Financial Windfalls, Portfolio Allocations, and Risk Preferences*

Joseph Briggs† David Cesarini‡ Sean Chanwook Lee§
Erik Lindqvist¶ Robert Östling‖

Abstract

We investigate the impact of financial windfalls on household portfolio choices and risk exposure. Exploiting the randomized assignment of lottery prizes in three Swedish lotteries, we find a windfall gain of $100K leads to a 5-percentage-point *decrease* in the risky share of household portfolios. We show theoretically that negative wealth effects are consistent with both constant and decreasing relative risk aversion and analyze how our empirical estimates help distinguish between competing models of portfolio choice. We further show our results are quantitatively aligned with the predictions of a calibrated dynamic portfolio choice model with nontradable human capital and consumption habits.

*We thank Jess Benhabib, Jaroslav Borovicka, John Campbell, Andrew Caplin, Samuel Hanson, David Laibson, Sydney Ludvigson, Peter Maxted, Andrei Shleifer, Paolo Sodini, Adi Sunderam, Boris Vallee, Luis Viceira, Gianluca Violante, and various seminar participants for insightful discussion and comments. We also gratefully acknowledge financial support from the NBER Household Finance working group (22-2382-13-1-33-003), the Swedish Council for Working Life and Social Research (2011-1437), the Swedish Research Council (421-2011-2139 and 2022-02686), Handelsbanken's Research Foundations (P2011:0032:1), Riksbankens Jubileumsfond (P15-0514:1), and the Ragnar Söderberg Foundation (E4/17). The analysis and conclusions set forth here are those of the authors and do not necessarily indicate concurrence by the Goldman Sachs Group, Inc.

†Goldman Sachs. E-mail: jsbriggs87@gmail.com.

‡Department of Economics, New York University, IFN, and NBER. E-mail: david.cesarini@nyu.edu.

§Corresponding author. Department of Economics, Harvard University. E-mail: seanclee@g.harvard.edu.

¶Swedish Institute for Social Research (SOFI), Stockholm University, and CEPR. E-mail: erik.lindqvist@sofi.su.se.

‖Department of Economics, Stockholm School of Economics. E-mail: robert.ostling@hhs.se.

1 Introduction

Models of portfolio choice in finance and macroeconomics often differ in their assumptions about risk preferences. A common strategy is to assume preferences with constant relative risk aversion (CRRA). Absent any frictions, models with CRRA utility predict that the share of a household's portfolio invested in risky assets (hereon, the *risky share*) will not depend on the level of wealth (Samuelson, 1969). A variety of modifications to the CRRA assumption have been proposed and explored. For example, decreasing relative risk aversion (DRRA) preferences have been invoked to generate procyclical stock prices and countercyclical risk premia in consumption-based asset pricing (Constantinides, 1990; Campbell and Cochrane, 1999) and to explain the positive cross-sectional relationship between household wealth and the risky share (Wachter and Yogo, 2010; Meeuwis, 2022).

Credible evidence on how the risky share is impacted by financial windfalls would be valuable for distinguishing between competing models. Despite a vast literature, substantial uncertainty remains about the range of plausible effects and estimates reported in the literature vary substantially in both sign and magnitude. For example, Brunnermeier and Nagel (2008) and Chiappori and Paiella (2011) do not find that changes in a household's wealth over time are associated with changes in its risky share. In contrast, Calvet, Campbell and Sodini (2009) and Calvet and Sodini (2014) find positive effects in their analyses of Swedish data using different identification strategies. The apparent lack of consensus about how exogenous wealth shocks impact the risky share remains a roadblock hampering progress in the literature.

The fundamental challenge in identifying wealth effects is that even the most sophisticated quasi-experimental studies conducted to date rely on identifying assumptions that are difficult to stringently evaluate given the available data. Carroll (2002, p. 420) notes that the "ideal experiment" to examine the causal relationship between wealth and risk tolerance would be to "exogenously dump a large amount of wealth on a random sample of households and examine the effect on both their expressed risk preferences and their risk-taking behavior." In this paper, we use a research design that, in the spirit of Imbens, Rubin and Sacerdote (2001), approximates this ideal experiment closer than any previous work. Specifically, we leverage the randomized assignment of lottery prizes among players from three separate Swedish lotteries. The players have been matched to government registers with detailed information about numerous characteristics, including year-end household finances. For each lottery, we observe the factors conditional on which the lottery prizes were randomly assigned and can therefore control for these factors in our empirical analyses. Our study thus has several methodological strengths that bolster the credibility of our causal estimates.

Our headline finding is that among lottery players who owned stocks before the lottery event, a windfall gain of $100K causes the risky share to fall by approximately five percentage points. The effect is immediate, precisely estimated, and appears to persist for at least four years (the longest horizon over which we estimate wealth effects). We show that the negative effect is robust to alternative definitions of the risky share and shows up reliably across all subsamples considered in our heterogeneity analyses. In additional analyses, we find little evidence that the negative effects are explained by (i) risky investments in alternative assets (e.g., real estate) that crowd out some investments in stocks, (ii) household inaction/sluggishness in investing the lottery wealth after it is deposited in the winner's bank account, or (iii) our use of a potentially unrepresentative estimation sample composed of lottery players.

Under the common convention in the literature to interpret the risky share as a proxy for relative risk aversion, our findings would suggest that relative risk aversion *increases* in wealth. However, such inferences are only valid under restrictive assumptions that do not align well with the empirical environment in which households make financial decisions. To clarify the basic argument, we formulate and analyze a static portfolio choice model that incorporates human capital and incomplete markets (Campbell and Viceira, 2002). A key feature of the model is that human capital – defined as the expected present discounted value of all future income – is assumed to be nontradable: agents cannot borrow against anticipated future income flows. We show that this simple model is consistent with negative wealth effects on the risky share even when the underlying risk preferences are CRRA or DRRA. Intuitively, in a model with nontradable human capital, a large windfall mechanically causes the share of a household's total wealth held in human capital to fall. This, in turn, dampens the household's incentives to over-invest in equities to offset its nontradable (and relatively less risky) position in human capital. The household thus reacts by reducing its exposure to equity, leading to a decline in the risky share.

We derive and test three additional predictions of the static model. First, the elasticity of the risky share with respect to a financial wealth shock should diminish (in absolute magnitude) with the size of the shock. Second, the elasticity should be increasing (in magnitude) in the ratio of human capital to pre-existing financial wealth. Third, the effect of an increase in future income (as opposed to an increase in financial wealth) on the risky share should be positive. Our unique empirical setting allows us to exploit variation in prize sizes, pre-win human-capital-to-wealth ratios, and differences in the mode of payment (lump-sums versus monthly installments) to test these predictions. Overall, our evidence aligns quite closely with all three predictions. While previous theoretical work has recognized human capital as an important determinant of asset allocations (Bodie, Merton and Samuelson, 1992; Heaton and Lucas, 1997; Viceira, 2001) and has inspired the development of so-called target-date funds (Parker, Schoar and Sun, 2022), empirical evidence consistent with the human-capital channel in the previous literature is relatively scant.

In further analyses, we find that a richer, dynamic portfolio choice model yields predictions consistent with our quasi-experimental estimates.[1] Under a standard calibration, our baseline model with nontradable human capital predicts that a $100K windfall causes the risky share to decline by over 10 percentage points, a reduction twice the size of our preferred quasi-experimental estimate. Extending the model to incorporate realistic consumption habits, as in Campbell and Cochrane (1999) and Polkovnichenko (2007), results in a predicted reduction of 5.2 percentage points per $100K won, closely matching the quasi-experimental estimate. Thus, the negative wealth effect we report is both qualitatively and quantitatively consistent with predictions from a broad class of models that incorporate human capital and incomplete markets.

Even though our results point to nontradable human capital as a potentially important mechanism, our results do not contradict previous studies that emphasize the significance of consumption habits and DRRA preferences. For example, the lifecycle model that best matches our quasi-experimental estimates allows for both consumption habits and nontradable human capital. This finding suggests that any model that incorporates only one of these factors is likely to be misspecified. Accordingly, inferences about risk preferences from the financial wealth elasticity of the risky share are more credible when they are based on an augmented model that adequately accounts for both nontradable human capital and consumption habits in a quantitative manner.

Finally, our paper contributes to the broader literature on how wealth impacts households' saving and investment behaviors.[2] Our rich data allows us to characterize the dy-

[1] Our model is based on Gomes and Michaelides (2005) and Cocco, Gomes and Maenhout (2005), and is comparable to the models used in other quantitative studies of household portfolio choice, including Fagereng, Gottlieb and Guiso (2017) and Calvet, Campbell, Gomes and Sodini (2021).

[2] There is an active literature studying the effect of wealth on household finances. In recent work, Andersen and Nielsen (2011) and Briggs, Cesarini, Lindqvist and Östling (2021) estimate the effect of wealth on stock market participation, Hankins, Hoestra and Skiba (2011) estimate the effect on bankruptcy, Cookson, Gilje and Heimer (2022) estimate the effect on household debt, Paiella and Pistaferri (2017) and Fagereng,

namic effects of lottery wealth on multiple asset and liability classes up to four years after the lottery. To set the stage for these analyses, we first analyze how winning the lottery impacts a player's year-end net wealth. These estimates provide potentially informative bounds on the marginal propensities to spend and save.[3] We find that a $100K windfall increases year-end net wealth by approximately $60K in the year of win, with the effect dissipating to $35K four years later. Next, we decompose the estimated increase in net wealth into three components: financial assets, real assets, and debt. We find that 80-90% of the estimated increase in net wealth is explained by an increase in financial assets, with the remaining share accounted for by real estate investments and reductions of debt. Lastly, we decompose the estimated effect on financial wealth into five subcomponents: equities, bonds, interest funds, bank account balances, and other. These analyses provide information about households' marginal propensities to invest in specific assets and to take risk. Our results suggest that investments in risky assets account for a relatively modest share of the total increase in financial wealth, with point estimates ranging from 14% to 31%. The causal estimates presented in our study could be further utilized in future work to test and refine different theories of investment and saving behavior.

The organization of the paper is as follows. Section 2 presents our simple, static, portfolio choice model and analyzes its predictions. Section 3 describes our data, the construction of our estimation sample, and our identification strategy. Section 4 presents our reduced-form empirical results and robustness analyses. Section 5 introduces a dynamic lifecycle model of portfolio choice and compares the model predictions with our empirical estimates. Finally, Section 6 discusses the implications of our findings and concludes.

2 A Simple Model of Portfolio Choice

This section examines a static model of portfolio choice that follows Campbell and Viceira (2002).[4] In a baseline model with CRRA, we examine the main implications of nontradable labor income for a household's portfolio allocation decision. We analyze how a windfall gain impacts the risky share and derive four predictions that we evaluate empirically in Section 4. A key insight is that in the presence of nontradable labor income,

Holm and Natvik (2021) estimate the effect on consumption, and Juster, Lupton, Smith and Stafford (2006) and Druedahl and Martinello (2020) estimate the effect on saving.

[3] While we can accurately estimate the saving response, we can only bound the spending response because we do not fully observe transfers, donations, and durable purchases in administrative tax records.

[4] See Chapter 6 of Campbell and Viceira (2002) for additional details and derivations.

estimates of how exogenous wealth shocks impact the risky share may be less informative about risk preferences than the prior literature has tended to acknowledge. We later extend the model to incorporate consumption habits and show that a similar conclusion holds in a model with DRRA. Since the primary purpose of this section is to clarify key mechanisms and concepts in the simplest possible way, the model is purposefully kept simple. In Section 5, we examine to what extent the key insights and conclusions from the static model hold in a richer, dynamic setting.

Baseline Model

Consider an agent who must allocate some fraction $\alpha \in [0,1]$ of initial financial wealth W_0 into a risk-free asset that yields a guaranteed return R_f, with $r_f = \log(1+R_f)$, and the remaining fraction $(1-\alpha)$ into a risky asset that yields a stochastic return R that is log-normally distributed with expected excess return $\mathbb{E}[r-r_f] = \mu$ and variance σ_r^2. The agent makes the allocation decision knowing that her terminal wealth will be the sum of the realized value of her financial assets and the realized value of her non-negative stochastic labor income H, where $h = \log(H) \sim N(\eta, \sigma_h^2)$. After the allocation decision is made, the value of the labor income is realized, along with the return on the risky investment. The agent then consumes her terminal wealth. We allow for arbitrary correlation between the risky labor income and the return on the risky asset, denoting their covariance by $\sigma_{h,r}$, and assume that the agent cannot borrow against future income to pay for the initial investments: human capital is nontradable. The agent's optimization problem is to maximize the expected utility of her consumption,

$$\max_{\alpha \in [0,1]} \mathbb{E}\left[\frac{C^{1-\gamma}}{1-\gamma}\right], \tag{1}$$

subject to the budget constraint,

$$C = W_0(1+R_p) + H, \tag{2}$$

where γ is the coefficient of relative risk aversion and $R_p = \alpha(R - R_f) + R_f$ denotes the portfolio return on total financial wealth. The optimal portfolio rule is given by:

$$\alpha^{\star} \approx \underbrace{\left(1 + \overline{H/W}\right)}_{\text{Upward adjustment}} \left(\frac{\mu + \sigma_r^2/2}{\gamma\sigma_r^2}\right) - \underbrace{\overline{H/W}\left(\frac{\sigma_{h,r}}{\sigma_r^2}\right)}_{\text{Hedging demand}}, \tag{3}$$

where $\overline{H/W}$ captures the ratio of expected human capital to expected financial wealth.[5] As a special case, without nontradable labor income, the optimal share is $\alpha^{\star} \approx \frac{\mu+\sigma_r^2/2}{\gamma\sigma_r^2}$, which is independent of the level of wealth (Samuelson, 1969).

Equation (3) highlights the factors that affect portfolio choice in the presence of nontradable labor income. The first term represents the optimal share when labor income risk is idiosyncratic and uncorrelated with the risky asset. The optimal portfolio share absent labor income, $\frac{\mu+\sigma_r^2/2}{\gamma\sigma_r^2}$, is distorted by the nontradability of labor income. Because future labor income is a source of positive wealth and is relatively safe compared to equity investments, the agent increases her equity exposure to offset this nontradable position, with the size of the distortion increasing in the ratio of human capital to financial wealth. The second term reflects the hedging demand arising from the covariance between labor income and the risky asset. For example, a negative covariance ($\sigma_{h,r} < 0$) makes equity investments more attractive as they help the agent hedge her consumption risk against bad realizations of labor income. Consequently, a negative covariance increases the risky share by $-\overline{H/W}\left(\sigma_{h,r}/\sigma_r^2\right)$, with the size of the adjustment increasing in the ratio of human capital to financial wealth.

