on their effects on market quality rather than on retail investors. Our results are also relevant for the regulation of payment for order flow (PFOF). Many countries have now implemented bans on PFOF, and this might reverse the decline in commission fees. Our paper thus helps shed light on the consequences for retail investors and the market.

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Figures

Figure 1: Percentage of Trading Days with Day Trades

This figure shows the average share of trading days with day trades, separately by year (2014–2017) and by investors' day trading status in the prior year. For 2017, only the post-reform period from April 28, 2017 is included. For each year, investors are grouped based on whether they executed at least one day trade in the previous calendar year (labeled "≥ 1 day trade (prior year)") or none ("0 day trades (prior year)"). Each bar shows, for investors in a given group and year, the average percentage of their trading days that included at least one day trade.

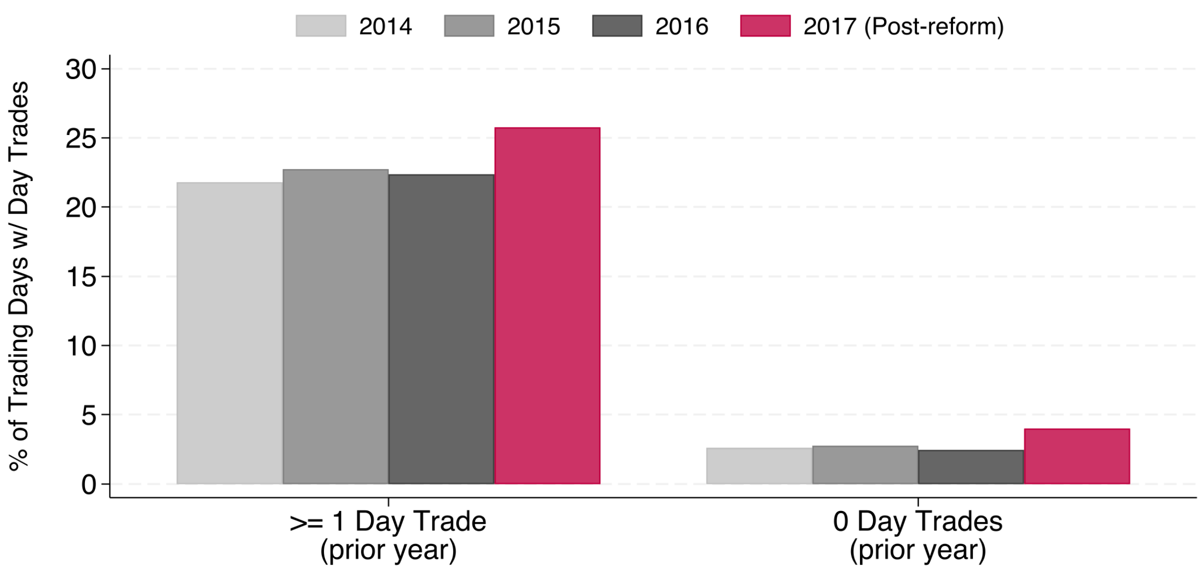
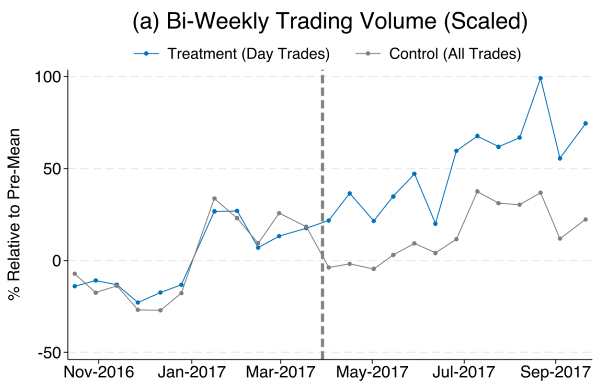


Figure 2: Effect of the Transaction Tax Reform on Day Trading Volume

This figure plots the effect of the transaction tax reform on day trading volume. Panel (a) shows the biweekly day trading volume for the treatment group and the total trading volume for the control group, each scaled by its pre-reform mean. Panel (b) displays coefficients from a dynamic difference-in-differences specification estimated at the biweekly level, based on the following equation:

$$\text{Volume}_{it} = \exp\left(\sum_{k\neq -1} \beta_k \text{ Treat}_i \times \mathbf{1}[t=k] + \gamma_i + \delta_t\right) \varepsilon_{it}$$

where i indexes investors and t indexes biweekly intervals. The outcome is day trading volume for the treatment group and total trading volume for the control group. Treat$_i$ is an indicator equal to 1 if investor i belongs to the treatment group. $\mathbf{1}[t=k]$ is an indicator for event time k relative to the tax reform date, with $k=-1$ omitted as the baseline. β_k captures the period-k treatment effect. γ_i and δ_t denote investor and time fixed effects, respectively. Coefficients are expressed as percent changes relative to baseline. Shaded areas represent 95% confidence intervals.



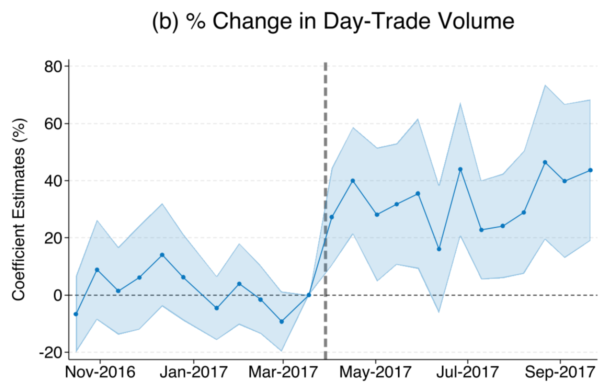


Figure 3: Effect of the Transaction Tax Reform on Non-Day Trading Volume

This figure plots the effect of the transaction tax reform on non-day trading volume. Panel (a) shows the biweekly non-day trading volume for the treatment group and the total trading volume for the control group, each scaled by its pre-reform mean. Panel (b) displays coefficients from a dynamic difference-in-differences specification estimated at the biweekly level, based on the following equation:

$$\text{Volume}_{it} = \exp\left(\sum_{k\neq -1} \beta_k \text{ Treat}_i \times \mathbf{1}[t=k] + \gamma_i + \delta_t\right) \varepsilon_{it}$$

where i indexes investors and t indexes biweekly intervals. The outcome is non-day trading volume for the treatment group and total trading volume for the control group. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group. $\mathbf{1}[t=k]$ is an indicator for event time k relative to the tax reform date, with $k=-1$ omitted as the baseline. β_k captures the period-k treatment effect. γ_i and δ_t denote investor and time fixed effects, respectively. Coefficients are expressed as percent changes relative to baseline. Shaded areas represent 95% confidence intervals.

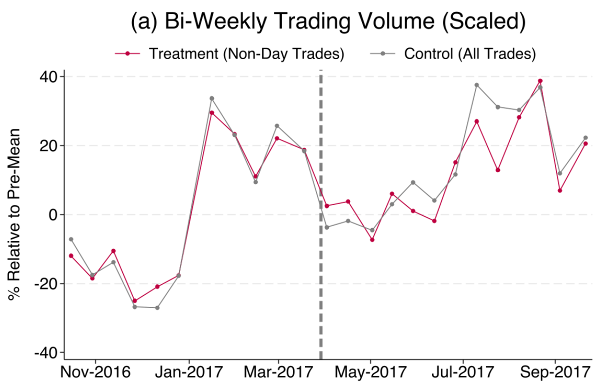


Figure 4: Heterogeneous Effect of the Transaction Tax Reform on Day Trading Volume

This figure plots heterogeneous effects of the transaction tax reform on day trading volume by investors' predicted wealth. The coefficients are from estimating the following equation separately for each wealth tercile g:

$$\text{Volume}_{it} = \exp(\beta_g \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t)\varepsilon_{it} \quad \text{where Wealth Tercile}_i = g$$

where Volume_{it} is trading volume for investor i in biweekly period t. For the treatment group, it refers to day trading volume; for the control group, it refers to total trading volume. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group. Post_t is an indicator equal to 1 for periods after April 28, 2017. Investors are grouped into terciles based on wealth, which is predicted from their combination of age, zip code, and gender. $g \in \{\text{Low, Medium, High}\}$. γ_i and δ_t denote investor and time fixed effects. Error bars indicate 95% confidence intervals.

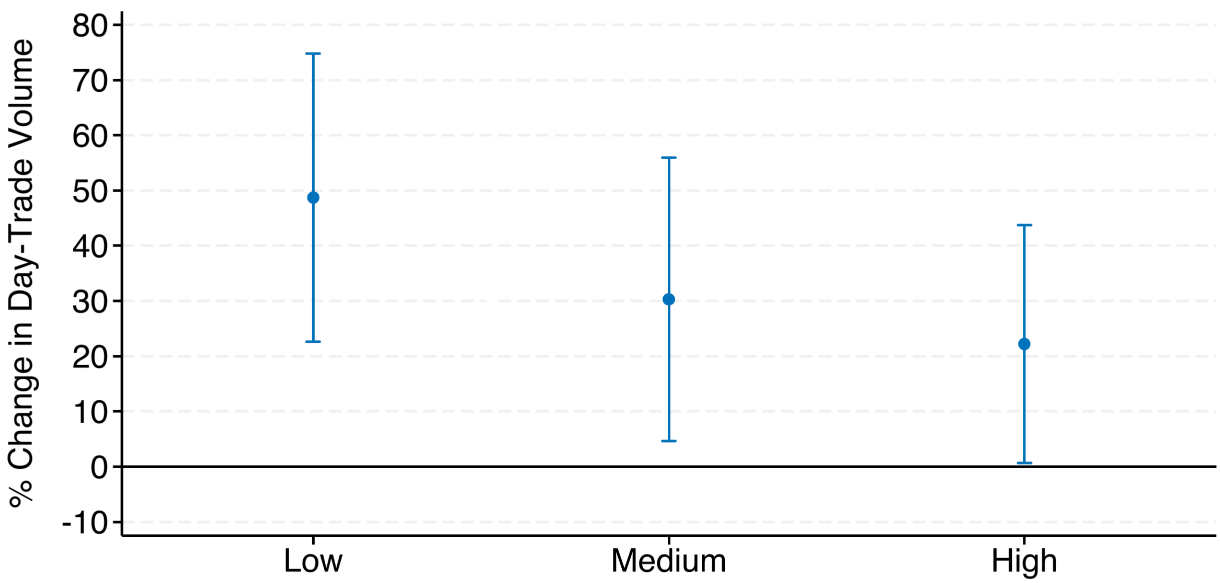
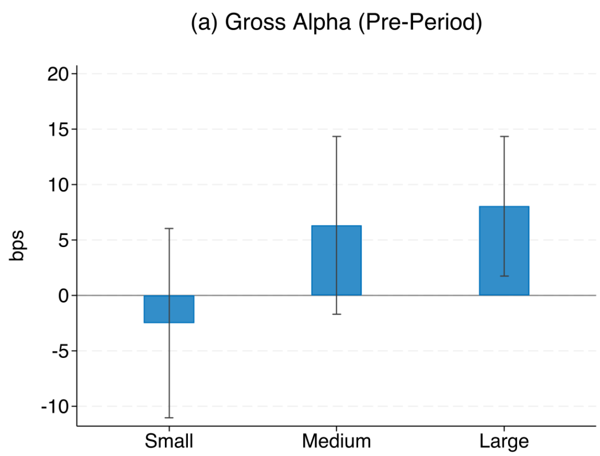


Figure 5: Ex Ante Performance of Day Traders by Sophistication

This figure plots day traders' alpha by wealth over the six months preceding the reform, where alpha is the intercept from regressing day-trading returns on the intraday market excess return. Day-trading returns are computed from *intended day trades*, which include all completed round-trip day trades together with any position-opening trades left open on the same day (Barber et al. 2014; Barber et al. 2020). Investors are grouped into terciles based on wealth, which is predicted from their combination of age, zip code, and gender. Panel (a) and Panel (b) plot alpha before and after accounting for transaction costs, respectively. Error bars represent 95% confidence intervals.



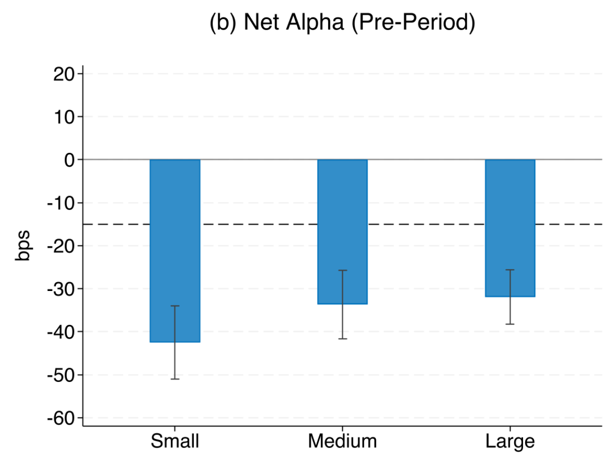


Figure 6: Changes in Propensity to Trade Stocks with Prior Extreme Returns

This figure plots coefficients from estimating the following equation:

$$\underset{\text{w/ extreme overnight returns X days ago}_{i,t}}{\text{Share of trading volume in stocks}} = \beta_X \ \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t + \varepsilon_{i,t}$$

where the outcome variable measures what percentage of investor i's trading volume on day t is concentrated in stocks that experienced extreme overnight returns X days prior. Stocks with extreme overnight returns are defined as those with absolute overnight returns in the top 5% of all stocks on a given day. The horizontal axis shows X ranging from -12 to 20 days, where negative values indicate future extreme returns and positive values indicate past extreme returns. For the treatment group, the outcome is calculated using day trades only; for the control group, it is calculated using all trades. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group. Post_t is an indicator equal to 1 for days after April 28, 2017. γ_i and δ_t denote investor and day fixed effects. Standard errors are clustered at the investor level. Solid error bars indicate 95% confidence intervals; dashed error bars indicate Bonferroni-corrected confidence intervals.

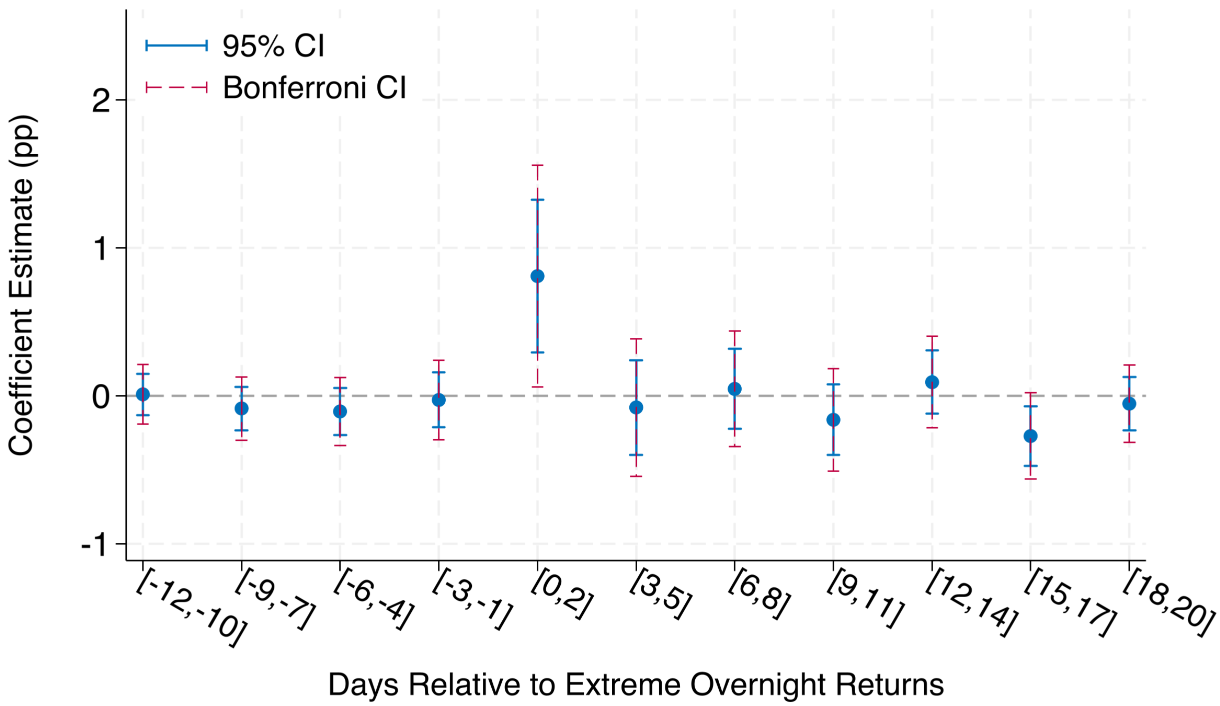


Figure 7: Decomposition of the Financial Impact of the Transaction Tax Reform

This figure decomposes the risk-adjusted contribution of day trading to portfolio returns into three components: (1) Mechanical tax-cut benefit (blue bars), (2) Performance-induced impact (green bars), and (3) Volume-induced impact (red bars). Net impact (black bars) sums all three components. Each component is calculated separately for investors grouped into wealth terciles based on predicted wealth. All values represent annualized changes in percentage points.

