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FULLY CLOSED: INDIVIDUAL RESPONSES TO REALIZED GAINS AND LOSSES

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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. We exploit plausibly exogenous sales due to mutual fund liquidations for identification. Specifically, we estimate the marginal propensity to reinvest out of one dollar received from a forced liquidation, when the investor either achieved a 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 sales should be 100%. Individuals should just reinvest all of their liquidity immediately into a fund with similar characteristics. Empirically, individuals reinvest 83% on average if the forced sale resulted in a gain, but only 40% in the event of a loss. If individuals do not reinvest, they keep a share of their newly found liquidity in cash, save it, or consume it. 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 than paper losses. We thus provide evidence for realization utility and effects (Barberis and Xiong, 2012; Imas, 2016) and that individuals do not appear to learn rationally from experiences in the stock market (Malmendier and Nagel, 2011; Koudijs and Voth, 2016).

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1 Introduction

Standard economic theory predicts that fluctuations in stock prices should affect households' savings and consumption decisions; after all, stock and mutual fund holdings represent a significant portion of household financial wealth – comparable to the stock of housing wealth. At the same time, stocks and mutual 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. Therefore, standard economic theory predicts that individuals should respond to changes in the value of their stockholdings independent of whether those holdings are liquidated, if transaction costs and tax considerations are negligible.

But there are also economic theories such as Barberis et al. (2001), Barberis and Xiong (2012), and Pagel (2018) that postulate how fluctuations in stock prices may elicit direct utility flows and be a source of emotional stress making investors reluctant to liquidate for consumption, rebalance optimally, or even invest in the first place. Moreover, there are a number of experimental studies such as Imas (2016), Thaler and Johnson (1990), and Kuhnen (2015) that analyze how subjects change their preferences in response to gains and losses as well as studies using using observational data and other personal experiences (Malmendier and Nagel, 2011; Koudijs and Voth, 2016; Kaustia and Knüpfer, 2008; Coval and Shumway, 2005). Nevertheless, clean empirical evidence using observational data about how individuals respond to realized and large capital gains and losses and how those experiences shape their preferences to invest into stocks and mutual funds is 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 conditions, such as income growth and consumer confidence. Therefore, the relationship between aggregate investment, consumption, and stock price fluctuations will be overestimated due to common shocks. Common shocks are arguably less problematic when utilizing individual-level data and abnormal returns as in Baker et al. (2006) and Maggio et al. (2017). This way, one could sensibly estimate the marginal propensity to invest or consume out of unrealized capital gains or irregular dividends. However, if one were to look at realized capital gains, there

are obvious endogeneity problems. When individuals decide to liquidate stockholdings, they do so endogenously because they decide to 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 113,031 retail investors in Germany spanning more than 10 years. We precisely measure each individual's daily activity by his or her log on and trading behavior as well as his or her 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 sales. 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. Not all of our individuals are affected by all of the fund closures; therefore, our sample of forced sale events equals 2,228 cases.

We find that individuals reinvest – on average – approximately 70% of their funds within a few days and up to a month after a forced sale event. Furthermore, the reinvestment share is approximately 83% for gains but only 40% for 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 at the time of the fund closure, they would reinvest 100% of their newly found liquidity in a fund with similar characteristics. If individuals held suboptimal portfolios because of transaction costs or tax considerations, they might use 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 the closing fund's holdings or stock holdings in general.

Our main findings can be easily observed in the raw data. Figure 1 shows the distribution of the share of funds individuals reinvest within 30 days after the forced sale event separately in response to a gain or a loss relative to their initial investment. The graphs clearly indicate that some individuals do not reinvest any funds and that individuals are much less likely to reinvest in response to a loss. Furthermore, Figure 2 shows the share of funds individuals reinvest within 30 days after the forced

sale event relative to the simple arithmetic return of their individual fund investment. This figure also indicates that individuals are much less likely to reinvest when the investment resulted in a loss. Instead of showing all observations, Figure 3 shows averages in bins of the arithmetic returns and standard deviation bars. One can clearly observe a statistically significant discontinuity at and below the fund return rate of 0%, i.e., the fund being a losing versus a winning investment.

We estimate the effects of forced liquidations on reinvestment using a simple cross-sectional approach. The cross-sectional approach treats the mutual fund liquidations as exogenous shocks and estimates the average response to it. We can complement the identification approach with controls for time and each liquidation, i.e., month-by-year and fund (ISIN) fixed effects and find that our results are robust to different econometric specifications and controls. To further understand whether the experience of being forced out of an investment at a gain versus a loss affects individuals' preferences, i.e., risk aversion, or beliefs about their ability to invest in stocks, is difficult. We find evidence for both channels by showing that risk-taking at the internal margin is affected, i.e., individuals invest less into securities with a high risk rating (as determined by German regulations), but also showing that individuals are less likely to reinvest into funds as an asset class and more likely to stay disinvested in the stock market as a whole. Arguably it is not the retail investors' faults that a given fund closes at a loss. We thus conclude that individuals do not appear to learn rationally from the experience of forced losses in the stock market.

It has to be kept in mind that we estimate a treatment effect on 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 in a mutual fund that closes out on them. We carefully address three potential concerns about this 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 the individual holding period fund returns to account for the factor that may affect their decision to sell. We also instrument the amount of the liquidation and whether it is a loss by the amount invested at time of the announcement and whether the investment was trading as 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 including month-by-year fixed effects as well as the market or individual portfolio returns over the fund investment or 3 and 12 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 a fund will lose or win, we may estimate different effects for individuals holding a losing versus a winning fund. However, – all of our individuals – hold both losing and winning funds. Therefore, if the liquidation is exogenous to them, the gain or loss is exogenous too. Furthermore, we show that, on observables, the losing group does not differ from the winning group. Additionally, we can control for a dummy of holding a losing fund to account for all time-invariant (un)observable characteristics of holding a winner versus a loser. Finally, we can also run a regression discontinuity design which compares individuals near the threshold of a fund return rate of 0%, i.e., the fund being a losing versus a winning investment.

We argue that we provide empirical evidence for the theoretical framework developed in Barberis and Xiong (2012) where the authors explain the disposition effect through 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 (Chang et al., 2016) after forced realizations provides evidence for the modeling assumptions put forward in Barberis and Xiong (2012) in the sense that our findings indicate that realizations matter in causing utility flows. 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).

By showing that an individual's propensity to reinvest appears to be affected by losses, we also provide new empirical evidence from observational data relating to a large but mostly experimental 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), and IPO investors (Kaustia and Knüpfer, 2008; Anagol et al., 2015). 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; Coval and Shumway, 2005), while other studies find 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). 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.

Our findings are related to the literature on experiential learning and how personal experiences affect beliefs and preferences, such as Malmendier and Nagel (2011), Anagol et al. (2015), and Andersen and Nielsen (2011). The way individuals react to gains versus losses suggests that they are 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). Related to our findings, Choi et al. (2009) show that individuals save less in their 401(k)s after their portfolio returns were relatively bad or had high variance and Strahilevitz et al. (2011) show that brokerage investors tend to repurchase individual stocks they previously sold for a gain while shunning individual stocks they previously sold for a loss.

Gaining an understanding of individual learning and preferences for investing in stocks and funds is important for long-standing puzzles in household finance such as the stock market-non-participation puzzle. In the context of stock market participation, we thus provide empirical evidence using observational data for the mechanism in Kuhnen (2015) that individuals learn less rationally from losses in stock market investments because they form more pessimistic beliefs. Our findings are also consistent with the empirical observation in Frydman et al. (2015) 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 liquidations, no equivalent reinvestment exists. Furthermore, two recent papers, Briggs et al. (2015) and Andersen and Nielsen (2011), estimate the marginal propensity to invest in stocks using administrative data and large wealth shocks but focus on first-time participants. In contrast to these two papers, we focus on stock-market participants' marginal propensities to reinvest out of forced liquidations and look at potential stock market exit in response to losses.