While the direction of the total impact of nontradable labor income on the risky share depends on the sign of the covariance term $\sigma_{h,r}$, the analyses in Section 5.2 show that as an empirical matter, $\frac{\mu+\sigma_r^2/2}{\gamma\sigma_r^2} \gg \mid \frac{\sigma_{h,r}}{\sigma_r^2} \mid \approx 0$. That is, the covariance between equity returns and labor income is very small, both in absolute value and relative to the first term. Thus, the overall impact of nontradable labor income on the risky share is always positive.

[5] More formally,

$$\overline{H/W} \equiv \frac{\exp\{\eta\}}{\exp\{w_0 + r_p\}}, \tag{4}$$

where $\eta = \mathbb{E}[\log(H)]$ denotes expected log labor income, $w_0 = \log(W_0)$ denotes log initial wealth, and $r_p = \mathbb{E}[\log(1 + R_p)]$ denotes expected log return on wealth. See Campbell and Viceira (2002) for derivation.

2.1 Model Predictions

Equation (3) can be used to derive a clear, qualitative prediction of how a change in financial wealth impacts $\alpha^\star$. For simplicity, we set $\sigma_{h,r} = 0$ for the purpose of deriving model predictions.

Model Prediction 1. *The risky share decreases after a financial wealth shock.*

Analytically, the prediction follows from the fact that the derivative of $\alpha^\star$ with respect to W_0 is everywhere negative. The main intuition behind this result is that the windfall gain reduces the proportion of total wealth held in nontradable human capital. As a result, the household's incentive to offset its nontradable position in future labor income through equity investments is weakened, leading to a decline in equity exposure.

Next, we turn to three additional testable predictions made by the model.

Model Prediction 2. *The elasticity of the risky share with respect to a financial wealth shock diminishes (in absolute magnitude) with the size of the shock.*

Model Prediction 3. *The elasticity of the risky share with respect to a financial wealth shock is larger (in magnitude) when the human-capital-to-financial-wealth ratio is higher.*

Model Prediction 4. *The risky share rises after an exogenous increase in future income.*

Prediction 2 follows from the fact that the second derivative of the portfolio share with respect to financial wealth is everywhere positive. Because the portfolio distortion is hyperbolic in financial wealth, the marginal effect of a dollar-increase becomes smaller as the size of the shock becomes larger. Prediction 3 follows from the sign of the cross-partial with respect to financial wealth and the human-capital-to-financial-wealth ratio. Intuitively, the financial wealth elasticity of the risky share depends on the baseline level of distortions created by the nontradability of human capital. When the initial distortion is large due to a high human-capital-to-financial-wealth ratio, the financial wealth shock induces a stronger unraveling of this distortion, which generates a larger effect. Lastly, Prediction 4 follows from the first derivative of the portfolio share with respect to future income. The intuition for this result is similar to the one underlying Prediction 1 but in the opposite direction. In Section 4, we empirically test each of the four predictions in our sample of Swedish lottery players.

2.2 Incorporating Habit Formation

Even though human capital is a key determinant of households' portfolio choices (Bodie et al., 1992; Viceira, 2001; Benzoni, Collin-Dufresne and Goldstein, 2007), much of the focus in the empirical literature has been on models with habit-formation preferences. Here, we consider an extension of our model that incorporates consumption habits.[6] We denote the consumption habit by $X > 0$ and define surplus consumption as $C_h \equiv C - X$. The optimization problem can then be expressed as:

$$\max_{\alpha_h \in [0,1]} \mathbb{E}\left[\frac{C_h^{1-\gamma}}{1-\gamma}\right] \tag{5}$$

subject to the budget constraint,

$$C_h = W_0(1+R_p) + H - X, \tag{6}$$

where $R_p = \alpha_h(R - R_f) + R_f$ denotes the portfolio return on total financial wealth. If $H = 0$ with non-zero probability, the problem is well-defined only if $W_0(1+R_p) > X$. The reason is that the utility costs of failing to meet the subsistence level are unbounded. To ensure next period's consumption habit is met, a utility-maximizing agent will first allocate the discounted value of consumption habit, $X/(1+R_f)$, to the risk-free asset and then invest the surplus wealth, $W_0 - X/(1+R_f)$, using the formula for the baseline model derived in the previous section. Hence, we have:

$$\alpha_h^\star = \underbrace{\left(1 - \frac{X}{W_0(1+R_f)}\right)}_{\text{Downward adjustment}} \alpha^\star \tag{7}$$

$$= \left(1 + \frac{\overline{H}}{W}\right)\left(1 - \frac{X}{W_0(1+R_f)}\right)\left(\frac{\mu + \sigma_r^2/2}{\gamma\sigma_r^2}\right) - \frac{\overline{H}}{W}\left(1 - \frac{X}{W_0(1+R_f)}\right)\frac{\sigma_{h,r}}{\sigma_r^2}. \tag{8}$$

Equation (7) shows that the optimal allocation in the extended model, $\alpha_h^\star$, is proportional to the optimal share in the baseline model, $\alpha^\star$. Since the adjustment factor is strictly less than one, the adjustment is always toward zero and we have $\alpha_h^\star < \alpha^\star$. Moreover, the adjustment-factor is increasing in financial wealth, a fact that provides insight into why

[6] Alternatively, incorporating subsistence levels (Rubinstein, 1976; Litzenberger and Rubinstein, 1976) or consumption commitments (Chetty and Szeidl, 2007) into the model can yield similar effects as consumption habits.

habit-formation models are often invoked to explain empirical evidence suggesting positive wealth effects on the risky share (e.g., Wachter and Yogo, 2010; Calvet and Sodini, 2014).

In the extended model incorporating nontradable human capital and consumption habits, a wealth shock thus influences the risky share through two distinct channels, which work in opposite directions. Heuristically, consumption habits can be viewed as negative labor income. Just as human capital is a nontradable asset that provides resources beyond the current financial wealth to generate utility in the future, consumption habits are a nontradable liability that withholds resources from the financial wealth available in future periods. Thus, human capital and consumption habits have opposite implications for the financial wealth elasticity of the risky share, as shown in Equation (8). Whereas human capital induces negative wealth effects on the risky share, consumption habits induce positive wealth effects. Consequently, the net effect is ambiguous and depends on the relative strengths of the two channels.

Despite the ambiguity, our analysis above makes two simplifying assumptions that are likely to cause the model to overstate the quantitative importance of consumption habits. First, our analysis implicitly assumes that the lower bound on the realized labor income is zero. Second, we assume that the worst-case equity returns is -100%, implying that all wealth not invested in the safe asset could be depleted entirely. These assumptions jointly imply that the investor will always choose to allocate the maximally conservative amount in the risk-free asset to fund the consumption habit. Under more realistic assumptions about the worst-case scenario,[7] the mandatory investment in the risk-free asset would be smaller, and accordingly, the positive relationship between financial wealth and the risky share induced by the consumption-habit channel would be weaker.

In Section 5, we conduct a quantitative analysis of an enriched lifecycle portfolio choice model featuring calibrated income profiles and consumption habits, and show that the human-capital channel indeed dominates the habit channel in this more realistic setting, as evidenced by the negative overall effect of financial wealth on the risky share.

[7] For instance, social insurance programs in Sweden, such as unemployment insurance, provide a safety net equivalent to 70-80% of a household's previous average earnings in Sweden (`https://www.oecd.org/social/soc/29736100.pdf`). The largest historical annual market downturns on the Swedish equity markets occurred in 1918 and 2008. In both years, the index declined by about 43% (Waldenström, 2014).

3 Data and Identification Strategy

Our analyses are conducted in a sample of lottery players who have been matched to administrative demographic and financial records using players' personal identification numbers (PINs). This section describes the key features of the administrative lottery samples, the government registers, and our identifciation strategy.[8]

3.1 Portfolio and Register Data

All financial variables used in our analyses are derived from the Swedish Wealth Register. The register is maintained by Statistics Sweden and contains detailed information about the year-end financial portfolios of the entire Swedish population from 1999 until 2007. These data have been used in a number of influential studies beginning with Calvet, Campbell and Sodini (2007) and are generally of very high quality. Despite its comprehensive coverage, however, we acknowledge a limitation of our data, namely that defined-contribution retirement wealth and private businesses are not reported (Bach, Calvet and Sodini, 2020; Calvet et al., 2021) and thus are excluded from our analyses. See Section 4.3 for additional details on the dataset and a discussion of possible limitations.

The Wealth Registry contains disaggregated measures of debt and multiple types of assets, including bank account balances,[9] mutual funds, directly held stocks, bonds, interest funds, debt, residential and commercial real estate, and other financial and real assets. Throughout the paper, we refer to several portfolio measures constructed from these underlying assets. Similar to Calvet and Sodini (2014), we refer to *cash* as the sum of bank account balances and interest funds, whereas *risky financial assets* refer to directly held stocks and mutual funds. *Equity market participants* are those who own risky financial assets. We measure *financial wealth* as the sum of cash, risky financial assets, capital insurance products, and directly held bonds. In our preferred specification, the *risky share* is defined as the year-end share of a household's total financial wealth that is held in risky financial assets. In addition to the financial variables, we use administrative records of demographics and income from Statistics Sweden.

[8]Our data and empirical strategy are similar to those of Cesarini, Lindqvist, Östling and Wallace (2016), Cesarini, Lindqvist, Notowidigdo and Östling (2017), and Briggs et al. (2021), with Cesarini et al. (2016) providing the most thorough description of our lottery samples.

[9]We impute bank account balances which fell below the required reporting threshold. We discuss our imputation procedure in detail in Appendix C.

Our baseline analyses are conducted at the household level, where a household is defined as the lottery player and, if present, his or her spouse. We limit all analyses to players aged at least 18 at the time of the lottery and who are known to have participated in equity markets in the year prior to the lottery.[10] Since we only observe equity-market participation annually from 1999 until 2007, the participation restriction effectively limits the sample to players whose lottery event occurred between 2000 and 2007.

3.2 Lottery Data

Our final estimation sample consists of players from three separate Swedish lotteries – Triss, Kombi and PLS – whose lottery events took place between 2000 and 2007. Our empirical strategy is to use the available data and instutional knowledge about each lottery to assign players to groups within which we know the prizes were randomly assigned under the rules of the lottery. We then control for group-identifier fixed effects in all analyses. Doing so ensures all identifying variation comes from players in the same group. Because the process by which we assign players to groups varies across lotteries, we briefly describe each lottery separately below.[11] Throughout the paper, lottery prizes and all other variables originally measured in units of SEK are converted to units of year-2010 USD before analyses. To convert each monetary variable, we first convert each value to units of year-2010 SEK using Statistics Sweden's annual consumer price index normalized so its value is 1 in 2010. We then convert the inflation-adjusted SEK amount to USD using the exchange rate on December 31, 2010 (6.72 SEK/USD). Except where explicitly noted otherwise, we restrict all analyses to prizes paid as lump sums.

Kombi

Kombi is a monthly subscription lottery used to raise funds for the Swedish Social Democratic Party, Sweden's largest political party. Participants specify how many tickets to purchase each month, and usually pay via direct debit. Kombi provided us with a longitudinal dataset containing the number of tickets purchased, information about any prizes won that are greater than or equal to 1M SEK, and the PIN for each participant between

[10] Households that did not participate in the equity market prior to the lottery can only weakly increase their risky share through the previously studied extensive-margin participation decision (Briggs et al., 2021), and thus are excluded from analysis for interpretational clarity in this paper.

[11] For a more detailed description of the data, we refer the reader to Section 2 and the Online Appendix of Cesarini et al. (2016), where the data was first introduced, pre-processed, and quality controlled.

1998 and 2011.

Prizes are awarded by randomly sampling a ticket from the set of tickets purchased for each monthly draw. Because all tickets have an equal probability of being selected, individuals with the same number of tickets are equally likely to win any given prize. To construct the group identifers, each winner is therefore matched to (up to) 100 non-winning players with the same age, gender, and number of tickets in the month of the draw. This matching procedure leaves a sample of 199 large-prize winners, matched to a total of 18,078 controls.

Triss

Triss is a scratch-ticket lottery run by Svenska Spel, the government-owned gambling company. Since 1994, a subset of Triss lottery winners are entitled to appear on a TV show where she can win a monetary prize. There are two ways to qualify for the show.

The first is to present a ticket with three matching television symbols. Each player in possession of such a ticket is invited to a live TV show where they win a lump-sum prize. At the show, the player draws a prize by selecting a ticket from a stack of scratch-off tickets that are indistinguishable (before they are scratched). The chosen ticket is then scratched to reveal the size of the lump-sum prize. The distribution of prizes in each stack of tickets is determined by a regulatory document called a prize plan. Since there is no element of skill or strategy in the ticket selection, the magnitude of the lump-sum prize won is random conditional on the prize plan (which is occasionally revised). We therefore assign two players to the same group if they appeared on the TV show exactly once in the same year and drew a lump-sum prize under the same prize plan. In our sample, the lump-sum prizes, measured net of taxes in year-2010 USDs, range in value from 8K to 866K.

The second way is to present a ticket with three matching clover symbols. Each player in possession of such a ticket wins a monthly installment prize, the value of which is determined on live television, this time by having the player draw two tickets. The first determines the size of the monthly installment to be paid out (range: \$1,500 to \$8,000) and the second the duration of the monthly installments (10 to 25 years). The two draws are independent, again with underlying distributions determined by the prize plan.

Svenska Spel provided us with demographic information about all TV show participants between 1994 and 2011. Using this information, we were able to identify the PINs of 99% of participants and match them to the government registers. In our main analyses, we only include the 1,065 lumpsum winners. We exclude monthly-installment winners since the purpose of our main analyses is to test theoretical predictions about the effect of a sudden windfall that increases initial wealth W_0, as described in Section 2. Since monthly prizes cannot be easily liquidated into a net present value, it is not appropriate to use them to test predictions about the effects of windfalls that increase the amount of liquid, financial wealth. Rather, they are conceptually more similar to a change in human capital, H. Therefore, we include the monthly prizes in testing the prediction that shocks to future income and initial financial wealth have opposite effects on the risky share. In this comparison, we include a sample of 227 participants who received prizes paid in monthly installments. To facilitate comparability, we convert each monthly prize to a net present value and exclude Triss monthly prizes with an estimated net present value above $1M. The latter restriction is intended to make the prize distributions more comparable.

Prize-Linked Savings

Prize-linked savings (PLS) accounts are savings accounts with a lottery element that randomly award monetary prizes to some accounts instead of (or sometimes in addition to) paying interests. PLS accounts were initially subsidized by the Swedish government. Although the subsidies ended in 1985, banks were authorized to continue offering PLS products to retail consumers. Participation was widespread across broad strata of Swedish society, with every other Swede owning an account in the late 1980s.

