Household Responses to Phantom Riches

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Abstract

We study household responses to "phantom riches"—the illusion of attaining substantial wealth—by using administrative data on Ponzi scheme investors. An event-study design exploiting staggered entry shows investors experience a 6 percent labor income loss. Income first declines when an investor joins the scheme, consistent with distorted beliefs lowering labor supply. The scheme's collapse evokes a further decrease, which we attribute to financial stress caused by the collapse. Investors also face higher unemployment and indebtedness and shy away from delegated investments. The long-run income loss twice exceeds the direct investment loss and substantially adds to the social cost of fraud.

Keywords: Investment fraud, distorted beliefs, financial stress, consumer financial protection, Ponzi scheme

JEL Classification: G11, G40, G51, J22

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1 Introduction

Phantom riches—the illusion of attaining substantial wealth through investments—has long been recognized as a common belief occurring in asset price bubbles (Kindleberger, 1978; Shiller, 2000). An obvious consequence of pursuing such riches is the eventual redistribution of wealth when prices crash. But it may also lead to other, possibly more important adverse consequences if distorted investment beliefs influence household decision-making outside the financial realm.

We study such effects of distorted investment beliefs by performing, to the best of our knowledge, the first systematic analysis of household responses to investment fraud. Fraudulent investment schemes resemble bubbles and crashes as they entice investors with prospects of high returns, only to reveal their true nature when they eventually collapse. The key advantage in studying fraud is that most economists agree the returns promised by investment scams are unattainable. Alleged mispricings in the legitimate investment domain do not necessarily garner such consensus.[1] Furthermore, investment fraud on its own is economically important. In 2022, households lost more than $20 billion in investment scams.[2] Yet, we know virtually nothing about fraud's impact on households—a research gap in stark contrast to the vast literature on legitimate investments.[3]

We combine administrative data on Ponzi scheme investors with an event-study design exploiting staggered entry into the scheme. We find investors initially respond to their perceived wealth gain from entering the scheme by decreasing their labor income. However, when the scheme collapses, the same investors do not readjust to the wealth loss by increasing income. In fact, income further declines. This behavior is inconsistent with the standard life-cycle model in which wealth shocks inversely and symmetrically affect labor supply. An augmented model in which financial stress emanating from the scheme's collapse adversely affects earning potential can deliver the key patterns in the data. The average income loss equals 6 percent of investors' annual income and its lifetime value twice exceeds the direct investment loss. The income loss thus surpasses the economic importance of the scheme's direct wealth redistribution.

[1] See, e.g., Fama's Nobel Lecture (Fama, 2014) for a skeptical view on the existence of bubbles and Greenwood, Shleifer, and You (2019) for empirical evidence.

[2] See Appendix Section A and Table IA.1 for our quantification of the size of the market and its participation rate. In what follows, we use the terms investment scams and investment fraud interchangeably to refer to fraudulent investment schemes, as distinct from other forms of securities fraud.

[3] The finance literature has a long tradition of quantifying the costs of financial mistakes in legitimate markets (see, e.g., Barber and Odean, 2000, 2001). More recent work has made significant progress in documenting the prevalence of financial misconduct, for example, among financial advisors (Egan, Matvos, and Seru, 2019).

Our data come from the largest investment scam and the most extensive police investigation in Finnish history: the "Wincapita" Ponzi scheme. The scheme was active in 2003–2008, advertised annual returns of several hundred percent, and attracted over 100 million euros in investments. During the investigation that ensued after its collapse, the police identified and interviewed over 3,000 investors. We merge these investors' identities extracted from the police interview transcripts by Rantala (2019) with longitudinal register-based data held by Finnish tax authorities and other government agencies. These data cover nearly three decades and a range of socio-economic and financial variables, the time of joining the scheme, and invested amounts.

While the combined dataset is the first of its kind, it comes with the caveat of covering a single investment scam. We find that compared to the adult population, the scheme investors are younger and have higher incomes, financial wealth, education, and cognitive ability. Males and entrepreneurs are overrepresented in our sample. The sample also appears more financially sophisticated: being more likely to hold legitimate financial assets and have a mortgage. Relative to the population, one would thus expect our sample to be more resilient to the financial shocks caused by the scheme.[4]

The unique nature of the spread of the scheme gives us a source of plausibly exogenous variation in the timing of entry. The scheme did not operate in the public domain and investors could only join by personal invitation from existing members. At the peak of the scheme, these potential sponsors represented less than 0.2 percent of the Finnish population. We find the year of entry of an investor strongly correlates with that of the sponsor. This correlation suggests the plausibly exogenous timing of the invitation to join the scheme drives the investor's timing of entry. This variation aids our empirical design in two ways. First, it allows us to instrument for the wealth loss at collapse with the lag between entry and collapse. Second, it helps us tighten the identification of the income response to joining the scheme by comparing—in the same calendar year—investors with early and late invitations.

We start our analyses with an estimator that covers responses to both entry and collapse. We first use tight non-parametric matching to find for each participant a subset of the population that is observably similar one year before joining. We then use this control group to estimate the income

[4] Comparison of our sample to the broad population of fraud investors is not feasible as representative data are not available. Higher participation of males is consistent with survey evidence in FINRA (2013). FBI (2022) reports that the age group between 30–49 is most frequent among cryptocurrency investment scam victims, whereas FINRA (2013) finds that Americans 65 years and older are more likely to fall prey to financial fraud. In Appendix Section B we show that the scheme's characteristics, such as size and length of activity, are not unusual in international comparison.

responses in an event-study difference-in-differences design that exploits the staggered entry to the scheme. This design eliminates time-invariant individual income differences while controlling for time, age, and observable participant heterogeneity by using the matched subsample of the population as a control group. As is standard in the difference-in-differences design, we support the main identifying assumption of parallel trends by assessing pre-trends. We find no evidence of pre-trends in the eight outcome variables we consider.

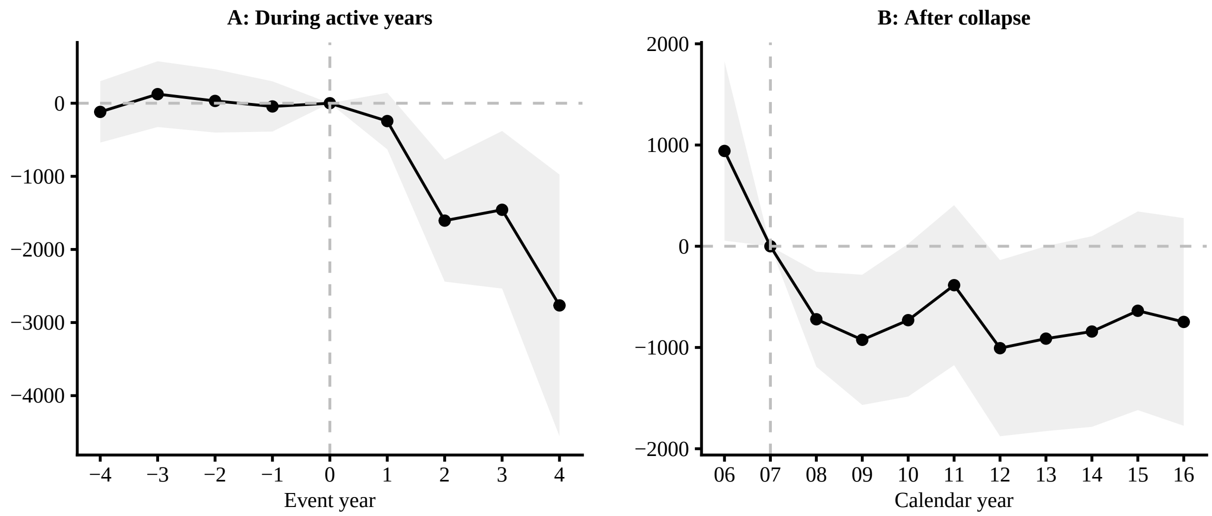

Figure 1. Labor income response to entry into the scheme and its collapse

The figures plot event-study difference-in-differences estimates of annual labor income response in euros. Panel A plots the response (δ_n) around the year of entry from regression (1), stacked by year of entry, in which the baseline year (0) is one year prior to joining the scheme. The post-entry period only covers the active years of the scheme's operation. Panel B plots the response around the year of collapse from a version of regression (2), stacked by calendar year, in which the baseline year is the last year before collapse (07). The pre-collapse period only covers active cohorts. The figures cover the first five cohorts of the scheme investors and their matched control group described in Section 3.2. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

Figure 1 summarizes our main finding. Panel A plots the income response around the year of entry and over the active operation of the scheme. The event-study DiD estimator is stacked by year of entry, with the year prior to joining being the baseline year. As each entering cohort experienced a different length of participation in the scheme, each post-entry coefficient covers only those cohorts that remained active. Panel B plots the income response around the year of collapse. Here, the event-study DiD estimator is stacked by calendar years, with the last year prior to the collapse being the baseline year.

The two panels in Figure 1 show a remarkable asymmetry between the income response to the wealth gain from entering the scheme and the income response to the wealth loss from the collapse.

Consistent with the evidence from lottery winnings (Cesarini, Lindqvist, Notowidigdo, and Östling, 2017; Golosov, Graber, Mogstad, and Novgorodsky, 2023) and predictions of the standard life-cycle model, investors reduce labor income immediately after joining the scheme. The gradual increase in the income response over the scheme's active years matches its cumulative wealth gains. In contrast, the same investors do not increase their labor income after the scheme's collapse. Instead, their labor income further declines in the year of collapse and remains persistently lower over the next eight years we observe.

To judge the economic magnitude of the response to entry, we impute the gains perceived by investors from the data on invested and withdrawn amounts (the scheme perpetrator destroyed all data on account values). For example, two years after joining the scheme, the median (average) imputed wealth gain is 61,000 (161,000) euros. Figure 1, Panel A, implies that investors lower their labor income by 1,458 euros (approximately 6 percent) over the same horizon. This response aligns well with the income responses to wealth changes previously documented for lottery winners. Specifically, scaling our estimate of income response with the labor income effects of lottery winnings (Cesarini et al., 2017; Golosov et al., 2023), our investors behave as if they received a windfall gain of 63,391–132,545 euros.

To interpret the further decline in labor income following the scheme's collapse, we discuss our findings in light of the theories commonly used to explain income responses to wealth shocks. The explanations most consistent with our results involve financial stress, whereby concerns about money or the psychological costs of financial constraints reduce earnings potential (Mani, Mullainathan, Shafir, and Zhao, 2013; Kaur, Mullainathan, Oh, and Schilbach, 2021; Fink, Jack, and Masiye, 2020; Banerjee, Karlan, Trachtman, and Udry, 2020). The negative income response to wealth losses is consistent with the model of Sergeyev, Lian, and Gorodnichenko (2022), in which financial stress can reverse the otherwise negative wealth effect on labor supply. This explanation is also consistent with the evidence from police interviews: we document many participants mention adverse effects of financial stress and psychological hardship after the scheme's collapse, including declarations of depressive and suicidal thoughts.

To shed more light on the ability of the financial stress model to explain our results, we test an additional prediction: the income loss at collapse should be proportional to the distance to financial constraints proxied by the wealth loss at collapse. Simple OLS estimates reveal the income loss in the year the scheme collapses increases with the wealth losses. However, these regressions may suffer from an endogeneity problem as the size of investments may be related to income and, in turn, to the income response. To address this concern, we develop an instrumental variable approach that exploits the variation in wealth losses at collapse generated by the variation in the length of participation in the scheme. We find that an additional year in the scheme, corresponding to an additional loss of about 25,000 euros, increases the income loss by a statistically significant amount of 275 euros.

The magnitude of the total income loss is substantial. Over the nine post-collapse years we observe, the average labor income loss represents nearly 6 percent of investors' pre-entry annual labor income or 1,376 euros. Cumulated over the mean remaining length of an individual's working life, the income loss equals 24,080 euros or 190 percent of the mean net investment amount in our sample (12,700 euros) and, therefore, nearly twice exceeds the direct financial losses from the scheme. These estimates indicate that the indirect costs of investment fraud can substantially add to its social cost.

We complement the analysis of labor income effects by quantifying the responses of other labor and financial outcomes. Consistent with the income decline after entry being caused by a voluntary decrease in labor supply, we find that entry into the scheme leads to increased labor force exit. Consistent with the decline after the collapse being driven by financial stress, we do not observe additional exits but instead an increase in the fraction of investors receiving unemployment and sickness benefits in the post-collapse years. Our analyses of leverage reveal participation in the scheme leads to a significant and persistent increase in the probability of having consumer loans and mortgages. This effect is partly driven by increased take-up of consumer loans in the year of entering the scheme, consistent with investors using debt to fund their investments.

Finally, fraud participation influences participation in legitimate risky investments. Investors' participation in equity markets significantly declines over the active years of the scheme both through direct stock holdings and equity mutual funds, consistent with substitution effects with the Ponzi investments. After the scheme collapses, stock market participation recovers, but only through direct equity holdings. Participation through mutual funds remains significantly and persistently lower after the scheme's collapse. This evidence is consistent with negative experiences driving mistrust in financial institutions (Zingales, 2015).

Although the event-study difference-in-differences estimator controls for age and time effects and observable differences of investors, the key remaining threat to identification comes from the possibility that unobservable factors drive our results. For example, investors experiencing worse job market prospects may be more likely to enter the scheme, which could generate differential income trends even in the absence of the scheme. To assess this threat, we turn to a triple-difference estimator that compares the income response of investors who joined the scheme early to those who joined late. In a further refinement, we use a triple-difference estimator that compares investors who enter the scheme after an early or late invitation proxied by their sponsor's timing of entry or the scheme's arrival in their zip code. The timing of receiving the invitation is arguably outside the control of investors and therefore those who received the invitation later serve as a natural control group to early joiners. These estimators yield remarkably similar results to our baseline estimator, which supports the validity of our empirical approach and suggests the tightly matched subset of the population is a suitable control group.

Several robustness analyses and alternative estimation strategies provide further comfort about our empirical strategy. First, we employ a series of placebo tests that replace the year of joining with placebo years of joining between 1993–99. Consistent with our main results being driven by scheme participation instead of differences in job market prospects, we find no placebo effects. Second, we also assess the threat to identification posed by the Great Recession. Our effects arise in 2003–2008, whereas the Great Recession arrived in Finland only in 2009 and thus cannot drive our results. Finally, we show the coefficients are stable across different matching specifications that add additional matching variables.

Related literature. Our paper contributes to the literature on the economics and social loss from crime going back to Becker (1968). Recent work focuses on the prevalence of misconduct and fraud among financial advisors (Egan et al., 2019; Parsons, Sulaeman, and Titman, 2018; Dimmock, Gerken, and Van Alfen, 2021), corporations (Dyck, Morse, and Zingales, 2023), financial intermediaries (Piskorski, Seru, and Witkin, 2015; Griffin and Maturana, 2016; Célérier and Tak, 2023), and cryptocurrencies (Griffin and Shams, 2020). Many studies in corporate finance analyze the consequences, determinants, and detection channels of corporate fraud.[5] However, household finance research on financial fraud is less developed. The few studies that focus on households exploit aggregate data and geographic variation in fraud. Giannetti and Wang (2016) examine the impact of fraud on stock market participation whereas Gurun, Stoffman, and Yonker (2018) studies its effect on trust in financial intermediaries.

By quantifying the indirect cost of investment fraud, our paper connects to the literature on the cost of financial mistakes (Barber and Odean, 2000; Grinblatt and Keloharju, 2000; Calvet, Campbell, and Sodini, 2007; Heimer and Simsek, 2019; Vokata, 2021) and to the discussion about optimal regulatory design and consumer financial protection (Campbell, 2006; Campbell, Jackson, Madrian, and Tufano, 2011). Our findings suggest the social loss from investment fraud, a key input to optimal regulatory design, may be substantial. The results on the characteristics of victims echo the existing evidence on widespread financial illiteracy (e.g., Lusardi and Mitchell, 2007)[6] and suggest that large-scale policies improving market transparency, as opposed to policies targeted at the least sophisticated consumers, may be more effective.[7] Our findings also connect to the literature evaluating the effectiveness of consumer financial protection and regulatory design (Agarwal, Chomsisengphet, Mahoney, and Stroebel, 2015; Charoenwong, Kwan, and Umar, 2019; Egan, 2019).

More broadly, our results connect to the literature on the role of beliefs and belief distortions in finance. Recent studies document an important role of beliefs and heterogenous expected returns in financial decisions (Giglio, Maggiori, Stroebel, and Utkus, 2021; Meeuwis, Parker, Schoar, and Simester, 2022; Weber, Candia, Coibion, and Gorodnichenko, 2023). A growing body of work presents evidence of systematic biases in financial beliefs (Greenwood and Shleifer, 2014; Bordalo, Gennaioli, Ma, and Shleifer, 2020) and links belief distortions to stock market puzzles (Barberis, Shleifer, and Vishny, 1998; Rabin, 2002; Bordalo, Gennaioli, La Porta, and Shleifer, 2022), bubbles (Scheinkman and Xiong, 2003; Barberis, Greenwood, Jin, and Shleifer, 2018; Bordalo, Gennaioli,

[5]Karpoff and Lott (1993); Fich and Shivdasani (2007); Karpoff, Lee, and Martin (2008) study the consequences to offenders. Povel, Singh, and Winton (2007); Wang, Winton, and Yu (2010); Khanna, Kim, and Lu (2015); Liu (2016) focus on the determinants and Dyck, Morse, and Zingales (2010) analyze the detection channels.

[6]Investor sophistication has also been shown to be an important determinant of behavior in financial bubbles and crashes (Brunnermeier and Nagel, 2004; Greenwood and Nagel, 2009; Griffin, Harris, Shu, and Topaloglu, 2011; Grinblatt, Keloharju, and Linnainmaa, 2012; Liao, Peng, and Zhu, 2022; An, Lou, and Shi, 2022; Liu, Makarov, and Schoar, 2023).

[7]See Barr and Diamond (2020) for an example of a government-financed guarantee to cover cases of fraud in the Swedish pension system.

Kwon, and Shleifer, 2021), and financial fragility (Gennaioli and Shleifer, 2018). We complement this line of work by showing that even a relatively short-lived episode resulting in distorted beliefs can lead to persistent welfare losses for households.

Finally, we add to the emerging literature on financial stress (Sergeyev et al., 2022). Evidence consistent with the adverse effects of financial stress has been documented both in developed (Bernstein, McQuade, and Townsend, 2021; Maturana and Nickerson, 2020; Engleberg and Parsons, 2016; Lin and Pursiainen, 2023) and developing (Mullainathan and Shafir, 2013; Kaur et al., 2021) countries. Our setting is unique as we observe a sequence of both positive and negative wealth shocks to the same investors. Our results reveal the negative labor response to both positive (found in lottery studies, e.g., Cesarini et al., 2017) and negative (found in the literature on financial distress, e.g., Bernstein et al., 2021) wealth shocks can take place for the same individual over time.

The rest of the paper unfolds as follows. Section 2 describes our data sources and research setting. Section 3 presents the empirical methodology and the labor income responses to the entry and collapse of the scheme. Section 4 discusses the effects on other labor and financial outcomes, and Section 5 concludes.

2 Data and background

2.1 Data

Our data originate from six sources. The individual-level data include a scrambled personal identification number that enables us to merge data from different databases.

National Bureau of Investigation (NBI). Information on the Ponzi scheme investors originates from the publicly available pre-trial protocol of the court case against the perpetrator of the scheme. This official collection of various documents (case number 2400/R/81/10) summarizes the police investigation and includes interview and interrogation transcripts and copies of relevant evidence material, such as bank statements, investigation reports, and e-mails. The materials comprise more than 53,000 pages, the majority of which are related to the victims' interviews. Rantala (2019) uses these documents to hand-collect information on victims' characteristics and behavior.

Statistics Finland (SF). The bulk of our data comes from SF, which matches the NBI data with the entire Finnish population in 1988–2016. This process involves SF using the identifying information on the victims in the NBI data (name, personal identification number or date of birth if the number is missing, and address) to match them with the subjects in the population data. SF also performs the merger of the data and hosts them on a remotely accessible server. The resulting data set is anonymized and does not include identifying information of the individuals. The variables cover the individual's employment status, annual income, benefit payments, consumer loans and mortgages (coverage starts in 2002), field and level of education, year of birth, gender, and marital status. The data set also covers identifiers of the individual's firm of employment and zip code of residence, which we use to classify workers into seven industries and five regions.

Finnish Tax Administration (FTA). We supplement the core panel at SF with data on the financial security holdings of each member of the population in 2004–2016. Ownership of mutual funds originates from asset-management firms that directly report to FTA. At the end of each year, these records indicate the mutual funds in which an individual has invested and the market value of each holding. FTA further receives information on stock holdings directly from Euroclear Finland, Finland's national central securities depository. These data detail the end-of-year holdings in each publicly listed stock on the Helsinki Stock Exchange (part of the NASDAQ group). Registering transactions with Euroclear Finland is mandatory and automatic for household investors, so these data represent a comprehensive and reliable account of directly held shareholdings.

Finnish Defense Forces (FDF). FDF provides the individual's cognitive ability test scores. Males in Finland must take a personality test when entering mandatory military service at the age of 19 or 20. Part of the test is a 120-question intelligence test for which we have comprehensive data beginning from January 1, 1982. FDF constructs a composite ability score from the results in three areas: mathematical ability, verbal ability, and logical reasoning. To account for the Flynn effect, the upward trend in IQ scores (Flynn, 1984), and to ease interpretation, we standardize the ability scores by enlistment year, so they have a mean of zero and a standard deviation of one. See Grinblatt, Keloharju, and Linnainmaa (2011) for an extensive description of the test procedure.

