Fully Closed: Individual Responses to Realized Capital Gains and Losses*

Steffen Meyer† and Michaela Pagel‡

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Abstract

We use transaction-level data of portfolio trades and holdings linked to checking, savings, and settlement account transactions and balances to explore how individuals respond to realized capital gains and losses. To identify the effects of realized gains and losses, we exploit plausibly exogenous mutual fund liquidations. Specifically, we estimate the marginal propensity to reinvest one dollar received from a forced sale event, when the investor either achieved a capital gain or a loss relative to his or her initial investment. Theoretically, if individuals held optimized portfolios, the marginal propensity to reinvest out of forced liquidations should be 100% independent of realizing a gain or a loss. Individuals should just reinvest all of their liquidity immediately into a fund with similar characteristics. Empirically, individuals keep a share of their newly found liquidity in cash, save it, consume it, or reinvest it into different funds, stocks, or bonds. Moreover, individuals reinvest 80% if the forced sale resulted in a capital gain, but only 40% in the event of a loss. Such differential treatment of gains and losses is inconsistent with active rebalancing or tax considerations, but consistent with mental accounting and the idea that individuals treat realized losses differently from paper losses providing evidence for realization utility and effects (Barberis and Xiong, 2012; Imas, 2016).

Keywords: reinvestment of capital gains and losses, realization effect, risk-taking after losses

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†Leibniz University of Hanover; meyer@gif.uni-hannover.de

‡Columbia Business School, NBER, and CEPR; mpagel@columbia.edu

1 Introduction

Fluctuations in stock prices should affect households' savings and consumption decisions; after all, stock and mutual fund holdings represent a significant fraction of household financial wealth – comparable to the stock of housing wealth. At the same time, stocks and funds are very liquid, much more so than housing wealth, and can be easily monetized any time when individuals adjust consumption or other liquidity needs arise. Moreover, fluctuations in stock prices may be a source of emotional stress for investors making them reluctant to liquidate for consumption, rebalance optimally, or even invest in the first place. The theoretical literature makes clear predictions about how individuals should respond to changes in the value of their stockholdings (independent of whether or not those are liquidated, if transaction costs and tax considerations are negligible). Moreover, there exists a number of experimental studies analyzing how subjects respond to gains and losses. However, empirical evidence using observational data about how individuals respond to realized capital gains and losses remains scarce.

Clearly, estimating the marginal propensity to reinvest or consume out of stock price changes or liquidations is difficult. Aggregate fluctuations in stock prices are endogenous with respect to other macroeconomic shocks, such as income growth and consumer confidence. Therefore, the relationship between aggregate consumption and stock price fluctuations will be overestimated due to common shocks. Common shocks are arguably less problematic when utilizing individual-level data and computing abnormal returns. This way, one could sensibly estimate the marginal propensity to consume out of unrealized capital gains or irregular dividends. However, if one were to look at realized capital gains, there are clear-cut endogeneity problems present. When individuals decide to liquidate stockholdings, they did so because they decided to either rebalance or consume.

To investigate the effect of capital gains and losses on individual investor reinvestment, savings, and consumption, we use a unique panel dataset on the daily trading of 105,000 retail investors in Germany spanning more than ten years. We precisely measure each individual's daily activity by his or her log in and trading behavior as well as their balances and transactions in checking, savings, and settlement accounts. To estimate the causal effect of realized capital gains and losses, we utilize

mutual fund liquidations. Mutual fund liquidations are arguably independent of individual retail investor characteristics and thus constitute an exogenous source of forced liquidations. For the period from 2006 to 2016, we obtain and use the International Securities Identification Number (ISIN) and dates of 3,306 mutual fund closures.

We find that individuals on average reinvest approximately 80% of their funds within a few days and up to a month after the forced sale event. Furthermore, the reinvestment share is approximately 80% for capital gains but only 40% for capital losses. These findings are not consistent with the idea that individuals hold optimized portfolios, actively rebalance, or optimize their taxes. If individuals held optimized portfolios in the first place, they would reinvest 100% of their newly found liquidity in a fund with similar characteristics. If individuals held suboptimal portfolios in the first place because of transaction costs or tax considerations, they may take the forced sale event as an opportunity to rebalance and not reinvest 100% of their funds. However, in that case, they should always reinvest a loss at a higher rate than a gain. After all, losses should not be affected by tax considerations and, moreover, losses should not cause a rebalancing away from the initial amount of stockholdings. Additionally, we find that individuals who sell voluntarily treat gains and losses somewhat less differently, suggesting that the experience of being forced out instead of deciding to liquidate matters for investors.

We estimate the effects of forced liquidations on reinvestment, savings, and consumption using either a cross-sectional or time-series approach. The cross-sectional approach simply treats the mutual fund liquidations as exogenous shocks and estimates the average response to it. To complement the identification approach with controls for each liquidation, i.e., ISIN fixed effects, we compare the investors that are forced out with their own voluntary liquidations as well as other investors trading the same fund voluntarily. Such a time-series approach thus looks at individuals before and after the forced liquidations, effectively employing an Event study or a Regression Discontinuity in Time (RDiT) design. The deployment of RDiT faces an number of challenges primarily due to its reliance on time-series variation for identification, which is different from the canonical cross-sectional identification of standard regression discontinuity (RD) designs. We argue that our setting is addressing all these challenges because we use high-frequency, high-accuracy, transaction-level

data for different short bandwidths of time around a number of events alleviating concerns due to time-series trends and time-varying confounds.

Our results are robust to different econometric specifications and controls at the time, investor, and fund levels. It has to be kept in mind though that we estimate a local average treatment effect (LATE) of a randomly selected sample of German clients of an online bank who hold a portfolio, trade at least once per year, and happen to be invested into a mutual fund that closes out on them. We carefully address three potential concerns about this LATE effect. 1) The announcement of the fund closure may affect individual decisions to sell prior to the fund closure. While we outline several reasons to think that most individuals miss the closure announcement, we address this concern by controlling for individual's holding period fund returns to account for the factor that may affect the decision to sell, as well as instrumenting the amount of the liquidation and whether or not it is a loss by the amount invested at time of the announcement and whether or not the investment represented a loss at announcement. 2) The decision to reinvest may be affected by omitted variables that also affect the return of the fund investment or overall portfolio, e.g., economic sentiment. We address this concern by controlling for the market or individual portfolio returns over the fund investment as well as three and twelve months prior to the closing of the fund. 3) Individuals that choose to hold on to a losing investment may be different from individuals choosing to hold a winning investment. While we do not think that individuals have the skill to predict whether or not a fund will be losing or winning, we may estimate different LATE effects for individuals holding a losing versus a winning fund. Clearly, we have limited ability to address this concern but we can control for a dummy of holding a losing fund to account for all time-invariant observable or unobservable characteristics of holding a winner versus a loser.

We provide empirical evidence for the theoretical framework developed in Barberis and Xiong (2012) who explain the disposition effect via a utility function in which individuals narrowly frame utility over individual stock's sales or realizations. Because individuals dislike realizing losses more so than they like realizing gains, the utility specification explains the disposition effect. Our empirical finding of a reverse disposition effect after forced realizations (Chang et al., 2016) is thus fully consistent with the modeling assumptions put forward in Barberis and Xiong (2012). Furthermore,

that our investors are subject to the disposition effect (Odean, 1998), and thus treat unrealized capital gains and losses very differently, has been documented by Koestner et al. (2017). Our findings are also consistent with the empirical observation in Frydman et al. (2017) that individuals do not display disposition effects because they may not close mental accounts when they reinvest again quickly. After all, for our mutual fund liquidiations, no equivalent reinvestment exists.