The PLS sample was obtained by combining prize records and account information from the PLS accounts maintained by commercial banks and the state bank. These data allow us to identify the account owner, account balance, and amount won for each prize paid between 1986 and 2003. The probability of winning was proportional to the number of tickets associated with an account, where one ticket was assigned for each 100 SEK in the account balance at the times of draws. PLS lotteries paid both odds prizes, which were awarded as a multiple of the account balance, and fixed prizes whose magnitude was independent of account balance. However, because we lack information on account balances after 1994, we only consider fixed prizes in this paper.

Following Imbens et al. (2001) and Hankins et al. (2011), our identification strategy for PLS exploits the fact that the prize distribution is independent of account balance among players who won the same number of fixed prizes in a given draw. Hence, we assign two individuals to the same group if they won an identical number of fixed prizes in that draw. This matching procedure leaves a sample of 30,613 PLS lottery winners for the period 2000 through 2007.

Table 1: Prize Distribution. This table shows the number of lottery prizes in the indicated prize-size categories for the pooled sample and the three lottery subsamples. Prize amounts are in year-2010 USD and net of taxes.

Prize Amount	(1) Pooled	(2) Kombi	(3) Triss	(4) PLS
$L_i = 0$	18,078	18,078	0	0
$0 < L_i \leq 1K$	30,345	0	0	30,345
$1K < L_i \leq 10K$	478	0	212	266
$10K < L_i \leq 100K$	801	0	801	0
$100K < L_i \leq 250K$	202	188	12	2
$250K < L_i$	51	11	40	0
Total	49,955	18,277	1,065	30,613

3.3 Identification

Normalizing the time of the lottery to $s = 0$, our main estimating equation is given by

$$Y_{i,s} = \beta_s L_{i,0} + \mathbf{X}_i \mathbf{M}_s + \mathbf{Z}_{i,-1} \boldsymbol{\gamma}_s + \eta_{i,s}, \tag{9}$$

where i indexes households, $L_{i,0}$ denotes the prize size (in 100K USD), $\mathbf{X}_i$ is a vector of group-identifier fixed effects, and $\mathbf{Z}_{i,-1}$ is a vector of controls that include a wide range of demographic characteristics (age, sex, marital status, higher education, household size, household income, Nordic born) as well as financial characteristics (net wealth, gross debt, real estate ownership, risky share). The control variables are measured in the year before the lottery and are included only to improve the precision of our estimates. Standard errors are clustered at the level of the player. The key identifying assumption needed for β_s to have a causal interpretation is that the prize amount won is independent of $\eta_{i,s}$ conditional on the group-identifier fixed effects.

Prize Variation

To provide a better sense of the source of our identifying variation, Table 1 summarizes the prize distribution in our final estimation sample. The total value of the after-tax prize money disbursed to the winners in our sample is approximately 90M USD (610M SEK), with 84% of the identifying variation coming from prizes whose value exceeds the median Swedish household income in 1999 of 21K USD (143K SEK). Even though small prizes account for a relatively large fraction of prizes won, most of the identifying variation comes from the larger prizes, especially in Kombi and Triss.[12]

Testing for Random Assignment

To test our key identifying assumption, we again normalize the time of lottery to $s = 0$ and run the following regression:

$$L_{i,0} = \mathbf{X}_{i,0}\boldsymbol{\Gamma}_0 + \mathbf{Z}_{i,-1}\boldsymbol{\rho}_{-1} + \varepsilon_i. \tag{10}$$

Under the null hypothesis of conditional random assignment, the characteristics determined before the lottery ($\mathbf{Z}_{i,-1}$) should not predict the lottery outcome ($L_{i,0}$) conditional on the group-identifier fixed effects ($\mathbf{X}_{i,0}$). We run these quasi-randomization tests in our pooled sample and in each lottery subsample separately. As expected, Appendix Table A.2 shows the lagged characteristics overall have no statistically significant predictive power once we control for group-identifier fixed effects in the analyses.[13] When the fixed effects are omitted, the null hypothesis is rejected. This finding illustrates the importance of controlling for factors conditional on which the lottery prizes are randomly assigned.

Representativeness of the Lottery Sample

Table 2 compares our lottery sample to a random sample of adult Swedish stock-market participants matched on sex and age. Columns (1) and (2) compare our pooled sample to the random sample. For most demographic characteristics, the distributions are very similar. The main difference is that players in our sample are ten percentage points less likely to have attended college. In terms of financial characteristics, players in our sample have lower net wealth on average, but the differences in other characteristics are quite small. For example, the average risky share in the pooled lottery sample is 0.41, compared to 0.43 in the representative sample. Columns (3)-(5) provide descriptive statistics separately by lottery.

---

[12] Our main results are robust to excluding PLS prizes, most of which are relatively small prizes.

[13] We reject the joint significance of the lagged characteristics in 10 of the 12 F-tests conducted. The two exceptions occur in the PLS subsample, with p-values of 0.08 and 0.035.

Table 2: Representativeness of the Lottery Sample. This table compares our lottery sample to a representative sample of stock market participants matched on age and sex. The pooled sample is generated by weighting each lottery sample by its share of the identifying variation. The summary statistics shown are all means and are measured at $s = -1$. All variables except female, age, and Nordic born are measured at the household level. Continuous financial variables are winsorized at the 0.5 and 99.5 percentiles.

	(1) Representative	(2) Pooled	(3) Kombi	(4) Triss	(5) PLS
Demographic Characteristics					
Female	0.479	0.479	0.421	0.511	0.539
Age (years)	56.1	56.1	62.2	52.8	62.1
Nordic born	0.952	0.971	0.986	0.963	0.968
Household size (#)	2.269	2.264	2.010	2.408	2.012
Household income ($K)	63.0	60.2	59.9	60.4	54.6
Married	0.617	0.592	0.624	0.575	0.581
College	0.385	0.284	0.255	0.300	0.314
Financial Characteristics					
Net wealth ($K)	220.5	162.6	174.2	155.7	257.1
Gross debt ($K)	62.6	56.8	45.4	63.2	37.9
Homeowner	0.800	0.807	0.851	0.783	0.780
Risky share	0.434	0.413	0.428	0.405	0.455
N	208,916	49,955	18,277	1,065	30,613

4 Empirical Analysis

4.1 Baseline Estimates of Wealth Effects on Risky Share

Figure 1 displays our estimates of how wealth impacts the year-end risky share at $s = 0, 1, .., 4$. Our estimates suggest that a $100K windfall leads to an immediate decrease in the share of financial wealth allocated to risky assets: in the year of win, the risky share declines by 4.8 percentage points (SE = 0.6). Figure 1 also illustrates that this reduction persists for at least four years, the longest post-lottery event horizon we consider in our analyses.[14]

[14] As shown in Appendix Table A.1, the confidence intervals widen as the the time horizon is extended. Two factors contribute to this widening. First, the pre-lottery characteristics absorb less and less residual variance. Second, the size of the estimation sample falls as the time horizon is extended.

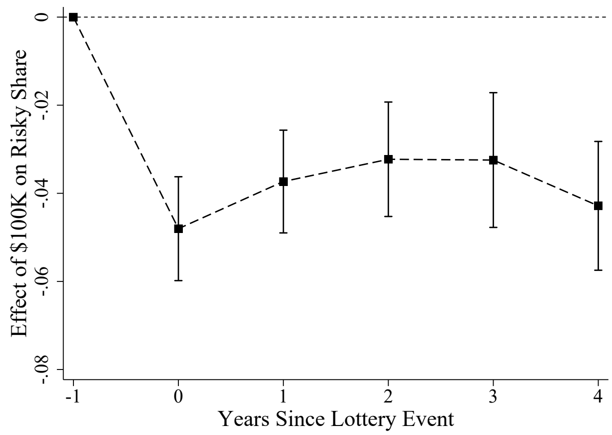


Figure 1: Effect of 100K USD of Lottery Wealth on Risky Share. Coefficients and 95% confidence intervals are obtained by estimating Equation (9) in our pooled sample. See Appendix Table A.1 for the underlying estimates.

4.2 Evaluating Model Predictions

Our next analyses are designed to empirically test predictions 2, 3, and 4 of the static model described in Section 2.

Prediction 2: Diminishing Marginal Wealth Effects on Risky Share

The model predicts that the risky share is everywhere decreasing in the magnitude of the lottery prize, but at a decreasing rate. To test this hypothesis, we derive five indicator variables from the lottery-wealth variable used in our main analyses. Each indicator variable takes on the value 1 if the prize falls within a given prize range and 0 otherwise. The five ranges are (i) 0 to 1K USD, (ii) 1K to 10K, (iii) 10K to 100K, (iv) 100K to 250K, and (v) 250K and more. We then re-run our main analyses using four of these indicator variables in lieu of the original treatment variable (the omitted category is the one with prizes less than $1K).

Figure 2 displays the estimated coefficients at year-end in $s = 0$. In the figure, the x-coordinate for each coefficient is the average prize won by players in the category. The results are broadly consistent with the theoretical prediction. Although the first two coefficient estimates are not statistically distinguishable from zero, the latter two coefficients provide supporting evidence that the marginal effect of wealth is larger (in absolute value) in the smaller category than the larger one.

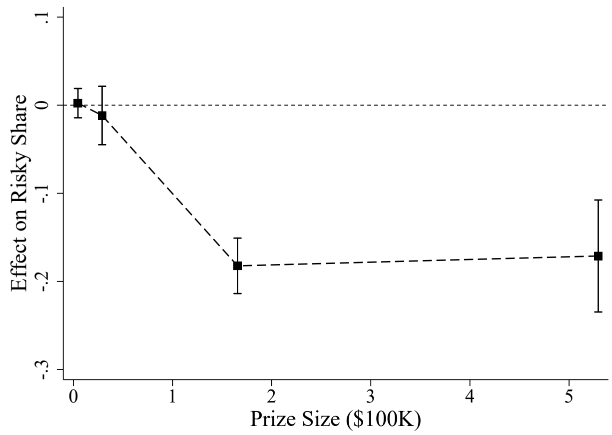


Figure 2: Effect of Lottery Wealth on Risky Share by Prize Size. Coefficients are obtained by estimating Equation (9) with the lottery-wealth variable replaced by indicators for five mutually exclusive prize categories: 0 to 1K USD, 1K to 10K, 10K to 100K, 100K to 250K, and 250K+ . The smallest prize category is omitted in the regression. Coefficient estimates and the 95% confidence intervals are plotted for each category. See Appendix Table A.3 for the underlying estimates.

Prediction 3: Wealth Effects by Human-Capital-to-Wealth Ratio

Next, we evaluate the prediction that the impact of a wealth shock should be larger in absolute value (i.e. more negative) when a larger share of total (pre-lottery) wealth is accounted for by human capital. To test this prediction, we first define a measure of each household's expected human capital, following Calvet and Sodini (2014). Household h's human capital at time t is given by:

$$\mathrm{HC}_{h,t} = \sum_{n=1}^{T_{h,t}} \pi_{h,t,t+n} \frac{\mathbb{E}_t(\mathrm{H}_{h,t+n})}{(1+r)^n}, \tag{11}$$

where $H_{h,t}$ denotes the household's period-t labor income, $\pi_{h,t,t+n}$ denotes the probability that household h is alive at date $t+n$ conditional on being alive at date t, and $T_{h,t}$ is the difference between 100 and the age of household h at date t. We use the observed income of household h at date t, together with the estimated labor income process specified in Equation (14), to compute the expected trajectory of future income $H_{h,t}$ over the life cycle.[15] We then use Equation (9) to estimate separate wealth effects for households with human-capital-to-wealth ratios below and above the median.

[15]For a more detailed description of the human capital calculation, we refer the reader to Appendix B.5.

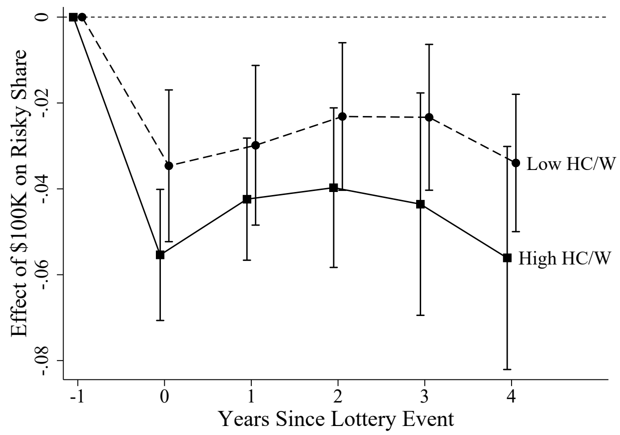


Figure 3: Effect of Lottery Wealth on Risky Share by $s = -1$ HC/W Ratio. Coefficients and 95% confidence intervals are obtained by estimating Equation (9) in our pooled sample stratified by pre-lottery human-capital-to-wealth ratio (above or below median). See Appendix Table A.4 for the underlying estimates.

The results, depicted in Figure 3, provide some support for the the prediction. For every $s = 0, 1, ..., 4$, our point estimate is consistently larger (in absolute value) for households with above-median ratios of human capital to wealth, as predicted by the theory. However, these differences are not statistically significant, indicating that the evidence is at most suggestive. Another potential limitation of our test is that human-capital-to-wealth ratios are not randomly assigned. It is therefore possible that the patterns displayed in the figure partly reflect differential selection into the two groups.

Prediction 4: Heterogeneity by Mode of Payment (Lump Sum vs. Installments)

Finally, we test the prediction that an exogenous increase in future income should have a *positive* impact on the risky share. Our test takes advantage of the fact that some players in our Triss sample won lump-sum prizes, whereas others won prizes paid out as monthly installments. Conditional on winning exactly one Triss prize, the type of prize is random and outside the control of the player. Lump-sum prizes generate an immediate increase in liquid financial wealth that is predicted to generate a negative effect on the risky share (per *Prediction 1*). Prizes paid as monthly installments, by contrast, have no impact on short-run liquidity but increase future income flows. For these prizes, the model predicts a positive effect on the risky share (per *Prediction* 4), provided there is no mechanism for liquidating the future income flows into a lump-sum payment approximately equal to the net-present

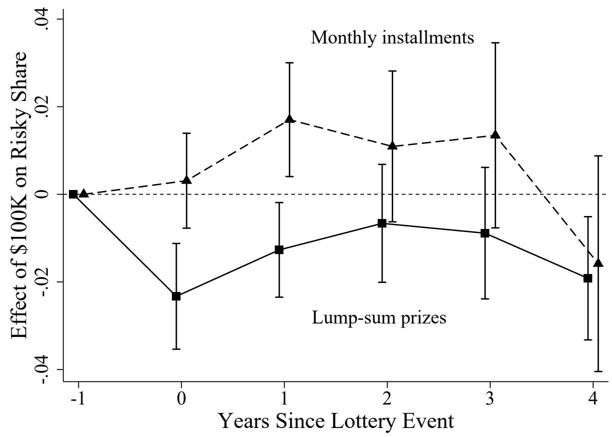


Figure 4: Effect of Lottery Wealth on Risky Share by Mode of Payment. Coefficients and 95% confidence intervals are obtained by estimating Equation (9) among Triss winners stratified by type of payment plan (lump sum or monthly installments). See Appendix Table A.4 for the underlying estimates.

value of the anticipated income flows. We therefore estimate separate treatment effects for the two types of windfalls.

