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Taxing the Disposition Effect: The Impact of Tax Awareness on Investor Behavior

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Abstract

Standard portfolio choice models predict that investors consider the tax implications of trading. However, individuals are disposed toward realizing gains and holding losing investments, behaviors that worsen their performance. We show, in an experimental market, that increasing tax salience reduces the disposition effect between 22% and 47%, leading to higher portfolio balances without increasing total trading activity. Using field data, we find that investors' disposition is sensitive to taxes around tax rate changes, when taxes are likely salient. Our analysis demonstrates that increasing tax awareness can affect households' portfolio choices, which suggests policy implications for improving financial decision-making.

Keywords: Disposition effect, individual investors, tax salience, behavioral finance.

JEL classification: D14, D91, G11, G41.

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I. Introduction

Standard theories of financial decision-making predict that individuals should consider the tax implications of their investment portfolio choices (e.g., Grossman and Stiglitz (1980), Vissing-Jorgensen (2002)). For instance, Constantinides (1983) suggests that investors' optimal liquidation policy in the presence of taxes may be to realize losses immediately and defer gains, potentially until their death. However, mounting evidence suggests that individual investors' trading decisions contradict the predictions of canonical models. Limits to attention and cognitive constraints may lead investors to overlook elements which should affect their portfolio choices, resulting in systematic mistakes (e.g., Simon (1955)). We evaluate whether making the tax implications of trading salient can improve investors' decision-making by moderating behavioral biases, particularly the disposition effect.

The disposition effect is one of the most robust behavioral biases, affecting investors and asset prices over a wide range of markets (Odean (1998), Genesove and Mayer (2001), Kaustia (2010), Cici (2012)). The disposition effect describes the tendency of individuals to hold assets that have decreased in value and sell assets that have appreciated in value (e.g., Shefrin and Statman (1985)), behaviors that ultimately lead to lower performance and wealth (Shumway and Wu (2005)). Growing evidence indicates that investors' disposition impedes the incorporation of news into prices (e.g, Frazzini (2006), Birru (2015)) and affects stock returns, volatility, and trading volume (Goetzmann and Massa (2008)), all of which have implications for the aggregate market. While the literature has put forth several potential mechanisms that might generate the disposition effect, its determinants are yet to be fully described (e.g., Barberis and Xiong (2009), Barberis and Xiong (2012), Ingersoll and Jin (2013), Meng and Weng (2017)).

We conjecture that raising individuals' awareness of the tax implications of trading may reduce the magnitude of the disposition effect. Our hypothesis is motivated by several observations. First, Odean (1998) notes time variation in the disposition effect. Specifically,

it declines in December, when investors could be potentially cognizant of capital gains taxes. Second, recent evidence suggests that salience, particularly tax salience (e.g., Chetty, Looney, and Kroft (2009), Goldin (2015), Taubinsky and Rees-Jones (2017)), can affect households' consumption and financial decisions as well as asset prices (Bordalo, Gennaioli, and Shleifer (2012), Bordalo, Gennaioli, and Shleifer (2013), Birru, Chague, De-Losso, and Giovannetti (2019)). In addition, prevailing tax policies confer economic incentives that should affect portfolio decisions. Capital gains taxes provide incentive for individuals to hold winning stocks, which defers tax payments, and to realize losses that offset taxable income. Ultimately, raising tax awareness could serve as a novel policy tool to improve households' financial decision-making.

We use multiple identification approaches to provide evidence that increasing individuals' tax awareness influences their investment choices. To isolate whether increasing tax salience can moderate the disposition effect, we first conduct an online trading experiment with a large heterogeneous group of participants. We also use data on retail investors' portfolios and trades from a nationwide brokerage and exploit tax regime changes as shocks to some investors' tax awareness. The experimental and empirical approaches are complementary. The experimental setting offers causal insights through a randomized controlled trial framework and the field data provide empirical evidence from the marketplace. The consistency in the observed effects of tax awareness on individuals' investment decisions increases the confidence in the generalizability of the findings.

Our experiment provides causal evidence that increasing tax awareness can affect trading behavior and moderate the disposition effect. Building on Frydman and Rangel (2014), we allow our participants to make a series of portfolio choices. Participants are initially endowed with $350 of experimental currency, and they invest the funds into three risky assets and retain a cash balance, which is risk-free. The investors are informed of the price processes of the risky assets and, in each period, may choose to trade one of the randomly selected stocks. All participants face the same tax implications when trading stocks

and are informed of the presence of taxes during the initial instructions. A tax liability arises when a stock appreciates in price whereas a write-off accrues on losing stocks in the portfolio. If the investor sells a stock holding, his/her cash balance in the following period includes the tax implications of the stock sale.

We test whether tax salience influences individuals' trading behavior by randomly assigning each participant into one of two conditions. Individuals in the High-salience (treatment) group observe the unrealized tax consequences of a potential stock sale when making a trading decision. Conversely, participants in the Low-salience (control) group do not view the tax implications. The information set is the same for all participants, and the only difference between the two conditions is the presence (i.e., salience) of the tax consequences cue.

As expected, investors display trading behavior consistent with the disposition effect. Evidence indicates that raising individuals' awareness of the tax consequences of trading affects their behavior. In particular, increasing tax salience reduces individuals' disposition effect by 22% to 47%, relative to that of participants in the control group. Tax awareness affects both dimensions of the disposition effect – it decreases one's willingness to realize gains and increases one's propensity to realize losses. Consequently, individuals in the High-salience condition have better trading performance; they earn seven basis points more per trading period. Increasing tax salience does not significantly increase total trading activity, suggesting that individuals are not likely to incur greater trading costs. The effects of higher tax awareness cannot be explained by heterogeneity in socioeconomic characteristics known to influence investors' portfolio choices, such as age, education, gender, income, race, risk preferences, financial literacy, investment experience, trust in financial markets, political views, and optimism.

We address concerns about the experimental results' external validity by analyzing field data on United States (U.S.) retail investors' trading decisions and portfolio holdings from 1991 through 1996 (e.g., Odean (1998)). To identify how tax awareness affects individ-

uals' trading behavior in a natural setting, we use two empirical approaches. We estimate panel regressions to examine whether heterogeneity in state-level taxes can explain investors' decisions. We find no significant evidence that predominantly time-invariant tax rates influence individuals' trading behavior, particularly the disposition effect. To assess whether taxes affect portfolio choices when the consequences are more likely to be salient, we exploit small tax schedule changes in New York, Michigan, and Delaware. While tax rates among most states are static during our sample period, these three states reduce capital gains tax rates in 1995. For example, in 1994, unmarried New York investors in the median income tax bracket face a 7.0% tax on capital gains. In 1995, the same investors pay a rate below 6.6%.

The rate changes may serve as plausibly exogenous shocks to tax awareness for investors in the treated states. That is, the reductions could remind individuals that stock trading has tax consequences. However, the schedule changes could also influence trading through a competing channel. The reductions may spur investors to realize gains as the tax burden of doing so is reduced. Importantly, these two channels imply opposite predictions for changes in individuals' trading behavior. The awareness channel suggests a decrease in the disposition effect while the economic incentive channel portends an increase. Alternatively, the rate reductions may not change trading behavior if residents are not aware of the changes or if the economic benefits do not exceed the trading costs. However, we expect residents to be aware of the tax rate reductions since they receive mention in mainstream media and news outlets, including the *Times Union*, *New York Times*, *New York Beacon*, *The Grand Rapids Press*, and *Detroit News*.

We test our prediction by estimating a matched difference-in-differences model (Heckman, Ichimura, and Todd (1997), Heckman, Ichimura, Smith, and Todd (1998)). The matching procedure allows us to assess the effect of a treatment against an estimate of a counterfactual outcome whereby the individual is not exposed to the treatment (Neyman (1923), Rubin (1974)). We employ a difference-in-differences estimator on the matched sample since

it provides unbiased effect estimates if, in the absence of the treatment, the trend over time would have been the same between the treatment and control groups.

To implement the methodology, we first match individuals in the treatment group (i.e., those subject to the tax reductions) with those in the control group (i.e., those whose rates did not change) based on demographic characteristics. In particular, we match individuals based on their income, age, gender, marital status, and home ownership status. We find no significant differences in observable characteristics between the two groups, and trends in the disposition effect of both investor groups appear similar prior to the rate changes. However, in response to the tax rate reductions, we find that treated individuals exhibit a lower disposition effect relative to their matched counterpart.

We also investigate whether the benefits of tax salience are accompanied by negative side effects. Higher salience could lead individuals to pay undue attention to taxes in their portfolio choices. Investors who refuse to sell winning stocks because of the tax consequences may, as a result, hold more concentrated and potentially riskier portfolios (e.g., Ivkovic, Sialm, and Weisbenner (2008)). However, we find that participants who view the tax cue conclude the experiment with more diversified portfolios that consist of a larger number of unique stock holdings. Another concern is that the adjusted trading behavior of investors could affect government tax revenues by reducing receipts from capital gains taxes.[1] While it is difficult to forecast how policymakers would adjust to fill a potential funding gap, we highlight that revenues from individuals' capital gains historically compose only about 2.35% of the total government revenue in the United States.[2] It is also unlikely that raising tax awareness will extinguish all revenues from the realization of gains.

Our study contributes to the literature examining the determinants of portfolio choice.

[1] An alternative perspective may be to view raising investors' tax awareness as an implicit rate adjustment. However, the net effect of tax rate changes is debated. While reducing capital gains tax rates may reduce revenues, doing so may also decrease economic distortions and potentially raise tax revenue by increasing the realizations of gains (Auerbach and Poterba (1988)).

[2] The estimate is based on data provided by the Organisation for Economic Co-operation and Development (OECD) that spans 1965 through 2016. Federal data are derived from the U.S. Budget, the Final Monthly Treasury Statement, and the Annual Report of the U.S. Treasury Department. State and local data are from information published by the Bureau of the Census.

There is significant heterogeneity in individuals' investment decisions (Campbell (2006), Barber and Odean (2013), Campbell (2016)). Neoclassical models suggest that taxes should influence trading behavior (Constantinides and Scholes (1980), Constantinides (1984)). However, the empirical evidence is mixed. Auerbach, Burman, and Siegel (1998) note that the avoidance of capital gains taxes is not prevalent except among some financially sophisticated investors. Ivković, Poterba, and Weisbenner (2005) compare investors' trading behavior across taxable and non-taxable accounts and identify a capital gains lock-in effect. We show that state-level tax rates, which are likely to affect net portfolio returns and wealth, does not seem to influence investors' trading decisions during quiet tax regime periods. However, we document a link between portfolio choices and tax regime transitions. Specifically, small reductions in state-level tax rates reduce investors' disposition. These results suggest that adjustments to tax policies can shape investors' behavioral responses.

We also contribute to a growing literature in behavioral financial economics that documents evidence of cognitive shortcomings, which lead to investment biases with deleterious impacts on individuals' welfare. These biases include mental accounting (Thaler (1999), Barberis and Huang (2001)), overconfidence (Barber and Odean (2001)), and the disposition effect. While the determinants of behavioral biases have received considerable attention, recent efforts are turning toward reducing them (Dhar and Zhu (2006)). Frydman and Rangel (2014) find that the disposition effect declines by about 25% if the salience of a stock's purchase price is reduced. Fischbacher, Hoffmann, and Schudy (2017) suggest that the commitment to a trading strategy, coupled with automated trading mechanisms, can decrease the disposition effect. However, social interaction can substantially increase investors' disposition, nearly doubling it (Heimer (2016)). Drawing on the emerging tax salience literature (e.g., Goldin (2015), Morrison and Taubinsky (2019)), we show that the disposition effect can be moderated by highlighting the tax implications of trading. Moreover, our findings have potential implications for market efficiency and equilibrium asset prices since the disposition effect slows the impounding of news into prices (e.g., Frazzini (2006), Birru

(2015)) and generates divergence between fundamental values and market prices (Grinblatt and Han (2002)).

Our findings suggest potential policies to improve households' financial outcomes (e.g., Benartzi, Beshears, Milkman, Sunstein, Thaler, Shankar, Tucker-Ray, Congdon, and Galing (2017)). Financial education initiatives can be designed to promote tax awareness. Innovations in financial technology, such as online robo-advising (e.g., D'Acunto, Prabhala, and Rossi (2019)), may offer an additional avenue by which to highlight the impacts of taxes to individuals making investment decisions. For instance, mounting evidence shows that choice architecture (Hirshleifer and Teoh (2003), Choi, Laibson, Madrian, and Metrick (2003), Thaler and Sunstein (2008)) and visual cues (Frydman and Wang (2020), Bazley, Cronqvist, and Mormann (2020)) can influence investors' attention and decisions. Our findings suggest that a visual "just-in-time" intervention (e.g., Fernandes, Lynch Jr, and Netemeyer (2014), Gomes, Haliassos, and Ramadorai (2020)) to raise tax awareness can be effective at influencing individuals' portfolio choices. Ultimately, the visual conveyance of information is a channel that is likely to be increasingly relevant given households' growing reliance on online financial services (e.g., Benartzi (2017)).

II. Hypotheses Development

Constantinides (1983) solves a model of optimal trading with capital gains taxes. The investor's optimal strategy is to realize capital losses immediately and to defer realizing any capital gains until a liquidity need, corporate takeover, or the investor's death forces liquidation. The investor's harvested capital losses can be withheld indefinitely to offset future capital gains taxes. Overall, the optimal trading strategy implies the opposite of the disposition effect, namely, investors should be more likely to realize losses than gains.

The Constantinides (1983) capital gains tax management strategy assumes the investment opportunity set is stationary. After liquidating an asset for a gain or loss, the investor

can reinvest those proceeds in an identical set of risky assets with the same expected returns, variances, and correlations. When the cross-section of risky assets exhibits price momentum (e.g., Jegadeesh and Titman (1993)), the optimal investment strategy tilts even further in favor of realizing losses and deferring gains.

Without any significant negative autocorrelation in stock returns to justify going against the optimal tax strategy, investors may exhibit the disposition effect because of alternative utility functions or inattention. Barberis and Xiong (2009) solve a two-period model to show that investors with prospect theory preferences and realization utility can exhibit a disposition effect. Investors with realization utility only derive utility from realizing gains and losses. Because the prospect theory value function is concave over gains, investors with realization utility prefer to take profits over multiple periods, leading to a disposition effect. Grinblatt and Han (2005) and Frazzini (2006) show that the momentum anomaly can be explained by the actual and withheld trades of investors with prospect theory preferences. Barberis and Xiong (2012) develop an infinite-horizon realization utility model to show that with a large enough time-discount rate, investors only realize losses due to a liquidity shock, leading to a large disposition effect.

When retail investors decide whether to sell any individual stock, they are likely to also consider the performance of their overall portfolio. An, Engelberg, Henriksson, Wang, and Williams (2021) develop a portfolio-driven theory to explain the disposition effect. They show that the magnitude of the disposition effect depends on whether the overall portfolio registers a gain or a loss. Specifically, investors' disposition increases when their portfolio is at a loss and vanishes if the portfolio is at a gain. This variation is consistent with the hedonic mental accounting heuristic (Thaler (1985), Thaler (1999)). Such portfolio-driven behavior is also consistent with a variation of the Barberis and Xiong (2009) realization utility model in which an investor has utility over both paper gains and realized gains. Frydman, Barberis, Camerer, Bossaerts, and Rangel (2014) analyzes neural data to confirm that individuals experience positive utility from both realized gains and paper gains.

