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ABSTRACT

We use individual-level data on all security trades, holdings, spending, and income from an online retail bank. We study the effects of an exogenous change in the displayed purchase prices of the mutual funds in individuals' portfolios. We find that individuals are more likely to sell what we call fictitious winners, i.e., funds that are winners under the newly displayed purchase price but are losers under the actual purchase price. We also document that individual consumption increases in response to realizing fictitious capital gains. We thus document a causal link among purchase prices, trades, and consumption using observational data and find that the trading and consumption results are more prevalent for less-informed investors. We thereby document a marginal propensity to consume out of (confused) capital gains, which is informative about the literature on consumption out of stock market wealth.

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1 Introduction

Stock and mutual fund holdings represent a significant fraction of household financial wealth comparable to the stock of housing wealth.[1] Fluctuations in stock prices should thus significantly affect households' investment, savings, and consumption decisions. Furthermore, fluctuations in stock prices may be a source of disutility that makes households reluctant to invest in the stock market in the first place (Campbell, 2006; Barberis et al., 2006; Pagel, 2018). Unlike house prices, stock prices are highly volatile and transparent, which may amplify the disutility associated with monitoring one's wealth. Additionally, stocks can be easily monetized if consumption needs arise or households experience changes in beliefs about stock market performance or preferences for risk taking. There exists a large empirical literature documenting that individuals prefer to sell winning stocks and hold losing stocks (see Odean, 1998; Chang et al., 2016). Researchers have argued that this finding is difficult to rationalize within standard economic models and have highlighted the importance of non-standard preferences or beliefs to explain such behavior. However, whether the disposition effect has real effects in terms of changing consumption is unclear and empirical evidence on how much individuals consume out of stock market wealth remains scarce.

Clearly, estimating the marginal propensity to consume (MPC) out of investing in winning stocks or stock market wealth more generally is difficult. Individuals endogenously decide to trade successful or unsuccessful investments in response to individual shocks to consumption needs or aggregate fluctuations in consumer confidence. Therefore, the relationships between trading, individual consumption, stock price fluctuations, and other aggregate variables are subject to common shocks. However, the economic environment, individual wealth, and changes in preferences are not the only factors that influence consumption; subjective feelings of being a successful investor also have an effect. Psychologists have long known that "being rich or poor is a state of mind" (Tang et al., 2004). In this paper, we show that subjective feelings of investment success, or having invested in winning as opposed to losing stocks or funds, affect trading. Additionally, we attempt to determine the extent to which the subjective feeling of being a successful investor matters and

[1] See, e.g., Pfeffer et al. (2016).

whether such behavior has any real consequences in affecting consumption.

To investigate the effect of selling a winning investment on individual investor consumption, we use a unique panel dataset on the daily trading of more than 100,000 private investors in Germany spanning the years 2003 to 2018. We precisely measure each individual's daily activity by his or her login and trading behavior and all of his or her transactions and balances in his or her settlement, savings, and checking accounts. As a source of exogenous changes in the subjective feeling of investment success, we utilize the implementation of a capital gains tax reform in Germany in January 2018. The capital gains tax reform simplified the tax treatment of retaining domestic and foreign funds, i.e., funds that automatically reinvest capital gains, dividends, and interest payments.

To simplify the process of assessing capital gains taxes after the reform and subtracting them at the bank level, the majority of German banks initiated a fictitious sale on January 1, 2018, of all funds and ETFs individuals held. This fictitious sale changed the displayed purchase prices of all funds and ETFs (but no other investments) on January 1, 2018, to take the December 29, 2017, closing price, as quoted by the exchange. In turn, this new purchase price determined the displayed absolute capital gains and losses as well as their displayed percentage returns. Importantly, the reform did not have any real implications for individual tax liabilities because the actual purchase prices and cost basis of any investments as well as the capital gains tax rate remained the same.

We estimate the effects of these changes in the displayed purchase prices and capital gains or losses on trades using a linear probability model as is standard in the disposition-effect literature.[2] We find that individuals are affected by the change in the purchase price and their respective displayed capital gains or losses: 1) individuals' likelihood to sell a fund is 4.09% higher when funds are displayed as (fictitious) winners even though they are losers based on the actual purchase prices and 2) individuals' likelihood to sell a fund is 0.82% lower when funds are displayed as (fictitious) losers even though they are actual winners.

[2]We document a disposition effect for all stocks and funds that is of similar magnitude to that in the literature. The propensity to sell winning stocks is higher than the propensity to sell winning funds. In the overall sample of 103,000 German investors over the period 2003 to 2018, we find a slightly negative disposition effect for only funds. This result is in line with Chang et al. (2016), who use the data from Odean (1998) consisting of 73,558 US households from January 1991 to November 1996 and document a reverse disposition effect for delegated investments such as mutual funds. However, when we split the sample by years, for instance, we generally find an attenuated, sometimes positive and sometimes negative disposition to sell winning funds. Nevertheless, the propensity to sell fictitious winning funds is large and positive in all sample splits and robustness checks we performed.

Next, we employ a two-stage instrumental variable strategy to estimate the effect of realizing fictitious winners on consumption. Specifically, we use individuals' tendencies to sell fictitious winners as a first stage to estimate the individual MPC out of realizing fictitious capital gains. We can use the effect of fictitious capital gains on selling as a first stage because the fictitious capital gain is a plausibly exogenous shock to each individual's wealth once we control for time fixed effects to account for fluctuations in all aggregate variables such as stock market performance or consumer confidence. In the second stage, we then estimate the MPC out of each euro of fictitious capital gains. We find that individuals consume approximately 15.7% out of each euro of realized fictitious capital gains in their funds when we measure consumption in ATM withdrawals and point of sale transactions. Individuals consume 24.5% out of each euro of realized fictitious capital gains when we also include domestic wires in our measure of consumption.

Our experiment is different from simply regressing consumption on capital gains for two reasons. First, 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. Second, in our case, individuals are not actually wealthier and therefore should not consume more. Our experiment provides a situation in which the fictitious capital gain status is not reflected in an individual's wealth because individuals are subject to an actual capital loss. Now, it matters for the interpretation of our results what are individuals' information sets and whether they know that they experienced a capital loss but are merely shown a fictitious capital gain.

We thus further analyze two potential explanations for our findings: 1) individuals are confused about what their capital gains are, and 2) individuals are confused about the tax implications of the reform. With respect to 1), it could be that, despite receiving a sales receipt by email and mail that clearly states the purchase and sales price as well as fees paid and the incurred capital gains or losses, investors remain confused about the tax implications of the reform. We attempt to assess whether individuals are simply confused about what their actual capital gains are using sample splits based on how informed investors may be. These sample splits indicate that our effects are generally stronger for investors who appear less informed. Specifically, we find stronger effects for both trading and consumption when we examine each investor's earlier trades (trades that occur earlier

in the year or also earlier trades relative to each investor's own history of trades in 2018). With respect to 2), it could be that individuals are confused about the tax implications of their selling behavior. In principle, if individuals think their cost basis is determined by the newly displayed purchase prices, they should not sell fictitious winners. Nevertheless, we also perform sample splits to single out groups of individuals who are likely to be certain that taxes are irrelevant. Specifically, we single out individuals with small portfolios whose capital gains are well below the annual tax-free allowances. Here, we do not find evidence that individuals are confused about the tax implications of the reform.

By estimating the effects of exogenously changed purchase prices on the disposition to sell winners and hold losers, we contribute to the large literature on the disposition effect. The initial finding by Odean (1998) was further analyzed in a number of follow-up papers such as Barber and Odean (2000), Kaustia and Knüpfer (2008), Chang et al. (2016), Koestner et al. (2017), and Meng and Weng (2017), among many others. Specifically, our findings are related to a few recent papers on the disposition effect. We follow Frydman and Wang (2019) in providing causal evidence for the disposition effect. In contrast to an exogenous change in the purchase price, Frydman and Wang (2019) analyze a change in the salience of purchase prices from a natural experiment in which the online broker added price variables and color-coded gains and losses to make them visually more apparent to investors. This paper follows Frydman and Rangel (2014), who use a laboratory experiment to demonstrate the effects of displaying or omitting individual purchase prices. Additionally, Birru (2015) also finds that retail investors confuse winning and losing stocks after stock splits rather than properly adjusting their purchase price points. Furthermore, the different effects of fictitious winners versus losers suggest that individuals are happy to take a fictitious winner at face value and close the mental account (as suggested in Frydman et al., 2015).

We argue that our findings help us to understand the mechanisms and psychologies behind the disposition effect. We have a natural experiment in which purchase prices are changed exogenously, and we find that the displayed purchase prices appear to partly determine a new "reference point" for investors, i.e., the focal point relative to which investments are classified as winners versus losers in theories of reference dependence that explain the disposition effect (Shefrin and Statman, 1985;

Barberis and Xiong, 2009; Meng and Weng, 2017; Barberis and Xiong, 2012) or stock market non-participation and low equity shares (Barberis et al., 2001, 2006; Pagel, 2018). Our results are also relevant for the two main potential confounders of the disposition effect: 1) tax implications and 2) optimal performance strategies. With regard to 1), selling a fictitious winner, even when individuals misperceive the new purchase price as the more tax-relevant one, is less tax efficient than selling a loser. With regard to 2), if individuals follow some optimal trading strategy, even if that cannot be shown in their portfolio performance (see Odean, 1998, for instance), then they should not be affected by the artificial change in the purchase price. Furthermore, we may learn from our paper whether the disposition to realize winners is solely driven by a belief in mean reversion of stock prices (as originally proposed in Odean, 1998). Our findings indicate that the act of selling a winner makes individuals feel richer and consume more. In that sense, the action of selling a winner is likely not solely driven by a belief in mean reversion of stock prices.[3]

