Consumption out of Fictitious Capital Gains and Selective Inattention*

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June 20, 2022

Abstract

Do retail investors' behavioral biases in trading affect their consumption? We exploit a natural experiment that changed the displayed purchase prices in investors' online portfolios. We find that investors readily sell and consume "fictitious" capital gains: displayed capital gains based on the new purchase prices that are truly capital losses based on the actual purchase prices. We argue that investors are selectively inattentive: they sell more fictitious winners when fictitious gains are larger and actual losses are smaller, they sell them even when actual purchase prices are very salient, but they notice fictitious losers, treating them the same as actual winners.

JEL codes: G5, D90, G41, D14

*We thank Adam Szeidl, Botond Köszegi, Chris Mayer, Brad Barber, Stijn Van Nieuwerburgh, Florian Peters, Neng Wang, Rawley Heimer, Alexander Michaelides, Paul Tetlock, Gur Huberman, Markku Kaustia, Ivo Welch, Samuli Knüpfer, Valentin Haddad, Camelia Kuhnen, Michael Weber, Cameron Peng, Christine Laudenbach, Alex Imas, Johannes Maier, Simeon Schudy, and Martin Weber as well as seminar and conference participants at the Chicago Booth Household Finance Conference, AEA, 2nd Annual Conference for Women in Economics at Princeton, AQR Institute Academic Symposium 2019, Cornell IBHF Symposium, WashU Household Finance Conference, UCLA Anderson, University of Rotterdam, ESMT, University of Mannheim, Endless Summer Conference, New Consumption Data Workshop in Copenhagen, Red Rock Finance Conference, MIT Junior Finance Conference, SFS Cavalcade, Norwegian Business School, University of Georgia, Columbia, and Rising 5 Star for valuable comments. This research would not have been possible without the collaboration of a German bank. We gratefully acknowledge the bank's data provision and thank all the bank's employees who helped us.

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1 Introduction

Retail investors are subject to behavioral biases in their trading and investing. Two biases in particular stand out in the recent literature: First, investors are subject to the so-called disposition effect (see Odean, 1998; Birru, 2015; Chang et al., 2016; Frydman and Wang, 2020; Ben-David and Hirshleifer, 2012; Kaustia, 2010; Frydman and Rangel, 2014; Gherzi et al., 2014); that is, they tend to sell winning investments and hold on to losing ones. Second, individual investors are selectively inattentive (Karlsson et al., 2009; Golman et al., 2017; Olafsson and Pagel, 2017; Andries and Haddad, 2020; Quispe-Torreblanca et al., 2020)—they pay attention to capital gains rather than losses. Two recent papers discuss how these two behavioral biases interact (Quispe-Torreblanca et al., 2021; Birru et al., 2019). In this paper, we explore how behavioral biases in investing affect consumption. We find that individuals are selectively inattentive to capital gains and sell what we call "fictitious winners" in order to consume more. Therefore, behavioral biases affect utility directly through consumption rather than only indirectly through wealth.

We thus show that behavioral biases are not constrained to the domain of individuals trading in their brokerage accounts; the biases also influence individual consumption from 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 (Campbell, 2006; Poterba, 2000; Di Maggio et al., 2020). Additionally, consumption is a direct component of utility and welfare in most economic models (Attanasio, 1999).

We use transaction-level data from a German online retail bank. The data, spanning from January 2003 to June 2018, 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. 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 test whether individuals selectively pay attention to, and have a disposition to, realize capital gains and whether this affects consumption.

To achieve this setting, we exploit a natural experiment: the 2018 capital gains tax reform in Germany. Here is what happened: on January 1, 2018, the majority of German banks, including the one we study here, changed the displayed purchase prices of all funds and ETFs (but no other investments) in individuals' online portfolios. The newly displayed purchase prices were the December 29, 2017, closing prices as quoted by the exchange. In turn, these new purchase prices determined the displayed absolute capital gains and losses as well as the displayed percentage returns. Importantly, the reform did not have any real implications

for individual tax liabilities. The reform did not change the capital gains tax rates or the actual purchase prices or cost bases of any investments.

The change of displayed purchase prices created what we call "fictitious winners and losers." The investor sees a fictitious winner if they are shown a capital gain based on the newly displayed purchase price when, in reality, they are subject to a capital loss. The opposite holds for fictitious losers: the investor is shown a capital loss based on the newly displayed purchase price when the holding actually carries a capital gain.

Investors are informed about their actual capital gains or losses in their order summaries, sales receipts, and bank communications. Theoretically, we would expect that an investor's consumption responds only to their total amount of liquidations and to their actual capital gains; that is, the investor's consumption plan and their wealth. We would not expect that the investor's trading and consumption respond to their investments' fictitious capital gains or losses (when we control for their actual capital gains).

We estimate the effects of fictitious capital gains and losses on individual investors' trades by using a linear probability model conditional on trading, 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 about 5% higher when it is shown as having a fictitious gain, even though it is a loser based on the actual purchase price; and (2) individuals' likelihood of selling a fund is barely changed when it is displayed as a fictitious loser; that is, individuals treat fictitious losers the same as actual winners.[1] The propensity to sell fictitiously winning funds is monotonically increasing in the fictitious gain and monotonically decreasing in the actual loss, and, overall, of similar magnitude as the disposition to sell winning stocks.[2] We argue that these findings can be explained by the interaction between selective inattention and the disposition effect, which results in attribution error (as in Barber and Odean, 2002); that is, individuals are looking for a reason to sell losing funds when they are fictitiously displayed as winners.

We measure consumption using transactions flagged as ATM withdrawals, point-of-sale (POS) transactions, and, in additional specifications, non-recurring domestic wires. We then estimate the marginal propensity to consume (MPC) out of each euro of liquidated fictitious capital gains using two approaches. We first run a simple OLS regression of monthly consumption on liquidated fictitious capital gains control-

[1]Depending 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.

[2]The propensity to sell fictitious winners 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). In rolling-window disposition-effect regressions (see Subsection 5.1 Figure 6) we find 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.

ling for the total value of liquidations and individuals' overall portfolio wealth.[3] We find that individuals consume up to 16.7% of each euro of realized fictitious capital gains. Our estimated MPC is in line with the high MPCs out of realized capital gains reported in Baker et al. (2007) and Di Maggio et al. (2020), as well as the high MPCs commonly estimated out of transitory income shocks (see Jappelli and Pistaferri (2010) for a literature survey).

To address the concern that individuals decide to liquidate fictitious gains, we implement a two-stage instrumental-variable (2SLS IV) approach to estimate the effect of fictitious capital gains on consumption. In the first stage, we estimate the propensity to realize fictitious capital gains by regressing the realized fictitious capital gains on the total potential fictitious capital gains an investor could have realized in a given month. To construct this variable, we sum up the end-of-the-month fictitious capital gains of all investor's beginning-of-the-month fund positions (independent of whether or not they were liquidated) in every month. To account for fluctuations in all aggregate variables such as stock market performance or consumer confidence, we control for month-by-year fixed effects. In the second stage, we then use the predicted realized fictitious capital gains to estimate individuals' MPCs. We can also control for actual capital gains and losses, total liquidations, and individual portfolio values. We also implement an alternative IV specification using an indicator variable for an investor having any fictitious capital gain in a given month (see, e.g. Parker et al., 2022) to address the concern that the size of the portfolio position is endogenous. Finally, we also regress consumption directly on the instrument to address the concern that the exclusion restriction is violated, i.e., investors increase their consumption in response to fictitious gains without liquidating them. Across all specifications, the MPCs out of fictitious capital gains are positive and significant.

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 natural experiment directly provides situations in which individuals observe fictitious capital gains even though they are actually subject to capital losses. Because the fictitious capital gains are plausibly exogenous and we control for all realized and unrealized capital gains and losses, liquidations, and the overall portfolio value, our setup ensures that individual wealth and tastes are held constant. Holding and selling a fictitious winner should not affect individual consumption. However, we find that individuals sell and consume distinctly more when they see a fictitious capital gain, whereas they seem to notice that fictitious capital losses are truly gains, and they

[3]In this approach, it is key to control for the total amount of the liquidation, actual capital gains, and the overall portfolio value. To see this, suppose an investor wants to buy a sofa costing 200 euros. They would liquidate 200 euros, and the actual amount of fictitious capital gains shown in the online brokerage is unimportant and does not affect how big a sofa they buy. However, we find that the fictitious capital gain is reflected in the amount spent (controlling for the total amount that individuals liquidate, their actual capital gains, and their portfolio wealth). Therefore, spending increases when investors liquidate a fictitious winner as opposed to a loser, even though the fictitious winner or loser status is irrelevant for their wealth. Alternatively, think of two consumers who decide to liquidate 200 euros to buy a sofa they fancy; one consumer has a fictitious winner in their portfolio whereas the other does not. We document that the consumer with the fictitious winner buys a larger sofa than the consumer who does not hold a fictitious winner.

do not respond to those.

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

We first document four findings to argue that explanation (3), 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 the 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 notice fictitious capital losses, since they treat them the same as actual winners, and thus are selectively inattentive about fictitious capital gains. Third, we show that the individual propensity to sell fictitious winners is monotonically increasing in the fictitious 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 patterns are consistent with individuals being more selectively inattentive when that is easier for them.

Fourth, we attempt to assess how much individuals' inattention to their actual capital gains matters by analyzing several interactions based on the salience and ready availability of the information on the fictitious capital gains. We use several different measures of investors being uniformed including (1) indicators for the first three months of 2018 before a pop-up window[4] in investors' online portfolios provided additional information about the fictitious purchase prices, (2) investors' first trades in 2018, (3) investors propensity to trade and login in 2018, and (4) whether an investor has automatic transactions set up. For most measures, we find stronger effects for both trading and consumption when investors are less informed; however, the effects on trades and consumption are significant and large even when investors are well informed. We thus argue that investors are not only confused but choose to be selectively inattentive to their actual capital gains, which, interestingly, affects how rich they feel and how much money they decide to spend.

In turn, we evaluate explanation (2), confusion about tax considerations or 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 realized). On the other hand, fictitious winners

[4]On April 1st, the pop-up window started showing right after individuals logged in. It simply stated that the implementation of the capital gains tax reform resulted in the display of new purchase prices for all fund holdings on January 1, 2018, and, as a result, different capital gains and losses. This information was previously provided to investors in an email in November 2017. Additionally, in April 2020, individuals were informed via email that they were provided with an overview of fictitious and actual purchase prices and capital gains.

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 effects are weaker for individuals who are not affected by taxes; thus, we argue that (confusion over) 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., 2017; Olafsson and Pagel, 2017; 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 brokerage accounts and potential trades. We thus show that selective inattention directly affects individual utility and welfare through their consumption beyond the indirect effects on welfare through 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), Birru (2015), and Ben-David and Hirshleifer (2012), among others. Our findings are most closely related to a few recent papers on the disposition effect and individual reference points, such as Arkes et al. (2008) and Meng and Weng (2018). Furthermore, Frydman and Wang (2020) analyze a change in the salience of the displayed purchase price using a natural experiment in which an 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. (2018) suggest that individuals can influence their perception of a winner versus a loser by their subsequent investment behavior. The specific interaction of inattention and the disposition effect is also analyzed in the two contemporaneous empirical papers and one theoretical paper, Quispe-Torreblanca et al. (2021), Birru et al. (2019), and Han et al. (2019). However, none of these papers studies 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 applies to 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; that is, the focal point relative to which investments are classified as winners versus losers in theories of reference dependence. This is important because reference dependence explains the disposition effect (Shefrin and Statman, 1985; Barberis and Xiong, 2009; Meng and Weng, 2018; Barberis and Xiong, 2012; Baillon et al., 2020) as well as 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 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).[5]

Third, we contribute to the literature on consumption out of stock market wealth, which includes studies employing aggregate and regional variation (e.g., Davis and Palumbo (2001), Dynan and Maki (2001), and Case et al. (2005)).[6] However, endogeneity concerns are likely to affect the interpretation of the estimates in these existing studies because 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 (e.g., Parker, 1999b 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 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 information on securities trades and holdings 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 MPC estimates are in line with the MPCs out of wealth that many macroeconomic models estimate, whereas this estimated MPC is much smaller in aggregate data (Poterba, 2000).

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, 1999a, 2015; Agarwal et al., 2007; Jappelli and Pistaferri, 2010; Olafsson and Pagel, 2018). However, like Meyer et al. (2018), we look at investment

[5]Attribution error is originally described as a bias in which people attribute successes to their own ability, and 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 and ultimately not attributing them to own ability.