Investors could also exhibit the disposition effect because they are consistently inattentive to capital gains tax implications. Patterns in trading activity suggest that investors consider the tax implications of trading toward the end of December and in early January but not during other periods of the year (Lakonishok and Smidt (1986), Ferris, Haugen, and Makhija (1988), Ritter (1988)). Notably, Odean (1998) finds that the disposition effect is absent in December, when investors are most likely to pay attention to the tax implications of their trades. Our experiment prompts investors to understand that their trading outcomes are subject to taxes and that the stocks they buy and sell tend to exhibit momentum. By doing so, we expect salient capital gains tax implications to cause investors to delay realizing gains and increase the realization of financial losses.

Laboratory and field experiments suggest that it is possible to increase or decrease the disposition effect by manipulating the underlying choice architecture that retail investors encounter when trading. Frydman and Rangel (2014) decrease the disposition effect by removing the purchase price, thereby reducing the salience of whether the current market price represents a gain or loss. Frydman and Wang (2020) confirm this effect in the field by showing that investors' disposition increases when an online trading platform starts displaying the purchase price. Fischbacher, Hoffmann, and Schudy (2017) conduct an experiment and show that allowing investors to submit stop-loss orders at the time of purchase reduces the disposition effect. Frydman, Hartzmark, and Solomon (2018) find that the disposition effect is lower when investors immediately reinvest the proceeds of the stock sale, highlighting the importance of capital constraints in limiting suboptimal behavior. Heimer and Imas (2021) find that leverage restrictions reduce the disposition effect by making it more costly for investors to hold on to losing positions. Our experiment manipulates the salience of another important financial friction: capital gains taxes. Capital gains taxes play a similar role to leverage constraints by increasing the cost of not liquidating losing positions.

III. Motivating Evidence: Google Search Volume

Odean (1998) suggests that tax awareness may influence investor trading and mitigate the disposition effect. In a sample of individual investors' trades, the disposition effect disappears in December, when investors are most likely to pay attention to the tax implications of their trades. However, Odean (1998) provides no direct evidence that investors' attention to taxes is driving this December effect.

Insert Figure 1 About Here.

Figure 1 provides initial evidence, using individuals' Google searches, that households pay more attention to investment-related taxes in December. Google Trends provides a monthly time series of Google search volume for any particular search term. The Search Volume Index (SVI) is normalized so that the month with the highest search volume in the sample has an SVI of 100. Da, Engelberg, and Gao (2014) show that Google SVI is a robust measure of individual investor attention and sentiment that predicts market activity. Figure 1 plots the average monthly SVI for the phrase "tax loss selling" from 2004 to 2019 in each calendar month. Average search volume is more than twice as high in December than any other calendar month. While investors should consider the tax implications of their trades throughout the year, the evidence indicates that tax awareness is time varying.

IV. Experimental Methodology and Evidence

To isolate the role of tax awareness in individuals' investment decisions, we conduct an online experiment. Our double-blind, randomized controlled trial paradigm is designed to provide causal evidence that tax awareness can reduce the magnitude of the disposition effect. The key advantage of the experimental approach is that it serves as a controlled environment in which to identify the effects of tax salience by holding asset characteristics, such as risk and expected returns, constant across individuals.

A. Experimental Design

Our experimental design builds on that of Frydman, Barberis, Camerer, Bossaerts, and Rangel (2014) and Frydman and Rangel (2014). All participants trade a portfolio of three stocks, A, B, and C, in an experimental market. Prior to trading, individuals receive instructions that describe the market, the structure of the price update process, and all other details of the experiment.[3] We also provide each individual with $350 in experimental currency. Participants start by purchasing one share of each stock for $100 per share and hold the remaining $50 in cash.

Trials 1 through 9 consist of price updates only, and participants cannot buy or sell during these trials. This restriction enables investors to accumulate information about the price process for the stocks prior to making their trading decisions. Investors then have the opportunity to trade each stock three times. Specifically, the remaining trials (i.e., 10 through 18) include two components: a price update screen and a trading screen. In the price update screen, we randomly select one of the three stocks, and the participant views the price change for that stock only. Stock prices evolve only during the price update screens and, as a result, investors are aware of the entire price path for each stock. In the trading screen, the participant is offered the opportunity to trade the stock. No new information is revealed on the trading display.

Participants may hold either one or zero shares of each stock in their portfolio at any point during the experiment. Investors' decide whether to sell a stock if they hold the stock in their portfolio or whether to purchase the stock if they do not already hold it. Transactions occur at the specified current market price. To avoid liquidity constraints, investors may carry negative cash balances. This feature ensures that participants may purchase a stock even if they do not have sufficient cash during a particular trial.[4]

[3]The instructions provided to participants are available in the Internet Appendix.

[4]A negative cash balance is subtracted from a participant's portfolio value at the conclusion of the experiment. The initial cash endowment and the constraint that individuals could hold one share of each stock at any moment ensure that no one completes the experiment with a negative portfolio value.

The stock prices evolve according to a two-state Markov chain, with a good state and a bad state. Specifically, in trial t, there is a price update for stock i. If the stock is in the good state, the probability of a price increase is 70% and the probability of a price decrease is 30%. If the stock is in the bad state, the probability of a price increase (decrease) is 30% (70%). Each stock's price process is independent of the Markov chains of the other stocks. The magnitude of the price change is independent of the direction (i.e., an increase or a decrease) and is randomly selected to be \$5, \$10, or \$15 with equal probability.

The underlying state of each stock evolves independently of the others. In particular, prior to trial 1, each stock is randomly assigned to a state. Subsequently, the stock's state is updated only after the stock receives a price change. That is, if stock i is randomly selected to receive a price update, then the state of the stock in trial t remains the same as in trial $t-1$ with a probability of 80%. The state switches with a probability of 20%. Participants are not informed of the stock's state in the trial. Rather, investors may infer the state from historical stock prices. To facilitate comparison of investors' trading performance, we use the same set of realized stock prices for all participants.

The experimental stock returns exhibit price momentum, a common feature of equity and capital markets around the world (Jegadeesh and Titman (1993), Fama and French (2012), Asness, Moskowitz, and Pedersen (2013)). If a stock performs well in its prior trial, it is likely in the good state. Consequently, the stock is likely (i.e., an 80% probability) to remain in the same state for the next price update, and the price is likely to increase in the next trial. We employ this design aspect because it implies the optimal trading strategy for a risk-neutral Bayesian investor.[5] Specifically, the optimal strategy, on average, is to sell (purchase) stocks that have performed poorly (well) in recent trials. Ultimately, this strategy has implications for the disposition effect. A risk-neutral Bayesian individual's trades should display the opposite of the disposition effect.

[5]The portfolio choice strategy is also in line with the investment decisions of attentive investors. For instance, Gargano and Rossi (2018) show that attentive investors outperform by purchasing attention-grabbing stocks that have positive price momentum.

After completing the experimental task, participants provide demographic information, such as age, education level, gender, income, risk preferences, and financial literacy. We also liquidate participants' stock holdings and combine the proceeds with any cash reserves. We use investors' final portfolio values to encourage diligent behavior during the task through the participation compensation structure. Participants receive a fixed fee of \$0.50 and a variable payment that depends on task performance. The maximum variable compensation is \$1.00 and depends on the participant's final portfolio value and his/her response to an attention check question. Given the duration of the task, experimental subjects earn about \$7.51 per hour.

B. Tax Consequences of Trading

We hypothesize that increasing individuals' awareness of the tax implications of stock trades will reduce their disposition. We test our conjecture by augmenting our experimental asset market with a tax regime that closely mimics what U.S. investors face when trading stocks in their portfolios. We base our tax consequences on the prevailing U.S. federal tax code as it consistently applies regardless of a citizen's state of residence. In particular, realized capital gains are subject to a 15% tax, which is deducted from the cash proceeds of the stock sale. The cash losses on any realized capital loss are reduced by 15%.[6] We inform all participants of these tax implications in the initial instructions.

Insert Figure 2 About Here.

To identify the causal effect of raising the salience of the tax consequences, we construct two conditions. Individuals in the High-salience (treatment) group observe the unrealized tax implications of a potential sale on the price update screen (Panel A of Figure 2). Conversely, individuals in the Low-salience (control) group do not observe the tax consequences of a potential sale (Panel B of Figure 2). The only difference between the two

[6]We treat the tax consequences in this manner because stocks held for at least a year are subject to a capital gains tax. Conversely, individuals may reduce their tax burdens by realizing capital losses.

conditions is the inclusion (i.e., salience) of information about the tax consequences of trading.

C. Experimental Platform and Participants

We implement the experiment online using Amazon's Mechanical Turk (MTurk) website. The platform provides cost-effective access to a large pool of potential participants and it is being rapidly adopted by consumer behavior researchers who rely on experiments, including researchers in economics and finance (e.g., Duarte, Siegel, and Young (2012), Olea and Strzalecki (2014), Kuziemko, Norton, Saez, and Stantcheva (2015), D'Acunto (2018), Meier, Niessen-Ruenzi, and Ruenzi (2018), Bazley et al. (2020)). The MTurk platform enables researchers to post "Human Intelligence Tasks" (HITs) which "Workers," that is, registered MTurk participants, can perform for compensation (Mason and Suri 2012). Workers are required to register (i.e., receive a worker identification number) and provide taxpayer information, including Social Security number and a permanent residence address.

A general concern about online experiments is the quality of data. While individuals on MTurk are typically compensated less than in-person laboratory participants, the quality of answers is similar between the two settings, reducing selection bias concerns (Casler, Bickel, and Hackett (2013), Goodman, Cryder, and Cheema (2013)).[7] For instance, Amir, Rand, and Gal (2012) show that experiments on MTurk provide comparable results to those in physical laboratories, even when using small compensation levels.

Regardless, we take steps to ensure the quality of our data by following the Goodman and Paolacci (2017) guidelines for facilitating high-quality research using the platform. First, we pay participants a competitive hourly rate for completing our experiment. Second, we limit participation to individuals who previously completed at least 100 HITs and have been positively rated on at least 95% of tasks. Third, we restrict individuals from repeat

[7] One exception is that MTurk participants can be less attentive during experiments, which can ultimately reduce statistical power. MTurk participants are also more likely to exit the task prior to finishing. While we rely on participants who fully complete our experiment, we note that the attrition rate is very low (1.4%) and does not differ significantly between the treatment and control groups.

participation to reduce concerns about "non-naïveté." Fourth, we limit participation in the experiment to individuals residing in the United States.[8] Finally, we exclude participants who complete the experiment in an unrealistically short (i.e., less than three minutes) or long (i.e., over one hour) time.[9] As is standard practice among researchers using MTurk, we exploit the double-blind nature of the platform and do not require participants to reveal their identities. We also do not disclose the nature and objectives of our experiment in order to limit demand characteristics concerns (Orne (1962)).

Insert Table 1 About Here.

We use 699 participants and randomly assign them into either the High-salience or Low-salience condition. Table 1 reports descriptive statistics for both groups. One key advantage of recruiting individuals on the MTurk platform is the heterogeneous participant pool. One-third of our participants are 30 years old or younger, but older participants are also well-represented. In contrast, laboratory samples are commonly constrained to younger participants (usually students) with some college education. We find that about 59% of our participants have at least a four-year undergraduate degree. There is also variation in annual income across our participants, with the average individual earning $61,363, while the median income is approximately $48,000. About 62% of our participants report participating in the stock market.[10] Importantly, we also find that participants in the treatment and control groups are similar along demographic characteristics, which suggests that the random assignment across conditions was effective.

[8]We find that less than 2% of our participants have a non-U.S. IP address, and dropping this subsample does not affect any of our findings.

[9]We find that these individuals account for about 1% of our sample, and the inclusion of all investors does not affect our findings.

[10]We classify individuals as stock market investors if they report owning stocks directly or in retirement accounts. Our participant pool generally aligns with the characteristics reported in nationwide surveys. For instance, a poll conducted in 2019 by Gallup finds that about 55% of Americans invest in equities while the 2016 wave of the Federal Reserve Board's triennial Survey of Consumer Finances (SCF) indicates about 52% of households own stocks (Bricker, Dettling, Henriques, Hsu, Jacobs, Moore, Pack, Sabelhaus, Thompson, and Windle (2017), Saad (2019)).

D. Measuring the Disposition Effect

Our primary measure of interest is an individual's disposition effect, which we calculate following Odean (1998). Specifically, each time an investor is offered the opportunity to sell a stock, we classify his/her decision into one of four mutually exclusive categories: paper gain, paper loss, realized gain, or realized loss. If the investor opts to sell the stock and the current market price is above (below) the purchase price, we classify the transaction as a realized gain (loss). If the current market price is above (below) the participant's purchase price but the investor chooses not to sell the stock, the decision is classified as a paper gain (loss).

We total the number of realized gains, realized losses, paper gains, and paper losses for each participant over the course of the experiment. Subsequently, we calculate the proportion of gains realized, PGR, as:

$$PGR = \frac{Number\ of\ Realized\ Gains}{Number\ of\ Realized\ Gains + Number\ of\ Paper\ Gains} \tag{1}$$

and the proportion of losses realized, PLR, as:

$$PLR = \frac{Number\ of\ Realized\ Losses}{Number\ of\ Realized\ Losses + Number\ of\ Paper\ Losses} \tag{2}$$

In line with the literature (e.g., Odean (1998), Frydman and Rangel (2014)), we then construct our primary measure, DISPOSITION_EFFECT, as PGR − PLR. The magnitude of the disposition effect increases in PGR − PLR while an individual with PGR < PLR displays the opposite of the disposition effect. If PGR equals PLR, then there is no disposition effect.

E. Experimental Evidence

We compare individuals' disposition effect across the treatment and control conditions in order to measure the impact of increasing the salience of the tax consequences of stock trades. Figure 3 shows that individuals randomly assigned to the High-salience (treatment) condition exhibit lower disposition than those in the Low-salience group. In the High-salience group, the average disposition effect is about 0.09. In contrast, the mean disposition effect is about 0.17 in the control condition. The difference, 0.08, is statistically significant (p-value = 0.007) and represents a 47% reduction relative to the control group. In other words, displaying the potential tax implications of selling stocks leads to lower disposition among investors.

Insert Figure 3 About Here.

Our research design randomly allocates each participant into one of the two conditions, but if individuals in the treatment group are less prone to the disposition effect for some exogenous reason, then the previous inference may be biased. For instance, if the High-salience condition oversamples wealthy individuals, who, on average, are less likely to behave according to the disposition effect (e.g., Dhar and Zhu (2006)), then our estimates are confounded. We address this potential concern using two complementary approaches. First, we examine differences in the means of individuals' socioeconomic traits between the treatment and control groups. In Table 1, we find no evidence of systematic differences between participants in the treatment and control conditions.

Second, we estimate a parametric model in which we account for individuals' characteristics. Specifically, we estimate the following ordinary least squares (OLS) regression:

$$DISPOSITION\_EFFECT_j = \alpha_0 + \beta_1 TAX\_SALIENCE_j + \theta X_j + \epsilon_j, \tag{3}$$

where TAX_SALIENCE is an indicator variable that equals one if the participant is in the

High-salience condition, and zero otherwise. The key coefficient of interest, β_1, measures how viewing the potential tax consequences of trading affects individuals' disposition effect. We also include a constant, α_0, and a vector of controls, X_j, which account for the variation in participants' socioeconomic traits.

Insert Table 2 About Here.