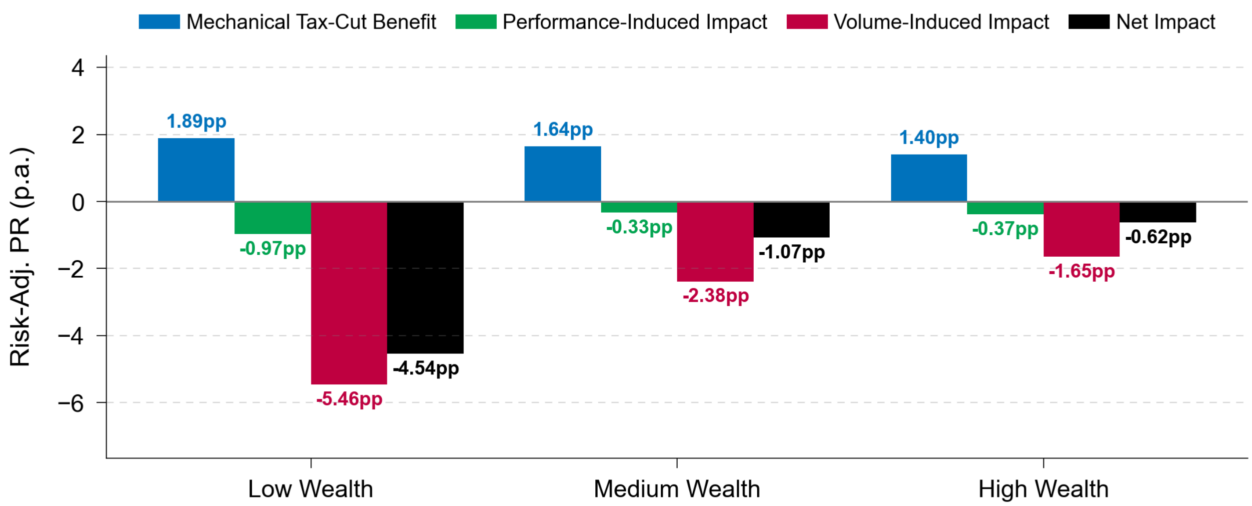


Figure 8: Distribution of Expected Returns under High and Low Attention

This figure plots the distribution of the trader's expected return $\hat{\mu}$ under high attention and under low attention. The vertical line marks the trading cost κ. The trader trades when the expected return exceeds the cost, and the shaded areas mark the opportunities that are traded. Parameter values are chosen for illustration.

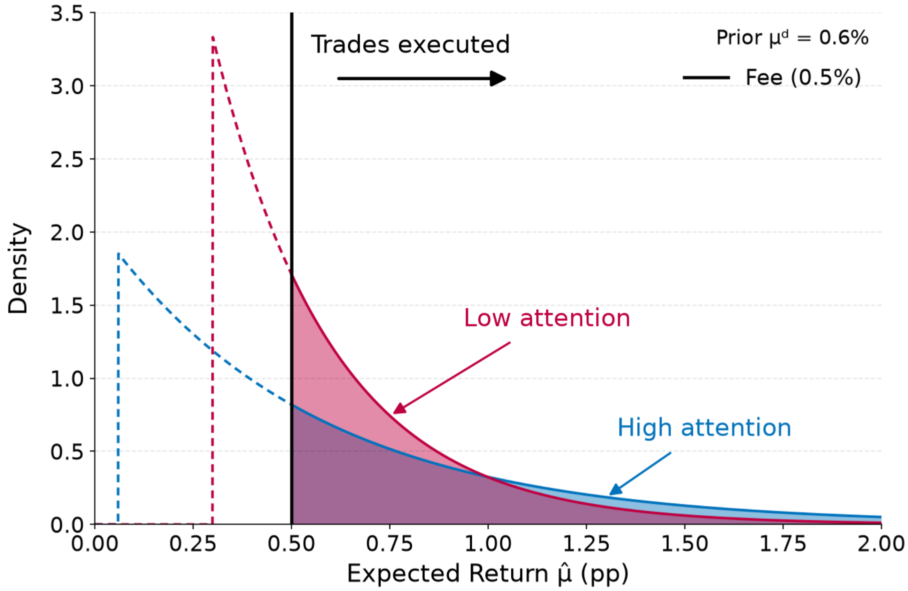


Figure 9: Effect of the Transaction Tax Reform on Market Quality

This figure plots coefficients from a dynamic difference-in-differences specification based on the following equation:

$$y_{st} = \sum_{k \neq -1} \beta_k \text{Treat}_s \times \mathbf{1}[t \in k] + \gamma_s + \delta_t + \varepsilon_{st}$$

where s indexes stocks and t indexes days. The outcome variable y_{st} is a daily market quality measure. Panel (a) presents results for Log($ Depth), the natural logarithm of dollar depth at the best bid and ask prices. Panel (b) presents results for Log(Quoted Spread), the natural logarithm of quoted spreads. Panel (c) presents results for Realized Volatility, the annualized standard deviation of 10-minute intraday returns measured in percentage points. Treat$_s$ is an indicator equal to 1 if stock s was eligible for day trading at the time of the tax reform. $\mathbf{1}[t \in k]$ is an indicator for event time k (in biweekly terms) relative to the tax reform date, with $k = -1$, the two weeks before the tax reform, omitted as the baseline. β_k captures the period-k treatment effect. γ_s and δ_t denote stock and day fixed effects, respectively. The sample uses entropy-balanced weights so that the first moments of order book depth, quoted spread, and realized volatility are comparable between treatment and control groups in the pre-period. Standard errors are clustered at the stock level. Shaded areas represent 95% confidence intervals.

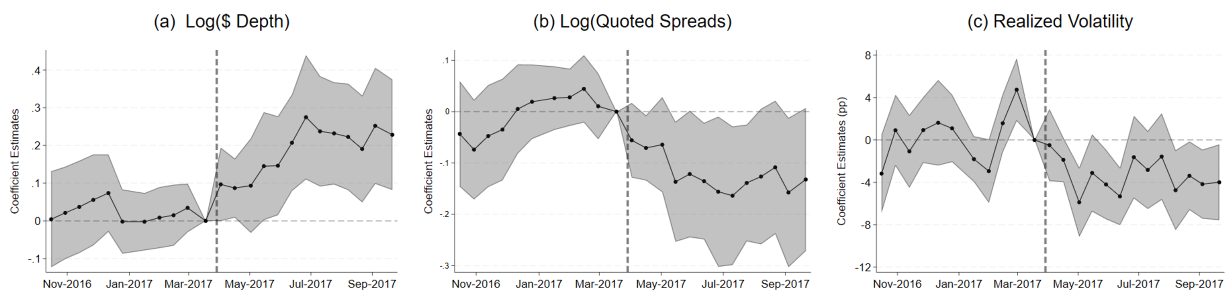
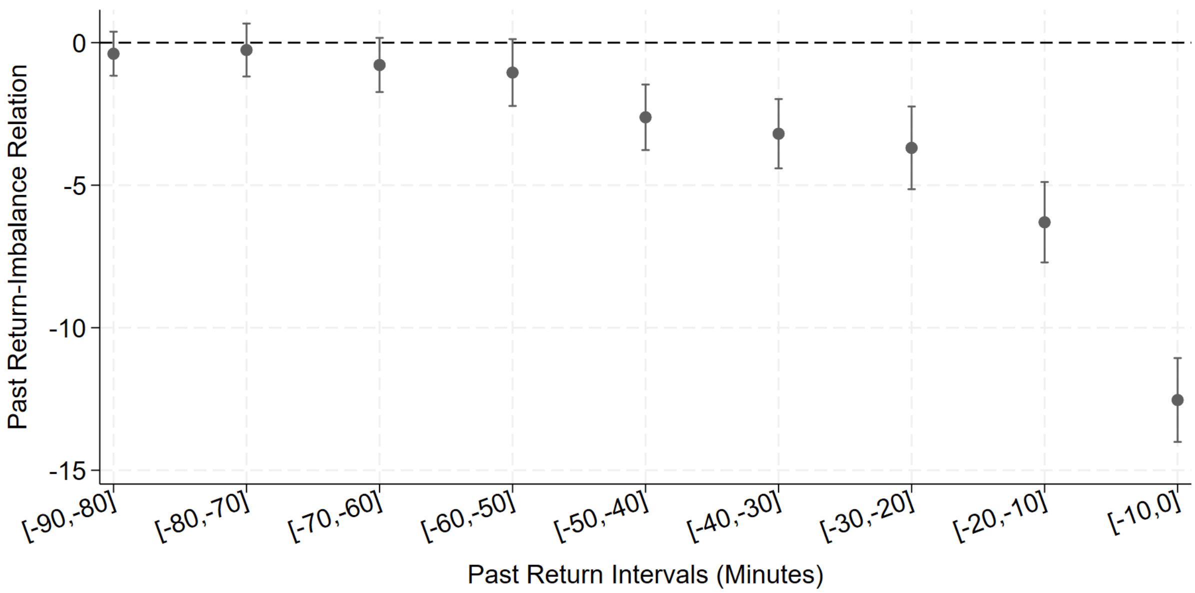


Figure 10: Contrarian Behavior of Aggregate Day Traders

This figure plots coefficient estimates from estimating Equation 23 during the post-reform period:

$$\text{Order Imbalance}_{s,t,\tau} = \sum_k \beta_k \, \Delta \log(\text{Midquote})_{s,t,\tau-k} + \gamma_s + \delta_t + \varepsilon_{s,t,\tau}$$

where Order Imbalance$_{s,t,\tau}$ is the aggregate order imbalance among day traders for stock s in 10-minute interval τ of day t, calculated as (Shares Bought $-$ Shares Sold)/(Shares Bought + Shares Sold). $\Delta \log(\text{Midquote})_{s,t,\tau-k}$ is the k-lagged 10-minute log change in the midquote price. The horizontal axis shows the time intervals in minutes before the current period, ranging from 90-80 minutes ago to the most recent 10-minute interval ([-10,0]). Each point represents the estimated coefficient β_k for returns over a specific 10-minute interval. γ_s and δ_t denote stock and day fixed effects, respectively. Error bars indicate 95% confidence intervals. The post-reform period is from April 28, 2017 to October 31, 2017.



Tables

Table 1: Effect of the Transaction Tax Reform on Trading Volume

This table reports the results from estimating Equation 5:

$$\text{Volume}_{it} = \exp(\beta \ \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t)\varepsilon_{it}$$

where i indexes investors and t indexes biweekly time intervals. The dependent variable is trading volume. For the control group, the outcome is total trading volume in all columns. For the treatment group, the outcome in Column (1) is total trading volume, in Column (2) is day trading volume, and in Column (3) is non-day trading volume. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group, and Post_t is an indicator equal to 1 for periods after April 28, 2017. γ_i and δ_t denote investor and time fixed effects. Standard errors are clustered at the investor level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Volume		
	(1) All vs. All Trades	(2) Day vs. All Trades	(3) Non-day vs. All Trades
Treat × Post	0.05	0.28***	-0.02
	(0.03)	(0.06)	(0.03)
Observations	425,376	425,184	422,472
Pseudo R^2	0.82	0.82	0.80
Investor FE	✓	✓	✓
Time FE	✓	✓	✓
Cluster	Investor	Investor	Investor

Table 2: Effect of the Transaction Tax Reform on Performance

This table reports the results from estimating Equation 8:

$$y_{it} = \beta \text{ Treat}_i \times \text{Post}_t + \lambda_1 \text{ Treat}_i \times R_t^m + \lambda_2 \text{ Treat}_i \times \text{Post}_t \times R_t^m + \gamma_i + \delta_t + \varepsilon_{it},$$

where y_{it} is the gross return per dollar traded by investor i on day t, in basis points. R_t^m is the intraday market return in excess of the risk-free rate on day t. In Column (1), the outcome is defined as gross returns per dollar traded from all trades for both treatment and control groups. In Column (2), the outcome is restricted to day trades for the treatment group and remains all trades for the control group. In Column (3), the outcome is restricted to non-day trades for the treatment group and remains all trades for the control group. For day trades, we compute gross returns using actual execution prices at both purchase and sale. For non-day trades, where positions remain open at day's end, we approximate gross returns by comparing the execution price of the initial trade to that day's closing price. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group. Post_t is an indicator equal to 1 for days after April 28, 2017. γ_i and δ_t denote investor and day fixed effects. Standard errors are double-clustered at the investor and day levels and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Gross Returns per $ Traded (bps)		
	(1) All vs. All Trades	(2) Day vs. All Trades	(3) Non-day vs. All Trades
Treat × Post	-2.44*	-5.51***	0.41
	(1.30)	(2.09)	(0.67)
Observations	1,642,862	634,108	1,513,930
Adj. R^2	0.04	0.18	0.02
Investor FE	✓	✓	✓
Day FE	✓	✓	✓
Market Controls	✓	✓	✓
Cluster	Investor, Day	Investor, Day	Investor, Day

Table 3: Effect of the Transaction Tax Reform on the Use of Cognitive Shortcuts

The table reports results from estimating the following equation:

$$y_{it} = \beta \ \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t + \varepsilon_{it}$$

where y_{it} is the share of round-number limit orders (in percentage points) submitted by investor i on trading day t. Round-number orders are defined as limit orders placed at prices ending in .00 or .50. For the treatment group, the outcome is calculated using only limit orders associated with day trades; for the control group, it is calculated using all limit orders. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group. Post_t is an indicator equal to 1 for days after April 28, 2017. γ_i and δ_t denote investor and day fixed effects, respectively. Standard errors are clustered at the investor level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Share of Round-Number Orders (pp)
	(1)
Treat × Post	1.88***
	(0.31)
Observations	719,027
Adj. R^2	0.29
Investor FE	✓
Day FE	✓
Cluster	Investor

Table 4: Effect of the Transaction Tax Reform on Monitoring Effort

The table reports results from estimating the following equation:

$$y_{it} = \beta \text{ Treat}_i \times \text{Post}_t + \gamma_i + \delta_t + \varepsilon_{it}$$

where y_{it} is the outcome variable for investor i on day t. Column (1) examines the log of mean order idle time, defined as the time (in seconds) between order submission and order execution, cancellation, or market close. Column (2) examines the probability of order modification or cancellation (in percentage points), defined as the percentage of limit orders that are modified or canceled before execution. For the treatment group, these measures are calculated using only limit orders associated with day trades; for the control group, they are calculated using limit orders from all trades. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group. Post_t is an indicator equal to 1 for days after April 28, 2017. γ_i and δ_t denote investor and day fixed effects. Standard errors are clustered at the investor level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Log(Order Idle Time)	Prob. of Modification (pp)
	(1)	(2)
Treat × Post	0.04***	-0.73***
	(0.01)	(0.24)
Observations	621,079	621,079
Adj. R^2	0.41	0.58
Investor FE	✓	✓
Day FE	✓	✓
Cluster	Investor	Investor

Table 5: Summary Statistics of Treatment and Control Stocks

This table reports summary statistics for stocks eligible for day trading (treatment group) and stocks ineligible for day trading (control group). Panel A presents statistics for the unmatched sample. Panel B presents statistics after entropy balancing, where stocks are reweighted so that the first moments of order book depth, quoted spread, and realized volatility are comparable between the two groups in the pre-period. For each stock, we first calculate the time-series average of each variable over the six-month pre-reform period (November 2016 to April 2017), then report the cross-sectional mean and standard deviation across stocks. Market cap. and Total asset are measured in billions of USD. Volume is average daily trading volume in millions of USD. Price is the average stock price in USD. Market beta is estimated using daily returns over the past five years. Depth is the average dollar liquidity at the best bid and ask prices. Quoted spread is the time-weighted average spread in basis points. Realized volatility is the annualized standard deviation of 10-minute intraday returns. Institutional ownership is the percentage of shares held by institutional investors. Day trading share is the percentage of total trading volume from day trades. ROA is return on assets.