Finally, by showing that individuals do not rebalance in due course after forced sales, we conclude that they do not appear to hold optimized portfolios following Kaustia and Rantapuska (2012), who show that individuals do not reinvest cash flows brought about by irregular dividends and tender offer proceeds, and 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.

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 112,072 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. In turn, we have data on a random 7% sample of the bank's customer base. 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 Lipper in 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 of the German population as a whole; less than half of Germans invest in stocks, either directly or indirectly. However, our sample is representative of self-directed retail investors in Germany and more generally of individuals in Germany holding an investment portfolio at a major bank. The average age of investors is 54 and the median age is 53. 14% of our

sample is female and 86% is male. 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 for our sample: 9% 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 72,181€, 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 investor to be around 48,000€. This value seems comparable to the average we observe in our sample when we restrict our data to the years up until 2013. The average portfolio value is then 55,854€. Additionally, we compare portfolio holdings to self-reported gross annual household income for those investors who self-reported income 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 4. 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 it represents the interests of the German fund industry at the national and international level.

[Insert Figure 4 about here]

Table 1 shows detailed summary statistics for our forced sale events including the holding periods before closure, the purchase and sale 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 also show that 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. When we perform a kitchen-sink regression in a linear probability model of mutual fund closures, we find that returns and size decrease the probability to close a fund but overall fund closures are not explained well by observables. After all, mutual funds are fairly diversified and thus mostly determined by market conditions and there is no clear evidence for manager skill (refer to Carhart, 1997, among many other studies). In any case, for identification, it matters whether investors can choose to invest in to-be-closed funds endogenously. We feel that is unrealistic and thus consider liquidations as plausibly exogenous.

Tables 2 and 3 show detailed summary statistics for all funds that did not close or closed respectively including returns. It can be seen that the closed funds did not necessarily perform much worse than 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 open and none of the statistics are significantly different except fund size. To make the comparison as appropriate as possible, we compare the return and size statistics to a matched sample of funds by asset class and regional focus. Overall, the size of the fund appears a more important factor for the sample of our closed funds than their performance. We confirm this finding in a kitchen-sink linear probability regression of a dummy for fund closure on all our observed fund characteristics.

[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 5.

[Insert Figure 5 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.[1]

[Insert Table 4 about here]

Furthermore, Table 5 shows detailed summary statistics for our investors that were forced to sell a fund at a gain at or a loss. It can be seen that the two samples of investors look similar. Individuals appear to be less active traders if they are forced out of a fund at a loss because their average holding period is longer and they have less of a disposition to sell both winners and losers. However, it is important to note that all individuals hold – both – winning and losing funds. To make that clear the table shows the sum of all paper and realized gains as well as losses at the time of sale (when individuals pay attention to their portfolios), and those are large for both groups of investors. The same holds true for the average number of paper and realized gains and losses in individuals' portfolios at the time of their sales.

[Insert Table 5 about here]

Figure 6 shows the average amounts 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 do not represent a wealth shock, but they are quite substantial liquidation shocks.

[Insert Figure 6 about here]

The average reimbursement is at 7,452€ with a median of 4,027€. We have 6 observations with liquidation amounts of more than 100,000€, where the 99th percentile is at 60,000€. We drop these

[1] Note that, for the average number of securities held by investors, we assume that all funds hold 100 securities.

6 outliers to mitigate outlier issues. That said, all our results hold for the full sample of observed liquidations.

Furthermore, Figure 7 shows all sales in the period before the fund closures. We do not find heightened sales activity at the announcement dates of the fund closures or a run-up before the fund closures. Funds that are domiciled in Luxembourg, which is 58% of our closed fund sample, have to announce their closure at least 1 month in advance in their semi-annual report. German funds, comprising 34% of our closed fund sample, have to announce the closure at least 6 months in advance in their semi-annual annual report. Note that we see the funds arrive in the individual settlement accounts and when the sales booking date is on or after the closure date, we consider the sale a forced sale. Otherwise we consider it a voluntary or deliberate sale. Depending on the time and day of week of the fund closure as well as the speed of the clearinghouse, the funds arrive on the closure day or up to a few days later in individual settlement accounts.

[Insert Figure 7 about here]

In contrast to the announcement of the fund closure, the actual fund closure is unlikely to be missed by individuals because they receive a sales receipt by email and mail that they are supposed to notice as the sales receipt is relevant for tax purposes. Figure 8 displays a scan of a sales receipt. One can see that the purchase price (Ausführungswert), the sales price (Kurswert), and the absolute capital gain or loss in this case (Veräußerungsverlust) are clearly indicated. It is thus fairly salient to the individuals if they lost money and how much.

[Insert Figure 8 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:

$$\Delta Y^i_{j,t,t+\tau} = \alpha + \beta F^i_{j,t} + \psi J_j + \gamma my_t + \epsilon^i_{j,t} \tag{1}$$

where $\Delta Y^i_{j,t,t+\tau}$ is the sum of the outcome variable of interest for investor i at the time of the forced sale event j on day t to $t+\tau$, α is an intercept, $F^i_{j,t}$ is the currency amount of the forced sale affecting investor i who invested in fund j at time t, J_j are fund (ISIN) fixed effects for each liquidation event j, and my_t are time controls, i.e., month-by-year fixed effects. We consider two bandwidths τ: 5 or 30 days since the day that the money arrives in individual's accounts. 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:

$$\Delta Y^i_{j,t,t+\tau} = \alpha + \beta F^i_{j,t} + \eta S^i_{j,t} + \psi J_j + \gamma my_t + \epsilon^i_{j,t} \tag{2}$$

where $\Delta Y^i_{j,t,t+\tau}$, α, J_j, and my_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. In this specification, we consider all individuals who at some point held the affected funds and all of their liquidations rather than only the forced ones.

We adjust standard errors for heteroskedasticity. Alternatively, we can cluster standard errors at the month-by-year or ISIN level. However, we do not think that standard errors are uncorrelated across time or ISINs and thus consider robust standard errors more appropriate.

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 primary outcome variable: (1) net transfers to the portfolio through purchases or sales of securities, i.e., reinvestment; (2) the risk class of each bought security as the outcome variable or the reinvested funds times their risk class; (3) a dummy for the first reinvestment going into a mutual fund as opposed to other security classes, a dummy for not reinvesting at all within 30 days, or a dummy for being invested into equity at all 6 months after the forced sale event; 4) the currency amount that the settlement balance is increased after 30 days, i.e., the money staying in the settlement account; (5) net transfers to the savings account at the bank, i.e., savings; (6) 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.

4 Results

4.1 Main results

Table 6 shows the estimation results for the conditional cross-sectional regression design, i.e., Equation 1. Here, we simply regress the share of liquidity that is reinvested in the 30 days after the forced sale event on the individual liquidation amount for just the 2,222 cases where individuals get back their investments (excluding the 6 outliers). The coefficient simply represents the average share of funds that individuals reinvest out of the forced sales. In the first column of Table 6, we only include a regression constant, whereas in columns 2 and 3, we also include month-by-year and fund or ISIN fixed effects. In turn, while we display robust standard errors in columns 1 to 3, column 4 displays standard errors that are clustered at the month-by-year level.