The estimates in Table 2 show that the effect of increasing awareness of trades' tax consequences persists after controlling for traits that are known to affect households' portfolio choices. Column 1 reports the univariate point estimate, which is consistent with the graphical evidence in Figure 3. In Columns 2 to 5, we include control variables to account for heterogeneous characteristics among our participants. The estimates in Column 2 suggest that the effect of tax awareness is not subsumed by standard traits that affect households' portfolio choices, including age, education, the natural log of participants' income, gender, race, and risk aversion.[11] That is, the estimate on TAX_SALIENCE is -0.08 and it remains statistically significant (p-value = 0.007).

We expand the specification in Columns 3 and 4 to control for other characteristics, such as financial literacy, trust in the stock market, whether the individual invests in stocks, employment status, optimism, economic expectations, and political affiliation. The impact of tax awareness is not absorbed by these controls. In Column 5, we include two variables that control for whether individuals focus on the experimental task. Specifically, we control for the time required for participants to complete the experiment and whether they answer an attention check question correctly. The estimate on TAX_SALIENCE remains statistically significant (p-value = 0.008) when incorporating these measures.

The estimates of the effects of TAX_SALIENCE vary only slightly, from -0.08 to -0.09, and remain statistically significant across the specifications. That is, the evidence indicates that increasing awareness of the tax implications of selling stocks can moderate

[11] We define all variables in Appendix I.

the disposition effect. Moreover, the estimates also suggest that raising investors' tax awareness can have an economically meaningful impact: it reduces individuals' disposition by approximately 45%.

1. Propensity to Realize Gains and Losses

A natural question is which dimension of the disposition effect, PGR, PLR, or both, does tax salience influence? Similar to the prevailing tax law in the United States, in our experimental setting, accrued tax liabilities and write-offs materialize only when the investor sells the stock. This structure bestows a timing option on the investor (e.g., Constantinides (1983)), whereby the value-maximizing trading behavior is to realize capital losses immediately and defer capital gains. As a result, individuals' tax awareness may affect both dimensions of the disposition effect – reducing the propensity to sell winning stocks and increasing the propensity to liquidate stocks at a loss.

In Figure 4, we plot the unconditional averages of PGR and PLR among investors in the High-salience and Low-salience groups. The graphical evidence suggests that raising tax awareness influences both elements of the disposition effect. Individuals in the High-salience condition retain more of their winning stocks while also selling more of their losing positions.

Insert Figure 4 About Here.

We empirically examine whether tax salience differentially affects investors' propensities to realize gains and losses by re-estimating equation 3 with PGR and PLR (equations 1 and 2, respectively) as the dependent variables. Table 3 reports the results. The estimates indicate that individuals in the High-salience condition are less likely to liquidate their winning stocks. Specifically, the coefficient in Column 1 of -0.052 (p-value = 0.052) translates to a 10.5% reduction in the proportion of gains realized relative to individuals in the Low-salience group.

Greater awareness of tax consequences also influences the willingness to realize losses. In particular, the estimate of 0.037 (p-value = 0.036) in Column 4 shows that investors in the

treatment condition sell more losing stocks relative to individuals in the Low-salience group. The estimate corresponds to a 12% increase in the proportion of losses realized compared to participants in the control condition. The collective evidence shows that both dimensions of the disposition effect are influenced by tax awareness, adjusting behavior toward predictions of canonical portfolio choice models.

Insert Table 3 About Here.

2. Tax Awareness and Trading Performance

Since the disposition effect is costly for investors (e.g., Odean (1998), Kaustia (2010)), we posit that reducing it through tax awareness may lead to an improvement in portfolio performance. We test this conjecture by examining whether the individuals in the High-salience group complete the experiment with more valuable portfolios than do investors in the Low-salience condition.

Insert Table 4 About Here.

In Table 4, we find that individuals in the treatment condition conclude the experiment with higher portfolio values. The univariate estimate in Column 1 indicates that High-salience individuals accumulate, on average, an extra \$2.13 (p-value = 0.003) through their trading decisions. This represents a 0.61% greater investment return during the experiment. In Columns 2 through 5, the estimates remain stable and statistically significant as the specification expands to account for potentially confounding socioeconomic characteristics of the participants. For instance, in Column 5, individuals in the treatment condition generate approximately \$2.19 more wealth (p-value = 0.002), which is about a 0.63% greater investment return. While the magnitudes of wealth gains in our experimental market are limited because of the short trading duration and small investment amounts, the real-world benefits to individual investors may be sizable given their high portfolio turnover (e.g., Barber and Odean (2000)) and longer investment horizons.

3. Tax Salience and Total Trading Activity

The evidence in Table 3 indicates that tax awareness spurs investors to sell losing stocks but reduces their propensity to liquidate winning stocks. However, whether tax salience affects total trading activity is unclear. We, therefore, create a variable, TOTAL_TRADES, that is the total number of buys and sells each participant implements. Since an investor has the opportunity to trade each share three times during the experiment, the maximum number of potential transactions is nine while the minimum is zero. We find that, on average, participants trade about 3.32 times (standard deviation = 2.09) while about 80% of the participants trade at least twice.

Insert Table 5 About Here.

We examine whether investors' total trading activity is influenced by tax salience by re-estimating equation 3 with TOTAL_TRADES as the dependent variable. The OLS estimates in Table 5 show that tax salience does not affect total trading activity. In Column 1, we find no significant difference in the unconditional means between the treatment and control conditions (p-value = 0.875). In Column 5, we include all controls, and the estimate on TAX_SALIENCE remains statistically non-significant. As an alternative test, in Column 6, we conduct a tobit regression where the dependent variable is censored at zero and nine. We again find that tax awareness does not significantly (p-value = 0.620) affect participants' total trading activity.

Collectively, the evidence in Tables 3 and 5 suggest that increasing tax salience influences the shares investors choose to trade, but does not change total trading volume. This finding has additional implications for portfolio performance in light of recent evidence that well-documented trading strategies are not profitable after adjusting for trading costs (e.g., Novy-Marx and Velikov (2015)). Since total trading activity is not affected by tax salience, incremental trading costs are less likely to consume any additional portfolio gains that accrue from reducing investors' disposition.

4. Tax Salience and Portfolio Concentration

With increased tax awareness, investors who sell losing stocks and hold winning stocks could shift toward holding concentrated portfolios. This is a concern since investors' higher portfolio returns could be mitigated by higher portfolio risk. We evaluate this concern by examining heterogeneity in the number of unique shares that participants hold in their portfolio at the conclusion of the experiment.

We report the evidence in Table IA1. The estimates show that participants who view the tax implications cue end the experiment with more stock holdings in their portfolios. For instance, the positive estimate of 0.127 (p-value = 0.049) in Column 5 indicates that individuals in the treatment group hold about 7.93% more stocks relative to subjects in the control condition. Overall, the results show that raising tax awareness is not likely to lead investors to hold less-diversified portfolios.[12]

5. Tax Salience and Socioeconomic Characteristics

Heterogeneity in socioeconomic characteristics may contribute to the variation in investors' willingness to sell winners and hold losing stocks. Dhar and Zhu (2006) suggest that financial sophistication, as measured by income and employment characteristics, reduces investors' propensity to behave according to the disposition effect. Learning, through experience and trading, also shapes behavior (e.g., Seru, Shumway, and Stoffman (2010), Nicolosi, Peng, and Zhu (2009)) and can limit individuals' disposition (Feng and Seasholes (2005), Campbell, Ramadorai, and Ranish (2014)). We, therefore, examine whether socioeconomic traits interact with tax salience to further influence participants' trading decisions.

Table IA2 shows that tax awareness consistently reduces participants' tendency to exhibit the disposition effect. When interacting TAX_SALIENCE with individuals' trust

[12]In unreported empirical results using the field data, we find that investors who experience the tax rate reductions do not have significantly different numbers of stock holdings, ex post, relative to investors who did not experience the rate changes. These results suggest that naïve portfolio diversification did not change in response to the tax rate adjustments.

in the stock market, financial literacy, a stock market participation indicator, or an indicator for racial minority status (i.e., non-White), we find no significant compound effects. In unreported analysis, we find no interactive effects between TAX_SALIENCE and other characteristics, including risk aversion and gender. Overall, the evidence suggests that increasing individuals' awareness of the tax consequences of their stock trades has an independent effect on their disposition.

6. Alternative Specification: Linear Probability Model

While our current measure of the disposition effect facilitates insights into the dimensions (i.e., PGR and PLR) influenced by tax salience, one limitation is that it can be potentially affected by the size of investors' portfolios (Odean (1998), Weber and Camerer (1998)). While this limitation is of greater concern in empirical studies, its impact is likely to be limited in our experimental context since all participants begin with identical portfolios. Nevertheless, as an alternative approach to the Odean (1998) methodology, we use a linear probability model to estimate individuals' disposition effect. Specifically, we estimate:

$$SALE_{i,j,t} = \alpha_0 + \beta_1 GAIN_{i,j,t} + \beta_2 TAX\_SALIENCE_j + \beta_3 GAIN_{i,j,t} \times TAX\_SALIENCE_j + \theta X_j + \epsilon_{i,j,t}, \tag{4}$$

to examine the relation between the disposition effect and tax awareness. In the OLS regression, $\text{SALE}_{i,j,t}$ equals one in trial t if participant j sells share i, and zero otherwise. The independent variable, $\text{GAIN}_{i,j,t}$, is an indicator that equals one if the share's current market price is above the investor's purchase price, and zero otherwise. A positive estimate for β_1 implies that participants are more likely to liquidate positions that are at a gain compared to those at a loss, which indicates a disposition effect. As before, TAX_SALIENCE_j is an indicator that equals one if the individual views the tax implications of the potential transaction, and zero otherwise. A positive estimate for β_2 implies that participants are more likely to sell stocks relative to investors who did not see the potential tax implications. The key

coefficient of interest, β_3, is on the interaction term between GAIN and TAX_SALIENCE. It measures the change in the disposition effect as a result of viewing the potential tax consequences of trading. We also include a vector of controls, X_j, to account for heterogeneity in participants' socioeconomic characteristics, and we cluster the standard errors at the participant level.

Insert Table 6 About Here.

Table 6 reports the estimates from using the linear probability model. Consistent with the preceding analysis, the evidence from the alternative model suggests that increasing the salience of the tax consequences of a potential transaction can reduce the disposition effect. In Column 1, the estimate on GAIN shows that participants are about 25.44% (p-value < 0.001) more likely to sell stocks that have increased in value. The estimate on TAX_SALIENCE suggests that individuals in the High-salience condition are about 2.40% (p-value $= 0.010$) more likely to sell a stock. Importantly, the coefficient on the interaction term is negative and statistically significant, which indicates that increasing the salience of the potential tax consequences of trading reduces investors' disposition. In particular, the estimate corresponds to a 22.05% (p-value $= 0.055$) reduction in the disposition effect.

The effect is not subsumed when controlling for heterogeneity in participants' socioeconomic characteristics in Columns 2 though 5. Estimates, in Column 5, from the strictest specification show that individuals in the High-salience group are about 6.11% (p-value $=$ 0.036) less likely to liquidate a winning stock position. With a propensity to sell gains relative to losses of about 27.24%, tax salience reduces individuals' disposition effect by about 22.43%. Overall, the evidence from this alternative analysis reinforces the primary findings, that tax awareness reduces the disposition effect.

7. Alternative Treatment Methodology: A One-time Tax Reminder

In the primary experiment, we raise individuals' tax awareness by displaying the potential dollar amount of the tax implications of their stock trades. An alternative approach

is to provide participants with a notice that trading can have tax consequences without directly displaying the dollar value of the consequences. We examine whether a one-time tax reminder influences investors' disposition. Specifically, we recruit 301 new individuals to perform the trading experiment. All elements of the experiment are the same as in the primary task except for one: we do not show participants the potential tax dollar implications of selling stocks on the price update screen. Rather, we randomly provide some individuals with a one-time notice, prior to the start of trading, which reminds them that selling a stock has tax consequences.[13] Specifically, the reminder states "When you sell a stock, you receive cash that is net of the trade's tax implications. If you sell a stock that has increased in value, a capital gains tax of 15% will be charged. If you sell a stock that has declined in value, the cash loss will be reduced by 15%."

We report the results of the follow-up experiment in Table 7. In Column 1, we find that investors who receive the tax reminder exhibit lower disposition.[14] We include all socioeconomic controls in Column 2 and find that the effect of the reminder is not subsumed.[15] Specifically, the estimate of -0.098 (p-value < 0.01) corresponds to about a 30% reduction in the disposition effect relative to counterfactual individuals who did not receive the tax notice. The economic magnitude of the tax reminder's effect on investors' disposition is slightly smaller (i.e., 30% compared to 45%) to that of displaying the dollar amount of the tax implications.

Insert Table 7 About Here.

As part of the follow-up experiment, we also ask individuals whether they personally file their taxes, as opposed to delegating tax preparations to a professional. In particular, we ask participants to rate their levels of agreement, on seven-point Likert scales, with the

[13] We find no significant differences across the treatment and control conditions with respect to participants' socioeconomic characteristics.

[14] Consistent with the primary evidence, we find that participants who received the tax reminder are less likely to realize winning stocks and have a higher propensity to sell losing stocks. That is, both dimensions of the disposition effect, PGR and PLR, are influenced.

[15] The number of observations varies slightly across regressions because not all individuals provide answers to all demographic questions.

statements: (i) "In a typical year, I personally prepare and file my own income taxes." and (ii) "In a typical year, I use an accountant to prepare and file my income taxes." We use the responses to these statements to create the indicator variable PERSONAL_FILER. Specifically, PERSONAL_FILER equals one if a participant strongly agrees that s/he personally file his/her taxes and strongly disagrees with the statement that s/he uses an accountant for taxes, and zero otherwise. We use this variable to examine potential heterogeneous effects across individuals who are likely to personally manage versus delegate their taxes.

In Columns 3 and 4 of Table 7, we find that personally filing taxes does subsume the impact of the tax reminder. In Column 5, we interact PERSONAL_FILER with TAX_REMINDER to assess the potentially heterogeneous effects. The estimate on the interaction term is negative and statistically significant (p-value = 0.085), which suggests that the effect of the tax notice is more pronounced for individuals who may interact with taxes more frequently. Including known determinants of portfolio choices, in Column 6, does not subsume the interactive effect. Moreover, the cumulative effect lowers individuals' disposition by about 46%. Overall, the collective evidence from both the primary and follow-up experiments indicates that raising individuals' awareness about the tax consequences of selling stocks can reduce their disposition.

V. Empirical Methodology and Evidence

The primary advantage of the preceding analyses is that the experimental paradigms provide controlled settings in which to isolate the causal impact of tax salience on trading decisions. However, as with any experiment, potential concerns related to external validity arise. We, therefore, use field data to further examine whether trading behavior is affected by tax consequences. The key advantage of the field data is that it provides a naturalistic setting. That is, the real-world setting assists with mitigating potential external validity concerns associated with the experimental approaches.

A. U.S. Investor and Tax Rate Data

We use data from a nationwide discount brokerage firm that has been analyzed in prior studies of retail investor behavior (e.g., Barber and Odean (2000), Barber and Odean (2001), Barber and Odean (2002)), including studies that examine the disposition effect (e.g., Odean (1998)). The data include the transactions and portfolio positions of U.S. retail investors starting in January 1991 and ending in November 1996. For individuals with multiple taxable investment accounts, we aggregate the investments and trades into one household-level account. Accompanying demographic information for each household consists of data on the head of the household's gender, age, income bracket, location of residence (five digit ZIP code), marital status, and home ownership status.[16]

Insert Figure 5 About Here.