[6]See Poterba (2000) for a survey of the literature.

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.[7]

3 Data overview, institutional background, and summary statistics

3.1 Data overview

Our dataset covers approximately 100,000 clients of one of the largest online banks in Germany. We have daily information from 2002 to 2018 on each investor's logins (from 2012 onwards), trades, and portfolio holdings as well as all balances and transactions of their other accounts at the bank. We also know customer demographics such as gender, age, occupation, and postal code. We retain only data regarding private investors who reside in Germany. There is usually no charge for having an account with German online banks. So, to avoid analyzing accounts of inactive investors, we require that individuals execute at least one trade and at least one ATM, point-of-sale, or wire transaction in their checking accounts during 2017.

For each trade, we have the International Security Identification Number (ISIN) and obtain additional information on the security, such as asset class, risk class, issuer, and 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 liquidate a position, there is a transfer to their settlement account (Verrechnungskonto), which is dedicated to making trades and is opened automatically when individuals open a portfolio. It pays some interest and is federally insured. Individuals can transfer funds to this account from their checking or savings accounts.

[7] In a separate related paper, Meyer and Pagel (fthc) 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 noisier 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).

3.2 Summary of the natural experiment

All major banks in Germany, including the online bank discussed here, reset the displayed purchase prices for funds and ETFs on January 1, 2018, to implement the 2018 capital gains tax reform. This price reset changed the displayed purchase prices, relative returns, and absolute returns in online portfolios but did not have any tax liability consequences for individuals—their actual capital gains taxes remained the same. 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 for each asset in their portfolio. On January 1, 2018, the displayed purchase prices for funds and ETFs were set equal to the December 29, 2017, closing prices, as quoted by the exchange, and the displayed returns were set to 0%. Thereafter, the values developed as usual. Figure 1 shows screenshots of the online portfolio interface that individuals see. The interface shows all fund and stock holdings as well as their daily absolute changes and percentage returns. Also shown are the cumulative absolute changes and percentage returns relative to the displayed purchase price of the position. These cumulative values were reset to 0 on January 1, 2018, to implement the capital gains tax reform.

[Insert Figure 1 about here]

The experiment creates four scenarios. As an example, Figure 2 shows the price path of a Deutsche Bank (DWS) US equities mutual fund along with two possible purchase prices (labeled (-2) and (-1)) and two possible sale prices (denoted by (1) and (2)). Point (0) represents the displayed, fictitious, purchase price after being reset. Purchasing the fund at (-1) and selling it at (1) creates an actual loser (or loss) because the fund is trading at a loss with respect to both its newly displayed purchase price and its actual one. Purchasing the fund at (-2) and selling it at (1) creates a fictitious loser 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 one. Purchasing the fund at (-2) and selling it at (2) creates an actual winner (or gain) because the fund is trading at a gain with respect to both its newly displayed purchase price and its actual one. Purchasing the fund at (-1) and selling it at (2) creates a fictitious winner because the fund is trading at a gain with respect to its newly displayed purchase price but at a loss with respect to its actual one.

[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 to equalize the tax assessment across domestic and foreign retaining funds—those that did not distribute earned investment income to investors. The goal was to ease tax liability calculations for banks and tax authorities, as capital gains and dividends are taxed at the source. A summary of the implications for our purposes is that only the displayed purchase prices and the displayed capital gains and losses were changed in individual online portfolios, individual tax liabilities were not changed.

3.3.1 Reasons for the 2018 capital gains tax reform

Since January 1, 2009, private investors in Germany have owed capital gains taxes. Before then, 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 of up to 42%. After January 1, 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) initially remained tax-free. Thus, the first reason for the 2018 capital gains tax reform and the resulting reset of the displayed purchase prices was to tax the Altbestände starting from their new cost basis as determined by their price on January 1, 2018. However, none of the investors in this study were actually affected because the initial allowance is very high (100,000 euros for singles and 200,000 euros for couples).[8]

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; that is, when capital gains are realized in a sale, the money that arrives in clients' settlement accounts is already after-tax funds. 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) the church tax (Kirchensteuer) (9% of the capital gains tax), for a total tax of 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 annual allowance is exceeded (Freistellungsauftrag). Furthermore, if capital losses are realized before capital gains, the losses will offset the gains, automatically lowering the capital gains tax. Losses exceeding gains are either rolled forward or, upon request, certified to

[8] On January 3, 2018, the so-called MiFID II/MiFIR regulation was enforced by the European Securities and Markets Authority (ESMA), whose aim was to strengthen investor protection by introducing more stringent organizational requirements regarding client asset protection or product governance, while also strengthening the role of management bodies. The new regime provides for reinforced conduct rules such as an extended scope for appropriateness tests and reinforced information to clients. Independent advice is clearly distinguished from non-independent advice, and limitations are imposed on the receipt of commissions (inducements). These regulations, however, do not apply to the investors in this study as we only have self-directed clients in our sample, and it only applies to new investments. MiFID II/MiFIR also introduces a market structure framework which enhances transparency, notably by closing loopholes and ensuring that trading, wherever appropriate, takes place on regulated platforms. Here, the regulation targets professional and institutional investors, outside the scope of our research.

be considered in individual tax returns.

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. Conversely, gains and dividends retained by domestic funds were taxed at the fund level, and distributed gains and dividends for both domestic and foreign funds were taxed at the investor level. In Germany, two-thirds of funds held are domesticated in Luxembourg and are thus foreign. After the reform, all funds are 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 retained earnings at the end of the tax year (Vorabpauschale).

3.3.2 How did the 2018 capital gains tax reform affect our investors?

To simplify calculating the tax liability of capital gains, the online bank and many other banks fictitiously bought and sold all fund holdings of all clients on January 1, 2018, and reset the displayed purchase price to the December 29, 2017, closing price, as quoted by the exchange. This changed 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 annual 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% (which is the average equity share assumed by the authority) times the December 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) is 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).

3.3.3 Retail investors' information environment

An important question for interpreting our results is whether individuals properly understand the tax consequences of the reform and that the displayed purchase prices changed but the cost bases did not. In Section 6, we provide evidence that confusion about tax implications is not driving our results.

Individuals were provided with accurate information in three ways. First, after each sale, they receive a sales receipt (Figure 3). It shows the following on page 1: the fund's description, ISIN, number of securities sold, current price, current value of the liquidation gross of taxes and fees, capital gains taxes, church taxes, solidarity 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 January 1, 2018, and accumulated retained capital gains since that date; note that the fictitious plus actual capital gains minus the tax-free allowance plus the retained capital gains equal the overall capital gains. The fund displayed in Figure 3 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, on April 1, 2018, a pop-up window about fictitious purchase prices appeared after individuals logged in (see Figure 1). It contained the same information as an email sent in November 2017. Additionally, in April 2018, individuals received another email and mail providing an overview of their fictitious and actual purchase prices as well as the fictitious and actual capital gains and losses for all their existing positions. Finally, throughout 2017 and 2018, individuals could see their actual purchase prices by clicking on the order summary.

3.4 Summary statistics

3.4.1 Descriptive statistics for investor demographics and portfolio characteristics

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, it 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% are 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, which 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, it should be interpreted with caution, as individuals may report only their liquid savings and not include other assets such as life insurance or real estate in this number. 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. After all, 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.[9] 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 5.4.

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 at 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, 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 index (HHI) (a measure of portfolio diversification), and turnover. The HHI ranges from 0 to 1, with smaller values indicating better diversification. If it is above 0.5, the investor holds only a handful of stocks and their portfolio is not well diversified. If the HHI is close to 0, the investor holds a fund or two consisting of many stocks; their portfolio is well diversified. 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, as established by German regulations, and ranges from 1 (safe securities such as money-market funds) to 5

[9]Note 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.

(stocks, options, and futures). We also display summary statistics for the number of total trades in funds, the holding periods of funds, and the share of the portfolio value held in funds. Finally, we report the average disposition effect for funds, that is, 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 in about nine times per year, and most portfolios are reasonably well diversified. Our investors are fairly active on average; the mean number of fund trades over 2017 and 2018 is 11 per year. Typically, our investors hold only a handful of different securities (the median number of securities is 7). Approximately half of our investors hold a fund in 2018; those that do make on average 18 trades per year, 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.

3.4.2 Descriptive statistics for all versus affected investors

In Table 1, we separately show the summary statistics for affected investors, those who held a fund in 2018. Unsurprisingly, the portfolios of this sample are 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 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 is correlated with certain characteristics, and some p-values are close to zero. Mechanically, affected individuals hold funds longer, have a higher share of fund holdings, trade more in funds, have less turnover, and have better diversified portfolios. There are no large differences in demographics, logins, or wealth. But in any case, in all our regressions, we focus on the subsample of affected investors only.

3.4.3 Descriptive statistics of fund positions and actual versus fictitious purchase prices and returns

The left graph in Figure 4 shows the distribution of the positions in all fund securities in 2018 and their fictitious returns calculated using the December 29, 2017, closing price and the price at either the time of sale or June 30, 2018, for open positions. The right graph in Figure 4 shows only fund positions that were sold and their returns since January 1, 2018. We can clearly see that the majority of funds' returns since

January 1, 2018, are slightly negative, given that the market performed poorly at the beginning of that year. However, the returns of funds that were sold after January 1, 2018, are more often positive; that is, 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 December 29, 2017, closing price; that is, 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; that is, the distribution of the return from December 29, 2017, to the date the position was sold in 2018 or the price on June 30, 2018, if the position remained unsold (the fictitious return), minus the actual return based on the true purchase price. 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]

As shown in Figure 5, the fund repricings had very large effects on the displayed returns. But these repricings do not represent an actual wealth shock, only a fictitious shock to the displayed returns.

3.4.4 Descriptive statistics for affected investors' consumption, capital gains and losses, and their disposition to sell winning investments

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 conditional on those variables being nonzero in a given month.

For consumption, we consider: (1) ATM withdrawals plus points of sale transactions, in which individuals swipe their debit or credit card in a store or enter their card information to purchase goods or services online, and (2) all domestic non-recurring 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. In contrast, recurring wire transfers are likely rent payments. We can see that investors consume reasonably large fractions of their monthly salaries on average. 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). Here, we find a familiar discrepancy in the proportion of realized gains versus losses—the disposition effect—as first documented by Odean (1998) and 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

4.1.1 Specification for trades

We first run a disposition-effect regression to determine how the displayed fictitious purchase prices affect trades. For either the full sample period or only 2018, we regress a dummy for selling on dummies for whether the sold security was a winner, a fictitious winner, or a fictitious loser:

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

wherein $SaleDum_j^{it}$ 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; that is, we run a regression conditional on individual trading days.[10]

The (fictitious) winner or loser status of a fund position is exogenous to individual investors, as it only depends on the last price in 2017 (i.e. the closing price on December 29, 2017), and the current price of the fund. We control for month-by-year fixed effects to account for fluctuations in all aggregate variables such

[10]To 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). One could instead run an unconditional regression as done by Ben-David and Hirshleifer (2012) who discuss the differences between the two approaches.

as stock market performance, consumer confidence, or any other 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.

4.1.2 OLS specification for consumption

To analyze the effects of fictitious capital gains on consumption, we aggregate all variables to the monthly level for the time period from January 2017 to June 2018. As a first approach, we simply regress the euro value of consumption ($Cons^{it}$) on the realized fictitious gain and loss ($RealFictGain^{it}$ and $RealFictLoss^{it}$) of individual i in month t as follows:

$$Cons^{it} = \eta_i + my_t + \beta_1 RealFictGain^{it} + \gamma_1 RealFictLoss^{it} + \theta_1 RealGain^{it} + \delta_1 RealLoss^{it} + \vartheta X^{it} + \epsilon^{it} \quad (2)$$

Specifically, $Cons^{it}$ is the euro value of ATM withdrawals and point-of-sale transactions plus domestic non-recurring outgoing wire transfers, $RealFictGain^{it}$ and $RealFictLoss^{it}$ are the capital gains or losses of the liquidated fictitious winners and fictitious losers, and $RealGain^{it}$ and $RealLoss^{it}$ are the actual capital gains or losses for the liquidated actual or fictitious winners or losers. If an investor does not consume or sell securities in a given month, we keep the observation as a zero.[11]

The other control variables, X^{it}, include Liq^{it}, which is the total amount 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. Additionally, we show various modifications of this specification. In particular, we control for the individual portfolio value $PortfVal^{it}$, we include or exclude the liquidated actual capital gains and losses as well as the liquidation minus reinvestment amount, and we include 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.

