The Economic Consequences of Lower Retail Trading Costs

Ching-Tse Chen* Ming-Jen Lin†

October 2025

Abstract

Brokerage commissions have fallen substantially worldwide over the past several decades. This paper studies how lower trading costs affect retail investors and the market through increased speculation. We leverage a 2017 tax reform in Taiwan that reduced the transaction tax specifically for day trading by 15 basis points. Using detailed account-level transaction data, we find that the reform hurt the average day trader financially due to two investor responses. First, day trading volume increases significantly by 30% but this increase is driven disproportionately by less sophisticated investors. Second, and surprisingly, day traders' gross returns per dollar traded worsen by 5 basis points, reducing the mechanical benefit from the tax cut. Consistent with the view that transaction costs serve a disciplinary role, we trace this performance deterioration to less attentive decision-making. Together, these responses result in a negative net impact on portfolio returns concentrated among investors with smaller holdings, whereas large investors benefit. In contrast with individual-level losses, market quality improves: intraday liquidity increases and volatility decreases. Overall, our findings highlight a policy-relevant trade-off: increased retail speculation from lower trading costs can benefit markets while harming individual investors.

*Booth School of Business, University of Chicago. Email: chingtse.chen@chicagobooth.edu.
†Department of Economics, National Taiwan University. Email: mjlin@ntu.edu.tw.

1 Introduction

Explicit trading costs for retail investors—most notably commission fees—have declined steadily around the world for decades, with brokers in many countries now offering zero-commission trading.[1] As lower trading costs make short-term speculation, such as day trading, more attractive among retail investors, the consequences for both individuals and financial markets more broadly remain unclear. On the one hand, lower trading costs can mechanically improve investors' net returns per trade and may enhance market liquidity through increased participation (Ross 1989; Schwert and Seguin 1993). On the other hand, cheaper trading can harm both investors and markets if it encourages excessive speculation by those less sophisticated (Tobin 1978, 1984; Stiglitz 1989; Summers and Summers 1989). These concerns have drawn growing regulatory attention as the global shift to commission-free trading has coincided with spikes in retail speculation, most notably the 2021 meme-stock frenzy and the rapid growth in short-dated options trading (CNBC 2020; SEC 2021; WSJ 2023).

Despite extensive debate and its policy relevance, causal evidence regarding the effects of lower trading costs on retail investors and the market through increased speculation remains limited. We fill this gap by examining a unique tax reform in Taiwan that halved the transaction tax on day trading—from 30 to 15 basis points per dollar traded—while keeping the tax on other trades unchanged. This setting offers several benefits. First, day trading, defined as the purchase and sale of the same stock within a day, is textbook speculation and dominated by retail investors in Taiwan. Second, day traders account for 15% of daily trading volume in Taiwan, making them an economically important investor group with potential market impacts. Finally, since this reform specifically targets day trading, it provides a unique opportunity for clean identification of both investor responses and their impact on the market.

Our analysis focuses on three connected questions: First, how do lower explicit trading costs affect trading behavior? Second, do investors benefit financially in terms of portfolio returns? Third, how do individual-level responses collectively shape market outcomes? Answering these questions has important implications for understanding the consequences of the recent rise in zero-commission trading and for informing policy debates on payment-for-order-flow (PFOF) and financial transaction taxes.

Throughout the paper, we use detailed account-level transaction data obtained from one of the largest brokerage firms in Taiwan. This dataset tracks trading activity across a large sample of individuals representing 4.5% of the aggregate trading volume. We confirm that the behavior of these investors is representative of the overall retail trading in Taiwan and that they share similar demographics and account characteristics with those studied in the classic retail trading literature such

1. Triggered by Robinhood's success, many brokerage firms worldwide now offer zero-commission trading. Examples include Australia (Morningstar 2020), Canada (Bloomberg 2021), the US (CNBC 2019), and the UK (TechCrunch 2018).

as Barber and Odean (2000) and Dorn and Huberman (2005). In addition to investor characteristics, we also observe granular order-level details. This information allows us to examine how day traders respond to the tax reform and the mechanisms underlying their market impacts.

To identify the effect of the tax reform on investor behavior, we employ a difference-in-differences strategy that compares traders directly affected by the reduction in day trading costs with those who share similar speculative motives but remain unaffected. Specifically, we leverage the fact that day trading is a persistent behavior and assign traders to treatment and control groups based on their day trading activity during a classification period—the year before our analysis sample begins. The treatment group consists of day traders, defined as individuals who executed at least one day trade during the classification period. The control group consists of active non-day traders who traded on more than 30 days but never executed a day trade during the same period. Given that non-day traders consistently avoid day trading, their behavior is largely unaffected by the reform. Our main identification assumption is parallel trends: active non-day traders provide a valid counterfactual for how day traders would have behaved in the absence of the reform. We support this assumption by showing that the two groups have similar observable characteristics and exhibit parallel pre-trends for various outcome variables.

We start by examining the impact of the tax reform on day traders' behavior, focusing on changes in trading volume and performance (measured by gross returns per dollar traded). Together, these investor responses determine the financial impact of the tax reform on individuals.

First, we document a significant 30% increase in day trading volume following the reform, while non-day trading volume remains virtually unchanged. This suggests that investors are aware of the transaction tax and confirms the reform specifically affected day trading behavior. Importantly, we find that the increase in day trading volume is concentrated among traders with smaller portfolios and worse past performance. Since these investors tend to incur large losses whenever they trade, their greater trading volume leads to increased losses.

Second, and strikingly, we find that day traders' gross returns per dollar traded fall by 5 basis points following the tax cut—a substantial 15% drop relative to the pre-reform mean. The 5-bp deterioration in gross returns leaves the actual improvement in day traders' net returns at only 10 basis points rather than the full 15 basis points from the tax cut, limiting the mechanical benefit of the tax reduction.

The large decline in per-dollar gross returns following the tax reform presents a puzzle worth further investigation because, by definition, higher trading volume alone does not directly affect performance at the individual trade level. It suggests that, rather than merely trading more while maintaining their existing strategies on average, day traders are changing their behavior in ways that systematically worsen performance—an outcome as difficult to achieve as consistently making profits.

One potential explanation relates to a widespread regulatory concern: that transaction costs serve a disciplinary function, and removing them creates an illusion of free trading that may encourage inattentive and careless trading (Cook 2021; ESMA 2021; Gensler 2021). Inattention, defined as underweighting relevant but low-salience information, has been shown to be associated with various retail investor biases and worse performance (Hirshleifer 2001; DellaVigna 2009; Gabaix 2014; Gargano and Rossi 2018; Gabaix 2019; Birru et al. 2024). Consistent with this view, we show that three inattentive trading behaviors, previously identified in the literature as detrimental to performance, increase following the tax cut, which helps explain the observed performance decline.

Specifically, we show that day traders (i) increasingly rely on cognitive shortcuts, (ii) reduce their monitoring of limit orders, and (iii) engage more frequently in salience-driven trading. First, we find 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 measure cognitive shortcut usage through the share of limit orders submitted at round-number prices. While this heuristic simplifies decision-making for the trader, it is associated with worse performance. We find that day traders' use of round-number limit orders increases by 2 percentage points after the tax reduction (7% of the pre-reform mean).

Second, we show that, following the reform, the idle time of limit orders submitted by day traders increases by 4%, while the probability of order modification or cancellation decreases by 0.77 percentage points (4% of the pre-reform mean). These behaviors are consistent with a reduction in monitoring effort and increased inattention, which increases investors' risk of being picked off by informed traders, leading to worse performance (Linnainmaa 2010).

Finally, we show day traders engage more frequently in salience-driven trading following the tax reform. Since attention is a scarce resource, retail investors often focus their trades on stocks that grab their attention (Seasholes and Wu 2007; Barber and Odean 2008). Using extreme overnight returns as a proxy for salient events, we find that day traders' probability of trading stocks that experienced extreme overnight returns increases by 0.8 percentage points post-reform (4% of the pre-reform mean). Crucially, their performance during these episodes is significantly worse than on typical trading days. These patterns suggest that as trading becomes cheaper, traders are increasingly influenced by salience rather than by information.

After documenting how the tax reform affects trading volume and performance, we then turn to the critical question: are investors financially better off in terms of portfolio returns? This is not obvious at first glance. On the one hand, the increased trading by less sophisticated investors has a negative impact on portfolio returns due to their poor performance. On the other hand, day traders realize a benefit from the tax cut (i.e. the 10-bp improvement in net returns per trade). In principle, they may still be better off if the realized tax cut benefit exceeds the additional losses from increased trading.

However, we find that lowering the cost of day trading actually reduces portfolio returns of the average day trader and carries substantial distributional consequences. The resulting negative impact from increased trading ultimately outweighs gains from the tax reduction. Moreover, the losses are unequally distributed. Investors with smaller portfolios experience a large decline in their portfolio returns from day trading, while large investors benefit. This finding highlights both the importance of investor responses to lower trading costs and the regressive nature of the day trading tax cut.

Besides investor financial welfare, increased retail speculation due to lower transaction costs may have market-level consequences. While retail investors are often considered noise traders (Barber, Odean, and Zhu 2009; Foucault, Sraer, and Thesmar 2011), the literature offers competing views regarding the market impact of noise trading. Noise traders may increase volatility by acting on incorrect beliefs, pushing prices away from fundamentals (Black 1986; De Long et al. 1990a; Campbell and Kyle 1993; Llorente et al. 2002) and harm liquidity by increasing market makers' inventory risk (Ho and Stoll 1981; Grossman and Miller 1988; Hendershott and Menkveld 2014). On the other hand, they can also provide liquidity either directly or indirectly (Kyle 1985; Glosten and Milgrom 1985), helping reduce price impact and volatility (Ross 1989; Schwert and Seguin 1993; Song and Zhang 2005).

To test which channel dominates, we exploit an institutional feature of Taiwan's market: a subset of stocks was ineligible for day trading and thus unaffected by the tax reform, providing a natural control group. Using a matched difference-in-differences approach, we find that the surge in day trading activity—despite driven mainly by less sophisticated investors—improves market quality. Specifically, following the tax reform, intraday liquidity increases (as measured by order book depth and quoted spreads) while volatility decreases (as measured by the standard deviation of 10-minute price changes). To shed light on the mechanism, we show that aggregate day traders exhibit contrarian behavior, consistent with them providing liquidity. Together, these results suggest that the liquidity-providing effect of noise trading can compensate for 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 reveals a nuanced picture of the impact of lower retail trading costs through increased speculation. At the individual level, lower trading costs harm investors—particularly the less sophisticated—by encouraging excessive trading that magnifies their total losses despite cheaper individual trades. At the market level, however, the increase in trading by unsophisticated investors does not lead to a deterioration in market quality. Instead, we document an increase in intraday liquidity and a decrease in volatility. Taken together, these findings highlight a trade-off policymakers face when designing interventions that affect retail investors' explicit trading costs.

Related literature. This paper contributes to the extensive literature on behavioral biases among 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). We contribute to this literature by showing that lowering explicit trading costs can exacerbate behavioral biases of retail investors. Moreover, retail investors, especially the least sophisticated ones, exhibit excessive sensitivity to reductions in transaction costs, increasing their trading activity to their own detriment.

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 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-based 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). Like 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). Importantly, because existing studies examine transaction cost changes that apply to all market participants, it is difficult to disentangle the source of these mixed results.[2]

2. 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

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 market-wide consequences of reduced transaction costs, painting a comprehensive picture of 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-commision 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), who study the effect of smartphone usage on investor behavior, and Barber et al. (2022), who examine the role of Robinhood's user interface, our work helps disentangle how different features of zero-commission platforms affect retail investors. Our findings therefore 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, by documenting the individual-level financial impact and market-level effects of lower trading costs. Moreover, our focus on day traders allows us to precisely measure changes in performance at the individual trade level due to their frequent trading and 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 on investors. Section 5 analyzes market-level effects of the tax reform. Section 6 discusses the external validity of our findings. 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). We then 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).

targeted transaction taxes on short-term speculators may be more effective in improving market quality.

2.1 Taiwan Stock Market and Day Trading

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 or Taipei Exchange, both of which operate consolidated limit order books that accept only limit orders. Regular trading sessions run from 9:00 AM to 1:30 PM, 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.

Retail traders are an important group of investors in Taiwan's stock market, accounting for approximately 70% of total daily trading volume. Among these retail traders, a significant subset engages in day trading—the purchase and sale of the same stock within a single trading day. Day trading appeals to many retail investors because it offers high leverage. 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. In 2016, day trading accounts for 15% of daily total trading volume, with retail investors conducting 92% of these day trades (Taiwan Ministry of Finance 2018).

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 typically charge commissions of 9.25 basis points on both purchases and sales, resulting in total round-trip transaction costs of 48.5 basis points.[3] 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 on day trading of common stocks (i.e., the purchase and sale of a stock within a single day) from 30 to 15 basis points (Business Today Taiwan 2017). Importantly, this tax cut only applies to those who conduct brokered transactions (e.g. retail investors), while proprietary trading by brokerage firms themselves only became eligible for the reduced rate one year later, starting April 28, 2018[4]. The reduced tax rate was initially implemented as a one-year temporary measure to boost stock market trading volume and liquidity.

3. While brokerage firms sometimes charge a statutory maximum commission of 14.25 basis points (one way), they often offer a 35 percent discount for online trades. To be conservative, we assume a commission fee of 9.25 basis points for all trades, consistent with the trade-weighted average commission of 10 basis points for the overall market reported in Barber et al. (2014).

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

However, it has since been extended multiple times and is currently scheduled to remain in effect through December 31, 2027.

The reform lowered total round-trip transaction costs (tax + commissions) for day trading from 48.5 to 33.5 basis points. Notably, not all common stocks are eligible for day trading in Taiwan. Roughly 85% of common stocks are eligible and the eligibility is determined by the exchanges based primarily on firms' listing tenure, market value, and financial status (e.g. profitability). In general, eligible stocks are larger, more liquid, and more profitable than ineligible stocks.

2.3 Data and Sample

For our analysis of investor responses to the transaction tax cut, we use comprehensive trading records for over 120,000 retail investors from a major Taiwanese brokerage. This dataset accounts for approximately 6% of total retail volume or 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 demographic characteristics (age, gender, and zipcode) and portfolio holdings. Panel A of Table C.1 presents summary statistics on the demographics and trading 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 those reported by Barber et al. (2014) and Barber et al. (2020), who observe the entire population of retail traders in Taiwan from 1995 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 witness 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 securities 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 stock, t indexes trading day, 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 where s indexes stock, t indexes trading day, and τ indexes quote updates within day t. where s indexes stocks, t indexes trading days, and τ indexes intraday quote updates. $\text{Bid}_{s\tau}$ 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 mid-quote 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 spread 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 gross return per dollar traded). The volume response is important for evaluating the incremental gains or losses investors generate through increased trading. The performance response allows us to assess the actual improvement in per-dollar net returns day traders realize from the tax cut. Together, these responses determine the financial impact of the reform on individuals.

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) and explore the mechanisms driving them (Section 3.5).

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 directly affected by the reduction in day trading costs (treatment) and investors with similar speculative trading motives but remain unaffected by the reform (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 more than 30 days but never executed a day trade during the same period.

This classification approach is motivated by a stable empirical pattern shown in Figure 1. Across all years 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%. Notably, 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.

Identification Assumption The validity of our approach 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.

