

Consumption out of Fictitious Capital Gains and Selective Inattention*

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Abstract

Do retail investors' behavioral biases in trading directly affect their consumption out of stock market wealth? We exploit a natural experiment that changed the displayed purchase prices in investors' online portfolios. Investors are more likely to sell and consume on average 25% of "fictitious" capital gains, i.e., displayed capital gains under the new purchase prices that are capital losses under the actual purchase prices. We argue that investors are selectively inattentive: they are more responsive when fictitious gains are larger and actual losses are smaller, they notice fictitious losses, and they react even when actual purchase prices are very salient.

JEL codes: G5, D90, G41, D14

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1 Introduction

The existing literature documents that individual investors are selectively inattentive (Karlsson et al., 2009; Golman et al., 2016; Olafsson and Pagel, 2018b; Andries and Haddad, 2020; Quispe-Torreblanca et al., 2020), i.e., they prefer to pay attention to capital gains. Additionally, investors are subject to the so-called disposition effect (see Odean, 1998; Kaustia, 2010; Frydman and Rangel, 2014; Birru, 2015; Chang et al., 2016; Meng and Weng, 2017; Frydman and Wang, 2019), i.e., they tend to sell winning stocks and hold on to losing stocks. Two recent papers discuss how these two behavioral biases interact (Gathergood et al., 2019; Birru et al., 2019). In this paper, we ask whether these two biases and their interaction effects are constrained to the domain of individuals trading in their brokerage accounts or whether they also affect individual consumption, a direct component of utility and welfare. Are individuals selectively inattentive to capital gains and sell them to consume more? The answer is yes, which implies that behavioral biases in trading are determinants of consumption out of stock market wealth. This is important, as stock market wealth accounts for roughly one-quarter of household net worth, and consumption is the largest component of GDP (Poterba, 2000; Di Maggio et al., 2020).

We use individual-level data from a German online retail bank. The data contain information on all security trades and holdings as well as all transactions and balances in the settlement, savings, and checking accounts of more than 100,000 individuals from January 2003 to June 2018. When we look at capital gains and their effects on trading and consumption, we have to overcome an identification challenge: investors liquidate capital gains because they may have experienced a wealth or taste shock and want to consume more. To overcome this endogeneity, we need a treatment in which people are shown capital gains and losses exogenously even though their wealth and tastes do not change. Then, we can identify whether individuals selectively pay attention to and have a disposition of realizing capital gains and whether that affects consumption.

To achieve this setting, we exploit a natural experiment: the implementation of the 2018 capital gains tax reform. On 1/1/2018, our bank, as well as the majority of other German banks, changed the displayed purchase prices of all funds and ETFs (but no other investments) in individuals' online portfolios to the 12/29/2017 closing price, as quoted by the exchange. In turn, this newly displayed purchase price determined the displayed absolute capital gains and losses as well as their displayed percentage returns. Importantly, the reform did not have any real implications for individual tax liabilities. The reform did not change the actual purchase prices or cost basis of any investments or the capital gains tax rates.

The change of displayed purchase prices created what we call “fictitious winners and losers.” The investor sees a fictitious winner if he or she is shown a capital gain relative to the newly displayed purchase price, but in reality, he or she is subject to a capital loss. The investor sees a fictitious loser if he or she is shown a capital loss relative to the newly displayed purchase price, but he or

she is subject to an actual capital gain. Investors are informed about their actual capital gains or losses in their order summaries, sales receipts, and bank communications. Therefore, we would expect that trading and consumption respond only to the total amount of liquidations an investor initiates and to his or her actual capital gains, i.e., the consumption plan of the investor and his or her wealth. We would not expect that trading and consumption respond to the fictitious capital gain or loss of that investment.

We estimate the effects of the changes in the displayed purchase prices and capital gains or losses on trades using a linear probability model, as is standard in the disposition-effect literature. We find that individuals are affected by the change in the displayed purchase price when it results in a fictitious capital gain but not a fictitious loss: 1) individuals' likelihood of selling a fund is 4.09% to 5.06% higher when funds are displayed as what we term fictitious winners, even though these funds are losers based on the actual purchase prices; and 2) individuals' likelihood of selling a fund is barely changed when funds are displayed as what we term fictitious losers, i.e., individuals treat them almost the same as actual winners.¹ The propensity to sell fictitiously winning funds is large and positive in all sample splits and robustness checks we perform and is of similar magnitude as the disposition to sell winning stocks over the whole sample period. Furthermore, it is considerably larger than the propensity to sell winning funds over the whole sample period (in line with the findings in [Chang et al., 2016](#)).² Our findings can be explained by the interaction between selective inattention, the disposition effect, and attribution error (as in [Barber and Odean, 2002](#)), i.e., individuals are looking for a reason to sell losing funds when they are fictitiously displayed as winners.

We then implement a two-stage instrumental-variable (IV) approach to estimate the effect of realizing fictitious winners on consumption. In the first stage, we use individuals' propensities to sell fictitious winners because the fictitious capital gain is plausibly exogenous: it only depends on the 12/29/2017 closing price and the current price, both cannot be controlled by investors. In addition, we control for month-by-year fixed effects to account for fluctuations in all aggregate variables such as stock market performance or consumer confidence. In the second stage, we use fictitious winners as an instrument to estimate how much individuals consume out of realizing fictitious capital gains.

We estimate the marginal propensity to consume (MPC) out of each dollar of liquidated fictitious capital gains, controlling for the total value of any liquidations and their actual capital gains.³ We

¹Depending on the specification, the point estimate for fictitious losers ranges from 0.0000149% to -0.82% with a standard error of 0.02%. Our effects are very tightly estimated as we have a large sample of retail investors making a number of trades in 2018 and considerable volatility in the prices of individuals' funds, constantly affecting their categorizations as actual or fictitious winners and losers.

²Over the whole sample period, our rolling-window disposition effect regressions (see Subsection 5.1 Figure 6) show that the disposition effect for funds is sometimes positive, sometimes negative, countercyclical, and time-varying closely with the disposition effect for all securities. We argue that this correlation suggests that the disposition effects in stocks and funds are driven by psychological mechanisms with common components.

³It is key to control for the total amount of the liquidation and actual capital gains. To see this, suppose an investor wants to buy a sofa costing 200 euros, then he or she would liquidate 200 euros and the actual amount

find that individuals consume approximately 15.7% of each euro of realized fictitious capital gains when we measure consumption by ATM withdrawals and point-of-sale transactions. Individuals consume 24.5% of each euro of realized fictitious capital gains when we also include nonrecurring domestic wires in our measure of consumption. When we look at the subsample of main customers, who are likely to only have this banking relationship, the effects increase to 29.9% and 49.0%, respectively. In contrast, the consumption effects of fictitious capital losses are tightly estimated zeroes.

In summary, our study is unique because we have an exogenous change in displayed purchase prices that serves as a first stage for selling a fund and liquidating a capital gain or loss. Our experiment directly provides situations in which individuals observe fictitious capital gains even though they are actually subject to capital losses. Because we control for all realized and paper capital gains and losses, our setup ensures that individual wealth and tastes are held constant. Selling a fictitious winner versus a loser should not affect their consumption. However, we find that individuals consume distinctly more after liquidating a fictitious capital gain, whereas they seem to notice and do not respond to fictitious capital losses.

We further analyze three potential explanations for our findings: 1) individuals are simply confused about their actual capital gains, 2) individuals are selectively inattentive to their actual capital gains, and 3) individuals are driven or confused by tax considerations related to the reform. In summary, we find multiple pieces of evidence for 2) that are not fully consistent with 1) but little reason to believe that 3) plays a major role.

We first document four findings to argue that explanation 2), selective inattention, is a more relevant mechanism than explanation 1), plain confusion. First, individuals should be fully informed about their actual capital gains because they receive a sales receipt by email and postal mail that clearly states the fictitious as well as actual capital gains or losses. This receipt is received immediately after the sale and therefore prior to the consumption response, which we document at the daily level. Second, we estimate tight zero effects of fictitious capital losses, suggesting that individuals are selectively inattentive about fictitious capital gains but notice fictitious capital losses as they treat them similar to actual winners. Third, we show that the individual propensity to sell fictitious winners is monotonically increasing in the fictitious (displayed) gain and monotonically decreasing in the actual loss (but we do not detect any pattern for fictitious losses). As an example, individuals respond most strongly when the fictitious capital gain is very large but the actual capital loss is small. We argue that these two results are consistent with individuals being more selectively inattentive when that is easier for them. Fourth, we attempt to assess how much individual inattention

of fictitious capital gains that are shown to him or her (in the online brokerage) is just some random number that does not affect how big a sofa he or she buys. However, we find that the fictitious capital gain is reflected in the amount spent (controlling for the total amount that individuals liquidate and their actual capital gains). Therefore, it increases spending when investors liquidate a fictitious winner as opposed to a loser, even though the fictitious winner or loser status is irrelevant for their wealth.

to their actual capital gains matters by analyzing several sample splits based on the salience and ready availability of the information on the fictitious capital gains. We find stronger effects for both trading and consumption when we examine investors' trades before a pop-up window in their online portfolios, providing additional information about fictitious purchase prices.⁴ We also find that our effects are stronger for infrequent and inattentive traders. However, the effects on trades and consumption are significant before and after the additional provided information and for all types of investors. We thus argue that investors are not only confused but choose to be selectively inattentive to their actual capital gains, which affects how rich they feel and how much money they decide to spend.

In turn, we shed additional light on explanation 3), tax considerations or confusion about the tax implications. In principle, if individuals think their cost bases are determined by the newly displayed purchase prices, they should not sell fictitious winners (as taxes are only levied if a capital gain is actually realized). On the other hand, fictitious winners are actual losers, and thus, individuals may want to sell them to offset other capital gains. To address these considerations, we perform sample splits to single out individuals who are not affected by taxes. Specifically, we single out individuals with small portfolios whose capital gains are below the annual tax-free allowances. Here, we do not find evidence that our effect is stronger for individuals more or less affected by taxes and thus argue that (confused) tax considerations are not the driving force behind our results.

2 Literature review

Our paper is related to three strands of literature. First, we contribute to the literature showing that retail investors are selectively inattentive (Karlsson et al., 2009; Golman et al., 2016; Olafsson and Pagel, 2018b; Andries and Haddad, 2020; Quispe-Torreblanca et al., 2020). We document that such inattention has real consequences in terms of affecting consumption as opposed to being restricted to the domain of logins to brokerage accounts and potential trades. We thus show that selective inattention directly affects individual utility and welfare beyond it affecting welfare through its effects on wealth (Gargano and Rossi, 2018) or through information-dependent utility (Quispe-Torreblanca et al., 2020).

Second, we contribute to the large literature on the disposition effect. The initial finding by Shefrin and Statman (1985) and Odean (1998) was further analyzed in a number of follow-up papers such as Chang et al. (2016), Hartzmark (2015), Heimer (2016), and Imas (2016), among many others. Our findings are most closely related to a few recent papers on the disposition

⁴The pop-up window started showing on March 1st right after individuals logged in and simply stated that the implementation of the capital gains tax reform resulted in newly displayed purchase prices for all fund holdings on 01/01/2018 and, as a result, different capital returns. This information was previously provided to investors in an information email in November 2017. Additionally, in March 2020, individuals were informed via email that they were provided with an overview of fictitious and actual purchase prices and capital gains.

effect. [Frydman and Wang \(2019\)](#) analyze a change in the salience of the displayed purchase price using a natural experiment in which the online broker added price variables and color-coded gains and losses to make them visually more apparent to investors. [Frydman and Rangel \(2014\)](#) use a laboratory experiment to demonstrate the effects of displaying or omitting individual purchase prices. Our findings on trades are also in line with [Birru \(2015\)](#), who finds that retail investors confuse winning and losing stocks after stock splits rather than properly adjusting their purchase price points. Finally, the findings in [Frydman et al. \(2015\)](#) suggest that individuals are happy to take a fictitious winner (unlike a loser) at face value and close that mental account. The specific interaction of inattention and the disposition effect is also analyzed in the two contemporaneous papers by [Gathergood et al. \(2019\)](#) and [Birru et al. \(2019\)](#). However, none of the aforementioned papers study a field setting in which the displayed purchase prices are exogenously changed and to the best of our knowledge, no existing research paper has been able to look at how the disposition effect affects consumption.

Our findings help to understand the mechanisms and psychologies behind the disposition effect. We have a natural experiment in which displayed purchase prices are changed exogenously, and we find that the displayed purchase prices appear to partly determine a new “reference point” for investors, i.e., the focal point relative to which investments are classified as winners versus losers in theories of reference dependence that explain the disposition effect ([Shefrin and Statman, 1985](#); [Barberis and Xiong, 2009](#); [Meng and Weng, 2017](#); [Barberis and Xiong, 2012](#); [Baillon et al., 2020](#)) or stock market non-participation and low equity shares ([Barberis et al., 2001, 2006](#); [Pagel, 2018](#)). Our findings indicate that the act of selling a fictitious winner makes individuals feel richer and consume, whereas fictitious losers do not elicit a reaction. Interestingly, the fictitious winners thus allow individuals to be selectively inattentive and sell losers, overcoming their disposition. It thus seems that selective inattention overcomes the disposition effect which is consistent with attribution error (as in [Barber and Odean, 2002](#)).

Third, we contribute to the literature on consumption out of stock market wealth. The literature linking stock market wealth with consumption includes studies employing aggregate and regional variation (e.g., [Davis et al. \(2001\)](#), [Dynan and Maki \(2001\)](#), and [Case et al. \(2005\)](#)).⁵ However, endogeneity concerns are likely to affect the interpretation of the estimates in these existing studies, as they use aggregate data and cannot distinguish between the direct effect of changes in stock wealth on consumption and the fact that stock prices are a leading indicator of economic growth and reflect consumer sentiment. There also exist studies employing household-level data, but they either lack disaggregated data on households’ actual stock holdings or on actual consumption (e.g., [Parker, 1999a](#) and [Baker et al., 2007](#)). Specifically, [Baker et al. \(2007\)](#) use CEX data and show that stockholder consumption responds strongly to changes in dividend payments but not to changes in stock prices. They also provide suggestive evidence that this behavior is driven

⁵See [Poterba \(2000\)](#) for a survey of the literature.

by mental accounting. Furthermore, [Di Maggio et al. \(2020\)](#) obtain the same findings using annual household consumption and asset holdings data from the Swedish wealth registry. We contribute to this literature by using high-frequency bank account data that links securities trades and holdings data with transaction-level spending and income data. In line with the literature, we show that mental accounting plays a significant role as single-position fictitious gains and losses shape consumption decisions. Additionally, we show that consumption out of stock market wealth is affected by behavioral biases in trading. Our large MPC estimates are in line with the MPCs out of wealth estimated in many macroeconomic models, whereas in aggregate data, this estimated MPC is too small ([Poterba, 2000](#)).