[Insert Table 6 about here]

We find that, on average, individuals reinvest 70% of their newly found liquidity within 30 days and this result is robust to the inclusion of controls or clustering standard errors instead of correcting for heteroskedasticity. Note that, the column showing the results for clustered standard errors displays fewer observations as all singleton observations, that would have single clusters, are dropped.

Furthermore, Table 6 shows the same regression specification results for the share of liquidity reinvested but including an interaction with a dummy of whether individuals realized a loss relative to their initial investment at the point of the forced sale. Again, we show our results for different sets of controls and calculations of standard errors. It can be seen that individuals are much more likely to reinvest a gain than a loss. Strikingly, in the baseline specification, the reinvestment share is approximately 83% for gains but only 40% for losses and this result is robust across specifications.

We now turn to the unconditional panel regression to compare all individuals who invested in the affected funds and sold deliberately or were forced to sell in one specification. The panel estimation results paint a picture similar to the simple cross-sectional results. Individuals reinvest approximately 70% on average after 30 days, and they reinvest less if they sell voluntarily before or after the announcement of the forced sale event. This makes sense because individuals liquidate voluntarily when they decide to rebalance or consume part of their funds. We again report results including different sets of controls and calculated standard errors which can be found in Table 7.

[Insert Table 7 about here]

Moreover, if individuals are forced to liquidate at a loss, they reinvest only half of their newly found liquidity relative to their reinvestment if they are forced to liquidate at a gain. If they liquidate deliberately after the announcement, our results are driven by very few observations because we do not have many individuals liquidating during the announcement period, which is only one month for Luxembourg funds that comprise the majority of our sample. When we look at deliberate liquidations before the announcement, our results in some specifications show that individuals also reinvest less in response to a loss. Of course, however, their baseline reinvestment is much smaller. The results with loss interactions can also be found in Table 7.

We thus find less reinvestment and therefore less risk taking at the external margin in response to forced sales at a loss. We can also look at the internal margin of risk taking and ask whether individuals not only reinvest less into stocks or bonds, but also whether they reinvest in a lower or higher risk class. For almost all securities in our sample, we observe the officially established risk classification 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 of 3 or 4) as the regressor. Results can be found in Table 8 and line up nicely and are internally consistent as well as in accordance with our previous results. Individuals take less risks, on the internal and external margins, after losses. Furthermore, we run a linear probability model to estimate the likelihood that individuals reinvest into funds as an asset class or the likelihood that individuals do not reinvest any liquidity at all after 30 days. The results can be found in Table 8 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 8 about here]

Furthermore, we can specifically look at stock market participation. When we use a dummy of whether individuals are invested in equities at all 6 months after the forced sale as the outcome variable, we find that the experience of being forced out of a losing fund causes a 3.62% reduction in the likelihood of participating in the stock market.

Table 9 shows the estimation results for other potential destinations of the individual funds: First, we observe the balance in the settlement account to estimate the share of liquidity that stays in the settlement account. Second, we also observe all transfers into savings accounts that we net out, i.e., if an individual transfers money in and out of the savings account within 30 days that is a net transfer of 0. Third, we can look at all transfers out of the bank minus all transfers into the bank to understand whether the funds leave the bank.

[Insert Table 9 about here]

When individuals sell deliberately before the funds' closure announcements, they tend to reinvest

a smaller share of their liquidity immediately. Presumably, this is because they decided to rebalance or consume part of their funds. In line with this presumption, we see some transfers into savings accounts and some transfers out of the bank as well as some liquidity simply remaining in the settlement account. Overall, however, our results appear to be noisy. After all we cannot earmark a € to make conclusive statements about what individuals do with their funds.

The results for deliberate sales after the announcement look very similar to the sales before the announcement though our estimates are noisier for the few trades we see after the announcement of the fund closures. We thus do not find consistently significant differences for the deliberate sales pre- and post-announcement; neither do we find additional sales activity around the announcement date nor a large run-up in sales before the closing date in Figure 7. 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.

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 simple cross-sectional specification basically conducts an experiment in which 2,228 individuals are chosen at some point in time to receive their investment back. In the simple cross-sectional specification, we thus identify a pure cross-sectional effect of individuals receiving more versus less funds back. In the specification with fund fixed effects, within each fund, we identify off of individuals trading at a loss versus a gain. On the other hand, the unconditional cross-sectional regression identifies the effect by also comparing individuals investing in the same fund and selling deliberately or being forced to sell, again conditional on having invested into the same funds. We now address a number of concerns and present robustness checks. The results of all robustness checks can be found in Table 10.

[Insert Table 10 about here]

Transfers to other brokerage accounts

In principle, it could be the case that individuals transfer their funds to a different brokerage account. However, we do not observe large or significant coefficients on the variable measuring outflows out of the bank in Table 9. Furthermore, we can look at customers who are flagged as clients without other banking relationships by the online bank. For these clients, the effect is even more pronounced.

Reasons for 2017 fund closures

While our results may be specific to the year 2007 because 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 month-by-year fixed effects in 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 effects 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 customers of this particular bank were invested in. These funds closed because a large German investment bank shut down an arm of its operations that white-labeled funds for our online bank. These white-labeled funds were marketed by our bank to its clients. Thus, most of the variation in our sample is generated by fund closures that are not due to the closure of small niche funds or underperformance. 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.

Selection into holding a closing fund

We estimate an average treatment effects of the population under consideration, a randomly selected sample of German retail clients of an online bank who hold a portfolio, trade at least once per year, and chose to invest in a fund that happens to close. Individuals could select into keeping (i.e., to be eventually forced out) or selling the fund either before or after the announcement of the closure. While we argue that most individuals do not notice the fund closure announcement (because it is only reported in the fund report, the pre- and post-announcement individuals behave not significantly different, and there is no sales activity around the announcement date or before the closure date), we still want to control for this selection to ensure our treatment is exogenous conditional on the controls. When we compare individual characteristics of our overall sample versus our affected sample, we do not find relevant differences. This can be seen in Table 4. Additionally, 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, which makes our treatment exogenous conditional on the controls. Our results are unaffected by that and can be found in Table 10.

Selection into holding a winning or losing fund

Moreover, individuals holding the affected funds may select into keeping (i.e., to be eventually forced out) a winning or losing fund such that the effect for individuals holding a losing fund may be different than for those holding a winning fund. We can include a dummy indicating that individuals hold a to-be-closed loser in our specification. The dummy for holding a losing 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 also be found in Table 10). However, in our data, both groups of individuals, those getting closed out of losing funds as well as those getting closed out of winning funds, hold both winners and losers. Given that we argue that our sample of closed funds are chosen exogenously to individual investor characteristics, if we pick a fund that happens to be a loser for some individuals and a winner for others and both of these groups hold winners and losers, the assignment into being

closed out at a loss is exogenous and the loss coefficient can be given a causal interpretation.

As can be seen in Table 5, the groups of investors being forced out at a gain versus a loss do not differ substantially in most dimensions. Most importantly, individuals forced out at a loss do not have substantially less risky portfolios or trade in less risky securities generally, which would explain their hesitation to reinvest a loss. Moreover, both groups of individuals hold winning and losing funds as described. While the group holding losing funds tends to have a longer holding period, they do not buy or sell substantially less than individuals holding the winning funds and while they have a lower propensity to sell winners and a higher propensity to hold losers, overall their holdings of paper winners and losers are similar (as captured by the variables called sum of paper and realized winners/losers at all sale dates). Furthermore, when we run a kitchen-sink regression of the dummy for being closed out at a loss on all our other observables, we only find the holding period to be statistically significant, which, given the number of regressors, could represent estimation error. Furthermore, note that our preferred specification with fund fixed effects does not compare the groups of individuals holding any losing or winning funds but rather compares individuals having invested into the same fund at different points in time.