Mutual Fund Report, an industry publication compiled by Investment Research Finland, includes a monthly account of characteristics and returns on all mutual funds available to Finnish investors. We use these data for classifying mutual funds into asset classes. Grinblatt, Ikäheimo, Keloharju, and Knüpfer (2016) and Knüpfer, Rantapuska, and Sarvimäki (2022) discuss the details of these data.

Helsinki Stock Exchange reports the daily closing prices for all stocks traded on the exchange, the dividends paid to each stock, and any events that influence the nominal share price. We use these data to calculate the euro values of an individual's stock holdings.

2.2 Key features of Wincapita

Appendix Figure IA.1 shows a timeline of the events during the scheme's operations and after its collapse. The first investors joined the scheme in September 2003. Initially, it alleged a sports betting system as the source of its revenue. In 2005, it announced a shift into currency trading, claiming to make profits with a trading system based on the EUR/USD exchange rate. The police investigation that ensued after the scheme's collapse revealed the entire scheme was fictitious and no trades or profits were ever made. The fictitious profits the scheme reported to its investors remained similar throughout the scheme and were several hundred percent over a six-month investment period. An investment in the scheme was subject to a six-month lock-up period, after which the funds could be reinvested or withdrawn. The minimum required investment increased gradually over the years; in the end, it was 3,000 euros. Investors could join only by personal invitation from an existing member, who was referred to as a sponsor and who received compensation for each new member.

Table 1, Panel A, shows the dramatic growth of the scheme: about two-thirds of the participants joined during the last two years. The police reported the scheme had over 10,000 investors who had altogether invested over 100 million euros. The number of investors corresponds to 0.2 percent of the Finnish population. The first public coverage of the scheme took place in September 2007 when an investigative TV journalist reported on the investment operation potentially functioning as a Ponzi scheme.

In March 2008, the sole perpetrator fled from Finland, took down the scheme's website, and destroyed all its records. After evading the police for over eight months, he was arrested in Sweden in December 2008. The police froze the scheme's bank account shortly after the perpetrator disappeared. The collapse of the scheme triggered one of the largest police investigations in Finnish history, and the perpetrator was convicted of aggravated fraud by the Vantaa District Court in 2011. In 2014, he was released on parole.

After the scheme collapsed, the investors whose withdrawals exceeded their investments had to pay the difference to the State; that is, no investor benefited financially from the scheme. The Finnish Criminal Code mandates any financial gain resulting from criminal activity has to be returned even when the person receiving the gain has not committed a crime herself. This provision also means an investor who recognized Wincapita as a Ponzi scheme, and assumed the scheme will be detected and successfully prosecuted, could not intend to benefit from the scheme by strategically joining with the intention of withdrawing profits before the collapse. Appendix Section B provides more details of the scheme's operations and discusses the representativeness of its characteristics in international comparison.

2.3 Our sample of investors

Our sample consists of 3,093 investors who were personally interviewed by the police. The police records indicate 57 percent of the investors contacted the police on their own initiative, and 43 percent were contacted by the investigators. Police did not specify why they contacted specific individuals, but the reasons include large investments or withdrawals, an active role within the scheme, and verifications related to other investors' statements (Rantala, 2019). Table 1, Panel B, reports the total investment amounts and losses. The median of the total amount invested during the scheme is 8,000 euros and the mean is 15,379 euros. About 25 percent of the investors withdrew some funds from the scheme during its operation.

Table 1, Panel C, reports the wealth losses investors experienced when the scheme collapsed. Data on investors' account values do not exist because the scheme's sole perpetrator destroyed all records. However, we can impute the approximate account values using the information collected by police on invested amounts, withdrawn amounts, and timing of investments (see Appendix Section C for the details of this imputation procedure). We observe dramatic variation in the experienced losses across cohorts: the median loss for the first two cohorts is more than 100,000 euros, whereas the medians for the last two cohorts are less than 22,000 euros. This variation is largely driven by a higher accumulation of gains over longer period of participation.

Table 2 reports a set of key characteristics for our sample of investors. All characteristics are measured in 2002, the year before the first investors joined the scheme. The exception are variables on financial assets, which are measured in 2004, the first year of their coverage. We compare participants to the adult population (age 18 or older in 2002) and to adult investors in legitimate financial assets.

Somewhat surprisingly, we find that on many variables correlated with financial literacy (Lusardi and Mitchell, 2014), our investors appear more literate than the population. Panels A and B show the investors earn higher income, have higher levels of education, and have more experience in equity and credit markets. Investors' mean taxable income is 29,430 euros, which is 60 percent higher than the population average. Gender and entrepreneurship display the largest differences, with 82 percent and 20 percent of investors being males and entrepreneurs, respectively. The entrepreneurship rate among the population is only 6 percent. The investors are six years younger and less likely to be retirees than the population, implying our scheme did not exclusively prey on the elderly who are believed to be more vulnerable (DeLiema, Deevy, Lusardi, and Mitchell, 2020).

Finally, Panel C reports the investors' cognitive ability score, measured in standard deviations. Again, contrary to a common presumption, we find that fraud participants do not have significantly lower cognitive ability scores compared to the population. This observation is further echoed by the police reports detailing a number of high-profile individuals who participated in the scheme, such as Finland's former chief of defense, a CFO of a publicly listed company, university professors, medical doctors, dentists, professional retail investment advisors, and retired professional athletes.

The picture looks very different when we compare the scheme investors with investors in mainstream financial assets. The scheme investors earn comparable income to mainstream investors. Conditional on having financial assets, the scheme investors are slightly wealthier, holding 26,000 euros in financial assets compared to 24,000 euros for mainstream investors. They are also more indebted. Both their level of education and cognitive ability are significantly lower than those of mainstream investors. In particular, the cognitive ability of mainstream investors is higher by 0.28 standard deviations. The higher scheme participation by males, entrepreneurs, and younger individuals arises even when we compare the scheme investors with mainstream investors.

Many of these comparisons may reflect the fact that having savings is a prerequisite to making any investments. In Figure 2, we examine the relations between participation in the scheme or mainstream risky assets with income, education, and cognitive ability in a multivariate setting. The figures plot coefficient estimates from regressions (reported in Appendix Table IA.5) explaining participation with indicators for females, birth years, deciles of taxable income, levels of education, and quintiles of cognitive ability.

The leftmost figures show participation both in the scheme as well as in risky assets increases with income. Education and cognitive ability, however, show very different patterns. Whereas the risky-asset participation rate increases monotonically with the level of education and cognitive ability, fraud participation displays a hump-shaped pattern. Victimization is most common for individuals with average education and cognitive ability, even after controlling for income. This pattern suggests that cognitive ability combines two forces that have an opposite effect on fraud participation. First, high-skill individuals may be better at detecting investment scams. Second, low levels of ability and educational attainment may reflect low risk-taking as previously documented in the literature (Dohmen, Falk, Huffman, and Sunde, 2010).[8]

Taken together, these patterns highlight the important role of income and personal traits driving selection into the scheme. The differences along gender, entrepreneurship, and investment in risky assets are consistent with risk aversion and overconfidence affecting an individual's susceptibility to investment fraud.[9] The fact many victims hold ordinary financial assets implies personal experience from mainstream investments does not prevent one from falling prey to investment fraud.

3 Labor income response

In this section, we describe and apply our event-study difference-in-differences design to quantify the labor income response to the scheme participation. We combine different estimators to quantify both the total response to participation in the scheme as well as the separate responses to entry and collapse. We start with an event-study difference-in-differences estimator that compares the investors to a tightly matched control group. We leverage the population dimension of our data and, guided by the patterns described in the previous section, construct a control group that is observably comparable to our investors. We now describe how we arrive at this control group, design the estimator, and assess the threats to identification using alternative estimators and placebo tests.

[8]This finding is relevant to studies aiming to determine financial mistakes from investor traits. Building on the common presumption that investment mistakes decrease with cognitive ability, previous work has used cognitive ability scores to determine catering to gullible investors and investor mistakes. Calvet, Célérier, Sodini, and Vallée (2022) document a similar hump-shaped pattern between cognitive ability and participation in structured products and interpret it as evidence against investment mistakes. Our focus on an obvious investment mistake and finding of the same pattern thus highlights that investor traits may not necessarily serve as reliable indicators of financial mistakes.

[9]Numerous studies document males are more likely to invest in risky financial securities. Hvide and Panos (2014) show entrepreneurs have a high risk tolerance in their personal investments. In addition, males (Barber and Odean, 2001) and entrepreneurs (Cooper, Woo, and Dunkelberg, 1988) appear more overconfident.

We then discuss complementary estimators to interpret the separate effects of entry and collapse.

3.1 Definitions and notation

We use the following definitions and notation for our research design. We call all investors who joined the scheme in a given year a *cohort*, and denote that year as c. We report results either in event time relative to the year of joining or in calendar years, denoted as t. The *event time* j for cohort c corresponds to the number of years from joining, $j = t - c + 1$, such that the *baseline year* ($j = 0$) is the last year prior to joining the scheme. We refer to the years from joining ($j \geq 1$) until collapse ($t < 2008$) as *active* years and the years after the collapse in March 2008 as *post-collapse* ($t \geq 2008$). We match investors to controls on variables in the baseline year and use it as the pre-treatment reference point.

3.2 Control group

We use an exact match on coarsened variables following Iacus, King, and Porro (2012). The coarsened exact matching (CEM) algorithm proceeds in three steps. First, we choose a set of matching covariates and define the categorical variables and the quantiles used to coarsen continuous variables. Second, we match the scheme investors with (never-treated) individuals so that the control observations corresponding to an investor have an exact match on every (coarsened) variable. Third, we retain treatment observations for which matching control observations exist and discard observations for which no match is available. Most investors are matched to more than one control, and we retain all matched control subjects and weigh them by the inverse of the number of control subjects for each investor.

Matching variables. We select the matching variables based on the characteristics associated with selection into the scheme we discovered in the previous section. Our baseline matching variables cover average earned income in the past five years coarsened to vigintiles, average capital income in the past five years coarsened to quintiles, birth year, gender, labor market status (employed, unemployed, retired, entrepreneur, and other), an indicator for individuals who have risky asset holdings (either direct equity holdings or equity mutual funds), and income trends in the five years prior to joining the scheme coarsened to vigintiles. In robustness checks reported in Section 3.6, we further tighten the match with categories for education, region, and industry.

We match the investor's characteristics available one year before an individual joined the scheme (event year 0) to those of the control individual's in the same baseline year. For a given event year, we thus follow the investors and the controls in the same calendar year. Together with matching on birth year, our design therefore differences out any cohort and life-cycle effects on the outcome variables. We match separately on earned income and capital income to account for different income sources. These two income types constitute an individual's total taxable income in the Finnish tax system.[10] We use a five-year average of these variables to reduce the impact of temporary income shocks. Moreover, matching both on income levels and income trends, defined as differences in income between five years ($j = -4$) and one year ($j = 0$) prior to joining, flexibly controls for each investor's income patterns.

The key advantage of our matching design is that it non-parametrically controls for investors' observable characteristics as well as their interactions. For example, compared to the standard approach of controlling for characteristics in linear regressions, our design does not rely on a linear functional form. The CEM design is also more attractive than propensity score techniques as it balances all matching variables individually, in contrast to the joint balance achieved with propensity scores. This is particularly important given the rare-event nature of our setting, in which propensity score techniques may work poorly as finding variables that predict treatment with strong explanatory power is difficult (King and Nielsen, 2019). The design is also computationally more efficient than propensity score and nearest neighbor methods, which is particularly useful given the large size of our dataset. On average, the matching yields 29 control individuals per investor, and we retain all of them to minimize the impact of idiosyncratic income shocks.

CEM involves a trade-off between the precision of the match and the number of observations covered in the treatment group. We lose observations if a treatment observation cannot be matched to any control individual. The baseline matching specification we choose has high precision, balancing both income levels and income trends, and high coverage. It retains a large fraction of the sample: 2,580 investors (83 percent) from the original sample of 3,093. Additional robustness checks reported in Section 3.6 approximate the sample bias in our estimates compared to the original sample and

[10]The Finnish tax system divides income into these two categories and they are taxed at different rates. Our outcome variable for labor income includes some entrepreneurial income, which is often taxed partially as earned income and partially as capital income. We cannot use labor income as a matching variable if we also match on capital income, because the two income measures overlap.

show that it is neither statistically nor economically significant.

Covariate balance. Table 3 reports the covariate balance between the treatment and control groups by showing the means and mean differences with associated t-statistics. Panel A reports all the variables entering our baseline match. By construction, all the categorical covariates in the exact match are perfectly balanced. The income variables we match coarsely show no statistically significant differences between the treatment and control groups. Panel B shows variables that do not enter the match are often balanced as well. These variables include business education and dummies for having a mortgage and a consumer loan. The largest differences relate to the level of education, cognitive ability, and divorced status. To ensure these imbalances do not affect our results, we return to additional matching specifications in Section 3.6. Appendix Table IA.6 shows that none of the variables remain persistently significant across the alternative matching specifications.

3.3 Event-study design

With the control group in hand, we assess the responses to investing in the scheme. For now, we focus on labor income because it directly speaks to the standard prediction that labor supply should decrease following wealth gains and increase following wealth losses. In Section 4, we shift our focus to other outcomes.

Pooled event-year design. We start with an event-year design that pools all cohorts of investors. Figure 3 reports the event-year coefficients, δ_n, from the regression:

$$y_{i,c,t} = \alpha_{i,c} + \lambda_{c,t} + \sum_{\substack{n=-q \\ n\neq 0}}^{n=m} \delta_n \mathbf{1}_{i=\text{investor}} \mathbf{1}_{j=n} + \epsilon_{i,c,t}, \tag{1}$$

where $y_{i,c,t}$ is calendar year t labor income of individual i belonging to cohort c, $\alpha_{i,c}$ are individual-cohort fixed effects, $\lambda_{c,t}$ are cohort-year fixed effects, $\mathbf{1}_{i=\text{investor}}$ is an indicator variable equal to 1 for the scheme investors, and $\mathbf{1}_{j=n}$ is an indicator variable equal to 1 for event-year j. The regression uses a panel of annual observations from 1993 to 2016 and therefore allows for up to $q = 14$ leads $(\delta_{-14}, \delta_{-13}, ..., \delta_{-1})$, or pre-treatment effects, and $m = 14$ lags $(\delta_1, \delta_2, ..., \delta_{14})$, or post-treatment effects. To ensure a consistent sample, we plot only coefficient estimates with $n \in [-4, 9]$, and in Appendix Figure IA.2 with $n \in [-9, 9]$, as further leads and lags are not available for all cohorts.

The year before joining ($j = 0$) is omitted as the baseline year. We winsorize labor income at the 99th percentile by year. In all analyses, we cluster standard errors at the CEM strata level and weigh each control-group subject by the inverse of the number of subjects matched to a treatment subject.[11]

The figure shows that the labor income of investors starts declining relative to the control individuals from the first year after joining the scheme. The decline is most dramatic in the first three years after joining. After these initial years, the income response plateaus at approximately $-1,500$ euros and does not recover.

The key identifying assumption to interpret the difference causally is that the outcomes for the treatment and control groups would have maintained parallel trends in the absence of participation in the scheme. The standard test of this assumption based on assessing the trends in the pre-treatment period shows no evidence of diverging trends in the ten-year pre-treatment period plotted in Appendix Figure IA.2.

Calendar-year evidence by cohort. The pooled event study combines the impact of both the entry into the scheme and its collapse, which had opposite effects on investors' perceived wealth. To piece apart the timing of the response relative to entry and collapse, we next estimate event-year regressions for each cohort individually. Figure 4 reports the calendar-year coefficients, δ_n, from the regressions:

$$y_{i,c,t} = \alpha_{i,c} + \lambda_{c,t} + \sum_{\substack{n=1993 \\ n \neq c-1}}^{n=2016} \delta_n \mathbf{1}_{i=\text{investor}} \mathbf{1}_{t=n} + \epsilon_{i,c,t}. \tag{2}$$

The vertical dashed lines mark the active years for each cohort: the first line denotes the last year before joining ($t = c - 1$) and the second line denotes the last active year ($t = 2007$).

This decomposition at the cohort level reveals the decline in labor income emanates predominantly from the time when the scheme was active up to the year of the scheme's collapse. For all five cohorts with at least one active year, we observe a swift and gradual decline in labor income already over the active years of the scheme. With the exception of the last cohort, all cohorts also experience a pronounced decline in the year of the scheme's collapse. We find little evidence of a significant

[11] We use a stacked regression with never-treated cohort-specific controls to avoid known econometric problems with two-way fixed effect estimators under staggered treatment timing (Baker, Larcker, and Wang, 2022).

recovery over the post-collapse period.

Interpretation. The labor income response over the active years in the scheme is consistent with the standard life-cycle model (Heckman, 1974). All else equal, positive wealth shocks depress labor supply by increasing leisure time. To ease interpretation, we turn back to Figure 1, Panel A, which plots the equivalent of Figure 3 where the post-entry period covers only active years and is therefore not tainted by the effects of the scheme collapse. The speed of the response we observe is similar to responses to lottery winnings (Cesarini, Lindqvist, Notowidigdo, and Östling, 2017; Golosov et al., 2023), with a statistically significant reduction in income in the first year after joining. The gradual decrease in labor income in subsequent years is consistent with the gradual increase in the wealth gains from the scheme.

The economic magnitudes of the effects are meaningful. Because the entry to the scheme likely affected both the perceived wealth and expected returns, our setting does not allow us to quantify the marginal propensity to earn (MPE) out of wealth gains. We can, however, use the income effects estimated in lottery studies to recover the certainty equivalent wealth gains that correspond to the labor income response we observe.

For example, our estimates imply that by the second active year after joining, the participants lower their annual labor income by $-1,458$ euros (approximately 6 percent). This estimate only pertains to the first three cohorts that experience three active years before the collapse. Scaling this response with the labor income effects of lottery winnings implies the investors behave as if they received a windfall gain of 63,391–132,545 euros (see Appendix Section D.1 for details on this calculation). This magnitude is reasonable given the average investment in the scheme of 15,380 euros and promised returns of several hundred percent per year. Our calculations show the median (average) imputed wealth gain for these cohorts by event year three is 61,000 (161,000) euros. Investors also likely expected additional wealth gains in coming years. Anecdotally, several of the investors mentioned to the police that joining the scheme felt like winning a lottery, led them to reduce their labor supply, and allowed them to enjoy a more luxurious lifestyle by buying new houses, cars, and trips abroad (see Appendix Table IA.2, Panel A). These results echo the evidence in Weber et al. (2023), who show that cryptocurrency investors treat realized earnings in the same way as lottery winnings.

Since the scheme's collapse triggered comparatively larger wealth losses, comprising both the wealth gains and any unwithdrawn investments, the standard lifecycle model predicts that investors should increase their labor income above the pre-entry level. The lack of recovery in labor income over the post-collapse years thus rejects the standard model and implies an additional channel driving the income response.

The magnitude of the response in 2008 not only suggests the investors are not able to recoup their income loss, but they also experience an additional decline in labor income triggered by the collapse. To see this, note that the scheme collapsed on March 7, 2008. Any labor income response to positive wealth gains over the first two months of 2008 should amount to approximately one-sixth of the effect of a full year of participation. Yet we find that for cohorts who joined before 2008, the labor income response in 2008 (Figure 1, Panel B) is comparable to the response in 2007, implying that the collapse of the scheme leads to an additional income loss.

We also find that the labor income response in 2008 is not uniform. Figure 5 plots the calendar-time responses, δ_n, from regression 2 estimated separately for those investors who withdrew some funds and those who did not withdraw any. We restrict the figure to cohorts 2003–2006 who had sufficient time to withdraw funds after the six-month lock-up period. We observe similar labor income responses over the active years independent of withdrawal activity. At the scheme collapse, however, only those investors who withdrew funds experience a negative labor income response. These patterns thus suggest that the post-collapse response is more pronounced among investors who were more likely to adjust their consumption before the collapse.

Existing work has considered the adverse effects of financial shocks on labor productivity and psychological well-being in various settings. Financial stress, broadly defined as concerns about money or the psychological cost of financial constraints, has been shown to reduce cognitive performance, psychological well-being, and labor productivity among poor (Mani et al., 2013; Haushofer and Fehr, 2014; Haushofer and Shapiro, 2016; Kaur et al., 2021). Among the U.S. households, Sergeyev et al. (2022) document financial stress is highly prevalent and Engleberg and Parsons (2016) show financial shocks affect psychological well-being. Bernstein et al. (2021) find adverse effects of financial distress on labor productivity of inventors and Maturana and Nickerson (2020) of teachers.

In the context of fraud, FINRA (2015) documents that two-thirds of fraud victims self-report psychological effects of fraud, such as severe stress, anxiety, difficulty sleeping, and depression.

18 percent of the victims report lost wages and work time due to being defrauded. We find corroborating anecdotal evidence in the police interviews. Appendix Table IA.2, Panel B, shows many investors report psychological suffering, including declarations of depressive and suicidal thoughts. Panel C of the table reports investors' statements about the financial stress inflicted by the scheme collapse. Instead of focusing on the dramatically smaller losses of invested funds, these comments predominantly mention the loss of the expected profits from the scheme, which magnifies the financial stress from collapse.