We have a unique experiment in which individuals sell a fictitious capital gain if they are subject to the disposition effect, and this fictitious gain is not actually reflected in a real capital gain (out of which individuals should consume). Despite a fictitious winner representing an actual capital loss, which individuals see on their sales receipt or in their order history, they consume out of fictitious capital gains. We thus argue that the disposition effect appears to have real consequences such that individuals feel richer or more successful, which, in turn, affects their consumption. This observation is the main lesson from our paper: the disposition effect is accompanied by feelings of investment success as individuals sell to consume, and if they are under the (mistaken) assumption that they are richer, they consume more. Moreover, perceiving a fictitious winner may relieve individuals of the sense of having made poor investment decisions, which makes them feel better and consume

[3]Furthermore, our paper relates to a literature on how gains, losses, and trading success affect retail investors. The literature has analyzed risk-taking in response to losses in a variety of settings, including choices over lotteries in laboratory experiments (Thaler and Johnson, 1990) and trading decisions of experienced market-makers (Coval and Shumway, 2005), IPO investors (Kaustia and Knüpfer, 2008; Anagol et al., 2015), and individuals receiving inheritances (Andersen et al., 2019). There also exists a literature on how retail investors learn (or not) from their trading success (Barber et al., 2018; Gervais and Odean, 2001; Seru et al., 2009; Linnainmaa, 2011). Beyond trading decisions, our findings are more broadly related to the literature on how personal experiences shape preferences, such as Malmendier and Nagel (2011) and Andersen et al. (2019). Meyer and Pagel (2018) show that individuals appear to learn from bad experiences in the stock market, which complements the experimental evidence in Kuhnen (2015) and Koudijs and Voth (2016). Understanding individual preferences for investing in stocks and funds is important for understanding puzzles in household finance such as the stock market-non-participation puzzle (Campbell, 2006).

more.

Because we provide some evidence that investors are simply confused about what their actual capital gains are, we also contribute to the literature on consumption out of stock market wealth, as we estimate the consumption response to confused capital gains. The literature linking stock market wealth with consumption includes studies employing aggregate and regional variation (e.g., Davis et al. (2001), Dynan and Maki (2001), and Case et al. (2005)).[4] However, endogeneity concerns are likely to affect the interpretation of the estimates in these existing studies, as they use aggregate data and cannot distinguish between the direct effect of changes in stock wealth on consumption and the fact that stock prices are a leading indicator of economic growth and reflect consumer sentiment. There also exist studies employing household-level data, but they either lack disaggregated data on households' actual stock holdings or actual consumption (e.g., Parker, 1999a and Baker et al., 2007). Specifically, Baker et al. (2007) use CEX data and show that stockholder consumption responds strongly to changes in dividend payments but not to changes in stock prices. They also provide suggestive evidence that this behavior is driven by mental accounting. Furthermore, Maggio et al. (2018) obtain the same findings using disaggregated household consumption and asset holdings data from the Swedish wealth registry. Finally, Meyer et al. (2018) estimate the MPC out of realized capital gains using mutual fund liquidations as an exogenous source of forced sale events. As in Meyer et al. (2018), we show that individuals have a high MPC out of capital gains once they are liquidated, which links our paper to the literature on consumption out of cash flows (Parker, 1999b, 2014; Agarwal et al., 2007; Jappelli and Pistaferri, 2010; Olafsson and Pagel, 2018a).

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

2 Data overview, institutional background, and summary statistics

2.1 Data overview

Our data set stems from the clients of one of the largest online banks in Germany.[5] We have daily information on logins (from 2012 onwards), trades, and portfolio holdings of approximately 103,000 customers as well as all balances and transactions of each investor's other accounts at the online bank from 1999 to 2016. We retain only private investors who reside in Germany and obtain data on customer demographics such as gender, age, occupation, and zip code. In online banks, silent attribution is a common phenomenon, as there is usually no charge for having an account. Therefore, to avoid analyzing accounts of investors who stopped trading, we require that individuals execute at least one trade per year. An advantage of our data set is that we can exclude quasi-automatic trades, such as savings plan transactions. Additionally, trading decisions in our sample are not moderated by any influence from third parties, such as financial advisers. To further ensure that our sample includes only self-directed online consumers, we exclude trades from limit orders because these transactions do not reflect current trading decisions of investors. For each trade, we have the security's international identification number (ISIN) and obtain additional information on the security, such as asset class, risk class, issuer or issue date, from Datastream.

2.2 Summary of the natural experiment

The online bank and all other major banks in Germany initiated a fictitious sale on January 1, 2018, to implement the 2018 capital gains tax reform. This sale did not have any tax liability consequences that changed the actual capital gains taxes for our individuals, but it changed the displayed purchase prices and the relative and absolute returns in individuals' online portfolios.

When individuals log in to their online brokerage, they see the purchase price, the current price, their total holdings, and their return since purchase. On January 1, 2018, the purchase price was

[5] The five largest online banks in Germany are (as of 2016): ING-Diba. (8.3 million customers), Deutsche Kreditbank (3.3 million), comdirect (2.1 million), Volkswagen-Bank (1.1 million), and Mercedes-Benz-Bank (1.1 million).

set equal to the December 29, 2017, closing price, as quoted by the exchange, and the return was set to 0%. Thereafter, the value developed as usual. Figure 1 shows screenshots of the online portfolio interface that individuals see. The portfolio interface shows all fund and stock holdings as well as their daily absolute changes and percentage changes, i.e., returns. In addition to the daily change, the interface also shows the absolute changes and percentage changes, i.e., returns, since the purchase of the security position (which are displayed right next to the buy and sell buttons). The latter portion was reset on January 1, 2018, to implement the capital gains tax reform. Investors were informed via email and online upon logging in starting in March 2018. The online notification is also displayed in Figure 1. Throughout 2017 and 2018, the actual purchase prices can be seen by clicking on the order summary. As a default, the order summary screen displays the purchase prices of the three most recent transactions. For many automatic transactions from savings plans, for instance, individuals must dig deeper to find their actual average purchase prices.

[Insert Figure 1 about here]

Let us discuss an example for the four scenarios that the experiment creates: Figure 2 shows the price path of a Deutsche Bank (DWS) US equities mutual fund and two possible purchase prices (called ⓪ and ①) as well as two possible sale prices (denoted by ③ and ④). Purchasing the fund at ① and selling it at ④ creates a real loser or loss because the fund is trading at a loss with respect to its new purchase price and with respect to its original purchase price. Purchasing the fund at ⓪ and selling it at ④ creates a fictitious loser or loss because the fund is trading at a loss with respect to its new purchase price but at a gain with respect to its original purchase price. Purchasing the fund at ⓪ and selling it at ③ creates a real winner or gain because the fund is trading at a gain with respect to its new purchase price and with respect to its original purchase price. Purchasing the fund at ① and selling it at ③ creates a fictitious winner or gain because the fund is trading at a loss with respect to its new purchase price but at a gain with respect to its original purchase price.

[Insert Figure 2 about here]

2.3 Institutional background of the 2018 capital gains tax reform

Since January 1, 2009, private investors in Germany have owed capital gains taxes. Before January 2009, capital gains taxes were owed only if assets were liquidated within a year after purchase and in some other special cases. In contrast, dividends and interest payments were taxed at the personal income tax rate, which could amount to up to 42%. After January 2009, dividends, interest payments, and capital gains were all taxed at the same rate. The capital gains taxes of stocks and funds bought before January 1, 2009, (Altbestände) remain tax-free up to an initial allowance of 100,000 euros. However, any capital gains after January 1, 2018, that exceed 100,000 euros will be taxed. For these stocks and funds, the January 1, 2018, price thus became the new cost basis. For stocks and funds bought after January 1, 2009, capital gains are taxed at the same rate as dividends and interest payments, and the tax is subtracted at the source, i.e., in the event of a capital gains realization, the money that arrives in clients' settlement accounts after a sale are already after-tax funds. This did not change with the 2018 capital gains tax reform. As of 2009, the capital gains tax was 25% (Abgeltungsteuer auf Kapitalerträge) plus the solidarity surcharge (Solidaritätszuschlag) (5.5% of the capital gains tax) and (if applicable) church tax (Kirchensteuer) (9% of the capital gains tax), which amounts to 26.375% to 28.625%. Furthermore, there is an annual tax-free allowance (Freibetrag) of 801 euros for singles and 1602 euros for married couples. Individuals can specify their main brokerage such that the capital gains tax will not be subtracted unless the initial allowance is exceeded (Freistellungsauftrag). Furthermore, if capital losses are realized before capital gains, then the capital gains tax will be automatically lowered by the realized losses. Thus, gains and losses are offset with losses exceeding gains either rolled forward or, upon request, being certified to be considered in individual tax returns. At the brokerage level, the initial allowance and the initial allowance for funds bought before January 2009 are taken into account. To begin taxing all funds bought before 2009 (Altbestände) from their value as of January 1, 2018, was one reason for the capital gains tax reform and the subsequent fictitious sale. However, none of our investors were actually affected because the initial allowance is very high (100,000 euros for singles and 200,000 euros for couples).

The second reason for the reform was to simplify the treatment of retaining foreign funds. Previously, retaining foreign funds capital gains as well as dividends had to be reported separately by investors in their individual tax returns (while retaining domestic funds were taxed at the fund level on their retentions and non-retaining domestic and foreign funds were taxed at the investor level). Now, all funds are treated equally and all tax assessments are done automatically without any reporting requirements for the investor. On all retaining funds, individuals now have to pay a fraction of the taxes on their retentions at the end of the tax year (Vorabpauschale).

The second reason for the reform was to simplify the treatment of retaining foreign funds. Previously, retained capital gains and dividends from foreign funds had to be reported separately by investors in their individual tax returns (while retaining domestic funds were taxed at the fund level on their retentions, and non-retaining domestic and foreign funds were taxed at the investor level).[6] Now, all funds are treated equally, and all tax assessments are done automatically without any reporting requirements for the investor. For all retaining funds, individuals now have to pay a fraction of the taxes on their retentions at the end of the tax year (Vorabpauschale).