By restricting our control group to frequent traders, we ensure it comprises investors who likely share day traders' speculative trading motives, rather than buy-and-hold investors who differ fundamentally from them. Moreover, Panel B of Table C.1 shows that our treatment and control groups have comparable demographics and account characteristics during the pre-reform period. This suggests both groups—treatment and control—are likely to respond comparably to changes in market conditions. Importantly, since control group members (defined by their classification-period behavior) are expected to have minimal day trading activity during the analysis period, their behavior should remain largely unaffected by 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 (Chen and Roth 2024; Wooldridge 2023):

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

where i denotes the investor 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 is an indicator for whether investor i is in the treatment group. Post_t is an indicator for 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 group 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:

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

Specifically, we estimate:

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

where i index investor and t index day. y_{it} denote outcomes 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 Figure 1), 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, in principle, require identical outcome definitions across groups. Instead, the validity depends on whether the relevant parallel trends assumptions—Equations 4 and 6—hold. We argue these assumptions are plausible given that our treatment and control groups share similar speculative motives and observable characteristics. Moreover, we provide direct empirical support through pre-period trend analysis. In Appendix B, we present the theoretical foundation for using different outcome variables in a DiD setting and show that our PT assumptions can be rationalized wtih two stronger conditions that are easier to interpret and economically meaningful.

Second, the validity of our control group could be compromised if control group investors also increase their day trading in response to the tax cut. However, any such response would have minimal impact on their total trading volume because their baseline probability of day trading is extremely low (Figure 1). Moreover, if the control group did increase day trading, it would likely bias our estimates toward zero, making our results conservative.

Third, our definition of "active" traders in the control group—trading on more than 30 days during the classification period—may appear arbitrary. We address this concern by showing our main results remain robust when the control group is defined using alternative thresholds for activeness (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—the year before the analysis period—we exclude investors who begin day trading after the classification period but before the reform. 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 suggest that, if anything, using our original group definition slightly understates the treatment effect of the tax reform.

Fifth, we only observe completed day trades—positions with both purchase and sale within the same trading day. However, an investor might initiate multiple positions intending to day trade but only close some of them within the day, holding others overnight due to unfavorable price movements or other considerations. To ensure our results reflect actual changes in day trading behavior rather than the tax reform affecting selective position closure, we follow the literature and examine intended day trades (Linnainmaa 2005; Barber et al. 2014; Barber et al. 2020). Specifically, we classify all new positions opened on days when an investor completes at least one day trade as intended day trades. First, Table C.2 shows that while the probability of day traders realizing intended day trades increases by 1.5 percentage points following the tax reform, the magnitude is economically small (day traders realize 77% of intended day trades on average pre-reform). Second, we show that our main results remain robust to this alternative definition of day trading in the following sections.

3.2 Trading Volume Responses

To understand the financial impact of the tax reform, we start by examining how day trading volume changes. While traders should theoretically account for transaction costs and respond to changes in them, whether this holds in practice remains an empirical question.

Estimating 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. Column (2) and (3) reveal that this increase is driven by day trading volume, which rises by 30% $(\exp(0.26) - 1)$, while non-day trading volume shows no significant change. In dollar terms, the 30% increase translate to $7500 a month. These estimates are internally consistent: given the treatment group's baseline day trading share of approximately 20%, a 30% increase in day trading volume implies a 6% increase in total trading volume if non-day trading is unaffected—remarkably close to the observed 5% increase.

Panel (a) of Figure 2 plots the biweekly day trading volume for the treatment group and the total trading volume for the control group, both scaled by their respective pre-reform means. Panel (b) shows the coefficients from our dynamic difference-in-differences specification at a biweekly frequency, with the coefficients converted into percentage terms for ease of interpretation. The figure supports our identifying assumption (Equation 4) as the percentage change in day trading volume for the treatment group evolves in parallel with the total trading volume of the control group prior to the reform. Following the implementation of the tax reform, we observe a sharp increase in day trading volume, suggesting that the response is indeed driven by the day trading tax cut.

Robustness Our results are robust to several design choices as mentioned in Section 3.1. Table C.3 shows that our estimated volume response is robust to alternative cutoffs for defining the control group. Column (1) of Table C.4 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 C.5 shows that our result is robust to expanding our definition of day trading to inteneded day trades (i.e. new positions opened on days when an investor completes at least one day trade).

We also examine whether day traders and other retail investors are exposed to different market trends from systematically trading different stocks. For instance, if day traders concentrate their trades in technology stocks while other investors trade more broadly, the observed treatment effect could reflect sector-specific trading frenzies rather than the tax impact. To address this, we calculate the contemporaneous correlation in trading volumes across stocks between the treatment and control groups. Figure C.1 shows the cross-sectional correlation between log day trading volume of the treatment group and log total trading volume of the control group at different time aggregation levels. The correlation averages 50% at the daily level and increases to 80% at the monthly level. This evidence is consistent with prior literature documenting the systematic and correlated nature of retail trades (Dorn, Huberman, and Sengmueller 2008; Barber, Odean, and Zhu 2009). These high correlations suggest the estimated treatment effect is unlikely to be driven by the two groups trading different sets of stocks.

3.3 Heterogeneity in Trading Volume Responses

Having documented that day trading volume increases following the tax reform, we now explore whether this response varies across investors with different levels of sophistication. The impact of the tax reform on individuals and the market could be very different depending on which type of investors drives the volume response. For instance, they would likely be more positive if the increased trading is driven primarily by more sophisticated investors. To this end, we proxy investor sophistication using investor's portfolio size and past trading performance.

For portfolio size, we calculate each investor's average holdings over the month preceding the tax reform and divide the sample into terciles. For past performance, we focus on investors with at least 30 days of day trading activity and compute their per-dollar gross returns from day trading during the classification period (i.e., the year prior to our analysis period, which is the same period for defining treatment and control groups). Based on these returns, we classify traders into three categories: (1) unprofitable before costs (negative gross returns), (2) unprofitable after costs (positive gross but negative net returns), and (3) profitable after costs (positive net returns). To calculate net returns, we apply round-trip transaction costs of 48.5 basis points, consisting of 30 basis points in taxes and 18.5 basis points in commissions. This approach follows Barber et al. (2020), who demonstrate that a similar measure reliably predicts future returns of day traders. Figure C.3 presents the distribution of past performance, revealing that most active day traders are unprofitable either before or after accounting for transaction costs.

Figure 3 presents the heterogeneous effects of the transaction tax reform on day trading volume across investors by sophistication. Panel (a) plots the effects by portfolio size terciles. Traders in the bottom tercile exhibit the largest percentage increase in day trading volume ($\approx$ 42%). The difference between the bottom and top terciles is statistically significant at the 5% level (t-statistic = 2.14). Panel (b) presents the effects by past performance and reveals a similar pattern: traders with poorer historical performance increase day trading volume more aggressively following the reform. Together, these results indicate that lower day trading costs disproportionately stimulate trading among less sophisticated investors.

Crucially, these less sophisticated investors tend to incur substantial losses whenever they trade. Figure 4 plots the average gross returns of investors by portfolio size terciles and by past performance during the six-month pre-reform period. The dashed red line in the figure indicates the total transaction costs following the tax reform (i.e. 33.5 basis points). As shown, those with smaller portfolios and worse past performance earn average gross returns that fail to cover transaction costs even after the tax cut. For instance, day traders in the bottom portfolio size tercile earn an average gross return of merely 20 basis points per dollar traded. Consequently, the more these investors trade due to the tax reform, the more losses they incur.

3.4 Performance Responses

We now examine how the tax reform affect day traders' performance, specifically their gross returns per dollar traded. This response is critical for assessing the financial impact of the tax cut, as it reflects the actual change in net returns that traders realize for each dollar traded. Even though we have shown that day trading volume increases following the reform, the effect on per-dollar gross returns remains unclear. If day traders increase trading while largely maintaining their existing strategies on average, we would expect no change in their gross returns per dollar traded.

To evaluate the effect of the reform on day trading performance, we estimate Equation 7, comparing per-dollar gross returns from day trades of the treatment group with per-dollar gross returns from all trades of the control group on the same day. Per-dollar gross returns are defined as

$$\text{Gross return}_{it} = \frac{\text{Gross Profits}_{it}}{\text{Trading Volume}_{it}}$$

where Trading Volume_{it} is the dollar value of positions opened by investor i on day t. Gross Profits_{it} of investor i on day t are calculated as the difference between the average sell price and the average buy price, multiplied by the total number of shares traded. For day trades, where both the opening and closing transactions occur on the same day, we use actual execution prices. For non-day trades, where the position remains open beyond the day of entry, we focus on the first-day return and use that day's closing price to approximate the cost of closing a position.

Surprisingly, we find that day traders' gross returns per dollar traded decline by 5 basis points following the tax reform. Table 2 reports the estimates from Equation 7, 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 to calculate gross returns. Column (1) shows that when all trades are included, the treatment group's per-dollar gross returns decrease by 2.3 basis points after the tax cut. However, columns (2) and (3) reveal that this decline is entirely driven by day trades. Specifically, per-dollar gross returns from day trades fall by 5.3 basis points, while per-dollar gross returns from non-day trades show no significant change. This pattern further confirms that the tax reform specifically affected day trading behavior.

The 5.3 basis point drop in gross returns implies that day traders' net returns increase by only 10 basis points rather than the full 15 basis points following the tax cut. To put the decline in perspective, the average treatment-group investor earns gross returns of 32 basis points per dollar day-traded. This means the tax reform results in a sizable 15% decline in day trading performance.

We also examine whether the decline in gross returns per dollar day-traded varies by investor size. Table C.11 reports the coefficient estimates from Equation 7 separately for each portfolio size tercile. All three coefficients are negative, indicating that day traders at all size levels experience declining per-dollar gross returns from day trading following the tax reform. Although the estimate for the top tercile is not statistically significant at 5% level, we cannot reject the null that the performance decline is equal between the top and bottom terciles.

Robustness Figure C.4 plots the estimated monthly treatment effects from a dynamic difference-in-differences specification. The outcome variable is gross returns per dollar traded. For the treatment group, gross returns are calculated from day trades only; for the control group, they are calculated from all trades. The figure shows that gross returns for the two groups evolve in parallel prior to the tax reform, supporting our parallel trends assumption that, absent the tax cut, the control group provides a valid counterfactual for the treatment group.

Our results are robust to several design choices as mentioned in Section 3.1. Table C.6 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 C.4, column 2). Column (2) of Table C.5 shows that our estimate is similar when we expand our definition of day trading to inteneded day trades (i.e. new positions opened on days when an investor completes at least one day trade).

We also examine whether our finding of day traders' performance decline is robust to alternative measures of counterfactual trading performance. First, Table C.7 shows that our result remain similar when we use market returns as an alternative benchmark. Day traders' average abnormal returns decline by 4.75 basis points following the tax cut, where abnormal returns are calculated as the intercept from regressing day traders' excess returns (average daily returns minus the risk-free rate) on excess value-weighted intraday market returns. Second, Table C.8 demonstrates that using longer holding periods for the control group—either 10-day or 30-day returns (normalized to daily)—instead of gross returns on the first day of trades yield similar results. These robustness checks confirm that our findings are not driven by the particular choice of control group performance measure.

3.5 Mechanisms

The substantial decline in gross returns per dollar day-traded following the tax reform presents a puzzle worth further investigation. If traders simply increase trading volume while maintaining their existing strategies, average per-dollar gross returns should remain unchanged. The observed decline in gross returns from day trading therefore indicates that, following the tax reform, day traders are altering their behavior in ways that systematically worsen their performance. This is quite striking since consistently selecting unprofitable trades is, in principle, as difficult as consistently choosing profitable ones.

In this section, we investigate the mechanisms underlying the observed performance decline by testing a widespread regulatory concern: that reducing explicit transaction costs weakens their disciplinary function and fosters impulsive or careless behavior among retail investors (Cook 2021; ESMA 2021; Gensler 2021). Investor Inattention can be defined as underweighting relevent but low-salience information and can lead to, for instance, greater reliance on intuitive and heuristic thinking (Hirshleifer 2001; DellaVigna 2009). Inattention has also been shown to be connected to greater retail investor biases and worse performance (Gabaix 2014; Gargano and Rossi 2018; Gabaix 2019; Birru et al. 2024).

Consistent with this view, we show that three inattentive behaviors that the literature has identified as detrimental to trading performance all increase following the tax reform, helping to explain the decline in day traders' gross returns. 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 as the placing 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 C.9. Figure C.5 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:

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

where # Limit orders at round-number$_{i,t}$ 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 portfolio value and poorer past performance are significantly more likely to submit round-number orders (See Figure C.6).

Table 3 reports the effect of the transaction tax reform on the reliance on cognitive shortcuts, estimated using Equation 7. The table indicates that the share of round-number limit orders submitted by day traders rises by 2 percentage points following the reform (7% of pre-reform mean). Figure

C.7 presents estimates from the dynamic DiD specification, which supports the parallel trends assumption. Because placing limit orders at round numbers can be viewed as reflecting less deliberative decision-making, our result suggests 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 reduces 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,

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

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 pm) refers to the market closing time, which is 1:30 pm in Taiwan.

The proability of order modificatioin is the share of limit orders modified or cancelled by investor i on day t:

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

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 0.77 percentage points (4% of 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 less attentive decision-making and may have contributed to the observed decline in trading performance.

Finally, we document that day traders' engagement in salience-driven trading increases following the tax reform, consistent with less attentive trading behavior. Retail investors face a formidable search problem when investing—there are thousands of stocks to choose from—but they have limited mental resources to assess the merits of each. Seasholes and Wu (2007) and Barber and Odean (2008) show that retail investors tend to trade stocks that are more likely to attract attention, suggesting that they rely on salient events to overcome the search problem. Based on this idea, we hypothesize that if day traders become less attentive and devote fewer mental resources to assessing potential trades, they will become more likely to trade stocks that have recently experienced salient events.

To test this hypothesis, we define salient events as days when a stock experiences an extreme overnight return, with an absolute value in the top 5% of all stocks.[5] For each investor, we compute the share of their trading volume on a given trading day that is in stocks with extreme returns X days prior to the trade, where X can be any chosen time interval (e.g., 2 days or 10 days). This measure captures the extent to which investors concentrate their trading on stocks that have recently exhibited significant price movements.

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

where Trading $\text{Volume}_{i,t}$ is the total trading volume of investor i on trading day t, and Trading volume in stocks with extreme overnight returns X days $\text{ago}_{i,t}$ is the trading volume in stocks that experienced extreme overnight returns X days prior to the trade by investor i on 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 pattern should be particularly prominent for stocks with very *recent* extreme returns and more muted for stocks that experienced extreme returns long time a ago (say, 10 days) or yet to experience those returns (say, -5 days). Therefore, we estimate multiple regressions varying the definition of salient stocks as placebo tests.

Figure 5 plots the coefficient estimates from estimating Equation 7, where the dependent variable measures the share of trades in stocks that experienced an extreme overnight return between -12 and 23 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 pre-reform mean). Crucially, we observe no similar change for stocks that experienced extreme returns three or more days earlier, nor do traders preemptively trade stocks ahead of future extreme returns. Moreover, Table C.10 shows that day traders, on average, earn 2

5. 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))

basis points lower returns from stocks with extreme overnight returns in the prior two days, suggesting these trades are driven more by salience rather than 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.

Taken together, the three behavioral changes documented in this section—(i) greater reliance on cognitive shortcuts, (ii) reduced monitoring of limit orders, and (iii) increased salience-driven trading—highlight an unintended consequence of lower transaction costs: cheaper trading encourages more inattentive behavior, which worsens trading performance and, thereby, limits the improvement in net returns per trade that lower trading costs should deliver.

4 The Financial Impact of the Transaction Tax Reform on Individuals

In this section, we show how we determine the financial impact of the tax reform on individuals. We define the financial impact as the change in the portfolio returns (PR) from day trading, calculated as changes in monthly net profits from day trading, scaled by the average portfolio size in the month before the reform. We focus on the contribution of day trading to portfolio returns since Sections 3.2 and 3.4 demonstrated that the reform has minimal impact on non-day trading volume and performance.