Imas (2016) develops a theoretical framework of dynamic cumulative prospect theory with mental accounting (Shefrin and Thaler, 1988). After a paper loss, the mental account of prior outcomes remains open and the loss is evaluated jointly with the outcome, causing the individual to take on more risk to recover from it. A realized loss closes the associated mental account and resets the reference point. Closing the mental account in the red causes the individual to be sensitized to the prospect of further losses, leading him or her to take on less risk. In contrast, after a realized gain, the investor is not sensitized, resulting in the prediction that realized gains should result in more reinvestment than realized losses. Imas (2016) presents evidence for this framework in a series of lab experiments. Our results are unique in providing clean evidence from consequential investment decisions in the field, and can thus be seen as additional empirical support for the realization effect.

By showing that individuals do not rebalance in due course after forced sales, we conclude that they do not appear to hold optimized portfolios providing evidence for investor inattention and inertia following Bilias et al. (2010), Alvarez et al. (2012), Bonaparte and Cooper (2011), Calvet et al. (2009a,b), Karlsson et al. (2009), Brunnermeier and Nagel (2008), Agnew et al. (2003), Dahlquist and Martinez (2013), and Mitchell et al. (2006). As shown by Chien et al. (2012), Reis (2006), and Gabaix and Laibson (2002), such inattention matters in the aggregate.

By showing that individual's propensity to reinvest appears to be affected by losses, we provide new empirical evidence from observational data relating to a large literature on how prior losses affect subsequent risk-taking. 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), 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., 2014). This research has produced contradictory results: some studies find that individuals

become more risk-seeking following losses (Andrade and Iyer, 2009; Langer and Weber, 2008), while other studies find the opposite, that they become more risk-averse (Shiv et al., 2005; Liu et al., 2010). Imas (2016) reconciles this evidence by arguing that individuals become more risk averse only after realized losses but not after paper losses (the realization effect).

Finally, our findings are related to the literature on experiential learning and how personal experiences shape preferences, such as Malmendier and Nagel (2011) and Andersen et al. (2014). The comparison between voluntary sales and forced sales appear to be consistent with individuals learning about their own ability from a bad experience, such as being forced out of an investment at a loss. Furthermore, they appear to become more risk averse in response as documented in Koudijs and Voth (2016). To understand individual preferences for investing into stocks and funds is of importance for long-standing puzzles in household finance such as the stock market-non-participation puzzle.

2 Data and Summary Statistics

Our data set stems from one of the largest online banks in Germany. The data contains daily information on logins (from 2012 onwards), trades, and portfolio holdings of approximately 105,000 customers as well as all balances and transactions of each investor's accounts at the online bank from 2003 to 2016. We keep only private investors that reside in Germany. Moreover, in online banks, silent attribution is a common phenomenon, as usually there is no charge for having an account. Therefore, in order to not analyze accounts of investors who stopped trading, we require that individuals execute at least 1 trade per year. Beyond all self-directed trades and holdings, we obtain data on customer demographics such as gender, age, and occupation as well as detailed information on traded securities such as asset class, risk class, issuer, and issue date of a security from Datastream. 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.

Our sample is not representative for the German population as a whole; less than half of Germans

are invested into 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 a roughly 10 percent sample of all customers. The bank did not pick the sample of retail investors by trading frequency but rather chose a random subsample of all bank users who held a brokerage account. In that sense, our sample is representative for individuals in Germany holding an investment portfolio at a major bank. The average age of investors is 53 and the median age is 52. 16.9 percent of our sample is female and 83.1 percent is male. Brokerage clients are generally expected (Cole et al., 2012) and found to be more sophisticated than the overall population (Dorn and Huberman, 2005). The same is true for our sample: 6 percent of our investors hold a doctoral degree, which is higher than average in the German population (1.1%, German Federal Bureau of Statistics, 2008).

Investors own portfolios that are worth 55,854€, on average. These descriptive statistics are comparable to those reported by household finance studies using US-data (Barber and Odean, 2000). In addition, we compare average portfolio values to official statistics in Germany. The Deutsche Bundesbank (2013) reports the average portfolio value of a German stock market investors to be around 48,000€. This value seems comparable to the average values we observe in our sample. 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 about 1.1 (German Federal Bureau of Statistics, 2008; Bundesbank, 2013).

We observe 3,606 fund closures roughly evenly distributed between 2006 and 2016 as can be seen in Figure 1. The information on fund closures was obtained from the Bundesverband Investment und Asset Management e. V. (BVI). The BVI is the point of contact for politicians and supervisory authorities on all issues related to the German Capital Investment Code (Kapitalanlagegesetzbuch, KAGB), and represents the interests of the German fund industry at the national and international level. Moreover, in Figure 2, we display the day of month and the day of week of all fund closures.

[Insert Figure 1 and 2 about here]

Table 1 shows detailed summary statistics for our forced sales events including the holding periods before closure, the purchase and selling share prices, and the average value and return of the forced sales.

[Insert Table 1 about here]

The SPIVA US Scorecard 2017 documents that over a 15-year period, 58% (48%) of equity (fixed income) funds were merged or liquidated and states that the main reason is continued poor performance. Brown and Goetzmann (1995), the forerunners of mutual fund termination studies, found that US mutual fund disappearance is a function of lagged relative returns, relative fund size, fund expenses, and fund age. Bu and Lacey (2009) argue that the importance of returns depends on the age and style of the fund and show that beyond returns also expenses, turnover, the S&P 500, and the short-term interest rate matter for mutual fund closures. Evans (2006) shows that total returns are more important than risk-adjusted returns in explaining mutual fund termination. In any case, we feel that is is unlikely investors can choose to invest into to-be-closed funds endogenously and thus consider liquidations as plausibly exogenous. Furthermore, mutual fund are fairly diversified and thus mostly determined by market conditions and there is no clear evidence for manager skill.

Table 2 shows detailed summary statistics of assets under management for all funds that did not close or merge, funds that were closed, and funds that were merged. The last row called total assets refer to the last value of total net assets right before closure or merger of the closed and merged funds or the total assets at the last observation for the non-closed or merged funds. Furthermore, Table 3 shows the annualized returns of all, closed, or merged funds from 500 to 125 days prior to the closing date relative to other funds in the same style category. It can be seen that the closed funds did not necessarily perform much worse than the merged or the remaining universe of funds. In fact, in the raw return numbers there does not appear to be a clear pattern in terms of the decision to keep a fund alive or not, however, none of the differences are statistically significant. Nevertheless, the size of the fund appears a more important factor for the sample of our funds than

their performance of the funds under consideration here and for which we have all data available from Datastream.

[Insert Tables 2 and 3 about here]

Of those 3,606 fund closures, we observe 2,228 forced sales, i.e., individuals affected by the mutual fund closures. Most forced sales happen in 2007 but we also observe many in 2008, 2013, and 2015, as can be seen in Figure 3.

[Insert Figure 3 about here]

Table 4 shows detailed summary statistics for our universe of investors relative to those affected by the fund closures, i.e., holding funds that were closed, and relative to those affected by the fund closures and ultimately forced to sell. It can be seen that the three samples of investors look very similar in terms of demographics and income as well as trading behavior and portfolio characteristics. Note that, for the average number of securities held by investors, we assume that all funds hold 100 securities.

[Insert Table 4 about here]

Figure 4 shows the average amounts (in €) of all fund liquidations per year. We can see that the average amounts are quite substantial ranging from 6,000€ to 10,000€. Clearly, the fund liquidation does not represent a wealth shock, but they are quite substantial liquidation shocks.