Finally, we may worry that individuals react differently to the announcement of the fund closure depending on holding a loser or a winner. 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. Furthermore, we can address all concerns about behavior after the fund closure announcement by using the amount invested in the fund at announcement and a loss at announcement as instruments for the liquidation amount and loss at closure indicator.

Regression discontinuity design

As an alternative econometric specification, we could consider a type of regression discontinuity (RD) design by only comparing individuals who happen to have a slightly positive arithmetic return at the time of the forced sale to individuals with a slightly negative return. Restricting the sample to individuals with arithmetic returns in the range of plus or minus 10%, and rerunning

specification 2 with those 7,998 observations, month-by-year, and ISIN fixed effects, we find that individuals reinvest 81% in response to a forced liquidation with a loss interaction coefficient of -22% which is significant at the 5% level. This simple RD design thus confirms our results and lends additional credibility to the causal interpretation of the loss coefficient.

Time-varying omitted variables

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 in 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 the fund 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 an 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. Moreover, the month-by-year fixed effects control for all time-varying macroeconomic trends such as market sentiment. Still, we can additionally control for the market return, in the past 3 or 12 months, and obtain the same result, as well as control for the individual portfolio returns, in the past 3 or 12 months, and obtain the same result.[2]

The econometric application has the following features: (1) there is cross-sectional variation in the experimental implementation, i.e., individuals are affected to varying degrees or not at all; (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 confounds, 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

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

event. Moreover, we are interested in relatively short-run effects, from the day of the announcement to approximately one month after, we use many events rather than just one, and we flexibly control for time using calendar fixed effects. For these reasons, 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 to control and treatment status. 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 and month effects can be included as controls and to improve precision (Lee and Lemieuxa, 2010).

Estimates may be affected 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 upward. 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 or the act of reinvesting the liquid funds (the wealth effects from potential fee payments can safely be seen as very small). Finally, one may worry about strategic behaviors around the threshold. Clearly, using time as an assignment variable makes such tests 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 simple cross-sectional analysis because they are not actually forced to sell. Furthermore, as we discussed, we do not observe sorting or bunching of sales near the announcement or forced liquidation date and the voluntary sales before and after the announcement of the fund closure look similar.

5 Mechanisms and implications

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). The initial allowances 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) and we observe customers flagged by the bank as main customers who have set up the Freistellungsauftrag. For stocks and funds that were bought before the 1st of January 2009, capital gains are still tax-free. For these funds, any capital gains will remain tax free until the end of 2017 and tax free up until 100,000€ from January 2018 on. When capital gains are taxed, 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. Thus, if capital losses are realized before capital gains, then the capital gains tax will be automatically lowered by the realized losses. In summary, for all practical purposes, the capital gains are either un-taxed or the tax is taken at the source, and all reimbursements individuals receive are after-tax funds.

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 100% of their liquidation. If this were the case, then capital losses should be reinvested at a higher rate than capital gains. While there exists a capital gains tax and capital losses are carried over, individuals should be incentivized to harvest losses because Germany does not have a wash sale rule. While individuals could, in principle, harvest losses, casual observation of online media suggests that this behavior is not common. In any case, because no wash sale rule exists, it cannot explain the lack of reinvestment of losses.

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% 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% with a minimum of 1.50€ and to 0.0025% with a minimum of 2.50€. While in a standard model, such fees would not majorly discourage an agent to rebalance optimally (Briggs et al., 2015), it may cause some insufficient rebalancing when individuals hold very small portfolios or are averse to paying fees. These transaction costs may be a reason why individuals do not hold optimally rebalanced portfolios before the forced sale event, which would imply that they would not optimally reinvest 100% of their liquidation. However, 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 a suboptimal portfolio initially due to transaction costs, then they should reinvest the funds of a losing investment at a higher rate than those of a winning investment. If individuals rebalance at the stocks versus bonds level rather than the individual fund level, then the individual portfolio performance should matter for rebalancing.

To address rebalancing and tax considerations, we can control for the individual fund return and the individual portfolio returns over the fund investment or over the past 3 and 12 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 10.

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 5 days after the forced sale. A natural question is whether individuals notice the announcement and/or the forced sale. Because the deliberate sales we observe before and after 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) in Figure 7.

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 semi-annual report to inform investors. More specifically, the company has to adhere to a notice period of 1 or 6 months after it informed the investors in writing via the semi-annual investment 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 as the one displayed in Figure 7. This sales receipt is much less likely to remain unnoticed as it also states the tax implications as well as whether the investor experienced a capital gain or loss.

Even if investors are inattentive, such inattention cannot explain our results because investors have to choose to be more inattentive in the event of a loss than a gain. Therefore, investors must know whether they incurred a loss or a gain in the first place.

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 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 some 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).[3] We argue that these

[3] In our sample of 113,031 German investors over the period 2003 to 2016, we find an attenuated but still positive and strongly significant disposition to sell winning mutual funds. Chang et al. (2016) use the data from Odean (1998) consisting of g 73,558 US households from January 1991 to November 1996 and document a reverse disposition effect

empirical observations provide 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 than they like realizing gains, the utility specification explains the disposition effect. What we observe is a reverse disposition effect after forced realizations and in a sense this is fully consistent with the modeling assumptions put forward in Barberis and Xiong (2012). After all, Barberis and Xiong (2012) postulate that liquidations or sales cause utility flows, and even though they do not specify a change in risk aversion in response, that would be a natural next step. 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 liquidations, no equivalent reinvestment exists.

Whether losses increase or decrease risk-taking has been analyzed 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., 2018). 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; Coval and Shumway, 2005), while other studies find 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).

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

for delegated investments such as mutual funds.

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 our evidence can thus be seen as strong empirical support for the realization effect. Another recent study analyzing risk-taking after realizations in the laboratory is Merkle et al. (2018), who replicate the findings in Imas (2016) for positively skewed lotteries and physical transfers of money.

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. (2018), and Kaustia and Knüpfer (2008). When individuals get forced out of a fund they have a bad experience, especially when the fund investment is a loser. Individuals may learn from this experience about their ability to invest into the stock market. Such experiential learning is likely to be relevant for the stock-market non-participation puzzle and the low portfolio shares we observe empirically (which has been puzzling economists, Campbell, 2006). Furthermore, individuals appear to become more risk averse in response to losses as documented in Koudijs and Voth (2016) and Malmendier and Nagel (2011). In the context of stock market participation, Kuhnen (2015) find that individuals learn less rationally from losses in stock market investments because they form more pessimistic beliefs.

In terms of the time to response, when we look at the reinvestment after 5 versus 30 days, we do not find large differences. Thus, it is not simply be a matter of an initial shock but persistent learning which is also indicated by our result that individuals are more likely to exit the stock market. To further understand how irrational learning from bad experiences in the stock market affects individuals, we split the sample in a variety of ways to understand for which demographic and other individual characteristics, such as income and financial sophistication, the effects of bad experiences are most pronounced. We do not find significantly different results when we split by portfolio size, size of the initial investment, or length of the banking relationship, which may be measures of financial sophistication. We find that the effect is more pronounced for female investors, who constitute approximately 14% of our sample, as well as customers who are flagged by the online bank as clients without other banking relationships.