While we find that day traders' net returns per dollar traded rise after the tax cut, this improvement alone does not necessarily translate to better overall financial outcomes. Recall that we've shown the increase in trading volume following the reform is driven primarily by less sophisticated traders. As they continue to make net losses on each trade—even if those losses are smaller than before due to the tax cut—their increased trading might ultimately lead to a negative impact on portfolio returns.

Building on volume and performance responses estimated in earlier sections, we assess the financial impact of the tax reform on day traders through a simple decomposition exercise. Motivated by the investor heterogeneity documented in Section 3.3, we start by computing the impact on portfolio returns within each portfolio size tercile, g. Specifically, we approximate the impact as

$$\begin{aligned}
\mathbb{E}_g[\Delta PR] &= \mathbb{E}_g\left[\frac{\Delta \text{Net profit}}{\text{Portfolio Size}_{\text{pre}}}\right] = \mathbb{E}_g\left[\frac{\Delta\ (\text{Day-Trade Volume} \times \text{Net return})}{\text{Portfolio Size}_{\text{pre}}}\right] \\
&\approx \mathbb{E}_g\left[\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times \left(\underbrace{\mathbb{E}_g[\Delta \text{Tax}]}_{\text{Tax cut}} + \underbrace{\mathbb{E}_g[\Delta \text{Gross return}]}_{\text{Performance response}}\right) \\
&\quad + \mathbb{E}_g\left[\frac{\text{Net return}_{\text{post}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times \underbrace{\mathbb{E}_g[\Delta \text{Day-Trade Volume}]}_{\text{Volume response in \$}}
\end{aligned} \tag{8}$$

In this expression, $\mathbb{E}_g\left[\cdot\right]$ denotes average across investors in group g. $\frac{\Delta\text{Net profit}}{\text{Portfolio Size}_{\text{pre}}}$ denotes the change in monthly net profits from day trading, scaled by each investor's average portfolio size in the month prior to the reform. The term $\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}$ represents each investor's monthly day trading volume pre-reform divided by portfolio size. The average change in net returns per dollar day-traded due to the tax reform, $\mathbb{E}_g[\Delta\text{Net return}]$, is decomposed into two components: $\mathbb{E}_g[\Delta\text{Tax}]$, which represents the mechanical increase in net returns due to the tax cut (i.e. 15 bps), and $\mathbb{E}_g[\Delta\text{Gross return}]$, which represents the causal effect of the tax reform on per-dollar gross returns from day trading, estimated as in Section 3.4. The term $\frac{\text{Net return}_{\text{post}}}{\text{Portfolio Size}_{\text{pre}}}$ represents the post-reform net returns per dollar day-traded, divided by portfolio size, while $\mathbb{E}_g[\Delta\text{Day-Trade Volume}]$ represents the causal effect of the tax reform on monthly day trading volume in dollar terms, obtained by multiplying the estimates in Section 3.3 and the average pre-reform monthly day trading volume for each group g.

The approximation in Equation 8 will be exact if we assume that within each group g, the volume and performance responses are uncorrelated with traders' scaled performance $\left(\frac{\text{Net return}_{\text{post}}}{\text{Portfolio Size}_{\text{pre}}}\right)$ and sacled day trading volume $\left(\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right)$. To compute the financial impact of the tax reform on the average day trader, we aggregate across groups using a weighted sum:

$$\mathbb{E}\left[\Delta PR\right] = \sum_g s_g \, \mathbb{E}_g\left[\Delta PR\right] \tag{9}$$

where s_g is the population share of group g.

The first component of Equation 8 (i.e., $\mathbb{E}_g\left[\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times \mathbb{E}_g[\Delta\text{Tax}]$) captures the effect of the tax reform absent any investor responses. The second term ($\mathbb{E}_g\left[\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times \mathbb{E}_g[\Delta\text{Gross return}]$) reflects the additional financial impact due to changes in trading performance. In our setting, this term is negative—indicating that the performance response partially offset the financial gains from the tax cut. Finally, the third term, $\mathbb{E}_g\left[\frac{\text{Net return}_{\text{post}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times \mathbb{E}_g[\Delta\text{Day-Trade Volume}]$, isolates the incremental profits (or losses) relative to portfolio size generated from the increase in trading volume at the post-reform return level.

Figure 6 presents the decomposition of the financial impact of the transaction tax reform across portfolio size terciles, following Equation 8. Blue bars in the figure indicate that, absent any behavioral responses, investors in the bottom and middle tercile would have enjoyed a higher boost to their annualized portfolio returns from the tax reform than those in the top tercile (1.49 pp and 1.22 pp vs. 0.98 pp) since day trading volume is larger relative to their portfolios. However, due to volume and performance responses, they ultimately fare worse. First, because the tax reform negatively affects per-dollar gross returns from day trading across all terciles, the performance-induced impact is negative for all groups. Second, investors in the bottom and middle terciles increase their day trading volume aggressively despite earning negative net returns from day trading (see Table C.12). This result in substantial volume-induced losses of -3.6 pp and -1.13 pp. In contrast, the top tercile incurs a smaller volume-induced loss of -0.4 pp, consistent with a more moderate trading response and better ex-ante performance.

Overall, we find that the net financial impact of the reform increases with investor portfolio size. Investors in the top tercile realize a net gain equivalent to an annualized portfolio return of 0.27 pp, while those in the middle and bottom terciles suffer net losses of -0.2 pp and -2.71 pp, respectively. Our results underscore both the importance of investor responses to lower trading costs and their distributional consequences. Absent any change in behavior, smaller investors would have benefited more from the reform. Instead, they respond most aggressively to the tax cut but to their own detriment, resulting in worse outcomes for themselves. Table C.12 reports the average impact of the reform computed using Equation 9, with Column (11) showing that the average day trader's annualized portfolio return decreases by 0.83 percentage points following the tax cut.

5 The Market Impact of the Transaction Tax Reform

Beyond investor financial welfare, another main debate over lower retail trading costs concerns the potential market impacts from increased speculation. Retail investors are typically considered noise traders (Barber, Odean, and Zhu 2009; Foucault, Sraer, and Thesmar 2011). Yet, the market impact of noise trading is a longstanding debate. 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 markers (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 (Ross 1989; Schwert and Seguin 1993; Song and Zhang 2005).

To understand the market-level consequences of lower retail trading costs, we examine the impact of the day trading tax cut on market quality in this section. We first outline our empirical strategy for identifying the market impact (Section 5.1), and then present the results (Section 5.2).

5.1 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 unlikely to be influenced by the reform. Stocks eligible for day trading account for approximately 85% of all listed stocks.

For our sample, we exclude penny stocks with prices below 1 NTD and any stocks whose day trading eligibility changed during the pre-period. Such changes, however, are uncommon. The resulting treatment group includes stocks that are eligible for day trading at the time of the tax reform. The control group consists of stocks that are ineligible. As shown in Table 5, Panel A, control stocks are typically much smaller, less liquid, and less profitable.

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 (PT) assumption—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 market quality measures: order book depth, quoted spread, and realized volatility. This method reweights stocks in both groups so that their first moments for these outcome variables are comparable in the pre-period (Hainmueller 2012; Hainmueller and Xu 2013). 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 derived from a logistic regression.

Table 5, Panel B, reports summary statistics for the entropy-balanced sample. While we target balancing order book depth, quoted spread, 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:

$$y_{st} = \beta \text{ Treat}_s \times \text{Post}_t + \gamma_s + \delta_t + \varepsilon_{st} \tag{10}$$

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.

5.2 Results

We find that intraday liquidity increases and volatility decreases following the tax reform. Table 6 reports estimates of β from Equation 10, using liquidity and volatility measures as outcome variables. After the reform, order book depth increases by approximately 14% as shown in column (1). This result indicates that intraday liquidity of the treated stocks improves relative to the control stocks post-reform. Column (3) indicates that quoted spreads of treated stocks decline by 13%

relative to control stocks post-reform. Since quoted spreads proxy transaction costs, this result is consistent with enhanced liquidity. Additionally, realized volatility decreases by approximately 2.6 percentage points (10% of the pre-reform mean), as reported in column (5). Columns (2), (4), and (6) present results based on our alternative matching procedure. These estimates are similar in magnitude and confirm the robustness of our findings to matching approach. The dynamic difference-in-differences estimates for the entropy-balanced sample are shown in Figure 7. The absence of differential pre-trends across the outcome variables strengthens the parallel trend assumption.

Overall, these findings indicate that reducing day trading costs improves market quality and suggest that the liquidity-providing role of noise trading can compensate for its destabilizing effect. For instance, by helping absorb exogenous order imbalances or encouraging participation from informed traders, thereby reducing volatility.

To support this interpretation, we examine whether aggregate day traders in Taiwan exhibit behavior consistent with liquidity provision (i.e., buy when prices fall and sell when prices rise). 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

$$\text{Order Imbalance}_{s,t,\tau} = \frac{\text{Shares Bought}_{s,t,\tau} - \text{Shares Sold}_{s,t,\tau}}{\text{Shares Bought}_{s,t,\tau} + \text{Shares Sold}_{s,t,\tau}} \tag{11}$$

where s indexes stocks, t indexes trading days, and τ indexes 10-minute intervals within day t. Shares $\text{Bought}_{s,t,\tau}$ and Shares $\text{Sold}_{s,t,\tau}$ 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 regressions

$$\text{Order Imbalance}_{s,t,\tau} = \sum_{k}^{9} \beta_k \, \Delta \log(\text{Midquote})_{s,t,\tau-k} + \gamma_s + \delta_t + \varepsilon_{s,t,\tau} \tag{12}$$

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 8 plots the coefficient estimates β_k from Equation 12. Each point in the figure represents the estimated coefficient for returns over a specific 10-minute interval, with error bars indicating 95% confidence intervals. The x-axis denotes the time intervals in minutes before the current period, ranging from 90-80 minutes ago to the most recent 10-minute interval ([-10,0]).

The results suggest aggregate day traders in Taiwan are indeed contrarians. The coefficient estimates are predominantly negative, suggesting that past price decrases 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 10 percentage point increase in day traders' current order imbalance. This pattern aligns with prior findings on the contrarian nature of aggregate retail order flows (Kaniel, Saar, and Titman 2008; Kaniel et al. 2012; Kelley and Tetlock 2013; Barrot, Kaniel, and Sraer 2016; Boehmer et al. 2021). Moreover, it provides empirical support for the interpretation that the increase in day trading activity reduces volatility through liquidity provision, albeit in an unsophisticated manner.

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 is 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 C.13 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 upward. 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 C.8 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.

6 External Validity

Given that our paper is based on data from Taiwan, it is important to consider the extent to which our findings generalize beyond this context. In this section, we discuss the external validity of both the individual-level results (Sections 3 and 4) and the market-level results (Section 5).

At the individual level, our findings are likely to generalize beyond Taiwan for two reasons. First, retail investors across countries have been shown to exhibit remarkably similar behavior. Table C.14 highlights the prevalence of three well-established behavioral biases—excessive trading, the disposition effect, and gambling preferences—across multiple markets including the United States, Taiwan, and other countries. Given the broad cross-country consistency of retail investor behavior, the individual-level responses to the tax reform documented in this paper are unlikely to be confined to Taiwan alone. Second, the demographics of our brokerage sample are comparable to those examined in seminal studies such as Barber and Odean (2000) and Dorn and Huberman (2005). Table C.15 shows that the average characteristics of investors in our sample, including age, account size, are similar to those reported in these studies, further supporting the generalizability of our results.

At the market level, while Taiwan's market structure has distinctive features, we demonstrate that the observed market impacts are consistent with a specific mechanism: the contrarian trading behavior of aggregate retail day traders. This enhances the generalizability of our results since aggregate retail order flows are found to be contrarian across countries (Kaniel, Saar, and Titman 2008; Kaniel et al. 2012; Kelley and Tetlock 2013; Barrot, Kaniel, and Sraer 2016; Boehmer et al. 2021) and, in some cases, have stabilizing effects similar to our day traders (Eaton et al. 2022). Identifying this mechanism also provides us with a framework for understanding how our findings would likely differ, if at all, in other markets. Below, we discuss three distinctive features of Taiwan's market that may affect the external validity of our results by shaping day traders' role as liquidity providers.

First, Taiwan employed a batch auction mechanism for order matching in 2017, which differs from the continuous trading systems prevalent in most major markets. This mechanism restricts traders' ability to update quotes instantaneously in response to order flow, potentially dampening the liquidity-providing effect of day traders relative to continuous markets.

Second, the Taiwanese market sees minimal high-frequency trading and offers few institutional incentives for market making. In this environment, day traders effectively function as informal liquidity providers, despite in a less sophisticated manner. As a result, their liquidity provision role may carry more weight in Taiwan than in markets dominated by algorithmic and institutional market makers.

Third, retail investors account for about 70% of trading volume in Taiwan, far more than in most developed markets. The implications of this feature for our results depend on how retail trading affects market quality. On the one hand, prior work finds that individual investors' margin trading increase return volatility (Foucault, Sraer, and Thesmar 2011). On the other hand, as we noted above, aggregate retail order flows tend to be contrarian and may have stabilizing effects. To the extent that retail investors' contrarian trading stabilizes markets, their large presence may reduce the need for day traders as liquidity providers. Hence, improvements in market quality from reducing retail day trading costs may be larger in markets where institutional investors dominate.

7 Conclusion

This paper examines how lower retail trading costs affect investor and market outcomes through increased speculation, leveraging a tax reform in Taiwan that reduced the transaction tax by 15 basis points specifically for day trading. We show that the reform hurts the average day trader due to two investor responses. First, the tax cut leads to a substantial increase in day trading volume, particularly among less sophisticated investors with smaller portfolios and poorer past performance. Second, we also find that day traders' performance decline following the reform, leaving the actual improvement in net returns per trade at only 10 basis points, smaller than implied by the 15-basis-point tax cut. Because of these responses, we show that the reform leaves the average day trader worse off, with losses concentrated among small investors, whereas large investors benefit. At the market level, however, greater participation by unsophisticated traders does not worsen market quality. Instead, we find that increased day trading improves intraday liquidity and reduces volatility, suggesting that the liquidity provision function of noise trading can compensate for its destabilizing effects.

These findings highlight a central trade-off for policymakers: while reducing transaction costs can enhance market-level outcomes such as liquidity, it may also encourage excessive trading that harms individual investors. This tension is directly relevant to ongoing policy debates, including the regulation of payment for order flow (PFOF), which could influence the availability of zero-commission trading, and the design of financial transaction taxes (FTTs), which aim to curb speculative activity. By identifying both investor-level and market-level effects, our results underscore the importance of evaluating how changes in trading costs affect not only the market but also investor behavior and welfare.

There are several natural extention for future research. First, our focus on short-term financial impacts may miss other important effects. Non-monetary costs—such as time diverted to trading from work or leisure—may rise even as transaction costs fall. In the longer run, lower trading costs may help some investors improve through learning, but they could also reinforce speculative habits or overconfidence. Second, our analysis abstracts away from substitution or complementarity across trading activities. When trading becomes cheaper in one domain, do investors scale back other speculative activities—such as options or cryptocurrency trading—or do they expand their overall speculative exposure? This question is particularly relevant given that many brokerage platforms cross-subsidize zero-fee trading by charging for other services, such as margin lending. Third, a comprehensive welfare analysis that incorporates both individual- and market-level effects in a quantitative framework could provide a more complete assessment of the net social impact of lower transaction costs.