Panel A. Before Matching

	Treatment stocks			Control stocks		
	N(Stocks)	Mean	SD	N(Stocks)	Mean	SD
Market Cap. (bn USD)	1330	0.53	1.47	189	0.06	0.11
Volume (mn USD)	1330	1.90	3.81	189	0.18	0.42
Price (USD)	1330	1.47	1.78	189	0.93	1.24
Market Beta	1330	0.87	0.26	189	0.75	0.31
Depth (K USD)	1330	61.64	141.89	189	9.64	13.47
Quoted Spread (bps)	1330	62.20	55.29	189	192.83	135.36
Realized Volatility (pp)	1330	26.37	9.56	189	37.48	10.01
Institutional Ownership (%)	1330	11.83	14.58	189	5.86	10.59
Day Trading Share (%)	1330	7.40	5.62	0	.	.
Total Asset (bn USD)	1330	1.45	6.06	189	0.15	0.72
Net Income (mn USD)	1330	42.96	132.21	189	0.79	33.39
ROA (%)	1330	9.40	7.34	189	2.42	9.45

Panel B. After Matching

	Treatment stocks			Control stocks		
	N(Stocks)	Mean	SD	N(Stocks)	Mean	SD
Market Cap. (bn USD)	1330	0.07	0.09	189	0.06	0.11
Volume (mn USD)	1330	0.32	0.76	189	0.18	0.42
Price (USD)	1330	0.84	0.70	189	0.93	1.24
Market Beta	1330	0.73	0.27	189	0.75	0.31
Depth (K USD)	1330	9.61	10.85	189	9.64	13.47
Quoted Spread (bps)	1330	192.85	157.08	189	192.83	135.36
Realized Volatility (pp)	1330	37.81	10.40	189	37.48	10.01
Institutional Ownership (%)	1330	5.98	11.16	189	5.86	10.59
Day Trading Share (%)	1330	7.33	4.71	0	.	.
Total Asset (bn USD)	1330	0.13	0.53	189	0.15	0.72
Net Income (mn USD)	1330	3.33	9.84	189	0.79	33.39
ROA (%)	1330	5.48	6.47	189	2.42	9.45

Table 6: Effect of the Transaction Tax Reform on Market Quality

This table reports the results from estimating Equation 21:

$$y_{st} = \beta \text{ Treat}_s \times \text{Post}_t + \gamma_s + \delta_t + \varepsilon_{st}$$

where s indexes stocks and t indexes trading days. Treat_s is an indicator equal to 1 if stock s was eligible for day trading at the time of the tax reform. Post_t is an indicator equal to 1 for days after April 28, 2017. γ_s and δ_t denote stock and day fixed effects. The dependent variables are measures of market quality: Log($ Depth) is the natural logarithm of dollar depth at the best bid and ask prices; Log(Quoted Spreads) is the natural logarithm of quoted spreads; and Realized Volatility is the annualized standard deviation of 10-minute intraday returns, measured in percentage points. Columns (1), (3), and (5) present results after entropy balancing, where stocks are reweighted so that the first moments of order book depth, quoted spread, and realized volatility are comparable between treatment and control groups in the pre-period. Columns (2), (4), and (6) present results using propensity score matching (PSM), where each control stock is matched to its five nearest neighbors in the treatment group. Standard errors are clustered at the stock level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Log($ Depth)		Log(Quoted Spreads)		Realized Volatility (pp)	
	(1) Matched Entropy	(2) Matched PSM	(3) Matched Entropy	(4) Matched PSM	(5) Matched Entropy	(6) Matched PSM
Treat × Post	0.13**	0.14**	-0.11**	-0.14**	-3.13**	-2.52*
	(0.06)	(0.06)	(0.06)	(0.05)	(1.26)	(1.33)
Observations	364,703	125,945	363,911	125,581	364,703	125,945
Adj. R^2	0.69	0.67	0.77	0.74	0.13	0.12
Stock FE	✓	✓	✓	✓	✓	✓
Day FE	✓	✓	✓	✓	✓	✓
Cluster	Stock	Stock	Stock	Stock	Stock	Stock

A Sample Coverage Analysis

Table A.1 compares trade characteristics between our sample and those of Barber et al. (2014) and Barber et al. (2020). Although their data covers a different period (1992–2006 vs. our 2012–2017) and includes the full population of retail investors (as opposed to one brokerage), trading patterns are remarkably similar. The average trade size in their sample is NTD 190,656, compared to NTD 227,702 in ours (Row 1). Among day traders—defined as investors who execute at least one day trade in a given year—the average number of trading days per year is 42.9 in their sample and 70.2 in ours (Row 2). The number of days with actual day trading activity is also comparable: 12.9 days in their data versus 15.1 days in ours (Row 3).

Panel (a) of Figure A.1 shows that the cross-sectional correlation between daily sample volume and aggregate retail volume is consistently around 90% throughout our sample period, with no significant change around the tax reform. Moreover, since our study relates to day trading, Panel B shows that the biweekly retail day trading share (defined as the ratio of retail day-trade volume to total retail volume) measured in our sample closely tracks that of the aggregate market.[20] Importantly, both series witness a substantial increase in day trading activity after the tax reform.

Table A.1: Comparison with Barber et al (2014, 2020)

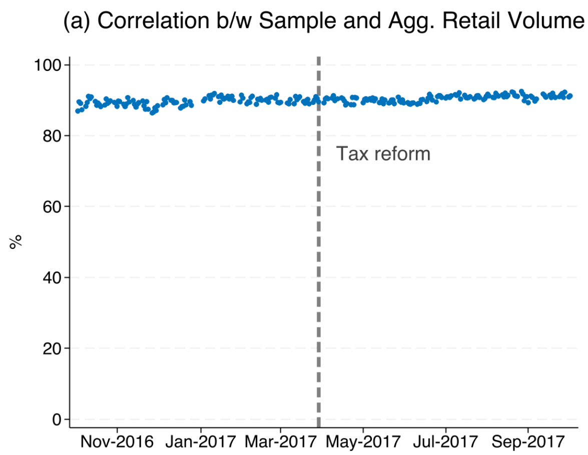
This table compares trading characteristics between our sample and the full population samples from Barber et al. (2014) and Barber et al. (2020). BLLO refers to Barber, Lee, Liu, and Odean. Their sample covers the period 1992-2006 and includes all retail investors in Taiwan, while our sample covers 2012-2017 and includes investors from a single major Taiwanese brokerage. Row 1 reports the average trade size in New Taiwan Dollars (NTD). Rows 2 and 3 focus on day traders, defined as investors who execute at least one day trade in a given year. Row 2 reports the average number of trading days per year, and Row 3 reports the average number of days with actual day trading activity per year.

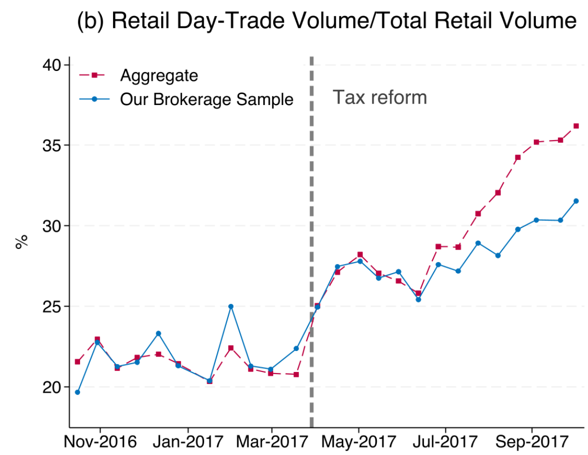
	BLLO's sample (1992-2006)	Our sample (2012-2017)
Average investor:		
Trade size (NTD)	190,656	227,702
Average day trader:		
# Trading days	42.9	70.2
# Day trading days	12.9	15.1

20. Since we only observe total day trading volume for the aggregate market, we approximate aggregate retail day trading volume by multiplying total day trading volume by 91.74%, according to Taiwan Ministry of Finance (2018).

Figure A.1: Comparison of Sample and Aggregate Volume

This figure compares trading patterns between our brokerage sample and the aggregate Taiwan market. Panel (a) plots the daily cross-sectional correlation between log trading volume in our sample and log aggregate retail trading volume in Taiwan over time. Panel (b) shows the biweekly day trading share, defined as the ratio of retail day trading volume to total retail trading volume, for both our sample (blue line) and the aggregate market (red line). For the aggregate market, retail day trading volume is approximated by multiplying total day trading volume by 91.74%, according to Taiwan Ministry of Finance (2018). The vertical dashed line indicates the implementation of the transaction tax reform on April 28, 2017.





B DiD Estimator with Different Outcome Variables

In this section, we discuss the theoretical validity of using different outcome variables for treatment and control groups in a DiD setting. Consider a two-period setting, $t \in \{pre, post\}$, and let Y_t denote any outcome variable. Since our outcomes represent day trades, we use Y_t^{Day} for clarity. The parameter of interest is the average treatment effect on the treated (ATT) in the post-period:

$$\tau = \mathbb{E}[Y_{post}^{Day}(1) - Y_{post}^{Day}(0) \mid D = 1] \tag{B.1}$$

where $Y_{post}^{Day}(1), Y_{post}^{Day}(0)$ denote the potential day-trade outcomes in the post-period with and without treatment, respectively. $D = 1$ indicates membership in the treatment group. By adding and subtracting $\mathbb{E}[Y_{pre}^{Day}(0) \mid D = 1]$, we get

$$\tau = \underbrace{\mathbb{E}[Y_{post}^{Day}(1) - Y_{pre}^{Day}(0) \mid D = 1]}_{\text{Observed}} - \underbrace{\mathbb{E}[Y_{post}^{Day}(0) - Y_{pre}^{Day}(0) \mid D = 1]}_{\text{Unobserved}} \tag{B.2}$$

The first term is observable under the no-anticipation assumption, while the second term—the counterfactual trend for the treatment group—is not. To address this, we typically identify a control group whose observed trend in the same outcome provides a valid counterfactual for what the treatment group would have experienced absent the intervention. This is the standard parallel trends assumption:

$$\mathbb{E}[Y_{post}^{Day}(0) - Y_{pre}^{Day}(0) \mid D = 1] = \mathbb{E}[Y_{post}^{Day}(0) - Y_{pre}^{Day}(0) \mid D = 0] \tag{B.3}$$

In our setting, because all day trading was subject to the tax cut, there is no control group whose day trading was unaffected by the reform. However, in principle, the parallel trends assumption does not require the same outcome variable for both groups. Instead, we can assume that the change in the outcome from all trades of the carefully chosen control group provides a valid counterfactual for the change in the day-trade outcome for the treatment group:

$$\mathbb{E}[Y_{post}^{Day}(0) - Y_{pre}^{Day}(0) \mid D = 1] = \mathbb{E}[Y_{post}^{Total}(0) - Y_{pre}^{Total}(0) \mid D = 0] \tag{B.4}$$

More generally, Y_t^{Total} can represent any outcome variable. What ultimately matters is whether the chosen control group's trend in that outcome provides a plausible counterfactual for the treatment group's unobserved trend.

Having explained the validity of using different outcome variables for the treatment and control groups, we now turn to how the parallel trends assumption can be rationalized in our setting. Take Equation 4 as an example: we assume that the percentage change in day trading volume for the treatment group would equal the percentage change in total trading volume for the control group. While we view this assumption as plausible given that our control group consists of frequent traders who likely share the speculative motives of day traders, it can also be expressed in terms of two underlying conditions that are easier to interpret and, in part, test directly with the data. The first condition relates day trading and total trading within the treatment group, while the second is a standard parallel trends assumption applied to total trading volumes. Together, these two conditions jointly imply Equation 4.

The first condition is what we call the fixed ratio assumption: absent the tax reform, the ratio of average day trading volume to total trading volume for the treatment group remains constant:

$$\frac{\mathbb{E}[\text{Volume}_{i,\, pre}^{Day}(0)|D_i = 1]}{\mathbb{E}[\text{Volume}_{i,\, pre}^{Total}(0)|D_i = 1]} = \frac{\mathbb{E}[\text{Volume}_{i,\, post}^{Day}(0)|D_i = 1]}{\mathbb{E}[\text{Volume}_{i,\, post}^{Total}(0)|D_i = 1]} \tag{B.5}$$

Economically, this implies that aggregate day trading and non-day trading volumes for the treatment group respond proportionally to general shocks affecting trading activity. It rules out shocks, such as changes in market conditions or investor sentiment, that would differentially affect day trading during the analysis window (six months before and after the reform), apart from the tax cut itself. Two pieces of evidence support this assumption. First, Panel (a) of Figure F.3 plots the ratio of day trading volume to total trading volume for the treatment group. The ratio is stable in the six months before the reform and increases sharply only after the tax cut. This stability is reassuring. However, it cannot rule out a shock that differentially affects day trading occurrs at the same time as the reform. Panel (b) helps address this issue. It plots the same ratio separately for common stocks and ETFs. Recall, the tax cut applies only to common stocks, so the tax on ETF day trades did not change (Section 2.2). If a shock other than the tax cut had differentially affected day trading at the time of the reform, the ratio would have increased for both common stocks and ETFs. Instead, the ratio increases only for common stocks. The ratio for ETFs does not increase after the reform and, if anything, declines. This pattern supports our assumption that no shock other than the tax reform differentially affected day trading during this period.

The second assumption is that, absent the tax reform, the percentage change in total trading volume would have been the same for the treatment and control groups. This is the standard parallel trends assumption applied to the same outcome variable for both groups:

$$\frac{\mathbb{E}[\text{Volume}_{i,\, post}^{Total}(0)|D_i = 1]}{\mathbb{E}[\text{Volume}_{i,\, pre}^{Total}(0)|D_i = 1]} = \frac{\mathbb{E}[\text{Volume}_{i,\, post}^{Total}(0)|D_i = 0]}{\mathbb{E}[\text{Volume}_{i,\, pre}^{Total}(0)|D_i = 0]} \tag{B.6}$$

This assumption requires that, in the absence of the reform, both groups would have experienced the same proportional change in total trading volume over time, ruling out shocks that differentially affect the trading activity of either group. Figure F.4 provides empirical support for this assumption. Panel (a) plots the biweekly total trading volume for the treatment and control group, each scaled by its respective pre-period mean. Panel (b) reports coefficients from a dynamic difference-in-differences specification at a biweekly frequency, converted into percentage terms for ease of interpretation. The figures show that total trading volume for the treatment and control groups evolves in parallel prior to the reform, with slight divergence occurring only after the reform.

Combining Equations B.5 and B.6 directly yields Equation 4:

$$\frac{\mathbb{E}[\text{Volume}_{i,\ post}^{Day}(0)|D_i = 1]}{\mathbb{E}[\text{Volume}_{i,\ pre}^{Day}(0)|D_i = 1]} \overset{(B.5)}{=} \frac{\mathbb{E}[\text{Volume}_{i,\ post}^{Total}(0)|D_i = 1]}{\mathbb{E}[\text{Volume}_{i,\ pre}^{Total}(0)|D_i = 1]} \overset{(B.6)}{=} \frac{\mathbb{E}[\text{Volume}_{i,\ post}^{Total}(0)|D_i = 0]}{\mathbb{E}[\text{Volume}_{i,\ pre}^{Total}(0)|D_i = 0]}$$

Besides the evidence that directly supports each condition, the two conditions have a joint implication that we can test. If both hold and the tax cut does not affect non-day trading, the effect on the total trading volume of the treatment group should equal the effect on day trading volume multiplied by the day trading share. Table 1 confirms this prediction. The 5% increase in total trading volume in column (1) is close to the 6% implied by the 32% increase in day trading volume in column (2) and the 20% baseline day trading share.

C Simple Model of Attention Allocation

In this section, we develop a stylized model of attention allocation in the spirit of Gabaix (2014, 2019) and Bastianello, Decaire, and Guenzel (2024). Its central prediction is that, when investors have fewer cognitive resources to spend, they cut attention unevenly: most heavily on the variables they regard as least salient, while preserving most of their attention on those they regard as most salient.

Consider an investor who treats K variables, $\{x_i\}_{i=1}^{K}$, as relevant for returns. None of them is observed perfectly. Their perception of each variable is a convex combination of their prior and its true value, with the weight on the true value reflecting how much attention they devote to it (Gabaix 2019):

$$x_i^s = a_i x_i + (1 - a_i) x_i^d, \tag{C.1}$$

where x_i^s is the perceived value of variable i, $a_i \in [0, 1]$ is the attention paid to it, and x_i^d is the prior. The expression can be read as the posterior mean of a Bayesian update following a signal about x_i, with a_i governing the signal's precision.