Interpretation The coefficient on the variable $RealFictGain^{it}$ states the following: how much does an additional euro of liquidated fictitious capital gain yield in extra consumption?[12] This approach is just a standard OLS regression analysis. Conceptually, suppose we have two investors with the same total liquidations in a month. We then test whether the investor who liquidates more fictitious gains (instead of non-fictitious gains or losses) has greater consumption. The idea here is that by controlling for the portfolio value and total liquidations, we control for the wealth and consumption plan of the investor, which

[11] We observe almost all individuals' consumption and capital gains in almost all months, but a few months are missing for a handful of individuals. We ultimately end up with 98% of a balanced panel.

[12] Controlling for the total amounts of capital gains, the liquidations minus reinvestments in that month, individual and month-by-year fixed effects as well as the actual capital gains and losses.

should fully determine consumption. Note that, by construction, $RealFictGain^{it}$ are always positive, and $RealFictLoss^{it}$ always negative; therefore, we would expect a significantly positive fictitious gain (loss) coefficient if investors consume more (less) in response to liquidating fictitious winners (losers).

If an individual liquidates because they want to consume or because their wealth is higher, then their consumption amount should not be affected by fictitious gains and losses ($RealFictGain^{it}$ and $RealFictLoss^{it}$). Ultimately, the displayed fictitious capital gains are, in a sense, just random numbers depending on the closing price of the fund on December 29, 2017, 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 minus reinvestment amount and actual capital gains and losses or portfolio wealth. We thus argue that the variables $RealFictGain^{it}$ and $RealFictLoss^{it}$ are exogenous conditional on the controls.

Omitted variables To further understand Specification 2 and how we identify the effect of fictitious capital gains that induce individuals to sell and consume, we can describe the omitted variables that could 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 or taste 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 December 29, 2017, and the subsequent price movement, 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 Specification 1, as time-varying economic sentiment may affect stock prices and trading behavior, and the same is true in Specification 2. Second, there are individual consumption plans that may change with time or taste shocks. If individuals want to consume more, then they decide to liquidate, as captured by the liquidation dummy $LiqDum^{it}$, and how much to liquidate, denoted by Liq^{it}. As mentioned previously, we can control for $LiqDum^{it}$. 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 the actual capital gains and losses as well as Liq^{it} takes care of this concern. Additionally, we can control for the individual portfolio value $PortfVal^{it}$

The omitted variable bias theorem tells us that when we run a regression of the form $Y = \beta X + \epsilon$ and

there is a variable Z (in our case, the decision to liquidate plus how much) that 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. We can thus 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 are entirely free of error.[13]

Even though we control for the relevant variables that should fully determine investor's consumption, there may be lingering concerns about reverse causality and omitted variables that are not perfectly controlled for (e.g., individual housing wealth). To further address any endogeneity concerns, in the next subsection we implement a 2SLS IV approach utilizing that fictitious gains cause individuals to sell.

4.1.3 2SLS IV specification for consumption

As a complementary approach, we implement a 2SLS IV specification. We use the total potential fictitious capital gains as the first-stage exogenous variable that causes liquidations; that is, we estimate the propensity to realize a fictitious capital gain out of all fictitious capital gains the investor could realize. We then estimate the MPC out of each euro of liquidated fictitious capital gains in the second stage. More formally, in the first stage of the IV approach, we regress the endogenous variable, the realized fictitious capital gains ($RealFictGain^{it}$) by individual i in each month t, on the total potential fictitious capital gains ($TotalPotFictGain^{it}$) that an investor could have liquidated. To construct this variable, we sum up the end-of-the-month fictitious capital gains of all investor's beginning-of-the-month fund positions (independent of whether or not they were liquidated) in each month. Hence, this variable measures the end-of-the-month value of fictitious capital gains the investor could have realized in a given month. The total potential fictitious capital gains are thus determined by the end-of-the-year 2017 prices and the end-of-the-month prices of all the investor's beginning-of-the-month fund positions. While the return of a fund in a given month is plausibly exogenous, the size of the initial position is determined by the investor. To address this concern, we also use a dummy for any of the beginning-of-the-month fund positions having a fictitious capital gain at the end of the month as the instrument.

In the first stage, we explain the realized fictitious capital gains using the total potential fictitious capital gains of investor i in month t. We also account for fluctuations in all aggregate variables, such as stock market performance or consumer confidence, that may affect consumption and investment decisions simultaneously

[13] As an alternative interpretation, we can view Specification 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 minus reinvestment amount, and we examine an interaction, $RealFictGain^{it}$ and $RealFictLoss^{it}$, of the randomly assigned fictitious winner or loser status of that investment.

using month-by-year fixed effects. Additionally, to control for all time-invariant investor characteristics, we include individual fixed effects. The predicted values of that regression are denoted by $\widehat{RealFictGain}^{it}$.

$$RealFictGain^{it} = \eta_i + my_t + \beta_2 TotalPotFictGain^{it} + \vartheta X^{it} + \epsilon^{it} \tag{3}$$

In the second stage of the IV approach, we then regress consumption $Cons^{it}$ on the predicted values of the first-stage regression ($\widehat{RealFictGain}^{it}$). Intuitively, our 2SLS retains only the variation in $\widehat{RealFictGain}^{it}$ that is generated by variation in the instrument, i.e. total potential fictitious capital gains.

$$Cons^{it} = \eta_i + my_t + \beta_3 \widehat{RealFictGain}^{it} + \vartheta X^{it} + \epsilon^{it} \tag{4}$$

As before, η_i and my_t denote individual and month-by-year fixed effects. As mentioned, controlling for month-by-year fixed effects is important in both the first- and second-stage regressions, as time-varying economic sentiment may affect stock prices and trading behavior. As an alternative to regressing the amounts in euros, we log all outcome and explanatory variables.[14] In additional specifications, we also include other control variables, X^{it}, such as actual capital gains or losses as well as individuals' consumption plans and wealth with the available liquidation minus reinvestment amount Liq^{it} and the individual portfolio value $PortfVal^{it}$.

The exclusion restriction is that total fictitious capital gains affect consumption only through their effects on realized fictitious capital gains conditional on individual and time fixed effects, individual consumption plans, liquidity needs, and portfolio wealth. Any other omitted variables should be captured in the error term in the first stage. To address the concern that individuals increase their consumption in response to the total fictitious capital gains they have in a given month without liquidating those fictitious capital gains, we also look at the results of a regression in which we regress consumption on the instrument directly.

Note that the coefficient estimates of the OLS specification and the 2SLS specification cannot be directly compared. Whereas the OLS specification estimates the average treatment effect (ATE), the 2SLS specification is estimating a local average treatment effect (LATE). As discussed by Angrist and Pischke (2008), the 2SLS IV approach looks at those investors that liquidate only because they are shown fictitious capital gains. Because the LATE isolates the effect of the fictitious capital gains, it is conceivable that the coefficient estimates of the IV approach are larger than the OLS estimates because the OLS approach also considers investors who sold fictitious gains for other reasons than a position being a fictitious gain. Consequently, the effects estimated by OLS (i.e., ATE) are smaller than the ones estimated using the 2SLS approach if other

[14] When we log variables with negative values, we first take their absolute value and then transform them back to negative. We keep observations with values between minus one and one euro, including when they are equal to zero, by not transforming them.

reasons to liquidate are caused by other motives, e.g., rebalancing rather than just consumption. Additionally, the OLS estimates captures the consumption response of the fictitious capital gains on the entire sample of investors, whereas the 2SLS approach captures only those who liquidate because they have a fictitious winner.

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. The likelihood that an individual sells a security is approximately 6 percent higher when the security is trading at a gain relative to the actual purchase price, conditional on the individual trading on a given day.[15]

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, we generally find an attenuated, sometimes positive and sometimes negative disposition to sell winning funds. Our rolling-window disposition-effect regressions can be seen in Figure 6, which contrasts the propensities to sell winning stocks and funds with the price path of the German stock market index (DAX). The disposition effect for all securities and funds is countercyclical relative to the DAX, as shown in Bernard et al. (2018) and discussed for US investors in An et al. (2019). The variation in the disposition effect for funds closely follows that 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 January 1, 2018, despite actually being losers (fictitious winners), or (3) displayed as losers after January 1, 2018, despite actually being winners (fictitious losers). We see that individuals are subject to the disposition effect with respect to fictitious winners but not losers: they are nearly 4 percent more likely to sell a fictitious winner but less than 1 percent less likely to sell a fictitious loser.[16]

[15] Here, we simply replicate the disposition-effect findings in Koestner et al. (2017).

[16] Note that when we include funds fixed effects, the number of observations decreases as singleton observations are dropped. Table A.16 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.

[Insert Figure 6 and Table 4 about here]

Column 4 in Table 4 includes individual fixed effects and month-by-year fixed effects, and Column 5 adds funds fixed effects. In the latter case, we thus control for all time-invariant effects at the investor and fund level. The findings are the same: fictitious winners have a large positive effect on the probability of selling whereas the effect of fictitious losers is a tightly estimated zero.

In terms of magnitudes, 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.[17] We also 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.

[Insert Figure 7]

5.2 Empirical results for consumption: OLS

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 percent is consumed out of a fictitious capital gain that is liquidated. For fictitious capital losses, the effects are again tightly estimated zeros. We also report specifications for the subsample of customers that the bank flags as main clients (as described in Subsubsection 5.4.1). Here, we find larger effects. Additionally, we find larger effects if we include domestic wires in our measure of consumption.[18]

The coefficient β_1 in the consumption regression, Specification 2, can thus be interpreted as the MPC out of one euro of realized capital gains or losses when the security was a fictitious winner. An MPC of 15 percent at the monthly level is in line with the high MPCs out of realized capital gains reported in Baker et al. (2007) and Di Maggio et al. (2020) as well as the high MPCs commonly estimated out of transitory income shocks (see Jappelli and Pistaferri, 2010, for a literature survey). Table 5 reports the results when

[17] The 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).

[18] Table A.17 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.

we log all variables (keeping values between minus one and one untransformed). We use absolute amounts and report similar results in Table A.13 in Appendix A.[19]

[Insert Table 5 about here]

5.3 Empirical results for consumption: 2SLS IV

Tables 6 and 7 show the 2SLS IV estimation results for consumption from Specification 4. Here, we also show the simple OLS version as well as the first-stage regression outcome. In the second stage, we regress different measures of consumption on liquidated fictitious capital gains instrumented by total potential fictitious capital gains, including different sets of control variables. In Table 6, we can see that the propensity to realize fictitious capital gains, that is, the first-stage coefficient, is estimated to be 0.113. Because we run the regressions in logs, this coefficient implies that approximately 11.3% of total potential fictitious capital gains are realized on average each month.

Columns 3 to 5 display the main 2SLS IV specification with different control variables. In Columns 6 and 7 we display two variants of the main 2SLS IV specification. In Column 6, we use a dummy for any of the investor's beginning-of-the-month funds having a fictitious capital gain in the end of the month as the instrument. Using a dummy for potential fictitious capital gains instead of their value takes into account that investors' initial positions are endogenous. In Column 7, we use the instrument as the main regressor in an OLS specification to address the concern that individuals might increase their consumption in response to fictitious capital gains without liquidating them. In all specifications, we document a significant effect of fictitious capital gains on consumption.

We can also include realized fictitious losses and actual capital gains and losses as controls. In Tables 6 and 7, we can see in the OLS specification that the fictitious capital gain coefficients are comparable to those estimated from actual capital gains in Column 1. However, when we do not log consumption and capital gains, as in Tables A.14 and A.15 in Appendix A, the coefficients for actual capital gains are smaller. This is due to scaling issues in the euro-based specification, as actual capital gains are much larger on average than fictitious ones.

[Insert Tables 6 and 7 about here]

In Tables 6 and 7, we can see that the second-stage IV coefficients are slightly larger than the OLS coefficients in Table 5. As discussed in Subsection 4.1.3 this can be explained by the 2SLS estimating

[19] Alternatively, we can calculate the deviation from the mean of the outcome variable as well as the regressor variables. The coefficients for fictitious and actual capital gains are comparable in magnitude. Furthermore, we show in the robustness section that we can include or exclude different sets of control variables .

a LATE not ATE effect and by the small first-stage coefficient. As mentioned, the 2SLS IV coefficient measures the additional consumption just from the additional sales caused by the fictitious capital gains—but these are a small fraction of all sales. When we multiply the large second-stage with the small first-stage coefficient, we find that up to 6 percent of the liquidated fictitious capital gains is consumed. Comparing Tables 6 and 7, as before, we find larger effects if we include domestic wires in our measure of consumption.