To facilitate comparisons, we convert each monthly-installment prize into a lump-sum equivalent by calculating its net present value before running any analyses. We also restrict the set of monthly prizes to ensure that the distribution of prizes (measured either as lump-sums or lump-sum equivalents) are similar in the two samples. Figure 4 displays the results. For winners of monthly installments, the estimated effects are indeed weakly positive (though not always statistically significant) in all post-lottery years except $s = 4$. This is in sharp contrast to the sizable decline in the risky share observed for winners of lump-sum prizes. A standard F-test rejects the null of identical effects at $s = 0$ (p-value = 0.002) and $s = 1$ (p-value < 0.001) but not at later time horizons, where the differences in treatment effects are estimated less precisely.

Heterogeneity by Household Characteristics

We conduct a number of additional analyses to examine how wealth effects vary by a range of demographic and financial characteristics. In each analysis, we stratify our pooled sample along some dimension of interest, and then compare the estimated wealth effects in the two subsamples. We estimate wealth effects for two time horizons: $s = 0$ and $s = 3$. Table 3 presents the results for $s = 0$ (see Appendix Table A.5 for the $s = 3$ results,

Table 3: Heterogeneous Effects of Lottery Wealth on Risky Share. Coefficients are obtained by estimating Equation (9) at time $s = 0$ in our pooled sample stratified by various demographic and financial characteristics. Hetero p is obtained from an F-test of the null hypothesis that the two lottery-wealth coefficients are identical. Income risk is proxied by the standard deviation of annual income changes prior to the lottery. Equity returns are based on the MSCI Sweden Index in the calendar year prior to the lottery.

	Demographic Characteristics							
	Sex		Age		College		Self-employed	
	Male	Female	≤ 45	> 45	No	Yes	No	Yes
Effect	-0.068	-0.040	-0.061	-0.051	-0.061	-0.036	-0.058	-0.016
SE	0.011	0.009	0.019	0.008	0.008	0.013	0.007	0.018
p	<0.001	<0.001	0.001	<0.001	<0.001	0.006	<0.001	0.365
Hetero p	0.051		0.603		0.116		0.027	
N	25,218	24,737	6,115	43,840	35,371	14,584	46,861	3,094

	Financial Characteristics							
	Gross Debt		Homeowner		Income Risk		Equity Returns	
	≤ 0	> 0	No	Yes	Low	High	≤ 0	> 0
Effect	-0.068	-0.048	-0.088	-0.049	-0.071	-0.042	-0.076	-0.042
SE	0.012	0.008	0.020	0.007	0.011	0.009	0.013	0.008
p	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Hetero p	0.173		0.070		0.041		0.030	
N	20,051	29,904	9,679	40,276	25,006	24,949	29,584	20,371

which are broadly similar in terms of magnitude, but with larger standard errors). Table 3 shows that across all subsamples analyzed, the estimated wealth effects are negative. The analyses also suggest that wealth effects may be larger in magnitude in some groups, including winning players who (i) are male, (ii) are not self-employed, (iii) do not own a home, (iv) face lower income risks, and (v) win the lottery at a time when equities have recently been generating negative returns.

Many of these findings are readily reconcilable with findings of other studies. For example, smaller wealth effects among homeowners are in line with a prior literature that finds that homeownerships reduces households' appetite for risk (Grossman and Laroque, 1990; Cocco, 2005; Flavin and Yamashita, 2011). In particular, if we view homeownership as a proxy for committed consumption (Chetty and Szeidl, 2007), it would generate an offsetting effect to nontradable human capital and weaken the negative relationship between financial wealth and the risky share, as discussed in Section 2. Similarly, if households hold less equity ex ante to compensate for background risk from more volatile income (Viceira, 2001) or entrepreneurial risk from self-employment (Heaton and Lucas, 2000), the scope for financial wealth shocks to reduce the risky share might be relatively limited for such households, which results in a smaller decrease in the risky share. Lastly, households being less willing to invest in risky assets in years following poor equity returns is consistent with findings that expectations about the stock market reflect extrapolation of recent realized returns into the future (Vissing-Jørgensen, 2003; Greenwood and Shleifer, 2014).

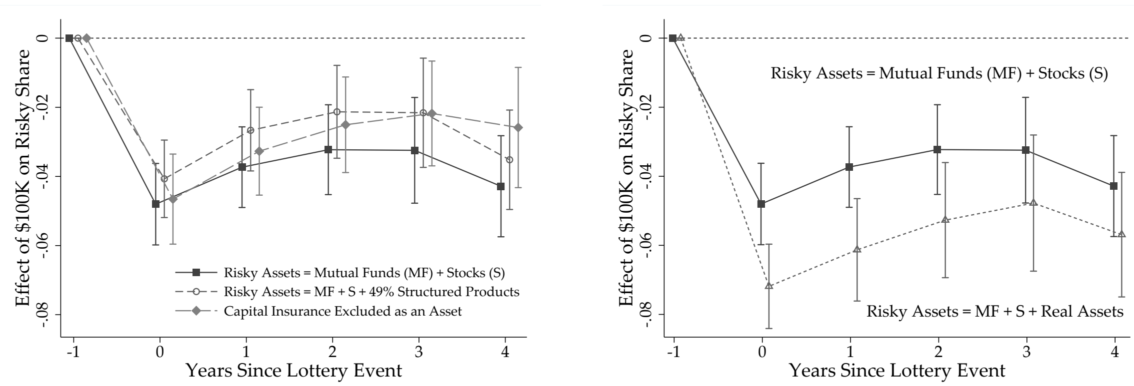
4.3 Robustness Analyses

In this section, we probe the robustness of our main results. We examine the sensitivity of our headline estimates to alternative definitions of the risky share, further discuss the external validity of our estimates, and investigate the concern that the negative wealth effects are an artifact explained by household inaction/sluggishness. We conclude by discussing the broader relevance of some of the treatment effects reported in this section for household-finance and macro-finance literatures.

Definition of Risky Share

In our main analyses, we follow the convention of defining a household's risky share as the proportion of its total financial wealth held in directly held stocks or mutual funds. In practice, not all types of financial assets can be straightforwardly categorized as either risky or risk-free. We examine how robust our results are to three alternative definitions of the risky share. First, we follow Calvet, Célérier, Sodini and Vallée (2023) by classifying structured products as partially risky, assuming a risk exposure of 49%. Second, we follow Calvet et al. (2007) by excluding capital insurance products altogether when calculating the risky share.[16] Third, we classify real estate as a risky asset and define the risky share as the proportion of a household's portfolio – defined as the sum of financial wealth and real estate wealth – that is held in risky financial assets or in real estate. Figure 5 compares our original results to the three alternatives. Classifying structured products as partially risky or dropping capital insurance products produces similar results, albeit with coefficient

[16]The Wealth Register records the total value of capital insurance products but not the asset allocation within these products. Dropping capital insurance from the calculation of the risky share is equivalent to assuming that the total amount in capital insurance is allocated between safe and risky assets in the same proportion to all other assets in a household's portfolio with determinable risk exposure.



(a) Alternative Financial Assets **(b) Real Estate Assets**

Figure 5: Robustness to Alternative Definitions of Risky Share. Coefficients and 95% confidence intervals are obtained by estimating Equation (9) using an alternative definition of the risky share. Panel (a) categorizes structured products as partially risky assets with a risk exposure of 49%, and excludes capital insurance products in calculating the risky share. Panel (b) classifies real estate as risky assets. See Appendix Table A.1 for the underlying estimates.

estimates that are smaller in magnitude. Treating real estate wealth as a risky asset instead gives coefficients further away from zero.

External Validity

A commonly voiced concern about lottery studies is their results may be hard to generalize. The concern is usually grounded in widespread beliefs that lottery players tend to be strongly negatively selected on factors such as financial sophistication and educational background. These popular beliefs are often reinforced by reports in the popular press about lottery winners who went on to quickly squander their wealth and claim to regret the experience of winning. However, a prior literature starting with Kaplan (1987) generally rejects these popular beliefs as inaccurate. Differences between lottery players and the general population tend to be smaller than common stereotypes might suggest. It is also rare for players to completely squander the financial windfalls within a matter of years, contrary to what the common stereotype suggests. For example, previous analyses of the sample of Swedish lottery players analyzed here provide no evidence that lottery winnings have deleterious long-run effects on winners' financial and emotional well-being (Lindqvist, Östling and Cesarini, 2020).

Here, we conduct additional analyses designed to test different explanations for the disparities between our results and those reported in the earlier literature. Our basic approach is to examine if large differences in results persist when the methodology of previous quasi-experimental papers is applied in our sample (as closely as possible). Our main analysis compares the estimates reported in Brunnermeier and Nagel (2008) to those we obtain when we follow their methodology as closely as possible in our data. Brunnermeier and Nagel (2008) use the following estimating equation in their analysis of the Panel Study of Income Dynamics (PSID):

$$\Delta_k \alpha_t = \beta q_{t-k} + \gamma \Delta_k h_t + \rho \Delta_k w_t + \varepsilon_t, \tag{12}$$

where α_t is the share of stocks or mutual funds in the financial portfolio, q_{t-k} is a vector of household characteristics (e.g., age, gender, marital status, household composition, unemployment, log income, union employment, vehicle ownership, and inheritances), $\Delta_k h_t$ is a vector containing changes in family composition or asset ownership (e.g., changes in number of children, homeownership, business ownership), and $\Delta_k w_t$ is the change in log wealth. To address concerns about measurement error, Brunnermeier and Nagel (2008) run additional analyses with quantile dummies for income growth from $t-k$ to t and inheritance receipts as instruments, and estimate Equation (12) using a two-stage least squares procedure.

We sought to replicate their original analyses as closely as possible in our sample of Swedish lottery players.[17] The results are summarized in Figure 6. At the $k=2$ year horizon, the estimates for both OLS and 2SLS are statistically indistinguishable from zero and/or economically small.[18] Estimates are similarly indistinguishable from zero at the $k=5$ year horizon. For three out of four coefficients, a test of the null that the parameters are identical in the PSID sample and the Swedish lottery sample fails to reject. Overall, the results are consistent with the hypothesis that most of the original differences in results disappear under a common methodology.

Our findings of robust negative wealth effects on portfolio risk also conflict with those reported in Calvet and Sodini (2014) and Calvet et al. (2009), both of whom analyzed large samples of Swedes. Although data limitations do not allow us to directly apply ei-

[17] Our specification is identical to the one originally used by Brunnermeier and Nagel (2008) except that we exclude four controls that are not available in the register data. To avoid contamination from lottery winnings, we restrict our analysis to households that won the lottery at least $k+1$ years after 1999. Hence, the estimation sample used in the $k=2$ (5) is restricted to players who won in 2002 (2005) or later.

[18] The OLS coefficient of -0.039 reported by Brunnermeier and Nagel (2008) implies that 10% growth in wealth leads to a tiny reduction in the share of risky assets by 0.0039, for example, from 50% to 49.61%.

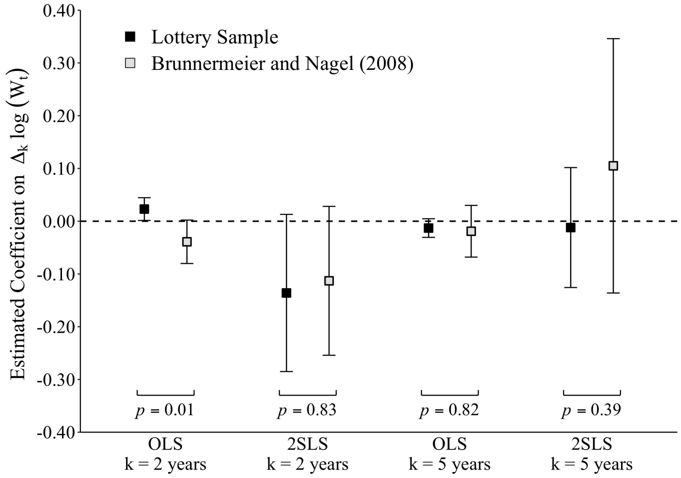
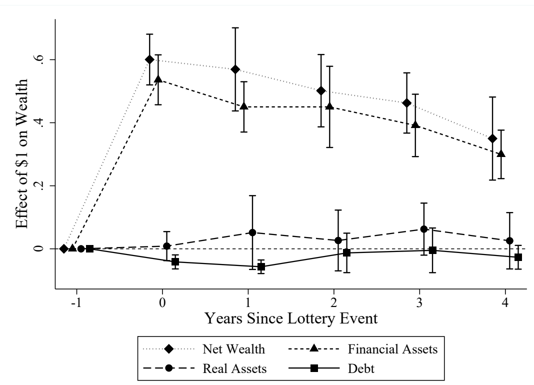


Figure 6: Non-Experimental Estimates of Financial Wealth Elasticity of Risky Share. Coefficients are obtained by estimating Equation (12) in our pooled sample with an end date the year prior to the lottery win. Regressions are estimated at 2- and 5-year horizons using both OLS and 2SLS. This estimation closely replicates Table 4 of Brunnermeier and Nagel (2008). Observations are weighted by their respective contribution to the identifying variation in the lottery regressions. p-value is obtained from a test of the null that the two coefficients are identical. See Appendix Table A.6 for underlying estimates.

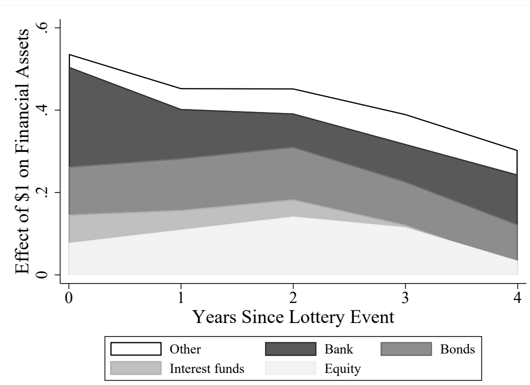
ther study's methodology in our lottery sample, Calvet et al. (2007) reported an analysis of the cross-sectional relationship between stock market participation and a number of demographic and financial characteristics. Briggs et al. (2021) show that when a similar specification is used in our sample of Swedish lottery players, the results are strikingly similar. In Appendix D, we provide a more detailed discussion of the assumptions underlying the identification strategies by Calvet et al. (2009) and Calvet and Sodini (2014). Overall, the available evidence thus points to methodological differences as a plausible explanation for the observed differences between our findings and those in the previous literature.