3.4 IV estimates of income responses to wealth loss at collapse

To test for the role of financial stress in the income response, we now shift to analyses that exploit the variation in wealth losses triggered by the collapse. These analyses build on the notion that the proximity to financial constraints and thus the role of financial stress increases with the wealth loss. Hence, we test the financial-stress hypothesis that income losses at collapse are larger for victims experiencing larger wealth losses. A possible concern with such analyses is the endogeneity problem arising from correlations between invested amounts, which directly affect the accumulated and eventually lost wealth, and levels of individual income, which may be related to the income loss at collapse. We address this concern by implementing an instrumental variable approach that exploits variation in the distance of the scheme's collapse to the time of entry.

For each investor i in the scheme, we measure the income response in the year of scheme collapse, 2008, as the difference between the investor's income change and the income change of the group of i's matched control individuals, K_i:

$$l_{i,c,2008} = \underbrace{y_{i,c,2008} - y_{i,c,2007}}_{\text{difference at collapse for investor } i} - \underbrace{\mathbb{E}[y_{k,c,2008} - y_{k,c,2007} \mid k \in K_i]}_{\text{difference at collapse for controls matched to } i} \tag{3}$$

The measure of income loss is thus equivalent to a difference-in-differences estimate from a regression where the pre-period consists of the year 2007 and the post-period is the collapse year 2008. We report average income losses by cohort in the Appendix Table IA.7.

We use these income losses to estimate the following IV model:

$$w_{i,c,2008} = \mu_0 + \phi z_{i,c} + \mu_1 \mathbf{x}_{i,c} + \epsilon_{i,c}, \tag{4}$$

$$l_{i,c,2008} = \theta_0 + \beta w_{i,c,2008} + \theta_1 \mathbf{x}_{i,c} + \nu_{i,c}. \tag{5}$$

The endogenous variable $w_{i,c,2008}$ is the wealth loss at collapse. The instrument $z_{i,c}$ is defined as the event year experienced by each cohort in 2008 and therefore takes on a value of one for cohort 2008 and a value of six for cohort 2003. The first stage coefficient ϕ thus captures the impact of active years in the scheme on wealth losses at collapse. The second stage estimates the impact of wealth losses on the income response in the year of the scheme's collapse.

We estimate both specifications with and without a vector of controls $\mathbf{x}_{i,c}$. Controls include fixed effects for gender, five-year birth cohorts, marital status, an indicator for having children, labor market status and education categories as defined previously, indicators for being a homeowner, having any financial assets, region fixed effects, and five-year income deciles. All controls are measured in 2002, the year before the start of the scheme, with the exception of financial assets, which are measured in 2004, the first year of their coverage. The sample consists of all investors in baseline matching for whom labor income loss is available.[12] To limit the impact of extreme observations, we winsorize the income loss at the 5th and the 95th percentile and the wealth loss at the 95th percentile.

Table 4 reports the results of the estimation. We start with OLS regressions of the income responses on the wealth losses because the difference between OLS and IV estimates is informative about the extent and direction of the endogeneity bias. Both the specifications without (column 1) and with controls (column 2) produce a statistically significant relation between the wealth loss and the income response. The coefficient of -0.007 (t-statistic of -2.17) implies that a wealth loss of 10,000 euros translates into an additional loss of 70 euros in annual labor income. We note, however, that the point estimates should be interpreted with caution as we use imputed wealth losses. The imputation is sensitive to the choice of annual scheme returns which we do not precisely observe.

Columns 3 and 5 report the first stage (equation 4) of the IV model without and with controls,

[12] The sample thus drops 13 participants for whom data on labor income of the investor or all of the matched controls is not available in 2007 or 2008. Data on labor income is not reported to tax authorities for individuals with tax residency outside of Finland.

respectively. Not surprisingly, we find that the wealth losses are strongly related to the timing of the scheme collapse relative to entry. The first-stage F-statistics of 2,226 and 2,215 are far above the conventional levels to reject weak instruments (Stock and Yogo, 2005). The estimates imply that an additional year of participation in the scheme leads to an additional wealth loss of 25,000 euros at collapse.

Column 4 relates the income losses to the instrumented wealth losses. We find a statistically significant negative relation. The coefficient of -0.012 (t-statistic -2.69) is almost twice as large as the respective coefficient in the OLS regression. The bias toward zero in the OLS estimates may reflect wealthier households investing more and being more resilient to financial stress, or measurement error in the imputed wealth losses leading to an attenuation bias.

The key identifying assumption of the IV design is that there is no correlation between the length of participation in the scheme and individual attributes that affect the labor income change in 2008. Such correlation may arise if there are systematic differences across cohorts. To address this concern, in column 6, we control for a battery of observable investor characteristics. We find that the coefficient remains virtually unchanged (coefficient -0.011, t-statistic -2.40). As an additional robustness check of the instrument validity, we assess the role of unobservable selection following Oster (2019) who builds on the work of Altonji, Elder, and Taber (2005). The approach allows us to bound the coefficient β by considering its movement and the movement of R^2 in the specification without and with controls (columns 4 and 6 in Table 4). Using the assumptions and approach in Oster (2019), we calculate a bias-adjusted β coefficient of -0.0105.[13]

Intuitively, the bounded β is very close to the one reported in Column 6 of Table 4 because the coefficient remains virtually unchanged when adding controls, but the R^2 substantially increases. The corresponding Oster's delta is 13.9, implying that the degree of selection on unobservables, relative to observables, would need to be more than an order of magnitude higher in order for unobservables to fully explain the effects we find.

The coefficient is also economically meaningful: a loss of 25,000 euros or an additional year of participation in the scheme reduces annual income by about 275 euros ($25,000 \times 0.011$). For the average distance of entry to collapse (2.35 years), this effect size translates to 646 euros.

[13]Specifically, we assume $\delta = 1$ and $R^2_{max} = 1.3\tilde{R}$, where $\tilde{R} = 0.0194$ is the unadjusted R^2 corresponding to Column 6 of Table 4.

Alternative channels. Could alternative channels explain the patterns we observe? One potential channel is social stigma or loss of social capital more broadly. Some of the investors invited their colleagues and family members to join the scheme. Under this mechanism, we would expect the post-collapse response to be concentrated among sponsoring investors. We explore this mechanism in Appendix Figure IA.3 and find that it does not fit the data. The figure shows that investors who sponsored at least one participant experience a more pronounced decline, but those who did not sponsor anyone also display a labor decline both before and after the collapse.

Another secondary channel that may contribute to the income response is human capital depreciation due to lower labor supply over the active years. Such depreciation may stem both from some investors not participating in labor force over the active years, or from taking different career paths and missing on promotions due to lower intensive margin of labor supply. Dinerstein, Megalokonomou, and Yannelis (2022) estimate skill depreciation rate of 4 percent per year out of the labor force. A simple calculation can illustrate that this depreciation rate generates effects an order of magnitude smaller than what we find. Specifically, combining 3.4 percent lower intensive margin of labor supply per year in the scheme with the 4 percent depreciation rate yields a 32 euros decline in labor income per active year in the scheme.[14]

We note that standard versions of preferences that feature asymmetric gain-loss utility (Kahneman and Tversky, 1979), habit formation (Constantinides, 1990; Campbell and Cochrane, 1999), or reference dependence (Kőszegi and Rabin, 2006, 2009; DellaVigna, Lindner, Reizer, and Schmieder, 2017; Pagel, 2017; Thakral and Tô, 2021) alone do not yield the asymmetric income response we observe. In these models, the loss from the scheme collapse has a disproportionate adverse effect on consumption and therefore yields an increase in labor supply.[15]

3.5 Robustness and alternative estimators

Placebo tests. The event-study design controls for cohort-year fixed effects and compares each investor to a tightly matched control. Our results, therefore, cannot be driven by aggregate changes

[14] $32 = 0.034 \times 0.04 \times 23,744$, where 23,744 is the average annual labor income measured over three years prior to joining the scheme. 3.4 percent is labor income decline per active year for event-year two (1,606/2/23,744) from Figure 1, Panel A. We consider 3.4 percent as an upper bound as this is the highest per active year decline we observe in Figure 1.

[15] Similarly, asymmetric consumption responses whereby households appear to smooth consumption more in response to negative shocks (Baugh, Ben-David, Park, and Parker, 2021; Ganong, Jones, Noel, Greig, Farrell, and Wheat, 2020) do not reconcile the patterns we observe.

in economic conditions, cohort effects, or differences between investors and controls in any of the variables that enter the matching algorithm. The key threat to the identifying assumption of the event-study design is that the evolution of labor income may vary not only because of the effect of the scheme but also due to other unobservable factors. We assess the robustness of our design to this threat by taking advantage of our long-term panel data in a series of placebo tests.

Figure 6, Panel A, plots the event-year coefficients from regression 1 ("True treatment") and seven placebo tests based on a version of the same regression, the same matching procedure, and the same sample of investors. The placebo treatments differ in the year of joining, which we assign to placebo years 1993–1999, respectively, for each of the seven tests. For each placebo year, we then find the relevant control group by applying the matching design of Section 3.2 on characteristics in the year prior to the placebo joining year.[16]

The figure highlights the clearly distinct evolution of labor income for our sample of investors following the "true" year of joining. Unlike the large negative effects of the true treatment, the placebo tests show no statistically significant effects. These patterns thus increase confidence in the causal interpretation of the income response.

Triple difference estimator. The remaining concern is that the role of unobserved confounders is unique to the 2003–08 period. We assess this concern using an alternative research design that exploits the staggered entry to the scheme and compares early joiners to later joiners over the 2003–06 period. The underlying assumption in this analysis is that early joiners and late joiners are arguably comparable on unobservables that drive selection to the scheme and thus would have experienced the same change in labor income over the 2003–06 period in the absence of entry.

We implement the comparison of early and late joiners with the following triple-difference framework:

$$y_{i,c,t} = \alpha_{i,c} + \lambda_{c,t} + \sum_{\substack{n=-q \\ n\neq 0}}^{n=m} \delta_n \mathbf{1}_{i=\text{earlyInvestor}} \mathbf{1}_{j=n} + \sum_{\substack{l=1993 \\ l\neq 2002}}^{l=2006} \delta_l \mathbf{1}_{i=\text{anyInvestor}} \mathbf{1}_{t=l} + \epsilon_{i,c,t}, \tag{6}$$

where $\mathbf{1}_{i=\text{earlyInvestor}}$ is an indicator variable equal to 1 for investors in 2003 and 2004 cohorts. In addition to these two cohorts of early joiners, the sample includes two cohorts of late joiners (2007

[16]The only difference in the matching design is that we replace the indicator for risky asset holdings with an indicator for dividend or rental income, as data on asset holdings are not available prior to 2004.

and 2008) and control observations for all four cohorts matched using 2002 characteristics. The sample period ends in 2006 to avoid capturing the effects of joining the scheme for late cohorts. The triple-difference estimator thus differences out both the income evolution of matched controls and the evolution of the late joiners and quantifies the additional difference experienced only by early joiners in the scheme.

In a further refinement of the triple-difference framework, we compare early investors to investors who were invited to join the scheme late. We define the late invited investors as those who were invited by a sponsor who joined in 2006 or later or those who live in a zip code with no potential sponsor before 2006 for observations with missing information on the sponsor's identity. The timing of receiving the invitation is arguably outside of the control of investors and therefore those who received the invitation later serve as a natural control group to early joiners.

Figure 6, Panel B, compares the event-year coefficients (δ_n) from these two triple-difference regressions to the respective coefficients from the baseline event study. These baseline coefficients come from estimating equation 1 for 2003 and 2004 cohorts. The effects from the baseline event study are remarkably similar to the triple-difference estimations, further supporting the causal interpretation of our results. These analyses imply that the income responses are not only unique to the 2003–2008 period but they also occur only once investors join the scheme.

Great Recession as a threat to identification. As the last threat to identification we discuss the possibility that the Great Recession drives our results. The ability of the Great Recession to generate our results crucially hinges on the timing of its adverse effects. Appendix Figure IA.5 plots the annual averages of our eight outcome variables over 1998–2016, separately for the treatment and control groups. The first figure reports the annual average labor income and shows that the control group experienced a pronounced decline in labor income only in 2009, in other words, only one year after the emergence of the labor income responses to the scheme (as documented, e.g., in Figure 1).

The pattern of improving labor market conditions throughout 2008 is not unique to our control group. Appendix Figure IA.6 reports the seasonally adjusted unemployment rate in Finland and the U.S. at a quarterly frequency. The unemployment rate in the U.S. starts increasing in the latter half of 2007. In Finland, unemployment is on a decreasing trend up to the last quarter of 2008. The Finnish unemployment rate increases only in 2009, peaking at 9.0 percent in that year's last quarter. These patterns show Finland's labor conditions started deteriorating significantly only in

2009, more than a year after the same happened in the U.S. Finland has an economy driven by exports and it did not experience a local banking crisis during the Great Recession.

For the Great Recession to explain our estimated labor income responses, we would thus expect to see the outcomes being affected only in 2009. By contrast, Figure 1 shows the differences between the treatment and control groups emerge over the active years and in the first post-collapse year of 2008 and that the response in 2009 is substantially smaller compared to the previous years. This differential timing implies the Great Recession cannot generate our results. Instead, the timing of the income decline maps well onto the period of positive and negative wealth shocks associated with the scheme.

3.6 DiD estimates of labor income response

In this section, we attempt to put a price tag on the combined labor income responses to entry and collapse we established in the previous section. To that end, we estimate a difference-in-differences regression:

$$y_{i,c,t} = \alpha_{i,c} + \lambda_{c,t} + \delta \mathbf{1}_{i=\text{investor}} \mathbf{1}_{t \geq 2008} + \epsilon_{i,c,t}, \tag{7}$$

where $\mathbf{1}_{t \geq 2008}$ is an indicator variable equal to 1 from year 2008 onward. We start the post period in 2008, the year of the scheme collapse because we are interested in quantifying the total average labor income response that combines both the effect of joining the scheme and the effect of its collapse. Here we thus exclude from the sample the years the scheme was active.[17] We study both short-term and long-term effects using either the first two years after the scheme's collapse (2008–2009) or the full nine-year post-period that we can observe (2008–2016) as the post-treatment period. The pre-treatment period covers three years prior to joining the scheme.

Table 5 reports the results. The first two rows show that the investors experienced 1,376 euros and 1,221 euros decline in annual labor income over the nine- and two-year period, respectively. Both estimates are highly significant, with t-values of -4.3 and -4.4, respectively. The effects are also economically important: the nine-year estimate represents 5.8 percent decline in the investors' average labor income over the pre-treatment period. Accumulated over the nine post-treatment

[17] For completeness, we report the average effects over all post-entry years, that is, without excluding active years, in Appendix Table IA.8. Not surprisingly, the effects are smaller, though still highly economically and statistically significant, reflecting the gradual decline in labor income documented in Figure 4.

years we observe, the total income loss amounts to 12,384 euros.

How large is the loss of income from a life-cycle perspective? The average investor is 47.5 years old when the scheme collapses and therefore has 17.5 years left until the general retirement age of 65 applicable to the cohorts in our sample. The dynamics presented in Figure 1, Panel B, show the income response remains persistent over the post-collapse period, suggesting it likely extends beyond the nine years we observe. Extrapolating the income response over the remainder of working life and ignoring discounting, the lifetime income loss amounts to 17.5 $\times$ 1,376 = 24,080 euros. This loss almost twice exceeds the direct financial losses in the scheme. It corresponds to 190 percent of the average unwithdrawn investments (12,700 euros, see Table 1).

Robustness to alternative matching specifications. To provide comfort about the robustness of our matching design, we complement the evidence with difference-in-differences estimates from alternative matching specifications. We first tighten our baseline match with the level of education as Table 3 shows these education categories display the greatest imbalances between our treatment and control groups. In additional specifications, we tighten the match with industry and region categories. Because coarsened exact matching involves a trade-off between precision and sample size, our sample of investors decreases to 2,197, 1,894, and 1,903 observations, respectively. Appendix Table IA.6 reports the balance of covariates for these alternative matches. None of the differences in observed variables between the treatment and control groups remains persistently significant across the specifications.

Table 5 shows the labor income effects are robust to the inclusion of additional matching variables. The long-term coefficient is $-1,333$ (t-value -3.8), $-1,289$ (t-value -3.3), and $-1,476$ (t-value -3.7) when adding education, industry, and region to the matching variables, respectively. Moreover, the event-time dynamics plotted in Appendix Figure IA.4 copy the same pattern as the baseline matching both for the "true" treatment as well as the placebo treatments. Our results thus cannot be driven by the time-varying role of education, region, or industry categories.

Sample bias. We also assess the potential bias in our estimates derived from the matched sample of investors ($i \in S, n_S = 2,580$) relative to the original sample ($i \in I, n_I = 3,093$). We cannot estimate the effects in the original sample ($\delta_{i \in I}$) with the baseline matching specification because 17 percent of the investors do not match to any control observation. We can, however, approximate the sample bias, ε, using a less precise ("fuzzy") matching specification:

$$\hat{\varepsilon}^F = \hat{\delta}^F_{i\in S} - \hat{\delta}^F_{i\in I} \approx \mathbb{E}(\delta_{i\in S} - \delta_{i\in I}), \tag{8}$$

where $\hat{\delta}^F_{i\in I}$ denotes the labor income effect from the difference-in-differences regression 7 estimated for the original sample of investors matched using "fuzzy" specification F and $\hat{\delta}^F_{i\in S}$ refers to the effect from the same matching and regression specification constrained to the sample covered by the baseline matching. This approximation allows us to quantify the sample bias under the assumption that any bias introduced by the fuzzy matching does not systematically differ between the baseline and original sample ($\delta_{i\in I} - \delta^F_{i\in I} = \delta_{i\in S} - \delta^F_{i\in S}$).

Appendix Table IA.11 reports the estimated sample biases, $\hat{\varepsilon}^F$, for three "fuzzy" matching specifications. One relaxes matching on age to five-year cohorts, one drops gender and indicator of risky asset holdings from matching variables, and one matches on total income instead of on earned and capital income separately. We find a negative sample bias of 140–200 euros, implying that the magnitude of the effect in the original sample is about 10–15 percent smaller. The difference is neither large nor statistically significant.

4 Responses of other outcomes

Thus far, our analysis has concentrated solely on the responses of labor income. We now shift our focus to other labor and financial outcomes. These outcomes reflect other indirect costs of financial fraud and speak to a number of important questions: Does fraud victimization affect trust in the financial industry? Do investment scams serve as substitutes or complements to legitimate investments? How do perceived wealth shocks affect household leverage?

4.1 Labor outcomes

We first examine other labor market variables to better understand the drivers of the labor income decline we observe. To that end, we define two indicator variables that capture the extensive margin of our main result (whether investors receive any labor income) and labor force exit. Specifically, *Receives labor income* is equal to one if an individual received any labor income in a year. This variable follows the definition in Golosov et al. (2023) to proxy for labor force exit. The second variable follows a broader definition to further include individuals who do not receive labor income for involuntary reasons. *Is in labor force* is equal to one if an individual received either labor income or unemployment and sickness benefits in a year.

We again start with graphical evidence. Figure 7, Panel A, reports the responses to entry from equation 1 stacked by event year and covering only years prior to the collapse in 2008. Panel B reports the responses to the collapse from a version of the regression 2 where the baseline year is 2007.

We find both variables display a pronounced decline of a comparable magnitude over the active years of the scheme. These patterns are consistent with a voluntary labor market exit in response to the perceived wealth gains. By event year three (two years after entering the scheme), the investors are three percentage points less likely to receive labor income and participate in labor markets. In relative terms, the extensive margin of the effect is about half the magnitude of the intensive margin quantified in the previous section. The labor income decline over the same horizon represents 6.1 percent of the pre-treatment mean (1,458/23,744), whereas the labor force variable declines by only 3.5 percent (0.03/0.85). This result is consistent with the labor market responses to lottery winnings, which have much larger effects on the intensive margin (Cesarini et al., 2017; Golosov et al., 2023; Imbens, Rubin, and Sacerdote, 2001).

After the collapse, the scheme investors experience an additional decline in the likelihood of receiving labor income; however, we do not find evidence suggesting additional labor market exit. Hence, the difference is explained by an increased likelihood of receiving unemployment and sickness benefits. These patterns are consistent with our interpretation that the labor income decline post-collapse reflects worse labor market performance driven by the adverse effects of financial stress.

To quantify the economic and statistical significance of the effects, we next turn to difference-in-differences estimates from regression 7 reported in Table 6. Over the nine-year post-period, our investors are 1.4 percentage points less likely to receive labor income (t-value -2.00), which equals 1.7 percent of the pre-entry mean. Not surprisingly, the effect on the labor force indicator is not statistically significant: we observe a 0.5 percentage point effect on labor market participation over the full post-collapse period (t-value -0.87).[18]

4.2 Financial outcomes

In the last section, we measure the responses of financial variables. We begin by exploring the effects on household debt and then turn to studying the impact of the scheme on financial asset holdings.

Leverage. The interplay of household debt and wealth shocks has attracted a great deal of attention in the aftermath of the 2008–2009 crisis as higher household leverage has been linked to more pronounced consumption declines (Mian, Rao, and Sufi, 2013) and slower economic growth (Mian, Sufi, and Verner, 2017). In our setting, a number of channels may contribute to an increase in household debt. First, distorted beliefs about one's wealth and expected returns may increase the take-up of loans during the active years. Second, the worse labor market performance we document in the previous section may slow down the repayment of existing loans. Alternatively, we may observe a temporary decline in debt if investors use the gains from the scheme to repay existing debt (Cookson, Gilje, and Heimer, 2022).