[Insert Table 4 about here]

3 Methodology

Specifications

We consider two approaches, one "conditional cross-sectional" regression and one "unconditional panel" regression. The conditional cross-sectional regression is specified as follows:

$$\Delta Y^i_{j,j+\tau} = \alpha + \beta F^i_j + \gamma w m y_t + \theta J_j + \epsilon^i_j$$

where $\Delta Y^i_{j,j+\tau}$ is the sum of the outcome variable of interest for investor i at the time of the forced sale event j to $j+\tau$, F^i_j is the currency amount of the forced sale affecting investor i at time j, myw_t are time controls, week-of-month, month, and year fixed effects, and J_j are additional fund or investor controls. We consider two bandwidths τ: five or thirty days since the day that the money arrives in individual's accounts. We adjust standard errors for heteroskedasticity. Because the forced sale is exogenous to individual investors, other control variables are not necessary but may increase precision.

The unconditional panel regression is specified as follows:

$$\Delta Y^i_{j,t,t+\tau} = \eta_j + \beta F^i_{j,t} + \psi S^i_{j,t} + \gamma wmy_t + \epsilon^i_t$$

where $\Delta Y^i_{j,t,t+\tau}$ and myw_t are specified as above for any fund j and time t. Furthermore, $F^i_{j,t}$ is the currency amount of the forced sale of fund j affecting investor i at time t and $S^i_{j,t}$ is the currency amount of any other sale of fund j by investor i at time t. We only consider affected funds and η_j is a fund fixed effect. Alternatively, we can cluster standard errors at the fund (ISIN) level, which represents the treatment level. However, we do not think that standard errors are uncorrelated across ISINs and thus consider robust standard errors more appropriate.

Alternatively to specifying the regression at the affected fund level, we can look at all the affected individuals who trade in and out of all funds over the sample period. In this specification, we identify variation within individual rather than within funds and can control for individual fixed effects.

Outcome variables

When investors make a trade or a position gets liquidated, then there occurs a transfer to the settlement account (Verrechnungskonto). The settlement account is an account dedicated for making trades and automatically opened when individuals open a portfolio. It pays some interest and is federally insured. We thus consider the following outcome variables: 1) the currency amount that the settlement balance is increased, i.e., the money staying in the settlement account; 2) transfers

to the portfolio via purchases or sales of securities, i.e., reinvestment, 3) transfers to the savings account within in the bank, i.e., savings, 4) all transfers out of the settlement account that do not stay within the bank and to the checking account within the bank, i.e., all other outflows out of the settlement account, and 5) the increases or decreases in the currency amount of all accounts, i.e., all net inflows or outflows out of the bank. All the variables are transfers or balance increases or decreases and thus flow variables.

Identification

The econometric application has the following features: (1) there is cross-sectional variation in the experimental implementation, i.e., some individuals are affected to varying degrees and others are not; (2) the relevant variables are available at a high frequency over a long period before and after each experiment; and (3) there exist potential time-varying confounders, but they must be assumed to change smoothly across the date of the experiment. We address the latter two concerns, by using transaction-level data that is measurement error free with homogeneous time bands around each event. Moreover, we are interested in relatively short-run effects, from the day of the announcement to approximately one month after, which makes potential time-series confounds less worrisome. Finally, because we use many events rather than just one, we are not concerned about other time-varying confounds.

Theoretically, our framework can be interpreted as good as a local randomized experiment solving selection concerns by randomly assigning subjects into control and treatment status in the same way as a canonical RD framework. The running variable is time itself, which, cannot be randomly assigned. However, we can safely argue that the forced sale date is randomly assigned to our investors as it is not chosen with individual-level investor characteristics in mind. Thus, whether a given investor at a given date is affected by a forced sale event can be thought of as good as random. Nevertheless, covariates that are discontinuous in time, such as year, month, and week-of-month effects can be included as controls and to improve precision (Lee and Lemieuxa, 2010). Similarly, to a cross-sectional RD framework, there is potential for bias when one chooses a time frame further away from the threshold. However, we only look at a short time window around

the experiment date. Furthermore, given the frequency and size of our sample, we do not have to increase the time window to increase statistical power.

As discussed in Hausman and Rapson (2017), estimates may be biased if the time-series properties of the data are ignored, for instance in the presence of autoregressive processes. Whenever a potential liquidation of funds itself would cause further liquidations, our estimates may be biased upwards. While such autoregression is a potential concern in many applications, it is not a concern here. After all, there are no wealth effects associated with the liquidation itself or the act of reinvesting the liquid funds (the wealth effects from potential fee payments can safely be seen as very small). Finally, as in a standard RD design, one may worry about strategic behavior around the threshold. Clearly, using time as an assignment variable makes such test logically irrelevant. However, one may worry about a type of sorting when individuals change their behavior to avoid the treatment, in our setting, by selling before the forced sell, which is announced either one month (for funds domiciled in Luxembourg) or six months (for funds domiciled in Germany) in advance. However, those individuals are automatically excluded from the analysis as they are not affected by the forced sale. Furthermore, we do not observe sorting or bunching of sales near the announcement or forced liquidation date.

4 Results

4.1 Main results

Table 5 shows the estimation results for the share of liquidity that remained in the settlement account, got reinvested, transferred to savings accounts, or transferred to checking accounts or out of the bank in the five days after individuals receive their liquidity from the forced sales.

[Insert Table 5 about here]

We find that, on average, individuals reinvest 70% of their newly found liquidity within a few days. Moreover, a fraction of investors leave the liquidity in the settlement account or transfer the funds to the savings account.

Furthermore, Table 6 shows the same estimation results for the share of liquidity that remained in the settlement account, got reinvested, transferred to savings accounts, and transferred to checking accounts or out of the bank in the five days after individuals receive their liquidity from the forced sales, interacted with a dummy of whether or not individuals realized a loss relative to their initial investment.

[Insert Table 6 about here]

It can be seen that individuals are much more likely to reinvest a capital gain, then to reinvest a capital loss. Strikingly, the reinvestment share is approximately 80% for capital gains but only 45% for capital losses. Furthermore, a capital loss is in the first few days significantly less likely to be transferred out and will stay in the settlement account.

Tables 7 and 8 show the same estimation results for the share of liquidity reinvested, transferred to savings accounts, and transferred to checking accounts or out of the bank in the thirty days after individuals receive their liquidity from the forced sales. The results for five versus thirty days look qualitatively and quantitatively similar. After thirty days, individuals also transfer a significant fraction of approximately 16% of their liquidity into savings accounts.

[Insert Table 7 and 8 about here]

We want to compare the estimated coefficients in response to forced sales to the estimated coefficients when sales were deliberate or voluntary. We estimate the same specification for two other groups of sales. First, we estimate the coefficients using all the sales of investors who sold the same funds after the announcements of the funds' liquidations but before the forced liquidations. Second, we estimate the coefficients using all the sales of investors who sold the same funds before the announcements of the funds' liquidations. The estimation results for the deliberate sales after the announcements of the funds' liquidations for either five or thirty days line up sensibly. When individuals sell deliberately before the funds' closure announcements, they tend to reinvest a smaller share of their liquidity immediately. Presumably, because they decided to rebalance or consume part of their funds. We see some transfers into savings accounts and some transfers to checking accounts and out of the bank as well as some liquidity simply remaining in the settlement account after 5

days though less so after 30 days. The results for deliberate sales after the announcement look very similar to the sales before the announcement. Furthermore, we do not find additional sales around the announcement date or a large run-up before the closing date. Both of these findings make us believe that most individuals miss the announcement of the fund closure that is only reported in the half-year investment prospectus.

We now turn to the unconditional panel regression to compare all individuals who sell deliberately or were forced to sell in one regression controlling for fund fixed effects. The panel estimation results paint a similar picture as the cross-sectional results. Individuals reinvest approximately 70% on average after 30 days, and approximately 30% less if they sell voluntarily before or after the announcement of the forced sale event. Additionally, in response to deliberate sales, individuals transfer more funds out of the bank which is likely consumption and perfectly consistent with endogenous liquidation motives. These results can be found in Table 9.