To form return expectations, the investor weights each variable by m_i, their belief about its relevance for returns. Subjective expected returns are then

$$r^s(\boldsymbol{a}) = \sum_{i=1}^{K} m_i x_i^s = \sum_{i=1}^{K} a_i m_i x_i. \tag{C.2}$$

Attention is costly, and the investor's cognitive resources are limited. They therefore allocate attention so as to bring their subjective forecasts as close as possible to the full-attention benchmark, minimizing the mean squared deviation of their forecasts subject to a linear budget on attention costs:

$$\min_{\{a_i\}} \mathbb{E}\left[(r^s(\boldsymbol{\iota}) - r^s(\boldsymbol{a}))^2 \right] \quad \text{s.t.} \quad \sum_{i=1}^{K} a_i c_i \le C,$$

where c_i is the marginal cost of attention to variable i and C is the investor's total cognitive budget.

To keep the analysis tractable, we normalize priors to zero ($x_i^d = 0$ for all i) and assume the variables are independent, with $x_i \sim N(0, \sigma_i^2)$. The first-order condition with respect to a_i is

$$a_i^* \;=\; \max\left\{0,\; 1 - \frac{\lambda c_i}{2 m_i^2 \sigma_i^2}\right\}, \tag{C.3}$$

where λ is the multiplier on the attention budget, determined by the budget constraint over the variables that receive positive attention. This shows that attention to variable i rises when the investor believes it matters more for returns (larger m_i), when it is more volatile and so each signal is more informative (larger σ_i), or when it is cheaper to monitor (smaller c_i).

We can now formalize the claim stated at the outset by deriving how the investor reallocates attention as their cognitive resources C decline. Substituting the binding budget constraint into the first-order condition to eliminate λ yields, when all variables receive positive attention, the closed form

$$a_i^* = 1 - \frac{c_i}{m_i^2 \sigma_i^2} \cdot \frac{\sum_{j=1}^{K} c_j - C}{\sum_{j=1}^{K} c_j^2/(m_j^2 \sigma_j^2)}. \tag{C.4}$$

Differentiating with respect to C and taking the ratio for any two variables gives

$$\frac{\partial a_i / \partial C}{\partial a_j / \partial C} = \frac{c_i/(m_i^2 \sigma_i^2)}{c_j/(m_j^2 \sigma_j^2)}. \tag{C.5}$$

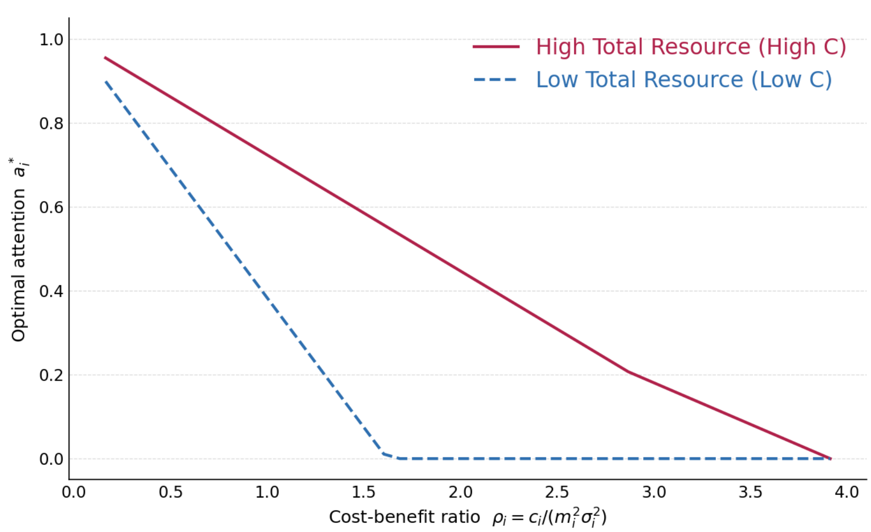
The ratio confirms the prediction. As C falls, the attention the investor withdraws from a variable scales with its cost-benefit ratio: variables that they view as less important (lower m_i), that are less volatile (lower σ_i), or that are more costly to track (higher c_i) lose attention the fastest.

Figure C.1 illustrates this graphically. The horizontal axis plots the cost-benefit ratio $\rho_i \equiv c_i/(m_i^2 \sigma_i^2)$ of each variable, which collapses the three drivers into a single measure of salience: a lower ρ_i corresponds to a variable the investor views as more important, more volatile, or cheaper to track. The vertical axis plots optimal attention a_i^*, with the solid line showing the allocation under a high cognitive budget and the dashed line under a low one.

Two patterns stand out. Attention to high-salience variables (low ρ_i) stays near one regardless of the budget. Attention to low-salience variables (high ρ_i) falls sharply as the budget tightens, dropping all the way to zero. In other words, when cognitive resources decline, the investor disproportionately reduces attention to low-salience variables.

Figure C.1: Optimal Attention Allocation Under High and Low Cognitive Budgets

The figure plots optimal attention a_i^* against the cost-benefit ratio $\rho_i \equiv c_i/(m_i^2 \sigma_i^2)$, computed from the closed-form solution in Equation C.5, with a_i^* truncated at zero to enforce non-negative attention. The solid line corresponds to a high cognitive budget C; the dashed line to a low cognitive budget. Parameter values are chosen for illustration.



These predictions extend to the retail trading biases discussed in Section 5.1. We interpret them as manifestations of limited attention. First, retail investors disproportionately submit limit orders at round-number prices. This may occur because round numbers are easier to process. As a result, they are less costly to attend to. Second, retail investors neglect their limit orders. This makes sense since continuing to monitor limit orders after submission is mentally more costly than ignoring them. Finally, Barber and Odean (2008) document that retail investors gravitate toward stocks with extreme past returns and high trading volume. These characteristics make these stocks more visible and may also lead investors to perceive their returns as more volatile. In all cases, attention concentrates on low-ρ_i variables. The model therefore implies that, if lower trading costs reduce the mental effort investors devote to trading (as regulators have warned), these biases should intensify after the reform: investors should, for example, submit more round-number orders, be more likely to neglect unexecuted limit orders, and concentrate their trading further on salient stocks.

D Derivations for the Model with Costly Attention

This appendix derives the three predictions in Section 5.2.

Prediction 1 (Lower-wealth traders pay less attention and have higher turnover) We first show that optimal attention increases with wealth, $da^*/dW > 0$, and that turnover decreases with attention, $\partial T/\partial q < 0$.

Optimal attention a^* satisfies the first-order condition in Equation 14, $\theta W\, q'(a^*)\, \partial V/\partial q = c$, where $\partial V/\partial q$ is evaluated at $q(a^*)$. Differentiating both sides with respect to W, holding κ fixed,

$$\theta\, q'(a^*)\, \frac{\partial V}{\partial q} + \theta W \Big[q''(a^*)\, \frac{\partial V}{\partial q} + q'(a^*)^2\, \frac{\partial^2 V}{\partial q^2} \Big] \frac{da^*}{dW} = 0 \tag{D.1}$$

The bracket is the second derivative of the objective in Equation 13 with respect to a, divided by θW. Denote it by

$$\Delta(a^*) \equiv q''(a^*)\, \frac{\partial V}{\partial q} + q'(a^*)^2\, \frac{\partial^2 V}{\partial q^2} \tag{D.2}$$

We assume that the interior optimum satisfies the second-order condition strictly, $\Delta(a^*) < 0$. Solving Equation D.1 for da^*/dW,

$$\frac{da^*}{dW} = -\frac{q'(a^*)\, \partial V/\partial q}{W\, \Delta(a^*)} \tag{D.3}$$

The numerator is positive because $q' > 0$ and because Equation 14 with $c > 0$ implies $\partial V/\partial q > 0$ at a^*. The denominator is negative. Therefore $da^*/dW > 0$, which is Equation 17.

For turnover, Equation 16 gives $T = \theta\,[1 - F(\tau)]$, so

$$\frac{\partial T}{\partial q} = -\theta\, f(\tau)\, \frac{\partial \tau}{\partial q} \tag{D.4}$$

The threshold is $\tau = \mu^d - (\mu^d - \kappa)/q$. Rearranging gives $\mu^d - \kappa = q\,(\mu^d - \tau)$, so

$$\frac{\partial \tau}{\partial q} = \frac{\mu^d - \kappa}{q^2} = \frac{\mu^d - \tau}{q} > 0 \tag{D.5}$$

where the inequality uses $\tau < \mu^d$, which follows from $\mu^d > \kappa$. Substituting into Equation D.4,

$$\frac{\partial T}{\partial q} = -\theta\, f(\tau)\, \frac{\mu^d - \tau}{q} < 0 \tag{D.6}$$

which is Equation 18. Since q is increasing in a and a^* is increasing in W, a lower-wealth trader has lower attention and higher turnover.

Prediction 2 (Lower-wealth traders have higher trading sensitivity to transaction costs) Now our goal is to show that the trading sensitivity to transaction costs, $-d\ln T/d\kappa$, is decreasing in attention. We first derive Equation 19, which decomposes the sensitivity into two terms. We then show that each term is decreasing in attention. For the first term, we show this for any F with a non-increasing hazard rate. For the second term, there is no analytical result for a general F. We show this when F is exponential and $q(a) = 1 - e^{-a}$.

Taking logs of Equation 16, $\ln T = \ln \theta + \ln[1 - F(\tau)]$. Differentiating with respect to κ,

$$-\frac{d \ln T}{d\kappa} = \frac{f(\tau)}{1 - F(\tau)} \frac{d\tau}{d\kappa} = h(\tau) \frac{d\tau}{d\kappa} \tag{D.7}$$

where $h \equiv f/(1-F)$. The threshold depends on κ directly and through optimal attention $q(a^*(\kappa))$, so

$$\frac{d\tau}{d\kappa} = \frac{\partial \tau}{\partial \kappa} + \frac{\partial \tau}{\partial q} q'(a^*) \frac{da^*}{d\kappa} = \frac{1}{q} + \frac{\mu^d - \kappa}{q^2} q'(a^*) \frac{da^*}{d\kappa} \tag{D.8}$$

where $\partial\tau/\partial\kappa = 1/q$ follows from $\tau = \mu^d - (\mu^d - \kappa)/q$ and $\partial\tau/\partial q$ is Equation D.5. Substituting into Equation D.7,

$$-\frac{d \ln T}{d\kappa} = \frac{h(\tau)}{q} + h(\tau) \frac{\mu^d - \kappa}{q^2} q'(a^*) \frac{da^*}{d\kappa} \tag{D.9}$$

which is Equation 19. The second term is positive because $da^*/d\kappa > 0$, which we show under Prediction 3.

For the first term, differentiating $h(\tau)/q$ with respect to q, where τ depends on q through Equation D.5,

$$\frac{\partial}{\partial q}\left[\frac{h(\tau)}{q}\right] = \frac{h'(\tau)\, q \dfrac{\partial \tau}{\partial q} - h(\tau)}{q^2} = \frac{h'(\tau)\,(\mu^d - \tau) - h(\tau)}{q^2} \tag{D.10}$$

Since $\mu^d - \tau > 0$ and $h(\tau) > 0$, this is negative if the hazard rate of F is non-increasing, $h'(\tau) \leq 0$. Under this condition the first term is decreasing in attention.

For the second term, assume that α is exponential with mean μ^d, so that $1 - F(\alpha) = e^{-\alpha/\mu^d}$, $f(\alpha) = e^{-\alpha/\mu^d}/\mu^d$, and $h(\alpha) = 1/\mu^d$, and that $q(a) = 1 - e^{-a}$, so that $q' = 1 - q$ and $q'' = -(1-q)$. Under these forms, Equation 15 gives

$$\frac{\partial V}{\partial q} = \int_\tau^\infty (\alpha - \mu^d) \frac{e^{-\alpha/\mu^d}}{\mu^d} d\alpha = (\tau + \mu^d)\, e^{-\tau/\mu^d} - \mu^d\, e^{-\tau/\mu^d} = \tau\, e^{-\tau/\mu^d} \tag{D.11}$$

Differentiating Equation 15 with respect to q, only the lower limit moves, so by Equation D.5

$$\frac{\partial^2 V}{\partial q^2} = -(\tau - \mu^d)\, f(\tau) \frac{\partial \tau}{\partial q} = \frac{(\mu^d - \tau)^2 f(\tau)}{q} \tag{D.12}$$

and Equation 20 gives $\partial^2 V/\partial q\, \partial\kappa = (\mu^d - \tau)\, f(\tau)/q$. Substituting these and $f(\tau) = e^{-\tau/\mu^d}/\mu^d$ into $\Delta(a^*)$ in Equation D.2,

$$\Delta(a^*) = (1-q)\, e^{-\tau/\mu^d} \left[-\tau + \frac{(1-q)(\mu^d - \tau)^2}{\mu^d q}\right] \tag{D.13}$$

Substituting the same expressions into $da^*/d\kappa = -q'(a^*)\,(\partial^2 V/\partial q\,\partial\kappa)/\Delta(a^*)$, which we derive under Prediction 3,

$$\frac{da^*}{d\kappa} = \frac{\mu^d - \tau}{\mu^d q\tau - (1-q)(\mu^d-\tau)^2} \tag{D.14}$$

Write $m \equiv \mu^d - \kappa$, so that $\mu^d - \tau = m/q$ and $\tau = \mu^d - m/q$, and define $B(q) \equiv (\mu^d)^2 q^3 - \mu^d m q^2 - (1-q)\,m^2$. Substituting τ into Equation D.13 gives

$$\Delta(a^*) = -\frac{(1-q)\,e^{-\tau/\mu^d}}{\mu^d q^3}\,B(q) \tag{D.15}$$

so the second-order condition $\Delta(a^*) < 0$ is equivalent to $B(q) > 0$. Substituting τ into Equation D.14 gives

$$\frac{da^*}{d\kappa} = \frac{mq}{B(q)} \tag{D.16}$$

Substituting $da^*/d\kappa$, $h = 1/\mu^d$, $q' = 1-q$, and $\mu^d - \kappa = m$ into the second term of Equation D.9,

$$h(\tau)\,\frac{\mu^d-\kappa}{q^2}\,q'(a^*)\,\frac{da^*}{d\kappa} = \frac{(1-q)\,m^2}{\mu^d\,q\,B(q)} \tag{D.17}$$

Since $B'(q) = 3(\mu^d)^2q^2 - 2\mu^d m q + m^2 = \left(\sqrt{3}\,\mu^d q - m/\sqrt{3}\right)^2 + 2m^2/3 > 0$ for all q, $B(q)$ is increasing in q. On the region where $B(q) > 0$, the denominator of Equation D.17 increases in q and the numerator decreases in q, so the second term is decreasing in attention. Wealth enters both terms only through $q(a^*)$, so a lower-wealth trader, who has lower attention by Prediction 1, has a higher trading sensitivity to transaction costs.

Prediction 3 (A cost cut lowers attention) Finally, our goal is to show that optimal attention decreases when the trading cost falls, $da^*/d\kappa > 0$.

Differentiating both sides of Equation 14 with respect to κ, holding W fixed,

$$\theta W\,q'(a^*)\,\frac{\partial^2 V}{\partial q\,\partial\kappa} + \theta W\,\Delta(a^*)\,\frac{da^*}{d\kappa} = 0 \tag{D.18}$$

where $\Delta(a^*)$ is defined in Equation D.2. Solving for $da^*/d\kappa$,

$$\frac{da^*}{d\kappa} = -\frac{q'(a^*)\,\partial^2 V/\partial q\,\partial\kappa}{\Delta(a^*)} \tag{D.19}$$

Since $q' > 0$ and $\Delta(a^*) < 0$, the sign of $da^*/d\kappa$ is the sign of $\partial^2 V/\partial q\,\partial\kappa$. It remains to compute this cross derivative.