We use two indicator variables to examine the impact on debt. *Has mortgage* is equal to one for investors with outstanding mortgage debt. *Has consumer loan* is equal to one for investors with an outstanding balance in short-term consumer loans. The coverage of both variables starts in 2002.

Panels A and B of Figure 8 plot the event-year and calendar year coefficients, δ_n, from regressions 1 and 2, respectively. We find the take-up of consumer loans increases over the active years. We do not observe the same effect on mortgages, which appear unaffected by participation in the scheme. These patterns are consistent with some investors using short-term debt to fund their investment in the scheme. We observe a statistically significant increase in consumer loans already in the year of joining (event-year one) of more than two percentage points. The magnitude matches the fraction of investors who mentioned to the police that they used debt to fund the scheme investments (77 out of 3,093 investors).

The patterns reverse over the post-collapse period. We observe a gradual increase in the fraction

[18] We examine additional labor market variables in Appendix Section D.2. We observe the investors experience a higher take-up of unemployment and sickness benefits as well as early pension benefits. We also observe an increase in the probability of getting divorced consistent with negative wealth shocks lowering the expected gains from marriage (Becker, Landes, and Michael, 1977).

of investors with outstanding mortgage debt, which is consistent with their worse repayment ability in the face of lower labor income. While we do not observe an additional increase in consumer loans over the post-collapse period, we also do not observe a decline, which means the increase in loan take-up over the active years is persistent.

The combined effect (over the active years and post-collapse period) is economically and statistically significant for both variables. Table 7 reports the differences-in-differences estimates from regression 7. Over the nine-year post-collapse period, the fraction of investors with a mortgage increases by 1.9 percentage points (t-value 2.2) which represents a 4 percent increase compared to the pre-treatment mean. The fraction of investors with a consumer loan increases by 3.8 percentage points (t-value 4.4) which represents an 11 percent increase compared to the pre-treatment mean.

Portfolio choice. The impact of fraud on stock market participation has received some attention in the literature. Giannetti and Wang (2016) show that the state-level stock market participation decreases after revelation of corporate scandals. Gurun et al. (2018) show that communities with a large fraction of Madoff Ponzi scheme victims decrease their holdings at investment advisers. Both studies attribute the effects to the role of trust which has been explored as an important determinant of stock market participation (Guiso, Sapienza, and Zingales, 2008). On top of the role of trust, the scheme may affect portfolio holdings through standard portfolio choice channels. The high levels of perceived returns from the scheme may lead to substitution effects from legitimate investments.

We use three indicator variables to examine the effect of the scheme on portfolio choice. *Has risky assets* is equal to one if an investor either directly holds stocks or has an investment in equity mutual funds. The other two variables, *Has directly held stocks* and *Has equity mutual funds*, indicate equity holdings through either of these two channels separately.

Panels A and B of Figure 9 again plot the event-year and calendar year coefficients, δ_n, from regressions 1 and 2, respectively. We find that both mutual fund holdings and direct equity holdings decline over the active years. These results pertain to cohorts 2005–7, that have at least one active year and one year of data before joining. This evidence is consistent with substitution effects between legitimate and fraudulent investments.

The post-collapse figures show that the effect reverses, but only through directly held equities. Holdings of equity mutual funds do not recover. These patterns are consistent with the investors losing trust in intermediated investments.

Going back to the difference-in-differences results in Table 7, over the post-collapse period the fraction of investors holding stocks directly increases by 1 percentage point, whereas the fraction of investors holding equity mutual funds decreases by 1.2 percentage points. The difference between the two coefficients, 2.2 percentage points, is statistically significant (t-value -2.2) and corresponds to 7 percent of the pre-entry average of either variable.

5 Conclusion

We document economically large and persistent household responses to investment fraud. Investors respond to the perceived wealth gain from entering the scheme by lowering labor income and exiting the labor force. The same investors, however, do not increase their labor income in response to the negative wealth shock from the scheme collapse. Instead, their labor income further declines and remains persistently lower in the long run. We attribute the labor income loss at collapse to the adverse effects of financial stress and show it is more pronounced for investors who experienced larger wealth losses. The investors also experience higher unemployment, and indebtedness, and shy away from delegated investments. The total income loss represents 6 percent of investors' annual income and over a lifetime twice exceeds the direct investment loss.

These results inform the calculation of the social cost of investment fraud—a key input to the optimal design of policies to combat fraud (Becker, 1968). Our results imply a reasonable estimate of the indirect costs of investment fraud is twice the amount of direct losses. Combining this multiple with our estimates of the market size of investment fraud at $20 billion (see Appendix A) and a 50 percent recovery rate of lost investments translates to annual damages of $50 billion.[19] The same dollar cost would arise from paying an excess one percentage point fee in a $5 trillion market, which is twice the size of the market for annuities or almost the size of target date funds and exchange-traded funds in the U.S.[20]

More broadly, studying household responses to investment fraud gives insights into potential distortions in other settings. There is little reason to believe that the deadweight loss from the lower labor supply we uncover is specific to investment fraud. Similar inefficiencies may arise both from

[19] Strictly speaking, the direct financial losses are transfers from victims to criminals, but they are a good approximation of the social loss associated with the wasteful use of the productive labor of criminals (Becker, 1968).

[20] See Koijen and Yogo (2022), Parker, Schoar, and Sun (2020), and Ben-David, Franzoni, Kim, and Moussawi (2023).

asset price movements or distorted return expectations. The recent retail trading frenzy, which has been associated both with early positive gains (Welch, 2022) but net losses on average (Bryzgalova, Pavlova, and Sikorskaya, 2022), featured many anecdotes of retail traders quitting their jobs.[21] Similar anecdotes pertain to the dot-com bubble in the late 1990s.[22] Our evidence shows people chasing phantom riches in these relatively short-lived episodes can experience substantial losses from depressed lifetime earnings.

[21] See, e.g., "Millennials are quitting jobs to become crypto day traders. Here's the risk, reward." available at https://www.yahoo.com/now/cryptocurrency-fomo-pushes-young-investors-100248559.html [Accessed on 6/15/2023], "Day Traders Go Back to Their Day Jobs as Stock Market Swoons" available at https://www.wsj.com/articles/day-traders-go-back-to-their-day-jobs-as-stock-market-swoons-11666148094 [Accessed on 6/15/2023], "Rookie Traders Are Calling It Quits, and Their Families Are Thrilled" available at https://www.wsj.com/articles/rookie-traders-are-calling-it-quits-and-their-families-are-thrilled-11672513272 [Accessed on 6/15/2023].

[22] See Aliber and Kindleberger (2005), p.159, or "Day Trading: It's a Brutal World" available at https://content.time.com/time/subscriber/article/0,33009,991726,00.html [Accessed on 6/30/2023].

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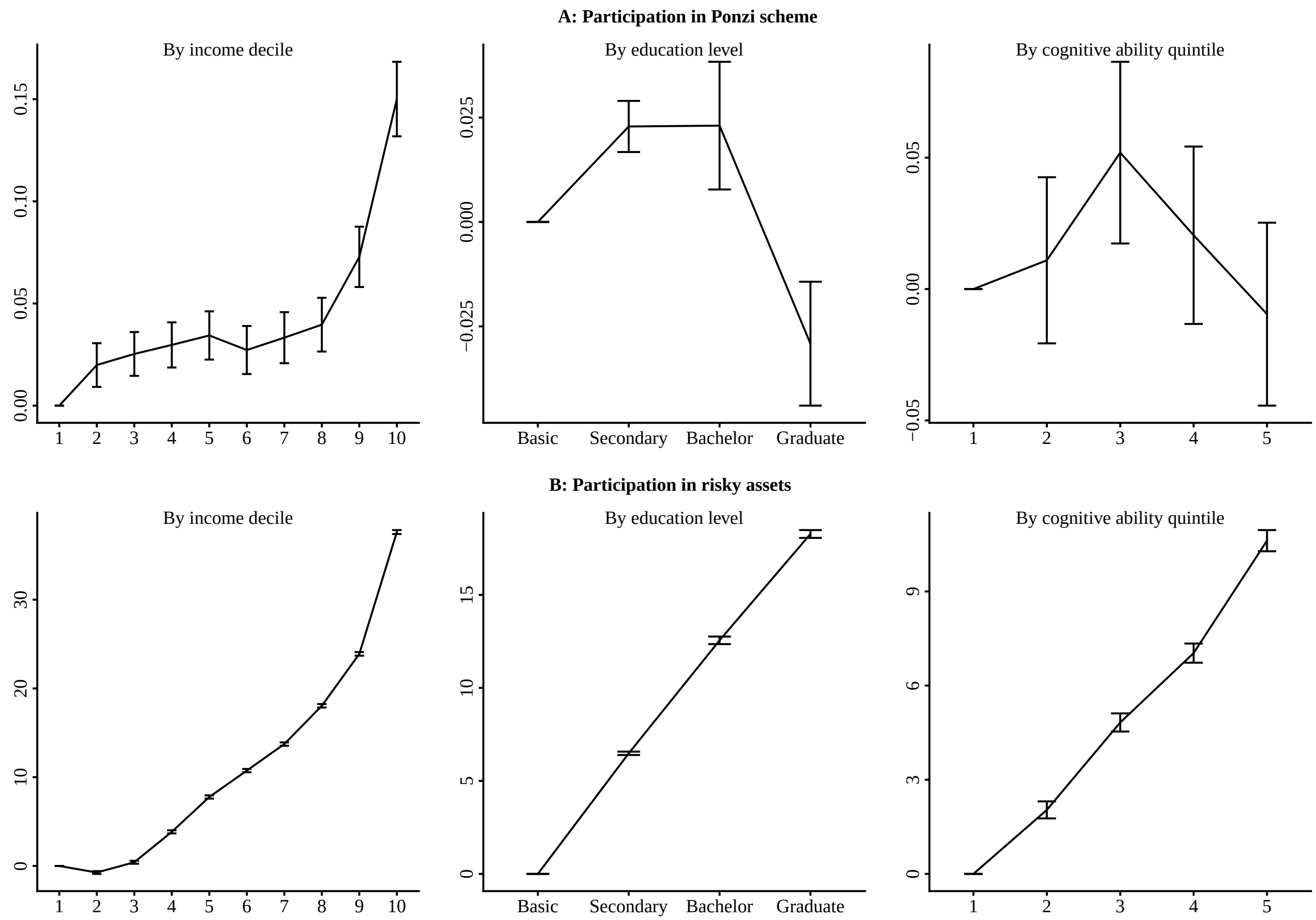

Figure 2. Likelihood of participation in the Ponzi scheme and in risky assets by income, level of education, and cognitive ability
The figures show the coefficient estimates of the likelihood of participating in the Ponzi scheme (Panel A) and in legitimate risky assets (Panel B) by deciles of income, level of education, and quintiles of cognitive ability. The results on cognitive ability are based on a subsample of males with available cognitive ability scores from the military enlistment test. The education categories are based on the individual's highest degree attained. The displayed values are coefficient estimates from cross-sectional regressions (reported in Table IA.5) explaining indicator variables for participation in the Ponzi scheme and in legitimate risky assets. These regressions include birth-year cohorts, an indicator variable for females (omitted in the regressions including cognitive ability) and indicators for income deciles and levels of education. The sample is the Finnish adult population in 2002 and the risky asset holdings are measured in 2004. The vertical bars show the 95 percent confidence intervals.

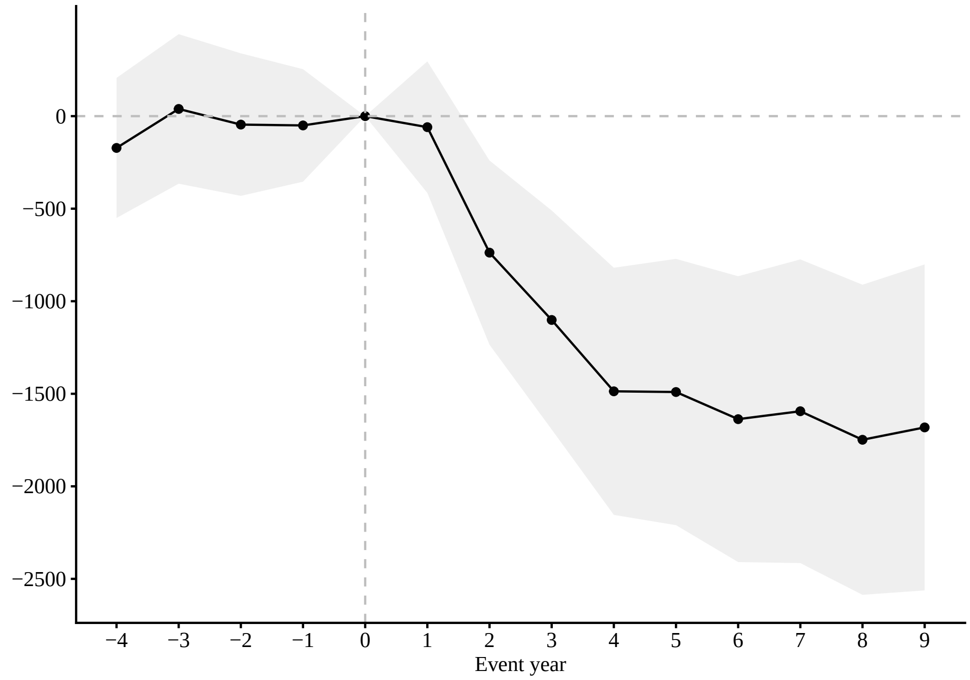

Figure 3. Labor income response relative to year before joining

The figure plots event-year coefficients, δ_n, from estimating regression (1). The dependent variable is annual labor income in euros. The sample consists of 2,580 fraud investors and their matched control group described in Section 3.2 and covers annual observations from 1993 to 2016. The vertical dashed line depicts the year before an individual joined the scheme. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

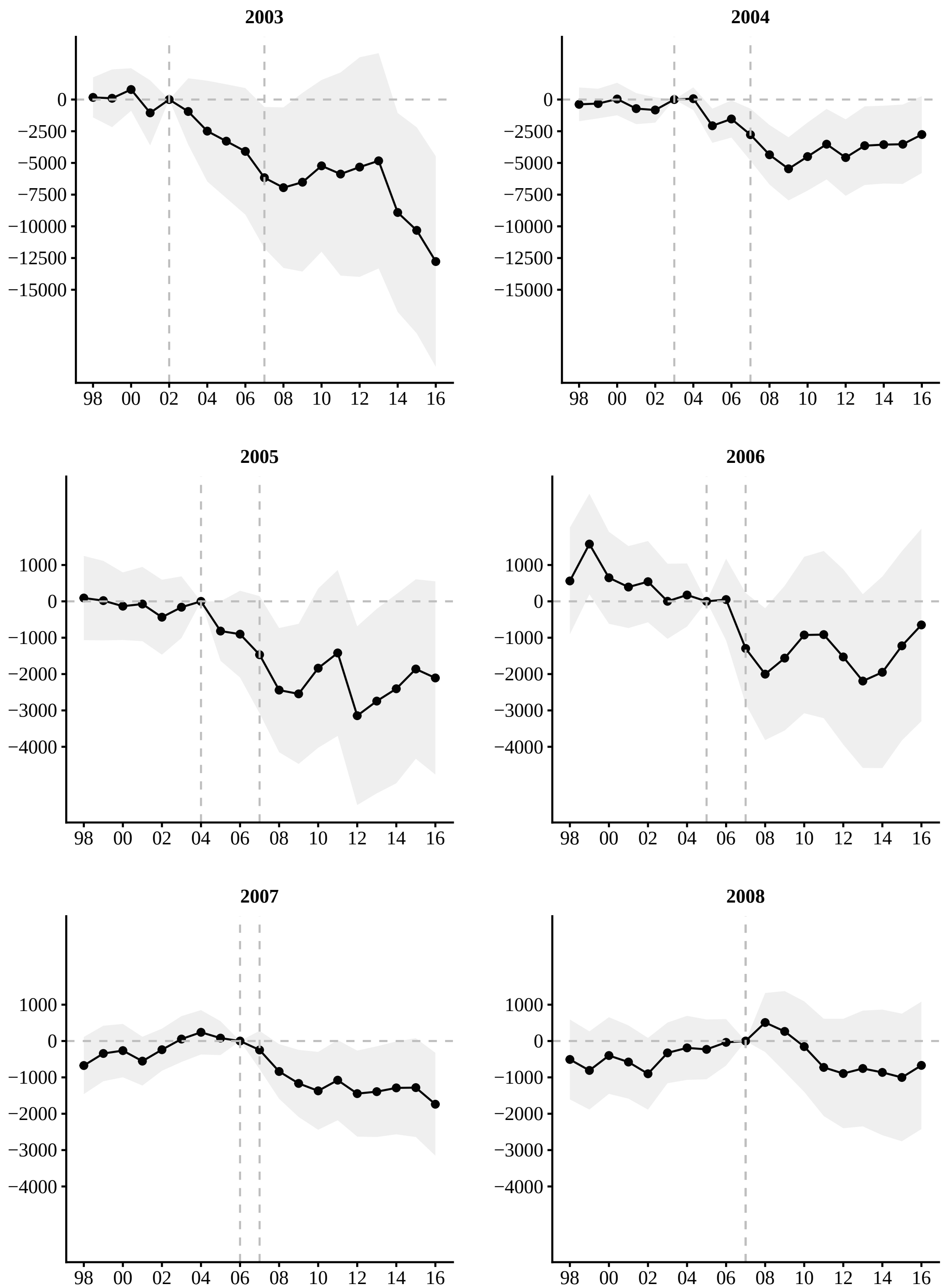

Figure 4. Labor income response by cohort

The figures plot calendar-year coefficients, δ_n, from estimating regression (2) for each cohort separately. The dependent variable is annual labor income in euros. The samples cover 2,580 fraud investors and their matched control group described in Section 3.2 and annual observations from 1993 to 2016. The first vertical dashed line depicts the year before an individual joined the scheme. The second vertical dashed line depicts the last year before the scheme collapsed. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

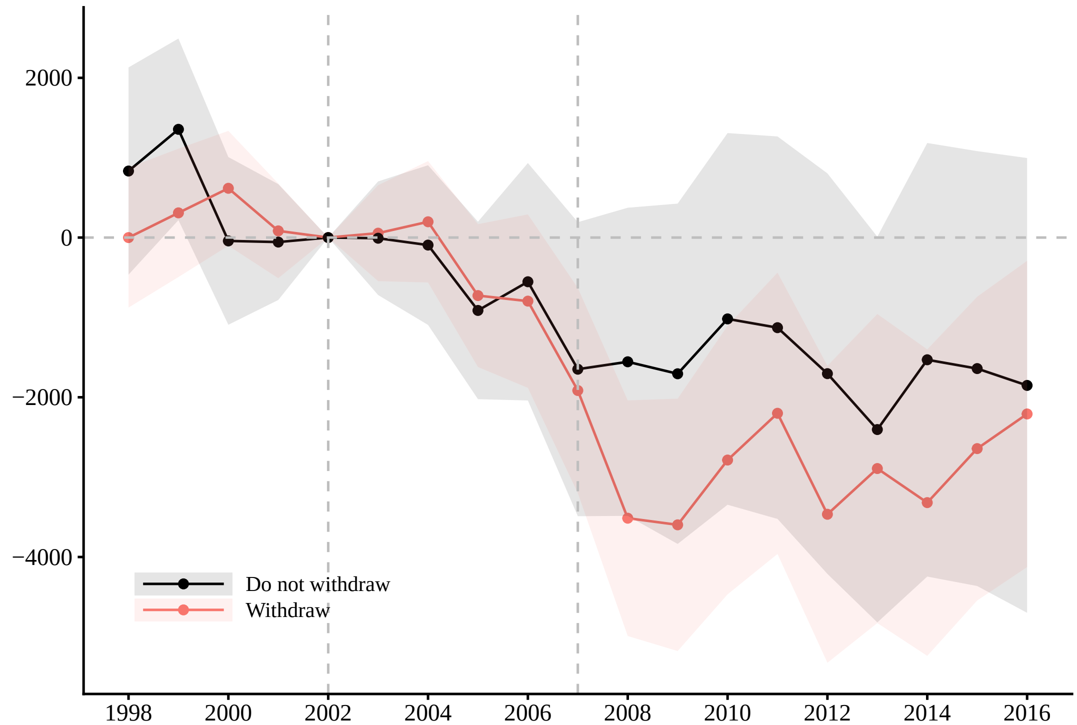

Figure 5. Labor income response by withdrawals

The figure plots event-year coefficients, δ_n, from estimating regression (2) separately for investors who withdrew some funds and those who did not. The dependent variable is annual labor income in euros. The sample covers 2003–2006 cohorts of fraud investors and their matched control group described in Section 3.2. The first vertical dashed line depicts the year before the first investors joined the scheme. The second dashed line depicts the last year before the scheme collapse. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