[Insert Table 9 about here]

Moreover, if individuals are forced to liquidate at a loss, they reinvest approximately 50% of their newly found liquidity less. In contrast, if they liquidate deliberately, they reinvest less in response to a loss but only approximately 30%. Additionally, individuals save more especially when they were forced out of a fund and, if they sold deliberately, transfer more out of the bank for consumption. The results with loss interactions can be found in Table 10.

[Insert Table 10 about here]

We find the same effects when we not only look at whether or not individuals reinvest into any funds, stocks, or bonds, but also whether they reinvest into a lower or higher risk class. For almost all securities in our sample, we observe the officially established risk class going from 1 (for instance, savings accounts) to 5 (for instance, stocks, options, and futures). We simply run the same specification but use the first reinvestment value times the risk class as the outcome variable and the liquidation value times the risk class (most funds have risk classes 3 or 4) as the regressor. Results can be found in Table 11 and line up nicely and internally consistent as well as in accordance with our previous results of less risk-taking after losses especially when individuals were forced out

of funds rather than liquidated voluntarily. Furthermore, we run a linear probability model to estimate the likelihood that individuals reinvest into funds or the likelihood that individuals do not reinvest any liquidity at all after 30 days. The results can be found in Table 11 and also line up nicely with our previous results. Individuals are less likely to reinvest into funds after a loss and more likely to not reinvest at all.

[Insert Table 11 about here]

Our results on gains and losses are not consistent with tax considerations as a reason for why individuals do not hold optimally rebalanced portfolios before the forced sale event, which would imply that they would not optimally reinvest a 100% of their liquidation. If this were the case, then capital losses should be reinvested at a higher rate than capital gains. Moreover, individuals do not appear to actively rebalance in the right direction. If they would take the forced liquidation as an opportunity to actively rebalance, potentially because they held an suboptimal portfolio initially because of tax considerations, then they should again reinvest a loss at a higher rate than a capital gain. Though, the overall portfolio performance should matter for rebalancing which we look at more closely in the following section. Furthermore, it could be argued that realizing a capital gain or loss is endogenous as the market entry decision is endogenous. However, it is important to keep in mind that individuals do not know that their fund will get liquidated at the time of investing. Furthermore, individuals do not successfully market time as a general rule. However, individuals who hold losers could react differently to the fund closure announcement than individuals who hold winners, a concern which we also address in the following section.

4.2 Robustness

We find consistent effects throughout specifications and sample splits that line up sensibly for individuals who are forced to sell versus those that sell before versus after the announcement of the fund closure. The cross-sectional specification basically treats the population as similar and conducts an experiment in which 2228 individuals are chosen at a point in time to give them their investment back. Thus, we identify a pure cross-sectional effect of individuals receiving more versus

less funds back. On the other hand, the unconditional cross-sectional regression identifies an effect off of individuals investing into the same fund and either selling deliberately or being forced out. Furthermore, within each fund, the specification identifies individuals off of investing at a loss versus a gain.

While our results may be specific to the year 2007, as many of our forced sale events happen in that year, it is important to note that the financial crisis did not hit before the end of 2008. In that sense, our results are unlikely to be affected by the financial crisis. Furthermore, we include year fixed effects in all our regressions and thus do not identify off of individuals being forced out of funds in year 2007 versus other years. Finally, in our preferred specification, we control for fund fixed effects and thus identify off of individuals being invested in the same fund, say one that closes in 2007, and selling deliberately versus not as well as at a gain versus a loss. Furthermore, the fund fixed effect control for the closure date and thus effectively for all market or other contemporaneous conditions at the time of the fund closure. Most of the forced sales in 2007 are due to the closures of a few funds that the investors of this particular bank were invested in. The reason that these funds closed were due to a large German bank closing an arm of their operations that white-labeled funds for our online bank which were marketed through their clients. Thus, most of the variation in our sample is because of fund closures that are not due to small niche funds or underperformance.[1] In any case, while we acknowledge that underperformance is probably a main driver of fund closures, we do not think individuals would choose to invest into a fund because they expect it to underperform and then close, which is our identifying assumption.

Individuals could select into keeping (i.e., to be forced out) or selling the fund after the announcement of the closure. While we argue that most individuals do not notice the fund closure (because it is only reported in the fund report and the pre- and post-announcement individuals behave very similarly), we still want to control for this selection to ensure our treatment is exogenous conditional on the controls. When we compare characteristics such as the holding period or individual fund return over his or her holding period, whether at a gain or a loss, we do not find significant differences. But, we can include a control dummy for being forced out at a loss

[1] 354 of our 2,228 funds closed at a loss over their entire lifespan. If markets are efficient, leverage is restricted, and funds are well diversified, their performance should not deviate too substantially from the market.

in our regression to control for all unobservable or observable characteristics affecting losers rather than winners. Alternatively, we can simply control for each individual's fund return over his or her holding period of the fund. The holding period return is the only fund characteristic that may affect individual behavior and is observable to both the individual and the econometrician. Thus our treatment is exogenous conditional on the controls. Our results are unaffected by that and can be found in Table 12.

We estimate a local average treatment effects (LATE) of the population under consideration, a randomly selected sample of German retail clients of an online bank who hold a portfolio and trade at least once per year and chose to invest into a fund that happens to be closed. Moreover, the LATE effect for individuals holding a losing fund may be different than for those holding a winning fund. The dummy for holding a losing fund that is to be closed versus holding a winning fund that is to be closed also controls for all time-invariant observable or unobservable characteristics of individuals holding a losing fund versus individuals holding a winning fund (results can be found in Table 12). Moreover, to the extent that the decision to hold a loser versus a winner is not affected by the announcement of the fund closure, which we address by controlling for the loser dummy or individual fund returns over their holding period, our treatment is exogenous for both winners and losers. Finally, we may worry that individuals react differently to the announcement of the fund closure depending on holding a loser or a winner. We can address all concerns about behavior after the fund closure announcement by using the amount invested into the fund at announcement and a loss at announcement as instruments for the liquidation amount and loss at closure indicator.

We argue that the liquidation event is exogenous to the retail investors that happen to invest into that fund. We think that it is unlikely retail investors would deliberately choose to invest into a certain fund because they expect it to be closing. Moreover, we think that the liquidation amount, as determined by the initial investment into the fund, is unrelated to the fact that the fund later happens to close. However, the return of the initial investment is potentially jointly determined by market factors that also determine whether individuals want to reinvest at a higher or lower rate at the time of event closure. Thus, while the initial investment and the closure date is exogenous, the return of the initial investment may be subject to an omitted variables problem that also determines

individual's propensity to reinvest (for instance, sentiment or market conditions). When we control for fund fixed effects in the unconditional cross-sectional regression, we also effectively control for the time and market as well as all other contemporaneous conditions at the time of the fund closures. Still, we can additionally control for the market return, in the past three or twelve months, and obtain the same result, as well as we can control for individual's portfolio returns, in the past three or twelve months, and obtain the same result.[2] Alternatively, we can use the initial investment amount as an instrument. All robustness checks can be found in Table 12.