In the most basic specification when we only control for individual and month-by-year fixed effects, we can see that the first-stage coefficient is highly significant and the regression's F-statistic is large in the second columns of Tables 6, A.14, A.14, and A.15. For the other IV specifications in Columns 3 to 5, we display the Cragg-Donald Wald F-statistic that is preferred in IV specifications with many fixed effects. By all standards, the F-statistics do not indicate a weak instrument problem.[20] Fictitious capital gains cause individuals to sell, as should also be established from our first-stage trading analyses. Finally, Table A.18 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.

We next present a number of robustness checks, followed by a discussion of the psychological mechanisms behind our results and what we can learn from them.

5.4 Robustness

5.4.1 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, who may have and use other banking relationships. We argue that it is unlikely that individuals in this study have a second brokerage account or additional savings vehicles. In Germany, individuals are incentivized to designate one main brokerage account to receive the tax-free allowance on capital gains. Additionally, the self-reported wealth measure gathered when individuals opened their accounts is in line with their actual portfolio size, indicating that they are, on average, unlikely to have other deposits of liquid wealth. We also have a flag variable from the bank itself that indicates customers without other banking relationships. This flag 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, 2018). The regression results for the subsample of main customers can be found in Tables 5 and A.13.

To further establish robustness and alleviate concerns about omitted variables, we report the results of

[20] A rule-of-thumb lower-bound for F-statistics in IV models is 10, as discussed in Angrist and Pischke (2008). Stock and Yogo (2002) tabulate critical values for the Cragg-Donald Wald F-statistic depending on the number of endogenous variables and instruments. Our statistics are all well above those tabulated.

the OLS consumption regressions, successively adding fixed effects and other control variables such as a dummy for liquidations, salary, dividends, and interest payments in Table A.19 in Appendix A.

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

From the bank's transaction system, we observe transaction categories 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, as mentioned, 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.[21]

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 more thoroughly categorized transaction-level spending data (Olafsson and Pagel, 2018; Bräuer et al., 2019; Gelman et al., 2014). When we replicate the analysis in Olafsson and Pagel (2018) and Gelman et al. (2014) by 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 (Figure 8). 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) (Figure 9). Here, we can also see that the daily consumption responses for actual and fictitious capital gain liquidations are similar.

[Insert Figures 8 to 9 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 their actual capital gains and losses. Investors are thus fully informed about their actual capital gains at the time of consumption after they might have closed their mental trading account (Imas, 2016).

[21] Domestic 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.

5.4.3 Placebo tests

All placebo checks can be found in Table A.20 in Appendix A as well as Figure 10. As our first placebo test, we run Specification 1 for all years of the sample, i.e., 2004 to 2018, beginning of January to end of June, using the placebo fictitious gains and losses, i.e., the year-to-date gains and losses even though the bank did not reset the displayed purchase prices in years 2004 to 2018. The results can be found in Table A.20 in Appendix A. In the data of Odean (1998), it is well known that there is also a recency effect: individuals tend to display a disposition effect relative to recent performance.

The recency or year-to-date placebo effects in Table A.20 are present in some years but not every year. Additionally, they are sometimes negative, very small, or insignificant, and sometimes are also present for losers (though not consistently negative or positive). The placebo fictitious gain coefficients range from -0.0768 in 2008 to 0.0320 in 2005 and the fictitious loss coefficient from -0.0299 in 2008 to 0.0279 in 2009.

To identify the difference between 2018 and all other years, we run a regression using data from 2004 to 2018, January to June. We construct a variable containing dummies for securities being fictitious winners or losers, either the actual fictitious winners and losers in 2018 or the placebo fictitious winners and losers in all other years. Finally, we include a dummy for 2018 and document the interaction coefficient of this dummy and the (placebo) fictitious capital gains and losses. This is reported in Table 8, and we see that the 2018 interaction effect is around 3%, which is large and statistically significant.

[Insert Table 8 about here]

That said, for estimating the MPC out of fictitious capital gains it is irrelevant whether individuals have a reason to sell because of the fictitious capital gain that is plausibly exogenous or an alternative random anchor, such as a year-to-date or recency effect.

Finally, in Figure 10, we show the distribution of fictitious gain coefficients in the OLS consumption regression 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 for 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 10 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. This fictitious gain is not reflected in an actual capital gain (in response to which individuals should consume more because they are wealthier). Despite the individual selling a fictitious winner representing an actual capital loss, which they see on their sales receipt or in their order history, they consume more in response to seeing and liquidating fictitious capital gains.

The interpretation of our findings depends on whether individuals know their actual wealth or are confused about fictitious capital gains versus actual capital gains. 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 contributes to the literature on the disposition effect, showing that the act of selling winners has effects on consumption and not just on trading. Individuals sell winners because they believe they have made money; hence, they 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.

In Subsection 6.1, we perform a number of interaction exercises to assess the validity of plain confusion versus selective confusion or inattention about actual capital gains. In Subsection 6.2, as an alternative explanation, we discuss the possibility individuals liquidate and consume because they believe doing so is advantageous from a tax perspective. We thus also look at confusion about tax implications. In short, we find multiple pieces of evidence that individuals appear to be choosing to misperceive their actual capital gains when that is easier, which tells us that selective inattention plays a role. We do not find evidence that individuals are confused about the tax implications of the reform.

6.1 Plain confusion or selective inattention?

In Table 4, we document two results. First, the propensity to sell fictitiously winning funds is larger than that of actually winning funds. We interpret this result as follows: individuals are looking for a reason to sell losing funds once they are fictitiously displayed as winners in order to feel richer and consume the proceeds (as we show). Second, the effect of fictitious losers is a tightly estimated zero. It seems that individuals know when an actual winner is displayed as a loser, as they barely react differently compared to when the fund is an actual loser or winner. 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, 2017; 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 monotonically 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. After all, it is likely that a large fictitious capital gain raises more attention than a small one. Additionally, we show in this figure 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.

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 investors paid (Figure 3). Individuals receive this receipt before consuming, as is made clear in Figure 9, which shows the spike in consumption a few days after the transaction is processed and booked in the settlement account. Additionally, we use variation in the supply of information by the bank: starting in March 2018, a pop-up window about the fictitious purchase prices appeared after individuals logged in (Figure 1).

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 (as shown in Table 9). 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 5.4.3). That said, the effect is still pronounced in late 2018.

[Insert Table 9 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 based on the median number of trades in 2018. Additionally, we interact with a dummy variable for whether an investor uses automatic savings plans. These plans make it more difficult for investors to determine whether the investment is an actual winner or loser because, as a default, the brokerage order

summary screen displays the purchase prices of the three most recent transactions (whereas the displayed capital gains or losses and returns on the main site are calculated relative to the fictitious purchase price). Therefore, savings plan investors must dig deeper to find their actual average purchase prices. Additionally, we consider an interaction with whether or not an investor logged in more often (relative to their 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.

Tables 10 and 11 provide the same set of interactions using the above measures of uninformedness but consider consumption as the outcome variable. Table 10 displays the 2SLS IV estimates interacted with those measures, and Table 11 shows the OLS results.[22] In almost all specifications, we find that traders in early 2018 and infrequent traders have a larger consumption response to fictitious capital gains. However, both the baseline estimate as well as the interactions are significant, which tells us that it is not only plain confusion playing a role. 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.

[Insert Tables 10 and 11 about here]

It is also important to point out that 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.

In summary, we find multiple pieces of evidence that people are not only confused, but selectively inattentive. Individuals do not react to fake losers, they respond more when it is easier for them to fool themselves, and even well-informed investors sell fictitious capital gains to consume.

6.2 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 investors 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).

[22] The OLS regressions in Table 11 use logged values as well because the scaling allows for tighter estimates to pinpoint the interaction terms (the regressions in euro values are subject to more noise in the euro amounts).

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 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 (the household tax-free allowance) or 801 euros (the individual 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]

6.3 Discussion of the interaction between selective inattention and the disposition effect

As the first lesson about the disposition effect, we document that selling winners has real consequences: individuals sell to consume, and if they are under the (mistaken) assumption that they are richer, they consume more. Here, the changed purchase price and selective inattention affect consumption, a direct component of utility and welfare. 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, beyond its effects on wealth, affects utility directly through consumption. This is a new channel through which behavioral biases affect welfare.

We also document that an interaction between selective inattention and the disposition effect affects consumption. We argue that the psychological mechanism behind this interaction is attribution error (as in Barber and Odean, 2002); that is, 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.

In Table 4, Figure 6, and Figure 7, the omitted category 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). The latter two results are in line with the absence of a clear disposition effect for funds, which we document over the entire sample period and is also seen in Chang et al. (2016). This finding is also documented in Figure 6, which shows a strong comovement between the disposition effect for stocks and funds. This suggests to us that the psychological mechanism behind the two have common components.

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 in bust periods the effect is correlated with individuals realizing gains. Additionally, 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, those funds that are displayed as winners but are actually losers. The relevant reference point 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 they are making money, and hence they 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.

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 individual consumption and the individual propensity to sell winners and hold losers respond to exogenous changes in the displayed purchase prices of funds. These changes in the displayed purchase prices are due to the implementation of a capital gains tax reform and also affect the funds' displayed returns and capital gains and losses. We 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% of their fictitious gains conditional on the value of their liquidation.

Additionally, we document three more findings: (1) individuals do not respond differentially to fictitious losers in their trading or their consumption behavior, (2) individuals sell more fictitious winners when the fictitious gain is large and the actual loss is small, and (3) individuals sell fictitious winners even when the information about their actual capital gains is very salient. We argue that these findings are 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, 2017; 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, 2018; Frydman and Wang, 2020; Quispe-Torreblanca et al., 2021). 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. Individuals are selectively inattentive and engage in the disposition effect and then appear to feel richer and consume more of the realized fictitious capital gains. This is consistent with attribution error (as in Barber and Odean, 2002); that is, individuals are looking for an excuse to sell fictitious winners in order 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 (2020), who analyze the salience of new price variables and their colors in online portfolio displays. 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 Quispe-Torreblanca et al. (2021) and Birru et al. (2019), but we also study their interaction effects on consumption.

As a contribution to the larger behavioral finance literature, we show that behavioral biases in trading are not constrained to the domain of investing but impact consumption out of stock market wealth (Poterba, 2000; Di Maggio et al., 2020). Therefore, behaviral biases affect welfare not only through their effect on wealth but also directly through consumption. This is important for the aggregate economy, as stock market wealth accounts for roughly one-quarter of household net worth.

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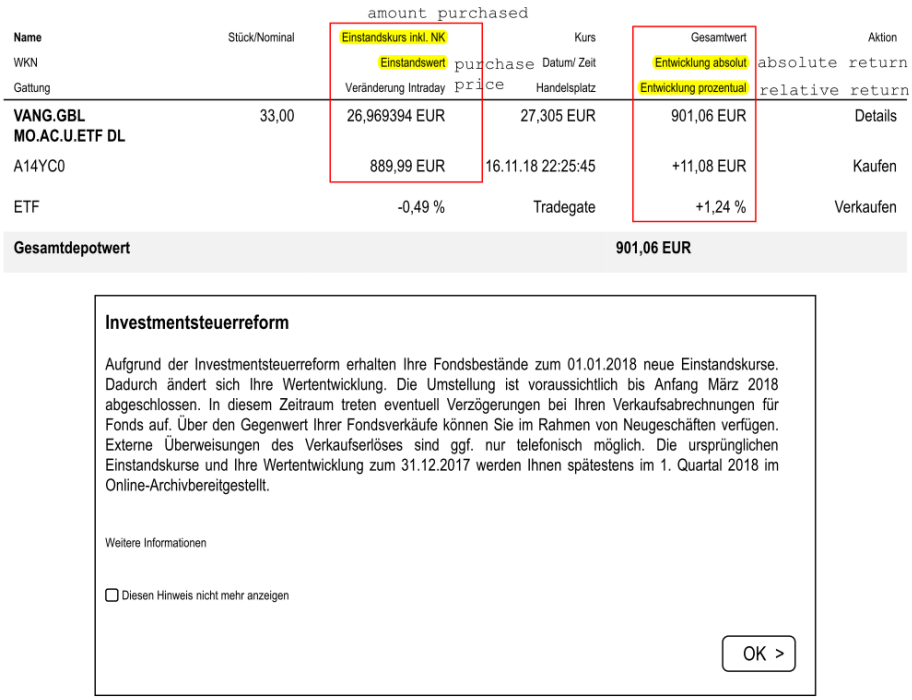


Figure 1: Screenshots of the bank's portfolio interface and notification of repricings due to the capital gains tax reform. On top, we display the portfolio interface showing the security name (VANG:GBL.MO.AC.U.ETF, that is, the Vanguard Global Momentum Factor UCITS ETF); number of shares held (33,00); (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). Below, we display the notification that clients saw. It translates 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."