Since U.S. states typically tax residents' capital gains at the same level as ordinary income, we supplement the brokerage data with tax rates hand collected from states' Department of Revenue. Figure 5 shows that there is substantial variation in tax rates across states during our sample period. We match the annual tax rates on the trade date to individuals in the brokerage data based on their locations of residence, income bracket, and marital status. Lastly, we obtain data on stock prices and returns from the Center for Research in Security Prices (CRSP).

1. Measuring the Disposition Effect

Following Ivković, Poterba, and Weisbenner (2005), we focus on investors' purchases and sales of common stocks. We only include round-trip transactions so that sales can be matched to the purchase of the shares during the sample period. Consistent with the experimental analysis, we then calculate investors' disposition as the difference PGR and PLR.

[16]We define all variables in Appendix I.

B. Empirical Methodology and Findings

To mirror the experimental analysis in an empirical setting, we aim to pinpoint distinct events where tax rates may become more salient to investors. Therefore, we exploit changes in three states' tax schedules that took place during our sample period.[17] Specifically, in 1995, residents of New York, Michigan, and Delaware experience reductions in their tax rates.[18] For example, in 1994, unmarried New York investors in the median income tax bracket face a 7.0% tax on capital gains. In 1995, the same investors face a rate below 6.6%. In Delaware, the typical investor's rate declines from 7.7% to 7.1%.

We posit that the rate changes may serve as plausibly exogenous shocks to resident investors' tax awareness, leading to a reduction of the disposition effect. However, ex-ante, the rate reductions could have several effects on trading. First, the reductions may spur investors to realize gains as the tax burden of doing so is lower. Alternatively, the rate changes could potentially remind individuals that realized gains remain subject to taxation, despite the reduced burden. These two channels lead to differing predictions for investors' disposition. The former suggests an increase in the disposition effect while the latter may reduce it. Finally, the rate reductions may not change investors' trading decisions if residents are not aware of the changes or if the benefits of trading following the rate reductions do not exceed the costs of trading. In such cases, we expect to find no significant change to investors' disposition. However, it is likely that residents of the states are aware of the tax rate adjustments because the reductions received coverage in mainstream media and news outlets, such as the *Times Union*, *New York Times*, *New York Beacon*, *The Grand Rapids Press*, and *Detroit News* (e.g., Gurnett (1995), Bauder (1995), Weeks (1995)).

To examine the relation between tax changes and the disposition effect, we focus our data to the years surrounding the tax changes, i.e., 1994 and 1995, and use a matched

[17]The enactments of the reductions were typically swift. For instance, Governor Engler of Michigan proposed the tax cut package in his State of the State Address in January and gave lawmakers a 30-day window to pass enabling legislation (Pierog (1995)).

[18]Both married and unmarried investors experienced tax rate changes in New York and Michigan. In Delaware, only unmarried individuals experienced changes to their tax rates.

difference-in-differences methodology. Specifically, we match individuals that experience the tax reductions (i.e., treatment group) to similar investors who do not experience tax rate changes (i.e., control individuals) based on demographic characteristics. This Neyman (1923) and Rubin (1974) matching procedure design assesses the effect of a treatment against an estimation of a similar counterfactual, which would be the outcome for an individual who is not exposed to the treatment.

Following the matching, we employ a difference-in-differences estimator. The underlying intuition for using the difference-in-differences method is that it provides unbiased effect estimates if, in the absence of the treatment, the trend over time would have been the same between the treatment and control groups. However, a potential issue is that the groups may differ in ways related to their trends over time. The matching is used to address this concern and, consequently, is expected to reduce bias from the potential misspecification of the regression model (Ho, Imai, King, and Stuart (2007)).

Insert Table 8 About Here.

Applying this framework to our setting, for each investor in a treatment state, we select a similar individual from a state with no tax changes using nearest-neighbor matching with replacement. We match investors based on their demographic traits in the year prior to the tax reduction. The characteristics underpinning the process include individuals' income, age, marital status, home ownership status, and gender. Table 8 reports the descriptive statistics for our sample of investors. We find that our matched investors are similar, with the mean propensities across the groups differing insignificantly (p-value = 0.983). This suggests that the control investors may serve as an appropriate counterfactual representation for the treated investors. We then estimate a difference-in-differences specification:

$$
\begin{aligned}
(5) \quad DISPOSITION\_EFFECT_{j,t} = {} & \alpha_0 + \beta_1 POST_{1994} \times TAX\_REDUCE\_STATE_{j,t} \\
& + \beta_2 POST_{1994} + \beta_3 TAX\_REDUCE\_STATE_{j,t} \\
& + \theta X_{j,t} + \gamma_t + \rho_s + \epsilon_{j,t}
\end{aligned}
$$

where DISPOSITION_EFFECT$_{j,t}$ measures investor j's disposition in month t. POST$_{1994}$ is an indicator equal to one if the year is 1995, and zero otherwise. TAX_REDUCE_STATE is an indicator equal to one if the investor resides in a state that reduces its tax rate, and zero otherwise. The key coefficient of interest, β_1, is on the interaction term between POST$_{1994}$ and TAX_REDUCE_STATE. It measures the change in investors' disposition following the tax reductions. We include a vector of investor-level controls, X, year fixed effects, γ, and state fixed effects ρ.

Insert Figure 6 About Here.

In Figure 6, we graphically examine whether the matched treated and control investors display similar trends in their disposition. We find that the trends appear similar in the years prior to the tax changes. Given the parallel trends, the aligned control investors seem to be reasonable counterfactuals for the treated investors. The plot also suggests that, after the tax rate reductions, treated investors' disposition is lower than similar investors who are not exposed to tax regime changes.[19]

The estimates from empirical difference-in-differences tests (Panel A of Table 9) further support the graphical evidence. In Column 1, the estimate on the interaction term is -0.034 (p-value = 0.004), meaning that the disposition effect declines in response to the tax rate reductions. Including control variables and fixed effects, in Columns 2 and 3, to increase efficiency and adjust for any residual bias from the matching procedure, does not materially affect the estimate. For instance, in Column 3, the interaction term's estimate is -0.033 (p-value = 0.006), which corresponds to a reduction of about 12% of the unconditional standard deviation.

Insert Table 9 About Here.

[19]The unconditional estimates depicted in the chart suggest that control group investors' disposition increases in the year of the tax change. This may be due to an aggregate time effect, which is accounted for by year fixed effects in the empirical tests.

In Panels B and C of Table 9, we examine how the underlying elements of the disposition effect, PGR and PLR, respectively, are influenced by the rate reductions. The estimates in Panel B suggest that investors reduce the proportion of winning stocks in their portfolio that are sold in response to the lower tax rates. In Column 6, the coefficient on the interaction term, -0.018 (p-value = 0.042), is about 30% of the unconditional average of PGR. In Panel C, Column 9, we find that the interaction term's estimate is 0.015 (p-value = 0.078), which shows that treated investors, compared to investors who are not exposed to the tax rate changes, sell a greater proportion of their losing positions. The estimate is about 8% of the unconditional standard deviation of PLR.

The graphical and empirical evidence suggest that tax rate changes affect investors' trading decisions. Although, in theory, tax rate reductions could encourage an increase in the disposition effect by lowering the tax burden on realized gains, we find that small rate reductions have the opposite effect. The small reductions in tax rates decrease investors' disposition.[20] This finding is consistent with the rate changes serving as a reminder that trading has tax implications. Moreover, the result may potentially be a conservative estimate of the salience effect because the rate reductions economically encourage realizing gains.

1. Alternative Specification: Linear Probability Model

We complement the empirical evidence by estimating an alternative linear probability model to examine whether the tax reductions affect investors' disposition. Specifically, we

[20] As an additional supportive test, we identify five states which reduce tax rates in 1990 and examine whether the disposition effect of investors residing in these treated states differs from the disposition of untreated investors. Untabulated estimates from cross-sectional regressions, which focus on trades executed in 1991, show that treated investors are less disposed towards realizing gains.

estimate:

$$
\begin{aligned}
(6) \quad SALE_{j,i,t} = {} & \alpha_0 + \beta_1 GAIN_{j,i,t} \times POST_{1994} \times TAX\_REDUCE\_STATE_{j,t} \\
& + \beta_2 GAIN_{j,i,t} \times POST_{1994} + \beta_3 GAIN_{j,i,t} \times TAX\_REDUCE\_STATE_{j,t} \\
& + \beta_4 POST_{1994} \times TAX\_REDUCE\_STATE_{j,t} + \beta_5 GAIN_{j,i,t} \\
& + \beta_6 POST_{1994} + \beta_7 TAX\_REDUCE\_STATE_{j,t} + \theta X_{j,t} + \gamma_t + \rho_s + \epsilon_{j,i,t}
\end{aligned}
$$

where SALE equals one if investor j sells stock i in month t, and zero otherwise. GAIN equals one if the stock was at a gain, and zero otherwise. POST_{1994} and TAX_REDUCE_STATE are as in Specification 5. The primary coefficient of interest is β_1 and is on the triple interaction term. It measures the change in the disposition effect among investors residing in tax reduction states in response to the tax reductions. As before, X, is a vector of investor-level controls while γ and ρ are year and state fixed effects, respectively.

Table 10 presents the estimates of equation 6. The estimate on the triple interaction term is consistently negative and statistically significant across the specifications. In the strictest specification, Column 3, which includes investor controls and all fixed effects, the estimate of -0.026 (p-value = 0.049) suggests that treated investors' disposition effect is reduced by about 28%. Overall, the evidence from this alternative analysis reinforces the preceding empirical findings.

Insert Table 10 About Here.

2. Local Economic Conditions

State-specific events or conditions that may influence investors' disposition could occur at the same time as the tax rate adjustments. For instance, the rate reductions could be correlated with the states' economic conditions, which can affect individuals' trading behaviors. In such cases, our estimates would be confounded since investors' trading decisions may be driven by economic factors instead of tax awareness. While this concern is chal-

lenging to fully assuage when using field data, we take several steps to address it. First, we search historical newspapers for concurrent economic and political events that may affect the disposition of investors in our treatment states. We find minimal evidence of prominent or systematic shocks.

As a second step, we conduct a test on investors' trading of stocks of firms that are geographically distant, i.e., not headquartered in the treatment states. We conduct this test because of the political controversy associated with some of the tax cuts (e.g., Pierog (1995), Mauro (1995)). For instance, the income-tax reductions in New York occur alongside proposals by political leaders for less spending.[21] The legislature approved the tax cuts despite polls demonstrating popular opposition to the proposal (Mauro (1995)). While the controversial nature of the rate adjustments could raise resident investors' tax awareness, it may also reflect changes in the economic conditions that local firms face.

To implement the test, we assume that firms with headquarters outside the treatment states are less likely to be influenced by the local economic conditions in the treatment states.[22] We focus on investors' geographically distant stock holdings by excluding firms headquartered in the treatment states and reperform our analysis. If local economic conditions are not the sole driver of investors' trading behavior in response to the tax changes, then we expect that treated investors should have lower disposition when trading geographically distant firms' shares.

Insert Table 11 About Here.

Table 11 reports the estimation results. Consistent with the primary evidence, we find that treated investors have lower disposition even when selling geographically distant

[21] Governor Pataki was the first New York governor since 1943 to call for less spending than in the prior year while also proposing income-tax cuts (Dao (1995)).

[22] While firms are likely to have operations across geographies, corporate headquarters are typically close to firms' core business activities. Moreover, firms' key decision makers commonly reside at corporate headquarters and these locations tend to serve as prominent points for the exchange of information between firms and stakeholders (Davis and Henderson (2008), Wang and Pirinsky (2010)). Consequently, firms' headquarter locations are frequently used to examine the relations between firms, investors, and financial markets (e.g., Coval and Moskowitz (1999), Ivković and Weisbenner (2005), Seasholes and Zhu (2010), Bernile, Korniotis, Kumar, and Wang (2015), Parsons, Sabbatucci, and Titman (2018)).

holdings (Panel A). The magnitude and statistical significance of the results are similar to those previously identified. In Panels B and C, we examine investors' tendencies to realize gains and losses, respectively. The results are consistent with the full-sample evidence: investors reduce the proportion of gains realized and increase the proportion of losses sold in response to the tax rate reductions. Our test results show that investors' disposition is influenced by the rate adjustments, even when trading shares of firms that are less likely be affected by the local economies of the treatment states.

3. Heterogeneous Responses to the Tax Rate Changes

Tax awareness is likely to vary through time and across investors. For example, individuals could become more cognizant of taxes as the year approaches its end. As a result, investors who are not affected by the tax rate adjustments, i.e., our counterfactual investors, may display trading behaviors in December that are similar to those of investors who experience the tax changes. To test this hypothesis, we create an indicator, DECEMBER, that equals one for stock trades that are executed in the month of December, and zero otherwise. We then expand Equation 5 to include a triple interaction term comprised of POST_{1994}, TAX_REDUCE_STATE, and DECEMBER. If control investors are aware of the tax implications of their stock trades in December, then we expect the estimate on the triple interaction to be not statistically significant.

In Column 1 of Table IA3, we find that the trading behaviors of treated and control investors do not significantly differ in December. That is, the estimate on the triple interaction term is statistically non-significant (p-value = 0.376). We interpret this evidence to suggest that investors, regardless of experiencing a tax rate adjustment, are likely to be aware of taxes during December, which influences their trading decisions.

Heterogeneity in investors' socioeconomic characteristics may also contribute to variation in tax awareness. For instance, certain individuals, such as those with higher incomes, may be more sensitive to tax changes. Consistent with this conjecture, in Column 2, we find

that the disposition of high earners, i.e., investors who earn income of about $87,500 or more per year, is more sensitive to the tax rate adjustments. We also examine whether individuals who are around retirement age may be more sensitive to the tax changes. We create an indicator, RETIREMENT, which equals one if the investor is at least 65 years of age, and zero otherwise. We construct a triple interaction term using RETIREMENT and our two primary explanatory variables. The coefficient on the triple interaction term is statistically non-significant, suggesting that the tax rate adjustments do not differentially affect older investors' disposition effect. A potential explanation for the lack of heterogeneous behavior is that the trades of investors around retirement age may be influenced by elements which supersede the tax consequences, such as liquidity needs. Nevertheless, the independent estimate on RETIREMENT is negative and statistically significant (p-value = 0.065), which shows that older investors are less likely to be disposed towards selling their winning stock holdings.

Marriage can have tax consequences and affect the trading behavior of individuals (e.g., Love (2010)). Moreover, income taxes can influence individuals' decisions to marry (Alm and Whittington (1995), Sjoquist and Walker (1995)). Households can also face marriage tax penalties or bonuses (Zelenak (1993), Brozovsky and Cataldo (1994), Feenberg and Rosen (1994)). As a result, it is unclear whether the tax rate adjustments will cause differential behavior between married and unmarried investors. We interact MARRIED, an indicator equal to one if the investor is married, and zero otherwise, with our primary variables and find no incremental effects on married investors.

Housing plays a prominent role in individuals' financial decision-making: variation in house prices heterogeneously affects households' consumption and portfolio choices (Ludwig and Sløk (2004), Yao and Zhang (2005), Hu (2005), Campbell and Cocco (2007), Hryshko, Luengo-Prado, and Sørensen (2010), Attanasio, Bottazzi, Low, Nesheim, and Wakefield (2012)). However, the dynamics between taxes, housing, and portfolio decisions are complex.