Household Inaction and Sluggish Reinvestment

We next explore the hypothesis that the negative wealth effects are an artifact of households' inaction and sluggishness in investing the prize money after it is deposited into players' bank accounts. Such inaction could cause the risky share to fall mechanically for reasons unrelated to risk preferences (e.g., procrastination or status quo bias causes the prize money to sit in a checking account for a long time before it is invested). To investigate this hypothesis, we analyzed how the lottery windfalls impact a number of specific asset



(a) Wealth Categories



(b) Financial-Asset Categories

Figure 7: Allocation of Lottery Wealth across Asset Classes. Coefficients and 95% confidence intervals are obtained by estimating Equation (9) where various asset classes are used as outcome variables. Panel (a) shows the allocation of lottery winnings across broad wealth categories. Panel (b) shows the allocation within financial assets. All outcome variables reflect year-end market values. See Appendix Table A.7 for the underlying estimates.

and liability subcategories.

The results from these analyses are summarized in Figure 7. The coefficients depicted are scaled as proportions, so an estimate of 0.10 implies that the total value of the asset class rises by 10 cents for each dollar won. In Panel (a), we plot the estimated coefficients for net wealth and its decomposition into financial assets, real assets, and debt. In Panel (b), we show a further decomposition of the financial assets into five mutually exclusive categories: equity, interest funds, bonds, bank account balances, and other financial assets.

Under widespread inaction, a considerable portion of the lottery winnings is expected to remain in the bank accounts, where the prizes are initially deposited, for an extended period of time. The $s = 0$ estimates in Panel (b) indicate that there is some initial tendency to keep the winnings in the bank accounts: in the year of win, each \$100 won is estimated to increase the year-end bank account balance by \$24 (SE = \$4.3). However, this effect diminishes quickly, and in all subsequent periods, it remains in the range of \$8 to \$12. Importantly, we also find that most of the prize money is actively reallocated to other financial assets, with much of the investments directed towards bonds and interest funds that carry low risks. These findings thus suggest that the reduction in the risky share cannot be attributed to inaction or a general aversion to investing the windfalls in financial assets.

Broader Relevance

The estimates depicted in Figure 7 are likely to prove valuable beyond the specific setting of this paper.[19] For example, credible and statistically precise estimates of how windfalls impact various asset and liability classes could help improve evaluations and calibrations of a wide range of household-finance and macro-finance models.

Consider the estimates in Panel (a) of the per-dollar effect of lottery winnings on net wealth. At the end of the year of win, we estimate that net wealth increases by $60 per $100 won, whereas the analogous estimate four years after the lottery is $35. These estimates provide information about the average household's marginal propensity to save (MPS) out of a wealth shock, whereas the residual effect (i.e., 1 - MPS) can be interpreted as an upper bound on the household's marginal propensity to spend (MPX), a crucial determinant of fiscal and monetary policies in heterogeneous-agent models (Kaplan, Moll and Violante, 2018; Auclert, 2019; Laibson, Maxted and Moll, 2022).[20] Furthermore, the panel nature of our data allows us to estimate dynamic saving responses over time, providing valuable information about *intertemporal* marginal propensities that are essential inputs for the general-equilibrium amplification of shocks (Auclert, Rognlie and Straub, 2023).

Our data also allow us to decompose the total saving response into allocations across different components of household balance sheets. In Panel (a), among broad wealth categories, financial assets account for 80-90% of the net wealth increase, whereas no more than 10-20% is used to invest in real estate or to pay off debt.[21] Among the financial assets, in Panel (b), investment in risky assets represents 14% of the total savings in financial assets in the year of win, which then rises to 31% after two years before declining to 30% (11%) by year 3 (year 4). Such a decomposition enables us to infer households' marginal propensities to invest in various asset classes and to take risk (MPR), which are proposed to be an important parameter shaping asset prices and investment dynamics in macro-finance models (Kekre and Lenel, 2022).

[19] The estimates presented in this paper are for households who already owned stocks before the lottery event. We refer the reader to Briggs et al. (2021) for similar results among households that do not participate in the stock market before the lottery event.

[20] We can only bound the *spending* response because we do not observe inter-household transfers and donations in our data. Relatedly, we can only bound the *consumption* response because we cannot distinguish between durable and nondurable expenditures.

[21] The small fraction of wealth invested in real estate suggests that housing investments are unlikely to explain the negative wealth effects in our baseline analyses. This conclusion is further bolstered by our finding (in Panel (b) of Figure 5) that re-classifying real estate as a risky asset leads to an even greater decline in the risky share.

5 Structural Analysis

Patterns in our reduced-form results suggest our estimates are qualitatively consistent with a model in which nontradable human capital induces households to hold a larger share of their financial assets in equity, and that as financial wealth increases, this distortion becomes smaller and the share of risky assets in the financial portfolio decreases. In this section, we turn to a lifecycle portfolio choice model to demonstrate *quantitatively* that the nontradable human-capital channel can explain our results in an enriched environment featuring lifecycle income profiles calibrated to the Swedish data. We also consider an extended model that incorporates a realistic level of consumption habits to numerically assess whether the portfolio distortion induced by the nontradable human capital is large relative to that induced by the consumption habits in the opposite direction.

The workhorse model we study is based on Gomes and Michaelides (2005) and Cocco et al. (2005) and is similar in structure to other models of lifecycle portfolio choice including Cocco (2005), Fagereng et al. (2017) and Calvet et al. (2021). We briefly introduce the main model features and calibration before introducing the main results, and refer the reader to Appendix B for details of the model solution.

5.1 Model

Each period, an agent of age t chooses how much to consume C_t and what fraction of her savings α_t to invest in equity, with returns realized at age $t+1$. We assume agents cannot hold short positions, so $\alpha_t \in [0,1]$.

Demographics Each agent in our model is a single household with a fixed marital status $m \in \{0,1\}$. Households in our model fall into one of three education groups: high school education ($e=0$), some post-secondary education ($e=1$), and college degree or higher ($e=2$). Households have a maximum lifespan of $T=100$, but prior to reaching age T, face an age-dependent mortality risk with an exogenous probability of surviving from period t to $t+1$, denoted by π_t.

Preferences Agents have Epstein-Zin preferences (Epstein and Zin, 1991) defined over consumption C_t and wealth W_t,

$$V_t = \left\{ (1-\beta\pi_t)C_t^{1-1/\psi} + \beta\mathbb{E}\left[\pi_t V_{t+1}^{1-\gamma} + (1-\pi_t)bW_{t+1}^{1-\gamma}\right]^{\frac{1-1/\psi}{1-\gamma}} \right\}^{\frac{1}{1-1/\psi}}, \tag{13}$$

where γ is the coefficient of relative risk aversion, ψ is the intertemporal elasticity of substitution, β is the discount factor, and b is the bequest multiplier.

Income The labor income process H_t has both a permanent component P_t and a transitory component U_t. The log of the permanent component follows a random walk with innovation N_t. All innovations are assumed to be lognormally distributed with mean zero and education group-specific variances, $\sigma^2_{n,e}$ and $\sigma^2_{u,e}$. Formally, the labor income process before retirement is given by

$$H_t = \exp(f(t,m,e))P_tU_t, \tag{14}$$

$$P_t = P_{t-1}N_t, \tag{15}$$

where $f(t,m,e)$ is a deterministic function of age, marital status, and education. Hence, the labor income process is summarized by parameters $\sigma_{n,e}$, $\sigma_{u,e}$, and the coefficients that define $f(t,m,e)$.

At retirement age $t_R = 65$, all future labor income becomes non-stochastic. For any age greater than 65, income is defined by a replacement rate $\lambda_{m,e}$ of the age-65 permanent component of income, where $\lambda_{m,e}$ varies with education and marital status. Thus, $H_t = \lambda_{m,e} \cdot \exp(f(t_R,m,e))P_{t_R}$ for all $t \geq t_R$.

Housing We do not formally model housing but follow Gomes and Michaelides (2005) in modeling housing expenditures as an age-dependent share of income $h(t)$. Thus, housing expenditures of amount $h(t)H_t$ are subtracted from income each period.

Assets Agents can invest in two assets: a risk-free asset that pays out certain return R_f and a risky equity that pays stochastic return $R_{s,t}$. Equity returns are assumed to be lognormally distributed, with mean excess return μ_s. Log equity returns are given by

$$r_{s,t} - r_f = \mu_s + \varepsilon_{s,t}, \tag{16}$$

where $\varepsilon_{s,t}$ is distributed normally with mean zero and variance σ_s^2. Innovations to excess returns are correlated with innovations to the permanent component of the labor income, and we denote the correlation coefficient as $\text{corr}(\ln N_t, \varepsilon_{s,t}) = \rho_{n,s}$. Note that because in our sample of interest, all households already participate in the equity market, we do not require entry costs that are common in similar models.

Wealth Accumulation and Lottery Prizes The intertemporal budget constraint is given by

$$W_{t+1} = (1+R_f)(W_t - C_t) + \alpha_t (R_{s,t+1} - R_f)(W_t - C_t) + H_{t+1}(1-h_{t+1}) + L_{t+1}. \qquad (17)$$

To align the model with our empirical setting, households can receive unanticipated lottery winnings L_t. Households do not form expectations over the prize distribution, meaning prizes are exogenous and unexpected. Accordingly, we set $L_t = 0$ when solving the model. Whenever lottery winnings L_t are positive, they enter additively into the budget constraint.

Household Problem Households' decision problem can be summarized as follows:

$$\begin{aligned} V_t(W_t, P_t) = \max_{C_t, \alpha_t} & \{(1-\beta\pi_t)C_t^{1-1/\psi} + \beta\mathbb{E}\left[\pi_t V_{t+1}^{1-\gamma} + (1-\pi_t) b W_{t+1}^{1-\gamma}\right]^{\frac{1-1/\psi}{1-\gamma}}\}^{\frac{1}{1-1/\psi}}, \\ W_{t+1} = & (1+R_f)(W_t - C_t) + \alpha_t (R_{s,t+1} - R_f)(W_t - C_t) + H_{t+1}(1-h_{t+1}), \qquad (18) \\ 0 \leq & \alpha \leq 1, \end{aligned}$$

where the process for labor income H_t is given by Equations (14) and (15), and the asset returns $R_{s,t}$ are given by Equation (16). The policy function for α_t is the key object of interest for our purpose, which characterizes the optimal allocation of financial wealth to risky assets.[22]

[22] We focus on the optimal portfolio allocation (conditional on the amount saved) and do not explicitly address the consumption response for two reasons. First, from an empirical standpoint, we cannot accurately estimate the consumption response and only bound it from above due to data limitations, as discussed in Section 4.3. Second, from a modeling standpoint, standard lifecycle portfolio choice models (including the one used in this paper) are admittedly not well-suited to jointly address the elevated consumption response and the portfolio response, as the former typically requires the modeling of assets with different degrees of liquidity with costly adjustment (e.g., Laibson, Lee, Maxted, Repetto and Tobacman, 2023; Kaplan and Violante, 2014), whereas the latter requires the modeling of assets with different risk characteristics.

Table 4: Calibration of Parameters. This table presents our baseline calibration of model parameters. Panel A presents our calibration of preference parameters from Gomes and Michaelides (2005) and asset returns from Waldenström (2014). Panel B shows the estimated income process parameters for each education group, including the standard deviation of transitory and permanent income innovations, the correlation of equity returns and permanent income innovations, and the replacement rates of retirement income for single and married households.

A. Structural Model Parameters	
Discount factor, β	0.96
Relative risk aversion, γ	5
IES, ψ	0.2
Bequest multiplier, b	2.5
Risk-free return, r_f	0.02
Mean excess return, μ_s	0.06
Return std. dev., σ_s	0.21

B. Income Process by Education Group	No College	Some College	College
Transitory risk, σ_u	0.156	0.163	0.172
Permanent risk, σ_n	0.089	0.081	0.088
Equity correlation, $\rho_{n,s}$	-0.024	-0.022	-0.025
Rep. rate (single), λ	0.685	0.641	0.617
Rep. rate (married), λ	0.644	0.608	0.589

5.2 Calibration

We calibrate the model with standard preference parameters from Gomes and Michaelides (2005) and calibrate the labor income and equity return processes to reflect historical Swedish data. The resulting calibration is presented in Table 4. We assume an annual discount rate of $\beta = .96$, the coefficient of relative risk aversion $\gamma = 5$, the intertemporal elasticity of substitution $\psi = .2$, and the bequest multiplier $b = 2.5$. Equity returns are calibrated with $\mu_s = .06$ and $\sigma_s = .21$ to match the historical Swedish stock market returns (Waldenström, 2014). Additionally, agents are assumed to die with certainty at age 100, and we solve for policy functions beginning at age 18, the minimum age in our sample.

The labor income process described in Equation (14) is estimated from pre-lottery labor income realizations from our sample of lottery players. The estimation procedure follows Cocco et al. (2005) and Carroll and Samwick (1997), and Appendix B.3 summarizes the exact procedure used to estimate the parameters of the income process. Appendix Figure B.2 presents the resulting estimates of $f(t,m,e)$ for both single and married households.

We estimate the standard deviations of transitory and permanent risks, σ_u and σ_n, in the range of 0.156 – 0.172 and 0.081 – 0.089, respectively, which are comparable to values estimated in the US by Carroll (1997), Gourinchas and Parker (2002), and Cocco et al.

(2005). Furthermore, we estimate the correlations between equity returns and permanent labor income shocks in the range of -0.022 to -0.025, which are not significantly different from zero both statistically and economically.[23,24] This implies that income is fairly safe and not highly correlated with equity market returns in Sweden. Finally, we calibrate the income replacement rate after retirement λ to 0.589 – 0.685 to reflect the Swedish pension system (Laun and Wallenius, 2015) (see Appendix B.4).

Other calibrated parameters include survival probabilities (π_t), which are calibrated to observed mortality rates (see Appendix B.2), and housing expenditures (h_t), which are calibrated to be 30% of income while working and 20% of income in retirement.

5.3 Results

Using the above calibration, we solve for optimal portfolio allocation decisions and explore how windfall gains affect the share of financial wealth allocated to risky assets. In solving for the optimal policy function, we observe that the optimal risky share in the financial portfolio is increasing in permanent income but decreasing in financial wealth. Because income is not positively correlated with equity returns and not easily tradable, a household tilts their financial portfolio toward equity to offset this position, as reviewed in Section 2. The degree to which households overweight equity is increasing in the ratio of human capital to financial wealth, causing the risky portfolio share to increase in permanent income and decrease in financial wealth.