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 the individual propensity to reinvest 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, a portion of that newly found liquidity remains uninvested because individuals leave it untouched or transfer some of it into savings accounts. These findings suggest that individuals do not hold perfectly optimized portfolios 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 83% of their funds. If, however, a loss is realized, then individuals only reinvest 40% 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, but it is consistent with mental accounting, realization utility, and the so-called realization effect (Imas, 2016). Furthermore, we provide particularly clean evidence relating to the literature on experiential learning (Malmendier and Nagel, 2011; Koudijs and Voth, 2016; Kuhnen, 2015) that individuals do not appear to learn rationally from experiences in the stock market.

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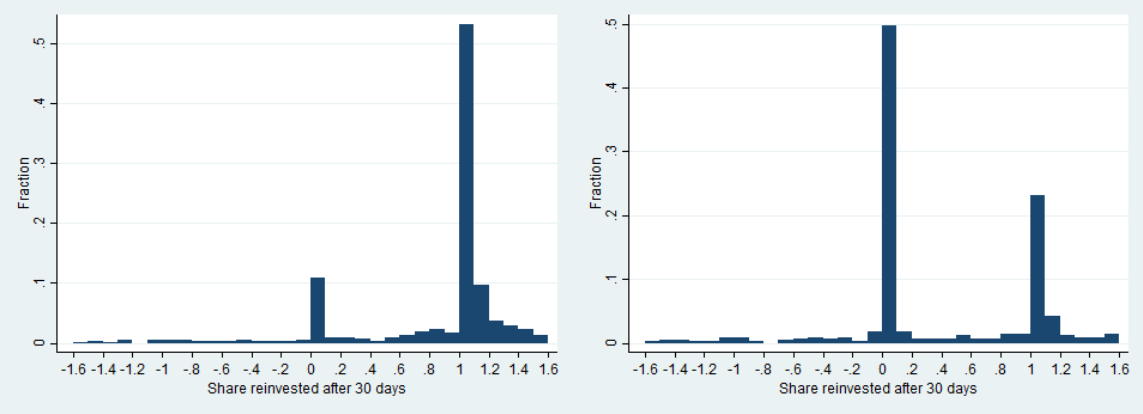

Figure 1: Distribution of the share of funds individuals reinvest after the forced sale event within 30 days in response to either a gain (left side) or a loss (right side) relative to their initial investment: raw data

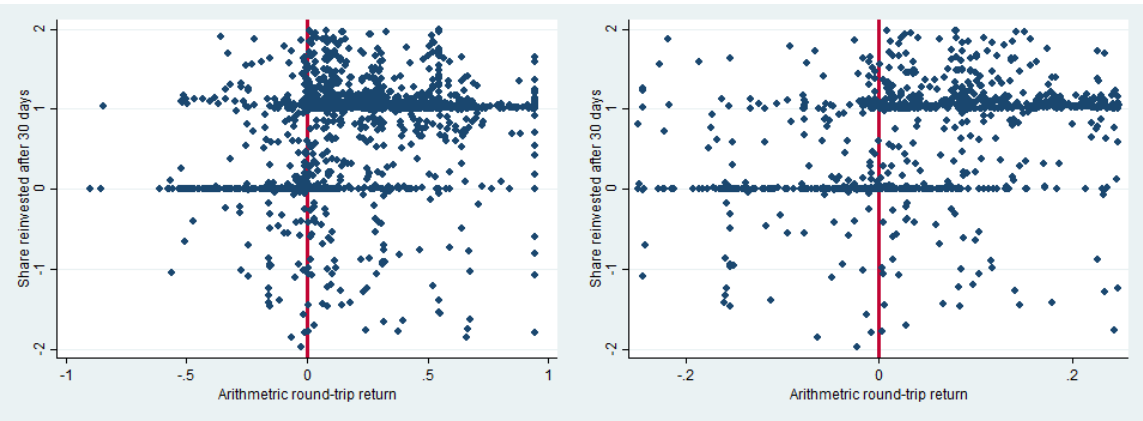

Figure 2: Share of funds individuals reinvest after the forced sale event within 30 days relative to the simple arithmetic return of their individual fund investment: raw data including all observations (left side) or observations with individual fund returns within 25% (right side)

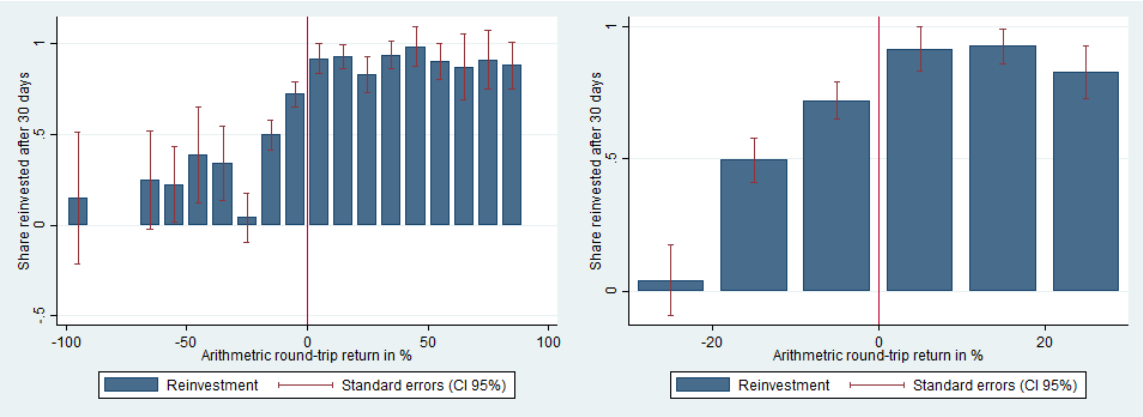

Figure 3: Share of funds individuals reinvest after the forced sale event within 30 days relative to the simple arithmetic return of their individual fund investment: raw data binned averages and standard deviations including all observations (left side) or observations with individual fund returns within 25% (right side)

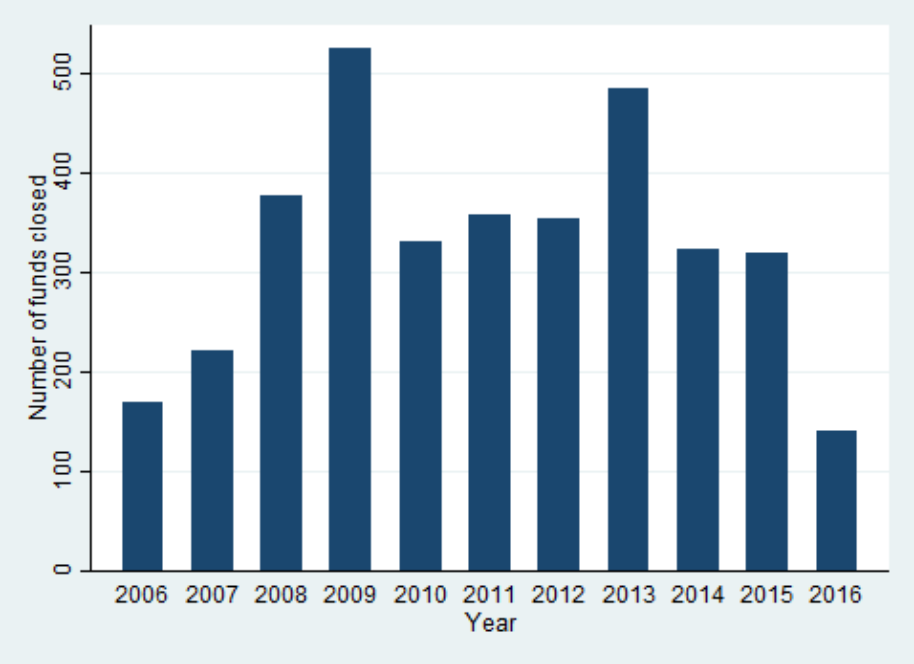

Figure 4: Number of mutual funds closures, as identified by the International Securities Identification Number (ISIN), per year over the period 2006 to 2016: raw data as obtained from the BVI