References

Barber, Brad M, Xing Huang, Terrance Odean, and Christopher Schwarz. 2022. "Attention-induced trading and returns: Evidence from Robinhood users." *The Journal of Finance* 77 (6): 3141–3190. (6, 7, 21).

Barber, Brad M, Yi-Tsung Lee, Yu-Jane Liu, and Terrance Odean. 2007. "Is the aggregate investor reluctant to realise losses? Evidence from Taiwan." *European Financial Management* 13 (3): 423–447. (6).

———. 2014. "The cross-section of speculator skill: Evidence from day trading." *Journal of Financial Markets* 18:1–24. (8, 9, 14, 51).

Barber, Brad M, Yi-Tsung Lee, Yu-Jane Liu, Terrance Odean, and Ke Zhang. 2020. "Learning, fast or slow." *The Review of Asset Pricing Studies* 10 (1): 61–93. (9, 14, 16, 51).

Barber, Brad M, and Terrance Odean. 2000. "Trading is hazardous to your wealth: The common stock investment performance of individual investors." *The journal of Finance* 55 (2): 773–806. (3, 28).

———. 2001. "Boys will be boys: Gender, overconfidence, and common stock investment." *The quarterly journal of economics* 116 (1): 261–292. (6).

———. 2008. "All that glitters: The effect of attention and news on the buying behavior of individual and institutional investors." *The review of financial studies* 21 (2): 785–818. (4, 6, 21).

Barber, Brad M, Terrance Odean, and Ning Zhu. 2009. "Systematic noise." *Journal of Financial Markets* 12 (4): 547–569. (5, 15, 24).

Barrot, Jean-Noel, Ron Kaniel, and David Sraer. 2016. "Are retail traders compensated for providing liquidity?" *Journal of Financial Economics* 120 (1): 146–168. (26–28).

Birru, Justin, Fernando Chague, Rodrigo De-Losso, and Bruno Giovannetti. 2024. "Attention and biases: Evidence from tax-inattentive investors." *Management Science* 70 (10): 7101–7119. (4, 19).

Black, Fischer. 1986. "Noise." *The journal of finance* 41 (3): 528–543. (5, 24).

Bloomberg. 2021. "Zero-Commission Fight Reaches Canada as National Bank Ends Fees." *Bloomberg,* https://news.bloomberglaw.com/banking-law/zero-commission-fight-reaches-canada-as-national-bank-ends-fees. (2).

Boehmer, Ekkehart, Charles M Jones, Xiaoyan Zhang, and Xinran Zhang. 2021. "Tracking retail investor activity." *The Journal of Finance* 76 (5): 2249–2305. (27, 28).

Bryzgalova, Svetlana, Anna Pavlova, and Taisiya Sikorskaya. 2023. "Retail trading in options and the rise of the big three wholesalers." *The Journal of Finance* 78 (6): 3465–3514. (7).

Business Today Taiwan. 2017. "Day Trading Tax Reduction Launched: Three Key Indicators for Finding Buy and Sell Timing," https://www.businesstoday.com.tw/article/category/183008/post/201705040030/. (8).

Calvet, Laurent E, John Y Campbell, and Paolo Sodini. 2009. "Fight or flight? Portfolio rebalancing by individual investors." *The Quarterly journal of economics* 124 (1): 301–348. (6).

Campbell, John Y, and Albert S Kyle. 1993. "Smart money, noise trading and stock price behaviour." *The Review of Economic Studies* 60 (1): 1–34. (5, 24).

Capelle-Blancard, Gunther, and Olena Havrylchyk. 2016. "The impact of the French securities transaction tax on market liquidity and volatility." *International Review of Financial Analysis* 47:166–178. (6).

Chen, Jiafeng, and Jonathan Roth. 2024. "Logs with zeros? Some problems and solutions." *The Quarterly Journal of Economics* 139 (2): 891–936. (12).

CNBC. 2019. "Fidelity joins the stampede to eliminate fees for online trading." *CNBC,* https://www.cnbc.com/2019/10/10/fidelity-joins-the-stampede-to-eliminating-fees-for-online-trading.html. (2).

———. 2020. "SEC's Gensler says brokerage apps want users to trade frequently, which can be bad for small investors." *CNBC,* https://www.cnbc.com/2022/01/19/secs-gensler-warns-investors-about-frequent-trades-on-brokerage-apps.html. (2).

Coelho, Maria. 2016. "Dodging Robin Hood: Responses to France and Italy's financial transaction taxes." *Available at SSRN 2389166,* (6).

Colliard, Jean-Edouard, and Peter Hoffmann. 2017. "Financial transaction taxes, market composition, and liquidity." *The Journal of Finance* 72 (6): 2685–2716. (6).

Cook, Robert W. 2021. "Statement before the Financial Services Committee U.S. House of Representatives." *FINRA,* https://www.finra.org/media-center/speeches-testimony/statement-financial-services-committee-us-house-representatives. (4, 19).

De Long, J Bradford, Andrei Shleifer, Lawrence H Summers, and Robert J Waldmann. 1990a. "Noise trader risk in financial markets." *Journal of political Economy* 98 (4): 703–738. (5, 24).

———. 1990b. "Positive feedback investment strategies and destabilizing rational speculation." *the Journal of Finance* 45 (2): 379–395. (24).

de Silva, Tim, Kevin Smith, and Eric C So. 2023. "Losing is optional: Retail option trading and expected announcement volatility." *Available at SSRN 4050165,* (7).

DellaVigna, Stefano. 2009. "Psychology and economics: Evidence from the field." *Journal of Economic literature* 47 (2): 315–372. (4, 19).

Deng, Yongheng, Xin Liu, and Shang-Jin Wei. 2018. "One fundamental and two taxes: When does a Tobin tax reduce financial price volatility?" *Journal of Financial Economics* 130 (3): 663–692. (6).

Dorn, Daniel, and Gur Huberman. 2005. "Talk and action: What individual investors say and what they do." *Review of Finance* 9 (4): 437–481. (3, 6, 28).

Dorn, Daniel, Gur Huberman, and Paul Sengmueller. 2008. "Correlated trading and returns." *The Journal of Finance* 63 (2): 885–920. (15).

Dorn, Daniel, and Paul Sengmueller. 2009. "Trading as entertainment?" *Management science* 55 (4): 591–603. (6).

Eaton, Gregory W, T Clifton Green, Brian S Roseman, and Yanbin Wu. 2022. "Retail trader sophistication and stock market quality: Evidence from brokerage outages." *Journal of Financial Economics* 146 (2): 502–528. (7, 28).

Engelberg, Joseph E, and Christopher A Parsons. 2011. "The causal impact of media in financial markets." *the Journal of Finance* 66 (1): 67–97. (6).

ESMA. 2021. "ESMA Public Statement PFOF and zero-commission brokers." *Statement,* ESMA35-43–2749. https://www.esma.europa.eu/sites/default/files/library/esma35-43-2749_esma_public_statement_pfof_and_zero-commission_brokers.pdf. (4, 19).

Even-Tov, Omri, Kimberlyn Munevar, Shimon Kogan, and Eric C. So. 2022. "Fee the people: Retail investor behavior and trading commission fees." *MIT Sloan Research Paper No. 6801-22,* (7).

Fedyk, Valeria. 2021. "This time is different: Investing in the age of Robinhood." *Available at SSRN 4112307,* (7, 21).

Foucault, Thierry, David Sraer, and David J Thesmar. 2011. "Individual investors and volatility." *The Journal of Finance* 66 (4): 1369–1406. (5, 24, 28).

Gabaix, Xavier. 2014. "A sparsity-based model of bounded rationality." *The Quarterly Journal of Economics* 129 (4): 1661–1710. (4, 19).

———. 2019. "Behavioral inattention." In *Handbook of behavioral economics: Applications and foundations 1,* 2:261–343. Elsevier. (4, 19).

Gao, Xiaohui, and Tse-Chun Lin. 2015. "Do individual investors treat trading as a fun and exciting gambling activity? Evidence from repeated natural experiments." *The Review of Financial Studies* 28 (7): 2128–2166. (6).

Gargano, Antonio, and Alberto G Rossi. 2018. "Does it pay to pay attention?" *The Review of Financial Studies* 31 (12): 4595–4649. (4, 19).

Gensler, Gary. 2021. “Prepared Remarks at the Global Exchange and FinTech Conference.” *U.S. Securities and Exchange Commission,* https://www.sec.gov/newsroom/speeches-statements/gensler-global-exchange-fintech-2021-06-09. (4, 19).

Gilovich, Thomas, Dale Griffin, and Daniel Kahneman. 2002. *Heuristics and biases: The psychology of intuitive judgment.* Cambridge university press. (4, 19).

Glaser, Markus, and Martin Weber. 2007. “Overconfidence and trading volume.” *The Geneva Risk and Insurance Review* 32 (1): 1–36. (6).

Glossner, Simon, Pedro Matos, Stefano Ramelli, and Alexander F Wagner. 2025. “Do institutional investors stabilize equity markets in crisis periods? Evidence from COVID-19.” *Management Science,* (7).

Glosten, Lawrence R, and Paul R Milgrom. 1985. “Bid, ask and transaction prices in a specialist market with heterogeneously informed traders.” *Journal of financial economics* 14 (1): 71–100. (5).

Gomber, Peter, Martin Haferkorn, and Kai Zimmermann. 2016. “Securities transaction tax and market quality–The case of France.” *European Financial Management* 22 (2): 313–337. (6).

Graham, John R, Campbell R Harvey, and Hai Huang. 2009. “Investor competence, trading frequency, and home bias.” *Management science* 55 (7): 1094–1106. (6).

Grinblatt, Mark, and Matti Keloharju. 2001. “What makes investors trade?” *The journal of Finance* 56 (2): 589–616. (6).

———. 2009. “Sensation seeking, overconfidence, and trading activity.” *The Journal of finance* 64 (2): 549–578. (6).

Grossman, Sanford J, and Merton H Miller. 1988. “Liquidity and market structure.” *the Journal of Finance* 43 (3): 617–633. (5, 24).

Grundfest, Joseph A, and John B Shoven. 1991. “Adverse implications of a securities transactions excise tax.” *Journal of Accounting, Auditing & Finance* 6 (4): 409–442. (6).

Hainmueller, Jens. 2012. “Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies.” *Political analysis* 20 (1): 25–46. (25).

Hainmueller, Jens, and Yiqing Xu. 2013. “Ebalance: A Stata package for entropy balancing.” *Journal of Statistical Software* 54:1–18. (25).

Hendershott, Terrence, and Albert J Menkveld. 2014. “Price pressures.” *Journal of Financial economics* 114 (3): 405–423. (5, 24).

Hirshleifer, David. 2001. "Investor psychology and asset pricing." *The journal of Finance* 56 (4): 1533–1597. (4, 19).

Ho, Thomas, and Hans R Stoll. 1981. "Optimal dealer pricing under transactions and return uncertainty." *Journal of Financial economics* 9 (1): 47–73. (5, 24).

Hvozdyk, Lyudmyla, and Serik Rustanov. 2016. "The effect of financial transaction tax on market liquidity and volatility: An Italian perspective." *International Review of Financial Analysis* 45:62–78. (6).

Jin, Lawrence J, and Cameron Peng. 2023. "The law of small numbers in financial markets: Theory and evidence." *Available at SSRN 3066369,* (6).

Jones, Charles M, and Paul J Seguin. 1997. "Transaction costs and price volatility: evidence from commission deregulation." *The American Economic Review* 87 (4): 728–737. (6).

Kalda, Ankit, Benjamin Loos, Alessandro Previtero, and Andreas Hackethal. 2021. *Smart (Phone) Investing? A within Investor-time Analysis of New Technologies and Trading Behavior.* Technical report. National Bureau of Economic Research. (7).

Kaniel, Ron, Shuming Liu, Gideon Saar, and Sheridan Titman. 2012. "Individual investor trading and return patterns around earnings announcements." *The Journal of Finance* 67 (2): 639–680. (27, 28).

Kaniel, Ron, Gideon Saar, and Sheridan Titman. 2008. "Individual investor trading and stock returns." *The Journal of finance* 63 (1): 273–310. (27, 28).

Kelley, Eric K, and Paul C Tetlock. 2013. "How wise are crowds? Insights from retail orders and stock returns." *The Journal of Finance* 68 (3): 1229–1265. (27, 28).

Kuo, Wei-Yu, Tse-Chun Lin, and Jing Zhao. 2015. "Cognitive limitation and investment performance: Evidence from limit order clustering." *The Review of Financial Studies* 28 (3): 838–875. (4, 19).

Kupiec, Paul H. 1996. "Noise traders, excess volatility, and a securities transactions tax." *Journal of financial services research* 10 (2): 115–129. (6).

Kyle, Albert S. 1985. "Continuous auctions and insider trading." *Econometrica: Journal of the Econometric Society,* 1315–1335. (5).

Linnainmaa, Juhani. 2005. "The individual day trader." *University of California, Berkeley, working paper,* (14).

———. 2010. "Do limit orders alter inferences about investor performance and behavior?" *The Journal of Finance* 65 (4): 1473–1506. (4, 20).

Liu, Hongqi, Cameron Peng, Wei A Xiong, and Wei Xiong. 2022. "Taming the bias zoo." *Journal of Financial Economics* 143 (2): 716–741. (6).

Liu, Shinhua, and Zhen Zhu. 2009. "Transaction costs and price volatility: new evidence from the Tokyo Stock Exchange." *Journal of Financial Services Research* 36:65–83. (6).

Llorente, Guillermo, Roni Michaely, Gideon Saar, and Jiang Wang. 2002. "Dynamic volume-return relation of individual stocks." *The Review of financial studies* 15 (4): 1005–1047. (5, 24).

Morningstar. 2020. "$0 commissions: Australia's trading fee shakedown gathers steam." *Morningstar,* https://www.morningstar.com.au/stocks/0-commissions-australia-s-trading-fee-shakedown-gathers-steam. (2).

Odean, Terrance. 1998a. "Are investors reluctant to realize their losses?" *The Journal of finance* 53 (5): 1775–1798. (6).

———. 1998b. "Volume, volatility, price, and profit when all traders are above average." *The journal of finance* 53 (6): 1887–1934. (6).

Ozik, Gideon, Ronnie Sadka, and Siyi Shen. 2021. "Flattening the illiquidity curve: Retail trading during the COVID-19 lockdown." *Journal of Financial and Quantitative Analysis* 56 (7): 2356–2388. (7).

Reuters. 2024. "EU approves new rules to shake up market price data for investors," https://www.reuters.com/markets/europe/eu-approves-new-rules-shake-up-market-price-data-investors-2024-01-16/. (7).

Ross, Stephen A. 1989. "Commentary: Using tax policy to curb speculative short-term trading." In *Regulatory Reform of Stock and Futures Markets: A Special Issue of the Journal of Financial Services Research,* 19–22. Springer. (2, 5, 6, 24).

Schwert, G William, and Paul J Seguin. 1993. "Securities transaction taxes: an overview of costs, benefits and unresolved questions." *Financial Analysts Journal* 49 (5): 27–35. (2, 5, 6, 24).

Seasholes, Mark S, and Guojun Wu. 2007. "Predictable behavior, profits, and attention." *Journal of Empirical Finance* 14 (5): 590–610. (4, 6, 21).

SEC. 2021. "Staff Report on Equity and Options Market Structure Conditions in Early 2021." *U.S. Securities and Exchange Commission,* https://www.sec.gov/files/staff-report-equity-options-market-struction-conditions-early-2021.pdf. (2).

Shleifer, Andrei, and Lawrence H Summers. 1990. "The noise trader approach to finance." *Journal of Economic perspectives* 4 (2): 19–33. (24).

Song, Frank M, and Junxi Zhang. 2005. "Securities transaction tax and market volatility." *The Economic Journal* 115 (506): 1103–1120. (5, 6, 24).