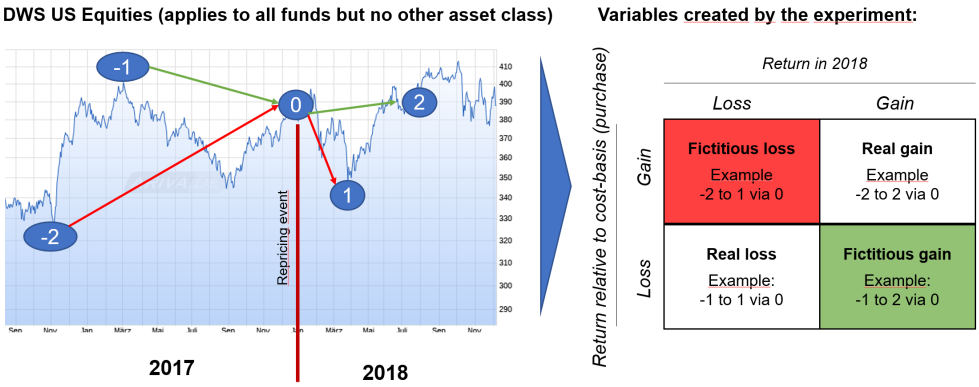
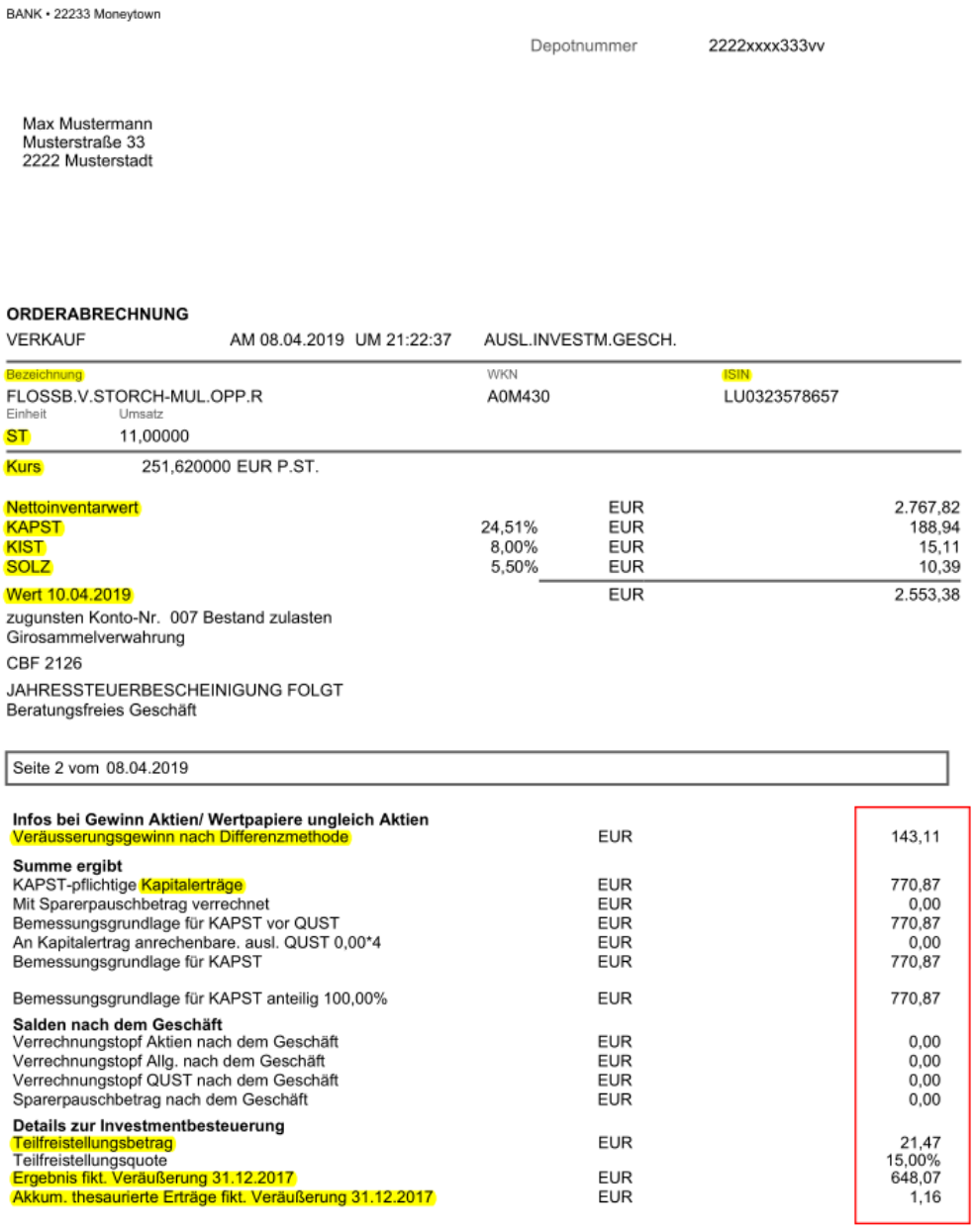


Figure 2: Gain, fictitious gain, loss, and fictitious loss variables created by the experiment. Price path of a Deutsche Bank (DWS) US equities mutual fund and two possible purchase dates and prices (called (-2) and (-1)) as well as two possible sale dates and prices (denoted by (1) and (2)). Purchasing the fund at (-1) and selling it at (1) creates an actual loser or loss because the fund is trading at a loss with respect to both its newly displayed purchase price and its actual purchase price. Purchasing the fund at (-2) and selling it at (1) 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 (-2) and selling it at (2) creates an actual winner or gain because the fund is trading at a gain with respect to both its newly displayed purchase price and its actual purchase price. Purchasing the fund at (-1) and selling it at (2) creates a fictitious winner or gain because the fund is trading at a gain with respect to its newly displayed purchase price but at a loss with respect to its actual purchase price.



BANK • 22233 Moneytown

Depotnummer 2222xxxx333vv

Max Mustermann
Musterstraße 33
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

Einheit	Umsatz
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. The receipt indicates 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), solidarity surcharge (SOLZ), and final liquidation value net of taxes and fees (Wert 10.04.2019). The receipt then displays 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: $143.11 + 648.07 - 21.47 + 1.16 = 770.87$.

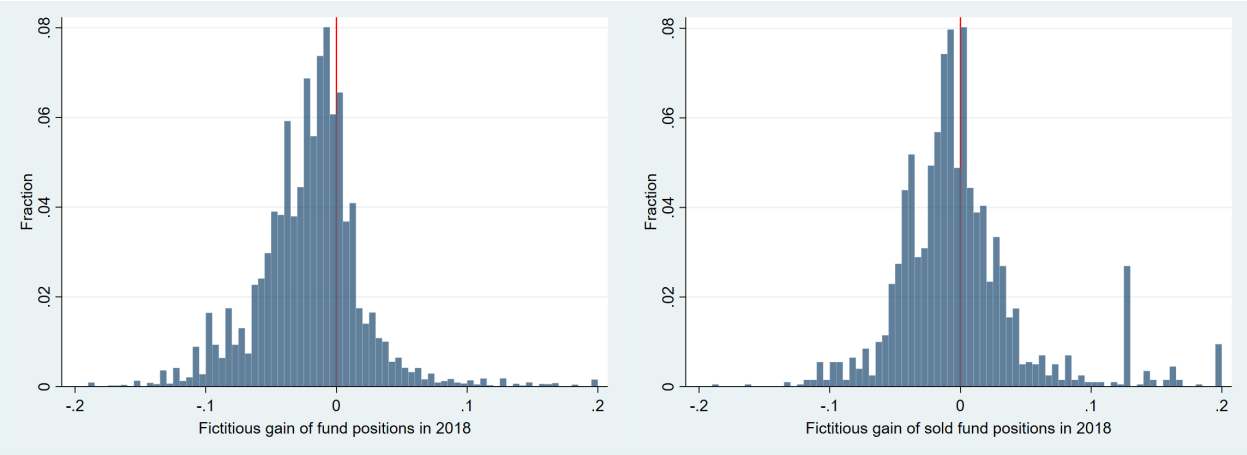


Figure 4: Distribution of the percentage "fictitious" (newly displayed based on the December 29, 2017, closing price) returns of all (left) and sold (right) fund positions. Returns are based on either the sale price or the June 30, 2018, price, relative to the December 29, 2017, closing price.

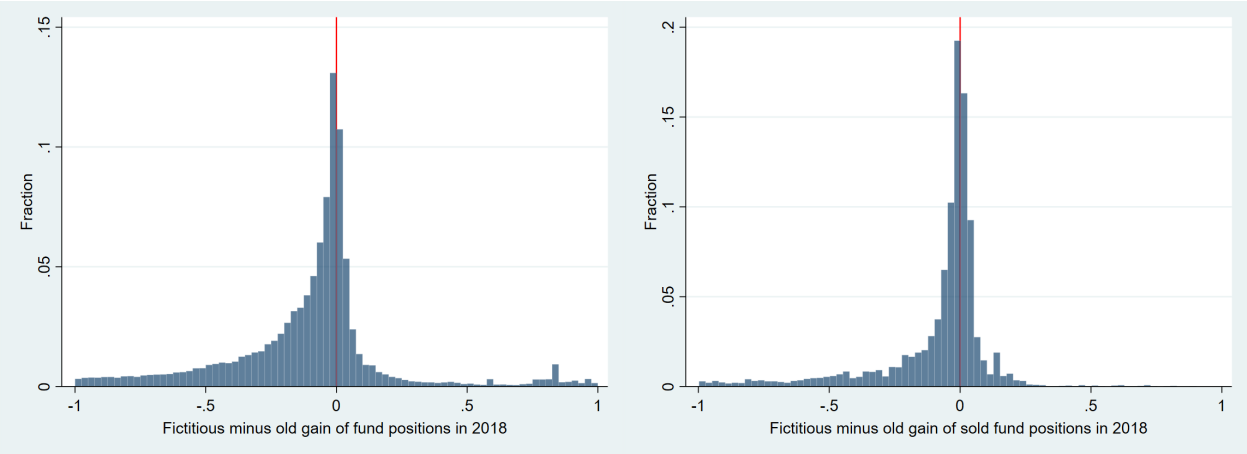


Figure 5: Distribution of the percentage "fictitious" (newly displayed based on the December 29, 2017, closing price) returns of all (left) and only sold (right) fund positions (return based on the sale price or the June 30, 2018, price, relative to the December 29, 2017, closing price) minus their percentage "actual" returns (returns from their actual purchase prices to the December 29, 2017, closing price).