By Equation 12 and $\mu^d - \kappa = q\,(\mu^d - \tau)$, the net return on a trade is $\hat{\mu} - \kappa = q\,(\alpha - \mu^d) + (\mu^d - \kappa) = q\,(\alpha - \tau)$, so the expected net return per dollar is

$$V(q,\kappa) = \mathbb{E}\big[(\hat{\mu}-\kappa)^+\big] = q\int_\tau^\infty (\alpha - \tau)\,dF(\alpha) \tag{D.20}$$

In Equation D.20, q appears in three places. It is the factor in front of the integral, and it enters the integrand and the lower limit through τ. Differentiating with respect to q gives one term for each. The term from the lower limit is $-q\,(\tau-\tau)\,f(\tau)\,\partial\tau/\partial q = 0$, so the remaining two terms give

$$\frac{\partial V}{\partial q} = \int_{\tau}^{\infty}(\alpha-\tau)\,dF(\alpha) - q\,\frac{\partial\tau}{\partial q}[1-F(\tau)] = \int_{\tau}^{\infty}(\alpha-\tau)\,dF(\alpha) - \int_{\tau}^{\infty}(\mu^d-\tau)\,dF(\alpha) = \int_{\tau}^{\infty}(\alpha-\mu^d)\,dF(\alpha) \tag{D.21}$$

where the second equality uses $q\,\partial\tau/\partial q = \mu^d - \tau$ from Equation D.5. This is Equation 15. Only the lower limit depends on κ, so with $\partial\tau/\partial\kappa = 1/q$,

$$\frac{\partial^2 V}{\partial q\,\partial\kappa} = -(\tau-\mu^d)\,f(\tau)\,\frac{\partial\tau}{\partial\kappa} = \frac{(\mu^d-\tau)\,f(\tau)}{q} > 0 \tag{D.22}$$

which is Equation 20. By Equation D.19, $da^*/d\kappa > 0$. A lower trading cost therefore lowers optimal attention.

E Simulation of the Model with Risk Aversion and Position Choice

The model in Section 5.2 assumes that the trader is risk-neutral and that the position size is a fixed fraction of wealth. This appendix relaxes both assumptions. We assume the trader has constant relative risk aversion (CRRA) utility over end-of-day wealth and chooses the position size. Our main predictions continue to hold under one additional assumption that the cost of attention also has a variable component that increases with the position size. Each unit of attention thus costs $c_f + c_v \cdot \theta W$ dollars, where θW is the position size. The fixed component can be interpreted as the cost of evaluating an opportunity (e.g., reading news and price charts) and the variable component can be thought of as the cost of monitoring the position. Because the model no longer has a closed-form solution, we solve it by simulation.

Setup The trader now has CRRA utility $u(x) = x^{1-\gamma}/(1-\gamma)$ with relative risk aversion γ. A risk-averse trader cares about the risk of a trade and not only about its expected return, so we need to specify the distribution of the return on a trade. In the main model, a trader who pays full attention earns the expected return α. Here, a trader who pays full attention earns

$$r_{\text{full}} \sim N(\alpha, \sigma^2_{\text{full}}) \tag{E.1}$$

where α is the full-attention expected return, drawn from a distribution F as in the main model. σ_{full} is the risk that remains even with full attention. A trader who pays no attention follows their prior:

$$r \sim N(\mu^d, \sigma^2_d) \tag{E.2}$$

where μ^d is the prior of the expected return as in the main model. We assume $\sigma_d > \sigma_{\text{full}}$, which reflects that attention reduces risk. With attention a, the trader believes they face

$$r_a \sim N(\hat{\mu}, \hat{\sigma}^2) \tag{E.3}$$

where $\hat{\mu} = \mu^d + q(a)\,(\alpha - \mu^d)$ and $\hat{\sigma}^2 = \sigma^2_{\text{full}} + [1 - s(a)]\,(\sigma^2_d - \sigma^2_{\text{full}})$. As in Equation 12, attention moves the expected return from the prior toward the full-attention expected return and moves the variance from σ^2_d toward σ^2_{full} in a reduced-form specification. In the simulation below, we assume $q(a) = s(a) = a/(1+a)$.

The trader also chooses how much to invest. After forming $\hat{\mu}$ and $\hat{\sigma}$, the trader chooses the position as a share $\theta \geq 0$ of wealth W. The trader faces the trading cost κ and the variable cost of attention $c_v \cdot a$ per dollar of position. Using the mean-variance approximation in Campbell and

Viceira (2002), the position is

$$\theta^* \approx \begin{cases} \dfrac{\hat{\mu} - \kappa - c_v \cdot a - r_f}{\gamma\, \hat{\sigma}^2} & \text{if } \hat{\mu} - \kappa - c_v \cdot a > r_f \\ 0 & \text{otherwise} \end{cases} \tag{E.4}$$

where r_f is the risk-free rate. We assume $r_f = 0$. The utility given a is

$$U(a) = W \left[\theta^* \left(\hat{\mu} - \kappa - c_v \cdot a \right) - \frac{\gamma}{2}\, \theta^{*2}\, \hat{\sigma}^2 \right] \tag{E.5}$$

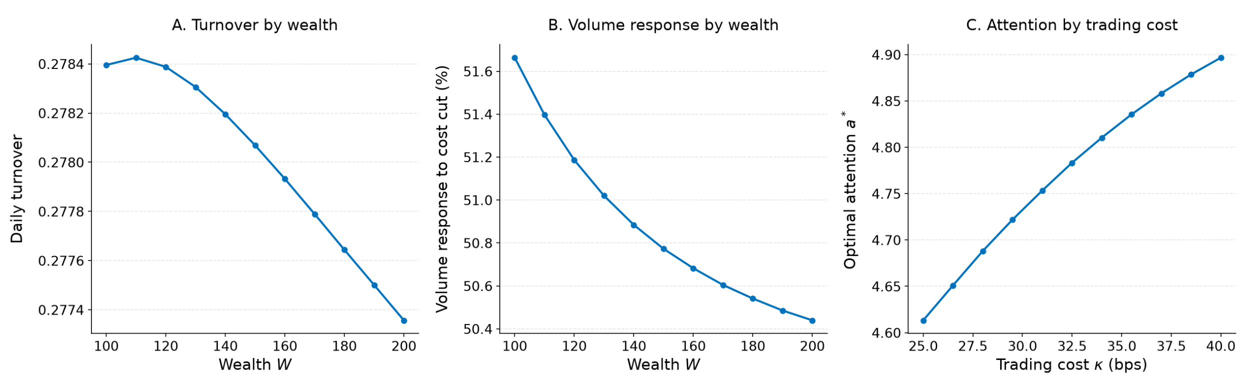
The attention choice is as in the main model. Before α is realized, the trader chooses attention to solve

$$\max_{a \geq 0}\ \mathbb{E}_\alpha \big[U(a) - c_f \cdot a \big] \tag{E.6}$$

where c_f is the fixed cost of attention.

Results For the simulation, we take F exponential with mean $\mu^d = 45$ basis points, and set $\kappa = 40$ basis points before and 25 after. We let $\sigma_d = 4\%$, $\sigma_{\text{full}} = 3.75\%$, $c_f = 0.005$, $c_v = 0.0002$, and $\gamma = 3$. Figure E.1 reports the results. The predictions are similar to those of the main model. Lower-wealth traders have higher turnover and respond more to the cost cut. A decline in trading cost lowers attention. It is worth noting that turnover is relatively flatter in wealth because a more attentive trader takes fewer but larger positions.

Figure E.1: Simulation of the Model with Risk Aversion and Position Choice



F Additional Figures and Tables

F.1 Figures

Figure F.1: Composition of Trading Volume

This figure shows the composition of total trading volume in Taiwan's stock market by investor type from November 2016 to October 2017. The stacked area chart displays the percentage share of trading volume for four investor categories: securities dealers (red), domestic institutional investors (green), foreign investors (blue), and retail investors (gray). The vertical axis represents the share of trading volume in percentage terms, and the horizontal axis shows the time period at monthly intervals. Data are from the Taiwan Stock Exchange and the Taiwan Economic Journal database.

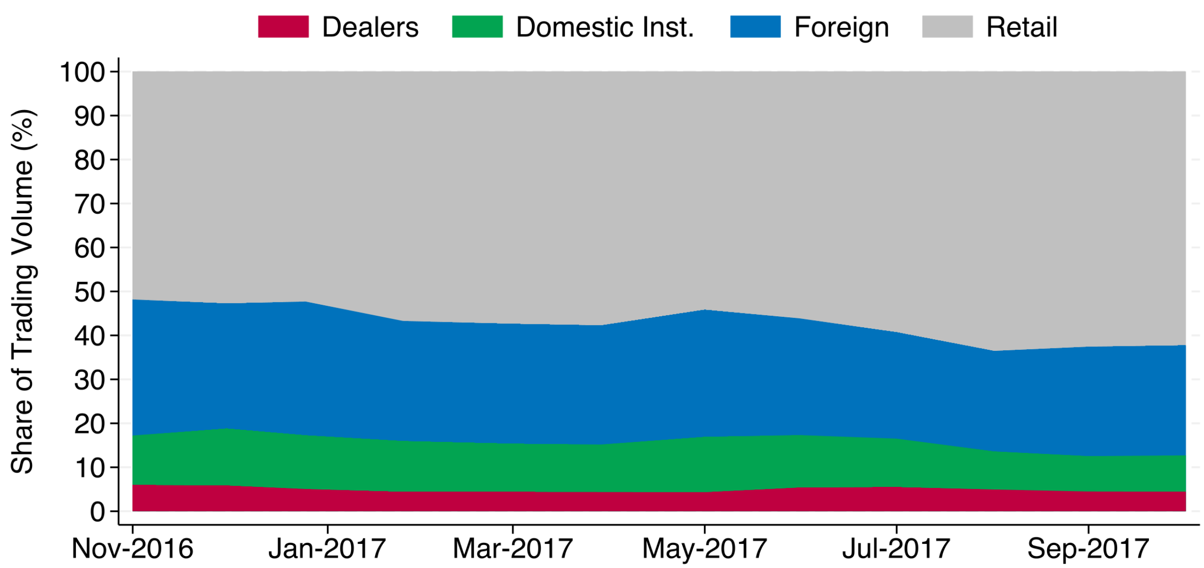


Figure F.2: Contemporaneous Correlation in Trading Volume between Treatment and Control

This figure shows the cross-sectional correlation between log day trading volume of the treatment group and log total trading volume of the control group across stocks at different time aggregation levels. Day trading volume for the treatment group and total trading volume for the control group are aggregated at the stock and frequency level (daily, biweekly, monthly). Each bar indicates the average cross-sectional correlation (in percentage terms) of log volumes over the pre-reform period from November 2016 to April 2017.

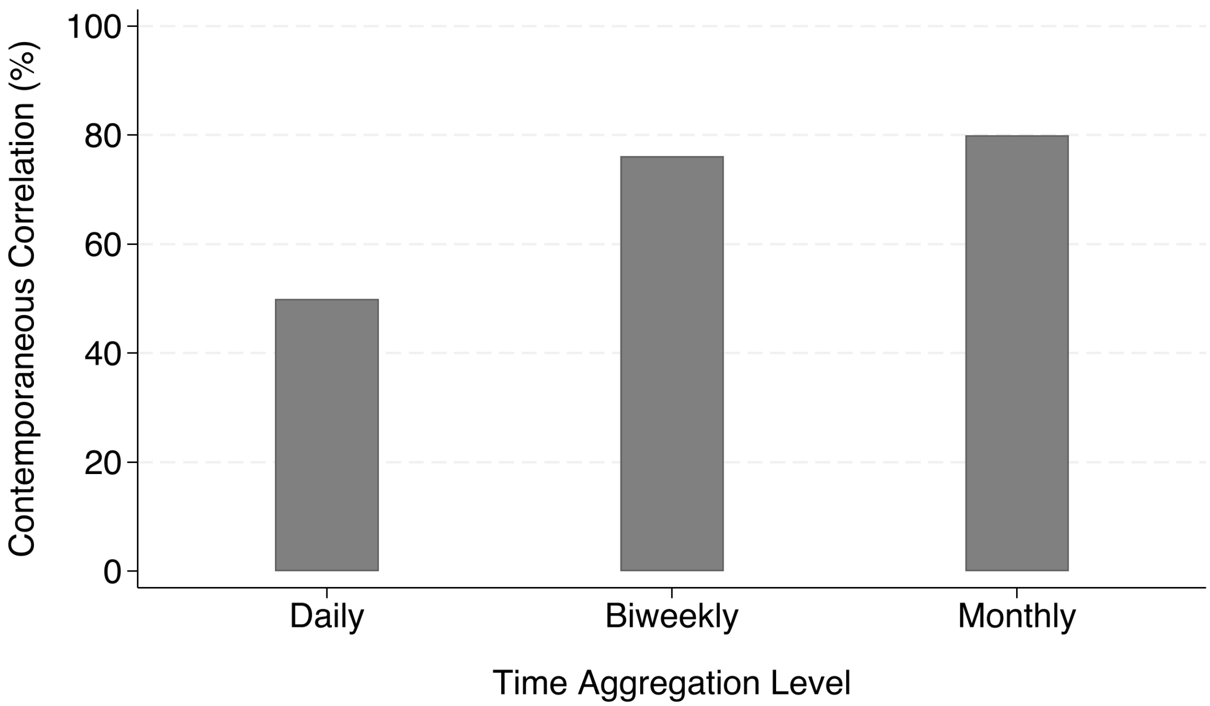


Figure F.3: Ratio of Day Trading Volume to Total Trading Volume Among Treatment Group

This figure plots the biweekly ratio of day trading volume to total trading volume for the treatment group from November 2016 to October 2017. Panel (a) uses all securities. The ratio is calculated as the aggregate day trading volume divided by the aggregate total trading volume among investors in the treatment group for each biweekly period, expressed in percentage points. Panel (b) computes the same ratio separately for common stocks and for ETFs. The vertical dashed line indicates the implementation of the transaction tax reform on April 28, 2017.

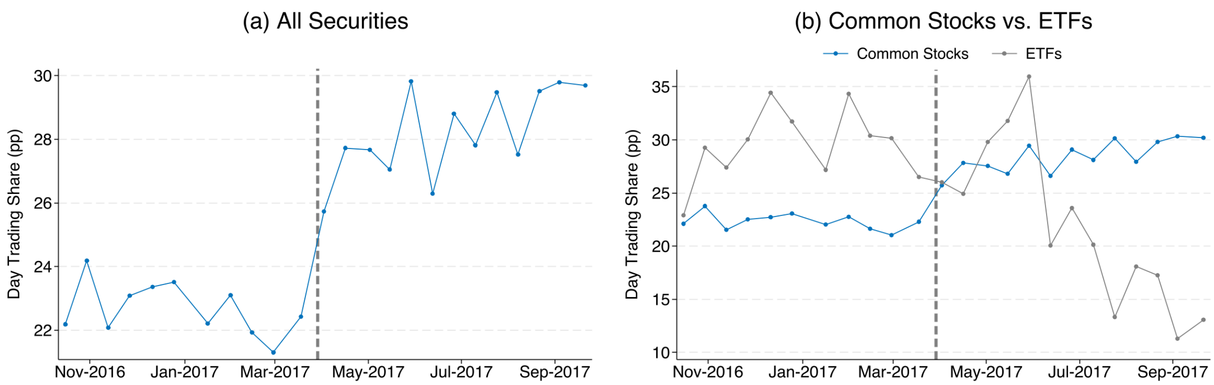


Figure F.4: Effect of the Transaction Tax Reform on Total Trading Volume

This figure plots the effect of the transaction tax reform on trading volume. Panel (a) shows the biweekly total trading volume for the treatment group and the total trading volume for the control group, each scaled by its pre-reform mean. Panel (b) displays coefficients from a dynamic difference-in-differences specification estimated at the biweekly level, based on the following equation:

$$\text{Volume}_{it} = \exp\left(\sum_{k\neq -1} \beta_k \ \text{Treat}_i \times \mathbf{1}[t=k] + \gamma_i + \delta_t\right) \varepsilon_{it}$$

where i indexes investors and t indexes biweekly intervals. The outcome is total trading volume for both groups. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group. $\mathbf{1}[t=k]$ is an indicator for event time k relative to the tax reform date, with $k=-1$ omitted as the baseline. β_k captures the period-k treatment effect. γ_i and δ_t denote investor and time fixed effects, respectively. Coefficients are expressed as percent changes relative to baseline. Shaded areas represent 95% confidence intervals.