As in [Meyer et al. \(2018\)](#), we show that individuals have a high MPC out of capital gains once they are liquidated, which links our paper to the literature on consumption out of cash flows ([Parker, 1999b, 2014](#); [Agarwal et al., 2007](#); [Jappelli and Pistaferri, 2010](#); [Olafsson and Pagel, 2018a](#)) but looks at investment liquidations instead of temporary income shocks. In contrast to [Meyer et al. \(2018\)](#), who study exogenous liquidations due to mutual fund closures, we study a situation in which individuals are induced to sell by the fictitious winner status of their investments even though they did not actually experience a capital gain. We thus study a specific situation in which we can disentangle the effects of a fictitious versus an actual capital gain and can thus rule out any wealth effects that are present in the [Meyer et al. \(2018\)](#) setting.⁶

3 Data overview, institutional background, and summary statistics

3.1 Data overview

Our dataset stems from the clients of one of the largest online banks in Germany. We have daily information on logins (from 2012 onwards), trades, and portfolio holdings of approximately 103,000 customers as well as all balances and transactions of each investor’s other accounts at the online bank from 1999 to 2016. We retain only private investors who reside in Germany and obtain data on customer demographics such as gender, age, occupation, and zip code. For German online banks, there is usually no charge for having an account. To avoid analyzing accounts of investors who are inactive, we require that individuals execute at least one trade per year in 2017 and 2018. Additionally, we require that investors have at least one wire or ATM transaction in their

⁶In a separate related paper, [Meyer and Pagel \(2018\)](#) study the differential reinvestment response out of mutual fund liquidations that resulted in either a gain or loss with respect to the initial investment. The authors find that individuals reinvest considerably less in response to a loss. However, [Meyer et al. \(2018\)](#) do not find a significantly different response with respect to the consumption of liquidations that were either gains or losses, as the consumption data is more noisy than the reinvestment responses. Given that we consider fictitious rather than actual capital gains and study their effects on consumption, our paper relates to the literature on consumer sentiment ([Gillitzer and Prasad, 2018](#), among others, provide causal evidence that sentiment affects consumption).

checking accounts in 2017 and 2018. This leaves us with a sample of approximately 30,000 clients. Additionally, for our robustness checks, we further restrict the sample to individuals who bank flags are the main customers.

For each trade, we have the security international identification number (ISIN) and obtain additional information on the security, such as asset class, risk class, issuer or issue date, from Datastream. For each account transaction, we obtain a category from the bank's transaction system, such as salaries, interest payments, ATM withdrawals, wires, or point-of-sale spending transactions.

When investors make a trade or a position is liquidated, there is a transfer to the settlement account (Verrechnungskonto). The settlement account is dedicated to making trades and opened automatically when individuals open a portfolio. It pays some interest and is federally insured. In turn, individuals can transfer their funds into their checking or savings accounts. For consumption, we consider the following outcome variables: 1) ATM withdrawals plus points of sale transactions, i.e., individuals swipe their debit or credit card in a store or purchase goods or services online by entering their card information, and 2) ATM withdrawals plus points of sale transactions plus all domestic nonrecurring wire transfers that leave the bank (Lastschrift and Überweisungen). In Germany, wire transfers are likely to be consumed because they are commonly used to pay for online shopping or other services or to pay people. All the variables are transfers and thus flow variables.

3.2 Summary of the natural experiment

The online bank and all other major banks in Germany reset the displayed purchase prices on 1/1/2018 to implement the 2018 capital gains tax reform. This sale did not have any tax liability consequences that changed the actual capital gains taxes for our individuals, but it changed the displayed purchase prices and the relative and absolute returns in individuals' online portfolios. We start by describing the effects of this change in the displayed purchase prices in this subsection and then provide a detailed description of the capital gains tax reform in Subsection 3.3.

When individuals log in to their online brokerage, they see the purchase price, the current price, their total holdings, and their return since purchase. On 1/1/2018, the displayed purchase price was set equal to the 12/29/2017 closing price, as quoted by the exchange, and the return was set to 0%. Thereafter, the value developed as usual. Figure 1 shows screenshots of the online portfolio interface that individuals see. The portfolio interface shows all fund and stock holdings as well as their daily absolute changes and percentage changes, i.e., returns. In addition to the daily change, the interface also shows the absolute changes and percentage changes, i.e., returns, relative to the displayed purchase price of the security position. These returns are displayed immediately next to the buy and sell buttons. The latter portion was reset on 1/1/2018 to implement the capital gains

tax reform. Investors were informed via email in November 2017, and an online pop-up window appeared upon logging starting in March 2018. The online notification is also displayed in Figure 1. Throughout 2017 and 2018, the actual purchase prices can be seen by clicking on the order summary. As a default, the order summary screen displays the purchase prices of the three most recent transactions. For many automatic transactions from savings plans, for instance, individuals must dig deeper to find their actual average purchase prices.

[Insert Figure 1 about here]

Let us discuss an example for the four scenarios that the experiment creates: Figure 2 shows the price path of a Deutsche Bank (DWS) US equities mutual fund and two possible purchase prices (called $\textcircled{-2}$ and $\textcircled{-1}$) as well as two possible sale prices (denoted by $\textcircled{1}$ and $\textcircled{2}$). Purchasing the fund at $\textcircled{-1}$ and selling it at $\textcircled{2}$ creates a real loser or loss because the fund is trading at a loss with respect to its newly displayed purchase price and with respect to its actual purchase price. Purchasing the fund at $\textcircled{-2}$ and selling it at $\textcircled{2}$ creates a fictitious loser or loss because the fund is trading at a loss with respect to its newly displayed purchase price but at a gain with respect to its actual purchase price. Purchasing the fund at $\textcircled{-2}$ and selling it at $\textcircled{1}$ creates a real winner or gain because the fund is trading at a gain with respect to its newly displayed purchase price and with respect to its actual purchase price. Purchasing the fund at $\textcircled{-1}$ and selling it at $\textcircled{1}$ creates a fictitious winner or gain because the fund is trading at a loss with respect to its newly displayed purchase price but at a gain with respect to its actual purchase price.

[Insert Figure 2 about here]

3.3 Institutional background of the 2018 capital gains tax reform

The 2018 capital gains tax reform in Germany implemented changes that served procedural purposes within banks to equalize the tax assessment across domestic and foreign retaining funds. The goal was to ease calculations of tax liabilities for banks and tax authorities, as taxes on capital gains and dividends are taxed at the source. However, individual tax liabilities were not changed. Only the displayed purchase prices in individual online portfolios as well as the displayed capital gains and losses were changed.

Since January 1, 2009, private investors in Germany have owed capital gains taxes. Before January 2009, capital gains taxes were owed only if assets were liquidated within a year after purchase and in some other special cases. In contrast, dividends and interest payments were taxed at the personal income tax rate, which could amount to up to 42%. After January 2009, dividends, interest payments, and capital gains were all taxed at the same rate. The capital gains taxes of stocks and funds bought before January 1, 2009, (Altbestände) remain tax-free up to an initial allowance of 100,000 euros. However, any capital gains after 1/1/2018 that exceed 100,000 euros

will be taxed. For these stocks and funds, the 1/1/2018 price thus became the new cost basis. For stocks and funds bought after January 1, 2009, capital gains are taxed at the same rate as dividends and interest payments, and the tax is subtracted at the source, i.e., in the event of a capital gains realization, the money that arrives in clients' settlement accounts after a sale are already after-tax funds. This did not change with the 2018 capital gains tax reform. As of 2009, the capital gains tax was 25% (Abgeltungsteuer auf Kapitalerträge) plus the solidarity surcharge (Solidaritätszuschlag) (5.5% of the capital gains tax) and (if applicable) church tax (Kirchensteuer) (9% of the capital gains tax), which amounts to 26.375% to 28.625%. Furthermore, there is an annual tax-free allowance (Freibetrag) of 801 euros for singles and 1602 euros for married couples. Individuals can specify their main brokerage such that the capital gains tax will not be subtracted unless the initial allowance is exceeded (Freistellungsauftrag). Furthermore, if capital losses are realized before capital gains, then the capital gains tax will be automatically lowered by the realized losses. Thus, gains and losses are offset with losses exceeding gains either rolled forward or, upon request, certified to be considered in individual tax returns. At the brokerage level, the initial allowance and the initial allowance for funds bought before January 2009 are taken into account. One reason for the capital gains tax reform and the subsequent reset of the displayed purchase prices was to begin taxing all funds bought before 2009 (Altbestände) according to their value as of 1/1/2018. However, none of our investors were actually affected because the initial allowance is very high (100,000 euros for singles and 200,000 euros for couples).

The second reason for the reform was to simplify the treatment of retaining foreign funds. Previously, retained capital gains and dividends from foreign funds had to be reported separately by investors in their individual tax returns (while retaining domestic funds were taxed at the fund level on their retentions, and non-retaining domestic and foreign funds were taxed at the investor level). In Germany, two-thirds of funds held are domesticated in Luxembourg. All funds are now treated equally, and all tax assessments are made automatically without any reporting requirements for the investor. For all retaining funds, individuals now have to make an advance payment of the taxes on their retentions at the end of the tax year (Vorabpauschale).

To simplify the process of calculating this fraction of capital gains, taxes to be paid were one reason that the online bank and many other banks fictitiously bought and sold all fund holdings of all clients on 1/1/2018 and reset the displayed purchase price to the 12/29/2017 closing price, as quoted by the exchange. This constitutes a change in when taxes are paid for those individuals who hold foreign retaining funds but not their effective tax rate. However, 1) some individuals do not hold foreign retaining funds, 2) many of our individuals do not earn capital gains above the initial tax-free allowance (801 euros for singles and 1602 euros for married couples) and thus do not have to pay the advance payment (Vorabpauschale), and 3) the Vorabpauschale is very small. For example, for purely retaining equity funds in 2018, Vorabpauschale equals the German prime interest rate (of 0.87%) times 70% (i.e., the average equity share assumed by the authority) times

the 12/29/2017 closing price of the fund. In total, this is 60.9 basis points of the fund price or 36.54 euros for a 6,000-euro fund position. The first time that individuals had to make an advance payment (Vorabpauschale) was December 2019.

In summary, the change in the displayed purchase prices did not have any tax liability consequences in terms of changing the actual capital gains tax rate for our individuals (as no investor exceeds 100,000 euros in capital gains for their funds bought prior to 2009). A subset of individuals who hold foreign retaining funds and whose capital gains exceed the tax-free allowance of 801 euros or 1602 euros are subject to a change in when the retained capital gains tax is paid (December 2019 versus the time of income tax reporting in July 2020 for most investors).

An important question is whether individuals properly understand the tax consequences and reset of the displayed purchase price without changing the cost basis. This is very important for the interpretation of our results, and we discuss it in greater depth in Section 6.

In summary, individuals were provided with the correct information in three ways. First, after each sale, individuals receive a sales receipt that is shown in Figure 3. It shows the following on page 1: the funds description, ISIN, number of securities sold, current price, current value of the liquidation gross of taxes and fees, capital gains taxes, church taxes, solidary surcharge, and final liquidation value net of taxes and fees. On page 2, the receipt shows for funds (not stocks) the difference between the fictitious purchase value and sale value (the fictitious capital gains), actual capital gains (indicated as such and used as a tax basis), partial tax-free allowance of the retaining funds, actual capital gains on 1/1/2018, and accumulated retained capital gains since 1/1/2018; note that the fictitious plus actual capital gains minus the tax-free allowance plus the retained capital gains equals the overall capital gains. This fund is an actual winner: both fictitious and actual capital gains are positive at the time of sale. Second, individuals were informed about the change in the displayed purchase prices via email and mail in November 2017. Third, starting in March 2018, a pop-up window about fictitious purchase prices appeared after individuals logged in (displayed in Figure 1) and contained the same information as the November 2017 communication. Additionally, individuals received another email and mail and were provided with an overview of their fictitious and actual purchase prices as well as fictitious and actual capital gains and losses for all their existing positions.

3.4 Summary statistics

Table 1 shows the detailed summary statistics for our universe of investors for the years 2017 and 2018 as well as the same summary statistics for our subsample of affected investors, those that held a fund in 2018.

[Insert Table 1 about here]

Our sample of investors is unlikely to be representative of the German population as a whole; less

than half of Germans invest in stocks, either directly or indirectly. However, our sample is arguably representative of self-directed retail investors in Germany and, more generally, of individuals in Germany holding an investment portfolio with a major bank. The average age of our investors is 48, and 81% is male. Brokerage clients are generally expected (Cole et al., 2014) and are found to be more sophisticated than the overall population (Dorn and Huberman, 2005). The same is true for our sample: 6% of our investors hold a doctoral degree, which is higher than average in the German population (1.1%, German Federal Bureau of Statistics, 2008).

Investors' portfolios are worth a median of 45,037 euros in 2017 and 2018. This figure and other descriptive statistics are comparable to those reported by household finance studies using US brokerage account data (e.g., Odean, 1998). We also have a measure of self-reported wealth in our data. This measure is surveyed in brackets by the bank upon account opening (we use the midpoint of each range as the reported value of investor wealth). Given that it is a self-reported measure of wealth, it should be interpreted with caution, as individuals may not include other assets such as life insurance or real estate in this number but report only their liquid savings. This is consistent with the fact that the mean and the percentile distribution of the self-reported wealth measure are less than the average portfolio values we observe. Additionally, stock market wealth grows after account opening, and individuals have been with this bank for 13 years, on average. Nevertheless, the self-reported wealth measure reassures us that most investors hold most of their liquid savings in this one brokerage account rather than having many others.⁷ This wealth measure is also comparable to official statistics. Bundesbank (2013) reports the average portfolio value of a German stock market investor to be approximately 48,000 euros in 2013. We also have an approximately 10% customer subsample that the bank flagged as "main clients," who do not have "any other banking relationships." The bank internally developed a so-called wallet share to assess which share of individuals' wallets are with this bank. The measures they use are 1) regular salary, pension, or other income transactions, 2) regular ATM withdrawals and point-of-sale transactions (when individuals use their debit card in stores to pay), and 3) individuals who have set up the tax-free allowance dedicating this brokerage as their main brokerage for tax purposes (Freistellungsauftrag). We perform robustness checks using this subsample of clients in Subsection 5.3.

Additionally, we compare portfolio values to the self-reported gross annual household income, which is also elicited by the bank upon account opening and reported in brackets. The mean ratio of the median portfolio value (in 2017 and 2018) to self-reported income about account opening is approximately 1. For comparison, the ratio of total financial assets to gross household income in the German population is approximately 1.1 (German Federal Bureau of Statistics, 2008; Bundesbank,

⁷Note that individual savings do not include pension savings. In Germany, the pension system is based on the pay-as-you-go (or redistributive) model. Funds paid in by contributors (employees and employers) are not saved (or invested) but are used to pay current pension obligations. Thus, unsurprisingly, individuals do not hold large savings for retirement, on average.

2013).