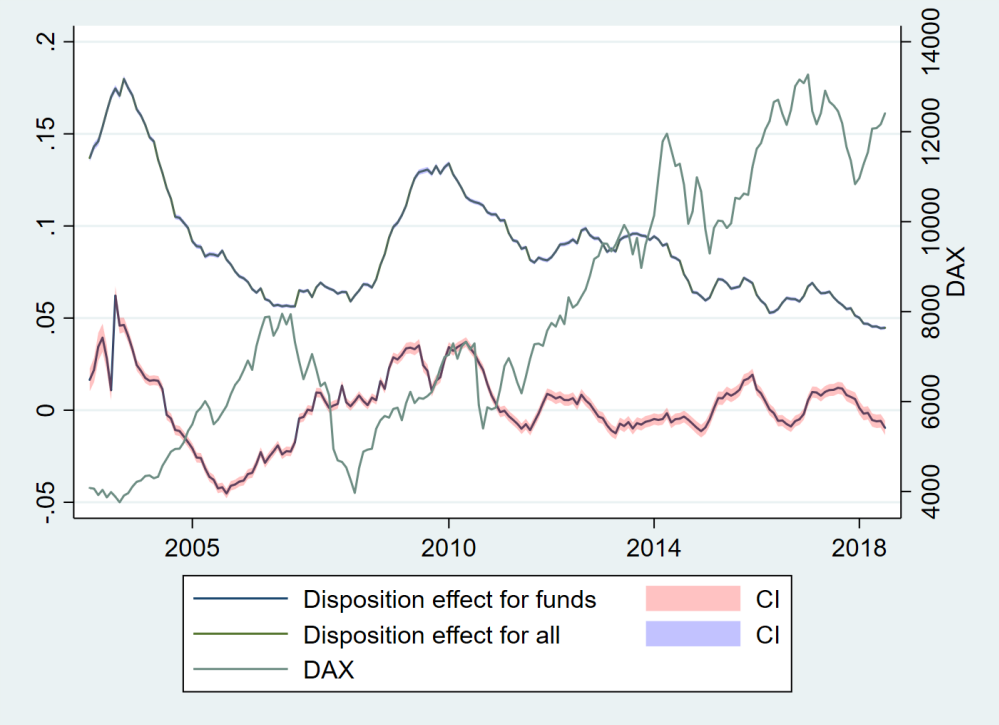


Figure 6: Twelve-month rolling-window disposition-effect regressions without any controls; that is, a variant of Specification 1 (regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner) using either all securities or only funds. The figure also displays the price path of the German stock market index (DAX).

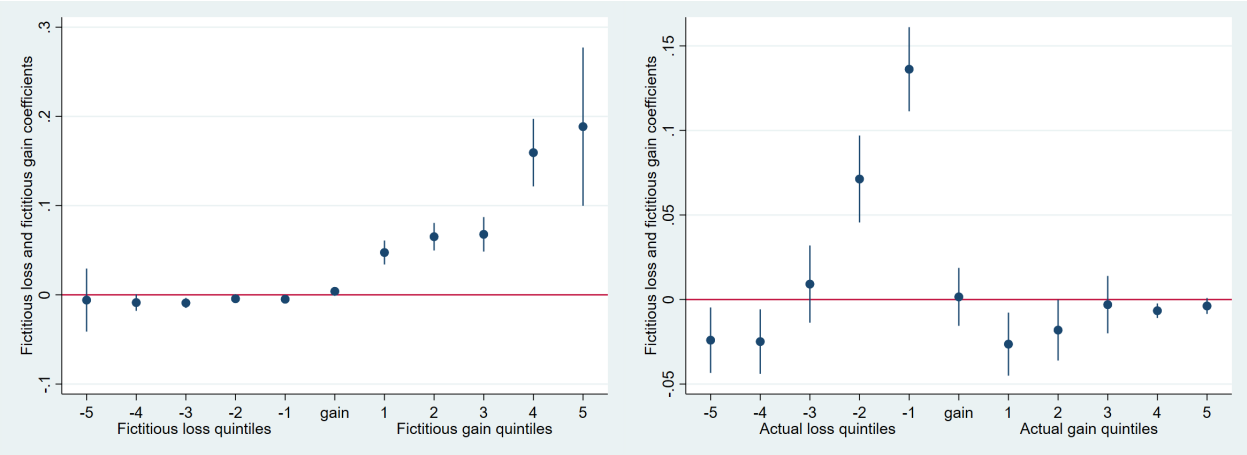


Figure 7: Fictitious loss and gain coefficients for five quintiles of the fictitious (newly displayed since the December, 29, 2017 closing price) or actual (relative to the actual purchase price) capital gains and losses in absolute euro values of Specification 1 (regressing a dummy for selling security 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 right and left hand side graphs stem from two regressions, one with the fictitious loss and gain split up into quintiles and one with the actual loss and gain split up into quintiles. 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, and the "actual gain quintiles" display the coefficients of a fictitious loser for each actual gain quintile.

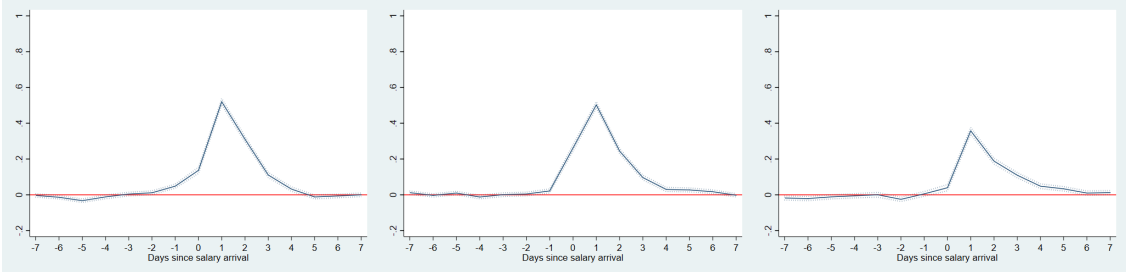


Figure 8: Payday responses of ATM withdrawals plus POS transactions plus non-recurring, domestic wires (Lastschrift and Überweisungen) in the two weeks around salary receipt for three terciles of income (left side, low income; right side, high income) when controlling for individual, day-of-week, week-of-month, and month-by-year fixed effects. Standard errors are clustered at the individual level and displayed as dashed lines.

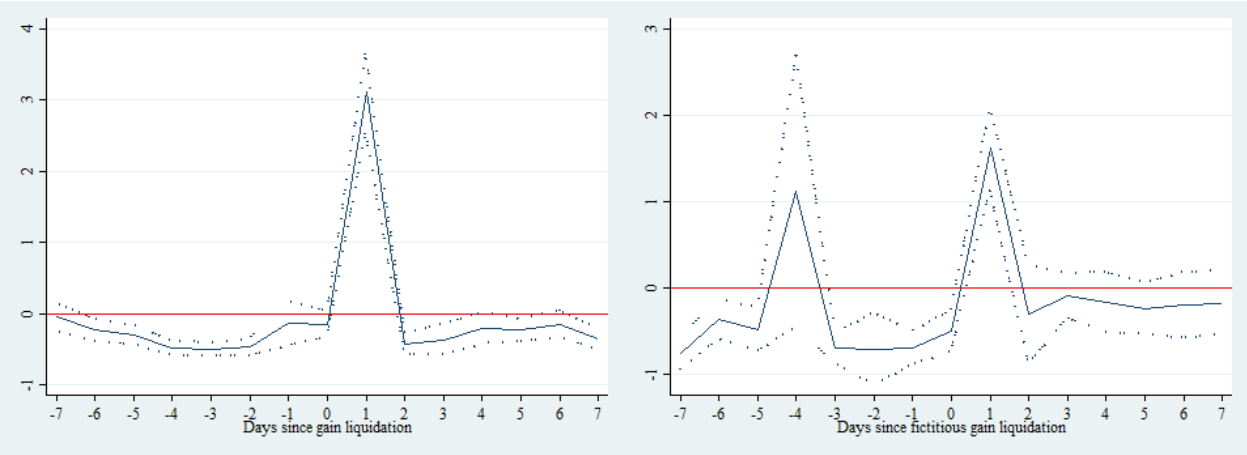


Figure 9: Responses of ATM withdrawals and POS transactions in the two weeks around liquidation of funds that were actual winners (left side) and fictitious winners (right side) when controlling for individual, day-of-week, week-of-month, and month-by-year fixed effects. Standard errors are clustered at the individual level and displayed as dashed lines.

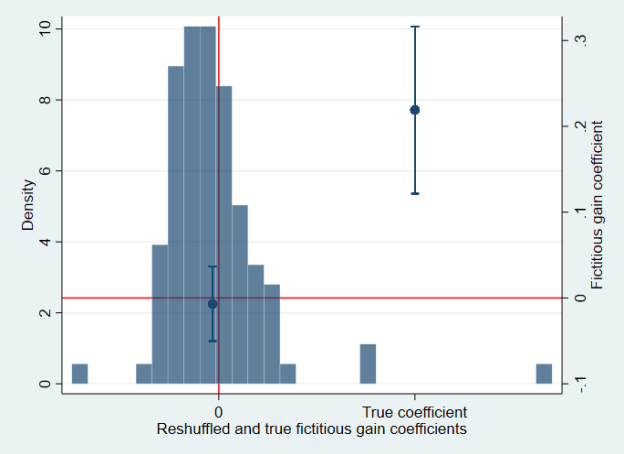


Figure 10: Distribution of fictitious gain coefficients for 100 reshuffled placebo regressions. As in Specification 2, we regress consumption (ATM withdrawals plus POS transactions plus non-recurring domestic wires) on fictitious capital gains while controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, and actual capital gains and losses, all aggregated to the monthly level, with standard errors clustered at the individual level. We reshuffle all variables among all investors. We display the distribution of estimated coefficients for fictitious capital gains as well as the average reshuffled coefficient and its average standard error. Additionally, we show the true fictitious gain coefficient with its standard error.

Table 1: Summary statistics for all individuals and affected individuals, those who held a fund in 2018, for the years 2017 and 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 (1 is an underdiversified portfolio, a mutual fund is assumed to consist of 100 stocks as is standard in the literature). Number of logins and trades are per year averages. Turnover equals 0.5 * the sum of all purchases per month divided by the end-of-month portfolio value plus 0.5 * 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 of a dummy for being affected on all 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
consumption							
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
recurring wire transfers	1,247.33	1,859.91	100.00	300.00	780.00	1,500.00	2,840.00
non-recurring wire transfers	1,691.03	12,396.22	51.19	200.00	635.87	1,617.20	3,366.59
income							
salary received	3,871.91	13,004.80	772.97	1,814.31	2,836.78	4,338.33	6,731.89
dividends received	624.01	9,901.23	5.39	022.67	094.13	352.01	1,032.55
trading costs							
trading fees	23.09	117.20	0.70	1.40	5.95	29.00	54.95
liquidations							
capital gains actual winners	9,061.99	148,811.39	7.09	056.50	449.90	3,136.40	12,948.92
capital gains fictitious winners	294.27	668.57	0.80	004.65	41.47	257.40	956.40
capital losses actual losers	-452.23	630.51	-1911.57	-566.40	-142.00	-30.60	-4.24
capital losses fictitious losers	-4,163.25	3,950.12	-11,258.73	-6,848.94	-2,697.21	-900.17	-136.33
liquidations net of reinvestments	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 from January 2017 through 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 and only 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 PLR	0.050	0.148	0.004	0.082

Notes: The proportion of realized gains (PGR) is defined as the number of realized gains relative to the number of all (realized and unrealized) gains in the portfolio, and the proportion of realized losses (PLR) is defined as the number of realized losses relative to the number of all (realized and unrealized) losses in the portfolio. The disposition effect then equals the difference between the two. The statistics are calculated using all investors and the whole sample period.

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		
	indicator of sale				
gain	0.0607***	-0.00312***	0.00771***	0.00797***	-0.00341
	(0.000808)	(0.000732)	(0.00198)	(0.00197)	(0.00251)
fictitious gain			0.0409***	0.0426***	0.0506***
			(0.00408)	(0.00407)	(0.00456)
fictitious loss			-0.00824***	-0.00463**	-1.49e-06
			(0.00211)	(0.00198)	(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, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: We estimate 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 controlling for individual, month-by-year, or fund fixed effects (as indicated).

Table 5: OLS estimation results of different measures of consumption on (fictitious) capital gains and losses

	all securities 2017 and 2018					
	log consumption (ATM + POS)				log consumption (ATM + POS + wires)	
	all bank clients			main customers	all bank clients	main customers
log fictitious gain	0.243***	0.169***	0.167***	0.421***	0.313***	0.651***
	(0.0235)	(0.0236)	(0.0202)	(0.0518)	(0.0317)	(0.0769)
log fictitious loss	-0.120***	-0.0545***	-0.0536***	-0.110***	-0.0989***	-0.181***
	(0.00265)	(0.00343)	(0.00250)	(0.00506)	(0.00387)	(0.00718)
log gain		✓	✓	✓	✓	✓
log loss		✓	✓	✓	✓	✓
log liquidation minus reinvestment			✓	✓	✓	✓
log portfolio value			✓	✓	✓	✓
individual fixed effects			✓	✓	✓	✓
month-by-year fixed effects			✓	✓	✓	✓
observations	609,693	609,693	597,005	141,104	597,005	141,104
R squared	0.042	0.087	0.403	0.443	0.433	0.490

Standard errors (clustered at the individual level, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: We estimate Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring 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 and portfolio values (as indicated), all aggregated to the monthly level. We keep observations with zero values.