Stiglitz, Joseph E. 1989. "Using tax policy to curb speculative short-term trading." *Regulatory Reform of Stock and Futures Markets: A Special Issue of the Journal of Financial Services Research*, 3–17. (2, 6).

Summers, Lawrence H, and Victoria P Summers. 1989. "When financial markets work too well: A cautious case for a securities transactions tax." *Journal of financial services research* 3:261–286. (2, 6).

Taiwan Ministry of Finance. 2018. "Evaluation Report on the Preferential Tax Treatment of Reduced Day Trading Transaction Tax," https://shorturl.at/Q3QNv. (8).

TechCrunch. 2018. "Freetrade launches 'zero-fee' investment app." *TechCrunch*, https://techcrunch.com/2018/10/02/freetrade/?utm_source=chatgpt.com. (2).

Tobin, James. 1978. "A proposal for international monetary reform." *Eastern economic journal* 4 (3/4): 153–159. (2, 6).

———. 1984. "On the efficiency of the financial-system." *Lloyds Bank Annual Review*, no. 153, 1–15. (2, 6).

Umlauf, Steven R. 1993. "Transaction taxes and the behavior of the Swedish stock market." *Journal of Financial Economics* 33 (2): 227–240. (6).

Welch, Ivo. 2022. "The wisdom of the Robinhood crowd." *The Journal of Finance* 77 (3): 1489–1527. (7).

Wooldridge, Jeffrey M. 2023. "Simple approaches to nonlinear difference-in-differences with panel data." *The Econometrics Journal* 26 (3): C31–C66. (12).

WSJ. 2023. "Amateurs Pile Into 24-Hour Options: 'It's Just Gambling'." *The Wall Street Journal*, https://www.wsj.com/finance/stocks/options-individual-investors-risk-gambling-a97bee1a. (2).

Figures

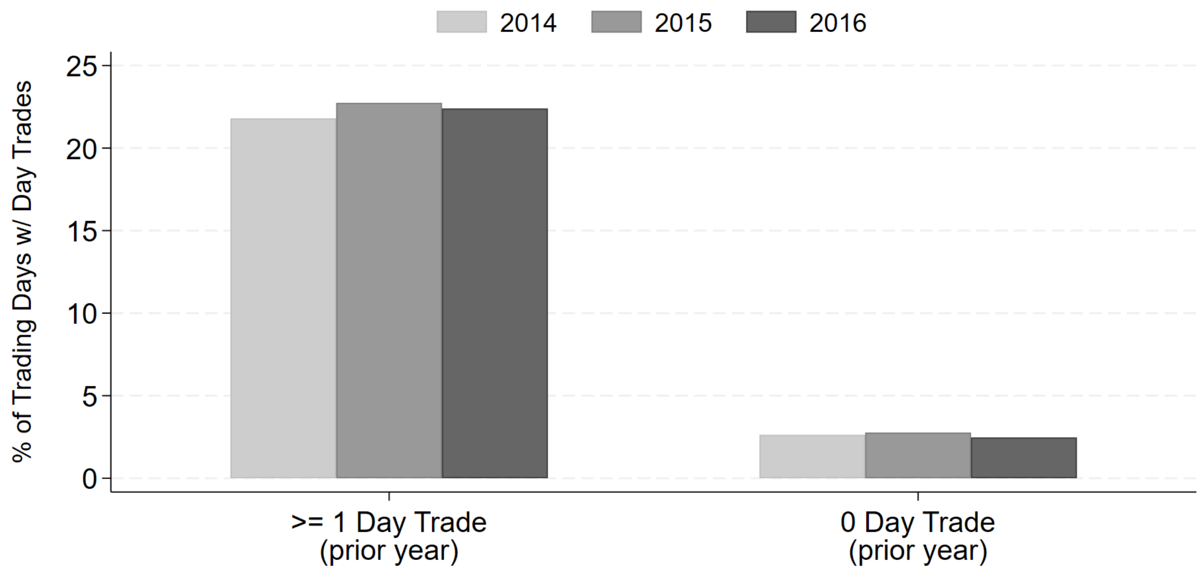


Figure 1: Percentage of Trading Days with Day Trades

This figure shows the average share of trading days with day trades, separately by year (2014–2016) and by investors' day trading status in the prior year. For each year, investors are grouped based on whether they executed at least one day trade in the previous calendar year (labeled "$\geq$ 1 day trade (prior year)") or none ("0 day trade (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.

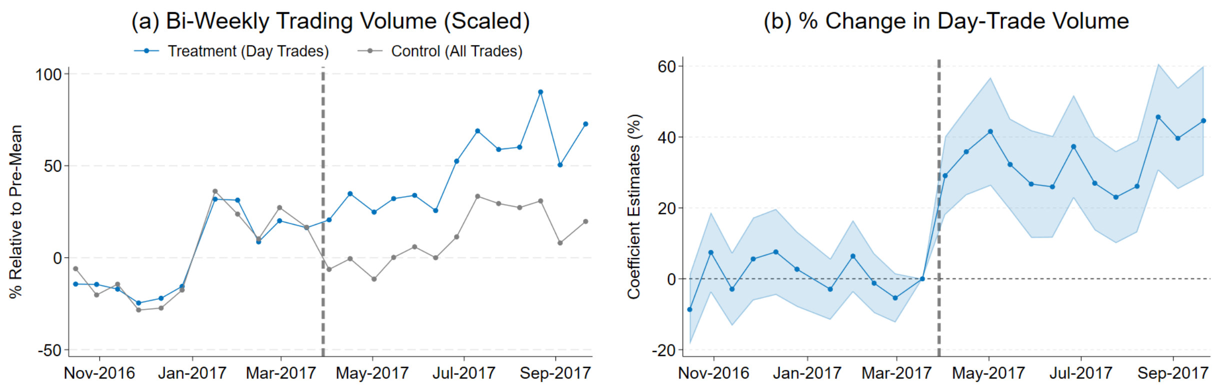


Figure 2: Effect of the Transaction Tax Reform on Day Trading Volume

This figure plots the effect of the transaction tax reform on trading volume. Panel (a) shows the biweekly average day trading volume for the treatment group and the average 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. 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. The outcome is day trading volume for the treatment group and total trading volume for the control group. Coefficients are expressed as percent changes relative to baseline. Shaded areas represent 95% confidence intervals.

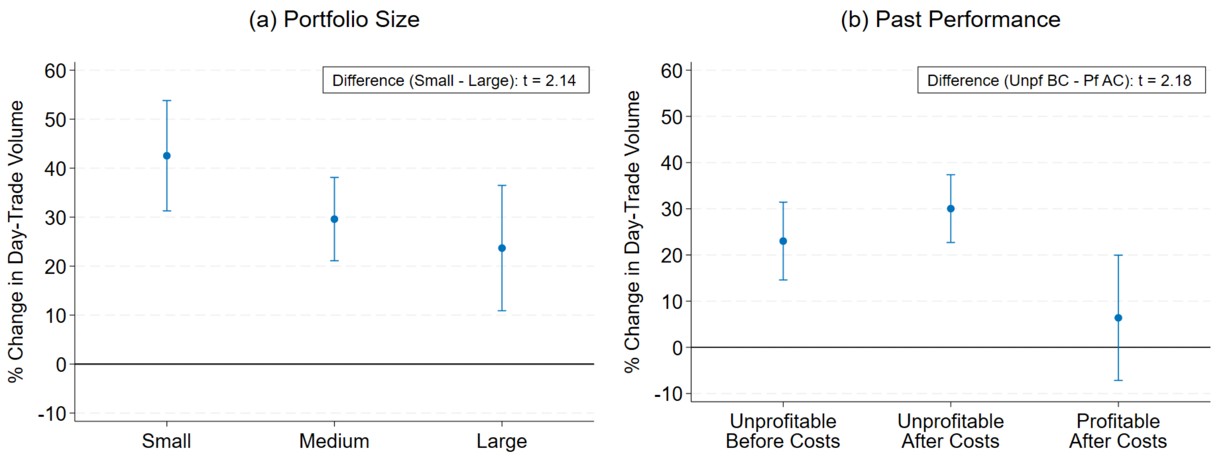


Figure 3: Heterogeous Effect of the Transaction Tax Reform on Day Trading Volume

This figure plots heterogeneous effects of the transaction tax reform on day trading volume by investor sophistication. Panel (a) estimates the following equation separately for each portfolio size tercile g:

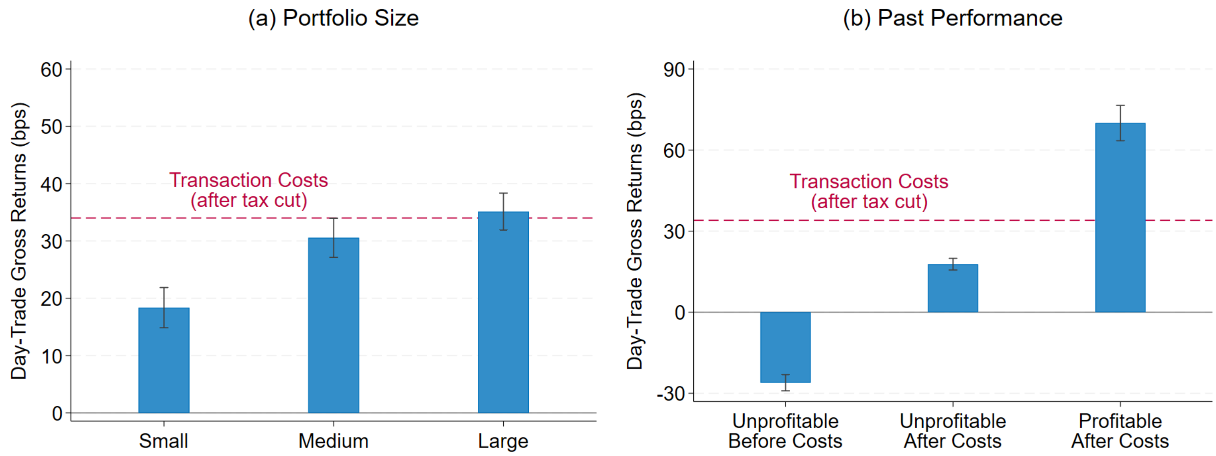
$$\text{Volume}_{it} = \exp(\beta_g \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t)\varepsilon_{it} \quad \text{where Portfolio Size Tercile}_i = g$$

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. Portfolio size terciles are based on average portfolio holdings in the month prior to the reform, where $g \in \{\text{Bottom, Middle, Top}\}$. γ_i and δ_t denote investor and time fixed effects.
Panel (b) estimates the following difference-in-differences specification:

$$\text{Volume}_{it} = \exp\left(\sum_g \beta_g \, \text{SkillBin}_{i,g} \times \text{Post}_t + \gamma_i + \delta_t\right)\varepsilon_{it}$$

where $\text{SkillBin}_{i,g}$ is an indicator equal to 1 if investor i is in the treatment group and belongs to skill group g, and 0 otherwise (including for all control investors). Skill groups are defined based on gross returns from day trading in the year prior to the analysis sample period: unprofitable before costs, unprofitable after costs, and profitable after costs. Volume_{it}, Post_t, γ_i, and δ_t are defined as in panel (a). Estimates are reported as $\exp(\beta) - 1$, representing percent changes in trading volume relative to the control group. Error bars indicate 95% confidence intervals. The t-statistics shown in the top right of each panel test whether the difference in coefficients between groups is statistically significant. In Panel (a), we test the difference between the bottom and top portfolio size terciles. In Panel (b), we test the difference between traders who were unprofitable after costs and those who were profitable after costs in the year prior to the analysis sample period.

Figure 4: Ex-ante Performance of Day Taders by Sophistication



This figure plots the average gross returns per dollar day-traded (in basis points) by investor sophistication for investors in the treatment group during the six-month pre-reform period (November 2016 to April 2017). Panel (a) groups investors into terciles based on average portfolio holdings in the month prior to the reform. Panel (b) groups investors into three categories based on their gross returns from day trading during the classification period (the year prior to the analysis sample period): (1) unprofitable before costs (negative gross returns), (2) unprofitable after costs (positive gross returns but negative net returns after accounting for 48.5 basis points in round-trip transaction costs), and (3) profitable after costs (positive net returns). The dashed horizontal line at 33.5 basis points indicates the total round-trip transaction costs (tax plus commission) following the tax reform. Error bars represent 95% confidence intervals.

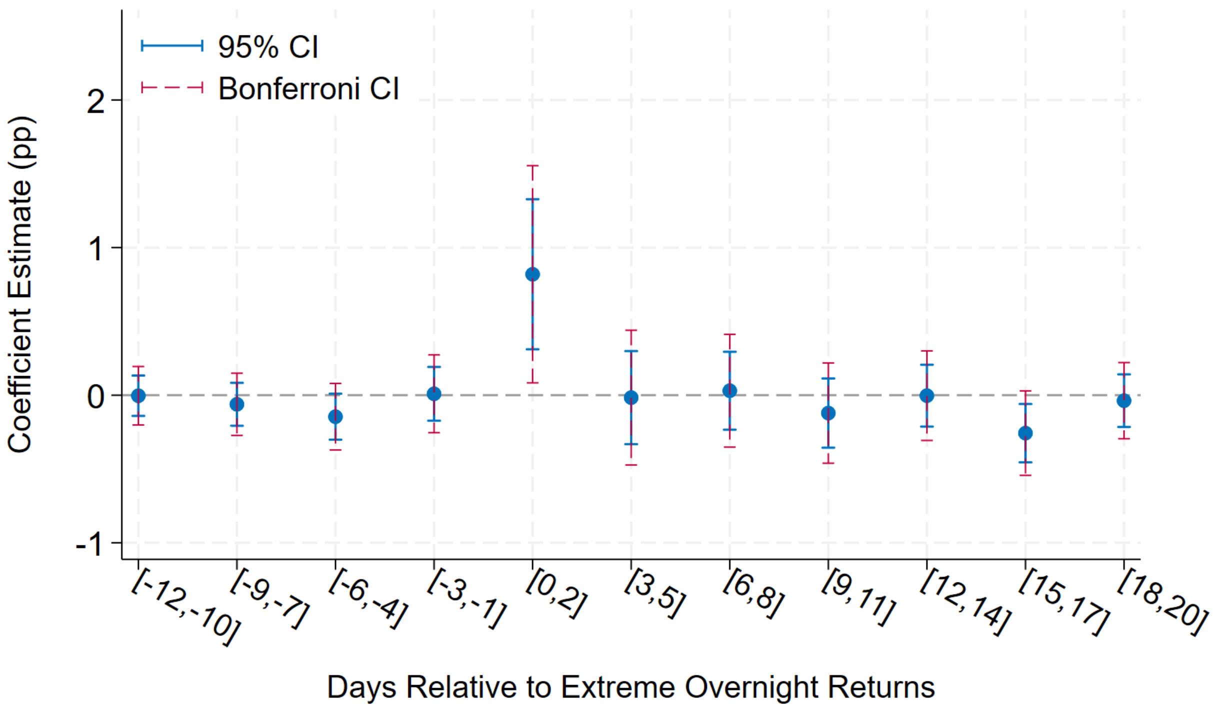


Figure 5: Changes in Propensity to Trade Stocks with Prior Extreme Returns

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. Extreme overnight returns are defined as stocks with absolute overnight returns in the top 5% of all stocks on a given day. The horizontal axis shows X ranging from -12 to 23 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.