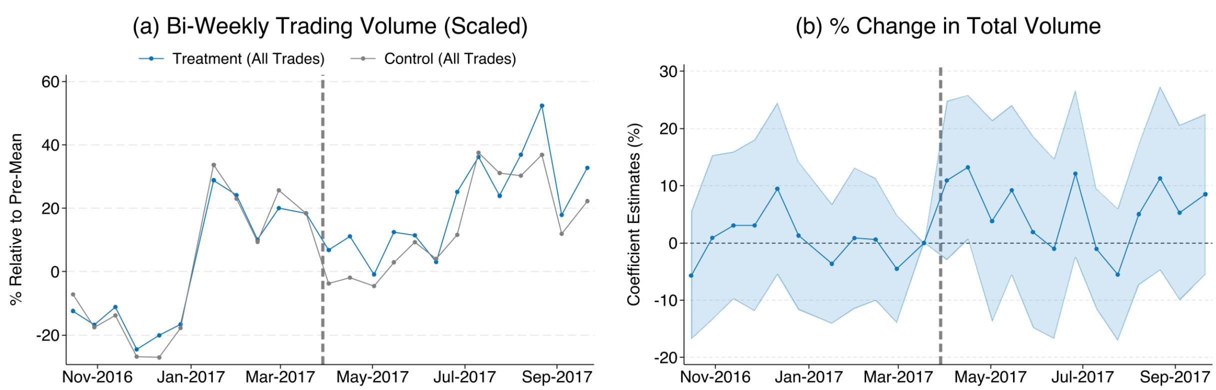


Figure F.5: Monthly Non-Day-Trade Volume Across Years

The figure plots monthly non-day-trade volume as a percentage relative to April for 2017 (red, solid) and pooled 2013–2016 (gray, dashed). Estimates come from a Poisson pseudo-maximum-likelihood regression of average daily non-day-trade volume on month indicators, with April omitted, at the account-year-month level.

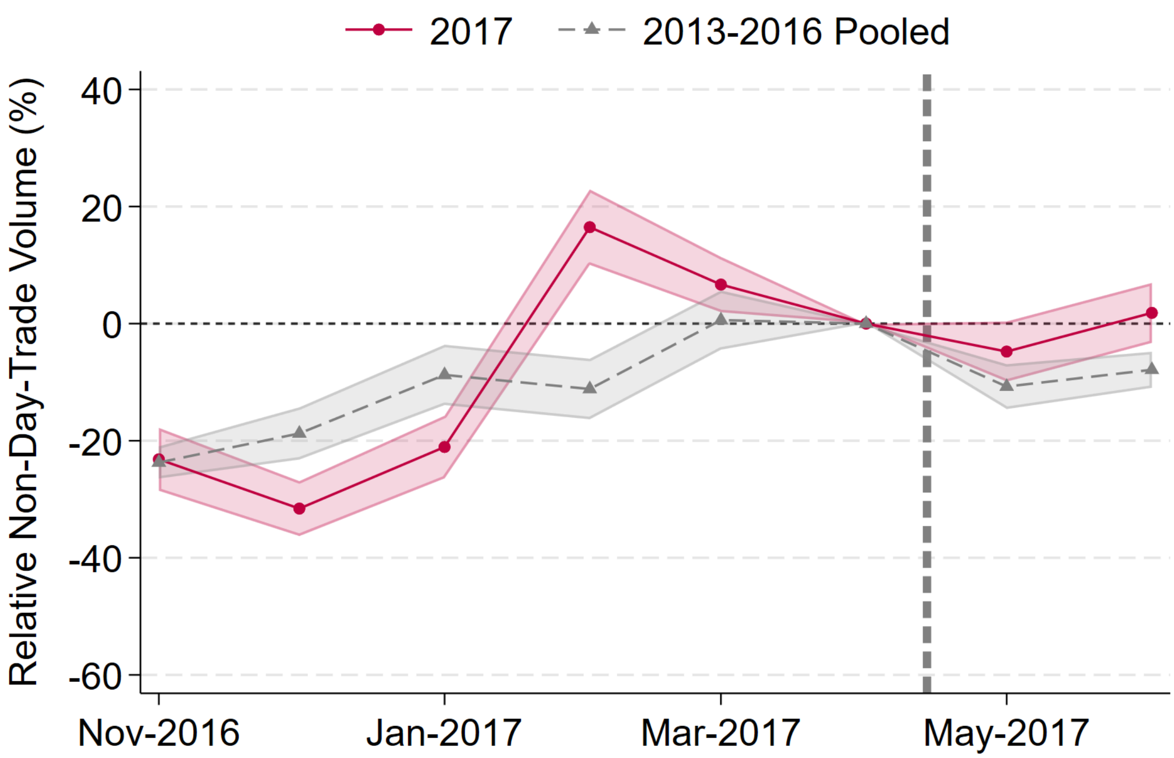


Figure F.6: Elasticity Estimate from the Literature

This figure shows estimated elasticities of trading volume with respect to transaction costs from various empirical studies compiled in Matheson (2011), as well as a recent study on Chinese stamp duty taxes by Deng, Liu, and Wei (2018). The elasticities measure the percentage change in trading volume resulting from a one percent change in transaction costs. Three different measures of transaction costs are represented by different colors and symbols, as some studies examine only the elasticity with respect to a subcomponent of transaction costs (STT or BAS). Red circles indicate Total Transaction Costs (TTC). Blue diamonds represent Securities Transaction Tax (STT). Green triangles show Bid-Ask Spread (BAS). When studies report ranges rather than point estimates, vertical lines span from the lower to upper bound of the estimated elasticity.

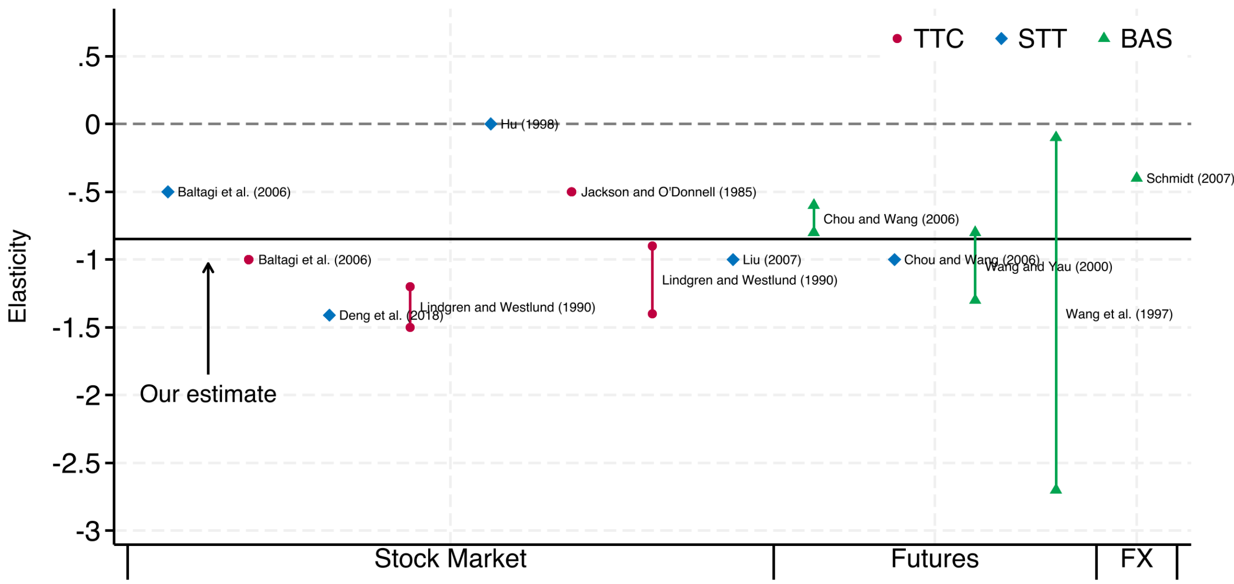


Figure F.7: Pre-Reform Day-Trade Turnover by Investor Wealth

This figure plots the average monthly day-trade turnover of day traders in the treatment group before the reform, separately by predicted wealth tercile. For each investor, monthly day-trade turnover is the average monthly day-trade volume from November 2016 to April 2017 divided by the investor's portfolio value over the month before the reform, expressed in percentage points. Investors are grouped into terciles based on wealth, which is predicted from their combination of age, zip code, and gender. Error bars indicate 95% confidence intervals. The t-statistic shown in the top right tests whether the difference in turnover between the bottom and top wealth terciles is statistically significant.

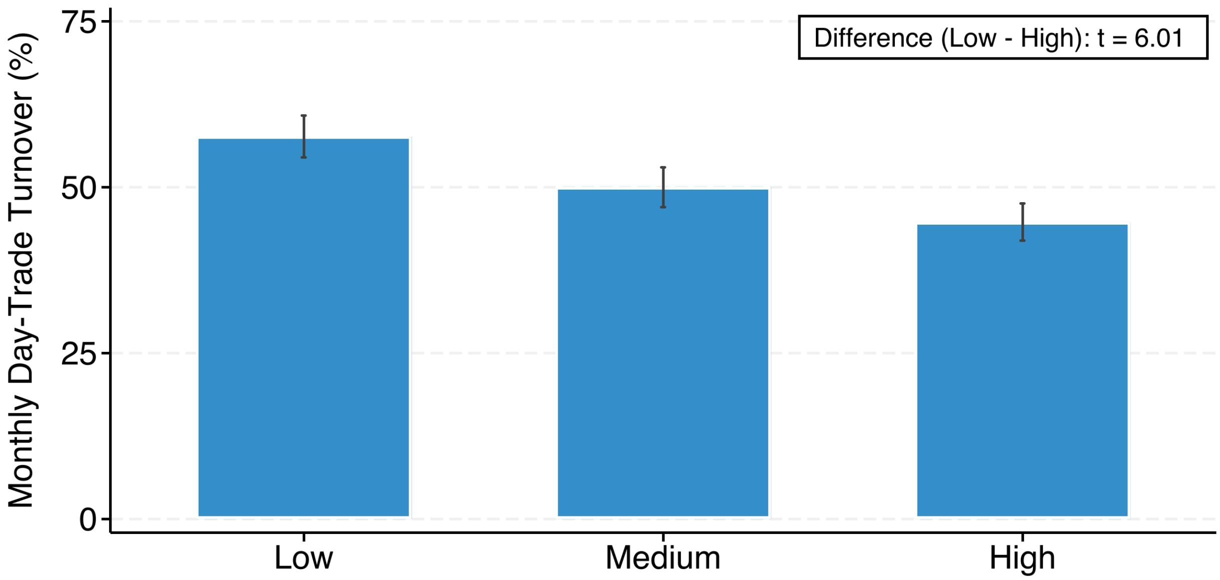


Figure F.8: Heterogeneous Effect on Day Trading Volume by Trading Experience

This figure plots heterogeneous effects of the transaction tax reform on day trading volume by investor trading experience. Trading experience is measured as the number of trades in the two years prior to the tax reform. The coefficients are from estimating the following equation separately for each experience tercile g:

$$\text{Volume}_{it} = \exp(\beta_g \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t)\varepsilon_{it} \quad \text{where Experience Tercile}_i = g$$

where Volume_{it} is trading volume for investor i in biweekly period t. For the treatment group, it refers to day trading volume; for the control group, it refers to total trading volume. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group. Post_t is an indicator equal to 1 for periods after April 28, 2017. Investors are split equally into terciles based on trading experience, where $g \in$ {Bottom, Middle, Top}. γ_i and δ_t denote investor and time fixed effects. Error bars indicate 95% confidence intervals.

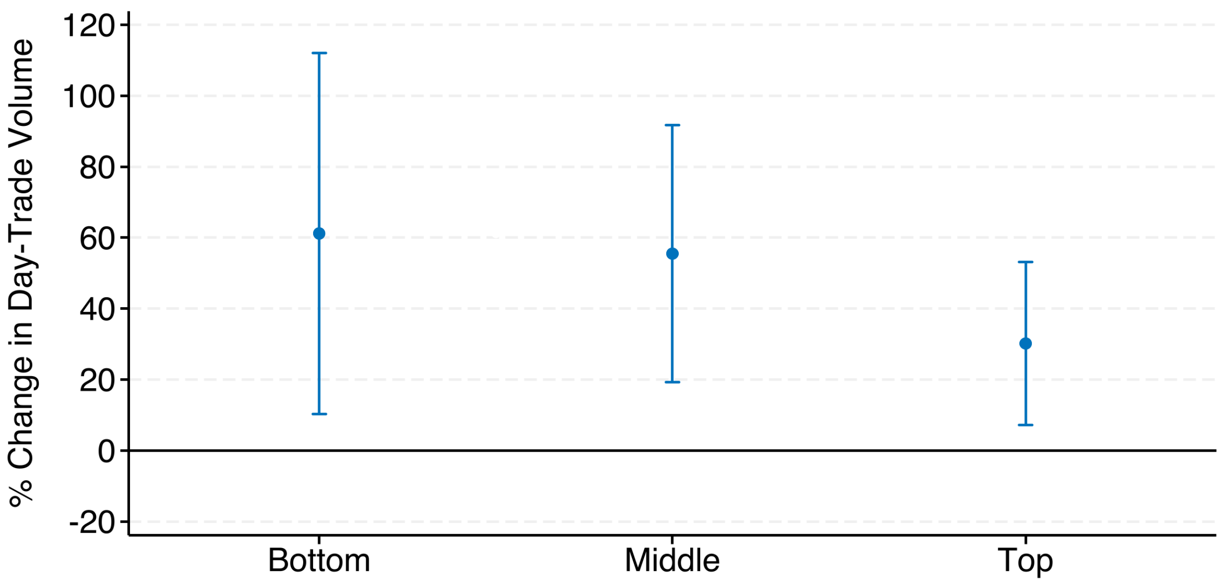


Figure F.9: Limit Order Clustering

This figure plots the frequency distribution of limit order prices by their last digit and first decimal place. The horizontal axis shows price endings from 0.0 to 9.9, and the vertical axis shows the frequency as a percentage of all limit orders. Vertical dashed lines indicate round-number prices ending in .00 or .50. The sample includes all limit orders submitted by day traders during the six-month pre-reform period from November 2016 to April 2017.

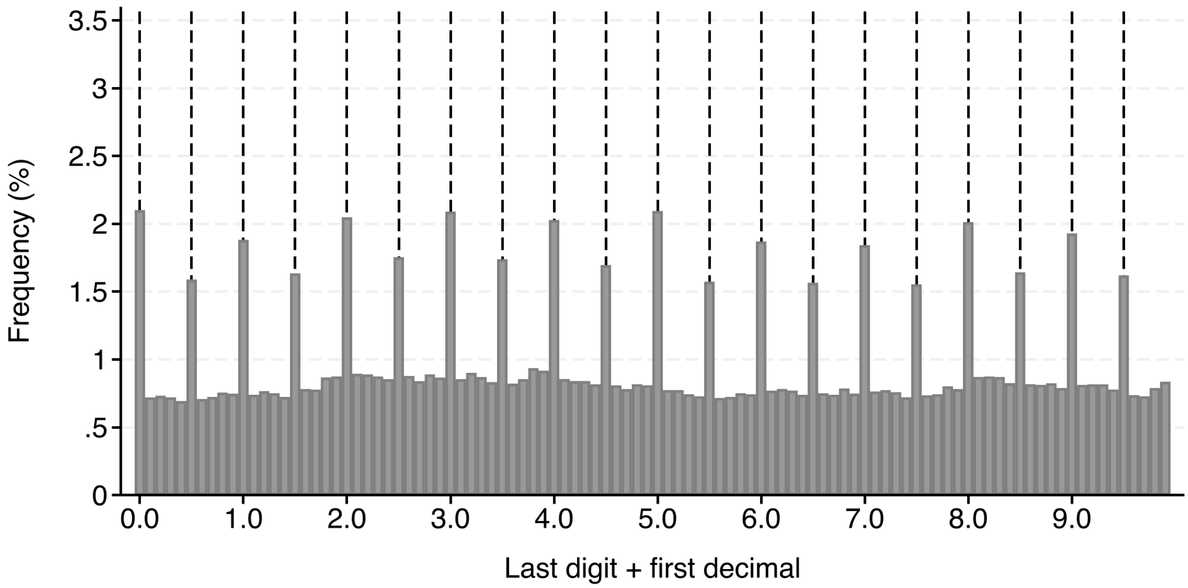


Figure F.10: Share of Round-Number Orders by Trader Sophistication

This figure plots the average share of round-number limit orders from day trades, separately by trader sophistication. Round-number orders are defined as limit orders placed at prices ending in .00 or .50. Panel (a) groups investors into terciles based on their predicted wealth. Panel (b) groups investors based on the number of trades in the two years prior to the reform. Error bars indicate 95% confidence intervals. The *t*-statistics shown in the top right of each panel test for significant differences between the least and most sophisticated groups.