Income tax rates can influence individuals' homeownership decision.[23] Moreover, homeowners and non-homeowners can face different tax schemes, which may lead to heterogeneity in their portfolio choices following tax rate changes. However, a plausible alternative hypothesis is that tax rate reductions could affect investors' housing and consumption decisions and, in turn, influence their stock trading choices, potentially resulting in liquidity trading or re-balancing from financial assets to real asset holdings. Ultimately, it is unclear whether homeowners' disposition in response to the tax rate changes will systematically differ from that of non-homeowners. We examine potential heterogeneity in homeowners' trading decisions by interacting HOMEOWNER, an indicator equal to one if the investor owns a home, and zero otherwise, with our primary explanatory variables. We do not find evidence that homeowners' disposition, in response to the tax rate adjustments, differs from the disposition of non-homeowners (p-value = 0.683).

Overall, we find heterogeneity in the responses of investors to the tax rate adjustments. The trading effects of the rate adjustments are time varying, with stock trades in December being similar among both treated and control investors. The trades of investors who have high incomes are also more sensitive to the changes in the tax schemes. Older investors are less likely to trade in line with the disposition effect, but the tax adjustments do not have incremental influence on their trades relative to younger investors.

4. Dividend-paying Stocks

Investors also pay taxes on dividend income. Accordingly, events which increase tax awareness could influence investors' trading behavior with respect to dividend-paying stocks. To examine whether investors are more likely to sell dividend-paying stocks in response to the tax rate changes, we obtain data on firms' cash dividends from CRSP. We create an indicator variable, DIVIDEND_PAYER, which equals one if the firm pays a dividend during the year, and zero otherwise. We then estimate a linear probability model where the dependent

[23]Narwold and Sonstelie (1994) link heterogeneity in income tax rates across states to variation in homeownership rates.

variable is the indicator SALE to examine the influence of dividends on the propensity to sell shares of the firm.

We do not find empirical support for the tax-motivated trading hypothesis with respect to dividends. The estimates in Table IA4 show that investors are not prone to selling dividend-paying stocks following the tax regime change. Moreover, we find no evidence that investors' disposition to sell winning, dividend-paying stocks is affected by the tax rate adjustments. The lack of heterogeneous trading behavior may arise because individuals tend to rely on mental accounting and treat dividends as an instrument for self-control (Thaler and Shefrin (1981), Thaler (1999), Baker, Nagel, and Wurgler (2006)). Shefrin and Statman (1984) suggest that investors mentally separate cash flows arising from capital gains and dividends. Individuals then use dividends as a self-control mechanism against overconsumption. Consequently, individual investors may retain dividend-paying stocks in their portfolios even in the face of tax salience events.

5. Placebo Test

To further examine the robustness of our results, we conduct a placebo test in which we randomize the set of treated investors. In particular, we exclude residents of the treatment states. We then assign investors in three random states to be "treated," i.e., to experience a fictitious tax rate change in 1995.[24] For these investors, TAX_REDUCE_STATE equals one. We then match treated investors along socioeconomic characteristics to similar investors in other states, who serve as counterfactuals. For the control investors, TAX_REDUCE_STATE takes a value of zero. Following the matching, we re-estimate Equation 5 and retain the estimate on $\text{POST}_{1994} \times$ TAX_REDUCE_STATE to examine the impact of the placebo tax change on investors' disposition. We repeat this process for all combinations of states in order to construct a distribution of coefficients, which we compare with our primary evidence in

[24] An alternative approach is to retain the matched investors in our primary analysis but assume a fictitious rate adjustment in another year. In untabulated results, we compare the trading behavior of our matched investors around a fictitious rate change in 1992. We find that treated investors' disposition does not significantly differ from that of investors in the control group.

Panel A of Table 9.

Insert Figure 7 About Here.

Figure 7 presents a histogram of the random-state placebo coefficient estimates. The estimates range from -0.094 to 0.148, with an average of 0.00. We plot a vertical line at -0.033, from Column 3 in Table 9, to represent the estimated impact of the real tax changes on investors' disposition. We find that our estimate is in the left tail, falling in the sixth percentile of the placebo test distribution. We interpret this finding as support for our hypothesis that the real state-level tax rate changes affect investors' trading behavior.

6. All Investors

We evaluate whether our matched-sample results could be driven by selection bias. If our sample of matched investors differs from the full population of investors along some characteristic, then inferences drawn from the findings may be confounded. To address this concern, we repeat our analysis using the full data set. Specifically, we abstain from matching treated investors to their similar counterfactual investors and re-estimate Equation 5 using all investors' trades across all years in our sample period.

In Table IA5, we find that the summary statistics for the full data set are similar to those in Table 8, suggesting limited potential for selection bias in the matched sample. Consistent with our primary evidence, in Table IA6, we again find that investors' disposition declines following the tax regime changes. Overall, the evaluation of the complete data set suggests that our primary findings are not likely to be predominantly driven by selection bias.

7. Trading During Quiet Tax Regimes

Although we find that increased tax awareness mitigates the disposition effect, a first-order question is whether cross-sectional variation in state tax rates explains investors'

heterogeneous trading behavior. We examine this question by testing the relation between investors' tax rates and their disposition. We posit that if individuals normally consider the tax consequences of their trading activity, there will be a negative relation between tax rates and the disposition effect given the timing benefit bestowed by the U.S. tax structure (e.g., Constantinides (1983)). We test our prediction by estimating panel OLS regressions:

$$(7) \qquad DISPOSITION\_EFFECT_{j,t} = \alpha_0 + \beta_1 TAX\_RATE_{j,t} + \theta X_{j,t} + \gamma_t + \rho_s + \epsilon_{j,t}$$

where DISPOSITION_EFFECT$_{j,t}$ measures investor j's disposition in month t. The key explanatory variable, TAX_RATE, is the individual's state-level tax rate during the year. We also include a vector of investor-level controls, X, to control for heterogeneity in socioeconomic characteristics. To account for aggregate time trends and time-invariant unobservables among states, we include year, γ, and state, ρ, fixed effects, respectively. We cluster the standard errors at the state-level.[25]

We report the estimates in Table IA7. The estimate on TAX_RATE is not statistically significant in any specification, which suggests that state-level tax implications are not likely to be strong determinants of the typical retail investor's disposition during times when tax rates are not salient. The non-significant relation could manifest through two channels. Investors may, on average, be unaware that selling stocks has tax consequences. Alternatively, individuals may be aware of the tax implications but the consequences are not sufficient to influence behavior. Overall, our empirical findings suggest that heightened tax awareness, through tax regime changes or visual cues, may nudge (e.g., Thaler and Sunstein (2008)) individuals' portfolio choices towards those predicted by canonical models.

[25]We find similar effects when clustering at the investor and ZIP code-levels.

VI. Conclusion

Death and taxes are typically certain elements of life (Bullock 1716). However, they may not be salient when individuals are making their investment decisions. While perceptions of mortality can affect financial choices (e.g., Heimer, Myrseth, and Schoenle (2019)), we focus on taxes. We conjecture that raising investors' tax awareness can influence their investment decisions. Simply observing the tax implications of stock trades may moderate one's disposition effect, a behavioral bias that reduces individuals' portfolio returns and affects asset prices.

We use multiple identification approaches to support our prediction. To isolate whether raising tax awareness can moderate the disposition effect, we conduct a trading experiment with a large group of heterogeneous participants. We find that increasing the salience of capital gains taxes reduces individuals' propensity to realize gains and increases their tendency to realize losses. Consequently, tax-aware investors have higher portfolio returns without trading more frequently. To lend external validity to our experimental evidence, we use data on retail investors' portfolios and trades from a nationwide brokerage. We find that the disposition effect is not explained by the cross-sectional variation in investors' state-level tax rates. However, using tax rate reductions across several states as potential shocks to individuals' tax awareness, we find that investors display a lower disposition effect when taxes are likely to be salient around the rate changes.

The empirical and experimental analyses are complementary. The experiment offers causal insights through a randomized controlled trial framework while the field data provide evidence from a naturalistic setting. The consistency in the evidence between the two settings increases the confidence in the generalizability of the findings. Overall, that tax awareness can affect individuals' portfolio choices suggests scope for policy initiatives or just-in-time interventions to improve households' financial decision-making.

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Figure 1: Search Volume for Tax Loss Selling by Calendar Month

This figure depicts the average number of monthly Google searches for "tax loss selling" in the United States in each calendar month from January 2004 to December 2019. Google Trends provides a Search Volume Index (SVI), normalized so that the month with the highest search volume in the sample, December 2008, has an SVI of 100. The x-axis shows the calendar month and the y-axis shows the average SVI for that month.

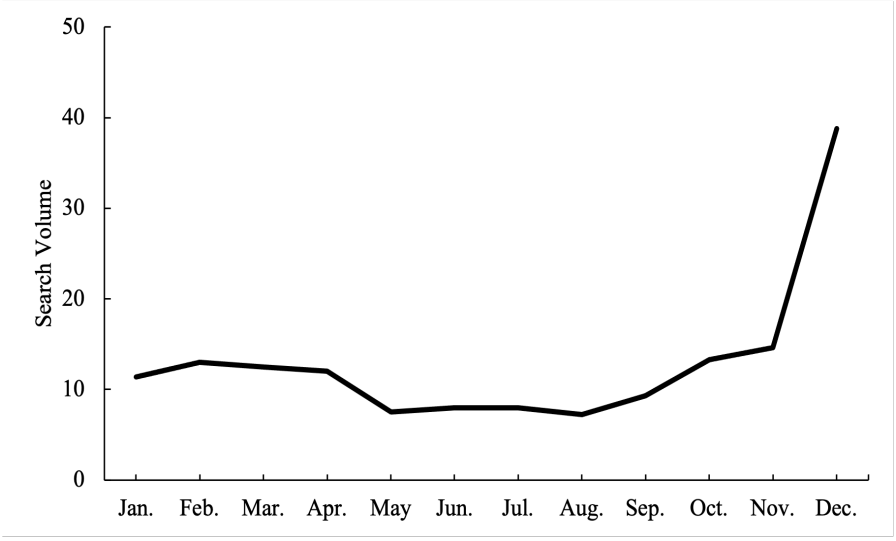

Figure 2: Trading Screens from the Two Experimental Conditions

Each participant is randomly assigned to one of two groups. Investors in the High-salience (treatment) group observe a visual cue of the unrealized tax implications for a potential stock sale. Investors in the Low-salience (control) group do not observe any visual cue of the unrealized tax implications for a potential stock sale. This figure displays an example of how individuals in the treatment group (Panel A) and control group (Panel B) are visually prompted with the question of whether to sell a stock in their portfolio.

Panel A: High-salience (Treatment) Condition

Panel B: Low-salience (Control) Condition

Figure 3: The Impact of Tax Salience on the Disposition Effect

This figure reports estimates of the effect of tax salience on individual investors' trading behavior. The bars show the mean DISPOSITION_EFFECT for investors in the treatment (High-salience) and control (Low-salience) conditions. We calculate DISPOSITION_EFFECT as PGR less PLR. Error bars show the mean ± one standard error.

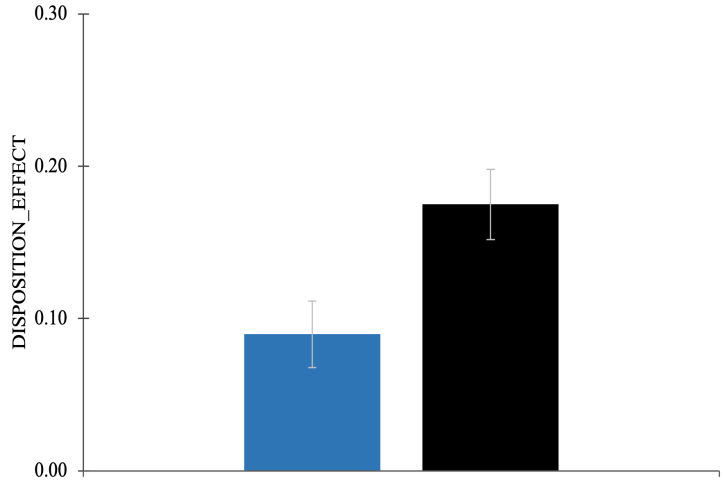

Figure 4: Effects of Tax Salience on the Proportions of Gains and Losses Realized

This figure reports estimates of the effect of tax salience on individual investors' propensity to realize gains and losses. The bars show the mean PGR and PLR for individuals in the treatment (High-salience) and control (Low-salience) conditions. Error bars show the mean ± one standard error.

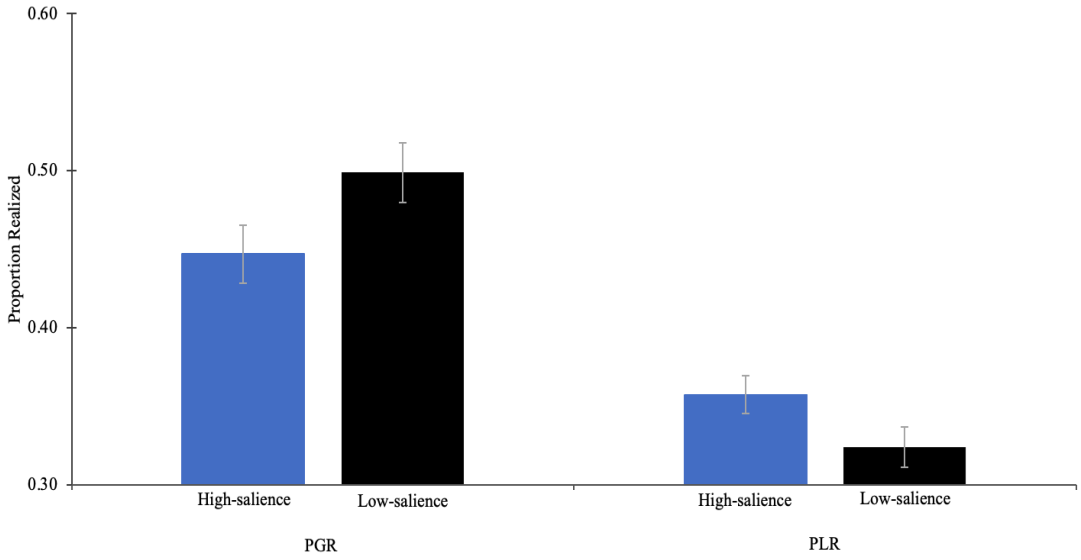

Figure 5: Tax Rates Across States

This figure illustrates the distribution of state-level individual tax rates across the United States. Specifically, it presents the average rate across investors within each state during our sample period, which covers 1991-1996.