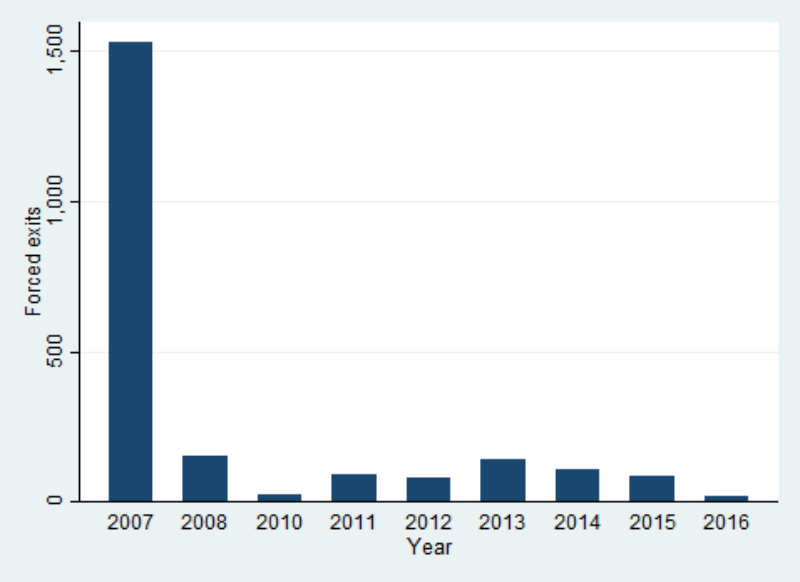

Figure 5: Number of forced sale events, i.e., number of individuals affected by each fund closure, per year over the period 2006 to 2016: raw data

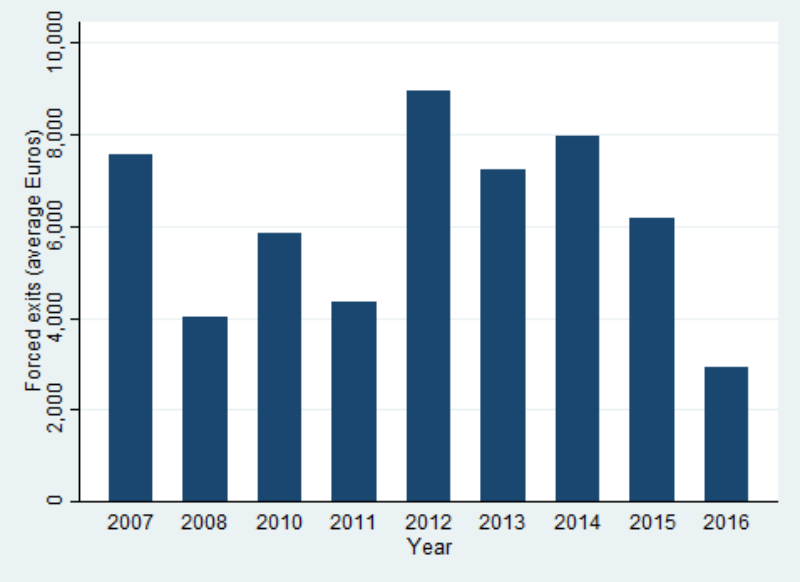

Figure 6: Average amounts of funds received from the forced sale events, per year over the period 2006 to 2016: raw data

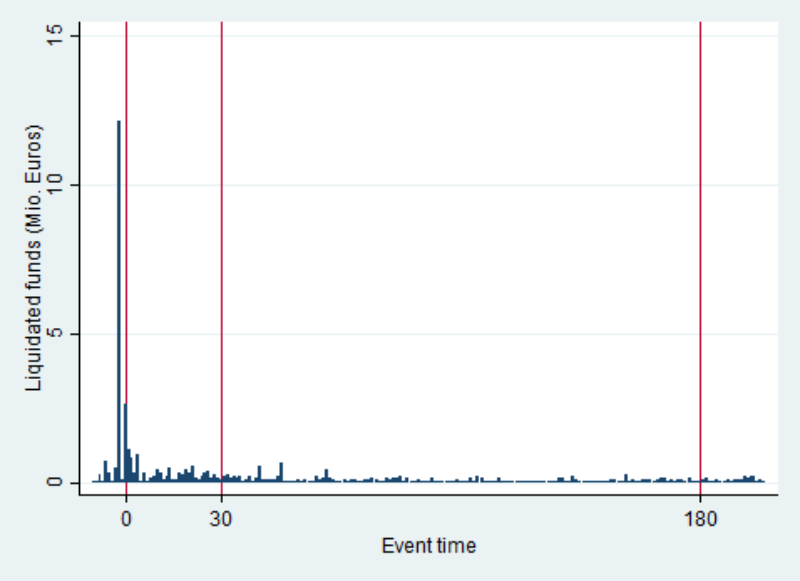

Figure 7: Average daily liquidation or sales activity over the period of 7 months before the fund closures until a few days after: raw data

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 8: Sales receipt as received by mail showing the purchase price (Ausführungswert), the sales price (Kurswert), and the absolute capital loss (Veräußerungsverlust)

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

	mean	median	standard deviation	25th percentile	75th percentile
holding period before closure (in days)	869.14	686.76	618.63	361.00	1,324.25
purchase share price	56.93	46.19	245.29	35.59	56.34
forced selling share price	62.32	50.25	223.98	49.60	60.58
value of forced sell	7,452.21	4,027.21	11,573.78	1,456.00	8,691.14
return of fund investment	0.21	0.13	0.35	0.01	0.38
observations	2,228				

Table 2: Summary statistics for all funds

	mean	median	standard deviation	25th percentile	75th percentile
fund characteristics					
retaining	62.22%				
domicile Luxembourg	61.99%				
domicile Germany	13.70%				
target fund	0.81%				
currency Euro	71.76%				
fund age (in months)	14.47	13.46	6.79	9.59	17.29
costs and fees					
total expense ratio	1.62%	1.72%	0.76%	1.10%	2.03%
initial charge	2.95%	3.50%	2.09%	0.00%	5.00%
annual charge	1.19%	1.25%	0.50%	0.80%	1.50%
asset classes					
alternatives					
bonds	16.80%				
commodity	0.14%				
equity	61.33%				
balanced	20.28%				
money market	0.50%				
other	0.73%				
fund size before deletion dates (mio Euro)					
1 month	871.00	41.20	21,000.00	11.50	152.00
6 months	860.00	41.40	19,800.00	11.50	153.00
12 months	838.00	41.20	17,400.00	11.30	152.00
24 months	807.00	40.30	15,400.00	11.00	150.00
48 months	803.00	41.70	14,100.00	11.20	154.00
fund returns before deletion dates					
6 months	1.60%	4.78%	32.26%	-6.02%	16.91%
12 months	2.82%	4.93%	21.96%	-2.91%	13.97%
24 months	4.11%	4.81%	14.56%	-0.61%	12.07%

Notes: The fund data is obtained from Lipper in Datastream. All statistics are annualized. To make the return statistics in Tables 2 and 3 comparable, for each closed fund, we consider a matched sample of funds by asset class and regional focus.