[Insert Table 12 about here]

5 Mechanisms

5.1 Tax considerations and rebalancing

In Germany, capital gains were un-taxed before 2009 and since 2009 are un-taxed until individuals reach their initial allowance (Freibeträge). When capital gains are taxed after the initial allowance, the tax is the same rate as dividends and interest payments and it is subtracted at the source. Thus, in the event of a taxed capital gains realization, the funds that arrive in the settlement account are already after tax funds. Since 2009, the capital gains tax (Abgeltungssteuer) is 25% plus solidary addition (Solidaritätszuschlag) (5.5% of the capital gains tax) and church tax (Kirchensteuer) (8 or 9% of the capital gains tax) which amounts to approximately 28% in total. In contrast, capital losses are carried over and applied to following capital gains at the source. Furthermore, the initial allowances (Freibeträge) are 801€ for singles and 1.602€ for married couples. Individuals can specify their main brokerage such that the capital gains tax will not be subtracted unless the initial sum 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. For stocks and funds that were bought before the 1st of January 2009, the sale does not initiate the automatic capital gains tax subtracted at the source. For stocks and funds bought but not sold before 1st of January 2009, any capital gains will remain tax free until the end of 2017 and tax free up until

[2]The performance of the individuals' portfolios is calculated following Bhattacharya et al. (2012).

100,000€ from January 2018 on. Thus, for all practical purposes, the capital gains are either un-taxed or the tax is taken at the source and all funds individuals receive are after-tax.

However, our results do not appear to be consistent with tax considerations as individuals should always reinvest a loss at a higher rate than a gain. While there exists a capital gains tax and capital losses are carried over, individuals should be incentivized to harvest losses as there does not exist a wash sale rule in Germany. While individuals could, in principal, harvest losses, casual observation of online media suggests that this behavior is not common. In any case, as no wash sale rule exists, this cannot explain the lack of reinvestment of losses.

As an alternative mechanism, individuals could hold a suboptimal portfolio in the first place because there are transaction costs associated with rebalancing. Transaction costs are not very high for the online bank under consideration but they are positive. In particular, one trade costs a basis of 4.90€ plus 0.25 percent of the order price times volume, a minimum of 9.90€ and a maximum of 59.90€. Additionally, the exchanges charge a small fee ranging from 0.0015 percent with a minimum of 1.50€ and to 0.0025 percent with a minimum of 2.50€. While in a standard model, such fees would not majorly discourage an agent to rebalance optimally, it may cause some insufficient rebalancing when individuals hold very small portfolios or are averse against paying fees. Nevertheless, again, the treatment of losses we observe is not consistent with insufficient rebalancing as individuals should reinvest a loss at a higher rate than a gain.

To address rebalancing and tax considerations, what we can do additionally is controlling for individual's fund return and overall portfolio returns over the fund investment or over the past three and twelve months. After all, rebalancing needs as well as tax considerations should be determined by the fund or overall portfolio return of the individual. Our results are unaffected by that and can be found in Table 12.

5.2 Inattention

We know from the existing literature that investors are inattentive and we observe quite substantial inertia, i.e., money staying in the settlement account especially in the first five days after the forced sale. A natural question is whether individuals notice the announcement and or the forced sale

or not. Because the deliberate sales we observe pre and post the closure announcement look very similar, we believe that most individuals miss the closure announcement. Furthermore, we do not observe heightened selling activity around the date of the announcement and only a small run-up in sales prior to the announcement (approximately one tenth of the number of sales at the date of closure).

After deciding to close the fund, the investment company has to report to the supervisory authority and responsible reserve bank. In turn, the closure is announced in the electronic federal gazette, the "Bundesanzeiger," as well as in the half-year report to inform investors. Furthermore, the company has to adhere to a notice period of one or six months after they have informed the investors in writing via the half-year report. We believe that many investors ignore all fund reports and thus miss the closure announcement. However, upon the liquidation of the fund, the investor receives a sales receipt by email or mail. This sales receipt is much less likely to remain unnoticed as it also states the tax implications as well as whether or not the investor experienced a capital gain or loss. Even if investors are inattentive though, such inattention cannot explain our results as they have to choose to be more inattentive in the event of a loss than a gain.

5.3 Mental accounting, realization utility, and effects

As an alternative theoretical explanation, we consider mental accounting. Clearly, the transfer of money from the fund to the settlement account caused individuals to treat it differently especially so when it represents a loss as opposed to a gain. We thus provide evidence for mental accounting, as the transfer between accounts matters, even though the money is theoretically fungible (abstracting from the transaction costs). Thaler (1985) and Shefrin and Thaler (1988) were pioneering the mental accounting literature and other empirical evidence exists Milkman and Beshears (2009); Feldman (2010); Choi et al. (2009); Abeler and Marklein (2008); Huffman and Barenstein (2005); Karle et al. (2011).

We also think that investors may take the liquidation as an exogenous reason to close their mental investment account, which leads them to not engage in the disposition effect (Odean, 1998). That our investors are subject to the disposition effect, and thus treat unrealized capital gains and

losses very differently, has been documented by Koestner et al. (2017). We thus provide empirical evidence for the theoretical framework developed in Barberis and Xiong (2012) who explain the disposition effect via a utility function in which individuals narrowly frame utility over individual stock's sales or realizations. Because individuals dislike realizing losses more so than they like realizing gains, the utility specification explains the disposition effect. What we observe is a reverse disposition effect after forced realizations (Chang et al., 2016) and fully consistent with the modeling assumptions put forward in Barberis and Xiong (2012). Our findings are also consistent with the empirical observation in Frydman et al. (2017) that individuals do not display disposition effects because they may not close mental accounts when they reinvest again quickly. After all, for our mutual fund liquidiations, no equivalent reinvestment exists.

Imas (2016) develops a theoretical framework of dynamic cumulative prospect theory with mental accounting. After a paper loss, the mental account of prior outcomes remains open and the loss is evaluated jointly with the outcome, causing the individual to take on more risk to recover from it. A realized loss closes the associated mental account and resets the reference point. Closing the mental account in the red causes the individual to be sensitized to the prospect of further losses, leading him or her to take on less risk. In contrast, after a realized gain, the investor is not sensitized, resulting in the prediction that realized gains should result in more reinvestment than realized losses. Imas (2016) presents evidence for this framework in a series of lab experiments. Our results are unique in providing clean evidence from consequential investment decisions in the field, and can thus be seen as strong empirical support for the realization effect.

5.4 Experiential learning

Our findings are related to the literature on experiential learning and how personal experiences shape preferences, such as Anagol et al. (2015), Andersen et al. (2014), and Kaustia and Knüpfer (2008). The differences in behavior between voluntary sales, that we argue are not majorly affected by the closure announcement that may be unobserved. When individuals get forced out of a fund then they have a worse experience, especially when the fund investment is a loser, then if they decide to sell voluntarily. This learning about one's ability to invest into the stock market is relevant for the

stock-market non-participation puzzle and low portfolio shares we observe empirically. Furthermore, they appear to become more risk averse in response as documented in Koudijs and Voth (2016) and Malmendier and Nagel (2011).

6 Conclusion

Using a large sample of transaction-level data on all asset holdings, trades, balances, spending, and income from a German retail bank, this paper explores how individual consumption responds to realized capital gains and losses. Our identification strategy exploits mutual fund closures, which are arguably exogenous to retail investors. We find that individuals reinvest a large part of their newly found liquidity immediately. However, even after a month, individuals have not reinvested a share of their liquidity and also transferred some of it into savings accounts. These findings suggest that individuals were not holding perfectly optimized portfolios in the first place and are partially inert. Furthermore, individuals behave very differently if a loss is realized instead of a gain relative to their initial investment. If a gain is realized, individuals reinvest almost 85% of their funds. If, however, a loss is realized, then individuals only reinvest 50% of their funds and tend to transfer more into savings accounts. This differential treatment of gains and losses is inconsistent with active rebalancing or tax considerations.