Table 6: 2SLS IV estimation results of consumption (ATM withdrawals and POS transactions) on fictitious capital gains instrumented by total potential fictitious capital gains

	all securities 2017 and 2018						
	OLS	first stage		IV		IV with dummy as instrument	OLS with instrument
	log consumption (ATM + POS)	log realized fictitious gains	log consumption (ATM + POS)				
log realized fictitious gain (instrumented in IV specs)	0.0954*** (0.0209)		0.563*** (0.0556)	0.426*** (0.0528)	0.233*** (0.0506)	0.590*** (0.0713)	
log realized fictitious loss	-0.0938*** (0.0132)				-0.0825*** (0.0138)	-0.0532*** (0.0154)	-0.0958*** (0.0130)
log realized gain	0.111*** (0.00213)				0.111*** (0.00213)	0.111*** (0.00213)	0.111*** (0.00213)
log realized loss	-0.139*** (0.00288)				-0.139*** (0.00288)	-0.139*** (0.00289)	-0.139*** (0.00288)
log total potential fictitious gain (instrument)		0.113*** (0.00569)					0.0254*** (0.00543)
individual fixed effects	✓	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓	✓
log liquidation minus reinvestment	✓			✓	✓	✓	✓
log portfolio value	✓			✓	✓	✓	✓
observations	598,069	598,924	598,924	598,069	598,069	598,069	598,069
R squared	0.400	0.239					0.089
F-statistic		26.06					
Cragg-Donald Wald F-statistic			> 100	> 100	> 100	> 100	

Standard errors (clustered at the individual level, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Column 1 contains the OLS regression results of Specification 2, and Column 2 contains the first-stage regression, Specification 3, of realized fictitious capital gains on total potential fictitious capital gains (the average fictitous capital gains the individual could have liquidated in that month). Columns 3 to 5 contain the 2SLS IV Specification 4 regressing consumption (ATM withdrawals plus POS transactions) on fictitious capital gains instrumented by total potential capital gains (using a dummy for this instrument being positive in Column 7) (using a dummy for this instrument being positive in Column 6) controlling for individual and month-by-year fixed effects as well as the fictitious capital losses, actual capital gains and losses, available liquidated amounts, and overall portfolio value (as indicated), all aggregated to the monthly level. Column 7 shows the equivalent OLS specification using the instrument as the regressor. All variables are logged (if the variable is negative, we first take the absolute value and then transform to negative again) leaving values between -1 and 1 untransformed and keeping observations with zero values.

Table 7: 2SLS IV estimation results of consumption (ATM withdrawals and POS transactions and domestic wires) on fictitious capital gains instrumented by total potential fictitious capital gains

	all securities 2017 and 2018						
	OLS	first stage	IV			IV with dummy as instrument	OLS with instrument
	log consumption (ATM + POS + wires)	log realized fictitious gains	log consumption (ATM + POS)				
log realized fictitious gain (instrumented in IV specs)	0.155*** (0.0357)		1.354*** (0.100)	0.998*** (0.0897)	0.501*** (0.0803)	1.325*** (0.115)	
log realized fictitious loss	-0.268*** (0.0221)				-0.240*** (0.0240)	-0.172*** (0.0288)	-0.268*** (0.0221)
log realized gain	0.308*** (0.00310)				0.308*** (0.00310)	0.308*** (0.00311)	0.308*** (0.00310)
log realized loss	-0.314*** (0.00406)				-0.314*** (0.00407)	-0.313*** (0.00408)	-0.314*** (0.00406)
log total potential fictitious gain (instrument)		0.113*** (0.00569)					0.0546*** (0.00842)
individual fixed effects	✓	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓	✓
log liquidation minus reinvestment	✓			✓	✓	✓	✓
log portfolio value	✓			✓	✓	✓	✓
observations	598,069	598,924	598,924	598,069	598,069	598,069	598,069
R squared	0.431	0.239					0.431
F-statistic		26.06					
Cragg-Donald Wald F-statistic			> 100	> 100	> 100	> 100	

Standard errors (clustered at the individual level, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Column 1 contains the OLS regression results of Specification 2, and Column 2 contains the first-stage regression, Specification 3, of realized fictitious capital gains on total potential fictitious capital gains (the average fictitous capital gains the individual could have liquidated in that month). Columns 3 to 5 contain the 2SLS IV Specification 4 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring domestic wires) on fictitious capital gains instrumented by total potential capital gains (using a dummy for this instrument being positive in Column 6) controlling for individual and month-by-year fixed effects as well as the fictitious capital losses, actual capital gains and losses, available liquidated amounts, and overall portfolio value (as indicated), all aggregated to the monthly level. Column 7 shows the equivalent OLS specification using the instrument as the regressor. All variables are logged (if the variable is negative, we first take the absolute value and then transform to negative again) leaving values between -1 and 1 untransformed and keeping observations with zero values.

Table 8: Estimation results of probability of sale on dummies for the security (2004 to 2018 January to June) being a winner, a (placebo) fictitious winner, or a (placebo) fictitious loser interacted with a dummy for 2018

	funds 2004 to 2018 January to June			
	indicator of sale			
gain	0.0265***	0.0268***	0.0243***	0.0191***
	(0.000553)	(0.000582)	(0.000820)	(0.000850)
fictitious gain	0.0246***	0.0215***	0.0219***	0.0199***
(placebo in 2004 to 2017)	(0.00145)	(0.00156)	(0.00189)	(0.00184)
fictitious gain	0.0286***	0.0320***	0.0310***	0.0313***
(placebo in 2004 to 2017) * 2018	(0.00332)	(0.00336)	(0.00360)	(0.00354)
fictitious loss	-0.0148***	-0.00969***	-0.00918***	-0.00629***
(placebo in 2004 to 2017)	(0.00141)	(0.00152)	(0.00181)	(0.00175)
fictitious loss	0.00130	-0.00276	-0.00105	-0.000673
(placebo in 2004 to 2017) * 2018	(0.00195)	(0.00208)	(0.00226)	(0.00227)
month-by-year fixed effects		✓	✓	✓
individual fixed effects			✓	✓
funds fixed effects				✓
observations	2,726,458	2,726,458	2,721,929	2,721,433
R squared	0.002	0.006	0.104	0.127

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

Notes: We estimate 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, month-by-year, or fund fixed effects (as indicated). The (placebo) fictitious winner or loser variables are placebos in the years 2004 to 2017, e.g., the fund is a placebo winner if the year-to-date return is positive but the actual return is negative, but the actual fictitious winners and losers in 2018. The year 2018 is a dummy for the year 2018. The sample period only includes January 1 to June 30 in the years 2004 to 2018.

Table 9: 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 uninformedness

	funds 2018					
	indicator of 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***	0.046***	0.037***	0.035***	0.053***	0.057***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
fictitious loss	-0.000	-0.000	-0.000	-0.000	0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
fictitious gain	0.022***					
* early 2018	(0.006)					
fictitious gain		0.031***				
* first five trades		(0.009)				
fictitious gain			0.026***			
* first half of trades			(0.005)			
fictitious gain				0.023***		
* traded little				(0.006)		
fictitious gain					-0.004	
* savings plan trader					(0.006)	
fictitious gain						-0.015**
* inattentive trader						(0.006)
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: Specifiation 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 trades before the end of March 2018, the subset of each individual's first five trades in 2018, 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 10: 2SLS IV estimation results of consumption (ATM withdrawals and POS transactions plus wires) on realized fictitious capital gains instrumented by total fictitious capital gains interacted with different measures for investor uninformedness

	all securities 2017 and 2018					
	log consumption (ATM + POS + wires)					
log fictitious gain (instrumented)	0.200**	0.090	0.825***	-0.244**	-0.082	0.593***
	(0.096)	(0.331)	(0.106)	(0.113)	(0.091)	(0.084)
log fictitious gain (instrumented) * early 2018 indicator	0.656***					
	(0.141)					
log fictitious gain (instrumented) * first five trades		0.432				
		(0.334)				
log fictitious gain (instrumented) * first half of trades			-0.790***			
			(0.138)			
log fictitious gain (instrumented) * infrequent trader				1.202***		
				(0.153)		
log fictitious gain (instrumented) * savings plan trader					1.104***	
					(0.161)	
log fictitious gain (instrumented) * inattentive trader						-0.946***
						(0.296)
log gain	✓	✓	✓	✓	✓	✓
log loss	✓	✓	✓	✓	✓	✓
log fictitious loss	✓	✓	✓	✓	✓	✓
log portfolio value	✓	✓	✓	✓	✓	✓
log liquidation minus reinvestment	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓	✓	✓
observations	598069	598069	598069	598069	598069	598069
Cragg-Donald Wald F-statistic	> 100	> 100	> 100	> 100	> 100	> 100

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: 2SLS IV Specification 4 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring domestic wires) on fictitious capital gains and losses instrumented by total fictitions both interacted with different variables indicating an early trade (before end of March 2018), one of the first five trades of each individual in 2018, one of the first half of trades of each individual in 2018, 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 portfolio value and the available liquidated amounts all aggregated to the monthly level. All variables are logged (transformed to negative again when negative) leaving values between -1 and 1 untransformed.

Table 11: OLS estimation results of consumption (ATM plus POS plus wires) on (fictitious) capital gains and losses interacted with different measures for investor uninformedness

	all securities 2017 and 2018					
	log consumption (ATM + POS + wires)					
log fictitious gain	0.250***	0.014	0.322***	0.109**	0.182***	0.316***
	(0.042)	(0.096)	(0.044)	(0.047)	(0.041)	(0.035)
log fictitious gain * early 2018 indicator	0.144**					
	(0.063)					
log fictitious gain * first five trades		0.323***				
		(0.100)				
log fictitious gain * first half of trades			-0.014			
			(0.063)			
log fictitious gain * infrequent trader				0.354***		
				(0.064)		
log fictitious gain * savings plan trader					0.256***	
					(0.066)	
log fictitious gain * inattentive trader						0.004
						(0.110)
log gain	✓	✓	✓	✓	✓	✓
log loss	✓	✓	✓	✓	✓	✓
log fictitious loss	✓	✓	✓	✓	✓	✓
log portfolio value	✓	✓	✓	✓	✓	✓
log liquidation minus reinvestment	✓	✓	✓	✓	✓	✓
liquidation dummy	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓	✓	✓
observations	607673	607673	607673	607673	607673	607673
R squared	0.372	0.372	0.372	0.372	0.372	0.372

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 end of March 2018), one of the first five trades of each individual in 2018, one of the first half of trades of each individual in 2018, 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. All variables are logged (if the variable is negative, we first take the absolute value and then transform to negative again) leaving values between -1 and 1 untransformed.

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€ (or less than 801€

	funds 2018				
	portfolio value < 50,000€ capital gains < 1602€				capital gains < 801€
	indicator of sale				
gain	0.0157***	0.0132***	0.0106***	-0.00925**	-0.00859**
	(0.00298)	(0.00310)	(0.00310)	(0.00428)	(0.00432)
fictitious gain	0.0394***	0.0377***	0.0410***	0.0453***	0.0422***
	(0.00456)	(0.00450)	(0.00470)	(0.00574)	(0.00573)
fictitious loss	0.00505*	0.00498*	0.00661**	-0.00732**	-0.00614
	(0.00266)	(0.00266)	(0.00267)	(0.00372)	(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, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: We estimate 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 tax-free allowance) or 801€ (individual tax-free allowance) controlling for individual, month-by-year, or fund fixed effects (as indicated).

A Appendix

Table A.13: OLS estimation results of different measures of consumption on (fictitious) capital gains and losses

	all securities 2017 and 2018					
	consumption in € (ATM + POS)				consumption in € (ATM + POS + wires)	
fictitious gain in €	0.206**	0.203**	0.154**	0.310**	0.238**	0.486**
	(0.0848)	(0.0844)	(0.0684)	(0.154)	(0.0937)	(0.224)
fictitious loss in €	-5.79e-05	-5.48e-05	-0.000115**	-0.000365*	-0.000276*	-0.00119*
	(4.87e-05)	(4.77e-05)	(4.73e-05)	(0.000214)	(0.000161)	(0.000613)
gain in €		✓	✓	✓	✓	✓
loss in €		✓	✓	✓	✓	✓
liquidation minus reinvestment in €			✓	✓	✓	✓
portfolio value in €			✓	✓	✓	✓
individual fixed effects			✓	✓	✓	✓
month-by-year fixed effects			✓	✓	✓	✓
observations	609,693	609,693	597,005	141,104	597,005	141,104
R squared	0.000	0.004	0.232	0.242	0.133	0.122

Standard errors (clustered at the individual level, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: We estimate Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring 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 and portfolio values (as indicated), all aggregated to the monthly level. We keep observations with zero values. All variables are logged (if the variable is negative, we first take the absolute value and then transform to negative again) leaving values between -1 and 1 untransformed and keeping observations with zero values.