In addition to these demographic and socioeconomic statistics, we also report the summary statistics for investor attentiveness, trading behavior, and portfolio risk in Table 1. We report the number of logins per year, the Herfindahl-Hirschman (HH) index, a measure of portfolio diversification, and turnover. The HH index ranges from 0 to 1, and a smaller value indicates better diversification. If it is above 0.5, then the investor holds only a handful of stocks, and if it is close to 0, the investor holds a fund or two. Portfolio turnover equals 0.5 times the sum of all purchases per month divided by the end-of-month portfolio value plus 0.5 times the sum of all sales divided by the beginning-of-month portfolio value multiplied by 12, following Barber and Odean (2001). The risk class of trades is the average of the risk classification of traded securities. This risk class is established by German regulations and ranges from 1 (safe securities such as money-market funds) to 5 (stocks, options, and futures). We also display summary statistics for the number of total trades in funds, the holding periods of funds, and the funds share in the portfolio value. Additionally, we report the average disposition effect for funds, i.e., the propensity to realize winning funds minus the propensity to realize losing funds, calculated following Odean (1998), with respect to the funds' actual purchase prices.

We can see that the average investor logs are 9 times per year, and most investors are reasonably well diversified. Our investors are reasonably active on average; the mean number of funds trades over the period of 2017 and 2018 is 11 per year. Furthermore, our investors are reasonably well diversified but typically hold only a handful of different securities (the median number of securities equals 7). Approximately half of our investors hold a fund in 2018, make on average 18 trades in them, hold those funds for an average of four years, and display a slight disposition to sell losing funds over winning funds (with respect to their actual purchase prices). Note that a share of the number of trades in funds are trades induced by monthly savings plans.

In Table 1, we separately show the summary statistics for affected investors, i.e., those investors who held a fund in 2018. Unsurprisingly, our sample of affected investors is thus slightly better diversified. Again, Note that the HH index, turnover, and risk class consider investors' overall portfolios. In contrast, the number of trades, the holding period, and the disposition effect consider only funds.

We compare the two groups of investors using a standard balanced characteristics approach by regressing a dummy for being affected on all investor characteristics. In Table 1, we report the adjusted R-squared from this kitchen-sink regression and display the Wald tests' p-values for all investor characteristics, indicating whether their coefficient in this regression is zero. As we perform 16 tests, we correct the p-values for multiple hypothesis testing problems using the Bonferroni method. As we can see, being an investor who invests in funds is not randomly assigned but correlated with certain characteristics, and most p-values are close to zero. That said, in all our regressions, we focus on the subsample of affected investors.

The left graph in Figure 4 shows the distribution of the positions in all fund securities in 2018 and their fictitious returns since the 12/29/2017 closing price, as quoted by the exchange (either at the time of sale or using their June 30, 2018, prices). The right graph in Figure 4 shows only fund positions that were sold and their returns since 1/1/2018. We can clearly see that the majority of funds' returns since 1/1/2018 are slightly negative, given that the market performed poorly at the beginning of 2018. However, the returns of funds that were sold after 1/1/2018 are more often positive, i.e., there is more probability mass in the bar just above 0% returns and more probability mass in the more positive returns domain. Thus, in the raw data, we see that individuals are more likely to sell funds at a gain relative to their 12/29/2017 closing price, i.e., their fictitious purchase price.

[Insert Figure 4 about here]

The left graph in Figure 5 shows a distribution of the individual changes in all funds' returns due to the repricing, i.e., the distribution of the return from 12/29/2017 to the date the position was sold in 2018 or the price on 06/30/2018 if the position remained unsold (the fictitious return), minus the return from the purchase price (the actual return). The right graph in Figure 5 shows the distribution of fictitious minus actual returns for the sold fund positions only. We can clearly see that the repricings resulted in very different displayed returns and that most repricings themselves resulted in a negative fictitious return (mostly because the market performed poorly at the beginning of 2018). However, for the funds that were sold, there is again a substantial probability mass in the positive return domain because individuals tend to sell funds that have a positive fictitious return.

[Insert Figure 5 about here]

Clearly, the fund repricings do not represent an actual wealth shock but only a fictitious shock to the displayed returns. However, as shown in Figure 5, the repricings had very large effects on the displayed returns.

Table 2 shows summary statistics for monthly consumption and liquidated assets at the individual investor level for 2017 and 2018. All variables are aggregated to the monthly level; for instance, we display the monthly amount of ATM withdrawals or the monthly amount of liquidated fictitious capital gains, conditional on those variables being nonzero in a given month. We can see that investors withdraw reasonably large fractions of their monthly salaries on average. Another share is consumed in the form of point-of-sale transactions (individuals swipe their card or use them online) or domestic wire transfers (an alternative common form of paying for online goods and services in Germany) as well as recurring wire transfers, such as rent payments. Additionally, the table shows the amounts of capital gains of the liquidated actual and fictitious winners and losers.

[Insert Table 2 about here]

Table 3 shows the propensity of our investors to sell at a gain or loss for all securities. The proportion of realized gains or losses is defined as the number of realized gains or losses relative to the number of all (realized and paper) gains or losses in the portfolio (as in Odean, 1998). In turn, we find a familiar discrepancy in the proportion of realized gains versus losses, i.e., the disposition effect, as first documented by Odean (1998) followed by a sizable literature. Our statistics are in line with the findings of these studies.

[Insert Table 3 about here]

4 Methodology and identification

4.1 Specifications for trades and consumption

We first run a disposition effect regression to determine how fictitiously changed displayed purchase prices affect trades. For either the full sample period or only 2018, we regress a dummy for selling either all securities or funds on whether the sold security was a winner, a fictitious winner (i.e., a loser relative to the actual purchase price but at the time of sale displayed as a winner), or a fictitious loser (i.e., a winner relative to the actual purchase price but at the time of sale displayed as a loser):

$$SaleDum_j^{it} = \eta_i + my_t + \beta GainDum_j^{it} + \gamma FictGainDum_j^{it} + \theta FictLossDum_j^{it} + \epsilon_j^{it} \quad (1)$$

where $SaleDum_j^{it}$ is a dummy for whether investor i sold security j at time t , my_t is a month-by-year fixed effect, η_i is an individual fixed effect, $GainDum_j^{it}$ is a dummy for whether security j of investor i at time t is a winner relative to the actual purchase price, $FictGainDum_j^{it}$ is a dummy for whether security j of investor i at time t is a winner relative to the newly displayed purchase price even though it is a loser at the actual purchase price (fictitious winner), and $FictLossDum_j^{it}$ is a dummy for whether security j of investor i at time t is a loser relative to the newly displayed purchase price even though it is a winner at the actual purchase price (fictitious loser). As is standard in the disposition-effect literature, investor i 's trades and (fictitious) gains or losses at time t are observations whenever investor i makes any trade at time t , i.e., we run a regression conditional on individual trading days.⁸ The (fictitious) winner or loser status of a security is exogenous to individual investors. Nevertheless, other control variables may increase precision, and time fixed effects are important because they control for any aggregate trends affecting stock prices and selling behavior. Furthermore, we cluster standard errors at the individual level or double-cluster standard errors at the individual and month-by-year levels.

⁸To facilitate comparability of our results to the literature, we run this regression conditional on individual trading days as is most standard (see, e.g., Chang et al., 2016; Koestner et al., 2017). Instead, one could run an unconditional regression as done by Ben-David and Hirshleifer (2012) who discuss the differences between the two approaches.

To analyze the effects of fictitious capital gains on consumption, we aggregate all variables to the monthly level and use the time period from January 2017 to June 2018 or only 2018. The time-series regression employs a panel for each month t from January 2017 to June 2018. We then regress the euro value of consumption ($Cons^{it}$) on the realized fictitious gain and loss ($RealFictGain^{it}$ and $RealFictLoss^{it}$) as follows:

$$Cons^{it} = \eta_i + my_t + \beta RealFictGain^{it} + \gamma RealFictLoss^{it} + \theta RealGain^{it} + \delta RealLoss^{it} + \vartheta X^{it} + \epsilon^{it} \quad (2)$$

Specifically, $Cons^{it}$ is the euro value of ATM withdrawals and point of sale transactions plus domestic nonrecurring outgoing wire transfers (Lastschrift und Überweisungen), $RealFictGain^{it}$ and $RealFictLoss^{it}$ are the capital gains or losses of the liquidated fictitious winners and the fictitious losers, and $RealGain^{it}$ and $RealLoss^{it}$ are the capital gains or losses for the liquidated actual winners or losers. The other control variables, X^{it} , include Liq^{it} , i.e., the total amount (including the capital gains or losses) that is liquidated by individual i in month t minus the reinvestment in the portfolio, and this amount naturally includes the actual capital gains of each investment. As we will discuss, our results are robust to the inclusion or exclusion of the liquidated actual winners and losers as well as the liquidation amounts and other control variables such as a liquidation dummy, salary payments, dividends, or interest payments. As before, we cluster standard errors at the individual level or double-cluster standard errors at the individual and month-by-year levels.

The coefficient on the variable $RealFictGain^{it}$ states the following: controlling for the total amount of liquidations in that month (including individual and month-by-year fixed effects as well as the actual capital gains and losses), how much does an additional euro of liquidated fictitious capital gain yields in extra consumption? This approach is just a standard regression analysis. It clarifies the conceptual experiment desired. Take two investors with the same total liquidations in a month and then test whether those with greater fictitious gain liquidations have greater consumption. The idea here is that by controlling for total liquidations, we control for the permanent income of the investor, which, in the standard model, should determine consumption.

4.2 Identification strategy

Our regression specification is a two-stage IV approach. We use the fictitious gain status as a first stage to cause liquidations and then estimate the MPC out of each dollar of liquidated fictitious capital gains. More specifically, in the first stage of the IV approach, we regress the endogenous variable, the decision and amount to sell (Liq^{it}) in euros by individual i in each month t on the (unrealized or realized) euro value of the fictitious capital gains ($UnrealFictGain^{it}$). The predicted

values of that regression (\widehat{Liq}^{it}) equal the realized fictitious capital gains.

$$Liq^{it} = \beta UnrealFictGain^{it} + \epsilon^{it} \Rightarrow \widehat{Liq}^{it} = RealFictGain^{it}$$

In the second stage of the IV approach, we then regress consumption $Cons^{it}$ on the predicted values of the first-stage regression ($\widehat{Liq}^{it} = RealFictGain^{it}$), as done in Specification 2. Thus, the variables $FictGain^{it}$ and $FictLoss^{it}$ are used as instruments. As the first stage, significant coefficients in Specification (1) for $RealFictGain^{it}$ and $RealFictLoss^{it}$ imply that fictitious capital gains and losses affect trading. As we will see, individuals sell fictitious winners, and we have a very strong first stage. $RealFictGain^{it}$ and $RealFictLoss^{it}$ are, of course, correlated with the actual capital gains $RealGain^{it}$ and losses $RealLoss^{it}$ because both depend on the fund's price path since 1/1/2018. However, we control for actual capital gains or losses with the available liquidation amount Liq^{it} . To the extent that individuals liquidated fictitious winners and losers, $RealFictGain^{it}$ and $RealFictLoss^{it}$ are correlated with the decision and amount to liquidate that we control for via Liq^{it} , the liquidation amount minus reinvestment. We thus have an instrument for liquidations, $RealFictGain^{it}$ and $RealFictLoss^{it}$, and we then assess in the IV regression whether the fictitious capital gains and losses cause liquidations and increase consumption.

If an individual liquidates because he or she wants to consume or because his or her wealth is higher, then his or her consumption amount should not be affected by fictitious gains and losses, i.e., $RealFictGain^{it}$ and $RealFictLoss^{it}$. Ultimately, the displayed fictitious capital gains are, in a sense, just random numbers depending on the price of the fund on 12/29/2017 at the end of the day and its performance since. Economic sentiment that could affect both consumption and stock prices can be controlled for by the time fixed effects, while the decision about how much to liquidate and the wealth considerations are controlled for by the liquidation amount and actual capital gains and losses. We thus argue that the variables $RealFictGain^{it}$ and $RealFictLoss^{it}$ are exogenous conditional on the controls.

To further understand Regression (2) and how we identify the causal effect of fictitious capital gains that induce individuals to sell and consume, we can describe the omitted variables that affect both consumption $Cons^{it}$ and fictitious capital gains $RealFictGain^{it}$ and would lead to a spurious correlation rather than a causal relationship in Specification (2). We are concerned about three types of omitted variables: time, consumption plans or preference shocks, and wealth shocks. First, there are time and aggregate variables that drive both consumption and fictitious capital gains in 2018 (e.g., economic sentiment). We can control directly for any aggregate variables correlated with time using the month-by-year fixed effects my_t . Whether a fund is displayed at a (fictitious) gain or loss depends on the price on 12/29/2017 and the price movement since, which, controlling for month-by-year fixed effects and thus all aggregate trends, is plausibly exogenous to individual investors. Controlling for month-by-year fixed effects is important in Regression (1), as time-varying

economic sentiment may affect stock prices and trading behavior, and the same is true in Regression (2).

Second, there are individual consumption plans or shocks to individual preferences. Specifically, there is the decision to liquidate, $LiqDum^{it}$, and then the amount that is liquidated, which we denote by Liq^{it} . The decision to liquidate is clearly correlated with the liquidated fictitious capital gains that equal $RealFictGain^{it} = LiqDum^{it} * UnrealFictGain^{it}$ if $UnrealFictGain^{it}$ denotes unrealized fictitious capital gains as above. However, we control for it directly by controlling for $Liq^{it} = LiqDum^{it} * (UnrealGain^{it} + InvL^{it} - ReInvL^{it})$ if $UnrealGain^{it} + InvL^{it} - ReInvL^{it}$ equals the unrealized capital gains plus the initial investment amount minus the reinvested amount upon liquidation. Additionally, as mentioned previously, we can control for a liquidation dummy, $LiqDum^{it}$. In addition to the decision to liquidate, controlled for by $LiqDum^{it}$, the amount liquidated is determined by individual shocks or plans and may be correlated with aggregate fluctuations in the stock market, but we can control for that directly with Liq^{it} and the time fixed effects. Third, we may be concerned about wealth shocks that affect consumption and individual capital gains that are correlated with fictitious capital gains. However, controlling for Liq^{it} also takes care of this concern.

The omitted variable bias theorem tells us that when we run a regression of the form $Y = \beta X + \epsilon$ and a variable Z (in our case, the decision to liquidate plus how much) affects both X and Y , then β is biased. However, β is unbiased if we control for Z in the regression $Y = \beta X + \rho Z + \epsilon$, which is exactly what we do here. As an alternative to interpreting our regression as a two-stage specification, we can simply identify a causal effect of fictitious capital gains and consumption by directly controlling for the omitted variables. We can control directly for these omitted variables because we do not use proxies, and the omitted variables that we control for (such as actual capital gains or the liquidated amount) are measured precisely in euro values and thus entirely free of error.⁹

5 Results

5.1 Empirical results for trading

Columns 1 and 2 of Table 4 show the estimation results for the probability that either all securities or only funds are sold when they are displayed as winners for the full sample period. We see in Column 1 that individuals are subject to the disposition effect over the whole sample and all securities, and the likelihood of sale is approximately 6 percent higher when the security is trading

⁹As an alternative interpretation, we can view Regression (2) as a differences-in-differences specification, in which the treatment variable is whether individuals are subject to a fictitious winner conditional on all individuals liquidating a certain amount. We then simply regress consumption on the liquidation amount, and we examine an interaction, $RealFictGain^{it}$ and $RealFictLoss^{it}$, of the randomly assigned fictitious winner or loser status of that investment.

at a gain relative to the actual purchase price.¹⁰

We document in Column 2 of Table 4 that the average disposition effect for all funds is slightly negative in the overall sample of 103,000 German investors over the period 2003 to 2018. This result is in line with [Chang et al. \(2016\)](#), who use the data from [Odean \(1998\)](#) consisting of 73,558 US households from January 1991 to November 1996 and document a reverse disposition effect for delegated investments such as mutual funds. However, when we split the sample by year, for instance, we generally find an attenuated, sometimes positive and sometimes negative disposition to sell winning funds (following the analysis in [Bernard et al., 2018](#); [An et al., 2019](#)). Our rolling-window disposition effect regressions can be seen in Figure 6, which contrasts the propensities to sell winning stocks and funds with the return of the German stock market index (DAX). The disposition effect for all securities and funds is countercyclical relative to the DAX. The variation in the disposition effect for funds closely follows the variation in the disposition effect for all securities, suggesting that a similar psychological mechanism is at play.