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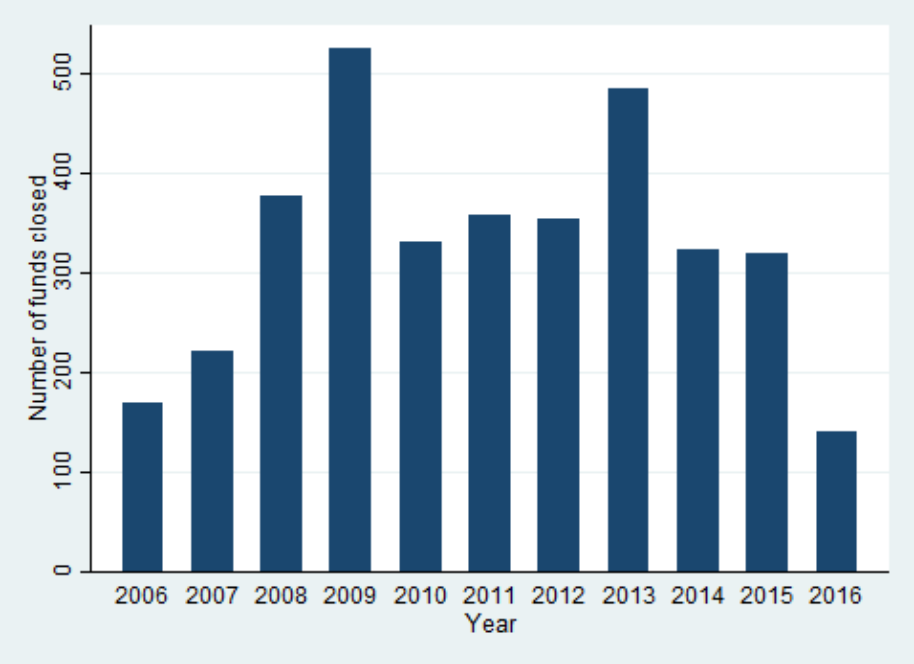

Figure 1: Number of mutual funds closures, as identified by the International Securities Identification Number (ISIN), per year over the period 2006 to 2016

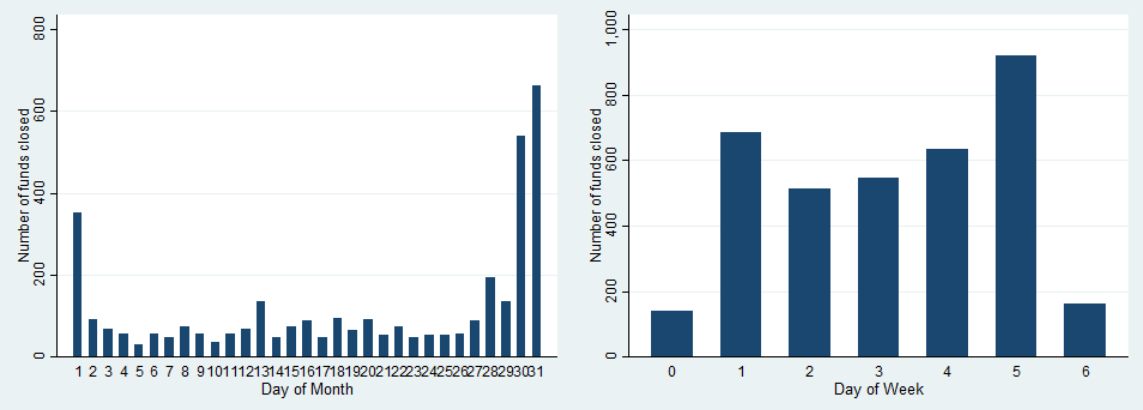

Figure 2: Number of mutual funds closures, as identified by the International Securities Identification Number (ISIN), per day of month and per day of week (0=Sunday to 6=Saturday)

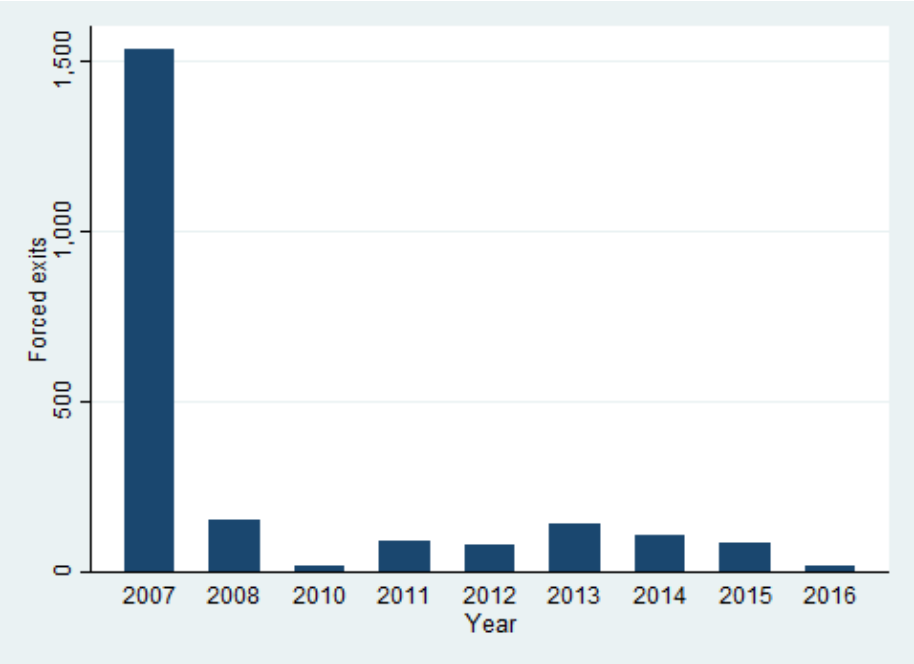

Figure 3: Number of forced sales, i.e., number of individuals affected by each fund closure, per year over the period 2006 to 2016

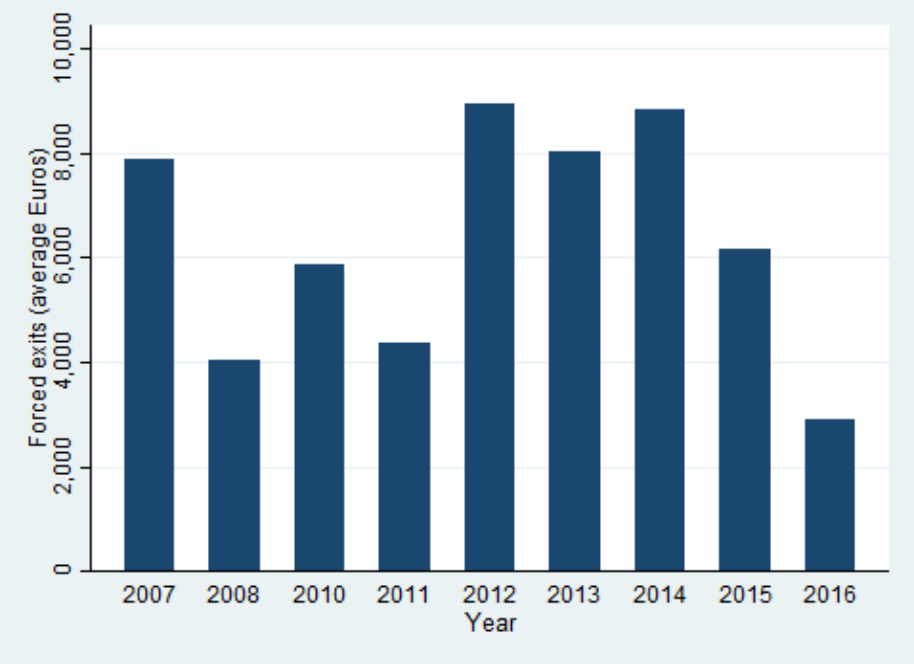

Figure 4: Average amounts of forced sales per year over the period 2006 to 2016

Table 1: Summary statistics for the forced sales events of all fund closures

	mean	standard deviation	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
holding period before closure (in days)	966	602	127	415	992	1,329	1,764
purchase share price	71	521	8.4	26	43	51	59
forced selling share price	76	510	13	27	50	61	61
value of forced sell	16,157	60,360	330	1,005	3,158	9,678	30,812
return of fund investment	.24	.37	-.16	.015	.15	.54	.66
observations	2,228						