After solving the model, we simulate lottery winnings and portfolio decisions for a sample that has identical characteristics to lottery winners in our data. We then record the resulting portfolio allocations and estimate Equation (9) on the simulated dataset. This procedure provides a set of model-implied coefficients $\tilde{\beta}$ comparable to the causal estimates presented in Section 4. In particular, we focus on comparing the overall effect of the windfall on the risky share, the non-linear effects by prize size, and the effects by the (pre-lottery) human-capital-to-wealth ratio, all in the year of win $s = 0$.

[23] Following Campbell, Cocco, Gomes and Maenhout (2001), we use the excess stock returns lagged one year in estimating the correlation between stock returns and labor income shocks to allow for potential lags in the realization of labor income.

[24] While these estimates are comparable to similarly low values estimated in the US (e.g., Cocco et al., 2005; Davis and Willen, 2013), the evidence on the correlations between equity returns and labor income shocks in the literature remains mixed. Hence, we later perform sensitivity analyses around these values by assuming a commonly used correlation coefficient of 0.15, as in Campbell et al. (2001) and Gomes and Michaelides (2005).

Baseline Model

Column (1) of Table 5 displays our empirical estimates in the year of win, and column (2) presents the corresponding model-implied estimates under our baseline calibration. The first striking feature of the model-implied effects is that the model overpredicts the negative wealth effects on the risky share. The model predicts each \$100K received causes a 10.9-percentage-point decrease in the risky share, which is twice the size of our preferred quasi-experimental estimate. Thus, the model suggests that the negative wealth effects we estimate in the data are not only theoretically plausible, but also possibly less negative than expected.

Additionally, the model broadly matches the patterns of heterogeneity by prize size and the human-capital-to-wealth ratio. The model generally overpredicts the effects but matches the qualitative pattern of diminishing marginal effects by prize size. The model also well captures the heterogeneity by the human-capital-to-wealth ratio, because those with a higher ratio reduce the risky share of their financial portfolio by more. Overall, the qualitative patterns implied by the model align closely with our empirical estimates, providing support for our interpretation of the estimated negative effects.

We next undertake a series of exercises to investigate how alternative calibrations and assumptions might affect the model-implied estimates. Appendix Table A.8 presents the results from these analyses. In columns (2) and (3), we examine the sensitivity to preference parameters by increasing the relative risk aversion to $\gamma = 8$ and increasing the intertemporal elasticity of substitution to $\psi = 0.5$. In columns (4) and (5), we vary our structural model assumptions by removing the bequest motive ($b = 0$) and allowing for a positive correlation between stock returns and income innovations ($\rho_{n,s} = 0.15$). Finally, Calvet et al. (2007) show the historical equity premium is larger than the return realized by most households. In column (6), we thus impose a 2% management fee and vary the expected excess returns on equity to $\mu_s = 0.04$. In all cases, we find that the model-implied estimates are robust and remain largely unchanged.

Extended Model with Consumption Habits

Human capital and habit formation mechanisms impact the financial wealth elasticity of the risky share in opposite direction. To assess the relative strength of each factor, we therefore extend our baseline model to also incorporate consumption habits. We do so by mandating a compulsory consumption expense equivalent to 50% of the previous period's

Table 5: Comparison of Empirical Estimates with Model-Predicted Effects. This table compares our empirical estimates with the model-predicted estimates of wealth on the risky share, the effects stratified by prize size, and the effects stratified by the human-capital-to-wealth ratio. Column (1) presents our empirical estimates from Section 4. Column (2) displays the model-implied estimates from our baseline model. Column (3) shows the model-implied estimates from our extended model incorporating consumption habits, where consumption habits are approximated as 50% of the previous period's average household income.

		(1) Empirical Estimate	(2) Model (Baseline)	(3) Model (Habit)
Effect of $100K		-0.048	-0.109	-0.052
Prize Size	1K to 10K	0.002	-0.010	0.016
	10K to 100K	-0.012	-0.063	-0.012
	100K to 250K	-0.182	-0.314	-0.103
	250K+	-0.171	-0.504	-0.274
HC/W Ratio	Low	-0.035	-0.087	-0.060
	High	-0.055	-0.121	-0.048

average disposable income in each period.[25] Incorporating habits into the model this way confers two methodological advantages. First, it captures a realistic level of consumption habits without imposing any additional computational burden in solving the model. This is in contrast to modeling a fixed subsistence level that remains constant throughout the life cycle, which is an unrealistic assumption. It also differs from explicitly modeling an internal consumption habit as a function of the agent's own past consumption, which would require an additional state space. Second, our approximation allows for micro-founded interpretations, because the previous period's average income can be viewed as a noisy proxy of the agent's own past consumption or as the average consumption of the agent's demographic group. While there is no consensus in the literature on how consumption habits are formed, our approximation captures elements of the two leading theories that habits are formed internally through one's own past consumption (Constantinides, 1990;

[25] We consider 50% of the previous period's disposable income to be a reasonable approximation of consumption habits. This level aligns with the existing literature, which often estimates consumption habits to be within the range of 30% to 60% of the agent's past consumption (Polkovnichenko, 2007). Additionally, the agent's disposable income is commonly used as a proxy for consumption (Calvet and Sodini, 2014).

Polkovnichenko, 2007) or externally influenced by the consumption of the peer group (Bakshi and Chen, 1996; Chan and Kogan, 2002).

Column (3) of Table 5 presents the model-implied estimates from our extended model incorporating consumption habits. The extended model predicts each \$100K received causes a 5.2-percentage-point decrease in the risky share, which well matches our empirical estimate of a 4.8-percentage-point reduction. In comparison to our baseline model, the inclusion of consumption habits in our extended model generates decreasing relative risk aversion and thus results in less negative wealth effects on the risky share. Nonetheless, the overall effect on the risky share remains negative, indicating that the nontradable human capital channel quantitatively dominates the habit channel in this enriched framework featuring both calibrated income profiles and realistic consumption habits.

The extended model also well matches the heterogeneity by prize size, although it does less well in matching the heterogeneity by the human-capital-to-wealth ratio. Although the patterns of heterogeneity remain broadly comparable to the baseline, the overall magnitude of the effects is better aligned in this extended specification. While we primarily focus on the nontradable human capital as a plausible mechanism underlying our empirical findings, these results also highlight the importance of consumption habits or DRRA preferences, because the model best matches our empirical estimates in the presence of such factors. Lastly, we conduct a series of robustness checks on model parameters and assumptions and find that the results are not too sensitive to alternative calibrations (see Appendix Table A.9).

In summary, under a wide range of specifications and assumptions, our structural analyses suggest that the effect of a windfall gain on the risky share is negative, and that the magnitude of our estimated effect is quantitatively plausible. Importantly, we confirm our analyses in Section 2 that nontradable human capital generates negative wealth effects that seemingly imply increasing relative risk aversion, even when relative risk aversion is constant or decreasing in financial wealth. This finding highlights that carefully accounting for nontradable human capital and the full household decision problem is critical before observed portfolio shares can be used to inform risk preferences.

6 Conclusion

Better evidence on the impact of financial windfalls on risky shares can help distinguish between competing theories of investment behavior. Our study provides such evidence by analyzing the randomized assignment of lottery prizes among a sample of Swedish lottery players who have been matched to government registers with demographic and financial information. We study how financial windfalls impact the risky share up to four years after the lottery event, and conduct a number of follow-up analyses aimed at exploring the ability of different models to explain our new findings, both qualitatively and quantitatively.

The main finding in our quasi-experimental analyses is that wealth effects on the risky share are negative. This conclusion is robust across specifications and subsamples. We attribute the discrepancy between our findings and those of the prior literature to our use of a natural experiment that better approximates the "ideal experiment" suggested by Carroll (2002, p. 420). While not our primary focus, we also analyze how lottery wealth impacts a number of specific asset and liability subcategories. These estimates provide moments that could prove informative for model calibration or validation in settings that go well beyond our study.

We demonstrate that negative wealth effects are consistent with the *qualitative* predictions of a static portfolio choice model featuring nontradable human capital. We also derive three additional predictions from the static model and find that each aligns well with the empirical evidence. In further analyses, we show that a richer, dynamic lifecycle portfolio choice model predicts wealth effects that *quantitatively* match our results. Our findings do not imply that the previous literature's emphasis on consumption habits and DRRA preferences was incorrect or misplaced. However, they suggest that a productive direction for future work may be to extend the workhorse models in much of the current literature to ones that additionally allow for the nontradable human capital mechanism to operate (Bodie et al., 1992; Heaton and Lucas, 1997; Viceira, 2001).

Finally, our results may have implications for the optimal design of target date funds, which have become increasingly popular among retail investors. Currently, these funds allocate assets solely based on information about the investor's age. However, our theoretical and empirical analyses indicate that the optimal allocation depends not just on age, but also on human capital and accumulated financial wealth, suggesting that incorporating these additional factors may lead to more effective asset allocations (Dahlquist, Setty and Vestman, 2018).

References

Andersen, Steffen and Kasper Meisner Nielsen, "Participation Constraints in the Stock Market: Evidence from Unexpected Inheritance due to Sudden Death," *Review of Financial Studies*, 2011, *24* (5), 1667–1697.

Auclert, Adrien, "Monetary Policy and the Redistribution Channel," *American Economic Review*, 2019, *109*, 2333–2367.

_, Matthew Rognlie, and Ludwig Straub, "The Intertemporal Keynesian Cross," *NBER Working Paper 25020*, 2023.

Bach, Laurent, Laurent E Calvet, and Paolo Sodini, "Rich Pickings? Risk, Return, and Skill in Household Wealth," *American Economic Review*, 2020, *110* (9), 2703–2747.

Bakshi, Gurdip S and Zhiwu Chen, "The Spirit of Capitalism and Stock-Market Prices," *American Economic Review*, 1996, pp. 133–157.

Benzoni, Luca, Pierre Collin-Dufresne, and Robert S Goldstein, "Portfolio choice over the life-cycle when the stock and labor markets are cointegrated," *Journal of Finance*, 2007, *62* (5), 2123–2167.

Bodie, Zvi, Robert C Merton, and William F Samuelson, "Labor Supply Fexibility and Portfolio Choice in a Life Cycle Model," *Journal of Economic Dynamics and Control*, 1992, *16* (3), 427–449.

Briggs, Joseph S, David Cesarini, Erik Lindqvist, and Robert Östling, "Windfall Gains and Stock Market Participation," *Journal of Financial Economics*, 2021, *139*, 57–83.

Brunnermeier, Markus K and Stefan Nagel, "Do Wealth Fluctuations Generate Time-Varying Risk Aversion? Micro-Evidence on Individuals' Asset Allocation," *American Economic Review*, 2008, *98* (3), 713–736.

Calvet, Laurent, Claire Célérier, Paolo Sodini, and Boris Vallée, "Can Security Design Foster Household Risk-Taking?," *Journal of Finance*, August 2023, *78* (4), 1917–1966.

Calvet, Laurent E and Paolo Sodini, "Twin Picks: Disentangling the Determinants of Risk-Taking in Household Portfolios," *Journal of Finance*, 2014, *69* (2), 867–906.

__ **, John Y Campbell, and Paolo Sodini**, "Down or Out: Assessing the Welfare Costs of Household Investment Mistakes," *Journal of Political Economy*, 2007, *115* (5), 707–747.

Calvet, Laurent, John Campbell, Francisco Gomes, and Paolo Sodini, "The Cross-Section of Household Preferences," *NBER Working Paper 28788*, 2021.

__ **, John Y Campbell, and Paolo Sodini**, "Fight or Flight: Portfolio Rebalancing by Individual Investors," *Quarterly Journal of Economics*, 2009, *124* (1), 301–348.

Campbell, John Y and John H Cochrane, "By Force of Habit: A Consumption-Based Explanation of Aggregate Stock Market Behavior," *The Journal of Political Economy*, 1999, *107* (2), 205–251.

__ **and Luis M Viceira**, *Strategic Asset Allocation: Portfolio Choice for Long-Term Investors*, Oxford University Press, 2002.

__ **, Joao F Cocco, Francisco J Gomes, and Pascal J Maenhout**, "Investing Retirement Wealth: A Life-Cycle Model," in "Risk Aspects of Investment-Based Social Security Reform," University of Chicago Press, 2001, pp. 439–482.

Carroll, Christopher D, "Buffer-Stock Saving and the Life Cycle/Permanent Income Hypothesis," *Quarterly Journal of Economics*, 1997, *112* (1), 1–55.

Carroll, Christopher D., "Portfolios of the Rich," in Tullio Jappelli Luigi Guiso, Michael Haliassos, ed., *Household Finance*, The MIT Press, 2002, chapter 10, pp. 389–429.

Carroll, Christopher D and Andrew A Samwick, "The Nature of Precautionary Wealth," *Journal of Monetary Economics*, 1997, *40* (1), 41–71.

Cesarini, David, Erik Lindqvist, Matthew J Notowidigdo, and Robert Östling, "The Effect of Wealth on Individual and Household Labor Supply: Evidence From Swedish Lotteries," *American Economic Review*, 2017, *107* (12), 3917–46.

__ **,** __ **, Robert Östling, and Björn Wallace**, "Wealth, Health, and Child Development: Evidence From Administrative Data on Swedish Lottery Players," *Quarterly Journal of Economics*, 2016, *131* (2), 687–738.

Chan, Yeung Lewis and Leonid Kogan, "Catching Up with the Joneses: Heterogeneous Preferences and the Dynamics of Asset Prices," *Journal of Political Economy*, 2002, *110* (6), 1255–1285.

Chetty, Raj and Adam Szeidl, "Consumption Commitment and Risk Preferences," *Quarterly Journal of Economics*, 2007, *122* (2).

Chiappori, Pierre-André and Monica Paiella, "Relative Risk Aversion is Constant: Evidence From Panel Data," *Journal of the European Economic Association*, 2011, *9* (6), 1021–1052.

Cocco, Joao F, "Portfolio Choice in the Presence of Housing," *Review of Financial Studies*, 2005, *18* (2), 535–567.

_ , Francisco J Gomes, and Pascal J Maenhout, "Consumption and Portfolio Choice Over the Life Cycle," *Review of Financial Studies*, 2005, *18* (2), 491–533.

Constantinides, George M, "Habit Formation: A Resolution of the Equity Premium Puzzle," *Journal of Political Economy*, 1990, *98* (3), 519–543.

Cookson, Anthony, Erik Gilje, and Rawley Heimer, "Shale Shocked: Cash Windfalls and Household Debt Repayment," *Journal of Financial Economics*, 2022, *146*, 905–931.