Columns 3 to 5 of Table 4 show the estimation results for the probability that funds are sold when they are 1) winners relative to their actual purchase prices, 2) displayed as winners after 1/1/2018 despite actually being losers (fictitious winners), or 3) displayed as losers after 1/1/2018 despite actually being winners (fictitious losers). We see that individuals are subject to the disposition effect with respect to fictitious winners and losers, and they are nearly 4 percent more likely to sell a fictitious winner but less than 1 percent less likely to sell a fictitious loser.¹¹

[Insert Figure 6 and Table 4 about here]

In turn, Table 4, Column 4 includes individual fixed effects and month-by-year fixed effects, and Column 5 also adds funds fixed effects. In the latter case, we thus control for all time-invariant effects at the investor and fund level.

In Figure 7, we show the fictitious loss and gain coefficients for five quintiles of the fictitious (displayed) loss (in percentage return terms) and five quintiles of the fictitious (displayed) gain. We see that the effect is monotonically increasing in the fictitious (displayed) gain, but we do not detect a pattern with respect to fictitious losses.¹² Additionally, in this figure, we show the fictitious loss and gain coefficients for five quintiles of the actual loss (in percentage return terms) and five quintiles of the actual gain. Here, we see that the fictitious gain coefficients monotonically decrease in the actual loss, but we do not detect a pattern with respect to fictitious losses.

¹⁰Here, we simply replicate the disposition effect findings in [Koestner et al. \(2017\)](#). The disposition effect is difficult to rationalize with efficient markets and rational expectations (as argued in [Odean, 1998](#)).

¹¹Note that when we include funds fixed effects, the number of observations decreases as singleton observations are dropped. Table A.13 in Appendix A shows the same table with standard errors double-clustered at the individual and month-by-year levels. Double-clustering standard errors does not make a difference to the significance of the coefficients.

¹²The analysis of the five quintiles of the fictitious (displayed) loss (in percentage return terms) and five quintiles of the fictitious (displayed) gain are reassuring that our dummy variable for a fictitious gain is not concealing any nonlinear relationships in recent returns or the probability of a sale. Our findings are perfectly in line with those in [Kaustia \(2010\)](#).

[Insert Figure 7]

5.2 Empirical results for consumption

Table 5 shows the estimation results for consumption from Specification (2). Here, we regress different measures of consumption on liquidated fictitious capital gains or losses, including different sets of control variables. As mentioned above, we run the regression using the sample period from January 2017 to June 2018. We find that individuals typically only consume a small fraction of their liquidations; however, fictitious capital gains have a large effect. Approximately 15 to 25 percent is consumed out of a fictitious capital gain that is liquidated. For fictitious capital losses, the effect is again attenuated. We also report specifications for the subsample of customers that the bank flags as main clients (as further discussed in the next subsection). Here, we generally find larger effects. Additionally, we generally find larger effects if we include domestic wires in our measure of consumption.¹³

[Insert Table 5 about here]

The coefficient ϑ in the consumption regression, Specification (2), can thus be interpreted as the MPC out of one dollar of realized capital gains or losses when the security was a fictitious winner. An MPC of 25 percent at the monthly level is in line with the high MPCs out of realized capital gains reported in Meyer et al. (2018) and Di Maggio et al. (2020) as well as the high MPCs out of transitory income shocks as surveyed in Jappelli and Pistaferri (2010). In Table 6, we also demonstrate that the coefficients are comparable to those estimated from actual capital gains.

Table 6 shows our initial set of robustness checks. First, we calculate the deviation from the mean of the outcome variable as well as the regressor variables. It can be seen that for the specification variants and sample splits, the coefficients for fictitious and actual capital gains are comparable in magnitude. Looking at the deviations from the mean takes into account that actual capital gains and losses are much larger on average than fictitious capital gains and losses. Furthermore, we show different sets of control variables in this table as well as a specification in which we restrict the sample to 2018 only. Note that we do not find an effect for fictitious capital losses, but in regard to actual capital losses, we estimate significant coefficients. The estimated coefficients for losses are positive because the capital loss variables are negative, and the sign is preserved in the transformations.

[Insert Table 6 about here]

We will now perform a number of robustness checks and in turn describe the psychological mechanisms behind our results and what we can learn from them.

¹³Table A.14 in Appendix A shows the same table with standard errors double-clustered at the individual and month-by-year levels. Double-clustering standard errors does not make a difference to the significance of the coefficients.

5.3 Robustness

5.3.1 Main customers and additional control variables

A standard concern when working with administrative datasets provided by banks is that they do not capture the total wealth of clients, as they may have and use other banking relationships. We argue that it is unlikely that individuals have a second brokerage account or additional savings vehicles because banking with multiple banks is discouraged in the German credit score system. Furthermore, individuals are incentivized to dedicate one brokerage account as their main account to receive the tax-free allowance on capital gains. Additionally, the self-reported wealth measure we have when individuals opened their accounts is in line with their actual portfolio size. This indicates that individuals are, on average, unlikely to have other deposits of liquid wealth. However, we have a flag variable from the bank itself that indicates customers without any other banking relationships. This flag variable matches well with our own measures of main customer status: a regular salary payment (which is flagged as such in the bank’s transaction system) or a minimum number of transactions per month (as used in [Ganong and Noel, 2019](#); [Kuchler and Pagel, 2019](#); [Olafsson and Pagel, 2018a](#)).

In [Table 5](#), we also display the results of the consumption regressions that consider only individuals who the bank has flagged as main customers, i.e., clients without any other banking relationships. As expected, the effects become stronger for this subset of individuals. Some individuals may not use the bank’s ATM network or debit card for point-of-sale transactions because they do not use this bank’s checking account as their main checking account.

To further establish robustness and alleviate concerns about omitted variables, we report the results of consumption regressions successively adding fixed effects and other control variables such as a dummy for liquidations, salary, dividend, and interest payments in [Table A.16](#) in [Appendix A](#). Further, in [Table A.17](#), we successively add unrealized gains and losses and unrealized fictitious gains and losses as controls.

[Insert [Tables A.16](#) and [A.17](#) in [Appendix A](#) about here]

To illustrate our findings in the raw data, we show a simple nonparametric illustration of the relationship between consumption (as measured by ATM withdrawals, POS transactions, and non-recurring domestic wires) and the available funds from trading during that month, i.e., liquidations minus reinvestments, either for individual-month combinations in which no fictitious capital gain liquidation took place or in which a fictitious gain liquidation took place in [Figure 8](#). For visibility and to examine the relevant range for our short-term consumption variable of ATM withdrawals and point-of-sale transactions, we restrict the available liquidated funds to be positive and less than 10,000 euros. The raw data show a level shift, i.e., consumption seems to be generally higher when individuals liquidate a fictitious capital gain, consistent with our estimation results.

5.3.2 Quality of the consumption data and responses at the daily level

We observe transaction categories from the transaction system that allow us to pinpoint ATM withdrawals, (international) POS transactions, (repeated, automated, international) wires, interest and dividend payments, (portfolio) fee payments, tax payments, check payments, salary transfers, cash deposits, social security payments and security purchases and sales. As discussed previously, point-of-sale transactions occur when individuals swipe their debit or credit card in the store or purchase goods or services online by entering their card information. Furthermore, domestic non-recurring wire transfers that leave the bank (Lastschrift and Überweisungen) are likely to reflect discretionary consumption in Germany, as they are commonly used to pay for online purchases of goods and to pay for in-person services.¹⁴

We thus use ATM withdrawals and POS transactions or ATM plus POS plus wires as a measure of discretionary consumption. To assess the quality of our spending data, we compare the spending responses to paydays to those that have been documented in the literature using transaction-level spending data that are more thoroughly categorized (Olafsson and Pagel, 2018a; Bräuer et al., 2019; Gelman et al., 2014). When we replicate the analysis in Olafsson and Pagel (2018a) and Gelman et al. (2014), i.e., plotting the daily deviation in spending around paydays for three income groups of our final sample of customers for the years 2017 and 2018, we obtain similar pictures in terms of magnitudes and tightness of the estimates (as Figure 9 shows). Furthermore, when we examine the daily consumption response from fund sales when individuals liquidate a fictitious or actual capital gain in 2018, we find similar responses to those in Bräuer et al. (2019), as shown in Figure 10. Here, we can also see that the daily consumption response is similar in response to actual and fictitious capital gain liquidations.

[Insert Figures 9 to 10 about here]

These figures also make clear that individuals consume after their transaction is booked in the settlement account, which is when they receive their sales receipt that details the actual purchase and sale prices as well as actual capital gains and losses. Investors are thus fully informed about their actual capital gains at the time of consumption.

5.3.3 Placebo tests

All placebo checks can be found in Tables 7, 8, and A.18 in Appendix A as well as Figure 11. As placebo tests, we can run Specification (1) and compute the placebo fictitious gains and losses of nonfunds in 2018, for which the bank did not reset the displayed purchase prices. Alternatively,

¹⁴Domestic wires are also likely used for recurring transfers, e.g., paying rent. However, in this case, individuals can very easily, and most individuals do, set up a recurring domestic wire transfer (Dauerauftrag) that transfers the rent automatically on a certain day of the month. These recurring transfers are flagged as such in the transaction system and are thus excluded from our measure of spending.

we compute the placebo fictitious gains and losses for funds but use the data from January to June 2017 when the bank did not change the displayed purchase prices. In Table 7, Column 1, we examine 2018 but use nonfund securities and their placebo fictitious gains and losses. Column 2 uses nonfund securities but considers data until June 2017 and the placebo fictitious gains and losses. Column 3 uses funds but data until June 2017 and their placebo fictitious gains and losses. In turn, the next columns add individual and month-by-year fixed effects. Overall, we can see here that there is a recency effect, as we obtain significantly positive coefficients.

In the data of Odean (1998), it is well known that there is also a recency effect: individuals tend to sell securities that performed well recently. However, the recency placebo effects we document are statistically significantly smaller than the effects of fictitious winners in 2018. Nevertheless, to identify the difference in one specification, we run a regression using data from January to June in 2017 and 2018. In turn, we include fictitious capital gains and losses, either the actual gains or the placebo gains, in one variable. Finally, we include a dummy for 2018 and document the interaction coefficient of this dummy and the (placebo) fictitious capital gains. This is done in Table 9, and we see that the interaction effect is large and statistically significant, ranging from 1.3% to 1.7%. We also perform the placebo test using placebo fictitious gains and losses of funds for all other years, the results of which can be found in Table A.18 in Appendix A. The fictitious gain coefficients range from -0.0768 in 2008 to 0.0320 in 2005. Again, as also shown in Figure 6, there appears to be a large variation in this recency or beginning-of-year effect with the business cycle. For our purposes, however, it is important that individuals have a reason to sell because of the fictitious capital gain that is plausibly exogenous and is not reflected in an actual capital gain. For our consumption regressions and our estimated MPC in Table 5, we control for actual capital gains (which incorporate the recency effect) and fictitious capital gains. Thus, the effect on fictitious capital gains excludes and appears to dominate the recency effect.

Furthermore, in Table 8, we show the consumption regressions for data up to June 2017 instead of 2018 using the placebo fictitious capital gains and losses for all individuals or individuals who the bank flagged as main customers, i.e., clients without any other banking relationships. In these regressions, the effects of placebo fictitious capital gains and losses are very small and mostly insignificant.

[Insert Tables 7 to 9 and Table A.18 in Appendix A about here]

Finally, in Figure 11, we show the distribution of fictitious gain coefficients for 100 reshuffling experiments, the average reshuffled coefficient and its average standard error, and the true fictitious gain coefficient with its standard error while controlling for individual and month-by-year fixed effects, the available liquidated amounts, and a dummy for liquidations. In these reshuffling experiments, we simply randomly allocate the fictitious capital gains or each person in each month to a different person and month. We do the same for the actual capital gains and fictitious capital

losses. We then rerun our regressions and plot all the estimated placebo coefficients along with the true coefficient for comparison. As expected, the reshuffled fictitious gain coefficients are not significant.

[Insert Figure 11 about here]

6 Lessons and psychological mechanisms

What are the main lessons from this study? In summary, we analyze a unique experiment in which individuals sell a fictitious capital gain in a manner consistent with the disposition effect, and this fictitious gain is not reflected in an actual capital gain (in response to which individuals should consume more because they are wealthier). Despite a fictitious winner representing an actual capital loss, which individuals see on their sales receipt or in their order history, they consume more in response to fictitious capital gains.

We thus learn four main lessons. 1) Investors choose to ignore provided information about their actual capital gains, but only if they are shown a fictitious capital gain rather than a loss. 2) These effects are stronger when the fictitious capital gain is large and the actual capital loss is small as well as when more salient information is supplied. 3) Selling because of the disposition effect has real consequences: individuals sell to consume, and if they are under the (mistaken) assumption that they are richer, they consume more. Here, the changing purchase price and selective inattention affect consumption, a direct component of utility and welfare. Thus, even in the absence of trading costs due to the disposition effect or costs of selective inattention to brokerage accounts, we know that behavioral biases affect welfare. 4) Because individuals choose to be selectively (in)attentive to their fictitious capital gains (losses), we obtain an estimate for the marginal propensity to consume out of stock market wealth using high-frequency transaction-level consumption data.

We thus document that selective inattention and the disposition effect affect consumption and that there is an important interaction between the two. We argue that the psychological mechanism behind this interaction is attribution error (as in [Barber and Odean, 2002](#)), i.e., individuals are looking for an excuse to sell a losing investment that is displayed as a winner, and this behavior has real consequences in terms of affecting consumption.