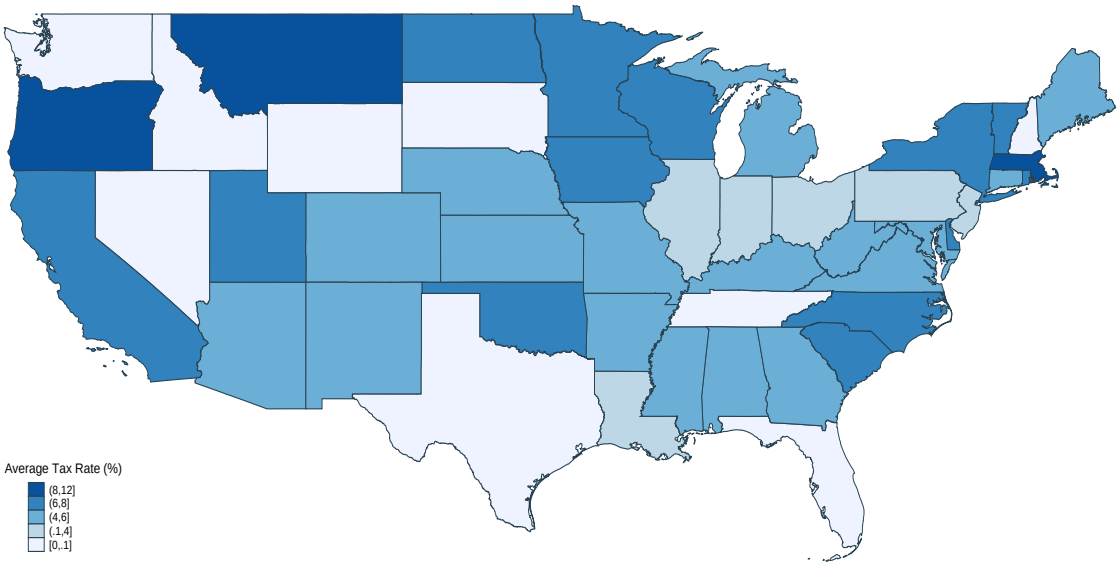

Figure 6: The Disposition Effect Around Tax Regime Changes

This figure presents the average DISPOSITION_EFFECT for the treatment and control groups of investors before and after tax regime changes. The treatment group includes investors in New York, Michigan, and Delaware during a period surrounding changes in state tax rates. The control group consists of a matched sample of investors. The criteria for matching include income, age, marital status, home ownership status, and gender. The horizontal axis shows time in relation to the tax regime changes. The vertical axis displays the DISPOSITION_EFFECT, calculated as PGR less PLR.

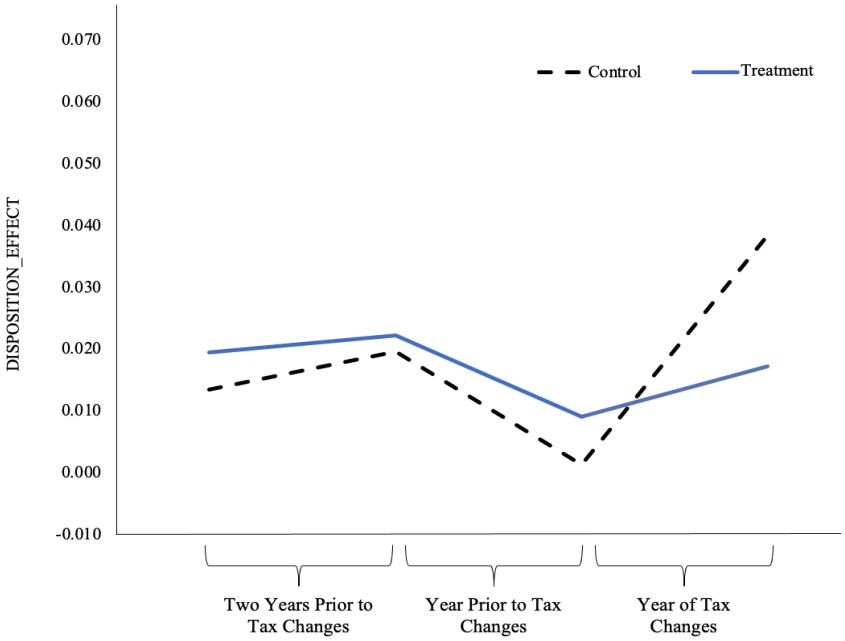

Figure 7: Placebo Test of Random State Tax Rate Changes

This figure presents a histogram of the distribution of outcomes from a placebo test of random state-level capital gains tax rate changes. We test every combination of three states and assume these three states experience a fictional capital gains tax rate change in 1995. In each test, we match the investors in the three treated states to similar investors in other states, who serve as the counterfactual. The criteria for matching include income, age, marital status, home ownership status, and gender. For the treated investors, TAX_REDUCE_STATE takes a value of one and for the counterfactual investors, TAX_REDUCE_STATE takes a value of zero. Following the matching, we re-estimate Equation 5 and retain the estimate on $POST_{1994}$ × TAX_REDUCE_STATE to examine the impact of the placebo tax change on the disposition effect. The x-axis reports the range of coefficient estimates in bins with a width of 0.01. The y-axis is the percent of observations which fall within each bin. The figure shows a vertical line at -0.033, which represents the coefficient estimate on the interaction term from the actual sample of three treated states from Column 3 in Table 9. Our estimate of the effect of the tax change in the three treated states falls in the sixth percentile of the distribution generated by the placebo test.

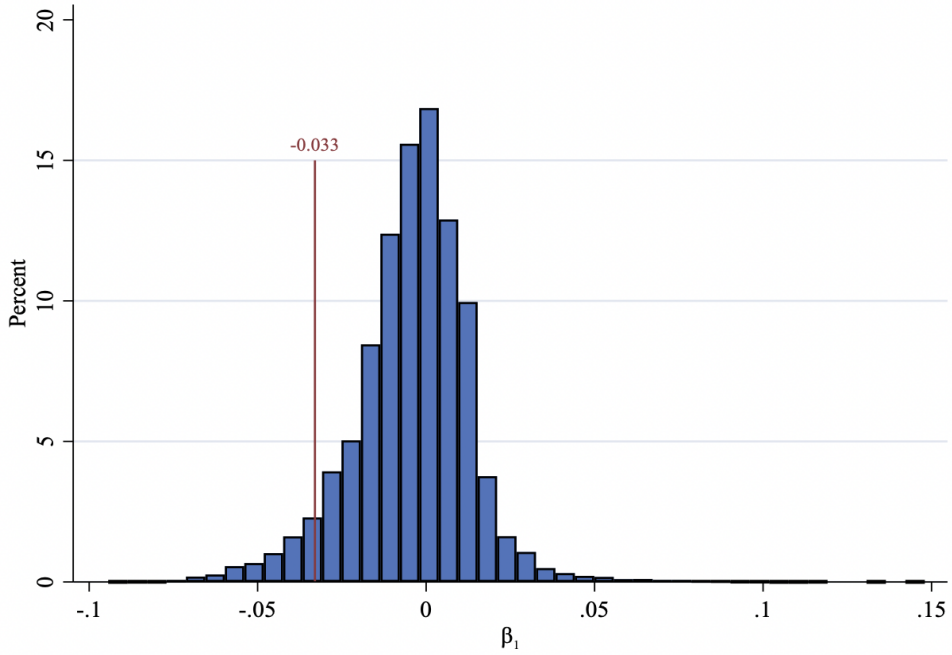

Table 1: Summary Statistics for the Experimental Participants

This table reports summary statistics for experimental participants in the High-salience (treatment) and Low-salience (control) conditions. The final column reports p-values from two-sample t-tests for the difference in means for each variable across participants in the treatment and control conditions. All variables are defined in Appendix I.

	High-salience (Treatment)			Low-salience (Control)			
	Mean	St. Dev.	N	Mean	St. Dev.	N	p-value
AGE	4.537	2.032	350	4.573	2.111	349	(0.819)
EDUCATION	3.437	1.478	350	3.585	1.479	349	(0.188)
INCOME	3.803	0.889	350	3.831	0.851	349	(0.668)
GENDER	1.246	0.968	350	1.181	0.982	349	(0.377)
RACE	6.926	2.073	350	6.713	2.247	349	(0.195)
RISK_AVERSION	28.929	5.219	350	28.811	5.226	349	(0.766)
FINANCIAL_LITERACY	2.214	0.977	350	2.183	0.989	349	(0.678)
TRUST_MARKET	0.414	1.554	350	0.404	1.654	349	(0.933)
INVESTOR	0.637	0.482	350	0.605	0.490	349	(0.376)
EMPLOYMENT_STATUS	6.460	1.362	350	6.544	1.235	349	(0.391)
OPTIMISM	1.289	1.520	350	1.350	1.493	349	(0.593)
ECONOMIC_OUTLOOK	1.194	1.098	350	1.229	1.088	349	(0.673)
POLITICS	2.243	1.604	350	2.309	1.684	349	(0.593)
TASK_DURATION	608.051	433.325	350	576.444	382.102	349	(0.307)
ATTENTION_CHECK	0.803	0.398	350	0.771	0.421	349	(0.301)

Table 2: Experimental Evidence on Tax Salience and the Disposition Effect

This table reports OLS estimates on the relationship between tax salience and the disposition effect. The dependent variable is the subject's DISPOSITION_EFFECT, calculated as PGR less PLR. The key explanatory variable of interest is TAX_SALIENCE, an indicator variable equal to 1 if the participant is randomly assigned to the High-salience (treatment) condition, and 0 otherwise. The other variables are defined in Appendix I. We estimate White (1980) heteroskedasticity-robust standard errors and corresponding $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4	5
TAX_SALIENCE	-0.085***	-0.082***	-0.082***	-0.079***	-0.081***
	(-2.70)	(-2.69)	(-2.67)	(-2.59)	(-2.65)
AGE		-0.034***	-0.031***	-0.031***	-0.032***
		(-4.55)	(-4.14)	(-4.10)	(-4.15)
EDUCATION		0.012	0.013	0.013	0.013
		(1.14)	(1.24)	(1.19)	(1.21)
INCOME		0.034*	0.029	0.017	0.018
		(1.85)	(1.29)	(0.78)	(0.82)
GENDER		0.003	0.006	0.008	0.008
		(0.22)	(0.40)	(0.49)	(0.53)
RACE		-0.000	0.001	0.001	0.002
		(-0.06)	(0.17)	(0.13)	(0.30)
RISK_AVERSION		-0.012***	-0.009**	-0.008**	-0.009**
		(-4.03)	(-2.47)	(-2.33)	(-2.40)
FINANCIAL_LITERACY			-0.034*	-0.031*	-0.033*
			(-1.87)	(-1.73)	(-1.85)
TRUST_MARKET			0.003	-0.002	-0.002
			(0.29)	(-0.15)	(-0.14)
INVESTOR			-0.005	-0.020	-0.021
			(-0.13)	(-0.57)	(-0.61)
EMPLOYMENT_STATUS			0.005	0.006	0.006
			(0.31)	(0.38)	(0.41)
OPTIMISM				0.020*	0.019
				(1.68)	(1.64)
ECONOMIC_OUTLOOK				0.015	0.014
				(0.90)	(0.87)
POLITICS				0.013	0.013
				(1.43)	(1.37)
TASK_DURATION					0.000
					(0.63)
ATTENTION_CHECK					0.033
					(0.92)
N	699	699	699	699	699
Adj. R-sq.	0.009	0.082	0.082	0.089	0.088

Table 3: Experimental Evidence on Tax Salience and Realized Gains and Losses

This table reports OLS estimates of the effects of tax salience on participants' tendencies to realize gains and losses. In Columns 1 and 2, the dependent variable is PGR, as measured in equation 1. In Columns 3 and 4, the dependent variable is PLR, as measured in equation 2. The key explanatory variable of interest is TAX_SALIENCE, which is an indicator variable that is 1 if the participant was randomly assigned to the High-salience (treatment) condition, and 0 otherwise. The set of control variables in Columns 2 and 4 are described in Appendix I. We estimate White (1980) heteroskedasticity-robust standard errors and corresponding $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	PGR		PLR	
	1	2	3	4
TAX_SALIENCE	-0.052*	-0.044*	0.033*	0.037**
	(-1.95)	(-1.84)	(1.89)	(2.10)
Controls	No	Yes	No	Yes
N	699	699	699	699
Adj. R-sq.	0.004	0.192	0.004	0.038

Table 4: Experimental Evidence on Tax Salience and Portfolio Performance

This table reports estimates of OLS regressions on the relation between tax salience and the investment performance of our experimental participants. The dependent variable is the participant's total cash value, in dollars, at the conclusion of the experiment after liquidating all portfolio holdings. The key explanatory variable of interest is TAX_SALIENCE, an indicator variable that equals 1 if the participant is randomly assigned to the High-salience (treatment) condition, and 0 otherwise. The other variables are defined in Appendix I. We estimate White (1980) heteroskedasticity-robust standard errors and corresponding $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4	5
TAX_SALIENCE	2.132***	2.215***	2.182***	2.171***	2.191***
	(3.03)	(3.14)	(3.08)	(3.06)	(3.09)
AGE		0.334*	0.334*	0.341*	0.348*
		(1.88)	(1.83)	(1.82)	(1.85)
EDUCATION		-0.061	-0.077	-0.089	-0.101
		(-0.25)	(-0.31)	(-0.35)	(-0.40)
INCOME		0.109	0.105	0.180	0.145
		(0.25)	(0.22)	(0.36)	(0.29)
GENDER		-1.117***	-1.101***	-1.118***	-1.130***
		(-3.12)	(-2.97)	(-3.00)	(-3.03)
RACE		-0.014	-0.028	-0.016	-0.040
		(-0.08)	(-0.16)	(-0.09)	(-0.23)
RISK_AVERSION		-0.053	-0.024	-0.016	0.007
		(-0.76)	(-0.30)	(-0.19)	(0.08)
FINANCIAL_LITERACY			-0.226	-0.198	-0.122
			(-0.49)	(-0.43)	(-0.26)
TRUST_MARKET			0.110	0.151	0.153
			(0.45)	(0.61)	(0.62)
INVESTOR			0.489	0.561	0.543
			(0.60)	(0.68)	(0.66)
EMPLOYMENT_STATUS			-0.215	-0.207	-0.223
			(-0.69)	(-0.66)	(-0.71)
OPTIMISM				-0.421	-0.424
				(-1.53)	(-1.54)
ECONOMIC_OUTLOOK				0.418	0.402
				(1.11)	(1.06)
POLITICS				0.060	0.071
				(0.28)	(0.33)
TASK_DURATION					0.001
					(0.58)
ATTENTION_CHECK					-1.228
					(-1.30)
N	699	699	699	699	699
Adj. R-sq.	0.012	0.023	0.019	0.019	0.019

Table 5: Experimental Evidence on Tax Salience and Total Trading Activity

This table reports estimates from OLS regressions, in Columns 1 through 5, and a Tobit regression, in Column 6, of the effects of tax salience on the trading activity of our experimental participants. The dependent variable is TOTAL_TRADES, the total number of buys and sells that each participant implements during the experiment. The key explanatory variable of interest is TAX_SALIENCE, an indicator variable that equals 1 if the participant is randomly assigned to the High-salience (treatment) condition, and 0 otherwise. The other variables are defined in Appendix I. We estimate White (1980) heteroskedasticity-robust standard errors and corresponding $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4	5	6 Tobit
TAX_SALIENCE	0.025	0.071	0.074	0.082	0.068	0.076
	(0.16)	(0.48)	(0.51)	(0.56)	(0.47)	(0.50)
AGE		-0.026	0.014	0.014	0.008	0.003
		(-0.69)	(0.38)	(0.37)	(0.22)	(0.06)
EDUCATION		0.069	0.083	0.082	0.082	0.085
		(1.20)	(1.46)	(1.44)	(1.43)	(1.40)
INCOME		0.231**	0.188*	0.151	0.149	0.138
		(2.48)	(1.79)	(1.40)	(1.38)	(1.22)
GENDER		-0.061	0.010	0.015	0.018	0.013
		(-0.76)	(0.12)	(0.18)	(0.22)	(0.15)
RACE		-0.061	-0.034	-0.035	-0.029	-0.031
		(-1.57)	(-0.89)	(-0.92)	(-0.76)	(-0.76)
RISK_AVERSION		-0.116***	-0.072***	-0.071***	-0.068***	-0.070***
		(-7.30)	(-4.19)	(-4.08)	(-3.84)	(-3.79)
FINANCIAL_LITERACY			-0.538***	-0.532***	-0.537***	-0.559***
			(-6.17)	(-6.06)	(-6.10)	(-6.03)
TRUST_MARKET			-0.007	-0.024	-0.023	-0.025
			(-0.14)	(-0.47)	(-0.45)	(-0.47)
INVESTOR			0.017	-0.032	-0.049	-0.036
			(0.10)	(-0.19)	(-0.30)	(-0.20)
EMPLOYMENT_STATUS			0.001	0.004	0.005	0.004
			(0.01)	(0.07)	(0.08)	(0.06)
OPTIMISM				0.081	0.075	0.072
				(1.33)	(1.25)	(1.12)
ECONOMIC_OUTLOOK				0.023	0.013	0.009
				(0.28)	(0.16)	(0.10)
POLITICS				0.031	0.029	0.034
				(0.69)	(0.64)	(0.71)
TASK_DURATION					0.000	0.000
					(1.53)	(1.63)
ATTENTION_CHECK					0.001	0.009
					(0.01)	(0.05)
N	699	699	699	699	699	699
Adj. R-sq.	-0.001	0.124	0.166	0.166	0.170	0.046