Dahlquist, Magnus, Ofer Setty, and Roine Vestman, "On the Asset Allocation of a Default Pension Fund," *Journal of Finance*, 2018, *73*, 1893–1936.

Davis, Steven J and Paul Willen, "Occupation-Level Income Shocks and Asset Returns: Their Covariance and Implications for Portfolio Choice," *Quarterly Journal of Finance*, 2013, *3* (03n04), 1350011.

Druedahl, Jeppe and Alessandro Martinello, "Long-Run Saving Dynamics: Evidence from Unexpected Inheritances," *The Review of Economics and Statistics*, 2020, *104* (5), 1079–1095.

Epstein, L. G. and Stan Zin, "Substitution, Risk Aversion, and the Temporal Behavior of Consumption and Asset Returns: An Empirical Analysis," *Journal of Political Economy*, 1991, *99*, 263–286.

Fagereng, Andreas, Charles Gottlieb, and Luigi Guiso, "Asset Market Participation and Portfolio Choice over the Life-Cycle," *Journal of Finance*, 2017, *72* (2), 705–750.

— **, Martin Holm, and Gisle Natvik**, "MPC Heterogeneity and Household Balance Sheets," *American Economic Journal: Macroeconomics*, Otober 2021, *13* (4), 1–54.

Flavin, Marjorie and Takashi Yamashita, "Owner-Occupied Housing and the Composition of the Household Portfolio Over the Life-Cycle," *American Economic Review*, 2011, *101* (3), 609–614.

Gomes, Francisco and Alexander Michaelides, "Optimal Life Cycle Asset Allocation: Understanding the Empirical Evidence," *Journal of Finance*, 2005, *60* (2), 869–904.

Gourinchas, Pierre-Olivier and Jonathan A Parker, "Consumption Over the Life Cycle," *Econometrica*, 2002, *70* (1), 47–89.

Greenwood, Robin and Andrei Shleifer, "Expectations of Returns and Expected Returns," *Review of Financial Studies*, 2014, *27* (3), 714–746.

Grossman, Sanford J and Guy Laroque, "Asset Pricing and Optimal Portfolio Choice in the Presence of Illiquid Durable Consumption Goods," *Econometrica*, 1990, *58* (1), 25–51.

Hankins, Scott, Mark Hoestra, and Paige Marta Skiba, "The Ticket to Easy Street? The Financial Consequences of Winning the Lottery," *Review of Economics and Statistics*, August 2011, *93* (3), 961–969.

Heaton, John and Deborah Lucas, "Market Frictions, Savings Behavior, and Portfolio Choice," *Macroeconomic Dynamics*, 1997, (1), 76–101.

— **and** — , "Portfolio Choice and Asset Prices: The Importance of Entrepreneurial Risk," *Journal of Finance*, 2000, *55* (3), 1163–1198.

Imbens, Guido W., Donald B. Rubin, and Bruce I. Sacerdote, "Estimating the Effect of Unearned Income on Labor Supply, Earnings, Savings, and Consumption: Evidence from a Survey of Lottery Players," *American Economic Review*, 2001, *91* (4), 778–794.

Juster, Thomas, Joseph Lupton, James Smith, and Frank Stafford, "The Decline in Household Saving and the Wealth Effect," *The Review of Economics and Statistics*, 2006, *88*, 20–27.

Kaplan, Greg and Giovanni L Violante, "A Model of the Consumption Response to Fiscal Stimulus Payments," *Econometrica*, 2014, *82* (4), 1199–1239.

— **, Benjamin Moll, and Gianluca Violante**, "Monetary Policy According to HANK," *American Economic Review*, 2018, *108*, 697–743.

Kaplan, H. Roy, "Lottery Winners: The Myth and Reality," *Journal of Gambling Behavior*, 1987, *3* (3), 168–178.

Kekre, Rohan and Moritz Lenel, "Monetary Policy, Redistribution, and Risk Premia," *Econometrica*, 2022, *90*, 2249–2282.

Laibson, David, Peter Maxted, and Benjamin Moll, "A Simple Mapping from MPCs to MPXs," *NBER Working Paper 29664*, 2022.

— **, Sean Chanwook Lee, Peter Maxted, Andrea Repetto, and Jeremy Tobacman**, "Estimating Discount Functions with Consumption Choices over the Lifecycle," *NBER Working Paper 13314*, 2023.

Laun, Tobias and Johanna Wallenius, "A Life Cycle Model of Health and Retirement:The Case of Swedish Pension Reform," *Journal of Public Economics*, 2015, *127*, 127–136.

Lindqvist, Erik, Robert Östling, and David Cesarini, "Long-Run Effects of Lottery Wealth on Psychological Well-Being," *The Review of Economic Studies*, 2020, *87*, 2703–2726.

Litzenberger, Robert and Mark Rubinstein, "The Strong Case for the Generalized Logarithmic Utility Models as the Premier Model of Financial Markets," *Journal of Finance*, 1976, *31*, 551–571.

Meeuwis, Maarten, "Wealth Fluctuations and Risk Preferences: Evidence from US Investor Portfolios," Working Paper 2022.

Paiella, Monica and Luigi Pistaferri, "Decomposing the Wealth Effect on Consumption," *The Review of Economics and Statistics*, 2017, *99*, 710–721.

Parker, Jonathan, Antoinette Schoar, and Yang Sun, "Retail Financial Innovation and Stock Market Dynamics: The Case of Target Date Funds," *NBER Working Paper 28028*, 2022.

Polkovnichenko, Valery, "Life-Cycle Portfolio Choice with Additive Habit Formation Preferences and Uninsurable Labor Income Risk," *Review of Financial Studies*, 2007, *20* (1), 83–124.

Rubinstein, Mark, "The Valuation of Uncertain Income Streams and the Pricing of Options," *Bell Journal of Economics*, 1976, *7*, 407–425.

Samuelson, Paul A, "Lifetime Portfolio Selection by Dynamic Stochastic Programming," *Review of Economics and Statistics*, 1969, *51* (3), 239–246.

Viceira, Luis M, "Optimal Portfolio Choice for Long-Horizon Investors with Nontradable Labor Income," *Journal of Finance*, 2001, *56* (2), 433–470.

Vissing-Jørgensen, Annette, "Perspectives on Behavioral Finance: Does Irrationality Disappear with Wealth? Evidence from Expectations and Actions," *NBER Macroeconomics Annual*, 2003, *18*, 139–208.

Wachter, Jessica A and Motohiro Yogo, "Why Do Household Portfolio Shares Rise in Wealth?," *Review of Financial Studies*, 2010, *23* (11), 3929–3965.

Waldenström, Daniel, "Swedish Stock and Bond Returns, 1856-2012," in Rodney Edvinsson, Tor Jacobson, and Daniel Waldenström, eds., *Historical Monetary and Financial Statistics for Sweden, Volume II:: House Prices, Stock Returns, National Accounts, and the Riksbank Balance Sheet, 1620-2012*, Ekerlids Förlag, 2014.

** ONLINE APPENDIX **

A Supplemental Tables

Table A.1: Effect of Wealth (100K USD) on Risky Share. Panel A of this table presents coefficients, standard errors, sample size, and mean predicted outcome when lottery wealth is zero ($\hat{y}|L_i = 0$) from estimating Equation (9) using our baseline definition of the risky share. Panel B shows the analogous estimates when we classify structured products as risky assets with a risk exposure of 49% (Calvet et al., 2023). Panel C drops capital insurance products from the calculation of the risky share (Calvet et al., 2007). Panel D classifies real estate as risky assets.

	A. Baseline				B. Structured Products			
(s)	β_s	SE	N	$\hat{y}_{\|L_i=0}$	β_s	SE	N	$\hat{y}_{\|L_i=0}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0	-0.048	0.006	49,995	0.418	-0.041	0.006	49,955	0.424
1	-0.037	0.006	46,003	0.393	-0.027	0.006	46,003	0.400
2	-0.032	0.007	42,238	0.387	-0.021	0.007	42,238	0.394
3	-0.032	0.008	39,317	0.392	-0.022	0.008	39,317	0.401
4	-0.043	0.007	36,503	0.385	-0.035	0.007	36,503	0.395

	C. Capital Insurance Excluded				D. Real Estate			
(s)	β_s	SE	N	$\hat{y}_{\|L_i=0}$	β_s	SE	N	$\hat{y}_{\|L_i=0}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0	-0.047	0.007	49,955	0.444	-0.072	0.006	49,955	0.719
1	-0.033	0.006	46,003	0.418	-0.061	0.008	46,003	0.707
2	-0.025	0.007	42,238	0.410	-0.053	0.009	42,238	0.704
3	-0.022	0.008	39,317	0.416	-0.048	0.010	39,317	0.707
4	-0.026	0.009	36,503	0.407	-0.057	0.009	36,503	0.703

Table A.2: Testing for Random Assignment. This table shows the results from estimating Equation (10) in the pooled sample and the three lottery subsamples. Standard errors are clustered at the individual level and reported in parentheses. F-statistics and corresponding p-values are obtained from testing the joint significance of the indicated controls.

	(1)	(2)	(3)	(4)	(5)
	Pooled		Kombi	Triss	PLS
Fixed Effects	Group ID	None	Group ID	Group ID	Group ID
Demographic Controls					
Female	222.5	131.7	-	6,211.2	22.4
	(233.8)	(195.1)	-	(7,231.0)	(15.6)
Nordic born	-716.2	-900.0	-875.1	-17,384.3	-175.7
	(759.2)	(816.6)	(1,784.5)	(21,988.2)	(183.1)
College	42.9	-327.8	68.3	-317.0	0.3
	(238.4)	(248.6)	(328.0)	(9,562.2)	(19.6)
Household size	105.0	251.7	271.7	301.7	-2.1
	(135.1)	(143.8)	(249.6)	(2,735.8)	(12.5)
Household income ($\times 100$)	0.034	0.481	-0.886	10.523	-0.013
	(0.480)	(0.505)	(0.513)	(19.600)	(0.018)
Married	-288.7	-398.7	-704.2	-277.4	1.6
	(301.2)	(316.0)	(463.5)	(11,296.0)	(5.1)
p for joint test of cubic in age	0.380	<0.000	-	0.105	0.311
F-stat (demographic controls)	**1.061**	**7.686**	**1.647**	**1.213**	**1.280**
p	**0.388**	**0.000**	**0.144**	**0.283**	**0.242**
Financial Controls					
Net wealth ($\times 1000$)	-0.180	-1.350	-0.725	4.375	-0.022
	(0.269)	(0.288)	(0.744)	(14.637)	(0.023)
Gross debt ($\times 1000$)	0.113	0.584	-1.231	50.425	0.017
	(0.211)	(0.385)	(0.991)	(64.133)	(0.006)
Homeowner	282.1	455.4	522.1	3,297.6	23.4
	(235.5)	(254.2)	(426.9)	(9,234.9)	(17.6)
Risky share	438.3	-154.8	355.4	12,271.0	16.5
	(305.5)	(315.2)	(514.7)	(11,009.9)	(28.0)
F-stat (financial controls)	**0.730**	**6.200**	**0.968**	**0.490**	**2.585**
p	**0.571**	**0.000**	**0.424**	**0.743**	**0.035**
Demographic + Financial Controls					
F-stat (all controls)	**1.074**	**6.629**	**1.421**	**0.993**	**1.590**
p	**0.376**	**0.000**	**0.172**	**0.456**	**0.080**

Table A.3: Effect of Wealth on Risky Share by Prize Size. Coefficients are obtained by estimating Equation (9) with the lottery-wealth variable replaced by indicators for five mutually exclusive prize categories: 0 to 1K USD, 1.K to 10K, 10K to 100K, 100K to 250K, and 250K+. The smallest prize category is omitted in the regression.

	Prize Size Category			
	$1K < L_i \leq 10K$	$10K < L_i \leq 100K$	$100K < L_i \leq 250K$	$250K < L_i$
	(1)	(2)	(3)	(4)
Estimate	0.002	-0.012	-0.182	-0.171
SE	0.008	0.017	0.016	0.032
N	478	801	202	51

Table A.4: Effect of Wealth (100K USD) on Risky Share, Heterogeneity by HC/W Ratio and Mode of Payment. This table presents coefficients, standard errors, sample size, and mean predicted outcome when lottery wealth is zero ($\hat{y}|L_i = 0$) from estimating Equation (9). Panels A and B present results for households with (pre-lottery) human-capital-to-wealth ratios that are above and below the median. Panels C and D present results for Triss winners stratified by type of payment plan, lump sum or monthly installments. Hetero p is obtained from an F-test of the null hypothesis that the two lottery-wealth coefficients are identical.

	A. HC/W High				B. HC/W Low				A - B
(s)	β_s	SE	N	$\hat{y}_{\|L_i=0}$	β_s	SE	N	$\hat{y}_{\|L_i=0}$	Hetero p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
0	-0.055	0.008	24,978	0.416	-0.035	0.009	24,977	0.420	0.081
1	-0.042	0.007	23,284	0.392	-0.030	0.009	22,719	0.394	0.293
2	-0.040	0.009	21,432	0.383	-0.023	0.009	20,806	0.390	0.199
3	-0.044	0.013	19,978	0.386	-0.023	0.009	19,339	0.399	0.201
4	-0.056	0.013	18,532	0.375	-0.034	0.008	17,971	0.396	0.155

	C. Triss Lump Sum				D. Triss Monthly				C - D
(s)	β_s	SE	N	$\hat{y}_{\|L_i=0}$	β_s	SE	N	$\hat{y}_{\|L_i=0}$	Hetero p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
0	-0.023	0.006	1,065	0.353	0.003	0.006	227	0.350	0.002
1	-0.013	0.006	904	0.358	0.017	0.007	205	0.253	<0.001
2	-0.007	0.007	731	0.347	0.011	0.009	167	0.244	0.122
3	-0.009	0.008	598	0.352	0.013	0.011	138	0.222	0.088
4	-0.019	0.007	472	0.360	-0.016	0.013	108	0.368	0.668

Table A.5: Heterogeneous Effect of Wealth (100K USD) on Risky Share. Coefficients are obtained by estimating Equation (9) at time $s = 3$ in our pooled sample stratified by various demographic and financial characteristics. Hetero p is obtained from an F-test of the null hypothesis that the two lottery-wealth coefficients are identical. Income risk is proxied by the standard deviation of annual income changes prior to the lottery. Equity returns are based on the MSCI Sweden Index the calendar year prior to the lottery.