6.1 The interaction between selective inattention and the disposition effect

In Table 4, Figure 6, and Figure 7, we document two results. First, the effect of fictitious winners is much more pronounced than the effect of fictitious losers. It seems that individuals know when an actual winner is displayed as a loser, which makes them even less likely to sell that fund. Second, the propensity to sell fictitiously winning funds is larger than that of actually winning

funds. We interpret these two results as follows: individuals are looking for a reason to sell losing funds once they are fictitiously displayed as winners to feel richer and consume the proceeds (as we show). Therefore, individuals can choose to be inattentive to the fact that their fictitious winners are actually losers consistent with selective inattention (as documented in [Karlsson et al., 2009](#); [Olafsson and Pagel, 2018b](#); [Gherzi et al., 2014](#)). We document additional evidence consistent with this interpretation in Figure 7. Here, we depict the fictitious loss and gain coefficients for five quintiles of the fictitious (displayed) loss (in percentage return terms) and five quintiles of the fictitious (displayed) gain. We can see that the effect is monotonously increasing in the fictitious (displayed) gain, but we do not detect a pattern with respect to fictitious losses. Again, the finding that fictitious winners induce a strong response while fictitious losers do not is consistent with selective inattention playing a role. Furthermore, consistent with this interpretation, it is likely that a large fictitious capital gain raises more attention than a small one. Additionally, in this figure, we show the fictitious loss and gain coefficients for five quintiles of the actual loss (in percentage return terms) and five quintiles of the actual gain. Here, we see that the fictitious gain coefficients monotonically decrease in the actual loss, but we do not detect a pattern with respect to fictitious losses. We argue that individuals have a more difficult time convincing themselves that an actual loser is a winner when the actual loss is very large.

The omitted category in all these regressions is actual losers; thus, we document that, compared to actual losers, individuals are much more likely to sell fictitious winners, but they are not more likely to sell actual winners or fictitious losers. The former result is in line with attribution error (as in [Barber and Odean, 2002](#)) applied to fictitious gains and losses, i.e., individuals are looking for an excuse to sell a losing investment that is displayed as a winner.¹⁵ The latter two results are in line with the absence of a clear disposition effect for funds that we document over the entire sample period in line with [Chang et al. \(2016\)](#). This finding is also documented in Figure 6. The strong comovement between the disposition effect for stocks and funds suggests to us that the psychological mechanism behind the two has common components.

[Barber and Odean \(2000\)](#) show that trading due to the disposition effect is primarily harmful because individuals who trade incur considerable trading costs. The trading costs of discount brokers have been reduced considerably. However, in this paper, we show that the disposition effect affects utility directly through consumption beyond its effects on wealth.

The finance literature that uses the data in [Odean \(1998\)](#) debates whether the disposition effect manifests in selling winners versus losers. [Jin and Scherbina \(2011\)](#) and [Fischbacher et al. \(2017\)](#) find that the disposition effect manifests itself mainly through holding losers, but [Bernard et al. \(2018\)](#) suggest that the disposition effect in bust periods is correlated with individuals realizing

¹⁵Attribution error is originally described as a bias in which people attribute successes to their own ability and attribute failures to noise. In our setting, we refer to the bias of questioning successes (fictitious winners) less and attributing them to own ability but questioning failures (fictitious losers) more critically ultimately not attributing them to own ability.

gains. In contrast, [An et al. \(2019\)](#) document that the disposition effect is more prevalent when individual portfolios trade at a loss. We find that individuals consume more after engaging in the disposition effect and selling fictitious winners, i.e., funds that are displayed as winners but actually losers. The reference point that is relevant for investors seems to be the displayed purchase prices if investors see a gain relative to them. Therefore, choosing to be inattentive about an actual loss allows investors to overcome the disposition effect and sell a loser. To push this further, investors exploit a fictitious disposition effect to overcome the actual disposition effect. Again, individuals sell winners because they believe that they are making money and hence consume more, consistent with attribution error (as in [Barber and Odean, 2002](#)). Perceiving a fictitious winner may relieve individuals' sense of having made poor investment decisions, which makes them feel better and consume more.

Table 5 shows our large consumption responses. Here, a reason is again selective inattention: the absolute and relative fictitious capital gain is very salient to investors when selling the fund (as opposed to the actual capital gain and the actual liquidation amount that is only stated on the sales receipt that individuals receive immediately after the sale has been processed by the clearing house and the bank has subtracted all fees and taxes). Immediately next to the sell button, the fictitious capital gain is displayed in both euros and percentage terms. Below, we will further explore whether less-informed individuals have a larger consumption response. That said, we also documented that individuals respond in their consumption to fictitious winners (in the same way they respond to actual winners) but not fictitious losers (i.e., they do not respond in the same way as they do to actual losers in their consumption).

In Subsection 6.2, we perform a number of interaction exercises to assess the validity of selective confusion about actual capital gains. In Subsection 6.3, as an alternative explanation, we discuss that individuals liquidate and consume because they believe that doing so is advantageous from a tax perspective. We thus also look at confusion about tax implications. In short, we find evidence that individuals choose to be confused about their actual capital gains when that is easier but no evidence that they are confused about the tax implications.

6.2 Marginal propensity to consume out of (confused) capital gains

After individuals sell a security, they receive a sales receipt that details their capital gains relative to the actual purchase price (if individuals bought at different points in time, the bank implements the first-in-first-out (FIFO) principle in line with the German tax authority), their sale price, the capital gains taxes, and the fees that investors paid (Figure 3). Individuals receive the sales receipt before consuming, as is made clear in Figure 10, showing the spike in consumption a few days after the transaction is processed and booked in the settlement account. Furthermore, individuals' trading and consumption are not majorly affected by fictitious losers, i.e., actual winners that are displayed

as losers. Our results thus point towards individuals choosing to be confused about their fictitious capital gains. Additionally, we can use variation in the supply of information because, starting in March 2018, a pop-up window about the fictitious purchase prices appeared after individuals logged in (displayed in Figure 1).

The interpretation of our findings depends on whether individuals know their actual wealth or are confused about the fictitious capital gain representing their actual capital gain. In the latter case, we simply estimate the MPC out of (confused) capital gains using the fictitious winner status as an instrument for the liquidation of capital gains. This estimate is of interest to the literature on stock market wealth and consumption. In the former case, we estimate a consumption effect solely from the act of selling a winning investment. Either way, the finding is of interest to the literature on the disposition effect, showing that the act of selling winners has effects on consumption and just trading. Individuals sell winners because they believe they make money and hence consume more. Additionally, our finding is of interest to the literature on selective inattention: retail investor inattention appears powerful enough to affect trading and consumption.

When we examine the interaction between early and late 2018 for our first stage (selling fictitious winners), we find statistically significant effects in early and late 2018, but the effects are stronger in early 2018 (see Table 10). This result is in line with the fact that the bank supplied more information to investors in late 2018 than in early 2018. Furthermore, the stronger early-2018 effects could be partially driven by the recency effects we documented in our placebo checks (see Subsection 5.3.3).

[Insert Table 10 about here]

In addition to the interaction with an early 2018 indicator, the table shows the interactions with a number of additional proxies for investor uninformedness. We first consider the first five trades and then the first half of trades for each investor (split at the individual level) in 2018. Then, we also interact by passive versus active investors by the median number of trades in 2018. Additionally, we interact with a dummy variable for whether an investor uses automatic savings plans, which presumably makes it more difficult for him or her to determine whether the investment is an actual winner or loser. Additionally, we consider an interaction with whether or not an investor logged in more often (relative to his number of trades). As we can see, it appears that all sample splits and interactions point towards the hypothesis that the more informed investors exhibit less of a disposition to sell fictitious winners. That said, the effects are significant even in late 2018 and for more informed investors.

Table 11 provides an additional set of interactions using the above measures of informedness but consumption as the outcome variable. Here, we consistently find that early trading and infrequent traders have a larger consumption response to fictitious capital gains. The same is true when we consider the deviation from the mean as the outcome variable, as shown in Table A.15. When

we look at the deviation-from-the-mean transformation of the outcome variable, we find significant effects for both early and late 2018 as well as all measures of investor informedness. The reason is that the deviation-from-the-mean transformation scales the responses in a sensible way and reduces the impact of noise in euro amounts.

[Insert Tables 11 and A.15 about here]

We thus conclude that significant effects remain, indicating that even after eliminating confusion, the interaction between selective inattention and the disposition effect is a substantial factor in determining consumption.

6.3 Tax considerations or confusion about tax implications

Individual trading may be driven by tax considerations, or individuals may be confused about the tax implications of the reform. If they believe that the newly displayed capital gains are somehow more tax-relevant, then they should not be more likely to sell at a capital gain than a capital loss. Therefore, the disposition effect in fictitious gain positions would be more surprising or irrational from a tax perspective than in the classic disposition effect literature (Odean, 1998).

On the other hand, individuals could also believe that they need to secure a tax advantage and hence be more inclined to sell. Alternatively, they could sell fictitious winners as they are actual losers. Thus, as an additional robustness check, we perform a sample split to include only individuals who were not affected by taxes. First, we can restrict our attention to individuals with portfolio sizes of less than 50,000 euros and additionally require that the to-date yearly capital gains be less than 1602 euros (this equals a household's tax-free allowance) or 801 euros (which equals an individual's tax-free allowance). However, in the trading and (therefore) consumption regressions, we do not find that tax considerations appear to matter, as seen in Table 12.

[Insert Table 12 about here]

7 Conclusion

We use a large sample of transaction-level data on all asset holdings, securities trades, spending, and income from clients of a German retail bank. We explore how the individual propensity to sell winners and hold losers as well as individual consumption respond to exogenous changes in the displayed purchase prices of funds. These changes in the displayed purchase prices also affect the funds' displayed returns and capital gains and losses. Our identification strategy exploits the implementation of a capital gains tax reform that facilitated the online bank's assessment of capital gains taxes. We can thus look at how individuals react to fictitious winners (funds that are displayed as winners but are actually losers) as well as fictitious losers (funds that are displayed as losers but

are actually winners) in their likelihood of selling and in their consumption. We document that people sell and consume up to 25% out of their fictitious gains conditional on the value of their liquidation.

Additionally, we document three more findings: 1) individuals respond much less to fictitious losers in their trading and barely at all in their consumption behavior, 2) individuals respond more when the fictitious gain is large and the actual loss is small, and 3) individuals respond even when the information about their actual capital gains is very salient. We argue that our collection of findings is driven by an interaction between two of the most-studied behavioral biases in retail financial markets: selective inattention (as documented in [Karlsson et al., 2009](#); [Olafsson and Pagel, 2018b](#); [Gherzi et al., 2014](#)) and the disposition effect (see [Odean, 1998](#); [Kaustia, 2010](#); [Frydman and Rangel, 2014](#); [Birru, 2015](#); [Chang et al., 2016](#); [Meng and Weng, 2017](#); [Frydman and Wang, 2019](#); [Gathergood et al., 2019](#)). We show that these two behavioral biases and their interaction effects are strong enough to affect not only trades but also consumption of stock market wealth. To the best of our knowledge, no paper links selective inattention and the disposition effect to consumption. Our findings show that individuals are selectively inattentive and engage in the disposition effect because they feel richer and thus consume more of the realized capital gains. This is consistent with attribution error (as in [Barber and Odean, 2002](#)), i.e., individuals are looking for an excuse to sell fictitious winners to consume more.

As a contribution to the literature on the disposition effect, we provide additional evidence for a preference for realizing (displayed) winners as opposed to losers and show that individuals care about the displayed purchase prices rather than the actual purchase prices. Our paper is related to [Frydman and Wang \(2019\)](#), who analyze the salience of new price variables and their colors in online portfolios. However, we also show that some investors may be confused about what their actual winners are, in line with the evidence in [Birru \(2015\)](#). Additionally, we provide evidence that winners and losers appear to be assessed relative to displayed prices, for which, to the best of our knowledge, only experimental evidence exists ([Frydman and Rangel, 2014](#)). Finally, we link the disposition effect to inattention as do the two contemporaneous papers by [Gathergood et al. \(2019\)](#) and [Birru \(2015\)](#) but study their interaction effects on consumption.

Finally, we show that behavioral biases in trading are important determinants of consumption out of stock market wealth ([Poterba, 2000](#); [Di Maggio et al., 2020](#)). This is important for the aggregate economy, as stock market wealth accounts for roughly one-quarter of household net worth. Showing that behavioral biases in the trading of individual funds positions matters for investor consumption may contribute to explaining why the marginal propensity to consume out of wealth is smaller in the aggregate than the estimates in many macroeconomic models suggest ([Poterba, 2000](#)).

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Name	Stück/Nominal	amount purchased	Kurs	Gesamtwert	Aktion
WKN		Einstandskurs inkl. NK	purchase Datum/ Zeit	Entwicklung absolut	absolute return
Gattung		Veränderung Intraday	price Handelsplatz	Entwicklung prozentual	relative return
VANG.GBL MO.AC.U.ETF DL	33,00	26,969394 EUR	27,305 EUR	901,06 EUR	Details
A14YC0		889,99 EUR	16.11.18 22:25:45	+11,08 EUR	Kaufen
ETF		-0,49 %	Tradegate	+1,24 %	Verkaufen
Gesamtdepotwert				901,06 EUR	

Investmentsteuerreform

Aufgrund der Investmentsteuerreform erhalten Ihre Fondsbestände zum 01.01.2018 neue Einstandskurse. Dadurch ändert sich Ihre Wertentwicklung. Die Umstellung ist voraussichtlich bis Anfang März 2018 abgeschlossen. In diesem Zeitraum treten eventuell Verzögerungen bei Ihren Verkaufsabrechnungen für Fonds auf. Über den Gegenwert Ihrer Fondsverkäufe können Sie im Rahmen von Neugeschäften verfügen. Externe Überweisungen des Verkaufserlöses sind ggf. nur telefonisch möglich. Die ursprünglichen Einstandskurse und Ihre Wertentwicklung zum 31.12.2017 werden Ihnen spätestens im 1. Quartal 2018 im Online-Archiv bereitgestellt.

Weitere Informationen

Diesen Hinweis nicht mehr anzeigen

OK >

Figure 1: Screenshots of the bank's portfolio interface and notification of repricings due to the capital gains tax reform, showing security (VANG:GBL.MO.AC.U.ETF), number of shares held (33,00), (fictitious) purchase price (26,969394), value of the purchased position (889,99), current price (27,305), current value of the position (901,06), absolute (fictitious) return (11,08), and relative (fictitious) return (1,24). The translation of the notification is as follows: because of the capital gains tax reform, your fund holdings will receive newly displayed purchase prices on 01.01.2018, and as a result, your returns will be different. All changes to our system will be completed by the beginning of March 2018, prior to which there may be delays in processing your sales receipts for funds. You can use the proceeds from any sale when doing new trades. External transfers of the sales proceeds are in some cases only possible via telephone. The actual purchase prices and your returns until 31.12.2017 will be provided to you online in the 1st quarter of 2018 at the latest.

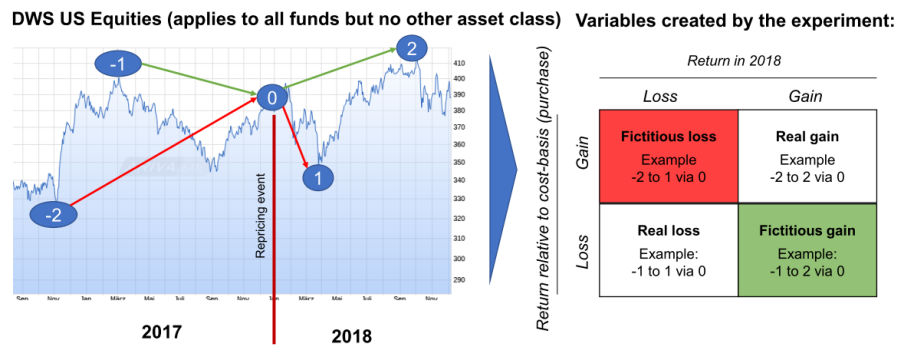


Figure 2: Gain, fictitious gain, loss, and fictitious loss variables created by the experiment: example fund

Max Mustermann
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2222 Musterstadt

ORDERABRECHNUNG

VERKAUF AM 08.04.2019 UM 21:22:37 AUSL.INVESTM.GESCH.