Table 3: Summary statistics for all closed funds

	mean	median	standard deviation	25th percentile	75th percentile
fund characteristics					
retaining	59.59%				
domicile Luxembourg	57.54%				
domicile Germany	33.90%				
target fund	8.26%				
currency Euro	87.03%				
fund age (in months)	15.40	14.61	6.64	10.50	18.41
costs and fees					
total expense ratio	1.50%	1.40%	0.84%	0.89%	1.92%
initial charge	3.27%	4.00%	1.81%	2.00%	5.00%
annual charge	1.11%	1.10%	0.48%	0.75%	1.50%
asset classes					
alternatives	1.27%				
bonds	19.42%				
commodity	1.03%				
equity	43.18%				
balanced	20.33%				
money market	3.74%				
other	11.04%				
fund size before deletion dates (mio Euro)					
1 month	92.00	18.40	633.00	5.42	57.20
6 months	113.00	22.40	805.00	7.08	65.20
12 months	123.00	26.30	845.00	8.43	73.10
24 months	134.00	31.40	788.00	10.40	84.00
48 months	169.00	35.50	1,130.00	12.20	106.00
fund returns before deletion dates					
6 months	-1.23%	1.73%	30.75%	-6.57%	10.86%
12 months	-0.13%	1.97%	20.11%	-3.95%	8.89%
24 months	1.58%	2.29%	13.55%	-2.05%	7.37%

Notes: The fund data is obtained from Lipper in Datastream. All statistics are annualized. To make the return statistics in Tables 2 and 3 comparable, for each closed fund, we consider a matched sample of funds by asset class and regional focus.

Table 4: Summary statistics for all individuals, all affected individuals, and affected individuals who were ultimately forced to sell

	mean	median	standard deviation	25th percentile	75th percentile
all individuals					
male	0.86	1.00	0.35	1.00	1.00
age	54.36	53.00	13.22	46.00	63.00
PhD educated	0.09	0.00	0.29	0.00	0.00
account tenure (in years)	13.42	11.00	3.50	11.00	18.00
wealth	54,071.87	45,000.00	100,459.70	20,000.00	45,000.00
income	54,111.35	50,000.00	24,834.21	30,000.00	80,000.00
number of purchases	433.26	68.00	2,328.30	20.00	182.00
number of sales	337.11	65.00	1,990.79	24.00	169.00
portfolio value	72,181.24	40,895.82	182,344.30	20,450.42	78,654.24
number of securities	49.29	47.05	28.13	25.98	71.44
HH index	0.13	0.08	0.14	0.03	0.17
observations	113,031				
affected individuals					
male	0.86	1.00	0.35	1.00	1.00
age	53.53	52.00	12.75	45.00	61.00
PhD educated	0.09	0.00	0.28	0.00	0.00
account tenure (in years)	13.39	11.00	3.49	11.00	18.00
wealth	54,198.54	45,000.00	112,619.10	20,000.00	45,000.00
income	54,893.84	50,000.00	24,996.56	30,000.00	80,000.00
number of purchases	564.73	62.00	3,082.28	18.00	168.00
number of sales	498.62	58.00	2,986.00	22.00	158.00
portfolio value	65,846.31	38,095.46	118,288.40	18,801.00	74,595.34
number of securities	52.10	50.50	28.03	29.38	74.17
HH index	0.12	0.07	0.13	0.03	0.16
observations	38,135				
affected individuals forced to sell					
male	0.84	1.00	0.37	1.00	1.00
age	52.55	51.00	11.82	45.00	59.00
PhD educated	0.10	0.00	0.30	0.00	0.00
account tenure (in years)	12.78	11.00	3.35	11.00	13.00
wealth	55,281.76	45,000.00	139,413.00	20,000.00	45,000.00
income	54,733.93	50,000.00	24,020.95	30,000.00	80,000.00
number of purchases	118.64	23.00	1,023.37	6.00	65.00
number of sales	104.56	21.00	1,059.39	9.00	53.00
portfolio value	60,589.54	36,732.08	123,922.30	19,709.69	63,516.90
number of securities	51.34	48.08	28.52	27.09	73.65
HH index	0.10	0.07	0.11	0.02	0.14
observations	2,228				

Notes: Affected individuals hold closed funds at some point over the sample period. Wealth, income, and risk aversion are self-reported statistics in brackets. HH index is the Herfindahl-Hirschman index measure of diversification ranging from 0 to 1.

Table 5: Summary statistics for individuals being forced to sell at a gain or a loss

	mean	median	standard deviation	25th percentile	75th percentile
individuals forced to sell at a gain					
risk class of portfolio	3.41	4.00	3.00	1.40	5.00
risk class of trades	4.29	3.98	3.67	1.71	4.36
average holding period (in days)	790.73	686.15	296.15	585.82	1,290.00
sum of paper and realized winners at all sale dates	2,266.82	416.50	139.50	13,316.89	1,154.00
sum of paper and realized losers at all sale dates	1,915.89	315.50	93.00	8,019.62	1,067.00
average of paper and realized winning funds at all sale dates	12.00	6.00	3.00	16.50	14.00
average of paper and realized losing funds at all sale dates	24.09	12.00	5.00	37.51	27.00
propensity to realize winning funds	0.46	0.43	0.17	0.31	0.74
propensity to realize losing funds	0.36	0.26	0.09	0.30	0.59
observations	1,712				
individuals forced to sell at a loss					
risk class of portfolio	3.22	4.00	1.00	1.58	5.00
risk class of trades	4.33	4.01	3.69	1.65	4.42
average holding period (in days)	1,129.26	1,173.50	618.50	653.10	1,764.00
sum of paper and realized winners at all sale dates	1,932.08	370.00	93.00	5,829.46	1,254.00
sum of paper and realized losers at all sale dates	1,953.12	355.50	108.00	6,245.63	1,228.50
average of paper and realized winning funds at all sale dates	11.86	7.00	3.00	15.85	14.00
average of paper and realized losing funds at all sale dates	14.49	8.00	3.00	19.52	19.00
propensity to realize winning funds	0.22	0.13	0.04	0.24	0.30
propensity to realize losing funds	0.24	0.16	0.08	0.24	0.32
observations	516				

Notes: Risk class of portfolio (the risk classification is established by German regulation going from 1 (for instance, savings accounts) to 5 (for instance, stocks, options, and futures)) refers to the overall portfolio and risk class of trades to the traded securities and is value weighted. The propensity to realize winning and losing funds is calculated as in Odean (1998).

Table 6: Estimation results from forced liquidations of fund closures after 30 days (interacted with losses)

	net buys in portfolio			
liquidation	0.7030***	0.7308***	0.8084***	0.8084***
	(0.0733)	(0.0686)	(0.0303)	(0.0918)
month-by-year fixed effects		✓	✓	✓
ISIN fixed effects			✓	✓
month-by-year clustering				✓
observations	2,222	2,222	2,222	2,137
R-squared	0.1847	0.2975	0.3943	0.3489

	net buys in portfolio			
liquidation	0.8325***	0.8359***	0.8855***	0.8855***
	(0.0604)	(0.0587)	(0.0327)	(0.0136)
liquidation*loss	-0.5362***	-0.4485***	-0.4137***	-0.4137*
	(0.1403)	(0.1493)	(0.0691)	(0.2234)
month-by-year fixed effects		✓	✓	✓
ISIN fixed effects			✓	✓
month-by-year clustering				✓
observations	2,222	2,222	2,222	2,137
R-squared	0.2140	0.3135	0.4048	0.3602