Table 6: Effects of Tax Salience Using a Linear Probability Model

This table reports OLS regression estimates of the relation between tax salience and the disposition effect using the linear probability model in Equation 4. The dependent variable, SALE, equals 1 in trial t if experimental subject j sells stock, and 0 otherwise. The independent variable GAIN is an indicator that equals 1 if the stock's current price is above the subject's purchase price, and 0 otherwise. TAX_SALIENCE is an indicator variable that equals 1 if the participant is randomly assigned to the High-salience (treatment) condition, and 0 otherwise. The key explanatory variable is GAIN × TAX_SALIENCE and it measures the change in the disposition effect as a result of viewing the capital gains tax implications of trading. All variables are defined in Appendix I. We cluster standard errors at the participant-level and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4	5
GAIN	0.254***	0.268***	0.271***	0.272***	0.272***
	(12.10)	(12.75)	(12.96)	(13.01)	(13.01)
TAX_SALIENCE	0.024***	0.028***	0.030***	0.031***	0.030***
	(2.58)	(2.65)	(2.78)	(2.89)	(2.78)
GAIN × TAX_SALIENCE	-0.056*	-0.058**	-0.061**	-0.061**	-0.061**
	(-1.93)	(-2.00)	(-2.10)	(-2.12)	(-2.10)
AGE		-0.009***	-0.006**	-0.006**	-0.006**
		(-2.91)	(-1.97)	(-1.98)	(-2.05)
EDUCATION		0.007	0.008*	0.008*	0.008*
		(1.52)	(1.82)	(1.82)	(1.80)
INCOME		0.017**	0.013*	0.009	0.009
		(2.36)	(1.67)	(1.11)	(1.06)
GENDER		-0.009	-0.004	-0.004	-0.003
		(-1.44)	(-0.69)	(-0.59)	(-0.55)
RACE		-0.004	-0.002	-0.002	-0.002
		(-1.32)	(-0.66)	(-0.73)	(-0.65)
RISK_AVERSION		-0.009***	-0.006***	-0.006***	-0.006***
		(-7.90)	(-4.66)	(-4.54)	(-4.16)
FINANCIAL_LITERACY			-0.039***	-0.039***	-0.038***
			(-5.84)	(-5.74)	(-5.71)
TRUST_MARKET			0.001	-0.001	-0.001
			(0.34)	(-0.17)	(-0.15)
INVESTOR			-0.002	-0.007	-0.009
			(-0.14)	(-0.58)	(-0.68)
EMPLOYMENT_STATUS			0.000	0.000	0.000
			(0.04)	(0.11)	(0.10)
OPTIMISM				0.011**	0.010**
				(2.26)	(2.21)
ECONOMIC_OUTLOOK				0.001	-0.000
				(0.11)	(-0.00)
POLITICS				0.004	0.004
				(1.19)	(1.17)
TASK_DURATION					0.000
					(1.62)
ATTENTION_CHECK					-0.008
					(-0.57)
N	6,111	6,111	6,111	6,111	6,111
Adj. R-sq.	0.065	0.086	0.091	0.092	0.092

Table 7: Effects of a One-time Tax Reminder on the Disposition Effect

This table reports OLS regression estimates of the relation between a tax reminder the disposition effect among our experimental participants. The dependent variable is each subject's DISPOSITION_EFFECT, calculated as the proportion of gains realized less the proportion of losses realized. The key explanatory variable of interest is TAX_REMINDER, an indicator variable that equals 1 if the participant is randomly assigned to the receive a one-time reminder, prior to trading, about the tax implications of selling stocks, and 0 otherwise. PERSONAL_FILER is an indicator variable that equals 1 if the participant reports personally filing his/her taxes, and 0 otherwise. Control variables are defined in Appendix I. We estimate White (1980) heteroskedasticity-robust standard errors and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4	5	6
TAX_REMINDER	-0.105***	-0.098***	-0.103***	-0.098***	-0.078**	-0.070**
	(-3.25)	(-2.84)	(-3.17)	(-2.85)	(-2.34)	(-1.98)
PERSONAL_FILER			-0.045	-0.000	0.070	0.117
			(-0.86)	(-0.00)	(0.65)	(0.98)
TAX_REMINDER × PERSONAL_FILER					-0.199*	-0.199*
					(-1.73)	(-1.68)
Controls	No	Yes	No	Yes	No	Yes
N	301	281	301	281	301	281
Adj. R-sq.	0.032	0.058	0.032	0.054	0.043	0.066

Table 8: Summary Statistics for the Individual Investor Sample

The table reports descriptive statistics for the matched sample of U.S. retail investors. All variables are defined in Appendix I.

	Mean	Standard Deviation	N
DISPOSITION_EFFECT	0.020	0.282	12,926
PGR	0.061	0.213	12,926
PLR	0.042	0.194	12,926
$POST_{1994}$	0.500	0.500	12,926
TAX_REDUCE_STATE	0.500	0.500	12,926
AGE	47.800	19.332	12,926
AGE_SQ.	2,658.580	1,672.847	12,926
HOMEOWNER	0.783	0.412	12,926
INCOME	5.970	2.138	12,926
MALE	0.902	0.298	12,926
MARRIED	0.673	0.469	12,926

Table 9: State Tax Rate Reductions and Individual Investors' Trading Behavior

This table reports OLS regression estimates of the effects of state tax rate reductions on the disposition effect. In Panel A, the dependent variable is DISPOSITION_EFFECT, the proportion of gains realized less the proportion of losses realized. In Panel B, the dependent variable is PGR, the proportion of gains realized. The dependent variable in Panel C is PLR, the proportion of losses realized. $POST_{1994}$ equals 1 if the year is 1995, and 0 otherwise. TAX_REDUCE_STATE equals 1 if the investor resides in a state which reduced its tax rate, and 0 otherwise. The key explanatory variable is $POST_{1994}$ × TAX_REDUCE_STATE, which measures the change in the disposition effect among investors following the tax rate reductions. Controls in the regressions include the investor's AGE, AGE_SQ., HOMEOWNER, INCOME, MALE, and MARRIED. All variables are defined in Appendix I. We cluster standard errors at the state-level and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	Panel A: DISPOSITION_EFFECT			Panel B: PGR			Panel C: PLR		
	1	2	3	4	5	6	7	8	9
$POST_{1994}$ × TAX_REDUCE_STATE	-0.034***	-0.035***	-0.033***	-0.017**	-0.018**	-0.018**	0.017*	0.017*	0.015*
	(-3.09)	(-3.12)	(-2.90)	(-2.06)	(-2.13)	(-2.10)	(2.01)	(2.01)	(1.81)
$POST_{1994}$	0.036***	0.036***	0.036***	0.046***	0.046***	0.047***	0.010	0.010	0.012
	(3.20)	(3.22)	(3.11)	(5.57)	(5.63)	(5.66)	(1.21)	(1.25)	(1.42)
TAX_REDUCE_STATE	0.009	0.008		0.003	0.002		-0.006	-0.006	
	(1.53)	(1.25)		(0.48)	(0.31)		(-0.81)	(-0.76)	
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes	No	No	Yes
N	12,926	12,926	12,926	12,926	12,926	12,926	12,926	12,926	12,926
Adj. R-sq.	0.012	0.013	0.016	0.026	0.026	0.030	0.021	0.021	0.027

Table 10: Effects of Tax Reductions on the Disposition Effect Using a Linear Probability Model

This table reports estimates of the effects of tax rate reductions on the disposition effect using a linear probability model. The dependent variable, SALE, equals 1 if the investor sells the stock in the month, and 0 otherwise. GAIN equals 1 if the price is higher than the investor's purchase price, and 0 otherwise. $POST_{1994}$ equals 1 if the year is 1995, and 0 otherwise. TAX_REDUCE_STATE equals 1 if the investor resides in a state which reduced its tax rate, and 0 otherwise. The key explanatory variable is GAIN $\times$ $POST_{1994}$ $\times$ TAX_REDUCE_STATE, which measures the change in the disposition effect among investors following the tax rate reduction. Controls in the regressions include the investor's AGE, AGE_SQ., HOMEOWNER, INCOME, MALE, and MARRIED. All variables are defined in Appendix I. We cluster standard errors at the state-level and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3
GAIN $\times$ $POST_{1994}$ $\times$ TAX_REDUCE_STATE	-0.018*	-0.024*	-0.026**
	(-1.69)	(-1.87)	(-2.03)
GAIN $\times$ $POST_{1994}$	0.039***	0.042***	0.044***
	(3.65)	(3.38)	(3.47)
GAIN $\times$ TAX_REDUCE_STATE	0.013	0.012	0.013
	(1.08)	(1.01)	(1.08)
$POST_{1994}$ $\times$ TAX_REDUCE_STATE	0.013	0.018**	0.019**
	(1.58)	(2.04)	(2.04)
GAIN	0.034***	0.035***	0.035***
	(2.91)	(2.94)	(2.78)
$POST_{1994}$	0.042***	0.113***	0.113***
	(5.07)	(11.07)	(10.61)
TAX_REDUCE_STATE	-0.011	-0.011	
	(-1.16)	(-1.18)	
Controls	No	Yes	Yes
Year FE	No	Yes	Yes
State FE	No	No	Yes
N	31,771	31,771	31,771
Adj. R-sq.	0.030	0.043	0.043

Table 11: Excluding Firms in the Treatment States

This table reports OLS regression estimates of the effects of tax rate reductions on the disposition effect. We exclude portfolio holdings in firms with headquarters in New York, Michigan, and Delaware because these states have a change in capital gains tax rates during our sample. In Panel A, the dependent variable is DISPOSITION_EFFECT, the proportion of gains realized less the proportion of losses realized. In Panel B, the dependent variable is PGR, the proportion of gains realized. The dependent variable in Panel C is PLR, the proportion of losses realized. $POST_{1994}$ equals 1 if the year is 1995, and 0 otherwise. TAX_REDUCE_STATE equals 1 if the investor resides in a state which reduced its tax rate, and 0 otherwise. The key explanatory variable is $POST_{1994}$ × TAX_REDUCE_STATE, which measures the change in the disposition effect among investors following the tax rate reductions. Controls in the regressions include the investor's AGE, AGE_SQ., HOMEOWNER, INCOME, MALE, and MARRIED. All variables are defined in Appendix I. We cluster standard errors at the state-level and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	Panel A: DISPOSITION_EFFECT			Panel B: PGR			Panel C: PLR		
	1	2	3	4	5	6	7	8	9
$POST_{1994}$ × TAX_REDUCE_STATE	-0.042***	-0.042***	-0.042***	-0.022**	-0.023***	-0.023***	0.020*	0.019*	0.019*
	(-3.19)	(-3.22)	(-3.13)	(-2.65)	(-2.71)	(-2.75)	(2.00)	(1.99)	(1.94)
$POST_{1994}$	0.037***	0.037***	0.038***	0.047***	0.047***	0.048***	0.009	0.009	0.011
	(2.85)	(2.84)	(2.81)	(5.47)	(5.52)	(5.66)	(1.01)	(1.03)	(1.12)
TAX_REDUCE_STATE	0.009	0.008		0.002	0.001		-0.007	-0.007	
	(1.40)	(1.17)		(0.37)	(0.22)		(-1.01)	(-0.97)	
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes	No	No	Yes
N	11,720	11,720	11,720	11,720	11,720	11,720	11,720	11,720	11,720
Adj. R-sq.	0.016	0.016	0.019	0.030	0.030	0.035	0.024	0.024	0.029

Appendix I: Variable Definitions

This table describes the important variables we include in our analysis.

Panel A: Experimental Variables	
Key Variables	Definition
DISPOSITION_EFFECT	The propensity to realize gains versus losses. Calculated as PGR less PLR.
GAIN	Indicator that is 1 if the stock had a gain in the trial, 0 otherwise.
PGR	Participant's number of realized gains scaled by the total number of winning portfolio holdings.
PLR	Participant's number of realized losses scaled by the total number of losing portfolio holdings.
SALE	Equal to 1 if the participant sold the stock during the trial, 0 otherwise.
TAX_SALIENCE	Equal to 1 if the participant is assigned to the High-salience (treatment) condition, and 0 otherwise.
TAX_REMINDER	Equal to 1 if the participant receives the tax reminder, and 0 otherwise.
TOTAL_TRADES	Participant's total number of buys and sells during the experiment.
Explanatory Variables	**Definition**
AGE	Age of the participant. Categorical variable: 1. 18 - 20; 2. 21 - 25; 3. 26 - 30; 4. 31 - 35; 5. 36 - 40; 6. 41 - 45; 7. 46 - 50; 8. 51 - 55; 9. 56 - 60; 10. 61 - 65; 11. Above 65 years old.
ATTENTION_CHECK	Equal to 1 if the participant correctly answers the attention check question: "Please select Asia from the list," and 0 otherwise.
ECONOMIC_OUTLOOK	Response on a 1 (much worse) to 7 (much better) Likert scale to "Five years from now, my household's economic status will be:"
EDUCATION	Highest level of education attained. Categorical variable: 1. Some high school; 2. High school graduate; 3. Some college; 4. Undergraduate degree; 5. Professional degree; 6. Master's degree; 7. Doctoral degree.
EMPLOYMENT_STATUS	Current employment status. Categorical variable: 0. Retired; 1. Disabled; 2. Student; 3. Homemaker; 4. Unemployed and not looking for work; 5. Unemployed and looking for work; 6. Employed part time; 7. Employed full time.
FINANCIAL_LITERACY	Participant's score, from 0 (low literacy) to 3 (high literacy), on the three literacy questions in Lusardi and Mitchell (2008, 2011).
GENDER	Categorical variable equal to 0 if the participant is female, 1 if other, and 2 if male.
INCOME	Natural log of the participant's income.
INVESTOR	Equal to 1 if the participant invests in the stock market, and 0 otherwise.
OPTIMISM	Response on a 1 (strongly disagree) to 7 (strongly agree) Likert scale to: "I am optimistic about my future."
PERSONAL_FILER	Equal to 1 if the participant personally files his/her taxes, 0 otherwise.
POLITICS	Participant's political affiliation. Categorical variable: 1. Democrat; 2. Independent; 3. Other; 4. Prefer not to answer; 5. Republican.
RACE	Race of the participant. Categorical variable: 1. American Indian or Alaskan Native; 2. Asian; 3. African American; 4. Latino or Hispanic; 5. Native Hawaiian or Pacific Islander; 6. Other; 7. White.
RACIAL_MINORITY	Indicator equal to 1 if the participant is non-White, 0 otherwise.
RISK_AVERSION	An index composed of the gambling and investing risk assessment questions from Weber, Blais, and Betz (2002).