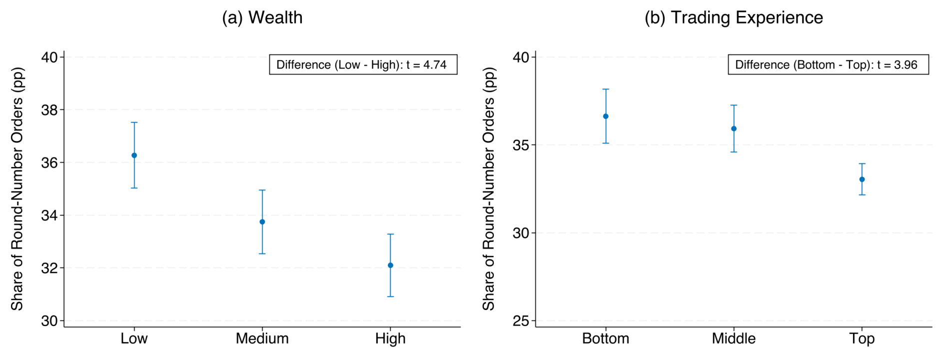


Figure F.11: Time Series of Market Quality

This figure plots the time series of market quality measures for treatment and control stocks from November 2016 to October 2017. Panel (a) shows the biweekly average of Log($ Depth), the natural logarithm of dollar depth at the best bid and ask prices. Panel (b) shows the biweekly average of Log(Quoted Spreads), the natural logarithm of quoted spreads. Panel (c) shows the biweekly average of Realized Volatility, the annualized standard deviation of 10-minute intraday returns measured in percentage points. The treatment group (blue line) consists of stocks eligible for day trading, while the control group (gray line) consists of stocks ineligible for day trading. The sample uses entropy-balanced weights so that the first moments of order book depth, quoted spread, and realized volatility are comparable between treatment and control groups in the pre-period. The vertical dashed line indicates the implementation of the transaction tax reform on April 28, 2017.

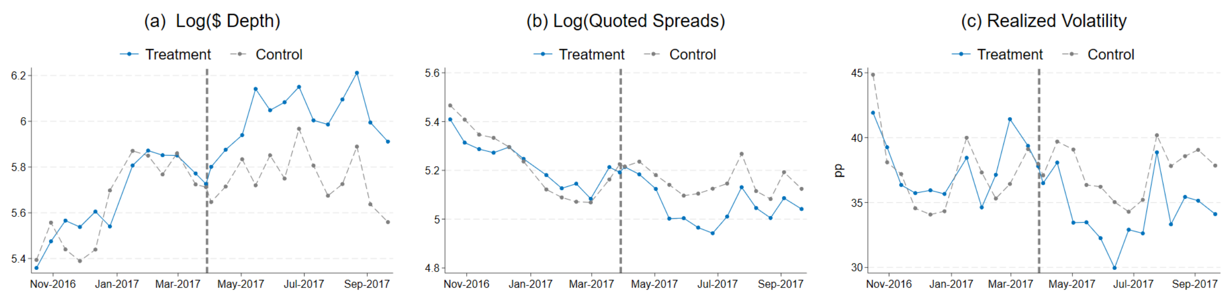
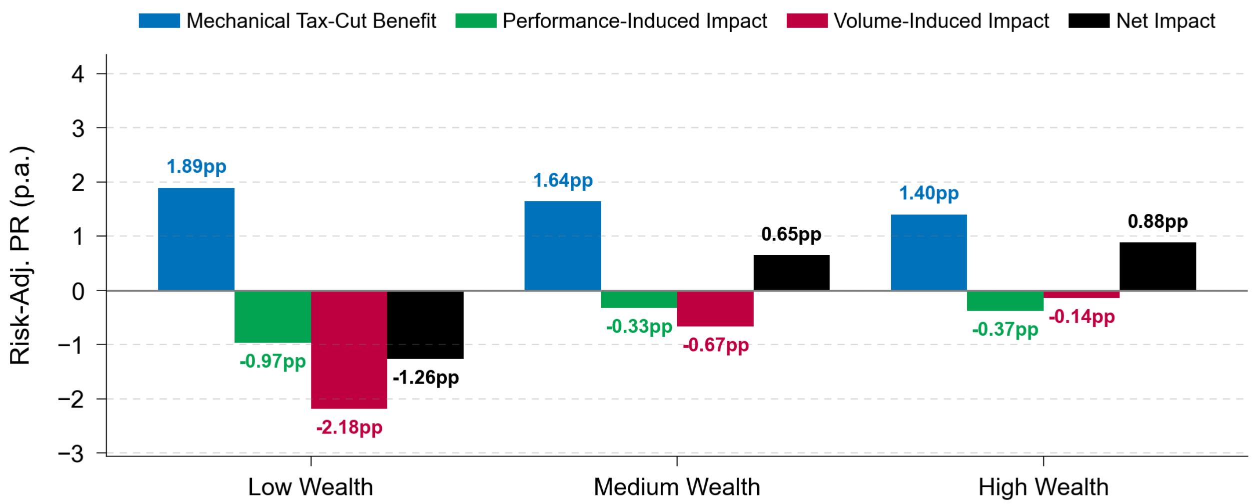


Figure F.12: Financial Impact of the Reform under a Counterfactual with No Commission and a Tax Cut from 15 to 0 Basis Points

This figure repeats the decomposition in Figure 7 assuming that investors pay no commission and that the transaction tax on day trades falls from 15 to 0 basis points instead of from 30 to 15. The estimated volume and performance responses are held fixed. The mechanical tax-cut benefit (blue bars) and the performance-induced impact (green bars) are unchanged from Figure 7; the volume-induced impact (red bars) values the additional day trading at returns before commissions and transaction taxes. Net impact (black bars) sums the three components. All values are annualized changes in percentage points.



F.2 Tables

Table F.1: Day Trading Volume by Investor Type in 2016

This table reports the composition of day trading volume by investor type in Taiwan's stock market in 2016. Day Trading Volume/Day is the average daily day trading volume in hundreds of millions of New Taiwan Dollars (NTD). Share represents each investor type's percentage of total day trading volume. Day trading is defined as the purchase and sale of the same stock within a single trading day. Data are from Taiwan Ministry of Finance (2018).

Investor Type	Day Trading Volume/Day (100 Million NTD)	Share
Individual Investors	97.00	91.74%
Foreign Investors	4.09	3.87%
Securities Dealers	3.61	3.41%
Domestic Institutional Investors	1.04	0.98%

Table F.2: Summary Statistics of Brokerage Data

This table reports summary statistics for the brokerage sample. Panel A presents statistics for the full sample of 120,577 retail investors. Panel B presents statistics for the main analysis sample, divided into treatment and control groups. The treatment group consists of day traders—investors who executed at least one day trade in the year prior to the analysis sample period. The control group consists of active non-day traders who traded on at least 30 days but never engaged in day trading during the same period. Account Tenure is measured in years since account opening as of 2017. Age is investor age in years as of 2017. Is male is the percentage of male investors. Portfolio Size is the average portfolio value in USD. Monthly volume is the average monthly trading volume in USD ((Buy+Sell)/2). Monthly # any trades is the average number of trades per month. Trade size is the average trade size in USD. Monthly turnover is monthly trading volume divided by portfolio value, expressed as a percentage. Monthly day-trade volume is the average monthly day trading volume in USD. Monthly # day trades is the average number of day trades per month. Day-trade size is the average size of a day trade in USD. Day-trade return is the average gross return per dollar day-traded in basis points. All statistics are calculated over the six-month pre-reform period (November 2016 to April 2017).

Panel A. Full Brokerage Sample

	N(investor)	Mean	SD	p25	p50	p75
Account Tenure (Years)	120,577	11.22	6.80	6.00	10.00	17.00
Age (Years)	120,577	48.39	13.09	39.00	47.00	58.00
Is male (%)	120,577	50.21	50.00	0.00	100.00	100.00
Portfolio Size (USD)	120,577	68,967.91	367,543.19	4,523.89	16,815.06	51,388.89
Monthly # any trades	120,577	3.99	11.60	0.33	1.00	3.00
Trade size (USD)	120,577	7,142.96	24,684.01	1,480.00	2,989.35	6,385.61
Monthly volume (USD)	120,577	17,370.73	144,848.55	425.00	1,630.14	6,998.33
Monthly turnover (%)	120,577	26.67	40.62	3.22	9.25	28.36

Panel B. Main Sample

	Treatment						Control					
	N(investor)	Mean	SD	p25	p50	p75	N(investor)	Mean	SD	p25	p50	p75
Account Tenure (Years)	14,051	11.43	6.48	6.00	11.00	17.00	3,655	12.48	6.52	7.00	13.00	17.00
Age (Years)	14,051	51.03	12.41	42.00	51.00	60.00	3,655	51.44	12.40	42.00	51.00	60.00
Is male (%)	14,051	57.13	49.49	0.00	100.00	100.00	3,655	57.98	49.37	0.00	100.00	100.00
Portfolio Size (USD)	14,051	135,630.52	293,378.33	13,590.56	41,841.11	119,915.00	3,655	148,396.88	251,556.57	25,849.08	65,385.25	152,909.58
Monthly volume (USD)	14,051	81,392.35	168,600.99	9,533.31	26,033.61	73,592.78	3,655	22,863.32	37,740.78	4,136.78	10,001.78	24,220.42
Monthly # any trades	14,051	18.51	21.97	5.17	11.17	22.67	3,655	8.00	6.75	3.33	6.17	10.50
Trade size (USD)	14,051	8,393.09	13,664.82	2,007.48	3,820.19	8,260.94	3,655	6,071.13	8,376.01	1,702.43	3,143.31	6,475.87
Monthly turnover (%)	14,051	136.65	179.43	23.64	60.25	160.91	3,655	40.31	74.57	6.51	16.66	41.27
Monthly day-trade volume (USD)	14,051	25,844.83	74,475.45	840.28	3,218.33	13,839.89	3,655	416.80	1,471.22	0.00	0.00	0.00
Monthly # day trades	14,051	3.45	7.39	0.33	0.83	2.67	3,655	0.08	0.20	0.00	0.00	0.00
Day-trade size (USD)	14,051	7,420.81	11,952.18	1,724.26	3,413.61	7,541.19	842	5,833.89	9,306.02	1,439.79	2,815.00	6,146.67
Day-trade return (bps)	14,051	27.98	118.67	-30.92	16.48	83.01	842	57.93	179.67	-30.49	31.69	134.93

Table F.3: Changes in Realization Rate of Intended Day Trades

This table reports the results from estimating the following equation:

$$\text{Realization Rate}_{it} = \beta \text{ Post}_t + \gamma_i + \varepsilon_{it}$$

where Realization Rate_{it} is the percentage of intended day trades that are actually completed (both opened and closed) within the same trading day by investor i on day t, calculated as:

$$\text{Realization Rate}_{it} = \frac{\text{\# Completed day trades}_{it}}{\text{\# Intended day trades}_{it}} \times 100$$

Intended day trades are defined as all new positions opened on days when an investor completes at least one day trade. Completed day trades are those positions that are both opened and closed within the same trading day. Post_t is an indicator equal to 1 for days after April 28, 2017. γ_i denotes investor fixed effects. The sample includes all investors in the treatment group who executed at least one day trade during the pre-reform period. Standard errors are clustered at the investor level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Realization Rate (pp)
	(1)
Post	1.50***
	(0.14)
Constant	76.76***
	(0.07)
Observations	404,743
Adj. R^2	0.36
Investor FE	✓
Cluster	Investor

Table F.4: Trading Volume Responses with Alternative Control cutoffs

This table reports the results from estimating Equation 5:

$$\text{Volume}_{it} = \exp(\beta \ \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t)\varepsilon_{it}$$

where i indexes investors and t indexes biweekly time intervals. The dependent variable is trading volume. For the treatment group, the outcome is day trading volume; for the control group, it is total trading volume. Treat_i is an indicator equal to 1 if investor i belongs to the treatment group, and Post_t is an indicator equal to 1 for periods after April 28, 2017. γ_i and δ_t denote investor and time fixed effects. Each column uses a different definition for the control group based on the number of trading days in the classification period (the year before the analysis sample period): Column (1) uses investors with at least 10 trading days, Column (2) at least 20 days, Column (3) at least 30 days, and Column (4) at least 40 days. All control group investors never engaged in day trading during the classification period. Standard errors are clustered at the investor level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Volume			
	(1) Control $\geq$ 10 days	(2) Control $\geq$ 20 days	(3) Control $\geq$ 30 days	(4) Control $\geq$ 40 days
Treat $\times$ Post	0.26***	0.25***	0.28***	0.28***
	(0.05)	(0.06)	(0.06)	(0.06)
Observations	792,504	519,144	425,184	381,768
Pseudo R^2	0.77	0.80	0.82	0.83
Investor FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Cluster	Investor	Investor	Investor	Investor

Table F.5: Effect of the Transaction Tax Reform with Alternative Classification Period

This table reports the results from estimating Equations 5 and 8 using an alternative classification period for defining treatment and control groups. Instead of the original classification period (the year before the analysis sample begins), treatment and control groups are defined based on trading behavior in the year immediately preceding the reform (May 2016 to April 2017). Column (1) reports results from estimating:

$$\text{Volume}_{it} = \exp(\beta \ \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t)\varepsilon_{it}$$

Column (2) reports results from estimating:

$$y_{it} = \beta \ \text{Treat}_i \times \text{Post}_t + \lambda_1 \ \text{Treat}_i \times R_t^m + \lambda_2 \ \text{Treat}_i \times \text{Post}_t \times R_t^m + \gamma_i + \delta_t + \varepsilon_{it},$$

where i indexes investors and t indexes biweekly intervals for Column (1) and days for Column (2). Treat_i is an indicator equal to 1 if investor i executed at least one day trade during the alternative classification period (May 2016 to April 2017). Post_t is an indicator equal to 1 for periods after April 28, 2017. γ_i and δ_t denote investor and time/day fixed effects. For Column (1), the dependent variable is day trading volume for the treatment group and total trading volume for the control group. For Column (2), the outcome is gross returns per dollar traded, calculated from day trades only for the treatment group and from all trades for the control group. R_t^m is the intraday market return in excess of the risk-free rate on day t. The control group consists of active non-day traders who traded on at least 30 days but never executed a day trade during the alternative classification period. Standard errors are clustered at the investor level for Column (1) and at the investor and day levels for Column (2). Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Day vs All Trades	
	(1) Volume	(2) Gross Returns (bps)
Treat × Post	0.36***	-6.00***
	(0.05)	(2.10)
Observations	667,080	759,707
Pseudo R^2	0.81	
Adj. R^2		0.18
Investor FE	✓	✓
Time FE	✓	✓
Market Controls		✓
Cluster	Investor	Investor, Day

Table F.6: Effect of the Transaction Tax Reform with Intended Day Trades

This table reports the results from estimating Equations 5 and 8 using an alternative definition of day trading that captures intended day trades. Column (1) reports results from estimating:

$$\text{Volume}_{it} = \exp(\beta \text{ Treat}_i \times \text{Post}_t + \gamma_i + \delta_t)\varepsilon_{it}$$

Column (2) reports results from estimating:

$$y_{it} = \beta \text{ Treat}_i \times \text{Post}_t + \lambda_1 \text{ Treat}_i \times R_t^m + \lambda_2 \text{ Treat}_i \times \text{Post}_t \times R_t^m + \gamma_i + \delta_t + \varepsilon_{it},$$

where i indexes investors and t indexes biweekly intervals for Column (1) and days for Column (2). Intended day trades are defined as all new positions opened on days when an investor completes at least one day trade, regardless of whether those positions are closed within the same day. In Column (1), the dependent variable is intended day trading volume for the treatment group and total trading volume for the control group. In Column (2), the outcome is gross returns per dollar traded, calculated from intended day trades for the treatment group and from all trades for the control group. R_t^m is the intraday market return in excess of the risk-free rate on day t. Treat$_i$ is an indicator equal to 1 if investor i belongs to the treatment group. Post$_t$ is an indicator equal to 1 for periods after April 28, 2017. γ_i and δ_t denote investor and time/day fixed effects. Standard errors are clustered at the investor level for Column (1) and double-clustered at the investor and day levels for Column (2), and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Intended Day vs All Trades	
	(1) Volume	(2) Gross Returns (bps)
Treat × Post	0.27***	-5.21**
	(0.05)	(2.32)
Observations	425,184	634,108
Pseudo R^2	0.82	
Adj. R^2		0.14
Investor FE	✓	✓
Time FE	✓	✓
Market Controls		✓
Cluster	Investor	Investor, Day