To simplify the process of calculating this fraction of capital gains, taxes to be paid were the second reason that the online bank and many other banks fictitiously bought and sold all fund holdings of all clients on January 1, 2018, and reset the purchase price to the December 29, 2017, closing price, as quoted by the exchange. This constitutes a change in when taxes are paid for those individuals who hold foreign retaining funds, but not their effective tax rate. However, 1) many individuals do not hold foreign retaining funds, 2) many of our individuals do not earn capital gains above the initial tax-free allowance (801 euros for singles and 1602 euros for married couples) and thus do not have to pay the Vorabpauschale, and 3) the Vorabpauschale is very small. For instance, for purely retained equity funds in 2018, the Vorabpauschale equals the German prime interest rate (Basiszins of 0.87% in 2018) times 70% 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 6000 euro fund position. The first time that individuals have to pay the Vorabpauschale will be December 2019.

In summary, the fictitious sale did not have any tax liability consequences in terms of changing

[6] In Germany, two-thirds of funds held are domesticated in Luxembourg.

the actual capital gains tax rate for our individuals (as none exceeds 100,000 euros in capital gains for their pre-2009 funds). A subset of individuals who hold foreign retaining funds and who's capital gains exceed the tax-free allowance of 801 euros or 1602 euros are subject to a change in when the retained capital gains tax is paid (December 2019 versus the time of income tax reporting in July 2020).

2.4 Summary statistics

Our sample is not representative of the German population as a whole; less than half of Germans are invested in equities, either directly or indirectly. However, it is a relatively representative sample of self-directed retail investors in Germany. Our sample does not comprise the entirety of the bank's customer base but roughly 7 percent of all customers. The bank did not select the sample of retail investors by trading frequency but instead chose a random subset of all brokerage clients that executed at least one trade per year. In that sense, our sample is representative of individuals in Germany that participate in the stock market. Table 1 shows detailed summary statistics for our universe of investors. As shown in Table 1, the average age of investors is 54, and the median age is 52. Women represent 16 percent of our sample. Brokerage clients are generally expected (Cole et al., 2014) and found to be more sophisticated than the overall population (Dorn and Huberman, 2005). The same is true of our sample: 7.8 percent of our investors hold a doctoral degree, which is higher than average in the German population (according to the German Department of Statistics, 2011).

[Insert Table 1 about here]

Investors own portfolios that are worth 55,836 euros, on average, with a median of 33,586 euros. These descriptive statistics are comparable to those reported by other household finance studies and to official statistics in Germany. The Deutsche Bundesbank (2013) reports the average portfolio value of a German stock market investor to be approximately 48,000 euros. Additionally, we compare our portfolio values to the self-reported (in brackets) wealth measure obtained from a survey when investors opened their accounts. Reassuringly, the numbers are very similar. Additionally,

we compare portfolio holdings to self-reported gross annual household incomes for those investors who reported these data. Since income is reported in several ranges, we use the midpoint of each range as a proxy for investor income. The mean ratio of the average portfolio value (over the entire sample period) to annual income is 1.3. For comparison, the ratio of total financial assets to gross household income in the German population is approximately 1.1 (Bundesbank, 2013).

Our investors are reasonably active on average, the mean number of security purchases over the sample period is 145, and the median is 39, which is slightly less trading than found in Barber and Odean (2000). Furthermore, our investors are reasonably well diversified but typically hold only a handful of different securities (distinct ISINs). When we count funds as containing 100 stocks, we find the mean number of securities to be 46, and when we consider the portfolio Herfindahl index of diversification, we find the mean to be 0.14 (a Herfindahl index larger than 0.5 corresponds to holding only a handful of stocks).

All investors in our sample who held a mutual fund on December 29, 2017, were affected by the exogenous change in the purchase price. Of the 103,000 clients we observe, 37,785 executed at least one sale between January 1, 2018, and June 30, 2018, for whom we have complete records including all spending and income transactions. Of those 37,785 clients, 27,545 were affected because they held funds and all funds were fictitiously repriced, and the average fund share was 18.28%.

The first (top) graph in Figure 3 shows a distribution of the positions in all fund securities in 2018 and their new returns since December 29, 2017, closing price, as quoted by the exchange (either at the time of sale or using their June 30, 2018, prices). The second (bottom) graph in Figure 3 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 in the beginning of 2018. However, the returns of funds that were sold after January 1, 2018, are more often positive, i.e., there is more probability mass in the bar just above 0% returns and more probability mass in the more positive returns domain. Thus, in the raw data, we see that individuals are more likely to sell funds at a gain relative to their December 29, 2017, closing price, i.e., their fictitious sale price.

[Insert Figure 3 about here]

The first (top) graph in Figure 4 shows a distribution of the individual changes in all funds' returns due to the repricing, i.e., 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 (the new return), minus the return from the purchasing date to December 29, 2017 (the old return). The second (bottom) graph in Figure 4 shows the distribution of new minus old 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 new return (mostly because the market performed poorly in 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 new return.

[Insert Figure 4 about here]

Clearly, the fund repricings do not represent an actual wealth shock but only a fictitious shock to subjective feelings of investment success and wealth. Nevertheless, as we can see in these pictures, the repricings had substantial effects on the displayed returns.

Table 2 shows the mean prices for all purchases and sales of all funds and other securities in 2017 and 2018. When funds were not sold, we use the average price from January 1 to June 30, 2018, for the 2018 columns and the average price in 2017 for the 2017 column. This table also shows the mean sizes of funds and other securities trades and the closing prices on December 29, 2017.

[Insert Table 2 about here]

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

[Insert Table 3 about here]

3 Methodology and identification

3.1 Specifications for trades and consumption

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

$$SaleD_j^{it} = \alpha_t + \eta_i + \beta GainD_j^{it} + \gamma FictGainD_j^{it} + \theta FictLossD_j^{it} + \epsilon_j^{it} \tag{1}$$

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

To analyze the effects of fictitious capital gains on consumption, we consider a two-stage instrumental variable (IV) approach after we aggregated all variables at the monthly level. The time-series regression thus employs a panel for each month t from January 2017 to June 2018. We use the fictitious gain status as a first stage to cause liquidations and then estimate the MPC out

of each dollar of fictitious capital gains. In the first stage of the IV approach, we thus regress the endogenous variable, the decision and amount to sell (Liq^{it}) in euros by individual i in each month t on the (unrealized or realized) euro value of the fictitious capital gains ($UFictGain^{it}$). The predicted values of that regression ($\widehat{Liq^{it}}$) equal the realized fictitious capital gains.

$$Liq^{it} = \beta UFictGain^{it} + \epsilon^{it} \Rightarrow \widehat{Liq^{it}} = FictGain^{it}$$

In the second stage of the IV approach, simply regress the euro value of consumption ($Cons^{it}$) on the predicted values of the first stage regression ($\widehat{Liq^{it}} = FictGain^{it}$) as follows:

$$Cons^{it} = \alpha_t + \eta_i + \gamma Gain^{it} + \theta Loss^{it} + \vartheta FictGain^{it} + \varphi FictLoss^{it} + \beta X^{it} + \epsilon^{it} \tag{2}$$

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

3.1.1 Identification strategy

We include individual and month-by-year fixed effects and thus utilize variation at the individual level over the months from January 2017 to June 2018. Whether a fund is displayed at a (fictitious) gain or loss depends on the price on December 29, 2017, and the price movement since, which, conditional on month-by-year fixed effects, is plausibly exogenous to individual investors. Controlling for month-by-year fixed effects is important in Regression (1), as time-varying economic sentiment

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

As discussed in the two-stage IV regression framework above, we can view the variables $FictGain^{it}$ and $FictLoss^{it}$ as instruments. As is evident from the significant coefficients in Specification (1), $FictGain^{it}$ and $FictLoss^{it}$, or fictitious capital gains and losses, affect trading. In particular, individuals sell fictitious winners. $FictGain^{it}$ and $FictLoss^{it}$ are, of course, correlated with the actual capital gains $Gain^{it}$ and losses $Loss^{it}$ because both depend on the course of the fund's price since January 1, 2018. However, we control for actual capital gains or losses with the available liquidation amount Liq^{it}. To the extent that individuals liquidated fictitious winners and losers, $FictGain^{it}$ and $FictLoss^{it}$ are correlated with the decision and amount to liquidate that we control for via Liq^{it}, the liquidation amount minus reinvestment. We thus have an instrument for liquidations, $FictGain^{it}$ and $FictLoss^{it}$, and we then assess in the instrumental variable regression whether the fictitious capital gains and losses cause liquidations and increase consumption.

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

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

To further understand Regression (2) and how we identify the causal effect of fictitious capital gains that induce individuals to sell and consume, we can describe the omitted variables that affect

both consumption $Cons^{it}$ and fictitious capital gains $FictGain^{it}$ and would lead to a spurious correlation rather than a causal relationship in Specification 2.

We are concerned about three types of omitted variables: time, consumption plans or preference shocks, and wealth shocks. First, there are time and aggregate variables that drive both consumption and fictitious capital gains in 2018 (e.g., economic sentiment). As mentioned above, we control directly for any aggregate variables correlated with time using the month-by-year fixed effects α_t. Second, there are individual consumption plans or shocks to individual preferences. Specifically, there is the decision to liquidate, $ILiq^{it}$, and then the amount that is liquidated, which we denoted by Liq^{it}. The decision to liquidate is clearly correlated with the liquidated fictitious capital gains that equal $FictGain^{it} = LiqD^{it} * UFictGain^{it}$ if $UFictGain^{it}$ denotes unrealized fictitious capital gains as above. However, we control for it directly by controlling for $Liq^{it} = LiqD^{it} * (UGain^{it} + InvL^{it} - ReInvL^{it})$ if $UGain^{it} + InvL^{it} - ReInvL^{it}$ equals the unrealized capital gains plus the initial investment amount minus the reinvested amount upon liquidation. Additionally, as mentioned previously, we can control for a liquidation dummy, $LiqD^{it}$. In addition to the decision to liquidate, the amount liquidated is determined by individual shocks or plans and may be correlated with aggregate fluctuations in the stock market, but we 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, again we control for realized capital gains directly via Liq^{it}. The omitted variable bias theorem tells us that when we run a regression of the form $Y = \beta X + \epsilon$ and a variable Z (in our case, the decision to liquidate plus how much) affects both X and Y, then β is biased. However, β is unbiased if we control for Z in the regression $Y = \beta X + \rho Z + \epsilon$, which is exactly what we do here. As an alternative to interpreting our regression as a two-stage specification, we can simply identify a causal effect of fictitious capital gains and consumption by directly controlling for the omitted variables. Furthermore, we again note that we control directly for these omitted variables, i.e., we do not use proxies and the omitted variables that we control for (such as actual capital gains or the liquidated amount) are measured precisely in euro values and thus entirely free of error.