Table 2: Summary statistics for all funds, all closed funds, and all merged funds

	mean	standard deviation	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
all funds							
mean total assets	1.4e+09	1.9e+10	724,455	5549601	2.7e+07	1.2e+08	6.1e+08
min total assets	3.1e+08	4.1e+09	100	76,000	2200000	1.6e+07	8.4e+07
max total assets	3.2e+09	5.2e+10	1723200	1.2e+07	6.0e+07	2.8e+08	1.5e+09
observations	49,605						
closed funds							
mean total assets	1.0e+08	8.3e+08	771,834	3235899	1.3e+07	4.9e+07	1.4e+08
min total assets	1.7e+07	9.8e+07	3,400	150,000	1610375	9225400	3.2e+07
max total assets	2.7e+08	2.6e+09	1307000	6472100	2.6e+07	9.5e+07	3.0e+08
last total assets	4.7e+07	5.1e+08	30,800	418,200	2787000	1.4e+07	5.8e+07
fund age (in days)	2,194	1,883	545	981	1,713	2,755	4,435
observations	2,809						
merged funds							
mean total assets	6.9e+07	2.0e+08	1197071	6813614	2.3e+07	6.5e+07	1.6e+08
min total assets	2.1e+07	9.7e+07	10,000	885,800	5027200	1.8e+07	4.1e+07
max total assets	1.5e+08	4.1e+08	2517000	1.3e+07	4.5e+07	1.3e+08	3.5e+08
last total assets	3.4e+07	1.1e+08	364,800	2475700	9915700	2.8e+07	7.5e+07
observations	1,077						

Table 3: Performance statistics for all funds, all closed funds, and all merged funds (annualized returns)

Fund type		N	125 tading days before		250 trading days before		500 trading days before	
			Mean	SD	Mean	SD	Mean	SD
Alternatives	All	2117	-2.20%	29.83%	-2.62%	16.73%	-3.10%	13.78%
	Deleted	15	3.80%	12.68%	2.20%	13.71%	2.91%	8.84%
	Merged	3	-1.20%	12.23%	0.13%	9.19%	-4.85%	7.77%
Bond	All	141918	-4.20%	14.61%	-4.42%	10.53%	-4.20%	6.90%
	Deleted	107	0.48%	48.31%	-0.89%	24.13%	-1.60%	12.27%
	Merged	70	0.74%	11.57%	-2.36%	7.28%	-3.77%	3.75%
Commodity	All	929	5.81%	37.01%	11.94%	34.16%	8.35%	13.00%
	Deleted	16	1.45%	16.14%	6.27%	11.51%	5.24%	7.73%
	Merged	4	-10.27%	8.08%	-0.21%	2.91%	1.25%	6.42%
Equity	All	423948	-4.18%	35.60%	-4.39%	26.09%	-3.26%	17.27%
	Deleted	292	0.42%	43.12%	0.95%	30.44%	1.46%	18.97%
	Merged	184	-4.33%	24.85%	-4.85%	17.68%	-6.41%	15.25%
Balanced Fund	All	156963	-3.05%	21.54%	-3.55%	14.46%	-3.14%	9.57%
	Deleted	142	6.80%	32.71%	6.62%	16.36%	4.62%	10.31%
	Merged	83	-3.75%	11.19%	-2.46%	8.66%	-1.77%	7.08%
Money Market	All	4892	-1.62%	7.87%	-1.91%	6.55%	-2.01%	3.88%
	Deleted	23	6.63%	17.79%	3.13%	10.66%	0.32%	4.88%
	Merged	13	0.40%	5.05%	-0.93%	2.45%	-1.62%	1.72%
Other	All	6117	3.90%	33.90%	2.97%	22.29%	2.73%	17.19%
	Deleted	92	-3.05%	19.08%	-0.98%	11.19%	-0.18%	9.94%
	Merged	22	-5.25%	7.16%	-3.54%	5.51%	-1.85%	6.21%
Real Estate	All	12	8.78%	27.22%	5.65%	16.18%	2.63%	9.88%
	Deleted	0						
	Merged	1	-0.14%	0.00%	-1.78%	0.00%	-1.95%	0.00%

Table 4: Summary statistics for all individuals, all affected individuals, and affected individuals who were ultimately forced to sell (wealth, income, and risk aversion are self-reported in brackets)

	mean	standard deviation	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
all individuals							
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	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
observations	32,051						
affected individuals							
male	.86	.35	0	1	1	1	1
age	54	13	39	45	52	61	72
PhD educated	.088	.28	0	0	0	0	0
account tenure	13	3.5	11	11	11	18	19
risk class	3.7	1.5	1	3	4	5	5
wealth	54,208	112,627	5,000	20,000	45,000	45,000	175,000
income	54,902	24,995	10,000	30,000	50,000	80,000	80,000
number of purchases	564	3,078	5	18	62	168	504
number of sales	498	2,986	9	22	58	158	417
risk class of trades	4.3	1.5	3.6	3.9	4.1	4.5	4.9
portfolio value	65,827	118,273	8,195	18,801	38,086	74,595	131,792
number of securities	52	28	15	29	51	74	94
HH index	.12	.13	.0062	.027	.075	.16	.29
observations	38,141						
affected individuals forced to sell							
male	.84	.37	0	1	1	1	1
age	53	12	40	45	51	59	68
PhD educated	.098	.3	0	0	0	0	0
account tenure	13	3.3	11	11	11	13	19
risk class	3.4	1.4	1	3	4	5	5
wealth	55,282	139,413	5,000	20,000	45,000	45,000	175,000
income	54,734	24,021	30,000	30,000	50,000	80,000	80,000
number of purchases	119	1,023	2	6	23	65	171
number of sales	105	1,059	4	9	21	53	137
risk class of trades	4.3	1.7	3.4	3.7	4	4.4	5
portfolio value	60,590	123,922	9,128	19,710	36,732	63,517	110,742
number of securities	51	29	15	27	48	74	96
HH index	.099	.11	.0053	.024	.065	.14	.24
observations	2,228						

Table 5: Estimation results from forced liquidations of fund closures after 5 days

	staying in settlement	outflows into portfolio	outflows into savings	all other outflows	outflows out of bank
liquidation	0.2375***	0.7021***	0.0493*	0.0110	-0.0140
	(0.0749)	(0.0821)	(0.0283)	(0.0209)	(0.0188)
year fes	✓	✓	✓	✓	✓
month fes	✓	✓	✓	✓	✓
week-of-month fes	✓	✓	✓	✓	✓
ISIN chars and fund age	✓	✓	✓	✓	✓
observations	2,228	2,228	2,228	2,228	2,228
R-squared	0.1431	0.4223	0.0278	0.0109	0.0129

Table 6: Estimation results from forced liquidations of fund closures after 5 days interacted with losses

	staying in settlement	outflows into portfolio	outflows into savings	all other outflows	outflows out of bank
liquidation	0.1235**	0.8195***	0.0400	0.0169	-0.0273
	(0.0508)	(0.0627)	(0.0323)	(0.0222)	(0.0272)
liquidation*loss	0.4064**	-0.4186**	0.0331	-0.0209	0.0473
	(0.1882)	(0.1993)	(0.0573)	(0.0444)	(0.0348)
year fes	✓	✓	✓	✓	✓
month fes	✓	✓	✓	✓	✓
week-of-month fes	✓	✓	✓	✓	✓
ISIN chars and fund age	✓	✓	✓	✓	✓
observations	2,228	2,228	2,228	2,228	2,228
R-squared	0.1913	0.4531	0.0283	0.0113	0.0143