Table F.7: Performance Responses with Alternative Control cutoffs

This table reports the results from estimating Equation 8:

$$y_{it} = \beta \text{ Treat}_i \times \text{Post}_t + \lambda_1 \text{ Treat}_i \times R_t^m + \lambda_2 \text{ Treat}_i \times \text{Post}_t \times R_t^m + \gamma_i + \delta_t + \varepsilon_{it},$$

where y_{it} is gross return per dollar traded for investor i on day t. R_t^m is the intraday market return in excess of the risk-free rate on day t. For the treatment group, the outcome is restricted to day trades; for the control group, it includes all trades. For day trades, we compute gross returns using actual execution prices at both purchase and sale. For non-day trades, where positions remain open at day's end, we approximate gross returns by comparing the execution price of the initial trade to that day's closing price. Treat$_i$ is an indicator equal to 1 if investor i belongs to the treatment group. Post$_t$ is an indicator equal to 1 for days after April 28, 2017. γ_i and δ_t denote investor and day fixed effects. Each column uses a different definition for the control group based on the number of trading days in the classification period (the year before the analysis sample period): Column (1) uses investors with at least 10 trading days, Column (2) at least 20 days, Column (3) at least 30 days, and Column (4) at least 40 days. All control group investors never engaged in day trading during the classification period. Standard errors are clustered at the investor and day level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Gross Returns per $ Traded (bps)			
	(1) Control $\geq$ 10 days	(2) Control $\geq$ 20 days	(3) Control $\geq$ 30 days	(4) Control $\geq$ 40 days
Treat × Post	-4.57**	-5.73***	-5.51***	-5.65***
	(1.88)	(1.96)	(2.09)	(2.13)
Observations	1,068,869	781,525	634,108	543,412
Adj. R^2	0.14	0.16	0.18	0.19
Investor FE	✓	✓	✓	✓
Day FE	✓	✓	✓	✓
Market Controls	✓	✓	✓	✓
Cluster	Investor, Day	Investor, Day	Investor, Day	Investor, Day

Table F.8: Holding Period Returns as Counterfactuals

This table reports the results from estimating Equation 8:

$$y_{it} = \beta \text{ Treat}_i \times \text{Post}_t + \lambda_1 \text{ Treat}_i \times R_t^m + \lambda_2 \text{ Treat}_i \times \text{Post}_t \times R_t^m + \gamma_i + \delta_t + \varepsilon_{it},$$

where y_{it} is gross return per dollar traded for investor i on day t. R_t^m is the intraday market return in excess of the risk-free rate on day t. For the treatment group, the outcome is restricted to day trades; for the control group, it includes all trades. For day trades, we compute gross returns using actual execution prices at both purchase and sale by investor i on day t. For non-day trades in the control group, we approximate gross returns using 10-day holding period returns (normalized to daily) in Column (1) and 30-day holding period returns (normalized to daily) in Column (2) for each trade on day t by investor i. Treat$_i$ is an indicator equal to 1 if investor i belongs to the treatment group. Post$_t$ is an indicator equal to 1 for days after April 28, 2017. γ_i and δ_t denote investor and day fixed effects. Standard errors are double-clustered at the investor and day levels and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Gross Returns per $ Traded (bps)	
	(1)	(2)
	10-Day Holding Period Returns	30-Day Holding Period Returns
Treat × Post	-4.02**	-3.92**
	(1.72)	(1.69)
Observations	634,108	634,108
Adj. R^2	0.20	0.21
Investor FE	✓	✓
Day FE	✓	✓
Market Controls	✓	✓
Cluster	Investor, Day	Investor, Day

Table F.9: The Use of Cognitive Shortcuts and Performance

This table reports the results from estimating the following equation:

$$y_{i,s,t} = \beta \text{ Cognitive shortcuts}_{i,s,t} + \gamma_i + \theta_s + \delta_t + \varepsilon_{i,s,t}$$

where $y_{i,s,t}$ is the gross return per dollar traded (in basis points) for investor i in stock s on day t. The outcome is calculated using only day trades. Cognitive shortcuts$_{i,s,t}$ is an indicator equal to 1 if more than 50% of limit orders associated with a day trade by investor i in stock s on day t are placed at round-number prices ending in .00 or .50, and 0 otherwise. Round-number orders are often used by decision makers to reduce cognitive load and save psychic costs. γ_i, θ_s, and δ_t denote investor, stock, and day fixed effects, respectively. The sample includes all day trades by the treatment group during the six-month pre-reform period. Standard errors are double-clustered at the investor and day levels and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Gross Returns (bps) (Day Trades)
	(1)
Cognitive shortcuts	-6.47***
	(1.14)
Observations	702,791
Adj. R^2	0.16
Investor FE	✓
Stock FE	✓
Day FE	✓
Cluster	Investor, Day

Table F.10: Performance from Salience-Driven Trading

This table reports the results from estimating the following equation:

$$y_{i,s,t} = \beta \text{ Extreme Overnight Returns } [0,2]_{s,t} + \gamma_i + \theta_s + \delta_t + \varepsilon_{i,s,t}$$

where $y_{i,s,t}$ is the gross return per dollar traded (in basis points) for investor i in stock s on day t. The outcome is calculated using only day trades. Extreme Overnight Returns $[0,2]_{s,t}$ is an indicator equal to 1 if stock s experienced an extreme overnight return (absolute return in the top 5% of all stocks) within the past 0 to 2 days relative to day t, and 0 otherwise. Extreme overnight returns are used as a proxy for salient events that may attract impulsive trading. γ_i, θ_s, and δ_t denote investor, stock, and day fixed effects, respectively. The sample includes all day trades by the treatment group during the six-month period pre-reform. Standard errors are double-clustered at the investor and day levels and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Gross Returns (bps) (Day Trades)
	(1)
Extreme Overnight Returns $[0,2]$ days ago	-1.99**
	(0.92)
Observations	705,495
Adj. R^2	0.17
Investor FE	✓
Stock FE	✓
Day FE	✓
Cluster	Investor, Day

Table F.11: Heterogeneous Effect of the Transaction Tax Reform on Performance

This table reports the results from estimating the following equation separately for each wealth tercile g:

$$y_{it} = \beta \text{ Treat}_i \times \text{Post}_t + \lambda_1 \text{ Treat}_i \times R_t^m + \lambda_2 \text{ Treat}_i \times \text{Post}_t \times R_t^m + \gamma_i + \delta_t + \varepsilon_{it}, \quad \text{where Wealth Tercile}_i = g$$

where y_{it} is gross return per dollar traded for investor i on day t. R_t^m is the intraday market return in excess of the risk-free rate on day t. The outcome is restricted to day trades for the treatment group and includes all trades for the control group. Columns (1) to (3) use completed day trades, positions opened and closed within the day; columns (4) to (6) use intended day trades, which add positions opened on a day-trading day and left open at the close. For completed day trades, we compute gross returns using actual execution prices at both purchase and sale. For positions left open at day's end, we approximate gross returns by comparing the execution price to the closing price. Treat$_i$ is an indicator equal to 1 if investor i belongs to the treatment group. Post$_t$ is an indicator equal to 1 for days after April 28, 2017. Wealth is predicted based on zip code, gender and age. γ_i and δ_t denote investor and day fixed effects. Within each panel, the three columns present results for wealth terciles $g \in \{\text{Low, Medium, High}\}$. Standard errors are double-clustered at the investor and day levels and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Gross Returns per $ Traded (bps)					
	Day Trades			Intended Day Trades		
	(1) Low Wealth	(2) Medium Wealth	(3) High Wealth	(4) Low Wealth	(5) Medium Wealth	(6) High Wealth
Treat × Post	-10.61***	-1.88	-4.72	-7.69*	-3.00	-4.01
	(3.80)	(3.13)	(2.93)	(4.02)	(3.15)	(3.33)
Observations	187,937	218,641	227,530	187,937	218,641	227,530
Adj. R^2	0.15	0.18	0.20	0.11	0.13	0.12
Investor FE	✓	✓	✓	✓	✓	✓
Day FE	✓	✓	✓	✓	✓	✓
Market Controls	✓	✓	✓	✓	✓	✓
Cluster	Investor, Day	Investor, Day	Investor, Day	Investor, Day	Investor, Day	Investor, Day

Table F.12: The Financial Impact of the Transaction Tax Reform by Wealth

This table presents the decomposition of the financial impact of the transaction tax reform following Equation 9. Investors are grouped into terciles based on their wealth, which is predicted by the combination of age, zip code, and gender. Volume and performance responses are estimated according to Sections 3.2 and 3.4, using intended day trades.

Wealth	(1) Day-trade Volume (pre) ($)	(2) $\frac{\text{Risk-Adj. Net Return}_{post}}{\text{Portfolio Size}}$ ($\times 10^{-6}$, 1/$)	(3) $\frac{\text{Day-Trade Volume}_{pre}}{\text{Portfolio Size}_{pre}}$	(4) Population Share (%)	(5) Volume Response (%)	(6) Performance Response (bps)	(7) Mechanical Tax-Cut Benefit (% p.a.)	(8) Net Impact (% p.a.)	(9) Avg. Mechanical Benefit (% p.a.)	(10) Avg. Net Impact (% p.a.)	(11) Risk-Adj. PR_{pre} (% p.a.)
Low	33,030	-0.345	1.05	33.4	40.0	-7.69	1.89	-4.54	1.64	-2.08	-5.36
Medium	41,066	-0.209	0.91	33.3	23.1	-3.00	1.64	-1.07			-3.68
High	48,500	-0.141	0.78	33.3	20.1	-4.01	1.40	-0.62			-2.99

Table F.13: Bootstrap Standard Errors for the Financial Impact of the Transaction Tax Reform

This table reports bootstrap standard errors for the decomposition in Table F.12 and Figure 7. Each of 1,000 replications draws investors with replacement from the estimation sample and, independently, pre-reform and post-reform trading days with replacement. The investor draw re-estimates the volume response and the pre-reform tercile averages and population shares; the day draw re-estimates the performance response, the gross return, and the gross return per dollar of holdings. Wealth terciles are re-cut within each replication. Standard errors, in parentheses, are the standard deviation of each component across replications. All values are annualized changes in portfolio returns in percentage points. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Mechanical Tax-Cut Benefit (1)	Performance-Induced Impact (2)	Volume-Induced Impact (3)	Net Impact (4)
Low	1.89***	-0.97*	-5.46***	-4.54**
	(0.13)	(0.50)	(1.58)	(1.78)
Medium	1.64***	-0.33	-2.38***	-1.07
	(0.11)	(0.35)	(0.91)	(1.04)
High	1.40***	-0.37	-1.65**	-0.62
	(0.08)	(0.30)	(0.71)	(0.84)
All				-2.08**
				(0.83)

Table F.14: Placebo Test: High vs. Low Profitability Firms

This table reports estimates from the following equation separately within treatment and control groups:

$$y_{st} = \beta \text{ High ROA}_s \times \text{Post}_t + \gamma_s + \delta_t + \varepsilon_{st}$$

where s indexes stocks and t indexes trading days. High ROA_s is an indicator equal to 1 if stock s has above-median return on assets (ROA) within its group. Post_t is an indicator equal to 1 for days after April 28, 2017. γ_s and δ_t denote stock and day fixed effects. The dependent variables are Log($ Depth), the natural logarithm of dollar depth at the best bid and ask prices, Log(Quoted Spreads), the natural logarithm of quoted spreads, and Realized Volatility, the annualized standard deviation of 10-minute intraday returns in percentage points. Columns (1)-(3) present results for stocks within the treatment group (eligible for day trading), while Columns (4)-(6) present results for stocks within the control group (ineligible for day trading). Standard errors are clustered at the stock level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Within Treatment Group			Within Control Group		
	(1) Log($ Depth)	(2) Log(Quoted Spreads)	(3) Realized Volatility	(4) Log($ Depth)	(5) Log(Quoted Spreads)	(6) Realized Volatility
High ROA × Post	-0.06**	0.02	-0.45	-0.08	0.09*	-2.23
	(0.03)	(0.01)	(0.44)	(0.07)	(0.05)	(1.43)
Observations	323,677	322,990	323,677	41,026	40,921	41,026
Adj. R^2	0.85	0.78	0.22	0.68	0.72	0.13
Stock FE	✓	✓	✓	✓	✓	✓
Day FE	✓	✓	✓	✓	✓	✓
Cluster	Stock	Stock	Stock	Stock	Stock	Stock

Table F.15: Retail Investor Biases Across Countries

This table presents prior research documenting three behavioral biases among retail investors across different countries. The table is organized by behavior type (Excessive Trading, Disposition Effect, and Sensation Seeking/Gambling) and geographic region (US, Taiwan, and Others). Each cell contains citations to studies that have documented the respective bias in that market.

Behavior	**US**	**Taiwan**	**Others**
Excessive Trading	Barber & Odean (2000)	Barber et al. (2009)	China: Chen et al. (2004) Finland: Grinblatt & Keloharju (2009) Germany: Dorn & Huberman (2005) Sweden: Anderson (2008)
Disposition Effect	Odean (1998)	Barber et al. (2007)	Australia: Brown et al. (2006) China: Feng & Seasholes (2005) Finland: Grinblatt & Keloharju (2001) Israel: Shapira & Venezia (2001) Sweden: Calvet et al. (2009)
Sensation Seeking/Gambling	Kumar (2009)	Gao & Lin (2015)	China: Liu et al. (2022) Finland: Grinblatt & Keloharju (2009) Germany: Dorn & Sengmueller (2009)

Table F.16: Comparison of Brokerage Samples Across Studies

This table compares investor characteristics across three brokerage samples from different countries and time periods. Our Study uses data from a Taiwan broker covering 2012-2017 with 120,577 traders. Barber & Odean (2000, 2001) uses data from a US discount broker covering 1991-1996 with 66,465 households. Dorn and Huberman (2005) use data from a German broker covering 1995-2000 with 21,528 traders. Age is the average investor age in years. Male is the percentage of male investors. Account Tenure is the average years since account opening. Trade Size is the average trade size in USD. Monthly Volume is the average monthly trading volume in USD ((Buy+Sell)/2). Monthly Turnover is monthly trading volume divided by portfolio value in percentage points. Portfolio Size is the average portfolio size in USD. Dashes indicate data not available in the original studies.

	Our Study	**Barber & Odean (2000, 2001)**	**Dorn & Huberman (2005)**
Data Source	Taiwan Broker	US Discount Broker	German Broker
Period	2012–2017	1991–1996	1995–2000
Sample Size	120,577 traders	66,465 households	21,528 traders
Demographics			
Age (years)	48.4	50.6	39.7
Male (%)	50.2	79.2	83.0
Account Characteristics			
Account Tenure (years)	11.2	—	3.2
Trade Size (USD)	7,143	12,350	—
Monthly Volume (USD)	17,371	—	—
Monthly Turnover (%)	26.7	6.36	16.1
Portfolio Size (USD)	68,968	47,334	68,360

Table F.17: Returns Following Retail Purchases and Sales: Comparison with Odean (1999)

This table compares the average gross return over the 84 trading days following purchases and sales by individual investors in our sample (2012–2017) with the corresponding figures reported in Table 3 of Odean (1999). Returns are market-adjusted by subtracting the return on the market index over the same period and are expressed in percent. Each transaction is weighted equally. Difference is the return following purchases minus the return following sales. *** denotes significance at the 1% level based on the bootstrap test in Odean (1999).

	Market-adjusted return over 84 trading days (%)		
	Purchases	Sales	Difference
Odean (1999)	-1.33	0.12	-1.45***
Taiwan 2012–2017	-1.41	-1.08	-0.33***

Table F.18: Gross Day-Trading Returns: Comparison with Linnainmaa (2005)

This table compares the average gross daily return of day traders in our sample with the figure reported in Table 4 of Linnainmaa (2005) for Finland. Following Linnainmaa (2005), a day trade is a purchase and a sale of the same stock by the same investor on the same day, a day trader is an investor with at least one day trade in the previous three months, and the daily return is the profit from the day's trades, with any net position marked to the closing price, divided by the day's purchases plus sales. The return is averaged across day traders each day and then across days. The Taiwan sample covers domestic individual investors over the two years before the reform, from April 2015 to April 27, 2017. t-statistics are in parentheses.

	Gross day-trading return (% per day)
Linnainmaa (2005)	-0.21
	(-1.3)
Taiwan April 2015–April 2017	-0.04
	(-12.9)