4 Results

4.1 Empirical results for trading

Columns 1 and 2 of Table 4 show the estimation results for the probability that either all securities or only funds are sold when they are displayed as winners for the full sample period. We see in Column 1 that individuals are subject to the disposition effect, over the whole sample and all securities, and the likelihood of sale is approximately 6 percent higher when the security is trading at a gain relative to the original purchase price. Here, we simply replicate the findings in Koestner et al. (2017). When we only include funds, we obtain a slightly negative disposition effect. However, this coefficient is highly dependent on the sample period: individuals are sometimes more likely to sell a winning fund but sometimes less likely to do so. This result is in line with the findings in Chang et al. (2016), who document a reverse disposition effect for funds. The disposition effect is difficult to rationalize with efficient markets and rational expectations (as argued in Odean, 1998). In principle, the decision to sell should be affected only by expectations about future risk-adjusted returns, and those should be incorporated into stock prices. In contrast, the decision to sell should not be affected by whether the investor made a gain or a loss relative to the initial investment.

More important, Columns 3 and 4 of Table 4 show the estimation results for the probability that funds are sold when they are 1) winners relative to their original 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 the fictitious winners and losers, and they are nearly 4 percent more likely to sell a fictitious winner and nearly 1 percent less likely to sell a fictitious loser.

[Insert Table 4 about here]

In turn, Table 4, Column 5 includes individual fixed effects and month-by-year fixed effects, and Column 6 also adds fund fixed effects. In the latter case, we thus control for all time-invariant effects at the investor and fund level. In Figure 5, we show the fictitious loss and gain coefficients

for five quintiles of the new (displayed) loss (in percentage return terms) and five quintiles of the new (displayed) gain. We see that the effect is monotonically increasing in the new (displayed) gain, but we do not detect a pattern with respect to losses.

[Insert Figure 5 about here]

4.2 Empirical results for consumption

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

[Insert Table 5 about here]

The coefficient ϑ in the consumption regression (2) can thus be interpreted as the MPC out of one dollar of realized capital gains or losses when the security was a fictitious winner.

4.3 Robustness

Main customers, additional controls, and reshuffling experiment

When investors make a trade or a position is liquidated, there is a transfer to the settlement account (Verrechnungskonto). The settlement account is dedicated to making trades and opened automatically when individuals open a portfolio. It pays some interest and is federally insured. In turn, individuals can transfer their funds into their checking or savings accounts. For consumption, we consider the following outcome variables: 1) ATM withdrawals plus point of sale transactions, i.e., individuals swipe their debit or credit card in a store or purchase goods or services online

by entering their card information, and 2) ATM withdrawals plus point of sale transactions plus 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. All the variables are transfers and thus flow variables.

We argue that it is unlikely that individuals have a second brokerage account or additional savings vehicles because banking with multiple banks is discouraged in the German credit score system. Additionally, the self-reported wealth measure we have when individuals opened their accounts is in line with their actual portfolio size. This is also reassuring and indicates that individuals are unlikely to have other deposits of liquid wealth. Furthermore, individuals are incentivized to dedicate one brokerage account as their main account to receive the tax-free allowance on capital gains. Finally, we have a flag variable from the bank itself that indicates customers without any other banking relationships. This flag variable matches well with our own measures of main customer status: a regular salary payment (which is flagged as such in the German transaction system) or a minimum number of transactions per month (as used in Ganong and Noel, 2019; Kuchler and Pagel, 2019; Olafsson and Pagel, 2018a).

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

Furthermore, Table A.16 in Appendix A, we report the results of consumption regressions successively adding fixed effects and other control variables such as a dummy for liquidations, salary, dividend, and interest payments. In Table A.17, we successively add unrealized gains and losses and unrealized fictitious gains and losses as controls. Finally, in Table A.18, we take logs or first-differences or calculate the deviation from the mean of the outcome variable and also divide the sample into early versus late 2018.

[Insert Tables A.16 to A.18 in Appendix A about here]

Finally, in Figure 6, we show the distribution of fictitious gain coefficients for 100 reshuffling experiments, the average reshuffled coefficient and its average standard error, and the true fictitious gain coefficient with its standard error while controlling for individual and month-by-year fixed effects, the available liquidated amounts, and a dummy for liquidations with standard errors clustered at the individual level. As expected, the reshuffled fictitious gain coefficients are not significant.

[Insert Figure 6 about here]

Quality of the consumption data

We observe transaction categories from the transactions system that allow us to pinpoint ATM withdrawals, (international) POS transactions, (repeated, automated, international) wires, interest and dividend payments, (portfolio) fee payments, tax payments, check payments, salary transfers, cash deposits, social security payments as well as security purchases and sales as FX-trading transactions. However, we have no spending categories, such as groceries, and we are limited in the sense that we cannot know for certain whether an individual has other bank accounts or portfolios. 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. Domestic nonrecurring wire transfers that leave the bank (Lastschrift and Überweisungen) are also likely spending in Germany as they are likely to be consumption as they are commonly used to pay for online shopping or other services or to pay people.[7]

Thus, using ATM withdrawals and POS transactions or ATM plus POS plus wires as a measure of spending should give us a relatively accurate picture of consumption. To assess the quality of our spending data, we compare the spending responses to paydays to those that have been documented in the literature using transaction-level spending data that are more thoroughly categorized (Olafsson and Pagel, 2018a; Bräuer et al., 2019; Gelman et al., 2014). When we replicate the analysis in Olafsson and Pagel (2018a) and Gelman et al. (2014), i.e., plotting the daily deviation in spending

[7] Domestic wires are also likely used for recurring transfers such as 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.

around paydays for three income groups of our final sample of customers for the years 2017 and 2018, we obtain similar pictures in terms of magnitudes and tightness of the estimates (as Figure 7 shows). Furthermore, when we examine the daily consumption response from fund sales when individuals liquidate a fictitious or actual capital gain in 2018, we find similar responses to those in Bräuer et al. (2019) as Figure 8 shows.

[Insert Figures 7 to 8 about here]

4.4 Placebo tests

As placebo tests, we can run the exact same regressions for non-funds in 2018, for which there was no fictitious sale. Alternatively, we could consider funds but use the data from January to June 2017 when no fictitious sale occurred. All placebo checks can be found in Tables 6 and 7 as well as Figure 6. In Table 6, Column 1, we examine 2018 but use non-fund securities and their placebo fictitious gains and losses. By contrast, Column 2 uses funds, but considers data until June 2017 and the placebo fictitious gains and losses. In turn, the next columns add individual and month-by-year fixed effects. Overall, we also find significant effects; however, the coefficients are only approximately half the size. In the data of Odean (1998), it is well known that there is also a recency effect: individuals tend to sell securities that performed well recently, which we confirm here.

We can clearly see that the recency placebo effects we document are statistically significantly smaller than the effects of fictitious winners in 2018. Nevertheless, to identify the difference in one specification, we run a regression using data from January to June in 2017 and 2018. In turn, we include fictitious capital gains and losses, either the actual ones or the placebo ones, in one variable. Finally, we include a dummy for 2018 and document the interaction coefficient of this dummy and the (placebo) fictitious capital gains. This is done in Table 8, and we see that the interaction effect is large and statistically significant at approximately 1.3%.

Furthermore, in Table 7, we show the consumption regressions for data up to June 2017 instead of 2018 using the placebo fictitious capital gains and losses, for all individuals or individuals that

the bank flagged as main customers, i.e., clients without any other banking relationships. In these regressions, as in the reshuffling experiment shown in Figure 6, the effects of placebo fictitious capital gains and losses are insignificant and small.

[Insert Tables 6 to 8 about here]

5 Lessons and mechanisms

5.1 Lessons for selective inattention

Table 4 shows that individuals are subject to the disposition effect with respect to the fictitious winners and losers: they are almost 4 percent more likely to sell a fictitious winner and almost 1 percent less likely to sell a fictitious loser. The effect of fictitious losers is attenuated. A potential explanation is selective inattention (as documented in Karlsson et al., 2009; Olafsson and Pagel, 2018b; Gherzi et al., 2014), i.e., individuals are happy to learn that a loser now appears to be a winner but become concerned when a former winner is now displayed as a loser. Furthermore, in the literature using the data in Odean (1998), it is well known that the disposition effect mainly manifests in selling winners.

In Figure 5, we depict the fictitious loss and gain coefficients for five quintiles of the new (displayed) loss (in percentage return terms) and five quintiles of the new (displayed) gain. We can see that the effect is monotonously increasing in the new (displayed) gain, but we do not detect a pattern with respect to losses. Again, this finding is consistent with selective inattention playing a role.

Table 5 shows the estimation results for the amount consumed, and we find a very large consumption response. Here, a potential reason is again inattention: because the absolute and relative 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 after the sale has been processed by the clearing house and the bank has subtracted all fees and taxes). Right next to the sell button, the fictitious capital gain is displayed in both euros and

percentage terms. Below, we will further explore whether less-informed individuals have a larger consumption response.