Notes: Standard errors in parentheses. Entries in the table represent the coefficient β of regression specification 1, i.e., the € amount of net buys in the portfolio regressed on the € amount of the liquidiation at the individual level. The controls month-by-year and ISIN fixed effects indicate dummies for each month over the entire sample period and each closed fund. Standard errors are computed using the robust White method or clustered at the month-by-year level. The column with clustered standard errors drops all singleton observations to be conservative.
Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Estimation results from forced liquidations as well as deliberate liquidations pre and post announcements of fund closures after 30 days (interacted with losses)

	net buys in portfolio			
forced liquidation	0.7030***	0.6985***	0.7562***	0.7562***
	(0.0732)	(0.0807)	(0.1124)	(0.1351)
post announcement liquidation	-0.8245***	-0.8698***	-0.8783***	-0.8783***
	(0.1912)	(0.1897)	(0.1765)	(0.2400)
pre announcement liquidation	-0.5289***	-0.5010***	-0.6361***	-0.6361***
	(0.1337)	(0.1333)	(0.1183)	(0.1986)
month-by-year fixed effects		✓	✓	✓
ISIN fixed effects			✓	✓
month-by-year clustering				✓
observations	38,042	38,042	38,042	37,758
R-squared	0.0025	0.0155	0.0652	0.0494

	net buys in portfolio			
forced liquidation	0.8619***	0.8489***	0.8873***	0.8873***
	(0.0535)	(0.0571)	(0.1174)	(0.0465)
post announcement liquidation	-0.8378***	-0.8225***	-0.9196***	-0.9196***
	(0.2415)	(0.2413)	(0.1981)	(0.2982)
pre announcement liquidation	-0.7975***	-0.7631***	-0.8566***	-0.8566***
	(0.1231)	(0.1235)	(0.1261)	(0.1062)
forced liquidation*loss	-0.5791***	-0.5888***	-0.6124***	-0.6124***
	(0.1121)	(0.1466)	(0.2320)	(0.2201)
post announcement liquidation*loss	-0.0255	-0.1138	0.0315	0.0315
	(0.2615)	(0.2891)	(0.2271)	(0.3261)
pre announcement liquidation*loss	-0.2717	-0.2914	-0.3219***	-0.3219
	(0.1808)	(0.1840)	(0.0633)	(0.2324)
month-by-year fixed effects		✓	✓	✓
ISIN fixed effects			✓	✓
month-by-year clustering				✓
observations	38,042	38,042	38,042	37,758
R-squared	0.0035	0.0150	0.0516	0.0485

Notes: Standard errors in parentheses. Entries in the table represent the coefficient β of regression specification 2, i.e., the € amount of net buys in the portfolio regressed on the € amount of the liquidiation at the individual level. The controls month-by-year and ISIN fixed effects indicate dummies for each month over the entire sample period and each closed fund. Standard errors are computed using the robust White method or clustered at the month-by-year level. The column with clustered standard errors drops all singleton observations to be conservative.
Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Estimation results from forced liquidations as well as deliberate liquidations pre and post announcements of fund closures after 30 days interacted with losses

	net buys times riskclass	reinvestment into funds	no reinvestment	participation
forced liquidation times riskclass	0.8034***			
	(0.0586)			
post announcement liquidation times riskclass	-0.4668***			
	(0.1181)			
pre announcement liquidation times riskclass	0.0390			
	(0.0964)			
forced liquidation times riskclass*loss	-0.5223**			
	(0.2157)			
post announcement liquidation times riskclass*loss	0.0233			
	(0.1498)			
pre announcement liquidation times riskclass*loss	-0.2036**			
	(0.0992)			
forced liquidation		0.4024***	0.1074***	0.7845***
		(0.0396)	(0.0280)	(0.0820)
post announcement liquidation		-0.0342	0.0021	-0.2656***
		(0.0281)	(0.0264)	(0.1025)
pre announcement liquidation		0.0269	-0.0610***	-0.1894**
		(0.0210)	(0.0194)	(0.0833)
forced liquidation*loss		-0.2578***	0.2490***	-0.0362***
		(0.0304)	(0.0301)	(0.0127)
post announcement liquidation*loss		-0.0772***	0.0582**	-0.0065
		(0.0271)	(0.0257)	(0.0140)
pre announcement liquidation*loss		-0.0486***	0.0272***	-0.0113***
		(0.0062)	(0.0054)	(0.0038)
month-by-year fixed effects	✓	✓	✓	✓
ISIN fixed effects	✓	✓	✓	✓
observations	37,982	38,042	38,042	38,042
R-squared	0.2342	0.1212	0.1159	0.2102

Notes: Standard errors in parentheses. Entries in the table represent the coefficient β of regression specification 2 with alternative outcome variables, i.e., the flows into investments multiplied by their risk class, a dummy for reinvesting into funds, a dummy for not reinvesting at all, or a dummy for not being invested in equity after 6 months. The controls month-by-year and ISIN fixed effects indicate dummies for each month over the entire sample period and each closed fund. Standard errors are computed using the robust White method.
Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

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

	balance increase in settlement account	net transfers into savings account	net transfers out minus into the bank
forced liquidation	0.0409	0.0650	-0.0685
	(0.0379)	(0.0611)	(0.0440)
post announcement liquidation	0.9640**	-0.2788	0.3115**
	(0.4414)	(0.3461)	(0.1381)
pre announcement liquidation	0.0571	0.1733*	0.2587***
	(0.0810)	(0.0988)	(0.0693)
forced liquidation*loss	0.5652***	0.0858	0.1092
	(0.1937)	(0.1203)	(0.0806)
post announcement liquidation*loss	-0.7333*	0.4797	0.1358
	(0.4188)	(0.3539)	(0.1914)
pre announcement liquidation*loss	-0.1021	0.2309**	0.4042***
	(0.1042)	(0.1046)	(0.1179)
month-by-year fixed effects	✓	✓	✓
ISIN fixed effects	✓	✓	✓
observations	38,042	38,042	38,042
R-squared	0.0600	0.0823	0.1215

Notes: Standard errors in parentheses. Entries in the table represent the coefficient β of regression specification 2 with alternative outcome variables, i.e., the € amount increase in the balance of the settlement account, the net transfers into the savings account, and the net transfers out of the bank. The controls month-by-year and ISIN fixed effects indicate dummies for each month over the entire sample period and each closed fund. Standard errors are computed using the robust White method. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Robustness estimation results from forced liquidations of fund closures after 30 days

	net buys into portfolio					
liquidation	0.8927***	0.8834***	0.8833***	0.8847***	0.8848***	
	(0.0642)	(0.0642)	(0.0643)	(0.0643)	(0.0643)	
liquidation*loss	-0.6809***	-0.6361***	-0.6372***	-0.6380***	-0.6381***	
	(0.2118)	(0.2111)	(0.2111)	(0.2113)	(0.2113)	
announcement						0.9307***
						(0.0679)
announcement*loss						-0.6824***
						(0.2126)
dummy for investment loss	✓					
fund return over investment		✓				
portfolio return over fund investment			✓			
three months portfolio return				✓		
twelve months portfolio return					✓	
month-by-year fixed effects	✓	✓	✓	✓	✓	✓
ISIN fixed effects	✓	✓	✓	✓	✓	✓
observations	38,135	38,125	37,975	37,994	37,994	31,446
R-squared	0.0693	0.0692	0.0691	0.0693	0.0693	0.0713

Notes: Standard errors in parentheses. Entries in the table represent the coefficient β of regression specification 2, i.e., the € amount of net buys in the portfolio regressed on the € amount of the liquidiation at the individual level with different control variables, a dummy for holding a losing fund, the return of the fund investment, the 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 regressor instead of the value of the liquidation. The controls month-by-year and ISIN fixed effects indicate dummies for each month over the entire sample period and each closed fund. Standard errors are computed using the robust White method.
Significance levels are *** p<0.01, ** p<0.05, * p<0.1.