Bezeichnung		WKN	ISIN
FLOSSB.V.STORCH-MUL.OPP.R		A0M430	LU0323578657
ST	11,00000		
Kurs	251,620000 EUR P.ST.		

Nettoinventarwert		EUR	2.767,82
KAPST	24,51%	EUR	188,94
KIST	8,00%	EUR	15,11
SOLZ	5,50%	EUR	10,39
Wert 10.04.2019		EUR	2.553,38

zugunsten Konto-Nr. 007 Bestand zulasten
Girosammelverwahrung

CBF 2126

JAHRESSTEUERBESCHEINIGUNG FOLGT
Beratungsfreies Geschäft

Seite 2 vom 08.04.2019

Infos bei Gewinn Aktien/ Wertpapiere ungleich Aktien

Veräußerungsgewinn nach Differenzmethode	EUR	143,11
Summe ergibt		
KAPST-pflichtige Kapitalerträge	EUR	770,87
Mit Sparerpauschbetrag verrechnet	EUR	0,00
Bemessungsgrundlage für KAPST vor QUST	EUR	770,87
An Kapitalertrag anrechenbare, ausl. QUST 0,00*4	EUR	0,00
Bemessungsgrundlage für KAPST	EUR	770,87
Bemessungsgrundlage für KAPST anteilig 100,00%	EUR	770,87
Salden nach dem Geschäft		
Verrechnungstopf Aktien nach dem Geschäft	EUR	0,00
Verrechnungstopf Allg. nach dem Geschäft	EUR	0,00
Verrechnungstopf QUST nach dem Geschäft	EUR	0,00
Sparerpauschbetrag nach dem Geschäft	EUR	0,00
Details zur Investmentbesteuerung		
Teilfreistellungsbetrag	EUR	21,47
Teilfreistellungsquote		15,00%
Ergebnis fikt. Veräußerung 31.12.2017	EUR	648,07
Akkum. thesaurierte Erträge fikt. Veräußerung 31.12.2017	EUR	1,16

Figure 3: Sales receipt of an actual winning fund shown on page 1. The fund description (Bezeichnung), ISIN, number of securities sold (ST), current price (Kurs), current value of the liquidation gross of taxes and fees (Nettoinventarwert), capital gains taxes (KAPST), church taxes (KIST), solidary surcharge (SOLZ), and final liquidation value net of taxes and fees (Wert 10.04.2019). On page 2 (Seite 2), the receipt shows for funds (not stocks) the difference between the fictitious purchase value and sale value (Veräußerungsgewinn nach Differenzmethode), actual capital gains (Kapitalerträge) indicated as such and used as a tax basis, partial tax-free allowance (Teilfreistellungsbetrag), actual capital gains at the time of the resetting of the displayed purchase prices or fictitious sale (Ergebnis fikt. Veräußerung 31.12.2017), and accumulated retained capital gains at the fund level since the fictitious sales (Akkum. thesaurierte Erträge fikt. Veräußerung); note that the fictitious plus actual capital gains minus the tax-free allowance plus the retained capital gains equals the overall capital gains: $143.11+648.07-21.47+1.16 = 770.87$.

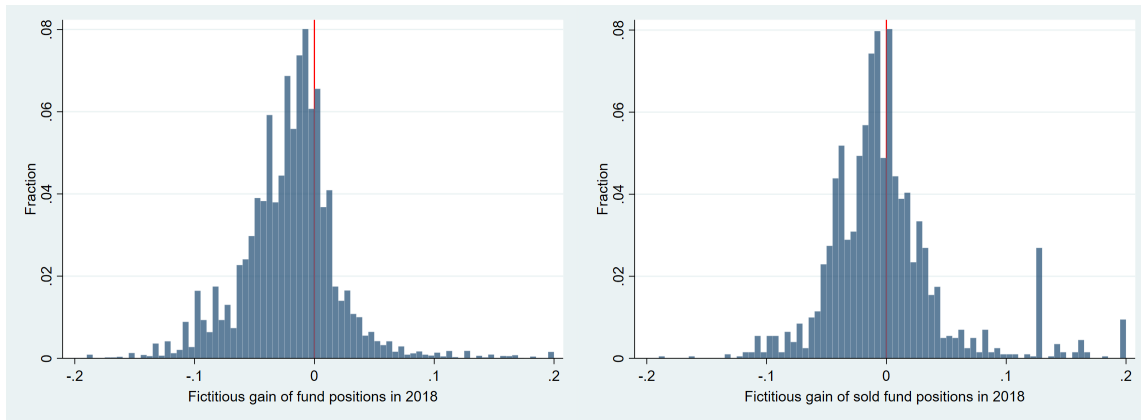


Figure 4: Distribution of the percentage “fictitious” (newly displayed since 12/29/2017 closing price as quoted by the exchange) returns of all (left) and sold (right) fund positions (returns of the sale price or using the 06/30/2018 prices relative to the 12/29/2017 closing price)

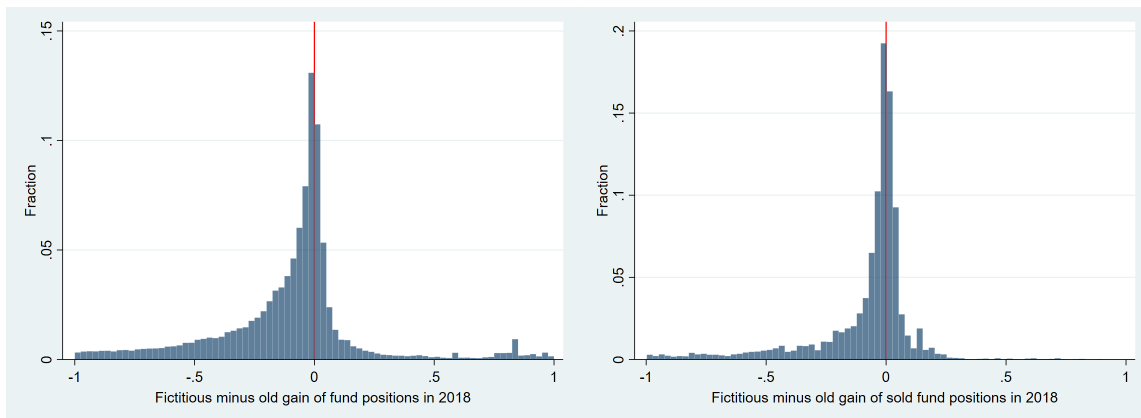


Figure 5: Distribution of the percentage “fictitious” (newly displayed since 12/29/2017 closing price as quoted by the exchange) returns of all (left) and only sold (right) fund positions (return of the sale price or using the 06/30/2018 prices relative to the 12/29/2017 closing price) minus their percentage “actual” returns (returns from their actual purchase prices to the 12/29/2017 closing price)

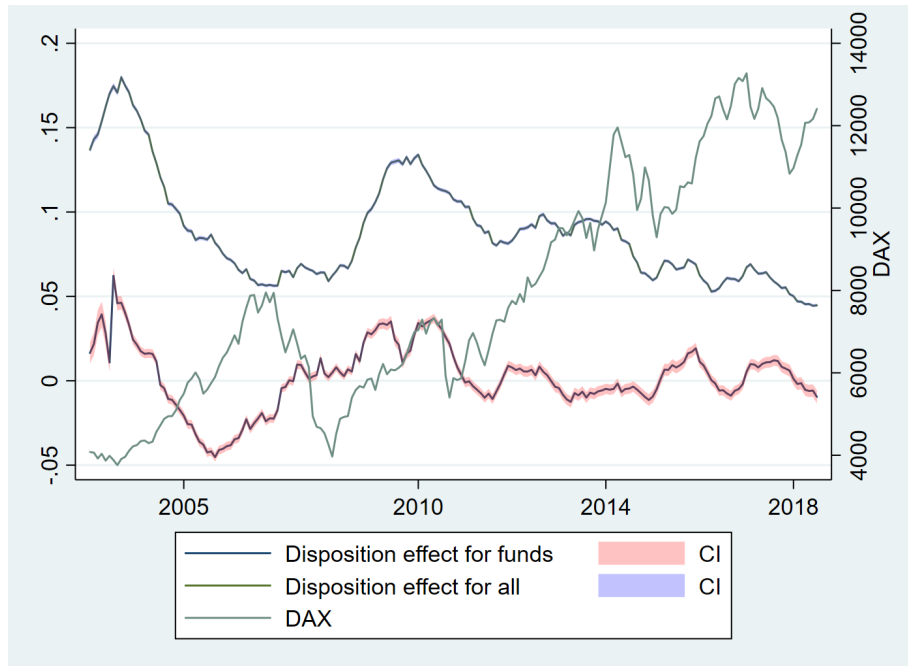


Figure 6: This figure follows the analysis in [Bernard et al. \(2018\)](#) in showing that the disposition effect for all securities (as well as the disposition effect for funds) appears to be countercyclical relative to the German stock market index (the DAX). We use 12-month rolling-window disposition effect regressions without any controls. The propensity to sell winning stocks is higher than the propensity to sell winning funds, and the two covary closely

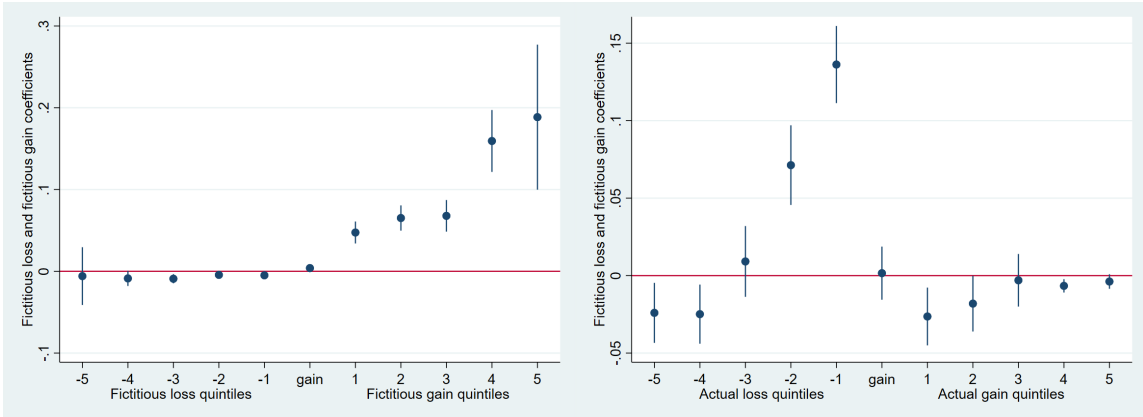


Figure 7: Fictitious loss and gain coefficients for five quintiles of the fictitious (newly displayed since 12/29/2017 closing price) or actual (relative to the actual purchase price) capital gains and losses in absolute euro values of Specification 1 (regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a fictitious winner, or a fictitious loser while controlling for individual and month-by-year fixed effects and clustering standard errors at the individual level), the “fictitious loss quintiles” display the coefficients of each fictitious loss quintile, the point labeled “gain” displays the coefficient of an actual winner, the “fictitious gain quintiles” display the coefficients of each fictitious gain quintile, the “actual loss quintiles” display the coefficients of the fictitious winner for each actual loss quintile, the “actual gain quintiles” display the coefficients of a fictitious loser for each actual gain quintile

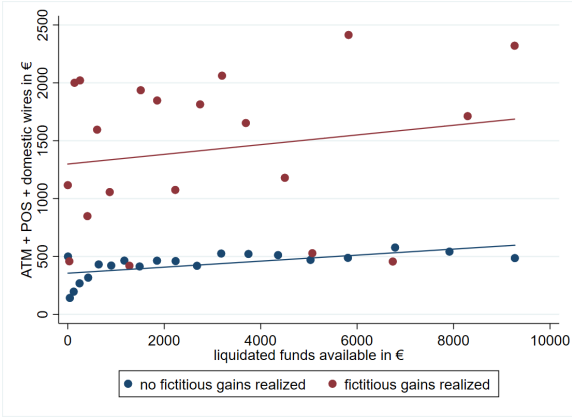


Figure 8: Nonparametric binscatter plot for consumption (ATM withdrawals plus POS transactions plus nonrecurring domestic wires) relative to the amount of available funds (liquidations minus reinvestments when they are positive but smaller than 10,000 euros) for individual-month combinations with and without fictitious capital gain liquidations

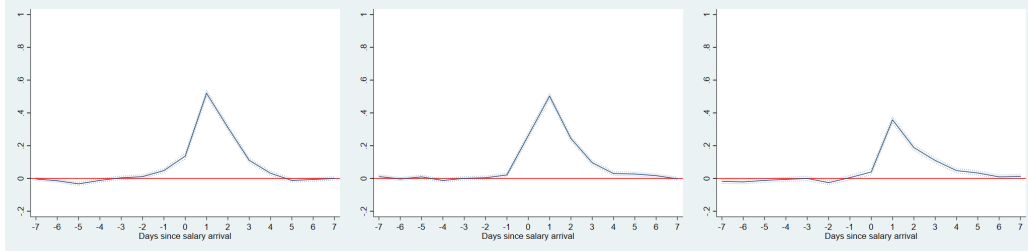


Figure 9: Payday responses of ATM withdrawals plus POS transactions plus nonrecurring, domestic wires (Lastschrift and Überweisungen) in the two weeks around salary receipt for three terciles of income (left side, low income; right side, high income) when controlling for individual, day-of-week, week-of-month, and month-by-year fixed effects. Standard errors are clustered at the individual level and displayed as dashed lines

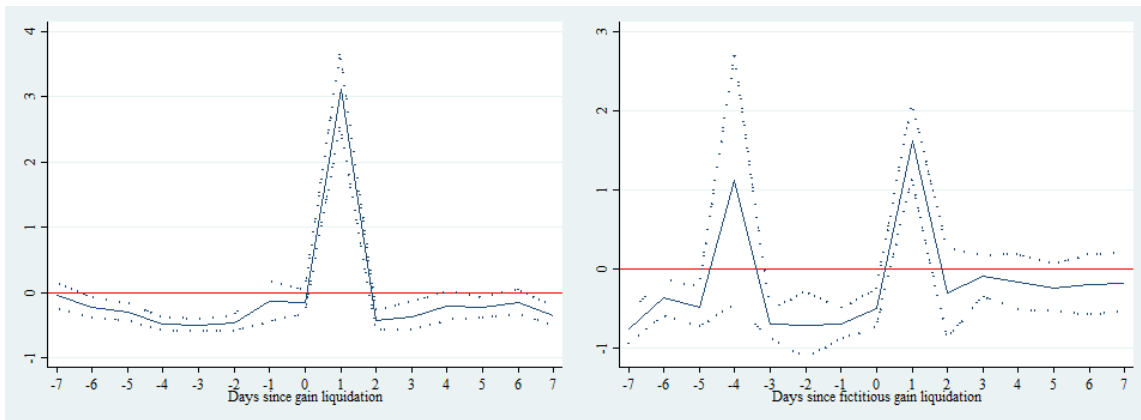


Figure 10: Responses of ATM withdrawals and POS transactions in the two weeks around liquidation of funds that were actual winners (left side) and fictitious winners (right side) when controlling for individual, day-of-week, week-of-month, and month-by-year fixed effects. Standard errors are clustered at the individual level and displayed as dashed lines