5.2 Lessons for the disposition effect

We find that individuals consume more after engaging in the disposition effect and selling fictitious winners. This finding helps us to understand the psychological mechanism behind the disposition effect: individuals sell winners because they believe that they are making money and hence consume more. How is our experiment different from simply regressing consumption on realized capital gains? Such a regression would also identify the effect of liquidating and consuming in response to a capital gain (if stock market returns are plausibly exogenous to individuals conditional on time fixed effects). However, in this case, individuals are wealthier and should consume more. In contrast, our experiment provides a situation in which the fictitious capital gain status is not reflected in an individual's wealth because individuals are subject to an actual capital loss. Now, it matters for the interpretation of our results whether individuals know that they experienced a capital loss but are merely shown a fictitious capital gain. As discussed previously, individuals receive a sales receipt that details their actual capital gains and losses, or they can look up their purchase prices in their order summary. Nevertheless, it could still be the case that individuals are confused about what their actual capital gains are.

As another alternative, it could be that individuals liquidate and consume because they believe that doing so is advantageous from a tax perspective. We now perform some sample splits to assess the validity of both mechanisms: confusion about actual capital gains and confusion about tax implications. In short, we find some evidence that individuals are confused about their actual capital gains but no evidence that they are confused about tax implications.

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

As discussed above, after individuals sell a security, they receive a sales receipt that details their 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 realized

capital gains (or losses), and the fees that investors paid. This sales receipt is shown in Figure 9.

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

When we perform sample splits or examine the interaction between early and late 2018 for our first stage (selling fictitious winners), we tend to find statistically significantly stronger effects in early versus late 2018. This result is in line with the hypothesis that investors are more informed in late 2018 than early 2018. The results can be found in Table 9.

[Insert Table 9 about here]

Tables A.12, A.13, and A.14 in Appendix A are intended to provide alternative sample splits and interactions on how informed investors are. We first consider the first five trades and then the first half of trades for each investor (split at the individual level). Then, we also split by passive versus active investors. 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. Nevertheless, we also observe significant effects on selling in late 2018.

[Insert Tables A.12, A.13, and A.14 in Appendix A about here]

In Table A.18, we provide a sample split into early versus late 2018 for the consumption regression specification using deviation from the mean of the outcome variable. Here, the sample split had no influence. However, Table 10 provides an additional set of interactions using the above measures of informedness and absolute consumption as the outcome variable. Here, we consistently find that early trading and infrequent traders have a larger consumption response to fictitious capital gains.

As indicated in Table A.18, when we consider the deviation from the mean as the outcome variable, we find less stark differences, as further explored in Table A.15.

[Insert Tables 10 and A.15 about here]

Nevertheless, these additional pieces of evidence are more consistent with investors being confused about what their actual capital gains are. Clearly, if investors believe that selling a winner is actually making them richer, it makes sense that they consume more. This finding is informative about the disposition effect, as it indicates that the act of selling a winner is actually reflected in consumption because individuals believe that they made money.

5.4 Confusion about tax implications

If individuals are confused about the tax implications of the reform and 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 new gain positions would be more surprising or irrational from a tax perspective than in the classic disposition effect literature (Odean, 1998).

On the other hand, individuals could also believe that they need to secure a tax advantage and hence be more inclined to sell. Thus, as an additional robustness check, we perform a sample split to include only individuals who were not affected by the tax reform. First, we can restrict our attention to individuals with portfolio sizes of less than 50,000 euros and additionally require that the to-date yearly capital gains are less than 1602 euros (this equals a household's tax-free allowance) or 801 euros (which equals an individual's tax-free allowance). In the trading and consumption regressions, we do not find that tax considerations appear to matter, as can be seen in Table 11.

[Insert Table 11 about here]

6 Conclusion

We use a large sample of transaction-level data on all asset holdings, securities trades, spending, and income from clients of a German retail bank. We explore how the individual propensity to sell winners and hold losers as well as individual consumption respond to an exogenous change in the displayed purchase price of funds. This change in the purchase price also affected the funds' displayed returns and capital gains and losses. Our identification strategy exploits the implementation of a capital gains tax reform that facilitated the online bank's assessment of capital gains taxes that the bank has to subtract at the brokerage level. We find that individuals react to fictitious winners (funds that are displayed as winners but are actually losers) and losers (funds that are displayed as losers but are actually winners) in their likelihood of selling. Furthermore, we document that this fictitious disposition effect has real effects on consumption beyond just trading.

As a contribution to the literature on the disposition effect, we provide causal 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. We follow Frydman and Wang (2019), who analyze the salience of new price variables and their colors in online portfolios. Documenting the causal effect of fictitious winners on trading is evidence for an inherent preference for realizing winners that is contrary to other explanations of the disposition effect, such as speculation motives (Ben-David and Hirshleifer, 2012). However, we also show that some investors may be confused about what their actual winners are, following the evidence documented in Birru (2015). Thus, we provide additional evidence against the idea that retail investors follow optimal trading strategies or act in tax-efficient manners. Additionally, we provide evidence that reference points are set by displayed prices, for which, to the best of our knowledge, only experimental evidence exists (Frydman and Rangel, 2014).

However, we believe that the more novel contribution of this paper is the following: we document that the disposition to sell winners has real consumption effects. To the best of our knowledge, no paper links the disposition effect to consumption. Our findings show that individuals are subject to the disposition effect because they feel richer and thus consume more out of the realized capital

gains. We have a unique experiment in which the fictitious capital gains are actually capital losses. However, as we show, individuals may be, to some extent, confused about what their actual capital gains are, and that increases their consumption. Thus, documenting that fictitious capital gains and reference points affect consumption is relevant for the literature on mental accounting and more generally on consumption out of stock market wealth.

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amount purchased

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

Investmentsteuerreform

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

Weitere Informationen

☐ Diesen Hinweis nicht mehr anzeigen

OK >

Figure 1: Screenshots of the bank's portfolio interface and notification of the repricings due to the capital gains tax reform

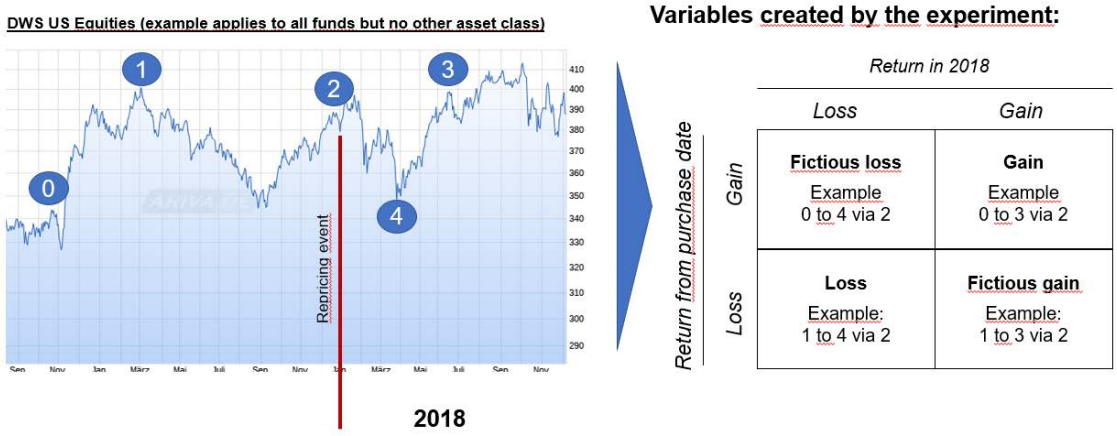

Figure 2: Gain, fictitious gain, loss, and fictitious loss variables created by the experiment: example fund

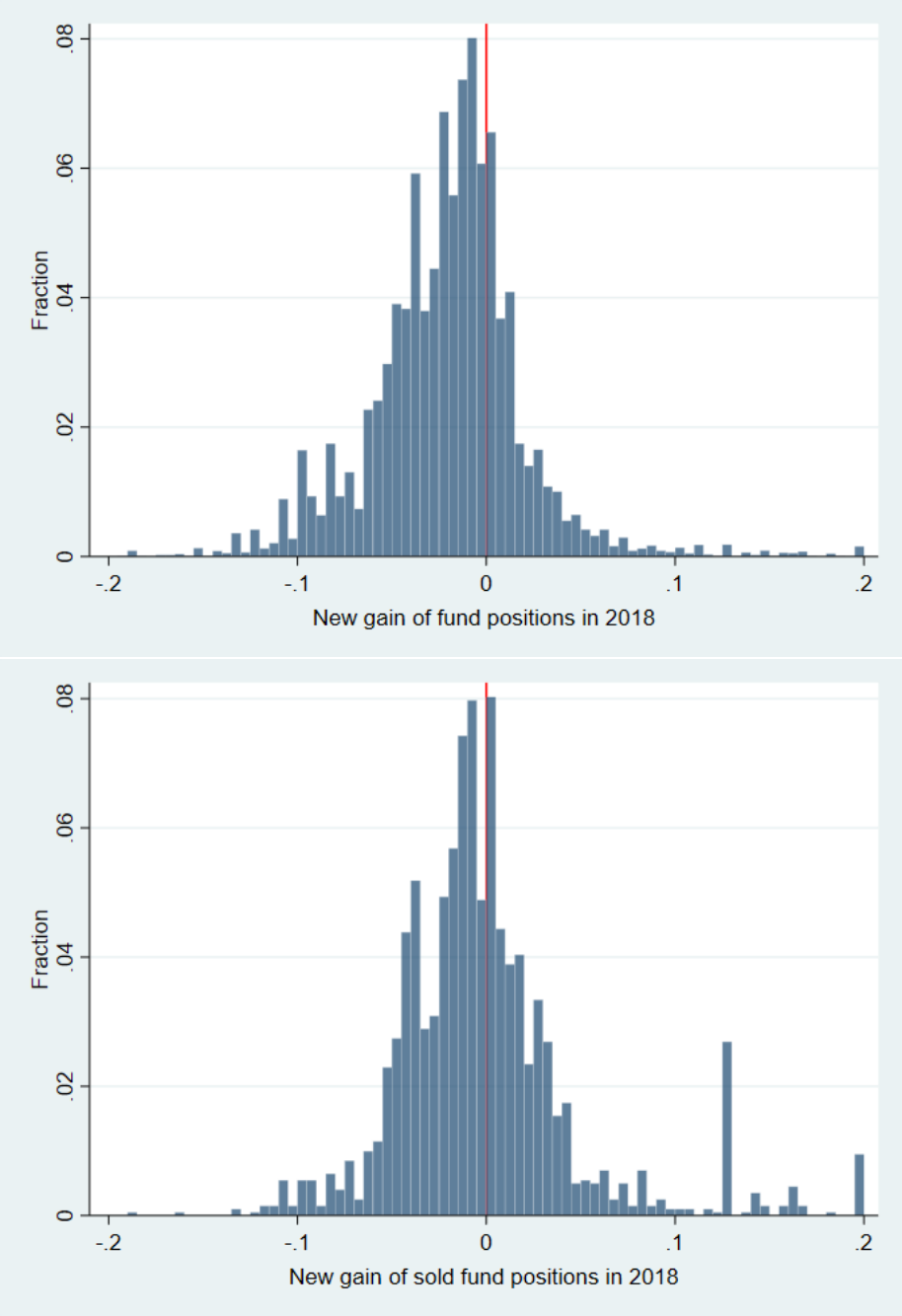

Figure 3: Distribution of the percentage "new" (newly displayed since December 29, 2017, closing price as quoted by the exchange) returns of all (top) and sold (bottom) funds' positions (returns of the sale price or using the June 30, 2018 prices relative to the December 29, 2017, closing price)