Table A.14: 2SLS IV estimation results of consumption (ATM withdrawals and POS transactions) on fictitious capital gains instrumented by total potential fictitious capital gains

	all securities 2017 and 2018						
	OLS	first stage	IV			IV with dummy as instrument	OLS with instrument
	consumption in € (ATM + POS)	realized fictitious gains in €	consumption in € (ATM + POS + wires)				
realized fictitious gain in €	0.142**		0.445**	0.435**	0.404**	3.376***	
(instrumented in IV specs)	(0.0678)		(0.178)	(0.178)	(0.176)	(0.767)	
realized fictitious loss in €	-0.149***				-0.143***	-0.0800	-0.149***
	(0.0280)				(0.0300)	(0.0596)	(0.0278)
realized gain in €	0.000166				0.000166	0.000163	0.000166
	(0.000241)				(0.000241)	(0.000239)	(0.000242)
realized loss in €	6.68e-05				6.63e-05	6.03e-05	6.67e-05
	(0.000186)				(0.000185)	(0.000183)	(0.000186)
total potential fictitious gain in € (instrument)		0.129***					0.0519**
		(0.0303)					(0.0226)
individual fixed effects	✓	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓	✓
liquidation minus reinvestment in €	✓			✓	✓	✓	✓
portfolio value in €	✓			✓	✓	✓	✓
observations	598,069	598,924	598,924	598,069	598,069	598,069	598,069
R squared	0.232	0.256					0.232
F-statistic		9.71					
Cragg-Donald Wald F-statistic			> 100	> 100	> 100	> 100	

Standard errors (clustered at the individual level, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Column 1 contains the OLS regression results of Specification 2, and Column 2 contains the first-stage regression, Specification 3, of realized fictitious capital gains on total potential fictitious capital gains (the average fictitous capital gains the individual could have liquidated in that month). Columns 3 to 5 contain the 2SLS IV Specification 4 regressing consumption (ATM withdrawals plus POS transactions) on fictitious capital gains instrumented by total potential capital gains (using a dummy for this instrument being positive in Column 6) controlling for individual and month-by-year fixed effects as well as the fictitious capital losses, actual capital gains and losses, available liquidated amounts, and overall portfolio value (as indicated), all aggregated to the monthly level. Column 7 shows the equivalent OLS specification using the instrument as the regressor. We keep observations with zero values.

Table A.15: 2SLS IV estimation results of consumption (ATM withdrawals, POS transactions, and domestic wires) on fictitious capital gains instrumented by total potential fictitious capital gains

	all securities 2017 and 2018						
	OLS	first stage		IV		IV with dummy as instrument	OLS with instrument
	consumption in € (ATM + POS + wires)	realized fictitious gains in €	consumption in € (ATM + POS + wires)				
realized fictitious gain in €	0.213**		1.134***	1.101***	1.037***	13.15***	
(instrumented in IV specs)	(0.0925)		(0.375)	(0.371)	(0.366)	(2.015)	
realized fictitious loss in €	-0.304***				-0.286***	-0.0294	-0.301***
	(0.0572)				(0.0517)	(0.111)	(0.0563)
realized gain in €	0.000325				0.000325	0.000315	0.000325
	(0.000401)				(0.000401)	(0.000390)	(0.000401)
realized loss in €	0.000191				0.000189	0.000165	0.000190
	(0.000252)				(0.000251)	(0.000242)	(0.000251)
total potential fictitious gain in € (instrument)		0.129***					0.133***
		(0.0303)					(0.0466)
individual fixed effects	✓	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓	✓
liquidation minus reinvestment in €	✓			✓	✓	✓	✓
portfolio value in €	✓			✓	✓	✓	✓
observations	598,069	598,924	598,924	598,069	598,069	598,069	598,069
R squared	0.133	0.256					0.133
F-statistic		9.71					
Cragg-Donald Wald F-statistic			> 100	> 100	> 100	> 100	

Standard errors (clustered at the individual level, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Column 1 contains the OLS regression results of Specification 2, and Column 2 contains the first-stage regression, Specification 3, of realized fictitious capital gains on total potential fictitious capital gains (the average fictitous capital gains the individual could have liquidated in that month). Columns 3 to 5 contain the 2SLS IV Specification 4 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring domestic wires) on fictitious capital gains instrumented by total potential capital gains (using a dummy for this instrument being positive in Column 6) controlling for individual and month-by-year fixed effects as well as the fictitious capital losses, actual capital gains and losses, available liquidated amounts, and overall portfolio value (as indicated), all aggregated to the monthly level. Column 7 shows the equivalent OLS specification using the instrument as the regressor. We keep observations with zero values.

Table A.16: 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	
	indicator of sale				
gain	0.0607***	-0.00312	0.00519	0.00797*	-0.00341
	(0.00262)	(0.00316)	(0.00533)	(0.00389)	(0.00592)
fictitious gain			0.0371*	0.0426**	0.0506***
			(0.0146)	(0.0108)	(0.00934)
fictitious loss			-0.00825	-0.00463	-1.49e-06
			(0.00528)	(0.00420)	(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, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: We estimate 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 controlling for individual, month-by-year, or fund fixed effects (as indicated).

Table A.17: OLS estimation results of different measures of consumption on (fictitious) capital gains and losses

	all securities 2017 and 2018					
	consumption in € (ATM + POS)				consumption in € (ATM + POS + wires)	
	all bank clients			main customers	all bank clients	main customers
fictitious gain in €	0.206***	0.203***	0.154**	0.298***	0.238**	0.486**
	(0.0503)	(0.0582)	(0.0591)	(0.0949)	(0.0921)	(0.207)
fictitious loss in €	-5.79e-05	-5.48e-05	-0.000115**	-0.000367	-0.000276	-0.00119*
	(4.61e-05)	(4.49e-05)	(5.45e-05)	(0.000245)	(0.000160)	(0.000676)
gain in €		✓	✓	✓	✓	✓
loss in €		✓	✓	✓	✓	✓
liquidation minus reinvestment in €			✓	✓	✓	✓
portfolio value in €			✓	✓	✓	✓
individual fixed effects			✓	✓	✓	✓
month-by-year fixed effects			✓	✓	✓	✓
observations	609,693	609,693	597,005	141,104	597,005	141,104
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, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: We estimate Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring 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 and portfolio values (as indicated), all aggregated to the monthly level. We keep observations with zero values.

Table A.18: 2SLS IV estimation results of consumption (ATM withdrawals and POS transactions) on fictitious capital gains instrumented by total potential fictitious capital gains

	all securities 2017 and 2018						
	OLS	first stage		IV		IV with dummy as instrument	OLS with instrument
	log consumption (ATM + POS)	log realized fictitious gains			log consumption (ATM + POS)		
log realized fictitious gain (instrumented in IV specs)	0.0954***		0.563***	0.426***	0.233***	0.590***	
	(0.0209)		(0.0766)	(0.0594)	(0.0412)	(0.0794)	
log realized fictitious loss	-0.0938***				-0.0825***	-0.0532**	-0.0958***
	(0.0132)				(0.0146)	(0.0252)	(0.0121)
log realized gain	0.111***				0.111***	0.111***	0.111***
	(0.00213)				(0.00303)	(0.00304)	(0.00303)
log realized loss	-0.139***				-0.139***	-0.139***	-0.139***
	(0.00288)				(0.00337)	(0.00337)	(0.00338)
log total potential fictitious gain (instrument)		0.113***					0.0254***
		(0.0177)					(0.00472)
individual fixed effects	✓	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓	✓
log liquidation minus reinvestment	✓			✓	✓	✓	✓
log portfolio value	✓			✓	✓	✓	✓
observations	598,069	598,924	598,924	598,069	598,069	598,069	598,069
R squared	0.400	0.239					0.400
F-statistic		26.06					
Cragg-Donald Wald F-statistic			> 100	> 100	> 100	> 100	

Standard errors (double-clustered at the individual and month levels, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Column 1 contains the OLS regression results of Specification 2, and Column 2 contains the first-stage regression, Specification 3, of realized fictitious capital gains on total potential fictitious capital gains (the average fictitous capital gains the individual could have liquidated in that month). Columns 3 to 5 contain the 2SLS IV Specification 4 regressing consumption (ATM withdrawals plus POS transactions) on fictitious capital gains instrumented by total potential capital gains (using a dummy for this instrument being positive in Column 6) controlling for individual and month-by-year fixed effects as well as the fictitious capital losses, actual capital gains and losses, available liquidated amounts, and overall portfolio value (as indicated), all aggregated to the monthly level. Column 7 shows the equivalent OLS specification using the instrument as the regressor. All variables are logged (if the variable is negative, we first take the absolute value and then transform to negative again) leaving values between -1 and 1 untransformed and keeping observations with zero values.

Table A.19: OLS estimation results of different measures of consumption on (fictitious) capital gains and losses with additional control variables

	all securities 2017 and 2018						
	consumption in € (ATM + POS + wires)						
fictitious gain in €	0.347***	0.323***	0.238**	0.207**	0.194**	0.195**	0.195**
	(0.125)	(0.122)	(0.0994)	(0.0977)	(0.0972)	(0.0973)	(0.0973)
fictitious loss in €	-0.000209	-0.000207	-0.000276	-0.000272	-0.000241*	-0.000240*	-0.000240*
	(0.000180)	(0.000177)	(0.000171)	(0.000166)	(0.000142)	(0.000143)	(0.000143)
gain in €	✓	✓	✓	✓	✓	✓	✓
loss in €	✓	✓	✓	✓	✓	✓	✓
liquidation minus reinvestment in €	✓	✓	✓	✓	✓	✓	✓
portfolio value in €		✓	✓	✓	✓	✓	✓
month-by-year fixed effects			✓	✓	✓	✓	✓
individual fixed effects			✓	✓	✓	✓	✓
liquidation dummy				✓	✓	✓	✓
salary received in €					✓	✓	✓
dividends received in €						✓	✓
interest received in €							✓
observations	609,693	607,673	607,673	607,673	607,673	607,673	607,673
R squared	0.000	0.001	0.134	0.136	0.137	0.137	0.137

Standard errors (clustered at the individual level, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: We estimate 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 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. We keep observations with zero values.

Table A.20: 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	funds 2009
	indicator of sale						
gain	0.0111***	0.00509**	0.0221***	0.0209***	0.0298***	0.0538***	0.0526***
	(0.00358)	(0.00244)	(0.00219)	(0.00205)	(0.00227)	(0.00582)	(0.00129)
fictitious gain	0.0115***	0.0320***	0.0304***	-0.0287***	-0.0768***	0.0299***	0.00368**
	(0.00379)	(0.00274)	(0.00256)	(0.00285)	(0.00503)	(0.00612)	(0.00170)
fictitious loss	0.00995***	-0.00273	0.00679***	-0.0263***	-0.0299***	0.0279***	0.00897***
	(0.00283)	(0.00207)	(0.00174)	(0.00166)	(0.00328)	(0.00585)	(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	funds 2014	funds 2015	funds 2016
	sale	sale	sale	sale	sale	sale	sale
gain	0.00797***	0.00503***	0.0169***	0.0112***	0.000452	0.0275***	0.0122***
	(0.00256)	(0.00163)	(0.00393)	(0.00221)	(0.00223)	(0.00470)	(0.00170)
fictitious gain	-0.00438	-0.0121***	0.00175	0.00122	0.00196	0.0256***	-0.00362
	(0.00271)	(0.00304)	(0.00419)	(0.00287)	(0.00294)	(0.00513)	(0.00324)
fictitious loss	-0.00457**	-0.00235	-0.00381	-0.00806***	-0.00714***	-0.0102**	-0.00952***
	(0.00214)	(0.00256)	(0.00361)	(0.00205)	(0.00199)	(0.00447)	(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, singleton observations dropped) in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: We estimate 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.