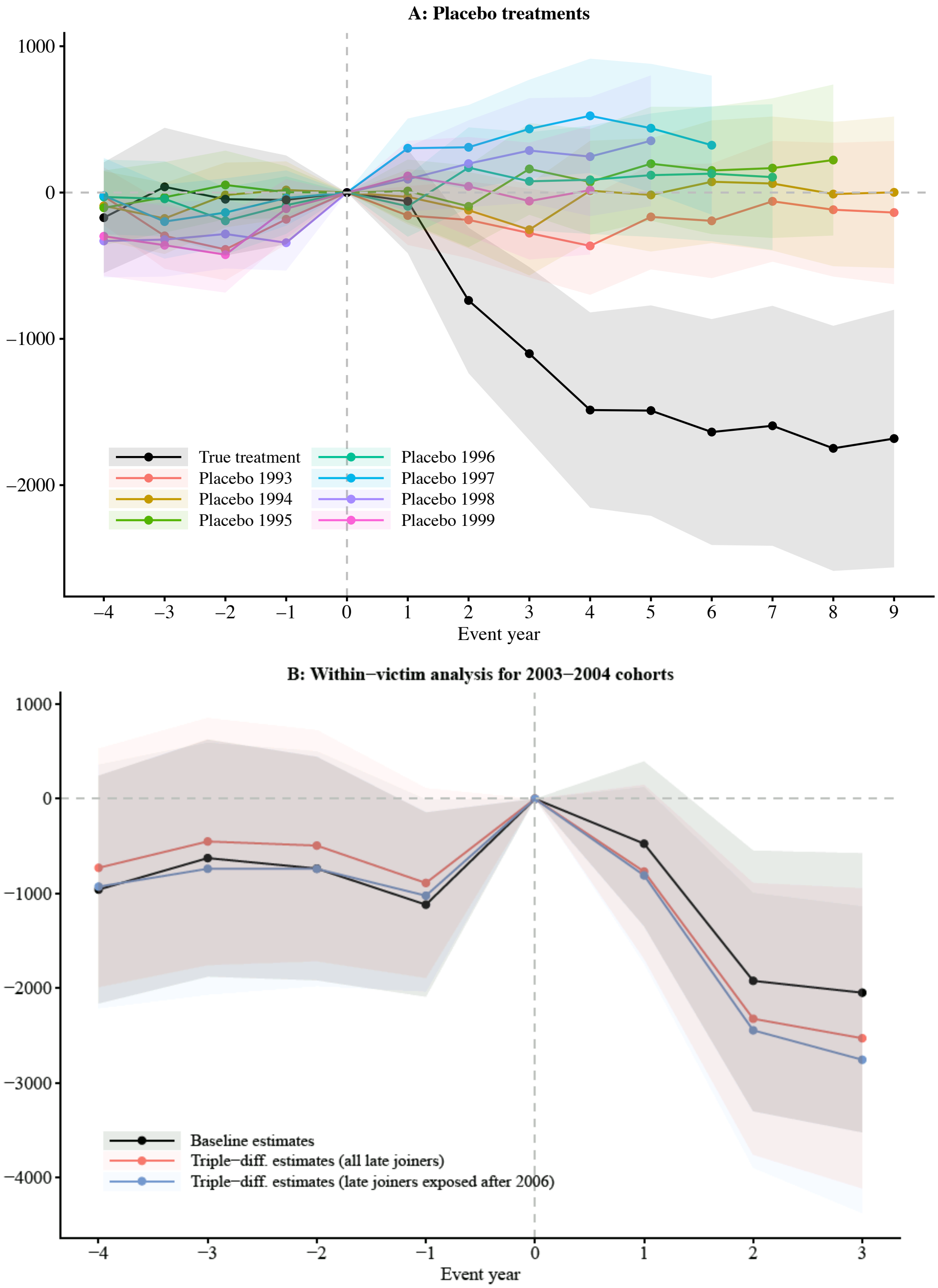

Figure 6. Validity of difference-in-differences estimator for causal identification

Panel A plots event-year coefficients, δ_n, from estimating regression (1) using either the actual years of joining ("True treatment") or assigning the placebo year of joining to 1993–1999, respectively. The samples for placebo treatments cover observations from five years prior to placebo joining (1988–1994) to 2002. Panel B plots event-year coefficients, δ_n, from estimating triple-difference regression (6) which compares two cohorts of early joiners (2003–2004) to two cohorts of late joiners (2007–2008) or late joiners invited after 2006 as defined in Section 3.5. For comparison, the figure also plots baseline coefficients from estimating regression (1) for cohorts 2003 and 2004.

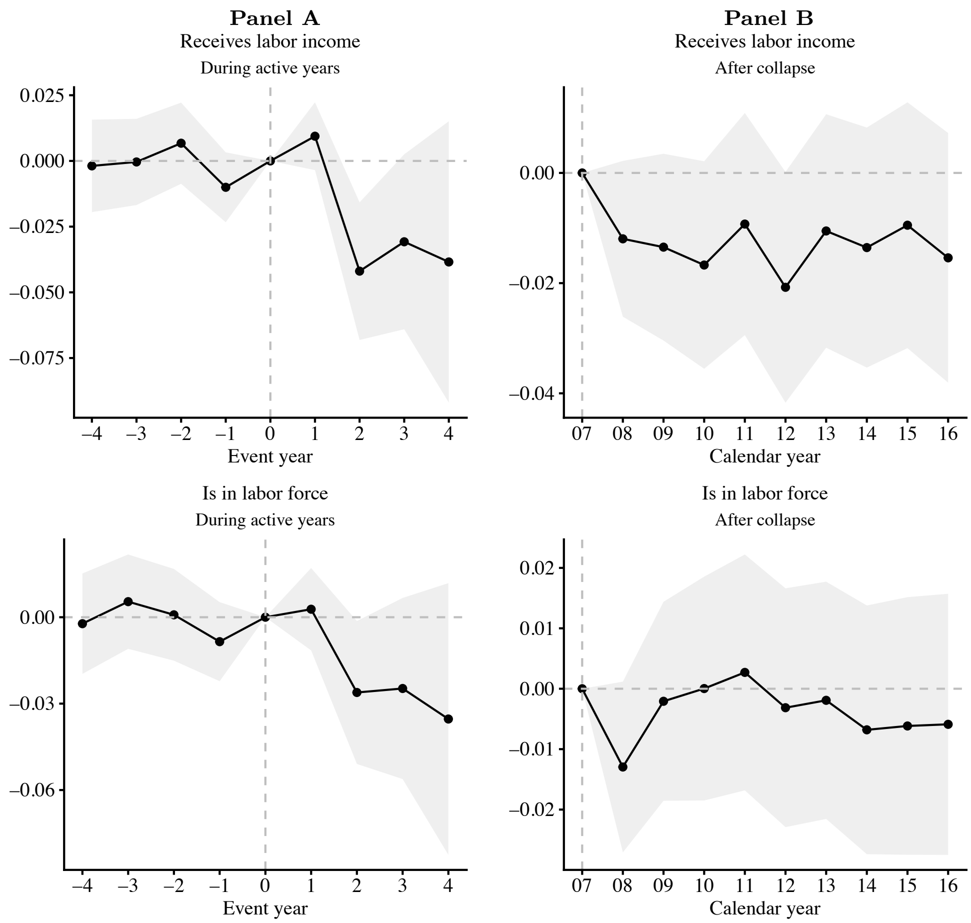

Figure 7. Responses on extensive margin and labor force exit

The figures plot event-year and calendar-year coefficients, δ_n, from estimating regressions (1) and a version of regression (2), where the baseline year is 2007. The dependent variables are indicators for individuals who receive labor income and those that are in the labor force, as defined in Section 4.1. The sample covers cohorts 03–07 and their matched control group described in Section 3.2 and annual observations from 1993 to 2016. The vertical dashed line in Panel A depicts the year before joining the scheme. The vertical dashed line in Panel B depicts the last year before the scheme collapsed. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

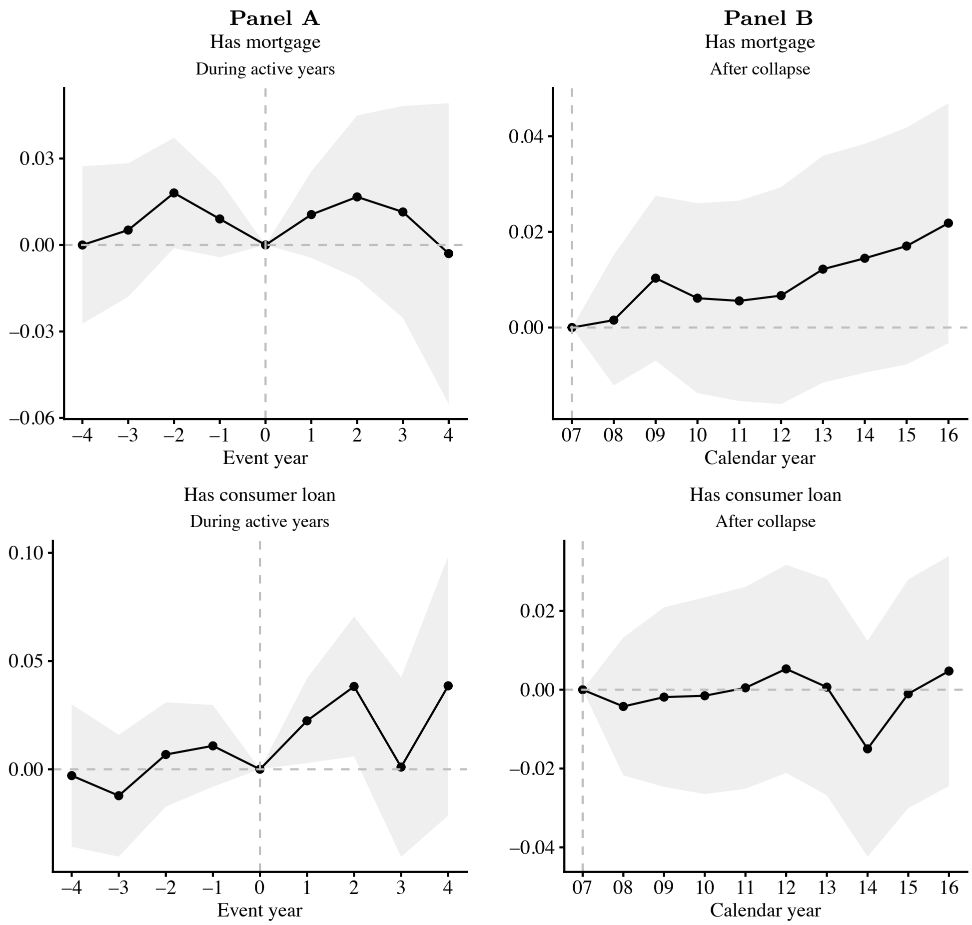

Figure 8. Effects on mortgage and consumer loan take-up

The figures plot event-year and calendar-year coefficients, δ_n, from estimating regressions (1) and a version of regression (2), where the baseline year is 2007. The dependent variables are indicators for individuals with outstanding consumer loans and mortgages. The sample covers cohorts 03–07 and their matched control group described in Section 3.2. The vertical dashed line in Panel A depicts the year before joining the scheme. The vertical dashed line in Panel B depicts the last year before the scheme collapsed. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

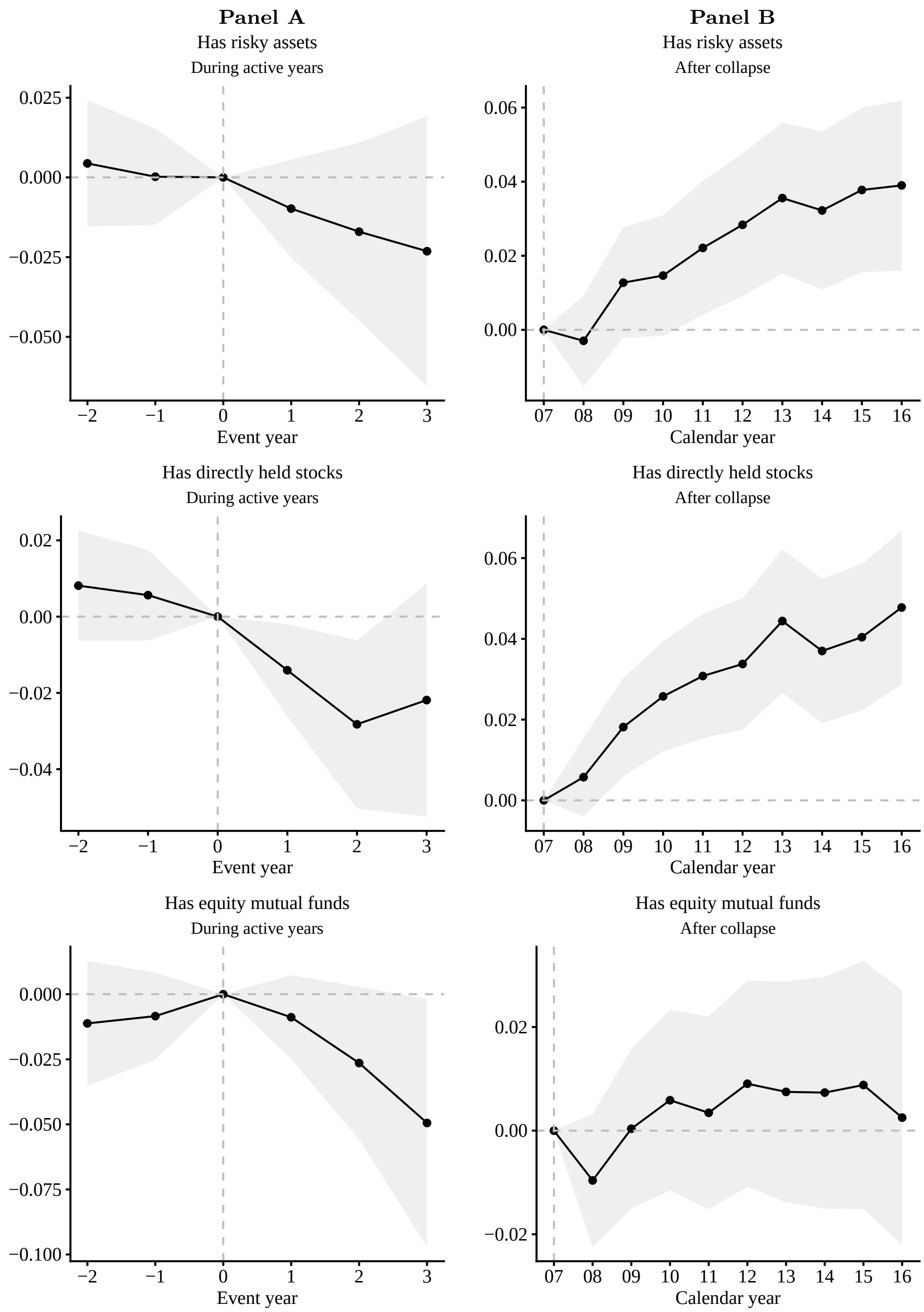

Figure 9. Effects on stock market participation

The figures plot event-year and calendar-year coefficients, δ_n, from estimating regressions (1) and a version of regression (2), where the baseline year is 2007. The dependent variables are indicators for risky financial asset holdings, direct equity holdings, and investments in equity mutual funds. Panel A is based on cohorts 2005–7, that have at least one active year and at least one year of data before joining. Panel B is based on cohorts 2003–7, that have at least one active year before collapse. The vertical dashed line in Panel A depicts the year before joining the scheme. The vertical dashed line in Panel B depicts the last year before the scheme collapsed. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

Table 1

Descriptive statistics on number of participants, investments, withdrawals, and wealth losses

Panel A shows the number of participants in the sample by cohort of entry into the scheme. Panel B reports descriptive statistics of invested and withdrawn amounts for 3,093 participants in the scheme. Invested is the sum of all invested amounts by a participant. Withdrawn is the sum of all withdrawn amounts. Net invested is the sum of all invested amounts minus the sum of withdrawn amounts or zero for victims who have withdrawn more than they invested. Invested / Total income presents the total invested amount divided by the average total income over five years prior to joining the scheme. Panel C reports summary statistics of the imputed wealth loss at collapse by cohort of entry. Wealth loss is defined as the sum of the imputed account balance at collapse plus any withdrawals in excess of invested amounts. See Appendix Section C for the details of the imputation procedure.

Panel A: Number of participants by cohort

	2003	2004	2005	2006	2007	2008
Number of participants	44	226	320	378	1320	787

Panel B: Investments and withdrawals

	Mean	q01	q05	q25	q50	q75	q90	q99	SD
Invested, €1,000	15.4	0.2	1.0	4.0	8.0	15.1	49.6	113.7	39.7
Withdrawn, €1,000	10.6	0.0	0.0	0.0	0.0	0.6	36.8	249.4	64.2
Net invested, €1,000	12.7	0.0	0.0	3.0	6.0	12.9	41.9	98.7	38.3
Invested / Total income	2.6	0.0	0.0	0.2	0.4	0.8	3.7	17.0	47.1

Panel C: Wealth loss at collapse by cohort, €1,000

	2003	2004	2005	2006	2007	2008
Median	106.7	154.0	66.7	40.6	21.4	6.1
Mean	314.0	503.8	152.2	64.4	34.1	10.0

Table 2
Ponzi scheme investors compared with population and mainstream investors

The table reports the characteristics of the adult population in 2002, Ponzi scheme investors, and mainstream investors in 2004. Characteristics and liabilities are measured in 2002 and financial assets are measured in 2004, the first available year. Business education refers to holding a degree in business or economics and a finance professional works in the finance industry. Employment status reports entrepreneurs, workers in employment or unemployment, and retirees with the remaining category omitted from the table. Financial wealth in Panel B is based on those who hold some financial assets. Standardized cognitive ability scores in Panel C are for males who took the military enlistment test after January 1982.

Panel A: Personal characteristics

	Population (N=4,155,601)		Ponzi investors (N=3,093)		Mainstream investors (N=992,024)	
	Mean	SD	Mean	SD	Mean	SD
Total income, €1,000	18.38	39.09	29.43	36.02	27.71	70.26
Age	47.21	18.20	41.56	11.97	49.36	16.63
Female	0.52	0.50	0.18	0.39	0.48	0.50
Level of education						
Basic	0.37	0.48	0.18	0.39	0.27	0.44
Secondary	0.50	0.50	0.63	0.48	0.51	0.50
Bachelor	0.06	0.23	0.09	0.29	0.09	0.29
Graduate	0.07	0.25	0.09	0.28	0.13	0.34
Business education	0.10	0.30	0.12	0.32	0.14	0.35
Finance professional	0.02	0.14	0.02	0.15	0.04	0.19
Entrepreneur	0.06	0.23	0.20	0.40	0.07	0.26
Retired	0.27	0.45	0.07	0.25	0.27	0.44
Employed	0.49	0.50	0.60	0.49	0.56	0.50
Unemployed	0.06	0.25	0.05	0.21	0.03	0.17
Married	0.48	0.50	0.53	0.50	0.54	0.50
Divorced	0.11	0.31	0.12	0.32	0.10	0.30

Panel B: Assets and loans

	Population (N=4,155,601)		Ponzi investors (N=3,093)		Mainstream investors (N=992,024)	
	Mean	SD	Mean	SD	Mean	SD
Has financial assets	0.24	0.43	0.44	0.50	1.00	0.00
Has stocks or equity mutual funds	0.22	0.41	0.42	0.49	0.91	0.29
Has mortgage	0.23	0.42	0.39	0.49	0.26	0.44
Has consumer loan	0.19	0.40	0.30	0.46	0.18	0.38
Financial wealth (if wealth > 0)	23.77	663.57	26.07	79.70	23.77	663.57

Panel C: Cognitive ability

	Population (N=611,624)		Ponzi investors (N=1,044)		Mainstream investors (N=143,136)	
	Mean	SD	Mean	SD	Mean	SD
Cognitive ability	0.00	1.00	0.04	0.88	0.32	0.95

Table 3
Covariate balance of Ponzi investors and matched controls

The table reports means and standard deviations of characteristics for 2,580 investors and their 73,949 matched controls described in Section 3.2. We also report the mean difference between investors and controls and the associated t-statistics. Statistics for controls are weighted by the inverse of the number of matched controls for each investor. Total, earned, and capital income are annual averages over five years prior to entry into the scheme. Age is measured in 2002. The remaining variables are measured at the end of the year preceding an individual's entry in the scheme. Asset-holding variables are equal to 0 for cohorts that joined the scheme before 2005. Panel A reports characteristics used in matching the investors to controls. Panel B reports other variables not used in matching. Earned and capital income are coarsened into vigintiles and quintiles, respectively, before they enter the matching algorithm.