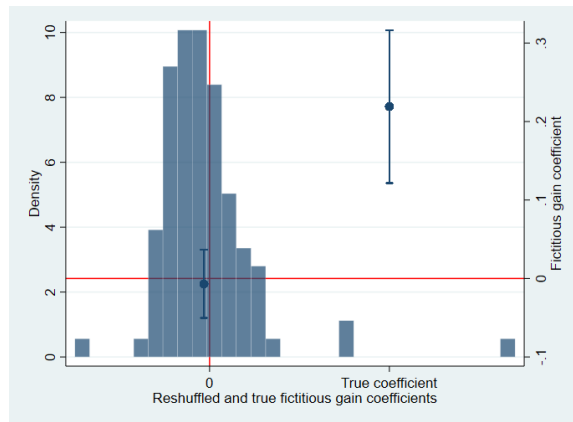


Figure 11: Distribution of fictitious gain coefficients for 100 reshuffled placebo regressions of Specification 2 (regressing consumption (ATM withdrawals plus POS transactions plus non-recurring domestic wires but reshuffling all variables among all investors) on fictitious capital gains while controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, and actual capital gains and losses, all aggregated at the monthly level, with standard errors clustered at the individual level), the average reshuffled coefficient and its average standard error, and the true fictitious gain coefficient with its standard error

Table 1: Summary statistics for all individuals and affected individuals for the years 2017 and 2018, i.e., individuals who held a fund in 2018

	mean	median	standard deviation	25th percentile	75th percentile	p-values of Wald tests
all investors						
age	47.56	50.00	18.77	40.00	60.00	
male	80.90%	100.00%	39.31%	100.00%	100.00%	
PhD educated	6.15%	0.00%	24.03%	0.00%	0.00%	
account tenure (in years)	12.57	14.00	5.87	11.00	14.00	
income	50,422.70	50,000.00	24,143.46	30,000.00	80,000.00	
wealth	46,798.48	45,000.00	46,956.91	20,000.00	45,000.00	
portfolio value	107,287.97	45,037.63	163,936.94	13,540.72	124,224.82	
number of logins	8.61	7.06	6.69	2.92	13.42	
HH index	0.21	0.10	0.25	0.03	0.28	
portfolio turnover	154.77%	69.69%	220.48%	36.13%	166.36%	
risk class of trades	3.49	4.38	1.98	3.75	4.97	
number of trades (funds)	10.51	1.00	30.29	0.00	11.00	
holding period (funds, in years)	2.90	1.44	3.64	0.00	4.78	
share of trades (in funds)	0.32	0.09	0.38	0.00	0.67	
share of portfolio value (in funds)	0.31	0.14	0.36	0.00	0.60	
disposition effect (for funds)	-0.04	0.00	0.21	0.00	0.00	
number of individuals	34,695					
affected investors						
age	48.24	50.00	17.56	41.00	59.00	0.00
male	80.72%	100.00%	39.45%	100.00%	100.00%	1.00
PhD educated	7.26%	0.00%	25.94%	0.00%	0.00%	1.00
account tenure (in years)	13.08	14.00	5.74	13.00	15.00	1.00
income	51,695.60	50,000.00	23,977.29	30,000.00	80,000.00	1.00
wealth	48,330.65	45,000.00	47,570.60	20,000.00	45,000.00	1.00
portfolio value	127,510.07	63,421.96	172,654.85	22,604.91	153,330.50	0.00
number of logins	8.30	6.49	6.65	2.68	12.87	0.87
HH index	0.08	0.04	0.12	0.01	0.10	0.00
portfolio turnover	98.31%	53.71%	146.06%	32.14%	99.60%	0.00
risk class of trades	3.37	4.20	1.96	3.00	4.80	0.00
number of trades (funds)	18.26	8.00	41.27	1.50	22.50	0.00
holding period (funds, in years)	4.19	3.46	3.42	1.55	6.33	0.00
share of trades (in funds)	0.51	0.52	0.38	0.12	0.91	0.00
share of portfolio value (in funds)	0.52	0.51	0.33	0.22	0.84	0.00
disposition effect (for funds)	-0.06	0.00	0.24	-0.10	0.00	1.00
number of individuals	16,275					

Notes: Account tenure is the length of the banking relationship. Income and wealth are self-reported statistics in brackets from a questionnaire upon account opening. The HH index is a measure of diversification ranging from 0 to 1 (above 0.5 implies holding a handful of stocks). Number of logins and trades are per year averages. Turnover equals 0.5 times the sum of all purchases per month divided by the end-of-month portfolio value plus 0.5 times the sum of all sales divided by the beginning-of-month portfolio value multiplied by 12 following [Barber and Odean \(2001\)](#). Risk class of trades is the average of the risk classification of traded securities (established by German regulation from 1 (money-market funds) to 5 (stocks, options, and futures)). The disposition effect is the propensity to realize winning minus losing funds and is calculated as in [Odean \(1998\)](#) using the actual purchase prices. The Wald tests' p-values result from a kitchen-sink regression on all except the self-reported characteristics testing whether their coefficient is zero (corrected for multiple hypothesis testing using the Bonferroni method).

Table 2: Descriptive statistics for consumption and liquidations for all investors for the years 2017 and 2018

	mean	standard deviation	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
ATM withdrawals	736.69	1,078.77	110.00	220.00	470.00	900.00	1,550.00
point-of-sale transactions	862.19	2,592.75	051.85	160.42	434.82	976.98	1,840.40
dividends received	624.01	9,901.23	005.39	022.67	094.13	352.01	1,032.55
trading fees	23.09	117.20	0.70	1.40	5.95	29.00	0,054.95
nonrecurring wire transfers	1,691.03	12,396.22	51.19	200.00	635.87	1,617.20	3,366.59
salary received	3,871.91	13,004.80	772.97	1,814.31	2,836.78	4,338.33	6,731.89
recurring wire transfers	1,247.33	1,859.91	100.00	300.00	780.00	1,500.00	2,840.00
capital gains actual winner	9,061.99	148,811.39	007.09	056.50	449.90	3,136.40	12,948.92
capital gains fictitious winner	294.27	668.57	000.80	004.65	41.47	257.40	956.40
capital loss actual loser	-70,117.70	2,488,005.50	-2,329.13	-510.00	-120.00	-24.96	-3.18
capital loss fictitious loser	-9,351.86	224,010.02	-15,815.98	-6,447.00	-2,457.18	-613.11	-50.64
liquidation net of reinvestment	2,629.31	94,515.05	-6,284.69	-976.87	-133.08	633.78	10,272.00

Notes: These summary statistics are at the individual investor level and aggregated for each month for the time period January 2017 and to the end of June 2018, conditional on being nonzero in a given month. Point-of-sale transactions are debit or credit card transactions in (online) stores. Wire transfers are a common way to pay for online goods and services in Germany or to pay recurring expenses such as rent. Salaries are flagged as such in the German transaction system. Both spending and income are positive variables in our specifications. All capital gains and losses are realized (liquidated) and aggregated to the monthly level. Fictitious capital gains or losses equal those displayed on the online brokerage platform at the time of liquidation and only occur in 2018 for funds. Actual capital gains and losses as well as the liquidation net of reinvestment measures stem from all securities.

Table 3: Proportion of gains realized versus proportion of losses realized as in [Odean \(1998\)](#)

	mean	standard deviation	25th percentile	75th percentile
PGR: proportion of gains realized	0.128	0.151	0.045	0.148
PLR: proportion of losses realized	0.077	0.131	0.020	0.077
PGR minus PGL	0.050	0.148	0.004	0.082

Notes: The proportion of realized gains is defined as the number of realized gains relative to the number of all (realized and paper) gains in the portfolio and the proportion of realized losses is defined as the number of realized losses relative to the number of all (realized and paper) losses in the portfolio. The disposition effect then equals the difference between the two

Table 4: Estimation results of probability of sale on dummies for the security being a winner, a fictitious winner, or a fictitious loser

	all securities full sample	funds full sample	funds 2018	funds 2018	funds 2018
	sale	sale	sale	sale	sale
gain	0.0607*** (0.000808)	-0.00312*** (0.000732)	0.00771*** (0.00198)	0.00797*** (0.00197)	-0.00341 (0.00251)
fictitious gain			0.0409*** (0.00408)	0.0426*** (0.00407)	0.0506*** (0.00456)
fictitious loss			-0.00824*** (0.00211)	-0.00463** (0.00198)	-1.49e-06 (0.00217)
individual fixed effects			✓	✓	✓
month-by-year fixed effects				✓	✓
funds fixed effects					✓
observations	43,945,085	6,325,451	126,249	126,249	123,383
R squared	0.007	0.000	0.271	0.271	0.278

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
 Notes: Specification 1 regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a fictitious winner, or a fictitious loser (as indicated) controlling for individual, month-by-year, or fund fixed effects (as indicated)

Table 5: Estimation results of different measures of consumption on liquidation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses

	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018	main customers all securities 2017 and 2018	all securities 2017 and 2018	main customers all securities 2017 and 2018
	ATM + POS in €	ATM + POS in €	ATM + POS in €	ATM + POS in €	ATM + POS + wires in €	ATM + POS + wires in €
fictitious gain in €	0.206** (0.0848)	0.203** (0.0844)	0.157** (0.0725)	0.299* (0.157)	0.245** (0.0993)	0.490** (0.230)
fictitious loss in €	-5.79e-05 (4.87e-05)	-5.48e-05 (4.77e-05)	-0.000116** (5.10e-05)	-0.000375* (0.000223)	-0.000282 (0.000175)	-0.00122* (0.000643)
gain in €		✓	✓	✓	✓	✓
loss in €		✓	✓	✓	✓	✓
liquidation minus reinvestment in €			✓	✓	✓	✓
individual fixed effects			✓	✓	✓	✓
month-by-year fixed effects			✓	✓	✓	✓
observations	609,693	609,693	609,693	141,442	609,693	141,442
R squared	0.000	0.004	0.241	0.243	0.135	0.122

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus nonrecurring national wires (as indicated)) on fictitious capital gains and losses controlling for individual and month-by-year fixed effects as well as the available liquidated amounts all aggregated to the monthly level

Table 6: Estimation results of consumption (ATM plus POS plus wires) on liquidation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses

	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2018
	ATM + POS + wires deviation from mean	ATM + POS + wires deviation from mean	ATM + POS + wires deviation from mean	ATM + POS + wires deviation from mean
fictitious gain (deviation from mean)	0.158*** (0.0168)	0.157*** (0.0182)	0.154*** (0.0182)	0.155*** (0.0202)
fictitious loss (deviation from mean)	-5.87e-06 (9.95e-06)	-5.78e-06 (1.02e-05)	-5.12e-06 (1.02e-05)	8.12e-06 (6.93e-06)
gain (deviation from mean)	0.282***	0.276***	0.266***	0.259***
loss (deviation from mean)	0.230***	0.226***	0.222***	0.219***
liquidation minus reinvestment (deviation from mean)		✓	✓	✓
month-by-year fixed effects		✓	✓	✓
individual fixed effects			✓	✓
liquidation dummy			✓	✓
observations	609,693	609,693	609,693	216,513
R squared	0.203	0.247	0.250	0.340

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus nonrecurring national wires (outcome variable transformed as indicated)) on fictitious capital gains controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, and actual capital gains and losses all aggregated to the monthly level; capital gains are positive and capital losses are negative and the signs are preserved in the deviation from mean transformations

Table 7: Placebo estimation results of probability of sale on dummies for the security (nonfunds in 2017 or 2018 or funds in 2017) being a winner, a fictitious winner, or a fictitious loser

	nonfunds 2018	nonfunds 2017	funds 2017	funds 2017	funds 2017
	sale	sale	sale	sale	sale
gain	0.0173*** (0.000700)	0.0147*** (0.000802)	0.00203 (0.00326)	0.000436 (0.00327)	-0.0150*** (0.00376)
fictitious gain	0.0180*** (0.00108)	0.0155*** (0.00102)	0.0210*** (0.00387)	0.0191*** (0.00398)	0.0124*** (0.00406)
fictitious loss	0.000342 (0.000759)	0.00247*** (0.000783)	-0.00479* (0.00283)	-0.00563** (0.00287)	-0.00457* (0.00273)
individual fixed effects	✓	✓	✓	✓	✓
month-by-year fixed effects				✓	✓
funds fixed effects					✓
observations	1,178,397	1,177,202	146,261	146,261	143,306
R squared	0.098	0.106	0.254	0.254	0.254

Standard errors (clustered at the individual level) in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Notes: Specification 1 regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a (placebo as indicated) fictitious winner, or a (placebo as indicated) fictitious loser controlling for individual, month-by-year, or fund fixed effects (as indicated)

Table 8: Placebo estimation results of different measures of consumption on liquidation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses in 2017

	all securities 2017	main customers all securities 2017	all securities 2017	main customers all securities 2017
	ATM + POS in €	ATM + POS in €	ATM + POS + wires in €	ATM + POS + wires in €
fictitious gain in €	0.00854 (0.00720)	0.00731 (0.00795)	0.0166 (0.0145)	0.0163 (0.0174)
fictitious loss in €	-0.000599 (0.000657)	-0.00443** (0.00225)	-0.000988** (0.000500)	-0.00304 (0.00327)
gain in €	✓	✓	✓	✓
loss in €	✓	✓	✓	✓
liquidation minus reinvestment in €	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓
observations	546290	34259	546290	34259
R squared	0.312	0.532	0.312	0.373

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus nonrecurring national wires (as indicated)) on placebo fictitious capital gains (sale price relative to the December 30, 2016 price) controlling for individual and month-by-year fixed effects as well as the available liquidated amounts and actual capital gains and losses all aggregated to the monthly level

Table 9: Estimation results of probability of sale on dummies for the security (2017 and 2018 January to June) being a winner, a (placebo) fictitious winner, or a (placebo) fictitious loser interacted with a dummy for a fund sale in 2018

	funds 2017 and 2018	funds 2017 and 2018	funds 2017 and 2018	funds 2017 and 2018
	sale	sale	sale	sale
gain	0.00637*** (0.00130)	0.00636*** (0.00131)	0.00633*** (0.00164)	-0.00555*** (0.00200)
fictitious gain	0.0235*** (0.00231)	0.0233*** (0.00233)	0.0274*** (0.00289)	0.0277*** (0.00290)
fictitious gain times 2018	0.0148*** (0.00364)	0.0162*** (0.00366)	0.0137*** (0.00386)	0.0177*** (0.00386)
fictitious loss	-0.00600*** (0.00159)	-0.00585*** (0.00163)	-0.000929 (0.00216)	0.00558*** (0.00206)
fictitious loss times 2018	-0.00182 (0.00197)	0.00125 (0.00210)	-0.00472** (0.00217)	-0.00607*** (0.00232)
month-by-year fixed effects		✓	✓	✓
individual fixed effects			✓	✓
funds fixed effects				✓
observations	272,510	272,510	269,591	269,189
R squared	0.002	0.003	0.191	0.218