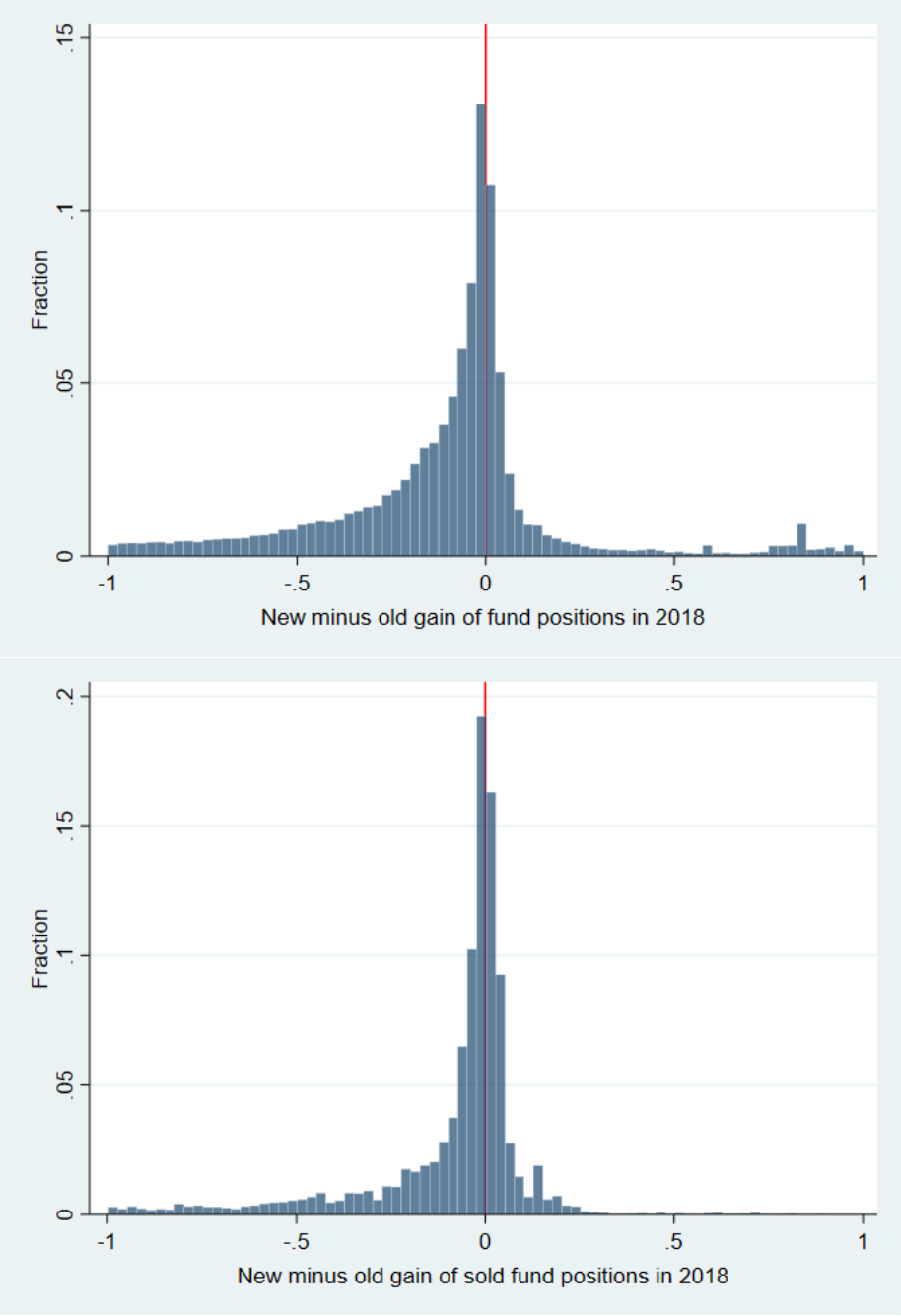

Figure 4: Distribution of the percentage "new" (newly displayed since December 29, 2017, closing price as quoted by the exchange) returns of all (top) and only sold (bottom) funds' positions (return of the sale price or using the June 30, 2018 prices relative to the December 29, 2017, closing price) minus their percentage "old" returns (returns from their actual purchase prices to the December 29, 2017, closing price)

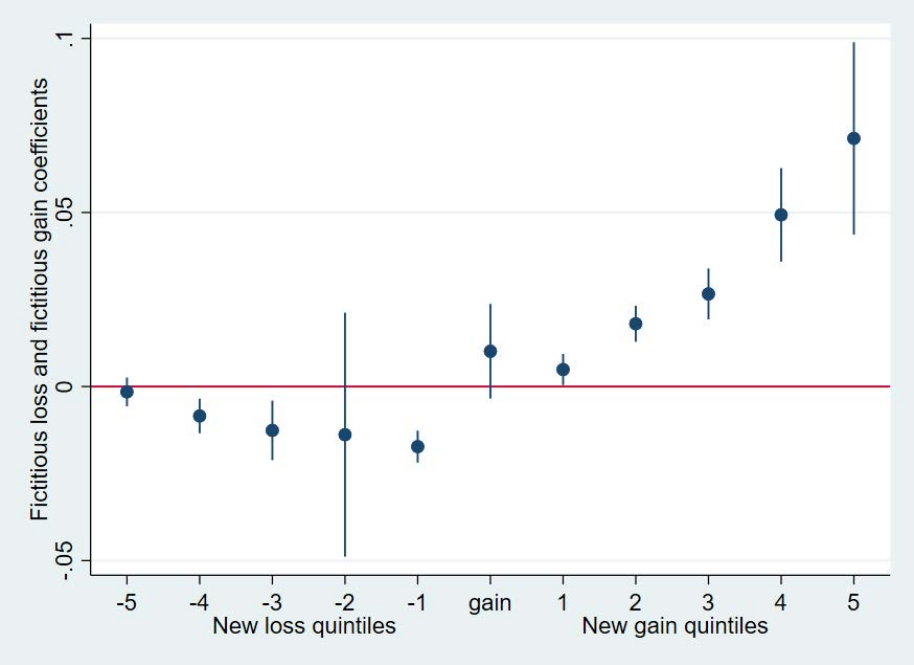

Figure 5: Fictitious loss and gain coefficients for five quintiles of "new" (displayed since December 29, 2018) losses in absolute Euro values and new (displayed since December 29, 2018) gains in absolute Euro values of Specification 1 (regressing a dummy for selling security j of individual i at time t on dummies for security j of individual i at time t being an actual winner, a fictitious winner, or a fictitious loser controlling for individual and month-by-year fixed effects and clustering standard errors at the individual level)

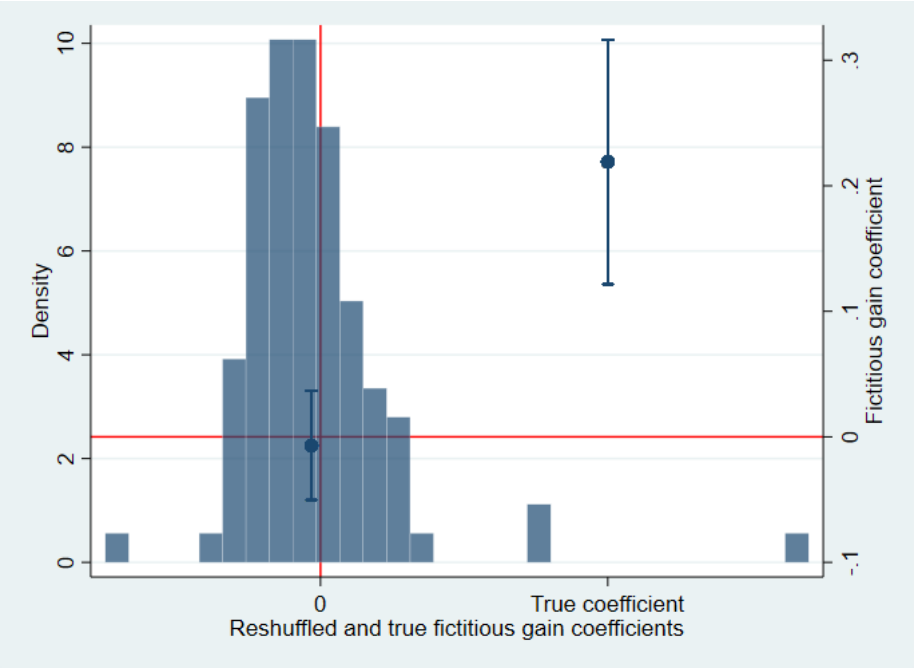

Figure 6: Distribution of fictitious gain coefficients for 100 reshuffling experiments of Specification 2 (regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires) on fictitious capital gains controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, and actual capital gains and losses all aggregated to the monthly level with standard errors clustered at the individual level), the average reshuffled coefficient and its average standard error, and the true fictitious gain coefficient with its standard error