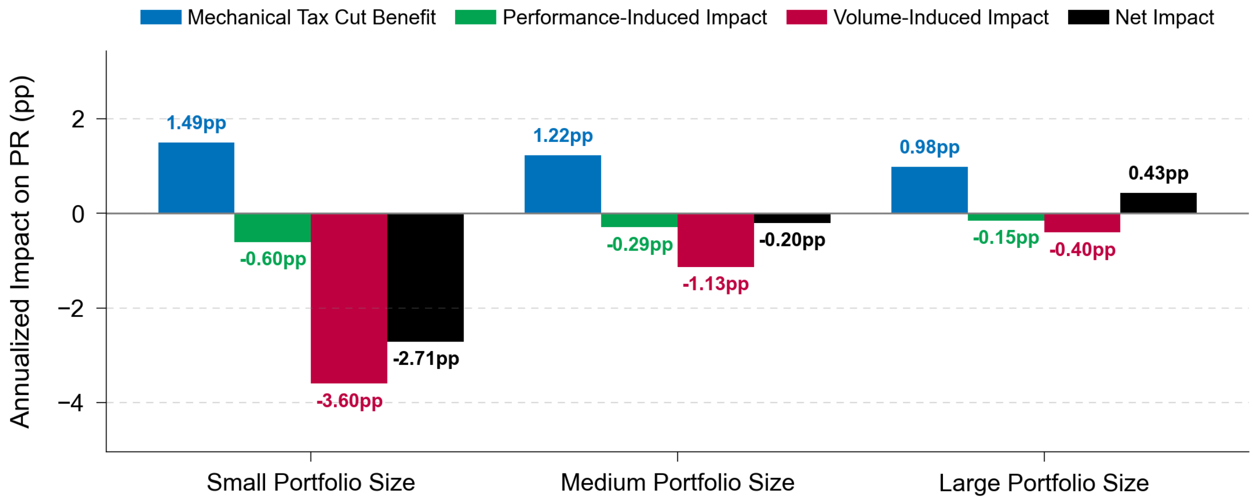


Figure 6: Decomposition of the Financial Impact of the Transaction Tax Reform

This figure decompose the financial impact of the tax reform on portfolio returns through day trading into three components following Equation 8:

$$E_g\left[\Delta PR\right] \approx E_g\left[\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times E_g[\Delta\text{Tax}] + E_g\left[\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times E_g[\Delta\text{Gross return}] + E_g\left[\frac{\text{Net return}_{\text{post}}}{\text{Portfolio Size}_{\text{pre}}}\right] \times E_g[\Delta\text{Volume}]$$

The three components are: (1) Mechanical tax cut benefit (blue bars), where $E_g\left[\frac{\text{Day-Trade Volume}_{\text{pre}}}{\text{Portfolio Size}_{\text{pre}}}\right]$ is each investor's monthly day trading volume pre-reform divided by portfolio size, averaged across investors within group g and $E_g[\Delta\text{Tax}]$ is the mechanical increase in net returns per dollar day-traded due to the tax cut (i.e. 15bps); (2) Performance-induced impact (green bars), where $E_g[\Delta\text{Gross return}]$ is the estimated causal effect of the tax reform on gross returns per dollar day-traded within group g; and (3) Volume-induced impact (red bars), where $E_g\left[\frac{\text{Net return}_{\text{post}}}{\text{Portfolio Size}_{\text{pre}}}\right]$ is the ratio of post-reform per-dollar net returns to the portfolio size, averaged within group g, and $E_g[\Delta\text{Day-Trade Volume}]$ is the estimated causal effect of the tax reform on monthly day trading volume in dollar terms within group g. Net impact (black bars) sum all three components. Each component is calculated separately for investors grouped into portfolio size terciles based on their average portfolio holdings in the month prior to the reform. All values represent annualized changes in portfolio returns in percentage points.

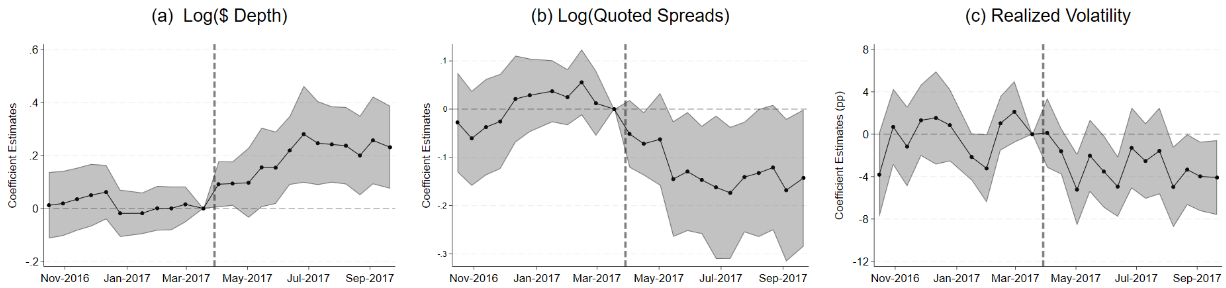


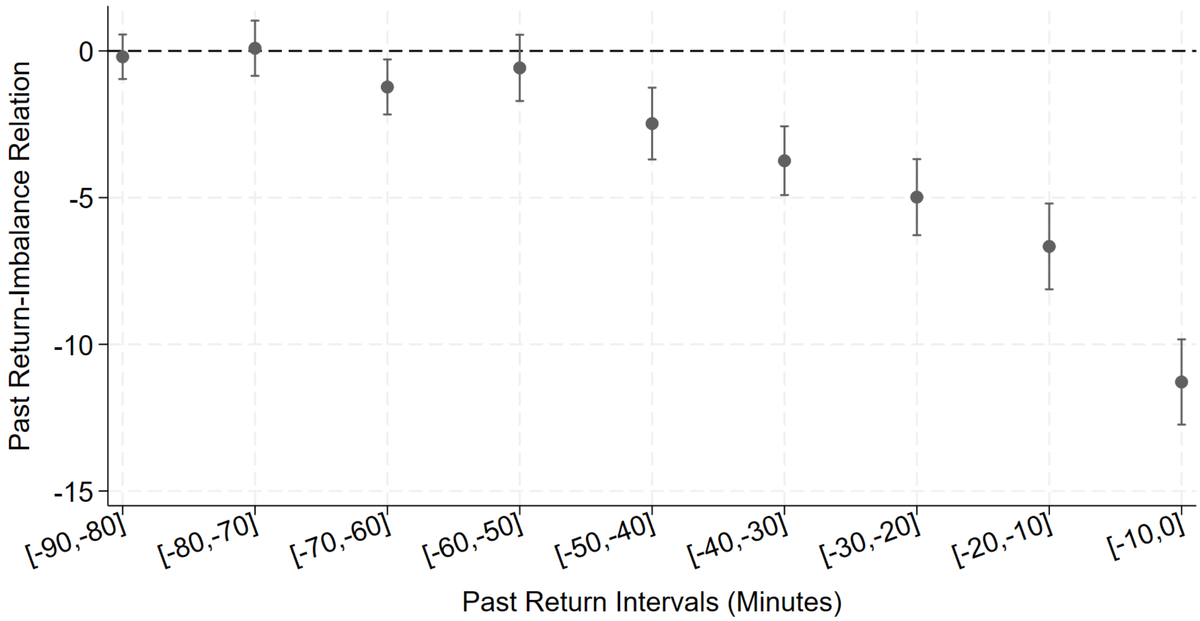
Figure 7: Dynamics of Market Quatilty (Treatment vs. Control)

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 biweekly intervals. The outcome variable y_{st} is the daily market quality measures. 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 8: Contrarain Behavior of Aggregate Day Traders



This figure plots coefficient estimates from estimating Equation 12:

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

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.

Tables

	Volume		
	(1) All vs. All Trades	(2) Day vs. All Trades	(3) Non-day vs. All Trades
Treat $\times$ Post	0.05**	0.26***	-0.02
	(0.02)	(0.03)	(0.02)
Observation	425,256	425,064	422,352
Pseudo R^2	0.82	0.80	0.79
Investor FE	✓	✓	✓
Day FE	✓	✓	✓
Cluster	Investor	Investor	Investor

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. 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. 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. Standard errors are clustered at the investor level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Gross Returns per $ Traded (bps)		
	(1)	(2)	(3)
	All	Day	Non-day
	vs. All Trades	vs. All Trades	vs. All Trades
Treat $\times$ Post	-2.47*	-5.32**	0.33
	(1.39)	(2.21)	(0.78)
Observation	1,642,414	633,955	1,513,473
Adj. R^2	0.04	0.18	0.02
Investor FE	✓	✓	✓
Day FE	✓	✓	✓
Cluster	Investor, Day	Investor, Day	Investor, Day

Table 2: Effect of the Transaction Tax Reform on Performance

This table reports the results from estimating Equation 7:

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

where y_{it} is gross return per dollar traded for investor i 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 may 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Share of Round-Number Orders (pp)
	(1)
Treat × Post	2.07***
	(0.33)
Observation	620,836
Adj. R^2	0.33
Investor FE	✓
Day FE	✓
Cluster	Investor

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 day trades; 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, respectively. Standard errors are clustered at the investor level and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Log(Order Idle Time)	Prob. of Modification (pp)
	(1)	(2)
Treat × Post	0.04***	-0.77***
	(0.01)	(0.24)
Observation	620,836	620,836
Adj. R^2	0.41	0.60
Investor FE	✓	✓
Day FE	✓	✓
Cluster	Investor	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 cancelled before execution. For the treatment group, these measures are calculated using only limit orders from 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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 (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	0.08	0.24	189	0.06	0.11
Volume (mn USD)	1330	0.36	1.30	189	0.18	0.42
Price (USD)	1330	0.88	0.97	189	0.93	1.24
Market Beta	1330	0.71	0.27	189	0.75	0.31
Depth (USD)	1330	11.50	30.48	189	9.64	13.47
Quoted Spread (bps)	1330	201.67	147.46	189	192.83	135.36
Realized Volatility (pp)	1330	37.84	10.08	189	37.48	10.01
Institutional Ownership (%)	1330	5.87	10.94	189	5.86	10.59
Day Trading Share (%)	1330	7.32	4.73	0	.	.
Total Asset (bn USD)	1330	0.14	0.82	189	0.15	0.72
Net Income (mn USD)	1330	3.81	21.06	189	0.79	33.39
ROA (%)	1330	5.46	6.53	189	2.42	9.45

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.

	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	0.14**	0.15**	-0.13**	-0.12**	-2.67**	-2.72**
	(0.06)	(0.06)	(0.06)	(0.05)	(1.29)	(1.29)
Observation	364,703	128,037	363,911	127,625	364,703	128,037
Adj. R^2	0.70	0.67	0.75	0.75	0.13	0.13
Stock FE	✓	✓	✓	✓	✓	✓
Day FE	✓	✓	✓	✓	✓	✓
Cluster	Stock	Stock	Stock	Stock	Stock	Stock

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

This table reports the results from estimating Equation 10:

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

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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A Sample coverage analysis

Table A.1 compares trade characteristics between our sample and that of Barber et al. (2014) and Barber et al. (2020). Although their data cover a different period (1995–2006 vs. our 2012–2017) and include 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 233,258 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).

	BLLO's sample (1995-2006)	Our sample (2012-2017)
Average investor:		
Trade size (NTD)	190,656	228,102
Average day trader:		
# Trading day	42.9	70.2
# Day trading day	12.9	15.1

Table A.1: Comparision 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 1995-2006 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.

Panel (a) of Figure A.1 shows that the cross-sectional correlation between daily sample volume and aggregate retail volume is consistently around 75% 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 day trading share (defined as the ratio of day-trade volume to total volume) measured in our sample closely tracks that of the aggregate market. Importantly, both series witness a substantial increase in day trading activity after the tax reform.

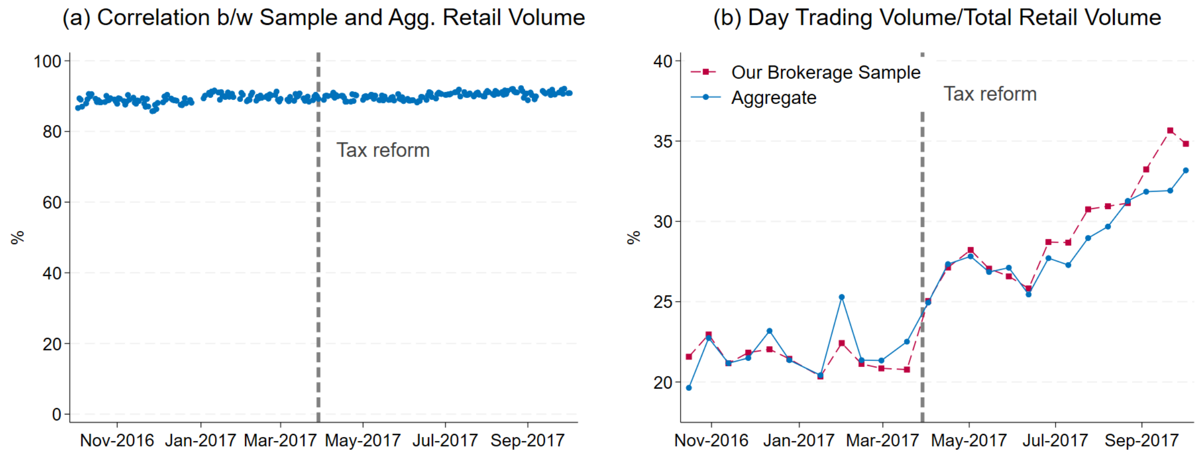


Figure A.1: Comparision of Sample and Aggregate Volume

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 day trading volume to total trading volume, for both our sample (blue line) and the aggregate market (red line). 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 for the 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 are equivalent to Equation 4.

The first 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:

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

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. Supporting this assumption, Figure C.2 shows that the ratio of day trading volume to total trading volume for the treatment group is stable in the six months prior to the reform, increasing sharply only after the tax cut.

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:

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

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 **??** 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 divergence occurring only after the reform.

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

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

Together, these two conditions imply that the counterfactual percentage change in day trading volume for the treatment group can be recovered from the observed percentage change in total trading volume for the control group.

C Additional Figures and Tables

C.1 Figures

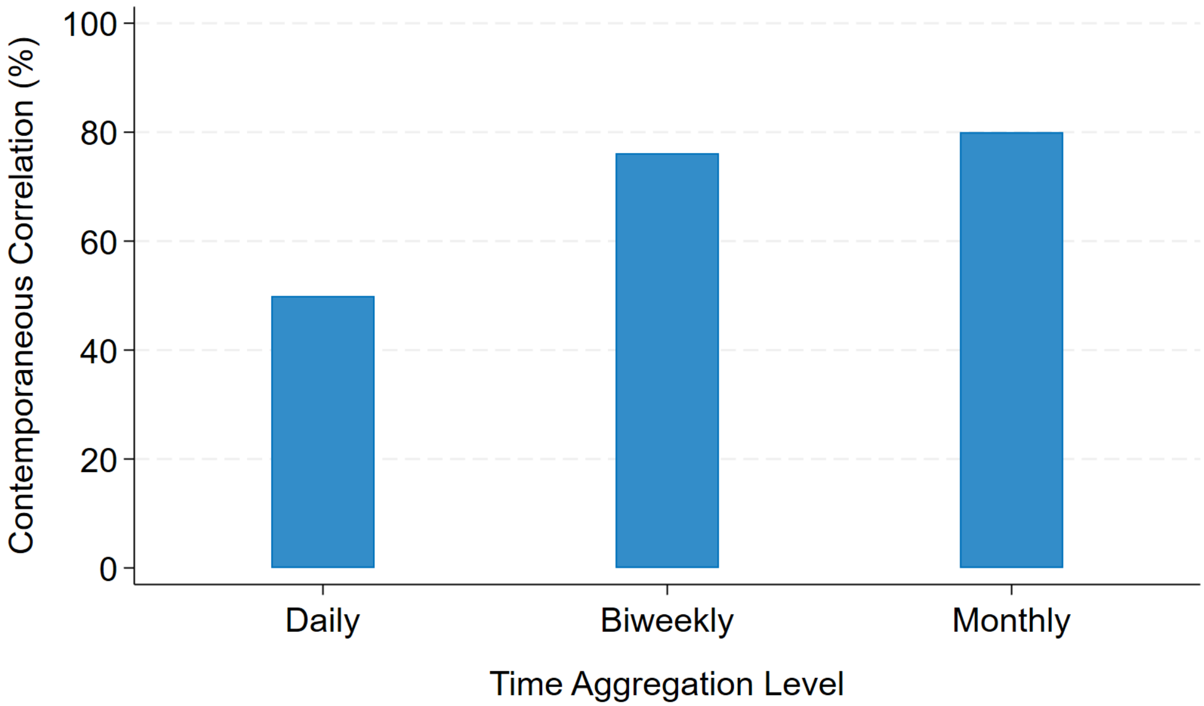


Figure C.1: Contemporaneous Correlation in Trading Volume between Treatment and Control

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.