Table 7: Estimation results from forced liquidations of fund closures after 30 days

	staying in settlement	outflows into portfolio	outflows into savings	all other outflows	outflows out of bank
liquidation	0.1437***	0.6824***	0.1565***	0.0174	-0.0156
	(0.0540)	(0.0837)	(0.0583)	(0.0297)	(0.0318)
year fes	✓	✓	✓	✓	✓
month fes	✓	✓	✓	✓	✓
week-of-month fes	✓	✓	✓	✓	✓
ISIN chars and fund age	✓	✓	✓	✓	✓
observations	2,228	2,228	2,228	2,228	2,228
R-squared	0.0471	0.2535	0.0390	0.0107	0.0260

Table 8: Estimation results from forced liquidations of fund closures after 30 days interacted with losses

	staying in settlement	outflows into portfolio	outflows into savings	all other outflows	outflows out of bank
liquidation	0.0766*	0.7869***	0.1272***	0.0093	-0.0421
	(0.0463)	(0.0628)	(0.0456)	(0.0265)	(0.0338)
liquidation*loss	0.2393	-0.3725*	0.1044	0.0288	0.0946
	(0.1483)	(0.2150)	(0.1462)	(0.0717)	(0.0724)
year fes	✓	✓	✓	✓	✓
month fes	✓	✓	✓	✓	✓
week-of-month fes	✓	✓	✓	✓	✓
ISIN chars and fund age	✓	✓	✓	✓	✓
observations	2,228	2,228	2,228	2,228	2,228
R-squared	0.0616	0.2691	0.0409	0.0111	0.0281

Table 9: Estimation results from forced liquidations as well as deliberate liquidations pre and post announcements of fund closures after 30 days

	staying in settlement	outflows into portfolio	outflows into savings	all other outflows	outflows out of bank
liquidation	0.1132	0.7314***	0.1299**	0.0254	-0.0231
	(0.0930)	(0.0953)	(0.0577)	(0.0445)	(0.0486)
liquidation*post	0.0971	-0.2496**	0.0384	0.1141**	0.1044*
	(0.1160)	(0.1189)	(0.0720)	(0.0555)	(0.0606)
liquidation*pre	0.1764*	-0.3532***	0.0093	0.1675***	0.1676***
	(0.0947)	(0.0971)	(0.0588)	(0.0453)	(0.0495)
ISIN fes	✓	✓	✓	✓	✓
year fes	✓	✓	✓	✓	✓
month fes	✓	✓	✓	✓	✓
week-of-month fes	✓	✓	✓	✓	✓
observations	38,141	38,141	38,141	38,141	38,141
R-squared	0.0506	0.0650	0.0578	0.0637	0.0888

Table 10: Estimation results from forced liquidations as well as deliberate liquidations pre and post announcements of fund closures after 30 days interacted with losses

	staying in settlement	outflows into portfolio	outflows into savings	all other outflows	outflows out of bank
liquidation*forced	0.0608	0.8543***	0.0779	0.0070	-0.0610
	(0.1047)	(0.1072)	(0.0649)	(0.0500)	(0.0546)
liquidation*post	0.0997	-0.2947**	0.0938	0.1012	0.1012
	(0.1301)	(0.1333)	(0.0807)	(0.0621)	(0.0679)
liquidation*pre	0.2621**	-0.4144***	0.0227	0.1297**	0.1458***
	(0.1067)	(0.1092)	(0.0662)	(0.0509)	(0.0556)
liquidation*forced*loss	0.2015	-0.4845**	0.2070*	0.0761	0.1523
	(0.1887)	(0.1933)	(0.1171)	(0.0901)	(0.0984)
liquidation*post*loss	0.1976	-0.3375**	-0.0038	0.1437**	0.1846**
	(0.1430)	(0.1465)	(0.0887)	(0.0683)	(0.0746)
liquidation*pre*loss	-0.1591***	-0.2994***	0.1867***	0.2718***	0.2884***
	(0.0428)	(0.0438)	(0.0266)	(0.0204)	(0.0223)
ISIN fes	✓	✓	✓	✓	✓
year fes	✓	✓	✓	✓	✓
month fes	✓	✓	✓	✓	✓
week-of-month fes	✓	✓	✓	✓	✓
observations	38,141	38,141	38,141	38,141	38,141
R-squared	0.0511	0.0664	0.0591	0.0682	0.0930

Table 11: Estimation results from forced liquidations as well as deliberate liquidations pre and post announcements of fund closures after 30 days interacted with losses and flows into first investment multiplied by risk class or a linear probability model of reinvesting into funds as well as no reinvestment after 30 days

	outflows*riskclass	reinvestment into funds	no reinvestment
liquidation*riskclass*forced	1.1469***		
	(0.0662)		
liquidation*riskclass*post	-0.2105***		
	(0.0752)		
liquidation*riskclass*pre	-0.5144***		
	(0.0675)		
liquidation*riskclass*forced*loss	-0.4723***		
	(0.1046)		
liquidation*riskclass*post*loss	-0.4037***		
	(0.0617)		
liquidation*riskclass*pre*loss	-0.0671***		
	(0.0209)		
liquidation*forced		0.6869***	0.1029***
		(0.0213)	(0.0192)
liquidation*post		-0.0277	-0.0149
		(0.0187)	(0.0169)
liquidation*pre		0.0092	-0.0522***
		(0.0161)	(0.0145)
liquidation*forced*loss		-0.2342***	0.2246***
		(0.0263)	(0.0238)
liquidation*post*loss		-0.0839***	0.0682***
		(0.0184)	(0.0166)
liquidation*pre*loss		-0.0513***	0.0299***
		(0.0060)	(0.0054)
ISIN fes	✓	✓	✓
year fes	✓	✓	✓
month fes	✓	✓	✓
week-of-month fes	✓	✓	✓
observations	36,271	38,135	38,135
R-squared	0.3119	0.1115	0.1091

Table 12: Estimation results from forced liquidations of fund closures after 30 days controlling for a losing fund investment and the return of the fund investment as well as portfolio returns over the time of the fund investment, the 3 months before fund closure, or the 12 months before fund closure or using announcement value and loss as an instrument

	outflows into portfolio	outflows into portfolio	outflows into portfolio	outflows into portfolio	outflows into portfolio	outflows into portfolio
liquidation	0.8439***	0.8222***	0.8196***	0.8195***	0.8197***	
	(0.0618)	(0.0654)	(0.0627)	(0.0627)	(0.0627)	
liquidation*loss	-0.5095**	-0.4230**	-0.4187**	-0.4187**	-0.4184**	
	(0.2286)	(0.2043)	(0.1993)	(0.1994)	(0.1993)	
announcement						0.8268***
						(0.1109)
announcement*loss						-0.4525**
						(0.2008)
dummy for investment loss	✓					
fund return over investment		✓				
portfolio return over fund investment			✓			
three months portfolio return				✓		
twelve months portfolio return					✓	
year fes	✓	✓	✓	✓	✓	✓
month fes	✓	✓	✓	✓	✓	✓
week-of-month fes	✓	✓	✓	✓	✓	✓
ISIN chars and fund age	✓	✓	✓	✓	✓	✓
observations	2,228	2,228	2,222	2,227	2,227	31,437
R-squared	0.4597	0.4532	0.4529	0.4531	0.4532	0.0683