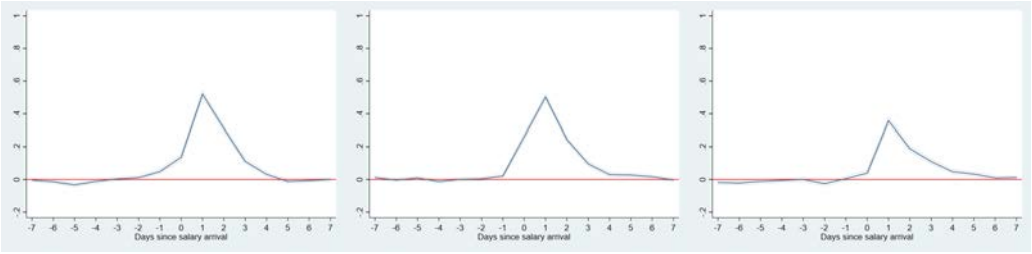

Figure 7: Payday responses of ATM withdrawals plus POS transactions plus non-recurring, national wires (Lastschrift und Überweisungen) in the two weeks around salary receipt for three terciles of income controlling for individual, day-of-week, week-of-month, and month-by-year fixed effects. Standard errors are clustered at the individual level and displayed as dashed lines

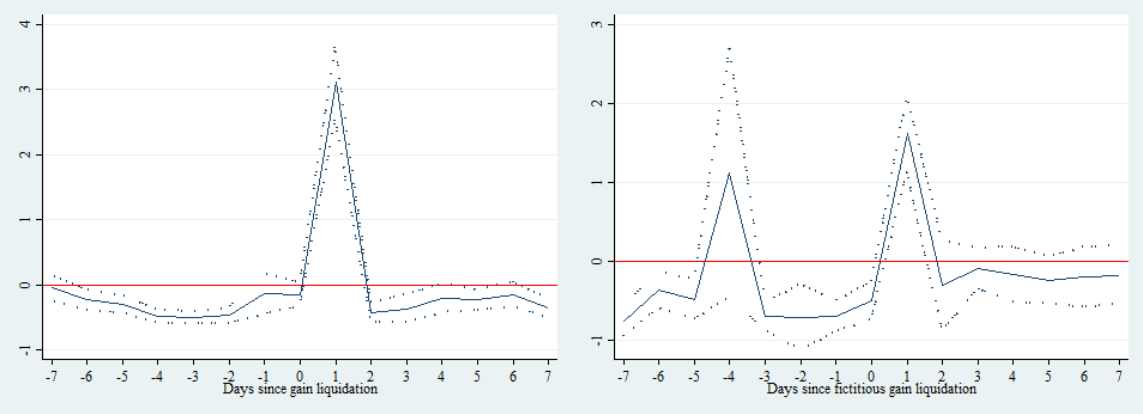

Figure 8: Responses of ATM withdrawals and POS transactions in the two weeks around liquidation of funds that were actual winners (left side) and fictitious winners (right side) controlling for individual, day-of-week, week-of-month, and month-by-year fixed effects. Standard errors are clustered at the individual level and displayed as dashed lines

Wertpapier Abrechnung Verkauf

Nominale	Wertpapierbezeichnung	ISIN	(WKN)
Stück 900	PRAKTIKER AG INHABER-AKTIEN O.N.	DE000A0F6MD5	(A0F6MD)

Börse Frankfurt (gemäß Weisung)
Schlusstag/-Zeit 09.05.2012 18:42:59
Ausführungskurs 1,579 EUR

Girosammelverw. mehrere Sammelurkunden - kein Stückeausdruck -

Kurswert		**1.421,10 EUR**
Provision		9,95- EUR
Transaktionsentgelt Börse		0,71- EUR
Übertragungs-/Liefergebühr		0,14- EUR
Handelsentgelt		3,00- EUR
Ermittlung steuerrelevante Erträge		
Veräußerungsverlust	3.870,91- EUR	
Ausmachender Betrag		**1.407,30 EUR**

Den Gegenwert buchen wir mit Valuta 11.05.2012 zu Gunsten des Kontos 561901550, BLZ 37011000.
Die Wertpapiere entnehmen wir Ihrem Depotkonto.

Sofern keine Umsatzsteuer ausgewiesen ist, handelt es sich um eine umsatzsteuerbefreite Finanzdienstleistung.

Für das Geschäft wurde keine Anlageberatung erbracht.

Figure 9: Sales receipt showing the current value of the liquidation gross of fees (Kurswert), the bank fees (Provision), other exchange fees (Transaktionsentgelt Übertrags-/Liefergebühr Handelsentgelt), the capital loss (VeräuSSerungsverlust), and the final liquidation value net of fees (Ausmachender Betrag).

Table 1: Summary statistics for all investors

	mean	standard deviation	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
male	.84	.37	0	1	1	1	1
age	54	13	40	45	52	61	72
PhD educated	.078	.27	0	0	0	0	0
account tenure (in years)	13	3.3	11	11	11	14	19
risk class	3.5	1.5	1	3	4	5	5
wealth	51,148	93,079	5,000	20,000	45,000	45,000	175,000
income	54,642	24,673	30,000	30,000	50,000	80,000	80,000
number of purchases	145	488	3	12	39	116	322
number of sales	124	434	7	15	36	101	264
risk class of trades	4.4	1.5	3.6	3.9	4.2	4.6	5
portfolio value	55,836	129,607	7,425	16,577	33,586	62,808	111,841
number of securities	46	30	8.6	20	41	68	92
HH index	.14	.15	.0083	.037	.095	.2	.35

Notes: Risk class are the value-weighted risk classes (regulated risk classification) of the investor's holdings or trades. Wealth, income, and risk aversion are self-reported statistics in brackets (surveyed upon account opening). Portfolio size is the average over the sample period and number of purchases and sales are the sum over the entire sample period. Number of securities is the average over the sample period and funds are counted as 100 securities. HH index is the Herfindahl-Hirschman index measure of diversification ranging from 0 to 1 (0 represents perfect diversification and >0.5 represents a handful of stocks).

Table 2: Descriptive statistics for bought and sold or kept securities

		2018	2018	2017	2017
		sale	no sale	sale	no sale
mean purchase price	funds	166.25	168.30	145.46	162.04
	other	83.87	74.21	82.17	70.59
mean sale price	funds	200.57	179.27	181.57	166.90
	other	94.42	75.14	90.53	68.38
size of trade	funds	7152.01	9355.07	7446.56	10617.70
	other	9887.04	7857.63	10046.56	7976.39
price end 2017	funds	200.96	172.56		

Notes: Mean purchase prices for funds and other securities in that are sold in 2017 or 2018. For unsold positions, we use the mean prices from January 1 to June 30, 2018 in the 2018 Column and the mean prices in 2017 in the 2017 Column.

Table 3: Propensity of gains realized versus propensity of losses realized as in Odean (1998)

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

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

	all securities full sample	funds full sample	funds 2018	funds 2018	funds 2018	funds 2018
	sale	sale	sale	sale	sale	sale
gain	0.0607***	-0.00312***	0.00771***	0.00797***	0.00797***	-0.00341
	(0.000808)	(0.000732)	(0.00198)	(0.00197)	(0.00186)	(0.00251)
fictitious gain			0.0409***	0.0426***	0.0426***	0.0506***
			(0.00408)	(0.00407)	(0.00383)	(0.00456)
fictitious loss			-0.00824***	-0.00463**	-0.00463**	-1.49e-06
			(0.00211)	(0.00198)	(0.00186)	(0.00217)
individual fixed effects			✓		✓	✓
month-by-year fixed effects				✓	✓	✓
funds fixed effects						✓
observations	52,619,347	7,317,370	147,762	147,762	123,836	123,383
R squared	0.019	0.003	0.261	0.092	0.234	0.278

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 fictitios loser (as indicated) controlling for individual, month-by-year, or funds fixed effects (as indicated)

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

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

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires (as indicated)) on fictitious capital gains controlling for individual and month-by-year fixed effects as well as the available liquidated amounts and actual capital gains and losses all aggregated to the monthly level

Table 6: Placebo estimation results of probability of sale on dummies for the security (non-funds in 2017 or 2018 or funds in 2017) of being a winner, a fictitious winner, or a fictitious loser

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

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: 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 (placebo as indicated) fictitious winner, or a (placebo as indicated) fictitios loser controlling for individual, month-by-year, or funds fixed effects (as indicated)

Table 7: Placebo estimation results of different measures of consumption on liquidiation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses in 2017

	all securities 2017 and 2018	main customers all securities 2017 and 2018	all securities 2017 and 2018	main customers all securities 2017 and 2018
	ATM + POS in ₫	ATM + POS in ₫	ATM + POS + wires in ₫	ATM + POS + wires in ₫
fictitious gain in ₫	0.00854	0.00731	0.0166	0.0163
	(0.00720)	(0.00795)	(0.0145)	(0.0174)
fictitious loss in ₫	-0.000599	-0.00443**	-0.000988**	-0.00304
	(0.000657)	(0.00225)	(0.000500)	(0.00327)
gain in ₫	✓	✓	✓	✓
loss in ₫	✓	✓	✓	✓
liquidation minus reinvestment in ₫	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓
observations	546290	34259	546290	34259
R squared	0.312	0.532	0.312	0.373

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires (as indicated)) on placebo fictitious capital gains (sale price relative to the December 30, 2016 price) controlling for individual and month-by-year fixed effects as well as the available liquidated amounts and actual capital gains and losses all aggregated to the monthly level

Table 8: Estimation results of probability of sale on dummies for the security (2017 and 2018 January to June) of being a winner, a (placebo) fictitious winner, or a (placebo) fictitious loser interacted with a dummy for a fund sale in 2018

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

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: 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 fictitios loser (either the 2018 status or the 2017 placebo status) as well as an interaction for the year being 2018 controlling for individual, month-by-year, or funds fixed effects (as indicated)