Panel A: Variables entering the match

	Investors (N=2,580)		Controls (N=73,949)		Investors − Controls	
	Mean	SD	Mean	SD	Mean	t-stat
Earned income, €1,000	30.85	22.38	31.52	27.85	-0.67	-1.40
Capital income, €1,000	7.00	32.14	5.85	52.49	1.16	1.66
Age	41.56	11.97	41.56	11.97	0.00	
Female	0.17	0.38	0.17	0.38	0.00	
Entrepreneur	0.20	0.40	0.20	0.40	0.00	
Retired	0.09	0.29	0.09	0.29	0.00	
Employed	0.64	0.48	0.64	0.48	0.00	
Unemployed	0.02	0.14	0.02	0.14	0.00	
Has stocks or equity mutual funds	0.47	0.50	0.47	0.50	0.00	

Panel B: Other variables

	Investors (N=2,580)		Controls (N=73,949)		Investors − Controls	
	Mean	SD	Mean	SD	Mean	t-stat
Total income, €1,000	37.85	41.56	37.36	63.34	0.49	0.55
Level of education						
Basic	0.16	0.37	0.19	0.39	-0.02	-2.90
Secondary	0.61	0.49	0.57	0.49	0.04	4.01
Bachelor	0.12	0.32	0.11	0.31	0.01	1.70
Graduate	0.10	0.31	0.13	0.34	-0.03	-4.63
Business education	0.12	0.32	0.11	0.32	0.00	0.64
Finance professional	0.02	0.15	0.03	0.17	-0.01	-1.79
Cognitive ability (if available)	0.03	0.88	0.11	0.95	-0.08	-2.35
Married	0.58	0.49	0.58	0.49	0.00	-0.15
Divorced	0.13	0.33	0.11	0.31	0.02	2.56
Has financial assets	0.49	0.50	0.48	0.50	0.00	0.26
Has mortgage	0.44	0.50	0.42	0.49	0.02	1.98
Has consumer loan	0.36	0.48	0.35	0.48	0.01	0.59

Table 4
Income loss at collapse as function of wealth loss

The table provides estimates of the effect of wealth loss on income loss in the year of the scheme collapse based on the instrumental variable model in equations 4 and 5. The dependent variable is either income loss in the year of scheme collapse as defined in equation 3 or imputed wealth loss as described in Appendix Section C. The sample consists of 2,567 investors in baseline matching with available income loss. Columns 1 and 2 report OLS estimation of equation 5 without instrumenting for wealth loss. Columns 3 and 5 report the first-stage equation 4 and Columns 4 and 6 report the second-stage equation 5. Controls include fixed effects for gender, five-year birth cohorts, marital status, an indicator for having children, labor market status and education categories as defined previously, indicators for being a homeowner, having any financial assets, region fixed effects, and five-year income deciles. All controls are measured in 2002, except for financial assets, which are measured in 2004, the first year of their coverage. The reported t-statistics are based on standard errors clustered at the strata level.

	OLS		IV			
IV stage			First	Second	First	Second
	Income loss	Income loss	Wealth loss	Income loss	Wealth loss	Income loss
	(1)	(2)	(3)	(4)	(5)	(6)
Wealth loss	-0.007	-0.006		-0.012		-0.011
	(-2.17)	(-2.03)		(-2.69)		(-2.40)
Active years			25,059		25,056	
			(35.8)		(35.5)	
Controls		✓			✓	✓
Observations	2,567	2,567	2,567	2,567	2,567	2,567
Adjusted R^2	0.0015	0.0040	0.4645	0.0002	0.4855	0.0031
F-test			2,226.4		2,215.2	

Table 5
Labor income response to investment fraud

The table provides estimates of the labor income response to the scheme participation based on difference-in-differences regression (7). The sample consists of 2,580 investors in baseline matching and their matched controls described in Section 3.2 and covers three years prior to joining the scheme and nine years (long-term) or two years (short-term) after its collapse. The alternative matching specifications in addition include education categories, industries, or regions. These specifications cover 2,197, 1,894, and 1,903 investors, respectively. The dependent variable is annual labor income in euros. Each row corresponds to a separate regression. The last column presents the mean value of the outcome variable for investors in the period before joining. The reported t-statistics are based on standard errors clustered at the strata level.

	δ	t-stat	N	Adj. R^2	Pre-mean
Baseline matching					
Long-term	-1,375.9	(-4.30)	899,457	0.74	23,743.8
Short-term	-1,221.2	(-4.36)	380,715	0.81	23,743.8
Adding education					
Long-term	-1,333.4	(-3.79)	470,261	0.73	23,743.8
Short-term	-1,251.1	(-3.96)	198,884	0.81	23,743.8
Adding industry					
Long-term	-1,289.1	(-3.28)	313,893	0.73	23,743.8
Short-term	-1,207.2	(-3.58)	134,202	0.82	23,743.8
Adding region					
Long-term	-1,476.1	(-3.73)	277,588	0.73	23,743.8
Short-term	-1,413.2	(-4.25)	117,547	0.82	23,743.8

Table 6
Responses on extensive margin and labor force exit

The table provides estimates of the effects of the scheme participation based on difference-in-differences regression (7). The dependent variables are indicator variables for individuals receiving labor income and for individuals in the labor force, as defined in Section 4.1. The sample consists of 2,580 investors and their matched controls described in Section 3.2 and covers three years prior to joining the scheme and two years (short-term) or nine years (long-term) after its collapse. Each row corresponds to a separate regression. The last column presents the mean value of the outcome variable for investors in the period before joining. The reported t-statistics are based on standard errors clustered at the strata level.

	δ	t-stat	N	Adj. R^2	Pre-mean
Receives labor income					
Long-term	-0.014	(-2.00)	899,457	0.62	0.820
Short-term	-0.013	(-1.88)	380,715	0.68	0.820
Is in labor force					
Long-term	-0.005	(-0.87)	899,457	0.60	0.854
Short-term	-0.009	(-1.39)	380,715	0.64	0.854

Table 7
Effect of investment fraud on financial outcomes

The table provides estimates of the effect of the scheme participation on investors' financial outcomes based on difference-in-differences regression (7). The sample consists of 2,580 investors and their matched controls described in Section 3.2 and covers up to three years prior to joining the scheme and two years (short-term) or nine years (long-term) after its collapse. The dependent variables are indicators for mortgages, consumer loans, risky financial asset holdings, direct stock holdings, and investments in equity mutual funds, defined in Section 4.2. The coverage of mortgages and consumer loans starts in 2002, whereas that for risky financial assets starts in 2004. Consequently, for cohorts 2003 and 2004, the baseline period for risky financial assets is the first year of data (2004). The last column presents the mean value of the outcome variable for investors in the period before joining. The reported t-statistics are based on standard errors clustered at the strata level.

	δ	t-stat	N	Adj. R^2	Pre-mean
Has mortgage					
Long-term	0.019	(2.23)	884,356	0.63	0.438
Short-term	0.008	(1.00)	365,614	0.69	0.438
Has consumer loan					
Long-term	0.038	(4.42)	884,356	0.47	0.349
Short-term	0.030	(3.32)	365,614	0.54	0.349
Has risky assets					
Long-term	0.004	(0.47)	844,314	0.74	0.459
Short-term	-0.019	(-2.61)	325,572	0.78	0.459
Has directly held stocks					
Long-term	0.010	(1.59)	844,314	0.81	0.317
Short-term	-0.008	(-1.45)	325,572	0.86	0.317
Has equity mutual funds					
Long-term	-0.012	(-1.52)	844,314	0.66	0.314
Short-term	-0.022	(-2.78)	325,572	0.70	0.314

Internet Appendix to
Household Responses to Phantom Riches

Samuli Knüpfer, Ville Rantala, Erkki Vihriälä, Petra Vokata

July 3, 2023

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Additional Figures

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A Market for investment fraud

To assess the size of the market for investment scams, we screen for cases uncovered by U.S. agencies in 2022.

Definition. We are interested in investment scams targeted at households. That is, we consider fraudulent schemes that use deceptive practices to deceive individuals into making investments based on misleading information. Typical forms involve Ponzi-like schemes, pyramid schemes, and misappropriation of funds invested based on false promises of often high returns and low risk.

We do not consider other types of securities fraud, such as pump-and-dump schemes, insider trading, financial reporting fraud or front-running. We also do not consider other types of financial fraud targeted at households, such as identity theft, credit card fraud, phishing scams, or advance fee fraud.

Size of the market. Table IA.1 details our estimate of the market size based on scams uncovered in 2022. With the exception of FBI (2022), U.S. agencies do not report information on the aggregate amount of investment fraud uncovered, reported, or prosecuted in a given year. We thus screen public information about uncovered cases, combining information from multiple agencies and sources. We conservatively estimate the size of the market at $20 billion. The table lists the five largest investment scams uncovered in 2022 and additional aggregate categories by agencies. We next describe how we arrive at our estimate.

SEC. We start by screening the SEC litigation notices and complaints available at https://www.sec.gov/litigation/litreleases.htm. We screen for investment scams exceeding $10 million and satisfying our definition. We exclude litigation filings related to earlier SEC complaints to restrict the sample to investment scams uncovered in 2022. This screen yields 19 cases. Three of them are within the five largest cases: FTX Trading and Alameda Research (charged in a parallel action by the CFTC), National Realty Investment Advisors, and Beasley Law Group. The remaining 16 cases account for more than a billion raised funds. Of these, more than 70 percent by value involve cryptocurrency or digital assets. The SEC has issued multiple investor alerts warning about fraudsters exploiting the rising popularity of cryptocurrencies.[23]

CFTC. We next screen for scams prosecuted by the Commodity Futures Trading Commission

[23] See, e.g., https://www.investor.gov/introduction-investing/general-resources/news-alerts/alerts-bulletins/investor-bulletins/crypto-asset-securities [Accessed on 6/22/2023].

(CFTC) using their press releases available at https://www.cftc.gov/PressRoom/PressReleases. Again, we only screen for cases exceeding $10 million, satisfying our definition, and not related to cases filed by the CFTC before 2022. This screen yields 9 cases, two of which are within the five largest scams listed in the Table: FTX Trading and Alameda Research, and MTI scheme. The remaining 7 cases account for over $250 million of raised funds. Of these, more than 60 percent by value involve cryptocurrency or digital assets.

DOJ and ponzitracker.com. Many investment scams are prosecuted by the U.S. Department of Justice (DOJ), which does not provide an easy way to screen for prosecuted investment scam cases. We instead use information collected by https://www.ponzitracker.com/2022-ponzi-schemes. To avoid double counting, we remove from this list of Ponzi schemes those that are already included in the 26 cases prosecuted by the SEC and the CFTC. The remaining list yields one scheme within the five largest cases: a multi-faceted cryptocurrency Ponzi-like scheme associated with a cryptocurrency mining service called HashFlare and a virtual currency bank called Polybius Bank. The remaining Ponzi schemes account for over a billion in raised funds.

FBI. We add the total amount of online investment scams reported to the FBI and presented in their Internet Crime Report 2022 (FBI, 2022). Of the $3.31 bn total amount of reported investment scams, $2.57 bn involved cryptocurrency scams. The scams listed by the FBI include liquidity mining, celebrity impersonation, and hacked social media accounts used to entice investors into fraudulent investment opportunities. FBI reports that compared to 2021, the amount of reported investment scams increased by 127 percent.

Unreported investment scams. A major challenge in assessing the magnitude of investment scams is that a large fraction is believed to remain undetected and unreported. FINRA (2013) found in their national survey many investors do not realize they have been scammed, and of those that do, only 45 percent report the case to the authorities. We conservatively estimate the price tag of unreported scams and those missed by our screening procedure described above at $3.31 bn, i.e., the same amount as scams reported to the FBI.

The largest case in 2022, FTX Trading and Alameda Research, accounts for $8 billion of investor losses, which raises a question about the representativeness of the 2022 year. We note that while the FTX case is large, there were other schemes of similar magnitude in the recent past. The Madoff Ponzi scheme resulted in $17.3 bn losses of the investment principal and Allen Stanford's Ponzi scheme in \$4.5–6 bn of losses. Our estimate also does not cover a multi-billion dollar crypto asset securities fraud, the Terra-LUNA case, whose crash in May 2022 wiped out \$50 billion of valuation (Liu et al., 2023) but was charged by the SEC only in February 2023.[24]

Participation rate. Estimates of the fraction of households that fall prey to investment scams vary. In their national survey of a representative sample of U.S. population age 40 or older, FINRA (2013) reports participation rate of 11 percent. DeLiema, Mottola, and Deevy (2017) address measurement issues and find a victimization rate of 17 percent in a demographically representative sample of the U.S. population. Leuz, Meyer, Muhn, Soltes, and Hackethal (2017) find nearly 6 percent of active investors among the clients of a German brokerage participate in fraudulent "pump-and-dump" schemes. Our estimate of a participation rate equal to one-sixth of that in equities assumes 10 percent participation rate in investment scams and equity market participation rate of 60 percent.[25]

[24] See the SEC press release available at https://www.sec.gov/news/press-release/2023-32 [Accessed on 6/22/2023].

[25] In 2022, U.S. households report 58 percent stock market participation rate. See national survey results available at https://www.statista.com/statistics/270034/percentage-of-us-adults-to-have-money-invested-in-the-stock-market/ [Accessed on 6/24/2023].

Table IA.1
Uncovered, reported, and unreported investment scams by U.S. agencies in 2022

The table details our estimate of the $ amount of fraudulent investment schemes uncovered by U.S. agencies in 2022. Amount refers to the total funds raised by the scheme (where available) or losses to investors. The first five rows display the five largest cases. The remainder of the table shows the aggregate amount by the agency. Appendix Section A describes the details of the table construction.

Agency	Amount	Name	Description	Source
SEC/CFTC	$8.000 bn	FTX Trading and Alameda Research	Fraudulent scheme related to sale of digital commodities	https://www.cftc.gov/PressRoom/PressReleases/8638-22, https://www.sec.gov/news/press-release/2022-219
CFTC	$1.700 bn	MTI	Multi-level marketing scheme involving Bitcoin	https://www.cftc.gov/PressRoom/PressReleases/8549-22
SEC	$0.600 bn	National Realty Investment Advisors	Ponzi-like real estate scheme	https://www.sec.gov/litigation/litreleases/2022/lr25558.htm
DOJ	$0.575 bn	HashFlare, Polybius	Cryptocurrency Ponzi-like scheme	https://www.justice.gov/opa/pr/two-estonian-citizens-arrested-575-million-cryptocurrency-fraud-and-money-laundering-scheme
SEC	$0.450 bn	Beasley Law Group	Ponzi scheme	https://www.sec.gov/litigation/litreleases/2022/lr25434.htm
SEC	$1.097 bn		Investment scams	https://www.sec.gov/litigation/litreleases.htm
CFTC	$0.252 bn		Investment scams	https://www.cftc.gov/PressRoom/PressReleases
DOJ/Other	$1.086 bn		Ponzi schemes	https://www.ponzitracker.com/2022-ponzi-schemes
FBI	$3.310 bn		Online investment scams	FBI (2022)
Unreported	$3.310 bn			Own estimate
Total	$20.380 bn			

B Wincapita Ponzi scheme - Additional information

B.1 Details of Wincapita's operation

Based on its returns, the scheme can be characterized as a get-rich-quick scheme. The sole perpetrator behind the scheme, Hannu Kailajärvi, kept up an elaborate charade and led investors to believe the scheme was an international operation with many employees. For example, he registered empty shell companies outside Finland to produce documentation of international operations and he often used made-up names in email responses to falsely imply the scheme had an office somewhere. Details about the operations were considered trade secrets.

Investors managed their investments through the scheme's website, and money was transferred through Moneybookers, a British payment service. Besides the website and the bank account, the scheme had no actual operations. Money was only paid out based on request. Whenever investors withdrew money, it was paid out of the Moneybookers account to which investors made their investments.

According to the rules, the investor would receive 70 percent of the profits earned by his investment, the sponsor was entitled to 20 percent of the profits, and the remaining 10 percent would go to the scheme to cover its expenses. A sponsor's sponsor was not entitled to any part of the 20 percent earned by the sponsor, so the scheme was not a pyramid scheme, in which investors' order of joining determines their payoffs. Sponsors additionally received 200 euros on their account for each sponsored investor.

Any investor could sponsor new members, but they were not required or expected to do so. The scheme's rules required the investor to personally know anyone he invited to join. The scheme did not want any publicity, and its rules explicitly forbade any public distribution of information. The content on the website was only available for members who logged in with their username.

When the first public allegations about Wincapita being a Ponzi scheme leaked in September 2007, Finnish authorities did not act against the scheme because little information was publicly available, and no one had suffered any losses yet.

B.2 International comparison

In an international comparison, Wincapita was large, but not exceptional based on the amount invested or the number of victims. According to a database available at Ponzitracker.com, 51 U.S. Ponzi schemes, in which the losses exceeded the combined value of Wincapita investments, were sentenced in court between 2008 and 2013.[26] Wincapita investors constituted 0.2 percent of the Finnish population, but globally, some schemes have an even higher participation rate in a single country. For example, Albanian Xhafferi scheme, which collapsed in 1997, had over 1.1 million depositors, accounting for a third of the country's population (Jarvis, 2000).

Deason, Rajgopal, and Waymire (2015) provide statistics on the characteristics of 376 Ponzi schemes prosecuted by the SEC. The average scheme had 3,127 investors and a duration of 4.3 years. Over half of the schemes were run by sole perpetrators, as was the case with our scheme. Wincapita's purported returns of 300–400 percent over a six-month period are at the high end of the distribution, but not exceptional: the mean value of maximum promised return in the Deason et al. (2015) sample is 437 percent with a median of 25 percent.

Wincapita's sponsoring system made the scheme diffuse solely through personal recommendation (Rantala, 2019). Similar arrangements are not uncommon: at least 8 percent of the schemes studied by Deason et al. (2015) used referral rewards and 27 percent used commissioned recruiters. The true fractions may be higher, because these characteristics are not always described in the SEC documents. More generally, social diffusion of investment scams is not limited to only Ponzi schemes, and fraud victims are often introduced to an investment opportunity through their social networks. In a study by FINRA (2013), 34 percent of fraud victims report a friend introduced them to the salesperson, 16 percent were introduced through a social contact, and 8 percent through a social setting.

Survey evidence from DeLiema et al. (2017) indicates Ponzi schemes represent a significant portion of all investment fraud. Among all investment-fraud victims, 42 percent believed their investment was part of a Ponzi scheme and another 21 percent were unsure.

[26]https://www.ponzitracker.com/ponzi-database. Data downloaded on September 24, 2019. Years are defined by the date of charges filed. Only limited data are available for other years. Wincapita's investments amount to approximately 100 million euros, which translates into $147 million at the 2008 exchange rate.

B.3 Excerpts from police interviews

Table IA.2 lists excerpts from police interviews that are informative about victims' beliefs and the effect of the scheme on victims' lives. We note that the police did not ask about the effects of the scheme and therefore the excerpts are based on side remarks that the victims made when answering other questions.

Table IA.2
Excerpts from police interviews

The table lists excerpts from the police interviews of the scheme's investors. We list investors' quotes translated into English in italics and added explanations in square brackets. Panel A presents excerpts related to the effects of the scheme during its active years. Panel B lists excerpts that mention the psychological effects of the scheme collapse and Panel C presents excerpts that mention the financial stress triggered by the scheme collapse.

Panel A: Beliefs and effects of the scheme during active years

1	*I stopped looking for new work projects last winter because I was expecting to receive income from Wincapita in the future. Currently, I am living with borrowed money and actively looking for new projects.*
2	*I was expecting to receive the projected return on the capital I invested and thus become free of all financial constraints. I was feeling like someone who won the lottery.*
3	*My trust in the scheme increased with its age.*
4	*When the scheme then described their foreign exchange trading with massive return expectations, I considered the situation for a while but, in the end, I decided to join despite the risks. I thought that I would never have an opportunity to reach a better position in life if I didn't invest with risk. The big return expectations of course lured me in.*
5	[A victim explaining how he used money withdrawn from the scheme:] *I bought myself a new car. I have also been able to live more freely. I have for example gone to a Formula 1 race in Monaco, done a fishing trip in Russia, and visited Budapest with my family using the money withdrawn from Wincapita. I have also been able to take extra parental leave, which was possible thanks to the money from the scheme.*
6	*The money I withdrew from the scheme secured my personal finances to the extent that I did not have to use my retirement income to cover my everyday expenses. This allowed me to accumulate savings in my accounts. I used the withdrawn funds to partially finance the purchase of a new house. The rest of the purchase price was financed with a mortgage and the proceeds from the sale of my previous home. I also used the funds to partially fund the purchase of a car.*
7	*I used all the money I got out for travel and other things. I spent all the money I received. Maybe even a little bit more.*
8	*After I joined the Winclub/Wincapita system, I used the money I got out to fund my general life expenses.*
9	[A victim describing the reasons behind the investment decision:] *My own past experiences from stocks, options, and other security investments proved that it is very difficult for an ordinary person to succeed on their own in the long run in a stock market that is going up and down. For someone who is working, there is not enough time for sitting at the computer all the time and following stock market movements and tips. Additionally, after high-reputation banks and their brokerage arms had caused significant financial damage to our family's small business by purposefully taking actions against their client's instructions, I started researching other investment alternatives. One of my long-term business partners invests in gold, another one in apartment units, and a friend of mine invests in property development, but none of these investment types interested me.*

Panel A: Beliefs and effects of the scheme during active years, cont.