Standard errors (clustered at the individual level) in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Notes: Specification 1 regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a fictitious winner, or a fictitious loser (either the 2018 status or the 2017 placebo status) as well as an interaction for the year being 2018 controlling for individual, month-by-year, or fund fixed effects (as indicated)

Table 10: Estimation results of probability of sale on dummies for the security being a winner, a fictitious winner, or a fictitious loser, the fictitious winner dummy is interacted with different measures for investor's uninformedness in 2018

	funds 2018	funds 2018	funds 2018	funds 2018	funds 2018	funds 2018
	sale	sale	sale	sale	sale	sale
gain	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)	-0.003 (0.003)
fictitious gain	0.038*** (0.005)	0.048*** (0.005)	0.042*** (0.005)	0.035*** (0.006)	0.053*** (0.006)	0.057*** (0.006)
fictitious loss	-0.000 (0.002)	-0.000 (0.002)	-0.000 (0.002)	-0.000 (0.002)	0.000 (0.002)	-0.000 (0.002)
fictitious gain times early 2018	0.060*** (0.005)					
fictitious gain times first five trades		0.062*** (0.009)				
fictitious gain times first half of trades			0.058*** (0.005)			
fictitious gain times traded little				0.058*** (0.005)		
fictitious gain times savings plan trader					0.049*** (0.005)	
fictitious gain times inattentive trader						0.042*** (0.005)
month-by-year fixed effects	✓	✓	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓	✓	✓
funds fixed effects	✓	✓	✓	✓	✓	✓
observations	123383	123383	123383	123383	123383	123383
R squared	0.184	0.184	0.184	0.184	0.184	0.184

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Specification 1 regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a fictitious winner, or a fictitious loser, the fictitious winner variable is interacted with an indicator for the subset of each individual's first five trades, for the subset of each individual's first half of trades in 2018, for the subset of individuals that traded little in 2018, for those individuals that have savings plan transactions, and for traders with less than median logins relative to trades controlling for individual, month-by-year, and funds fixed effects (as indicated)

Table 11: Estimation results of consumption (ATM plus POS) on liquidation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses with an early 2018 indicator, with each investor's first five and first half trades indicator as well as an interaction with an indicator for the infrequently trading investors

	funds 2018	funds 2018	funds 2018	funds 2018	funds 2018	funds 2018
	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €
fictitious gain in €	0.183 (0.127)	0.032 (0.097)	0.095 (0.081)	0.196 (0.166)	0.211 (0.143)	0.335** (0.151)
fictitious gain in € early 2018 indicator	0.589** (0.299)					
fictitious gain in € first five trades		0.278** (0.131)				
fictitious gain in € first half of trades			0.683*** (0.252)			
fictitious gain in € infrequent trader				0.320 (0.199)		
fictitious gain in € savings plan trader					0.373* (0.218)	
fictitious gain in € inattentive trader						0.053 (0.061)
gain in €	✓	✓	✓	✓	✓	✓
loss in €	✓	✓	✓	✓	✓	✓
fictitious loss in €	✓	✓	✓	✓	✓	✓
liquidation minus reinvestment in €	✓	✓	✓	✓	✓	✓
liquidation dummy	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓	✓	✓
observations	216513	216513	216513	216513	216513	216513
R squared	0.214	0.214	0.214	0.214	0.214	0.214

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires) on fictitious capital gains with different interaction variables indicating an early trade (before April 1, 2018), one of the first five trades of each individual, one of the first half of trades of each individual, infrequent traders, a trader with savings plan transactions, and a trader with less than median logins relative to trades (as indicated) controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, and actual capital gains and losses all aggregated to the monthly level

Table 12: Estimation results of probability of sale on dummies for the security being a winner, a fictitious winner, or a fictitious loser for individuals with portfolio sizes of less than 50,000€ and year-to-date capital gains of less than 1,602€

	funds	funds	funds	funds	funds capital gains < 801€
	sale	sale	sale	sale	sale
gain	0.0157*** (0.00298)	0.0132*** (0.00310)	0.0106*** (0.00310)	-0.00925** (0.00428)	-0.00859** (0.00432)
fictitious gain	0.0394*** (0.00456)	0.0377*** (0.00450)	0.0410*** (0.00470)	0.0453*** (0.00574)	0.0422*** (0.00573)
fictitious loss	0.00505* (0.00266)	0.00498* (0.00266)	0.00661** (0.00267)	-0.00732** (0.00372)	-0.00614 (0.00380)
month-by-year fixed effects		✓	✓	✓	✓
individual fixed effects			✓	✓	✓
funds fixed effects				✓	✓
observations	49,980	49,980	48,508	48,115	46,126
R squared	0.003	0.003	0.283	0.332	0.331

Standard errors (clustered at the individual level) in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Notes: Specification 1 regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a fictitious winner, or a fictitious loser for subsets of individuals with portfolio sizes of less than 50,000€ and the year-to-date capital gains less than 1,602€ (household's tax free allowance) or 801€ (individual's tax free allowance) (as indicated) controlling for individual, month-by-year, or fund fixed effects (as indicated)

A Additional tables

Table A.13: Estimation results of probability of sale on dummies for the security being a winner, a fictitious winner, or a fictitious loser

	all securities full sample	funds full sample	funds 2018	funds 2018	funds 2018
	sale	sale	sale	sale	sale
gain	0.0607*** (0.00262)	-0.00312 (0.00316)	0.00519 (0.00533)	0.00797* (0.00389)	-0.00341 (0.00592)
fictitious gain			0.0371* (0.0146)	0.0426** (0.0108)	0.0506*** (0.00934)
fictitious loss			-0.00825 (0.00528)	-0.00463 (0.00420)	-1.49e-06 (0.00578)
individual fixed effects				✓	✓
month-by-year fixed effects				✓	✓
funds fixed effects					✓
observations	43,945,085	6,325,451	126,249	123,836	123,383
R squared	0.007	0.000	0.003	0.234	0.278

Standard errors (double-clustered at the individual and month-by-year levels) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 1 regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a fictitious winner, or a fictitious loser (as indicated) controlling for individual, month-by-year, or fund fixed effects (as indicated)

Table A.14: Estimation results of different measures of consumption on liquidation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses

	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018	main customers all securities 2017 and 2018	all securities 2017 and 2018	main customers all securities 2017 and 2018
	ATM + POS in €	ATM + POS in €	ATM + POS in €	ATM + POS in €	ATM + POS + wires in €	ATM + POS + wires in €
fictitious gain in €	0.206*** (0.0503)	0.203*** (0.0582)	0.157** (0.0632)	0.299*** (0.1000)	0.245*** (0.0831)	0.490* (0.243)
fictitious loss in €	-5.79e-05 (4.61e-05)	-5.48e-05 (4.49e-05)	-0.000117** (5.44e-05)	-0.000375 (0.000247)	-0.000282 (0.000164)	-0.00122* (0.000672)
gain in €		✓	✓	✓	✓	✓
loss in €		✓	✓	✓	✓	✓
liquidation minus reinvestment in €			✓	✓	✓	✓
individual fixed effects			✓	✓	✓	✓
month-by-year fixed effects			✓	✓	✓	✓
observations	609,693	609,693	598,924	141,442	598,924	141,442
R squared	0.000	0.004	0.232	0.243	0.133	0.122

Standard errors (double-clustered at the individual and month-by-year levels) in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus nonrecurring national wires (as indicated)) on fictitious capital gains and losses controlling for individual and month-by-year fixed effects as well as the available liquidated amounts all aggregated to the monthly level

Table A.15: Estimation results of consumption (ATM plus POS) on liquidation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses with an early 2018 indicator, with each investor's first five and first half trades indicator as well as an interaction with an indicator for the infrequently trading investors

	funds 2018	funds 2018	funds 2018	funds 2018	funds 2018	funds 2018
	ATM + POS + wires dev from mean	ATM + POS + wires dev from mean	ATM + POS + wires dev from mean	ATM + POS + wires dev from mean	ATM + POS + wires dev from mean	ATM + POS + wires dev from mean
fictitious gain dev from mean	0.169*** (0.031)	0.010 (0.024)	0.188*** (0.027)	0.047** (0.023)	0.063** (0.032)	0.178*** (0.024)
fictitious gain early 2018 indicator	0.148*** (0.025)					
fictitious gain first five trades		0.161*** (0.021)				
fictitious gain first half of trades			0.119*** (0.026)			
fictitious gain infrequent trader				0.218*** (0.028)		
fictitious gain savings plan trader					0.169*** (0.023)	
fictitious gain inattentive trader						0.074** (0.029)
gain dev from mean	✓	✓	✓	✓	✓	✓
loss dev from mean	✓	✓	✓	✓	✓	✓
fictitious loss dev from mean	✓	✓	✓	✓	✓	✓
liquidation minus reinvestment dev from mean	✓	✓	✓	✓	✓	✓
liquidation dummy month-by-year fixed effects	✓	✓	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓	✓	✓
observations	216513	216513	216513	216513	216513	216513
R squared	0.125	0.125	0.125	0.126	0.125	0.125

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires) on fictitious capital gains with different interaction variables indicating an early trade (before April 1, 2018), one of the first five trades of each individual, one of the first half of trades of each individual, infrequent traders (as indicated), a trader with savings plan transactions, and a trader with less than median logins relative to trades (as indicated) controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, and actual capital gains and losses all aggregated to the monthly level

Table A.16: Estimation results of consumption (ATM plus POS plus wires) on liquidation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses and additional control variables

	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018
	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €
fictitious gain in €	0.520*** (0.192)	0.499*** (0.190)	0.380** (0.148)	0.349** (0.146)	0.332** (0.145)	0.333** (0.145)	0.333** (0.145)
fictitious loss in €	-0.000242 (0.000208)	-0.000241 (0.000207)	-0.000320 (0.000206)	-0.000319 (0.000203)	-0.000279 (0.000173)	-0.000279 (0.000174)	-0.000279 (0.000174)
gain in €	✓	✓	✓	✓	✓	✓	✓
loss in €	✓	✓	✓	✓	✓	✓	✓
liquidation minus reinvestment in €	✓	✓	✓	✓	✓	✓	✓
month-by-year fixed effects		✓	✓	✓	✓	✓	✓
individual fixed effects			✓	✓	✓	✓	✓
liquidation dummy				✓	✓	✓	✓
salary received					✓	✓	✓
dividends received						✓	✓
interest received							✓
observations	609,693	609,693	609,693	609,693	609,693	609,693	609,693
R squared	0.001	0.001	0.151	0.154	0.155	0.155	0.155

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus nonrecurring national wires) on fictitious capital gains controlling for individual and month-by-year fixed effects (as indicated) as well as the available liquidated amounts, a dummy for liquidations, actual capital gains and losses, regular salaries, dividend payments, and interest payments (as indicated) all aggregated to the monthly level

Table A.17: Estimation results of consumption (ATM plus POS) on liquidation amounts less reinvestment of all securities as well as realized and unrealized capital gains, fictitious capital gains, and fictitious capital losses

	funds 2017 and 2018	funds 2017 and 2018	funds 2017 and 2018	funds 2017 and 2018	funds 2017 and 2018
	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €	ATM + POS + wires in €
gain in €	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
fictitious gain in €	0.219** (0.097)	0.219** (0.097)	0.215** (0.097)	0.215** (0.097)	0.222** (0.098)
loss in €	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
fictitious loss in €	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000* (0.000)	-0.000*** (0.000)
unrealized gain in €		0.000* (0.000)	0.000* (0.000)	0.000** (0.000)	0.000** (0.000)
unrealized fictitious gain in €			0.030 (0.024)	0.029 (0.024)	0.029 (0.026)
unrealized loss in €				-0.000*** (0.000)	-0.000*** (0.000)
unrealized fictitious loss in €					-0.000*** (0.000)
liquidation minus reinvestment in €	✓	✓	✓	✓	✓
liquidation dummy	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓	✓
observations	609693	609693	609693	609693	609693
R squared	0.027	0.028	0.028	0.028	0.028

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires) on fictitious capital gains controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, actual capital gains and losses, and unrealized fictitious as well as actual capital gains and losses (as indicated) all aggregated to the monthly level

Table A.18: Placebo estimation results of probability of sale on dummies for the security being a winner, a placebo fictitious winner (i.e., an actual loser but a winner relative to the closing price of the previous year), or a placebo fictitious loser (i.e., an actual winner but a loser relative to the closing price of the previous year) for funds and all years 2004 to 2016

	funds 2004	funds 2005	funds 2006	funds 2007	funds 2008	funds 2009	nonfunds 2009
	sale	sale	sale	sale	sale	sale	sale
gain	0.0111*** (0.00358)	0.00509** (0.00244)	0.0221*** (0.00219)	0.0209*** (0.00205)	0.0298*** (0.00227)	0.0538*** (0.00582)	0.0526*** (0.00129)
fictitious gain	0.0115*** (0.00379)	0.0320*** (0.00274)	0.0304*** (0.00256)	-0.0287*** (0.00285)	-0.0768*** (0.00503)	0.0299*** (0.00612)	0.00368** (0.00170)
fictitious loss	0.00995*** (0.00283)	-0.00273 (0.00207)	0.00679*** (0.00174)	-0.0263*** (0.00166)	-0.0299*** (0.00328)	0.0279*** (0.00585)	0.00897*** (0.00165)
individual fixed effects	✓	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓	✓
observations	145,017	194,664	313,660	386,268	233,172	109,790	641,535
R squared	0.265	0.243	0.210	0.186	0.214	0.314	0.129
	funds 2010	funds 2011	funds 2012	funds 2013	nonfunds 2014	funds 2015	funds 2016
	sale	sale	sale	sale	sale	sale	sale
gain	0.00797*** (0.00256)	0.00503*** (0.00163)	0.0169*** (0.00393)	0.0112*** (0.00221)	0.000452 (0.00223)	0.0275*** (0.00470)	0.0122*** (0.00170)
fictitious gain	-0.00438 (0.00271)	-0.0121*** (0.00304)	0.00175 (0.00419)	0.00122 (0.00287)	0.00196 (0.00294)	0.0256*** (0.00513)	-0.00362 (0.00324)
fictitious loss	-0.00457** (0.00214)	-0.00235 (0.00256)	-0.00381 (0.00361)	-0.00806*** (0.00205)	-0.00714*** (0.00199)	-0.0102** (0.00447)	-0.00952*** (0.00235)
individual fixed effects	✓	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓	✓
observations	160,613	170,777	127,650	163,594	146,563	172,719	129,461
R squared	0.308	0.281	0.302	0.259	0.261	0.269	0.249

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: Specification 1 regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a placebo fictitious winner, or a placebo fictitious loser controlling for individual and month-by-year fixed effects