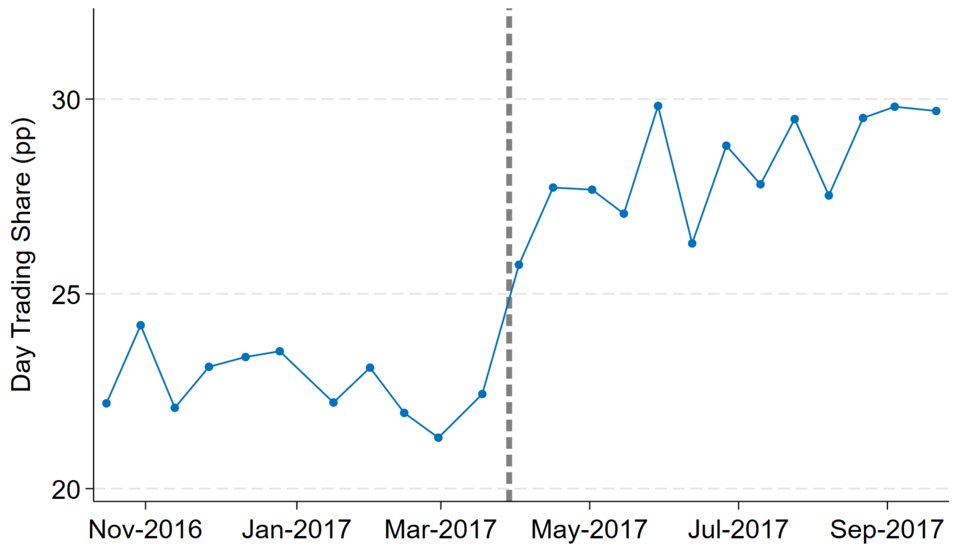


Figure C.2: Ratio of Day Trading Volume to Total Trading Volume Among Treatment Group

This figure plots the biweekly ratio of day trading volume to total trading volume for the treatment group from November 2016 to October 2017. The ratio is calculated as the aggregate day trading volume divided by aggregate total trading volume among investors in the treatment group for each biweekly period, expressed in percentage points. The vertical dashed line indicates the implementation of the transaction tax reform on April 28, 2017.

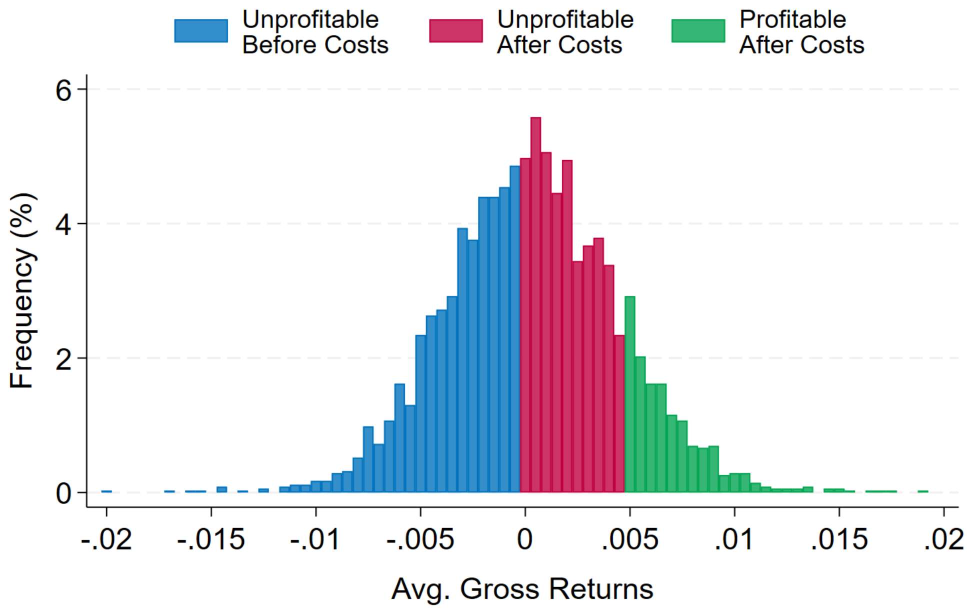


Figure C.3: The Distribution of Past Day Trading Performance

This figure plots the frequency distribution of gross returns from day trading for those with more than 30 days of day trading activity during the classification period (the year prior to the analysis sample period). The histogram shows the frequency distribution, with bars colored according to three performance categories: unprofitable before costs (blue bars, negative gross returns), unprofitable after costs (red bars, positive gross returns but negative net returns after transaction costs), and profitable after costs (green bars, positive net returns after transaction costs). Gross returns are computed using actual transaction prices and displayed on the horizontal axis. Total round-trip transaction costs are 48.5 basis points.

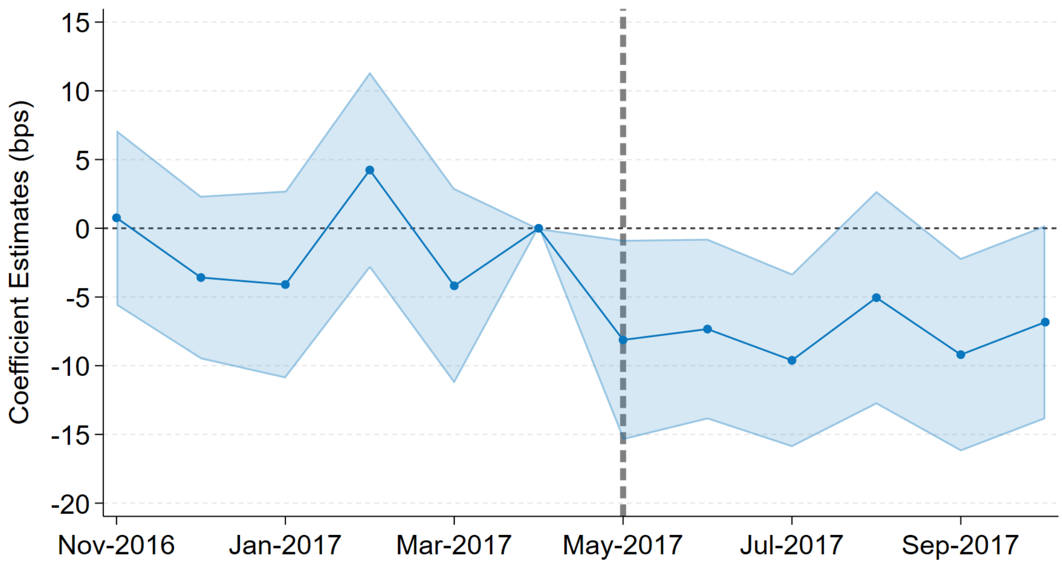


Figure C.4: Effect of the Transaction Tax Reform on Gross Day Trading Performance

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

$$y_{it} = \sum_{k \neq -1} \beta_k \text{ Treat}_i \times \mathbf{1}[t \in k] + \gamma_i + \delta_t + \varepsilon_{it}$$

where i indexes investors and t indexes days. Treat$_i$ is an indicator equal to 1 if investor i belongs to the treatment group. $\mathbf{1}[t \in k]$ is an indicator for months relative to the tax reform, with $k = -1$ indicating the month before the tax reform (i.e. April), omitted as the baseline. β_k captures the month-k treatment effect. γ_i and δ_t denote investor and month fixed effects, respectively. The outcome variable y_{it} is the daily gross returns per dollar traded (in basis points) for investor i. For the treatment group, the outcome includes only 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 may remain open at day's end, we focus on the first day of trades and approximate gross returns by comparing the execution price of the initial trade to that day's closing price. Standard errors are double-clustered at the investor and month levels. Shaded areas represent 95% confidence intervals.

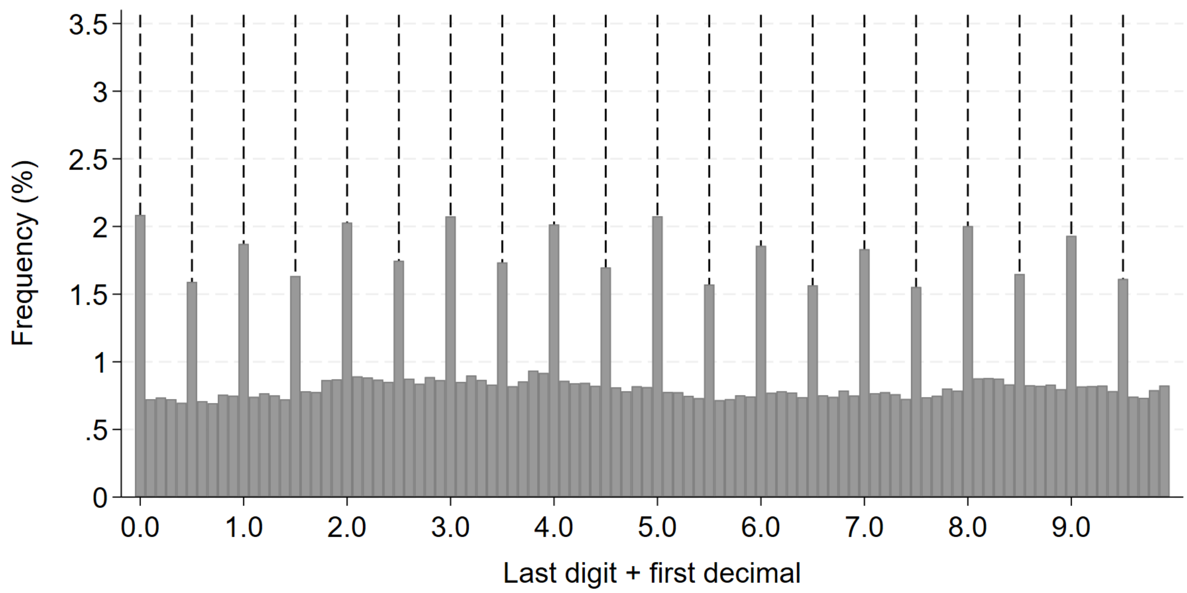


Figure C.5: Limit Order Clustering

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.

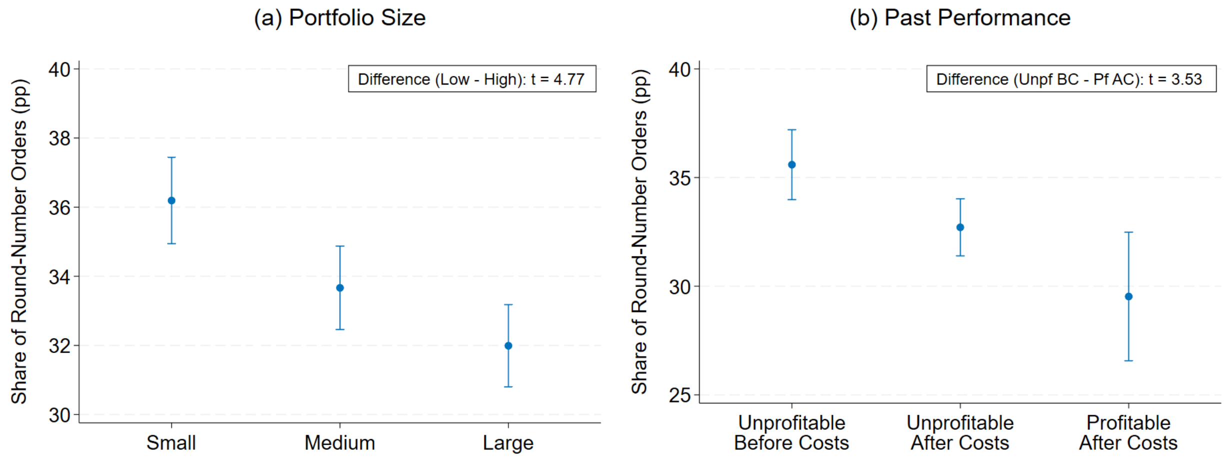


Figure C.6: Share of Round-Number Orders by Trader Sophistication

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 the average portfolio size in the month prior to the reform. Panel (b) groups investors into three categories based on gross returns from day trading during the classification period (the year prior to the analysis sample period): (1) traders with negative gross returns (unprofitable before costs), (2) traders with positive gross but negative net returns assuming a 48.5 basis point round-trip cost (unprofitable after costs), and (3) traders with positive net returns (profitable after costs). 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.

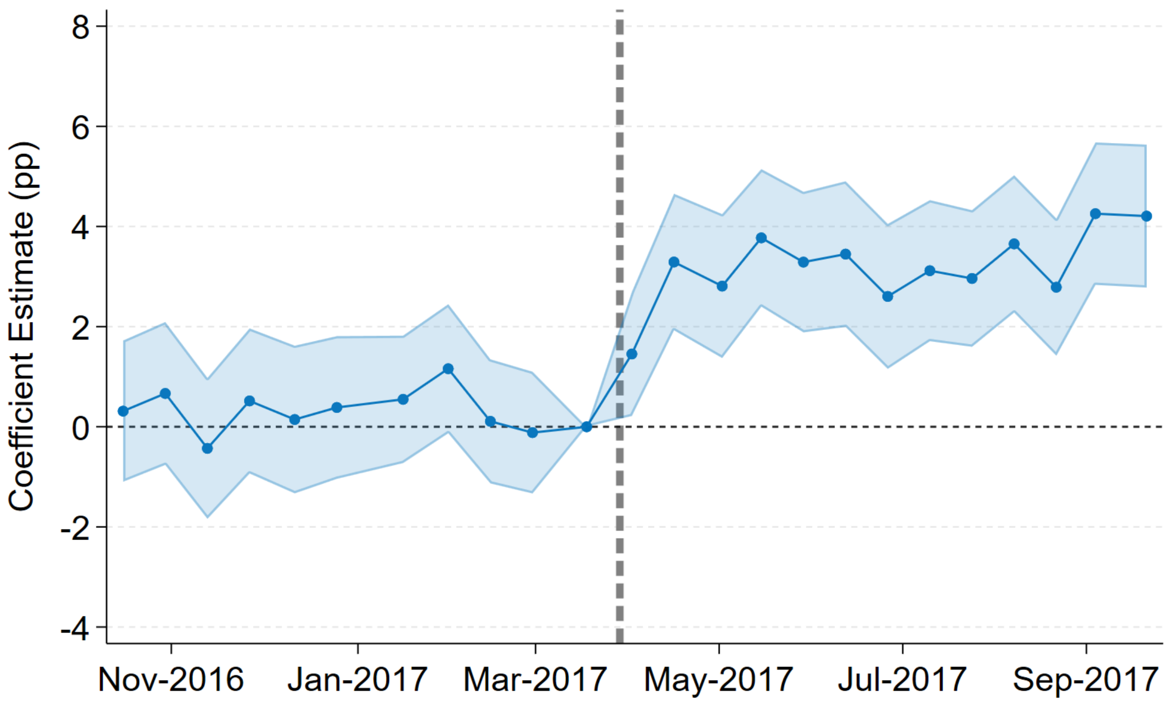


Figure C.7: Dynamics of the Relative Use of Cognitive Shortcuts

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

$$y_{it} = \sum_{k \neq -1} \beta_k \text{Treat}_i \times \mathbf{1}[t \in k] + \gamma_i + \delta_t + \varepsilon_{it}$$

where i indexes investors and t indexes days. The outcome variable y_{it} is the share of round-number limit orders (in percentage points) submitted by investor i in on day t. For the treatment group, the outcome is calculated using only day trades; 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. $\mathbf{1}[t \in k]$ 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_i and δ_t denote investor and time fixed effects, respectively. Shaded areas represent 95% confidence intervals.