10	[A person who withdrew 250,000 euros and invested it in other online investment schemes describing the withdrawals. The person apparently also lost the money he put in these other investments.] *With these other investment activities, I wanted to diversify my Wincapita gains. It just so happened that I lost the money in all of these investments.*
11	*My financial situation changed radically for the better in 2007 when I started receiving income from Wincapita.*
12	*My financial situation is weak right now. I have been living a very luxurious life for the past three years based on the profits from Wincapita. The situation has got worse after the profits from Wincapita stopped. A financially inexperienced person like me can spend more than he earns.*
13	*My financial situation improved considerably in 2007-2008 when I started withdrawing some of the profits I had earned on my Wincapita investment.*
14	*During the last couple of years my financial situation has become worse because Wincapita ended its operation. I made capital investments in my private business, and I was planning to fund them with income received from Wincapita. I have had to take a bank loan to cover the expenses. My business also moved to a new facility, and its construction costs went up.*

Panel B: Psychological effects of the scheme collapse

15	*When we discussed it* [the collapse of the scheme] *with my son* [another investor]*, he was shocked that the scheme's operations had been illegal. In the beginning, he was even having self-destructive thoughts and I had him stay in our home for a week.*
16	*This* [investment in the scheme] *has caused me a financial loss and the whole process has caused mental suffering.*
17	*The people I sponsored have also suffered financial damage and their mental suffering is even bigger* [than the financial damage].
18	*I lost 48,000 euros in WinCapita. I would of course not have invested in this scheme and helped my friends invest if I had known what its future is going to be. The ability to track the investments in real time convinced me that it is legitimate and got me to invest these large amounts, mainly with borrowed money. Even though none of the people I sponsored lost this much money I am of course very sorry for them. It is shocking to lose 48,000 euros but even 1,000 euros is a big amount.* [The invested amount was rounded to 48,000 euros so that the person is not identifiable.]
19	[Comment from an investor who had convinced his mother to invest and sponsored her:] *My father does not know about this issue* [the victim's mother's investment in the scheme] *and he should never find out about it. If my father finds out, my parents may get divorced.*
20	*This* [Ponzi investment] *has caused me significant mental suffering and financial losses.*

Panel B: Psychological effects of the scheme collapse, cont.

21	*When they started discussing the scheme in the media, it was hard for me to face the content of the discussions and the suspicions about the scheme's legitimacy. I had invested a lot of money in the scheme, and it was mentally impossible to follow the news and other discussions about it. For example, when they were discussing it on the TV news, I had to change the channel to prevent getting more depressed.*
22	*If I had known that the operation is not honest and only causes financial damage, I never would have invited my wife and brother-in-law to invest in Winclub* [earlier name of Wincapita].
23	*March to December 2008* [time between the scheme's collapse and the interview] *has been a shocking and very hard time period for me both financially and otherwise. I am annoyed and regretful of my naivety. I cannot understand how I could get involved with something like this. Even though I did not make my decision on a whim, I was definitely too trusting. Stupidly trusting. Luckily my everyday life does not depend on the money I invested, even though it* [the loss] *has definitely made life more difficult for me.*

Panel C: Financial stress triggered by the scheme collapse

24	*My current situation is catastrophic. I bought an apartment with a mortgage and my plan was to cover mortgage payments with money withdrawn from Wincapita. Before Wincapita I was doing well with the income I received from side jobs I had while studying.*
25	*Even though I only invested less than 5,000 euros in Wincapita, it caused serious financial difficulties for me. This, together with other financial factors, has caused my financial situation to not be very good right now.*
26	*At this point of the investigation, I absolutely think that I am a crime victim. As a young 20-year-old, I joined this scheme together with my two friends like I described earlier. The consequence has been significant mental suffering and financial losses, which are unreasonable relative to the mistake I made. I never believed or knew that I had joined an illegal scheme.*
27	*As I said earlier, I've had to sell other investments at a loss* [to make up for losses from the Ponzi scheme]. *This whole process has caused mental suffering.*
28	*My financial situation is currently bad because I lost ten years' savings in Wincapita. I am still getting along* [financially].
29	*In April 2008 I was supposed to be able to withdraw 16,000 euros from Wincapita but I didn't get the money. I invested 32,000 euros. It has had a critical effect on my personal financial situation.* [Note: the scheme collapsed in March 2008.]
30	*My standard of living fell* [after the scheme's collapse] *because I had adjusted my spending based on the expectation that I was going to get a lot of money from the scheme.*
31	*My life situation is average. Financially, it's getting worse all the time because all my savings, about 60,000 euros, went into Wincapita.*

Panel C: Financial stress triggered by the scheme collapse, cont.

32	*I have been saving money for a long time to buy my own apartment and I invested all of it in Wincapita.*
33	*I was supposed to receive 18,000 euros from Wincapita in April but it collapsed in February [actual collapse date is in March] and I never received the money. That has caused my personal finances to be quite tight.*
34	*My financial situation is relatively stable, but it has become worse during the last years because internet discussions on Wincapita have made my work as an entrepreneur more difficult.* [Someone who was a sponsor. Presumably, the internet discussions refer to the person by name.]
35	*It is a financially tight time for me right now because of this event.*
36	*We had counted that we could get about 20,000 euros from Wincapita in March, and we would've at least used the money to repay our car loan. It just so happened that the scheme's operation ended before that.*
37	*Before Wincapita, my financial situation was good and it had been like that for a long time. After Wincapita, it has become significantly worse.*

Table IA.3
Lag to sponsor entry year by cohort

The table provides summary statistics on the lag between investor entry year and the entry year of their sponsors. *P(Sponsor link available)* shows the fraction of investors in a cohort for whom we observe their sponsor. *Sponsor entry year* is the average entry year of the sponsors. *Lag to sponsor entry* is the difference between cohort year and entry year of sponsors.

Cohort	P(Sponsor link available)	Sponsor entry year	Lag to sponsor entry
All cohorts	0.64	2005.43	1.14
2003	0.66	2003.00	0.00
2004	0.72	2003.51	0.49
2005	0.76	2004.11	0.89
2006	0.66	2004.83	1.17
2007	0.58	2005.92	1.08
2008	0.66	2006.41	1.59

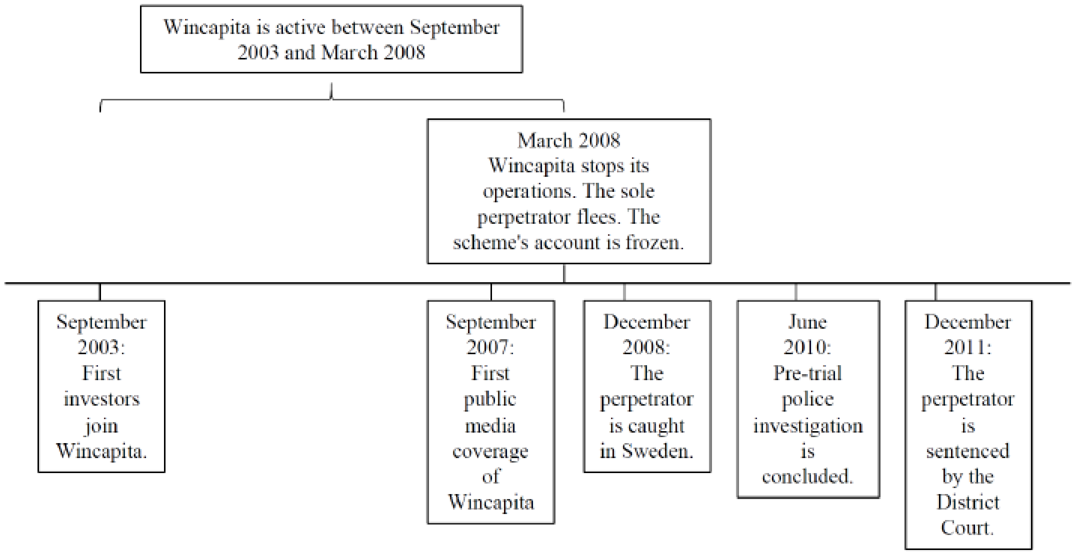

Figure IA.1. Timeline of the scheme's operation and events following its collapse
The figure shows the timeline of the most important events during and after the collapse of the Wincapita Ponzi scheme.

C Wealth loss imputation

C.1 Imputation procedure

To facilitate economic interpretation of the labor income response, we impute investors' account balances during the scheme operation. The scheme perpetrator destroyed all records when he fled Finland, but we can use the information collected during the police interviews to impute approximate account values. We observe the first investment year i_1, the last investment year i_T, the first withdrawal year w_1, and the last withdrawal year w_T, the first investment amount $invested_{i_1}$, the total investment amount *invested(total)*, and the total withdrawn amount *withdrawn(total)*.

We impute investor balances recursively over an investment horizon discretized to five periods. The first investment period ($t = 2003$) is from November 2003 (the start of the scheme) to June 2004, the last period ($t = 2007$) from July 2007 to February 2008 (the scheme's collapse), and the remaining three periods span four years in between.

Treatment of investments. We assign the first investment amount to the investment period that starts in the first investment year and distribute the remaining investment amount equally over the subsequent periods until the last investment year.

$$invested_t = \begin{cases} invested_{i_1}, & \text{if } t = i_1 \\ \frac{invested(total) - invested_{i_1}}{i_T - i_1}, & \text{if } t \in (i_1, i_T] \\ 0, & \text{otherwise} \end{cases}$$

Treatment of withdrawals. We assign withdrawals equally between the first and last withdrawal year.

$$withdrawn_t = \begin{cases} \frac{withdrawn(total)}{w_T - w_1 + 1}, & \text{if } t \in [w_1, w_T] \\ 0, & \text{otherwise} \end{cases}$$

For each investment period, we then calculate the end and beginning values as:

$$beginning_t = \max\{0, end_{t-1} + invested_t - withdrawn_t\},$$

$$end_t = beginning_t R^{l_t},$$

where R is the gross annual rate of return, and l_t is the length of the investment period in years. We set $R = 4$ based on the rates of returns reported by the investors. We calculate the final account balance at the scheme's collapse as

$$end_{2008} = \max\{0, end_{2007} + invested_{2008} - withdrawn_{2008}\}.$$

Wealth loss. We define wealth loss at the scheme's collapse as the final account balance plus any withdrawals that had to be returned to the state

$$loss = end_{2008} + \max\{0, withdrawn(total) - invested(total)\}.$$

For approximately 280 investors, missing or inconsistent data prevent the calculation of wealth losses. For these investors, we impute the wealth loss based on the median wealth loss in the cohort.

D Additional results, tables, and figures

D.1 Labor income response and certainty equivalent windfall gain

To interpret the economic magnitude of the labor income response to joining the scheme, we compare our income response to those reported by lottery studies. We cannot calculate the marginal propensity to earn (MPE) in our setting because joining the scheme affects both investors' wealth as well as their expectations about returns. Instead, we use the income effects reported in the literature to derive the certainty equivalent windfall gain corresponding to the income response we observe.

We focus on two studies that are most related to our context. Cesarini et al. (2017) find relatively low labor supply responses to lottery winnings in Sweden (MPE of -0.17 to -0.04) whereas Golosov et al. (2023) find comparatively larger responses in the U.S. (MPE of -0.5). While the Swedish context may be institutionally and culturally more similar to Finland, we take the estimates from the U.S. as a lower bound of the range of responses documented by the literature. Imbens et al. (2001) find an MPE comparable to Cesarini et al. (2017) in a survey of lottery winners in Massachusetts.

Specifically, we scale the labor income response to joining the scheme in Figure 1 with reduced-form effects on pretax earnings in Cesarini et al. (2017) (1.1 percent of the prize amount per year) and IV estimates of annual labor earnings effects in Golosov et al. (2023) (2.3 percent of the prize amount per year). For example, the labor income response of $-1,458$ euros in event year three corresponds to 63,391–132,545 windfall gain ($1,458/0.023$ and $1,458/0.011$).

D.2 Responses of other variables

Table IA.4 reports the effect of the scheme on additional labor and family outcomes. *Unemployment and sickness benefits* is an indicator for individuals who receive any of these benefits in a year. *Pension income* is an indicator variable for individuals who receive an old-age pension or early pension. The most common reasons for early retirement are a voluntary decrease in labor supply or poor labor market performance. The Finnish pension system allows early retirement due to verified health problems, of which mental health reasons are the second most common. Our data do not report the reason for retirement. *Divorced* is an indicator variable for investors who divorced. The variable equals one for individuals who are divorced at the end of the year and zero for individuals who are married, have never married, or have remarried after a previous divorce.

Mirroring the results from Section 4.1, we find that the scheme investors are more likely to receive unemployment and sickness benefits. Their take-up increases by two percentage points (t-value 3.05), which corresponds to 12 percent of the pre-treatment mean. Investors also increase their take-up of pension benefits by one percentage point (t-value 2.14), which corresponds to 8 percent increase compared to the pre-treatment mean.

Divorces increase on average by 1.2 percentage points over the nine-year period (t-value 2.39), which is consistent with negative wealth shocks lowering the expected gains from marriage (Becker et al., 1977). The economic magnitude of the coefficient is large. It corresponds to 2.1 percent of the pre-period married fraction and a ten percent increase relative to the pre-period divorced fraction. It is also significantly larger than the existing estimates of divorce responses to positive wealth shocks (Hankins and Hoekstra, 2011; Golosov et al., 2023). This finding complements and extends recent studies linking financial shocks and constraints to family outcomes. Goodman, Isen, and Yannelis (2021) show that credit constraints affect marital formation and Lin and Pursiainen (2023) show that shocks to portfolio value affect marital well-being.

Table IA.4
Effects on other labor and family outcomes

The table provides estimates of the effect of participation in the scheme on investors' labor and family outcomes based on difference-in-differences regression (7). The sample consists of 2,580 investors and their matched controls described in Section 3.2 and covers three years prior to joining the scheme and two years (short-term) or nine years (long-term) after its collapse. The dependent variables are indicator variables for individuals receiving unemployment and sickness benefits, individuals receiving pension income, and divorced individuals, defined in Section D.2. Each row corresponds to a separate regression. The last column presents the mean value of the outcome variable for investors in the period before joining. The reported t-statistics are based on standard errors clustered at the strata level.

	δ	t-stat	N	Adj. R^2	Pre-mean
Unemployment and sickness benefits					
Long-term	0.020	(3.05)	899,457	0.27	0.170
Short-term	0.017	(2.12)	380,715	0.29	0.170
Pension income					
Long-term	0.010	(2.14)	899,457	0.73	0.129
Short-term	0.008	(1.77)	380,715	0.79	0.129
Divorced					
Long-term	0.012	(2.39)	899,457	0.79	0.121
Short-term	0.010	(2.40)	380,715	0.84	0.121

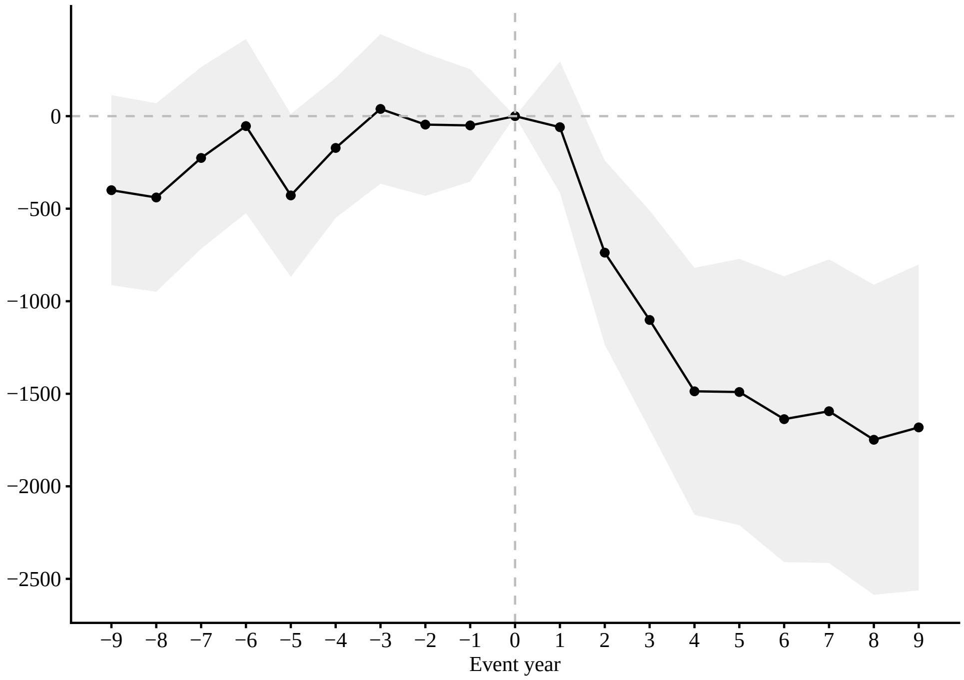

Figure IA.2. Labor income response relative to year before joining with longer pre-period
The figure plots event-year coefficients, δ_n, from estimating regression (1). The dependent variable is annual labor income in euros. The sample consists of 2,580 fraud investors and their matched control group described in Section 3.2 and covers annual observations from 1993 to 2016. The vertical dashed line depicts the year before an individual joined the scheme. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

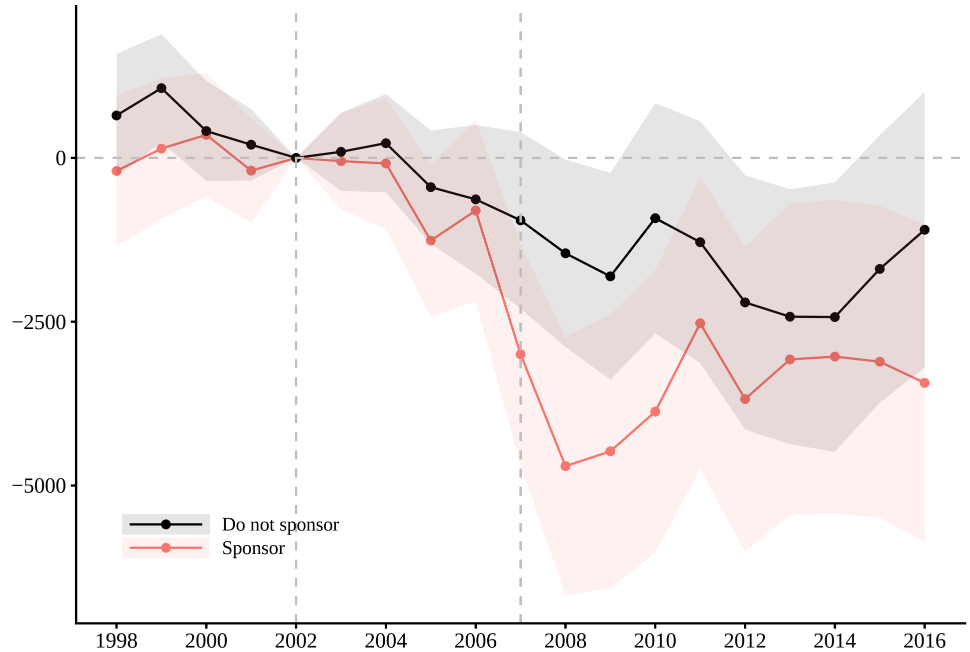

Figure IA.3. Labor income responses by sponsoring

The figure plots event-year coefficients, δ_n, from estimating regression (2) separately for investors who sponsored at least one another investor and those who did not sponsor anyone. The dependent variable is annual labor income in euros. The sample covers cohorts 2003–2006 and their matched control group described in Section 3.2. The first vertical dashed line depicts the year before the first investors joined the scheme. The second dashed line depicts the last year before the scheme collapse. Gray areas represent 95 percent confidence intervals based on standard errors clustered at the strata level.

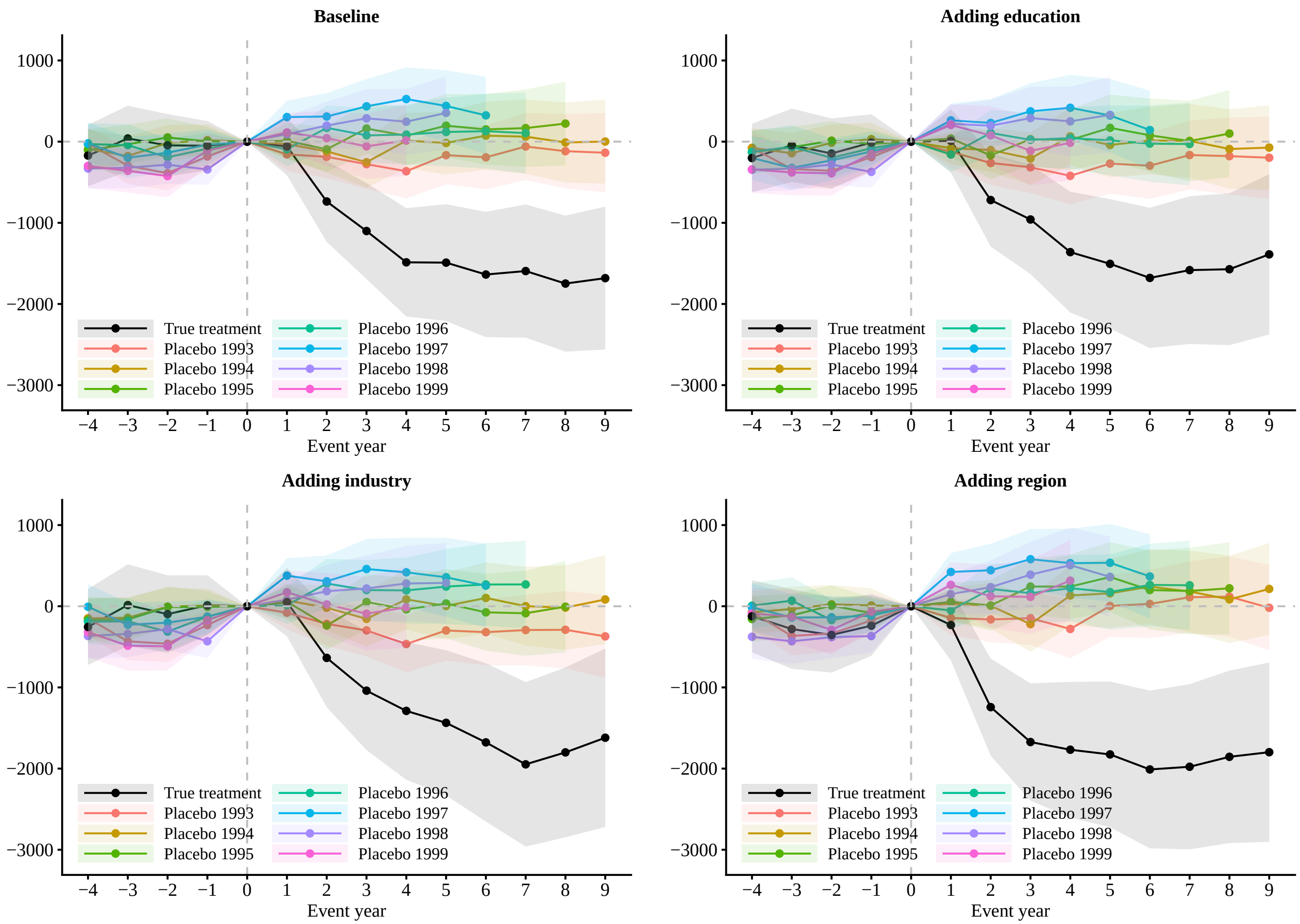

Figure IA.4. Placebo tests with alternative matching specifications

The figures plot event-year coefficients, δ_n, from estimating regression (1) using either the actual year of joining ("True treatment") or assigning the placebo year of joining to 1993–1999, respectively. The baseline specification is equivalent to Figure 6, Panel A. The alternative matching specifications add education categories, industries, or regions. The vertical dashed line depicts the year before joining the scheme.