Table 9: Estimation results of probability of sale on dummies for the security of being a winner, a fictitious winner, or a fictitious loser for early and late 2018 as well as the interaction with an early/late indicator

	funds early 2018	funds late 2018	funds 2018	funds 2018	funds 2018
	sale	sale	sale	sale	sale
gain	-0.0160***	0.00132	0.00773***	0.0128***	-0.00330
	(0.00463)	(0.00416)	(0.00198)	(0.00216)	(0.00290)
fictitious gain	0.0499***	0.0429***	0.0299***	0.00867**	0.0159***
	(0.00618)	(0.00679)	(0.00508)	(0.00351)	(0.00354)
fictitious loss	-0.00770**	0.00525	-0.00830***	0.00803***	-0.00342
	(0.00359)	(0.00354)	(0.00211)	(0.00186)	(0.00251)
fictitious gain				0.0366***	0.0381***
times late 2018				(0.00445)	(0.00505)
fictitious gain			0.0196***	0.0647***	0.0760***
times early 2018			(0.00590)	(0.00545)	(0.00577)
month-by-year fixed effects	✓	✓		✓	✓
individual fixed effects	✓	✓		✓	✓
funds fixed effects	✓	✓			✓
observations	65,841	55,036	126,249	123,836	123,383
R squared	0.329	0.346	0.271	0.235	0.278

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 fictitios loser in either early or late 2018 (before versus after March 31, 2018) as well as an interaction for the day being earlier than April 1, 2018 controlling for individual, month-by-year, or funds fixed effects (as indicated)

Table 10: Estimation results of consumption (ATM plus POS) on liquidiation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses with an early 2018 indicator, with each investor's first five and first half trades indicator as well as an interaction with an indicator for the frequently trading investors

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

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires) on fictitious capital gains with different interaction variables indicating an early trade (before April 1, 2018), one of the first five trades of each individual, one of the first half of trades of each individual, and infrequent traders (as indicated) controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, and actual capital gains and losses all aggregated to the monthly level

Table 11: Estimation results of probability of sale on dummies for the security of being a winner, a fictitious winner, or a fictitious loser for individuals with portfolio sizes of less than 50,000€ and year-to-date capital gains of less than 1,602€

	funds	funds	funds	funds	funds capital gains $<$ 801€
	sale	sale	sale	sale	sale
gain	0.0157***	0.0132***	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) 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 fictitios loser for subsets of individuals with portfolio sizes of less than 50,000€ and the year-to-date capital gains less than 1,602€ (household's tax free allowance) or 801€ (individual's tax free allowance) (as indicated) controlling for individual, month-by-year, or funds fixed effects (as indicated)

A Additional tables for online publication

Table A.12: Estimation results of probability of sale on dummies for the security of being a winner, a fictitious winner, or a fictitious loser for each investor's first five trades in 2018 versus more trades as well as the interaction with an indicator for each investor's first five trades in 2018

	funds first five trades	funds more than five trades	funds all 2018	funds all 2018	funds all 2018
	sale	sale	sale	sale	sale
gain	-0.022*	0.000	0.017***	0.015***	0.003***
	(0.011)	(0.003)	(0.001)	(0.001)	(0.001)
fictitious gain	0.108***	0.046***	0.013***	0.017***	0.018***
	(0.019)	(0.005)	(0.001)	(0.001)	(0.001)
fictitious loss	0.010	-0.001	0.002**	-0.000	-0.002***
	(0.010)	(0.002)	(0.001)	(0.001)	(0.001)
fictitious gain times first five trades			0.062***	0.017***	0.015***
			(0.003)	(0.003)	(0.003)
month-by-year fixed effects	✓	✓		✓	✓
individual fixed effects	✓	✓		✓	✓
funds fixed effects	✓	✓			✓
observations	12705	105045	1304646	1303133	1281546
R squared	0.232	0.159	0.002	0.069	0.106

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 fictitios loser for the subset of each individual's first five trades or later trades as well as an interaction for the trade being one of the first five trades controlling for individual, month-by-year, or funds fixed effects (as indicated)

Table A.13: Estimation results of probability of sale on dummies for the security of being a winner, a fictitious winner, or a fictitious loser for each investor's first half trades versus second half in 2018 as well as the interaction with an indicator for the first half of trades

	funds first half of trades	funds second half of trades	funds all 2018	funds all 2018	funds all 2018
	sale	sale	sale	sale	sale
gain	-0.003	-0.001	0.017***	0.015***	0.003***
	(0.004)	(0.003)	(0.001)	(0.001)	(0.001)
fictitious gain	0.061***	0.037***	0.018***	0.019***	0.021***
	(0.007)	(0.006)	(0.001)	(0.001)	(0.001)
fictitious loss	0.011***	-0.001	0.002**	-0.000	-0.002***
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)
fictitious gain times first half			0.005***	-0.002	-0.003**
			(0.001)	(0.001)	(0.001)
month-by-year fixed effects	✓	✓		✓	✓
individual fixed effects	✓	✓		✓	✓
funds fixed effects	✓	✓			✓
observations	58254	59290	1304646	1303133	1281546
R squared	0.192	0.218	0.001	0.069	0.106

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 fictitios loser for the subset of each individual's first half of trades within 2018 as well as an interaction for the trade being one of the first half of trades controlling for individual, month-by-year, or funds fixed effects (as indicated)

Table A.14: Estimation results of probability of sale on dummies for the security of being a winner, a fictitious winner, or a fictitious loser for active and passive investors as well as the interaction with an active investor indicator

	funds passive accounts	funds active accounts	funds all 2018	funds all 2018	funds all 2018
	sale	sale	sale	sale	sale
gain	0.005	0.010**	0.023***	0.019***	0.010***
	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
fictitious gain	0.056***	0.011	-0.001	0.010***	0.013***
	(0.010)	(0.010)	(0.002)	(0.002)	(0.002)
fictitious loss	-0.009*	-0.007	0.006***	0.001	-0.001
	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
fictitious gain times passive account			0.038***	0.011***	0.007***
			(0.002)	(0.002)	(0.002)
month-by-year fixed effects	✓	✓		✓	✓
individual fixed effects	✓	✓		✓	✓
funds fixed effects	✓	✓			✓
observations	36585	20193	623569	622484	602213
R squared	0.215	0.075	0.003	0.076	0.116

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 fictitios loser for the subset of infrequent versus frequent traders within 2018 as well as an interaction for the trade being made by an infrequent trader controlling for individual, month-by-year, or funds fixed effects (as indicated)

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

	funds 2018	funds 2018	funds 2018	funds 2018
	ATM + POS + wires dev from mean	ATM + POS + wires dev from mean	ATM + POS + wires dev from mean	ATM + POS + wires dev from mean
fictitious gain	0.164***			
dev from mean	(0.031)			
fictitious gain	0.148***			
early 2018 indicator	(0.026)			
fictitious gain		-0.001		
dev from mean		(0.025)		
fictitious gain		0.163***		
first five trades		(0.021)		
fictitious gain			0.185***	
dev from mean			(0.027)	
fictitious gain			0.117***	
first half of trades			(0.029)	
fictitious gain				0.047**
dev from mean				(0.023)
fictitious gain				0.218***
infrequent trader				(0.028)
gain dev from mean	✓	✓	✓	✓
loss dev from mean	✓	✓	✓	✓
fictitious loss dev from mean	✓	✓	✓	✓
liquidation minus reinvestment dev from mean	✓	✓	✓	✓
liquidation dummy	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓
observations	216513	216513	216513	216513
R squared	0.125	0.125	0.125	0.126

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires) on fictitious capital gains with different interaction variables indicating an early trade (before April 1, 2018), one of the first five trades of each individual, one of the first half of trades of each individual, and infrequent traders (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 and all transformed into deviations from mean values (except the liquidation dummy)

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

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

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus 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

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

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

Standard errors (clustered at the individual level) in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: Specification 2 regressing consumption (ATM withdrawals plus POS transactions plus non-recurring national wires) on fictitious capital gains controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, actual capital gains and losses, and unrealized fictitious as well as actual capital gains and losses (as indicated) all aggregated to the monthly level

Table A.18: Estimation results of consumption (ATM plus POS plus wires) on liquidiation amounts less reinvestment of all securities, capital gains, fictitious capital gains, and fictitious capital losses

	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2017 and 2018	all securities 2018	all securities 2018 early	all securities 2018 late
	ATM + POS + wires logged if > 1	ATM + POS + wires first-differenced	ATM + POS + wires deviation from mean	ATM + POS + wires deviation from mean	ATM + POS + wires deviation from mean	ATM + POS + wires deviation from mean
fictitious gain (logged, first-differenced, deviation from mean)	0.348***	0.640**	0.151***	0.151***	0.144***	0.171***
	(0.0343)	(0.275)	(0.0179)	(0.0198)	(0.0312)	(0.0352)
fictitious loss (logged, first-differenced, deviation from mean)	-0.125***	-0.00102**	-5.11e-06	8.97e-06	0.000334	7.60e-06
	(0.00419)	(0.000461)	(1.07e-05)	(7.78e-06)	(0.00118)	(4.90e-06)
gain (logged, first-differenced, deviation from mean)	✓	✓	✓	✓	✓	✓
loss (logged, first-differenced, deviation from mean)	✓	✓	✓	✓	✓	✓
liquidation minus reinvestment (logged, first-differenced, deviation from mean)	✓	✓	✓	✓	✓	✓
month-by-year fixed effects	✓	✓	✓	✓	✓	✓
individual fixed effects	✓	✓	✓	✓	✓	✓
liquidation dummy	✓	✓	✓	✓	✓	✓
observations	609,693	455,009	609,693	216,513	99,604	116,909
R squared	0.455	0.021	0.261	0.349	0.506	0.456

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 (outcome variable transformed as indicated)) on fictitious capital gains controlling for individual and month-by-year fixed effects as well as the available liquidated amounts, a dummy for liquidations, and actual capital gains and losses all aggregated to the monthly level