Variable Definitions – *Continued*

TASK_DURATION	Time, in seconds, taken to complete the experiment.
TRUST_MARKET	Response on a 1 (strongly disagree) to 7 (strongly agree) Likert scale to the question: "Are you confident that the stock market is fair and that you will not be cheated when investing?"

Panel B: Empirical Variables

Key Variables	Definition
DISPOSITION_EFFECT	The propensity to realize gains versus losses. Calculated as PGR less PLR.
PGR	Participant's number of realized gains scaled by the total number of winning portfolio holdings.
PLR	Participant's number of realized losses scaled by the total number of losing portfolio holdings.
GAIN	Indicator that is 1 if the stock had a gain, and 0 otherwise.
$POST_{1994}$	Indicator equal to 1 if the year is 1995, and 0 otherwise.
SALE	Equal to 1 if the participant sold the stock in the month, and 0 otherwise.
TAX_RATE	State-level tax rates applied to investment gains and losses.
TAX_REDUCE_STATE	Equal to 1 if the investor lives in New York, Michigan, or is unmarried and lives in Delaware, 0 otherwise.

Explanatory Variables	Definition
AGE	Investor's age in years.
AGE_SQ.	Square of the investor's age.
DECEMBER	Equal to 1 if the investor is a homeowner, and 0 otherwise.
DIVIDEND_PAYER	Equal to 1 if the firm pays a cash dividend, and 0 otherwise.
HIGH_INCOME	Equals 1 if the investor earns about $87,500 or more per year, and 0 otherwise.
HOMEOWNER	Indicator equal to 1 if the investor owns a home, and 0 otherwise.
INCOME	Investor's income. Categorical variable denoted by midpoints: 1. $7,500; 2. $17,500; 3. $25,000; 4. $35,000; 5. $45,000; 6. $62,500; 7. $87,500; 8. $112,500; 9. $250,000.
MALE	Indicator equal to 1 if the investor is male, and 0 otherwise.
MARRIED	Indicator equal to 1 if the individual is married, and 0 otherwise.
RETIREMENT	Equal to 1 if the investor is at least 65 years of age, and 0 otherwise.

Internet Appendix

Experiment Instructions

You will be given 350 experimental dollars to invest in three different stocks. Your job is to choose when to buy and sell each stock, so that you earn the most after-tax money by the end of the experiment. Throughout the experiment, you will see the stock prices change and you can use this information to decide when to buy and sell. When you sell a stock, you receive cash that is net of the trade's tax implications. If you sell a stock that has increased in value, a capital gains tax of 15% will be charged. If you sell a stock that has declined in value, the cash loss will be reduced by 15%.

You will start the experiment with 1 share of Stock A, 1 share of Stock B, and 1 share of Stock C. Each share is worth $100. You will also start with $50 in cash. For the remainder of the experiment, you are only allowed to hold either 1 share or 0 shares of each stock, and the rest of your portfolio is held in cash. The cash balance can be positive or negative. Either way, the cash balance earns a 0% return.

Structure of the Market

The experiment will begin by showing you information about the price history for Stock A, Stock B, and Stock C over the past nine periods. Then, you will have nine trading sessions where you decide whether to buy or sell one of the three stocks.

In each trading session, you will be given a price update for either Stock A, Stock B, or Stock C. One of the three stocks will be randomly selected and you will see if the selected stock price has gone up or down, and by how much.

Then, you will be asked whether you would like to trade the stock and you have to answer "yes" or "no." You will see whether you currently own 1 or 0 shares of the stock. If you choose "yes" and you own 1 share, you will sell it. If you choose "yes" and you own 0 shares, you will buy 1 share. If you choose "no," then you will keep your current position of 0 or 1 shares.

How Stock Prices Change

The prices of Stock A, Stock B, and Stock C all change over time according to the same rule. At any time, each stock is either in a "good state" or a "bad state." A stock in the good state has a 70% chance of going up and a 30% chance of going down in the next period. A stock in the bad state has a 30% chance of going up and a 70% chance of going down in the next period. In either state, the size of the stock price change is equally likely to be \$5, \$10, or \$15. After each time period, there is a 20% chance that the stock switches state.

Table IA1: Experimental Evidence on Tax Salience and Portfolio Concentration

This table reports OLS regression estimates on the relationship between tax salience and portfolio concentration among our experimental participants. The dependent variable is the experimental subject's number of unique stock holdings at the conclusion of the experiment. The key explanatory variable of interest is TAX_SALIENCE, an indicator variable that equals 1 if the participant is randomly assigned to the High-salience (treatment) condition, and 0 otherwise. The other variables are defined in Appendix I. We estimate White (1980) heteroskedasticity-robust standard errors and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4	5
TAX_SALIENCE	0.130*	0.132*	0.130*	0.125*	0.127**
	(1.90)	(1.94)	(1.92)	(1.85)	(1.97)
AGE		0.022	0.018	0.021	0.022
		(1.29)	(1.05)	(1.21)	(1.32)
EDUCATION		0.024	0.023	0.020	0.018
		(0.96)	(0.90)	(0.82)	(0.78)
INCOME		-0.042	-0.028	-0.007	0.005
		(-0.98)	(-0.57)	(-0.13)	(0.09)
GENDER		-0.034	-0.037	-0.041	-0.033
		(-0.99)	(-1.04)	(-1.14)	(-0.96)
RACE		0.004	0.002	0.005	0.008
		(0.28)	(0.12)	(0.30)	(0.53)
RISK_AVERSION		0.023***	0.019**	0.019**	0.020**
		(3.16)	(2.24)	(2.36)	(2.57)
FINANCIAL_LITERACY			0.047	0.047	0.046
			(1.12)	(1.13)	(1.13)
TRUST_MARKET			-0.004	0.007	0.003
			(-0.16)	(0.31)	(0.12)
INVESTOR			0.007	0.031	0.034
			(0.09)	(0.38)	(0.43)
EMPLOYMENT_STATUS			-0.016	-0.015	-0.008
			(-0.54)	(-0.51)	(-0.30)
OPTIMISM				-0.085***	-0.079***
				(-3.31)	(-3.13)
ECONOMIC_OUTLOOK				0.065*	0.057*
				(1.81)	(1.66)
POLITICS				-0.013	-0.020
				(-0.63)	(-0.96)
TASK_DURATION					-0.000
					(-0.24)
ATTENTION_CHECK					-0.025
					(-0.29)
N	699	699	699	699	699
Adj. R-sq.	0.004	0.025	0.022	0.033	0.029

Table IA2: Interactive Effects of Tax Salience on the Disposition Effect

This table reports OLS regression estimates of the effects of tax salience on the disposition effect for our experimental subjects. The dependent variable is the subject's, DISPOSITION_EFFECT, calculated as the proportion of gains realized less the proportion of losses realized. TAX_SALIENCE is an indicator variable that equals 1 if the participant is randomly assigned to the High-salience (treatment) condition, and 0 otherwise. The other variables are defined in Appendix I. We estimate White (1980) heteroskedasticity-robust standard errors and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4
TAX_SALIENCE	-0.078**	-0.159**	-0.123**	-0.094***
	(-2.39)	(-2.50)	(-2.47)	(-2.66)
TAX_SALIENCE × TRUST_MARKET	-0.008			
	(-0.40)			
TAX_SALIENCE × FINANCIAL_LITERACY		0.035		
		(1.27)		
TAX_SALIENCE × INVESTOR			0.067	
			(1.06)	
TAX_SALIENCE × RACIAL_MINORITY				0.055
				(0.79)
RACIAL_MINORITY				-0.044
				(-0.88)
TRUST_MARKET	0.002	-0.002	-0.002	-0.002
	(0.14)	(-0.14)	(-0.14)	(-0.15)
FINANCIAL_LITERACY	-0.033*	-0.051**	-0.034*	-0.034*
	(-1.84)	(-2.22)	(-1.90)	(-1.88)
INVESTOR	-0.021	-0.022	-0.054	-0.021
	(-0.62)	(-0.65)	(-1.14)	(-0.61)
Controls	Yes	Yes	Yes	Yes
N	699	699	699	699
Adj. R-sq.	0.087	0.088	0.088	0.088

Table IA3: Heterogeneous Trading Behavior in Response to the State Tax Rate Changes

This table reports OLS regression estimates of the effects of state tax rate reductions on the disposition effect. The dependent variable is DISPOSITION_EFFECT, the proportion of gains realized less the proportion of losses realized. $POST_{1994}$ equals 1 if the year is 1995, and 0 otherwise. TAX_REDUCE_STATE equals 1 if the investor resides in a state which reduced its tax rate, and 0 otherwise. Controls in the regressions include the investor's AGE, AGE_SQ., HOMEOWNER, INCOME, MALE, and MARRIED. All variables are defined in Appendix I. We cluster standard errors at the state-level and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4	5
$POST_{1994}$ × TAX_REDUCE_STATE	-0.030**	-0.021*	-0.035**	-0.033*	-0.012**
	(-2.46)	(-1.70)	(-2.18)	(-1.72)	(-2.18)
$POST_{1994}$ × TAX_REDUCE_STATE × DECEMBER	-0.053				
	(-0.90)				
$POST_{1994}$ × TAX_REDUCE_STATE × HIGH_INCOME		-0.035*			
		(-1.72)			
$POST_{1994}$ × TAX_REDUCE_STATE × RETIREMENT			0.011		
			(0.37)		
$POST_{1994}$ × TAX_REDUCE_STATE × MARRIED				-0.001	
				(-0.03)	
$POST_{1994}$ × TAX_REDUCE_STATE × HOMEOWNER					0.004
					(0.41)
DECEMBER	-0.069**				
	(-2.26)				
HIGH_INCOME		-0.078			
		(-1.52)			
RETIREMENT			-0.059*		
			(-1.85)		
MARRIED				0.026	
				(0.72)	
HOMEOWNER					0.018***
					(3.02)
$POST_{1994}$	0.034***	0.032***	0.035**	0.040***	0.006
	(2.88)	(3.54)	(2.58)	(2.64)	(1.13)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
N	12,926	12,926	12,926	12,926	12926
Adj. R-sq.	0.020	0.021	0.016	0.015	0.004

Table IA4: Tax Rate Reductions and Trading in Dividend-Paying Stocks

This table reports OLS regression estimates for the impact of an investor's state tax rate reductions on the propensity to trade stocks that pay dividends. The dependent variable, SALE, equals 1 if the investor sells the stock in the month, and 0 otherwise. GAIN equals 1 if the price appreciated during the month, and 0 otherwise. $POST_{1994}$ equals 1 if the year is 1995, and 0 otherwise. TAX_REDUCE_STATE equals 1 if the investor resides in a state which reduces its tax rate, and 0 otherwise. DIVIDEND_PAYER equals 1 if the stock pays a dividend during the year, and 0 otherwise. Controls in the regressions include the investor's AGE, AGE_SQ., HOMEOWNER, INCOME, MALE, and MARRIED. For parsimony, we suppress estimates on the ancillary indicators and interaction terms. All variables are defined in Appendix I. We cluster standard errors at the state-level and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3
$POST_{1994}$ × TAX_REDUCE_STATE × DIVIDEND_PAYER	-0.016	-0.015	-0.022
	(-1.17)	(-1.09)	(-1.19)
GAIN		0.059***	0.037***
		(14.45)	(3.02)
GAIN × $POST_{1994}$ × TAX_REDUCE_STATE			-0.035*
			(-1.73)
GAIN × $POST_{1994}$ × TAX_REDUCE_STATE × DIVIDEND_PAYER			0.019
			(0.67)
DIVIDEND_PAYER	-0.003	-0.004	-0.002
	(-0.63)	(-0.71)	(-0.18)
$POST_{1994}$	0.133***	0.127***	0.110***
	(9.76)	(9.72)	(10.54)
TAX_REDUCE_STATE	0.120***	0.145***	0.144
	(5.76)	(7.16)	(1.54)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
N	31,771	31,771	31,771
Adj. R-sq.	0.035	0.043	0.044

Table IA5: Summary Statistics for the Full Sample of Individual Investors

This table reports descriptive statistics for the full sample of U.S. retail investors. The sample period is from 1991 through 1996. All variables are defined in Appendix I.

	Mean	Standard Deviation	N
DISPOSITION_EFFECT	0.020	0.277	209,018
PGR	0.064	0.223	209,018
PLR	0.044	0.187	209,018
$POST_{1994}$	0.185	0.388	209,018
TAX_REDUCE_STATE	0.085	0.279	209,018
AGE	47.716	18.415	209,018
AGE_SQ.	2,616.021	1,601.690	209,018
HOMEOWNER	0.884	0.321	209,018
INCOME	6.188	2.001	209,018
MALE	0.906	0.292	209,018
MARRIED	0.723	0.447	209,018

Table IA6: State Tax Reductions and Individual Investor Trading Behavior in the Full Sample

This table reports OLS regression estimates of the effects of tax rate reductions on the disposition effect. Columns 1 through 3 show estimates from the full sample. For reference, Column 4 reports estimates from the matched sample. The dependent variable is DISPOSITION_EFFECT, the proportion of gains realized less the proportion of losses realized. AGE_SQ.$_{1994}$ equals 1 if the year is 1995, and 0 otherwise. TAX_REDUCE_STATE equals 1 if the investor resides in a state that reduces its tax rate, and 0 otherwise. The key explanatory variable is POST$_{1994}$ × TAX_REDUCE_STATE, which measures the change in the disposition effect among investors following the tax rate reductions. Controls in the regressions include the investor's AGE, AGE_SQ., HOMEOWNER, INCOME, MALE, and MARRIED. All variables are defined in Appendix I. We cluster standard errors at the state-level and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4 Matched Sample
POST$_{1994}$ × TAX_REDUCE_STATE	-0.011**	-0.011***	-0.011***	-0.033***
	(-2.60)	(-2.68)	(-2.76)	(-2.90)
POST$_{1994}$	0.009**	-0.002	-0.002	0.036***
	(2.56)	(-0.67)	(-0.77)	(3.11)
TAX_REDUCE_STATE	-0.001	-0.001	0.031	
	(-1.00)	(-0.72)	(0.72)	
Controls	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes
State FE	No	No	Yes	Yes
N	209,018	209,018	209,018	12,926
Adj. R-sq.	0.000	0.001	0.001	0.016

Table IA7: State Tax Rates and the Disposition Effect

This table reports OLS regression estimates for the impact of tax rates on the disposition effect. The dependent variable is DISPOSITION_EFFECT, the proportion of gains realized less the proportion of losses realized. The key explanatory variable is TAX_RATE, the state-level tax rate applied to investment gains and losses. All variables are defined in Appendix I. We cluster standard errors at the state-level and $t-$statistics are in parentheses. The *, **, and *** symbols indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3
TAX_RATE	-0.004	0.001	0.047
	(-0.11)	(0.02)	(1.04)
AGE		-0.000	-0.000
		(-0.08)	(-0.18)
AGE_SQ.		-0.000*	-0.000
		(-1.83)	(-1.66)
INCOME		-0.001***	-0.001***
		(-3.06)	(-3.29)
HOMEOWNER		0.005**	0.005**
		(2.53)	(2.28)
MALE		0.002	0.002
		(0.77)	(0.89)
MARRIED		0.001	0.001
		(0.76)	(0.71)
Year FE	No	Yes	Yes
State FE	No	No	Yes
N	230,866	230,866	230,866
Adj. R-sq.	-0.000	0.002	0.002