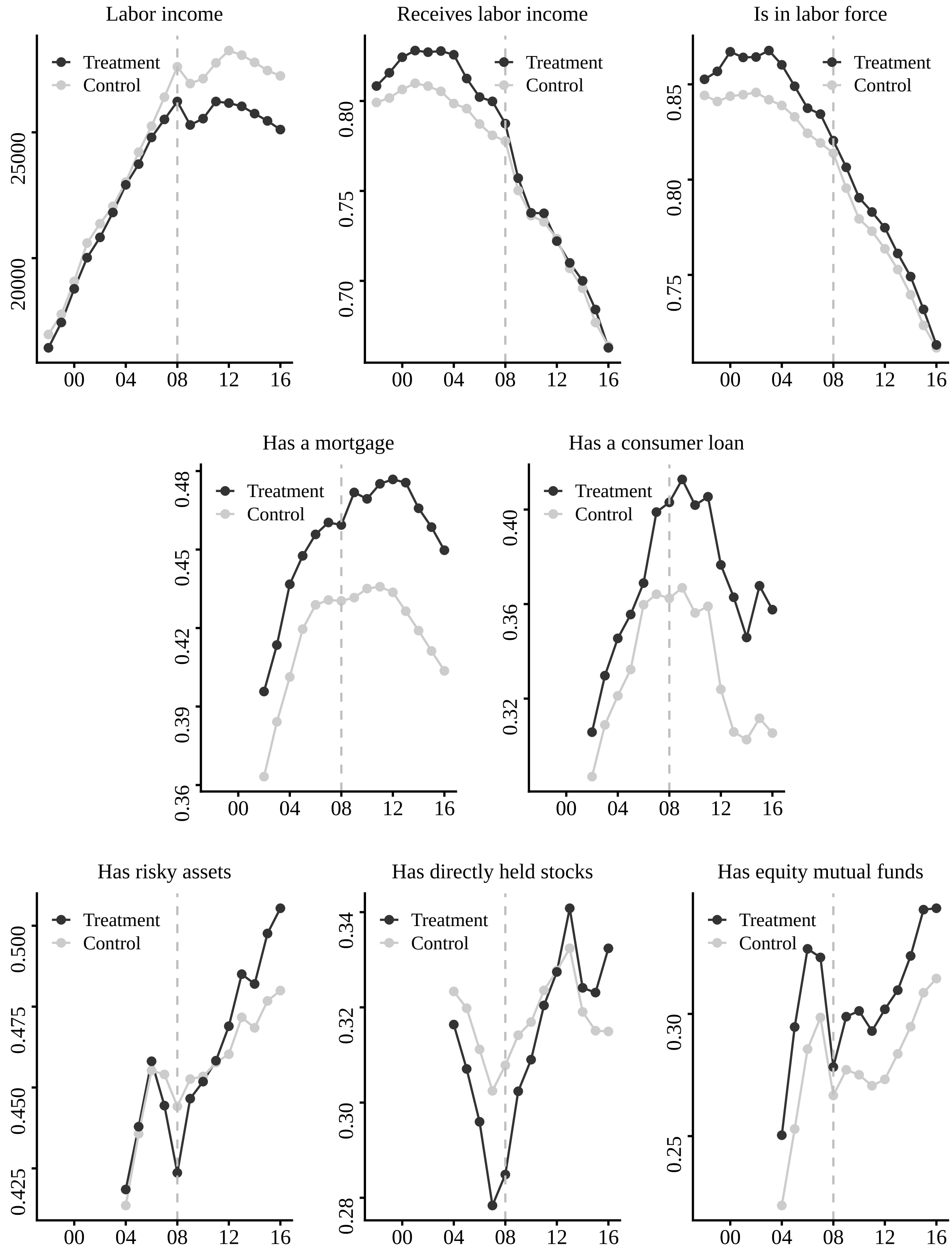

Figure IA.5. Levels of outcome variables for treatment and control groups in calendar time
The figures plot the levels of outcome variables in calendar time for treatment and control groups. The vertical dashed line depicts the first post-collapse year, 2008. The outcome variables are defined the same way as in Figures 3, 7, and 8, and 9. The period is 1998–2016 except for mortgages and consumer loans, which are available from 2002, and financial asset holdings, which are available from 2004.

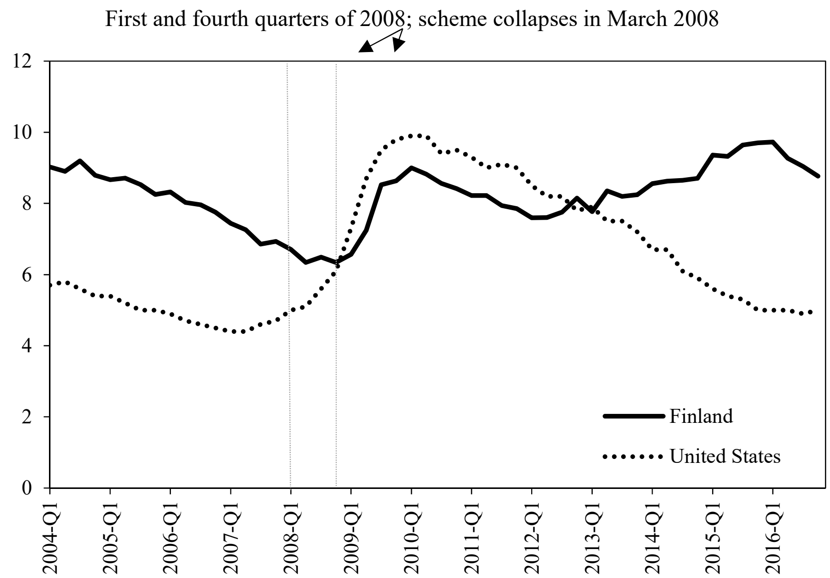

Figure IA.6. Unemployment rate in Finland and the United States, 2004-2016

The figure plots the beginning-of-quarter values of the seasonally adjusted unemployment rate (in percent) in Finland and the United States. These data are retrieved from the FRED Economic Data platform available at https://fred.stlouisfed.org/. The vertical lines highlight the first and fourth quarter of 2008, the first year of our post-collapse period.

Table IA.5

Regressions explaining participation in the Ponzi scheme and in ordinary risky investments

This table reports results from regressions in which the dependent variable is an indicator for participation in the Ponzi scheme or for participation in ordinary risky investments. Risky-investment participants are defined as those who own stocks or equity mutual funds in 2004. The sample is the entire Finnish adult population in columns (1) and (3), whereas columns (2) and (4) restrict the sample to males with available cognitive ability scores. The explanatory variables are dummies for the nine highest income deciles, dummies for education-level categories, dummies for the four highest IQ quintiles (omitted in columns (1) and (3)), dummies for birth year cohorts, and a dummy for females (omitted in columns (2) and (4)). Income and education are measured in 2002. The coefficients are multiplied by 100 for readability. Robust t-statistics are reported in parentheses.

	Participation in Ponzi scheme		Participation in risky assets	
	(1)	(2)	(3)	(4)
Income decile				
2	0.020	0.053	-0.738	2.06
	(3.64)	(2.42)	(-9.19)	(9.05)
3	0.025	0.042	0.417	3.37
	(4.63)	(1.75)	(4.84)	(12.9)
4	0.030	0.068	3.85	4.38
	(5.27)	(2.55)	(42.8)	(16.0)
5	0.034	0.105	7.77	4.91
	(5.70)	(3.75)	(83.9)	(18.5)
6	0.027	0.081	10.7	5.95
	(4.54)	(3.09)	(113.5)	(22.9)
7	0.033	0.055	13.7	7.58
	(5.22)	(2.15)	(141.3)	(29.3)
8	0.040	0.072	18.0	10.2
	(5.91)	(2.81)	(179.3)	(39.5)
9	0.073	0.123	23.9	15.4
	(9.67)	(4.59)	(226.7)	(57.8)
10	0.150	0.188	37.6	27.5
	(16.1)	(6.12)	(330.1)	(93.3)
High school or vocational education	0.023	0.027	6.48	6.58
	(7.32)	(1.75)	(140.0)	(54.3)
Bachelor's degree	0.023	0.036	12.6	16.3
	(2.96)	(1.22)	(121.4)	(61.2)
Graduate degree	-0.029	-0.042	18.3	25.7
	(-3.85)	(-1.51)	(170.0)	(90.8)
IQ quintile				
2		0.011		2.04
		(0.679)		(14.7)
3		0.052		4.82
		(2.94)		(32.7)
4		0.020		7.04
		(1.19)		(45.2)
5		-0.010		10.6
		(-0.538)		(61.7)
Birth year fixed effects	✓	✓	✓	✓
Female fixed effects	✓		✓	
Observations	4,155,601	611,624	4,155,601	611,624
Adjusted R^2	0.0007	0.0002	0.1098	0.0955

Table IA.6
Covariate balance with alternative matching specifications

This table reports the covariate balance for the three alternative matching specifications. Panel A reports the match that adds categories of education to the set of covariates. Panel B adds industry categories. Panel C adds regions. See Section 3.6 for details.

Panel A: Adding education

	Investors (N=2,197)		Controls (N=37,757)		Investors − Controls	
	Mean	SD	Mean	SD	Mean	t-stat
Earned income, €1,000	31.66	23.24	32.11	27.22	-0.45	-0.82
Capital income, €1,000	7.18	34.17	6.20	70.35	0.98	1.04
Age	41.28	11.94	41.28	11.94	0.00	
Female	0.17	0.38	0.17	0.38	0.00	
Entrepreneur	0.18	0.39	0.18	0.39	0.00	
Retired	0.09	0.29	0.09	0.29	0.00	
Employed	0.67	0.47	0.67	0.47	0.00	
Unemployed	0.02	0.14	0.02	0.14	0.00	
Has stocks or equity mutual funds	0.46	0.50	0.46	0.50	0.00	
Total income, €1,000	38.84	43.97	38.31	79.16	0.53	0.46
Level of education						
Basic	0.13	0.34	0.13	0.34	0.00	0.00
Secondary	0.67	0.47	0.67	0.47	0.00	0.00
Bachelor	0.10	0.30	0.10	0.30	0.00	0.00
Graduate	0.10	0.30	0.10	0.30	0.00	0.00
Business education	0.12	0.33	0.12	0.33	0.00	-0.27
Finance professional	0.02	0.15	0.03	0.17	-0.01	-1.52
Cognitive ability (if available)	0.03	0.86	0.09	0.93	-0.07	-1.84
Married	0.56	0.50	0.57	0.49	-0.01	-1.01
Divorced	0.13	0.33	0.11	0.31	0.02	2.16
Has financial assets	0.48	0.50	0.48	0.50	0.00	0.21
Has mortgage	0.45	0.50	0.43	0.49	0.02	1.77
Has consumer loan	0.35	0.48	0.37	0.48	-0.01	-1.13

Panel B: Adding industry

	Investors (N=1,894)		Controls (N=25,151)		Investors − Controls	
	Mean	SD	Mean	SD	Mean	t-stat
Earned income, €1,000	32.38	24.31	32.92	30.16	-0.54	-0.86
Capital income, €1,000	7.43	36.46	5.76	44.10	1.68	1.84
Age	41.82	12.51	41.82	12.50	0.00	
Female	0.18	0.38	0.18	0.38	0.00	
Entrepreneur	0.14	0.34	0.14	0.34	0.00	
Retired	0.12	0.33	0.12	0.33	0.00	
Employed	0.67	0.47	0.67	0.47	0.00	
Unemployed	0.02	0.15	0.02	0.15	0.00	
Has stocks or equity mutual funds	0.46	0.50	0.46	0.50	0.00	
Total income, €1,000	39.81	46.56	38.68	56.99	1.13	0.96
Level of education						
Basic	0.15	0.35	0.19	0.39	-0.04	-3.91
Secondary	0.61	0.49	0.55	0.50	0.05	4.07
Bachelor	0.13	0.34	0.12	0.32	0.01	1.26
Graduate	0.12	0.32	0.15	0.35	-0.03	-3.04
Business education	0.12	0.32	0.11	0.31	0.01	1.09
Finance professional	0.02	0.15	0.02	0.15	0.00	-0.39
Cognitive ability (if available)	0.05	0.86	0.14	0.94	-0.09	-2.02
Married	0.58	0.49	0.57	0.49	0.00	0.34
Divorced	0.12	0.32	0.12	0.32	0.00	0.33
Has financial assets	0.48	0.50	0.48	0.50	0.00	0.24
Has mortgage	0.44	0.50	0.42	0.49	0.02	1.46
Has consumer loan	0.35	0.48	0.34	0.47	0.01	0.99

Panel C: Adding region

	Investors (*N*=1,903)		Controls (*N*=21,728)		Investors − Controls	
	Mean	SD	Mean	SD	Mean	*t*-stat
Earned income, €1,000	32.85	24.36	33.46	30.24	-0.61	-0.96
Capital income, €1,000	7.42	36.15	6.52	76.28	0.90	0.85
Age	41.34	12.05	41.34	12.04	0.00	
Female	0.17	0.37	0.17	0.37	0.00	
Entrepreneur	0.16	0.37	0.16	0.37	0.00	
Retired	0.09	0.29	0.09	0.29	0.00	
Employed	0.69	0.46	0.69	0.46	0.00	
Unemployed	0.02	0.13	0.02	0.13	0.00	
Has stocks or equity mutual funds	0.46	0.50	0.46	0.50	0.00	
Total income, €1,000	40.27	46.36	39.98	86.45	0.29	0.22
Level of education						
Basic	0.16	0.37	0.17	0.37	0.00	-0.44
Secondary	0.59	0.49	0.56	0.50	0.03	2.30
Bachelor	0.13	0.33	0.12	0.32	0.01	0.73
Graduate	0.12	0.32	0.15	0.36	-0.03	-3.73
Business education	0.12	0.32	0.11	0.31	0.01	0.84
Finance professional	0.03	0.16	0.03	0.18	-0.01	-1.60
Cognitive ability (if available)	0.04	0.91	0.13	0.96	-0.09	-2.02
Married	0.57	0.49	0.59	0.49	-0.01	-1.00
Divorced	0.13	0.33	0.11	0.31	0.02	2.16
Has financial assets	0.48	0.50	0.48	0.50	0.00	0.32
Has mortgage	0.44	0.50	0.43	0.49	0.01	0.75
Has consumer loan	0.36	0.48	0.35	0.48	0.01	0.66

Table IA.7
Income loss at collapse by cohort

The table provides estimates of income loss in euros in the year of the scheme collapse based on a version of equation (2) where we estimate the equation separately for each cohort and use 2007 as the omitted baseline year. The reported t-statistics are based on standard errors clustered at the strata level.

	$l_{i,c,2008}$	t-stat
2003	-785.6	(-0.45)
2004	-1,583.9	(-2.19)
2005	-973.2	(-1.98)
2006	-707.1	(-1.12)
2007	-591.3	(-1.81)
2008	509.1	(1.23)

Table IA.8
Labor income response to investment fraud including active years

The table provides estimates of the labor income response to the scheme participation based on difference-in-differences regression (7) where the post-period covers all years from joining the scheme. The sample consists of 2,580 investors in baseline matching and their matched controls described in Section 3.2 and covers three years prior to joining the scheme and all (long-term) or two years (short-term) after entry. The alternative matching specifications in addition include education categories, industries, or regions. These specifications cover 2,197, 1,894, and 1,903 investors, respectively. The dependent variable is annual labor income in euros. Each row corresponds to a separate regression. The last column presents the mean value of the outcome variable for investors in the period before joining. The reported t-statistics are based on standard errors clustered at the strata level.

	δ	t-stat	N	Adj. R^2	Pre-mean
Baseline matching					
Long-term	-1,225.3	(-4.20)	1,029,081	0.73	23,743.8
Short-term	-883.8	(-3.82)	510,339	0.83	23,743.8
Adding education					
Long-term	-1,149.2	(-3.58)	542,459	0.73	23,743.8
Short-term	-867.8	(-3.34)	271,082	0.83	23,743.8
Adding industry					
Long-term	-1,151.1	(-3.21)	363,030	0.73	23,743.8
Short-term	-855.6	(-3.07)	183,339	0.84	23,743.8
Adding region					
Long-term	-1,336.1	(-3.70)	315,882	0.73	23,743.8
Short-term	-1,046.2	(-3.77)	155,841	0.83	23,743.8

Table IA.9

Responses on extensive margin and labor force exit including active years

The table provides estimates of the effects of the scheme participation based on difference-in-differences regression (7) where the post-period covers all years from joining the scheme. The dependent variables are indicator variables for individuals receiving labor income and for individuals in the labor force, as defined in Section 4.1. The sample consists of 2,580 investors and their matched controls described in Section 3.2 and covers three years prior to joining the scheme and two years (short-term) or all years (long-term) after entry. Each row corresponds to a separate regression. The last column presents the mean value of the outcome variable for investors in the period before joining. The reported t-statistics are based on standard errors clustered at the strata level.

	δ	t-stat	N	Adj. R^2	Pre-mean
Receives labor income					
Long-term	-0.012	(-1.93)	1,029,081	0.61	0.820
Short-term	-0.009	(-1.56)	510,339	0.69	0.820
Is in labor force					
Long-term	-0.005	(-0.82)	1,029,081	0.59	0.854
Short-term	-0.006	(-1.17)	510,339	0.65	0.854

Table IA.10

Effect of investment fraud on financial outcomes including active years

The table provides estimates of the effect of the scheme participation on investors' financial outcomes based on difference-in-differences regression (7) where the post-period covers all years from joining the scheme. The sample consists of 2,580 investors and their matched controls described in Section 3.2 and covers up to three years prior to joining the scheme and two years (short-term) or all years (long-term) after entry. The dependent variables are indicators for mortgages, consumer loans, risky financial asset holdings, direct stock holdings, and investments in equity mutual funds, defined in Section 4.2. The coverage of mortgages and consumer loans starts in 2002, whereas that for risky financial assets starts in 2004. Consequently, for cohorts 2003 and 2004, the baseline period for risky financial assets is the first year of data (2004). The last column presents the mean value of the outcome variable for investors in the period before joining. The reported t-statistics are based on standard errors clustered at the strata level.

	δ	t-stat	N	Adj. R^2	Pre-mean
Has mortgage					
Long-term	0.018	(2.26)	1,013,980	0.63	0.438
Short-term	0.007	(0.91)	495,238	0.71	0.438
Has consumer loan					
Long-term	0.037	(4.58)	1,013,980	0.47	0.349
Short-term	0.026	(3.29)	495,238	0.56	0.349
Has risky assets					
Long-term	0.002	(0.26)	958,873	0.74	0.459
Short-term	-0.018	(-2.70)	440,131	0.80	0.459
Has directly held stocks					
Long-term	0.008	(1.24)	958,873	0.81	0.317
Short-term	-0.011	(-2.15)	440,131	0.87	0.317
Has equity mutual funds					
Long-term	-0.012	(-1.55)	958,873	0.66	0.314
Short-term	-0.018	(-2.52)	440,131	0.72	0.314

Table IA.11

Long-term income effect in baseline sample vs. population using fuzzy matching

The table provides estimates of the sample bias in the labor income responses based on equation (8). $\hat{\varepsilon}^T$ reports the difference in the difference-in-differences coefficient from equation (7) between the baseline sample and the sample obtained using fuzzy matching. N(investors) reports the number of investors in the sample obtained with fuzzy matching. Each row corresponds to a different matching specification. In the first alternative matching, we match on five-year birth cohorts. In the second specification, we omit gender and indicator of risky asset holdings from the baseline matching specification. In the third specification, we match on vigintiles of total income instead of earned and capital income separately.

Fuzzy-matching specification	$\hat{\varepsilon}^T$	t-stat	N(investors)
Five-year age groups	-140.9	(-1.42)	2,923
Drop risky assets and gender	-146.1	(-1.52)	2,874
Match on total income	-203.4	(-1.84)	2,976

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