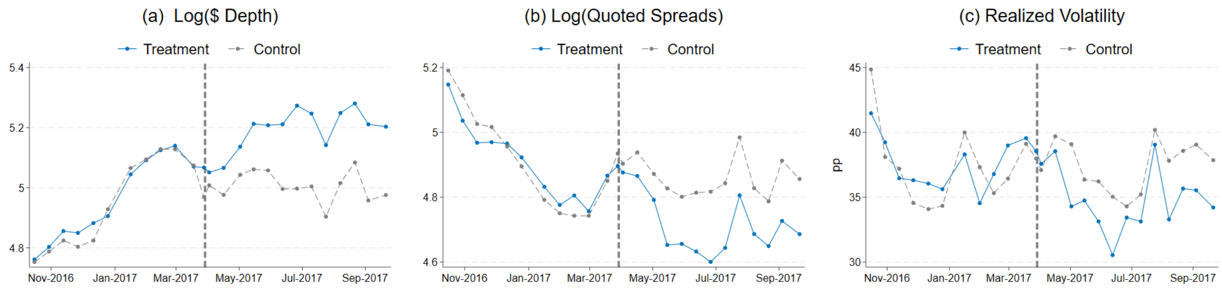


Figure C.8: Time Series of Market Quality

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.

C.2 Tables

Panel A. Full Brokerage Sample

	N(investor)	Mean	SD	p25	p50	p75
Account Tenure (Years)	120,257	11.23	6.80	6.00	10.00	17.00
Age (Years)	120,257	48.40	13.09	39.00	47.00	58.00
Is male (%)	120,257	50.21	50.00	0.00	100.00	100.00
Portfolio Size (USD)	120,257	68,795.53	377,236.73	4,502.78	16,727.86	51,006.71
Monthly # any trades	120,257	3.99	11.61	0.33	1.00	3.00
Trade size (USD)	120,257	7,160.12	24,825.78	1,478.72	2,993.75	6,396.67
Monthly volume (USD)	120,257	34,860.71	290,394.54	857.00	3,276.67	14,052.78
Monthly turnover (%)	120,257	27.00	41.21	3.24	9.34	28.68

Panel B. Main Sample

	Treatment						Control					
	N(investor)	Mean	SD	p25	p50	p75	N(investor)	Mean	SD	p25	p50	p75
Account Tenure (Years)	14,049	11.43	6.48	6.00	11.00	17.00	3,652	12.48	6.51	7.00	13.00	17.00
Age (Years)	14,049	51.03	12.41	42.00	51.00	60.00	3,652	51.46	12.41	42.00	51.00	60.00
Is male (%)	14,049	57.14	49.49	0.00	100.00	100.00	3,652	58.08	49.35	0.00	100.00	100.00
Portfolio Size (USD)	14,049	134,343.23	290,180.77	13,454.56	41,438.75	118,734.80	3,652	147,637.00	249,960.09	25,649.91	65,301.43	152,104.23
Monthly volume (USD)	14,049	162,755.75	337,148.28	19,072.56	52,067.22	147,088.28	3,652	46,037.84	76,617.73	8,306.47	20,020.69	48,590.39
Monthly # any trades	14,049	18.51	21.97	5.17	11.17	22.67	3,652	8.00	6.75	3.33	6.17	10.50
Trade size (USD)	14,049	8,393.64	13,667.13	2,007.48	3,819.35	8,264.66	3,652	6,126.78	8,604.83	1,704.48	3,148.29	6,505.65
Monthly turnover (%)	14,049	137.78	180.70	23.79	60.94	163.10	3,652	40.74	75.50	6.58	16.79	41.57
Monthly day-trade volume (USD)	14,049	25,844.99	74,472.74	841.39	3,221.22	13,839.89	3,652	417.10	1,471.78	0.00	0.00	0.00
Monthly # day trades	14,049	3.45	7.39	0.33	0.83	2.67	3,652	0.08	0.20	0.00	0.00	0.00
Day-trade size (USD)	14,049	7,421.53	11,952.47	1,724.26	3,413.61	7,541.19	841	5,839.70	9,310.03	1,445.58	2,820.00	6,146.67
Day-trade return (bps)	14,049	32.40	130.40	-30.40	19.93	91.34	841	60.79	190.75	-22.95	32.89	140.43

Table C.1: Summary Statistics of Brokerage Data

This table reports summary statistics for the brokerage sample. Panel A presents statistics for the full sample of 120,257 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 more than 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. Holdings is the average portfolio value in USD. Monthly volume is the average monthly trading volume in USD. 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. Portfolio Size is the average holdings size of day trades 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).

	Realization Rate (pp)
	(1)
Post	1.50***
	(0.14)
Constant	76.77***
	(0.07)
Observations	404,704
Adj. R^2	0.36
Investor FE	✓
Cluster	Investor

Table C.2: 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Volume			
	(1) Control > 10 days	(2) Control > 20 days	(3) Control > 30 days	(4) Control > 40 days
Treat $\times$ Post	0.25***	0.26***	0.26***	0.29***
	(0.02)	(0.02)	(0.03)	(0.04)
Observation	791,376	518,688	425,064	381,720
Pseudo R^2	0.76	0.79	0.80	0.81
Investor FE	✓	✓	✓	✓
Day FE	✓	✓	✓	✓
Cluster	Investor	Investor	Investor	Investor

Table C.3: 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. 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. 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. 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 more than 10 trading days, Column (2) more than 20 days, Column (3) more than 30 days, and Column (4) more than 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Day vs All Trades	
	(1)	(2)
	Volume	Gross Returns (bps)
Treat × Post	0.34***	-5.85***
	(0.01)	(2.14)
Observations	666,792	759,490
Pseudo R^2	0.77	
Adj. R^2		0.18
Investor FE	✓	✓
Time FE	✓	✓
Cluster	Investor	Investor, Day

Table C.4: Effect of the Transaction Tax Reform with Alternative Classification Period

This table reports the results from estimating Equations 5 and 7 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:

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

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. The control group consists of active non-day traders who traded on more than 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Intended Day vs All Trades	
	(1) Volume	(2) Gross Returns (bps)
Treat × Post	0.25***	-4.94**
	(0.02)	(2.45)
Observations	425,064	633,955
Pseudo R^2	0.78	
Adj. R^2		0.14
Investor FE	✓	✓
Day FE	✓	✓
Cluster	Investor	Investor, Day

Table C.5: Effect of the Transaction Tax Reform with Intended Day Trades

This table reports the results from estimating Equations 5 and 7 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:

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

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. For Column (1), the dependent variable is intended 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 on day t by investor i, calculated from intended day trades for the treatment group and from all trades for the control group. 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Gross Returns (bps)			
	(1) Control $>$ 10 days	(2) Control $>$ 20 days	(3) Control $>$ 30 days	(4) Control $>$ 40 days
Treat $\times$ Post	-4.51**	-5.51***	-5.31**	-5.37**
	(1.91)	(2.06)	(2.22)	(2.38)
Observation	1,075,497	788,729	641,468	550,880
Adj. R^2	0.14	0.16	0.18	0.19
Investor FE	✓	✓	✓	✓
Day FE	✓	✓	✓	✓
Cluster	Investor	Investor	Investor	Investor

Table C.6: Performance Responses with Alternative Control cutoffs

This table reports the results from estimating Equation 7:

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

where y_{it} is gross return per dollar traded for investor i 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 may 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 more than 10 trading days, Column (2) more than 20 days, Column (3) more than 30 days, and Column (4) more than 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	Excess Returns	
	(1)	(2)
	Day Trades	All Trades
Alpha	6.11***	-20.56***
	(0.95)	(1.88)
Post	-4.75**	-1.56
	(1.94)	(2.06)
Market Ex. Returns	0.16***	0.03
	(0.02)	(0.03)
Market Ex. Returns $\times$ Post	-0.00	-0.07
	(0.04)	(0.04)
Observation	247	247
Adj. R^2	0.27	0.00
Cluster	Month	Month

Table C.7: Changes in Abnormal Returns

This table reports the results from estimating:

$$\text{Excess Returns}_t = \alpha + \gamma \text{ Post}_t + \beta_1 \text{ Market Ex.Returns}_t + \beta_2 \text{ Market Ex.Returns}_t \times \text{Post} + \varepsilon_t$$

where Excess Returns$_t$ represents excess returns from day trades (average daily returns minus the risk-free rate) of the treatment group on day t in column (1) and excess returns from all trades of the control group in column (2). Post$_t$ is an indicator equal to 1 for days after April 28, 2017. Market Ex.Returns$_t$ represents excess value-weighted intraday market returns (i.e. value-weighted intraday market returns minus the risk-free rate). Standard errors are clustered at the month 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) 10-Day Holding Period Returns	(2) 30-Day Holding Period Returns
Treat × Post	-3.80**	-3.71**
	(1.73)	(1.70)
Observation	633,955	633,955
Adj. R^2	0.20	0.21
Investor FE	✓	✓
Day FE	✓	✓
Cluster	Investor, Day	Investor, Day

Table C.8: Holding Period Returns as Counterfactuals

This table reports the results from estimating Equation 7:

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

where y_{it} is gross return per dollar traded for investor i 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 (bps) (Day Trades)
	(1)
Cognitive shortcuts	-6.50***
	(1.14)
Observation	702,733
Adj. R^2	0.16
Investor FE	✓
Stock FE	✓
Day FE	✓
Cluster	Investor, Day

Table C.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)
Extreme Overnight Returns [0, 2]	-1.96**
	(0.92)
Observation	705,449
Adj. R^2	0.17
Investor FE	✓
Stock FE	✓
Day FE	✓
Cluster	Investor, Day

Table C.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 per \$ Traded (bps)		
	(1) Small Portfolio Size	(2) Medium Portfolio Size	(3) Large Portfolio Size
Treat × Post	-6.04***	-3.56*	-2.33
	(2.08)	(1.87)	(1.61)
Observation	187,830	218,440	227,685
Adj. R^2	0.15	0.19	0.21
Investor FE	✓	✓	✓
Day FE	✓	✓	✓
Cluster	Investor, Day	Investor, Day	Investor, Day

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

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

$$y_{it} = \beta_g \text{Treat}_i \times \text{Post}_t + \gamma_i + \delta_t + \varepsilon_{it} \quad \text{where Portfolio Size Tercile}_i = g$$

where y_{it} is gross return per dollar traded for investor i on day t. The outcome is restricted to day trades for the treatment group and includes 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 may remain 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. Portfolio size terciles are based on average portfolio holdings in the month prior to the reform. γ_i and δ_t denote investor and day fixed effects. Each column presents results for a different size tercile $g \in$ {Bottom, Middle, Top}. Standard errors are double-clustered at the investor and day levels and reported in parentheses. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Portfolio Size	(1) Day-trade Volume (pre) ($)	(2) Net Return (post) (bps)	(3) $\frac{\text{Day-Trade Volume}_{pre}}{\text{Portfolio Size}_{pre}}$	(4) $\frac{\text{Net return}_{post}}{\text{Portfolio Size}_{pre}}$ (bps/$)	(5) Population Share (%)	(6) Volume Response (%)	(7) Performance Response (bps)	(8) Mechanical Tax Cut Benefit (% p.a.)	(9) Net Impact (% p.a.)	(10) Avg. Mechanical Benefit (% p.a.)	(11) Avg. Net Impact (% p.a.)	(12) PR_{pre} (% p.a.)
Small	24,694	-21.68	0.83	-0.0029	33.4	41.9	-6.04	1.49	-2.71	1.23	-0.83	-6.34
Medium	28,813	-7.02	0.68	-0.0011	33.3	29.7	-3.56	1.22	-0.20			-4.74
Large	31,750	-1.22	0.55	-0.0005	33.3	23.4	-2.33	0.98	0.43			-3.26

Table C.12: The Financial Impact of the Transaction Tax Reform by Portfolio Value

This table presents the decomposition of the financial impact of the transaction tax reform following Equation 8. Each row represents a portfolio size tercile according to the average portfolio holdings in the month prior to the reform. Column (1) reports average pre-reform day trading volume in USD. Column (2) reports average post-reform net return per dollar day-traded in basis points. Column (3) reports the average of individual investors' ratios of pre-reform monthly day trading volume to pre-reform portfolio size. Column (4) reports the average of individual investors' ratios of post-reform net return to pre-reform portfolio size. Column (5) reports the population share of each tercile. Column (6) reports the estimated causal effect of the tax reform on day trading volume in percentage terms. Column (7) reports the estimated causal effect of the tax reform on per-dollar gross returns in basis points. Column (8) reports the annualized mechanical tax cut benefit calculated as Column (3) × 15 basis points × 12. Column (9) reports the annualized net financial impact calculated as: Column (8) + [Column (3) × Column (7) / 100 × 12] + [Column (1) × Column (4) × Column (6) / 10000 × 12], representing the sum of the mechanical tax cut benefit, performance-induced impact, and volume-induced impact. Column (10) reports the mechanical tax cut benefits for the average day trader, derived from the population-weighted average of Column (8) across terciles. Column (11) reports the net financial impact of the tax reform on the average day trader, derived from the population-weighted average of Column (9) across terciles. Column (12) reports the annualized average portfolio return from day trading for each group g during the pre-reform period. All values in Columns (8) through (12) are annualized and are in percentage points.

	Within Treatment Group		Within Control Group	
	(1) Log($ Depth)	(2) Realized Volatility	(3) Log($ Depth)	(4) Realized Volatility
High ROA × Post	-0.06**	-0.41	-0.08	-2.23
	(0.03)	(0.44)	(0.07)	(1.43)
Observation	323,677	323,677	41,026	41,026
Adj. R^2	0.85	0.22	0.68	0.13
Stock FE	✓	✓	✓	✓
Day FE	✓	✓	✓	✓
Cluster	Stock	Stock	Stock	Stock

Table C.13: 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, and Realized Volatility, the annualized standard deviation of 10-minute intraday returns in percentage points. Columns (1) and (2) present results for stocks within the treatment group (eligible for day trading), while Columns (3) and (4) 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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 C.14: 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.

	Our Study	Barber & Odean (2000, 2001)	Dorn et al. (2005)
Data Source	Taiwan Broker	US Discount Broker	German Broker
Period	2012–2017	1991–1996	1995–2000
Sample Size	120,257 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)	7,160	12,350	—
Monthly Volume (USD)	34,860	—	—
Monthly Turnover (%)	27.0	12.7	16.1
Portfolio Size (USD)	68,795	47,334	68,360

Table C.15: 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 120,257 traders. Barber & Odean (2000, 2001) uses data from a US discount broker covering 1991-1996 with 66,465 households. Dorn et al. (2005) uses 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. 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.