	Demographic Characteristics							
	Sex		Age		College		Self-employed	
	Male	Female	≤ 45	> 45	No	Yes	No	Yes
Effect	-0.051	-0.025	-0.037	-0.035	-0.048	-0.005	-0.043	0.002
SE	0.013	0.012	0.028	0.009	0.010	0.017	0.010	0.015
p	<0.001	0.038	0.177	<0.001	<0.001	0.742	<0.001	0.877
Hetero p	0.146		0.929		0.029		0.011	
N	19,113	20,204	5,641	33,676	27,385	11,932	37,067	2,250

	Financial Characteristics							
	Debt		Homeowner		Income Risk		Equity Returns	
	≤ 0	> 0	No	Yes	Low	High	≤ 0	> 0
Effect	-0.070	-0.029	-0.053	-0.035	-0.064	-0.017	-0.063	-0.006
SE	0.023	0.010	0.016	0.011	0.013	0.011	0.013	0.012
p	0.003	0.005	0.001	0.001	<0.001	0.112	<0.001	0.619
Hetero p	0.106		0.342		0.006		<0.001	
N	16,237	23,080	7,586	31,731	19,915	19,402	27,758	11,559

Table A.6: Non-Experimental Estimates of Financial Wealth Elasticity of Risky Share. Coefficients are obtained by estimating Equation (12) in our pooled sample with an end date the year prior to the lottery win. Regressions are estimated at 2-year and 5-year horizons using both OLS and 2SLS. This estimation closely replicates Table 4 of Brunnermeier and Nagel (2008). Observations are weighted by their respective contribution to the identifying variation in the lottery regressions. p-value is obtained from an F-test of the null hypothesis that the two coefficients are identical.

	2 Year		5 Year	
	(1)	(2)	(3)	(4)
Estimator	OLS	2SLS	OLS	2SLS
Lottery Sample (LS)				
$\Delta\log(W_t)$	-0.039	-0.113	-0.019	0.105
SE	0.021	0.072	0.025	0.123
N	10,770	10,770	2,458	2,458
Brunnermeir and Nagel (BN)				
$\Delta\log(W_t)$	0.023	-0.136	-0.013	-0.012
SE	0.011	0.076	0.009	0.058
N	1,455	1,455	1,234	1,234
Test of Equal Coefficients				
LS-BN	-0.062	0.023	0.006	0.117
SE	0.024	0.105	0.027	0.136
p-value	0.009	0.83	0.82	0.39

Table A.7: Effect of Lottery Wealth on Net Wealth, Bonds, Stocks, Bank Account Balances, Real Assets, and Debt. This table presents results from estimating Equation (9) in our pooled sample where various asset classes are used as outcome variables. Lottery wealth and financial variables are measured in USD. The coefficients are scaled so that an estimate of 0.10 implies the total value of that asset class increases by 10 cents for each dollar won.

	Net Wealth $\sim (2)+(3)-(9)$	Real Assets	Financial Assets						Debt
			Total						
				Bonds	Interest Funds	Bank	Other	Equity	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$s = 0$	0.600	0.009	0.537	0.117	0.069	0.243	0.031	0.077	-0.041
$N = 49,955$	(0.041)	(0.024)	(0.041)	(0.024)	(0.030)	(0.040)	(0.010)	(0.026)	(0.011)
$s = 1$	0.569	0.052	0.454	0.126	0.048	0.120	0.051	0.110	-0.057
$N = 46,003$	(0.067)	(0.060)	(0.040)	(0.026)	(0.028)	(0.029)	(0.011)	(0.027)	(0.011)
$s = 2$	0.502	0.026	0.453	0.128	0.042	0.081	0.061	0.142	-0.013
$N = 42,238$	(0.059)	(0.049)	(0.064)	(0.029)	(0.032)	(0.024)	(0.021)	(0.058)	(0.032)
$s = 3$	0.463	0.062	0.391	0.105	0.006	0.091	0.073	0.116	-0.005
$N = 39,317$	(0.049)	(0.042)	(0.050)	(0.016)	(0.006)	(0.028)	(0.026)	(0.050)	(0.036)
$s = 4$	0.350	0.026	0.303	0.096	-0.008	0.121	0.059	0.034	-0.027
$N = 36,503$	(0.067)	(0.046)	(0.037)	(0.020)	(0.003)	(0.046)	(0.020)	(0.026)	(0.019)

Table A.8: Sensitivity of Results to Alternative Calibrations and Assumptions, Baseline Model. This table shows the sensitivity of the model-implied estimates from the baseline model in Section 5.3 to alternative calibrations and assumptions. Column (1) repeats the model-implied estimates under our baseline calibration summarized in Table 4. Column (2) shows the model-implied estimates when we increase the coefficient of relative risk aversion from $\gamma = 5$ to $\gamma = 8$. Column (3) increases the elasiticity of intertemporal substitution from $\psi = 0.2$ to $\psi = 0.5$. Column (4) removes the bequest motive by changing the bequest multiplier from $b = 2.5$ to $b = 0$. Column (5) assumes a positive correlation between equity returns and innovations to the permanent component of labor income $\rho_{n,s} = 0.15$. Column (6) lowers the mean excess returns on equity from $\mu_s = 0.06$ to $\mu_s = 0.04$.

		Baseline (1)	$\gamma = 8$ (2)	$\psi = 0.5$ (3)	$b = 0$ (4)	$\rho_{n,s} = 0.15$ (5)	$\mu_s = 0.04$ (6)
Participants		-0.109	-0.102	-0.107	-0.115	-0.104	-0.118
Prize Size	1K to 10K	-0.010	0.000	-0.005	-0.006	-0.012	-0.038
	10K to 100K	-0.063	-0.014	-0.038	-0.053	-0.065	-0.144
	100K to 250K	-0.314	-0.306	-0.298	-0.333	-0.306	-0.403
	250K+	-0.504	-0.437	-0.496	-0.524	-0.478	-0.520
HC/W Ratio	Low	-0.087	-0.078	-0.085	-0.101	-0.084	-0.066
	High	-0.121	-0.115	-0.118	-0.123	-0.115	-0.145

Table A.9: Sensitivity of Results to Alternative Calibrations and Assumptions, Extended Model. This table shows the sensitivity of the model-implied estimates from the extended model in Section 5.3 to alternative calibrations and assumptions. Column (1) repeats the model-implied estimates under our baseline calibration summarized in Table 4. Column (2) shows the model-implied estimates when we increase the coefficient of relative risk aversion from $\gamma = 5$ to $\gamma = 8$. Column (3) increases the elasiticity of intertemporal substitution from $\psi = 0.2$ to $\psi = 0.5$. Column (4) removes the bequest motive by changing the bequest multiplier from $b = 2.5$ to $b = 0$. Column (5) assumes a positive correlation between equity returns and innovations to the permanent component of labor income $\rho_{n,s} = 0.15$. Column (6) lowers the mean excess returns on equity from $\mu_s = 0.06$ to $\mu_s = 0.04$.

		Baseline	$\gamma = 8$	$\psi = 0.5$	$b = 0$	$\rho_{n,s} = 0.15$	$\mu_s = 0.04$
		(1)	(2)	(3)	(4)	(5)	(6)
Participants		-0.052	-0.039	-0.085	-0.052	-0.047	-0.035
Prize Size	1K to 10K	0.016	0.008	0.016	0.017	0.015	0.006
	10K to 100K	-0.012	-0.064	0.012	-0.014	-0.009	-0.032
	100K to 250K	-0.103	-0.098	-0.198	-0.104	-0.087	-0.086
	250K+	-0.274	-0.216	-0.401	-0.278	-0.257	-0.182
HC/W Ratio	Low	-0.060	-0.042	-0.087	-0.061	-0.058	-0.035
	High	-0.048	-0.039	-0.086	-0.048	-0.042	-0.036

B Model Solution and Details

B.1 Model Solution

Following Carroll (1997) and Gomes and Michaelides (2005), we exploit the model's homotheticity and normalize the value function, state variables, and controls by the permanent component of income P_t, thereby dropping P_t as a state variable to reduce the computational burden. We use lower case letters to denote the normalized variables (e.g., $v_t = V_t/P_t$, $w_t = W_t/P_t$). After these transformations, the model is solved by backward induction using a modified endogenous grid method (see Carroll (2006) and Barillas and Fernández-Villaverde (2007) for more details). Assuming the last period's utility specified as $v_T = b(w_T)^{1-\rho}$, we solve for the optimal saving policy $w_{T-1} - c_{T-1}$ and portfolio allocation rule α_{T-1} using an interpolated grid search (100 grid points). To calculate the expected value of next period's value function, we follow the procedure described in Gomes and Michaelides (2005) to create a state transition matrix that makes integration less computationally costly. After having obtained the optimal saving and portfolio allocation policies, we calculate the value function v_{T-1}. We then repeat this process and iterate backward until reaching age t_0. We repeat this procedure for all combinations of education group and marital status and store the resulting policy functions.

B.2 Survival Probabilities

The survival probability is calculated using the observed survival probabilities from years 1999-2000. We randomly select 100,000 individuals in year 1999 from the Swedish population and define a binary indicator equal to 1 if the individual is observed alive in 2000. We then regress a quartic in age on this indicator. For simplicity, we do not permit time or cohort effects in our estimation and do not allow survival probabilities to vary with wealth, income, or sex. Importantly, there is no attrition or selection concerns in this sample, because it is drawn randomly from the entire population. The resulting estimates are presented in Figure B.1.

B.3 Labor Income Process Estimation

We estimate the labor income process following the procedure described in Cocco et al. (2005). Our definition of income is total income after taxes and trasfers. As noted in

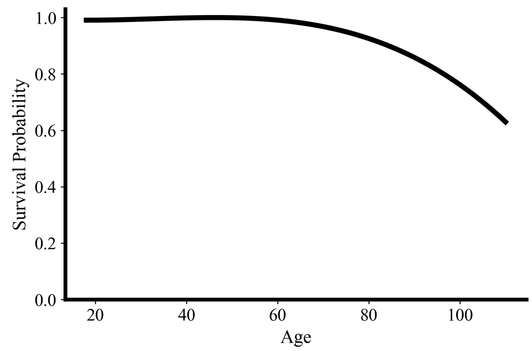


Figure B.1: Survival Probabilities. This figure presents the estimated one-year survival probability for each age. Survival probabilities are calculated as the average observed 1999-2000 survival probabilities for a random 100,000 individual sample from the Swedish population.

Campbell et al. (2001) and Storesletten, Telmer and Yaron (2007), this definition captures (potentially endogenous) insurance mechanisms – including government transfers, family transfers, and spousal labor supply decisions – without an explicit modeling of such means through which households protect themselves against labor income risk. Our estimation sample is the sample of lottery winners in (up to) 22 years prior to the lottery event. We estimate the income process separately for each of the education groups that we consider.

The estimation procedure proceeds in several steps. We first regress the log of household after-tax income on a quartic polynomial in age for households between age 18-65. We then regress the fitted value of this regression on an indicator variable for marital status to obtain an average income profile $f(t,m,e)$. The resulting average income profile estimates $exp(f(t,m,e))$ are shown in Figure B.2. Our estimated income profile is similar to the one estimated in Calvet et al. (2021) for the whole Swedish population.

To estimate the variances of permanent and transitory shocks to labor income for each education group, we follow Carroll and Samwick (1997) and define

$$log(H^*_{i,t}) \equiv log(H_{i,t}) - \hat{f}(\text{age}_{i,t}, m_{i,t}) \tag{19}$$

which then implies

$$Var[log(H^*_{i,t+d}) - log(H^*_{i,t})] = d\sigma_u^2 + 2\sigma_n^2. \tag{20}$$

We estimate σ_u^2 and σ_n^2 by running an OLS regression of $Var[log(H^*_{i,t+d}) - log(H^*_{i,t})]$ on d

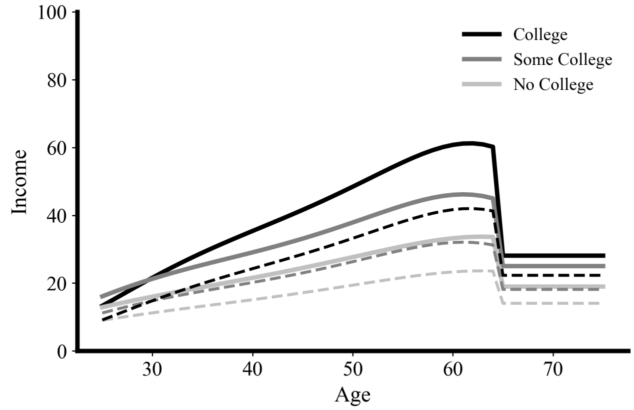


Figure B.2: Average Income Profiles. This figure presents the deterministic component of the estimated income process for single (dashed) and married (solid) households. The income profiles are estimated from our sample of lottery winners up to 22 years prior to the lottery event, using the methodology described in Cocco et al. (2005).

and a constant term.

We follow a similar procedure to estimate the correlation between equity returns and labor income shocks. Note that the change in $log(H^*_{i,t})$ can be written as

$$\Delta log(H^*_{i,t}) = log(N_{i,t}) + log(U_{i,t}) - log(U_{i,t-1}). \tag{21}$$

Decomposing $N_{i,t}$ into (implicit) aggregate and idiosyncratic components and averaging across households yields

$$\overline{\Delta log(H^*_{i,t})} = log(N^{agg}_t). \tag{22}$$

The correlation between equity returns and $log(N_{i,t})$ is then recovered by the coefficient from an OLS regression of $\overline{\Delta log(H^*_{i,t})}$ on excess returns, where excess returns are defined as the difference between Stockholm Stock Exchange and short-term Swedish Treasury returns (Waldenström, 2014).

B.4 Retirement Income Replacement Rates

We approximate retirement income replacement rates using the formulas described in Laun and Wallenius (2015), which conducts a detailed analysis of the Swedish pension system. We adopt a slightly simplified version of the formulas under the assumption that labor supply is exogenous.

The retirement income has two parts. First, all households receive 96% of a basic amount (BA) of 43,600 SEK (6,500 USD). Second, a supplemental earning is given by $0.6 \times AP \times BA$, where AP denotes pension points calculated from the 15 years with highest observed income computed recursively by the following formula:

$$AP_{t+1} = AP_t + \frac{1}{15}\max(0, \frac{\min(Y_t, 7.5 \times BA) - BA}{BA} - AP_t). \tag{23}$$

Hence, we approximate retirement income as the ratio of the following to age 65 income:

$$0.96 \times BA + 0.6 \times AP \times BA. \tag{24}$$

To reduce computational burden, we do not carry pension points as a state variable as in Laun and Wallenius (2015), but instead simulate 20,000 income processes for each education and marital status and calculate the average replacement rate for each group.

B.5 Expected Human Capital Calculation

We describe how we calculate the expected human capital (introduced in Section 4.2). Since our goal is to explore the heterogeneity of wealth effects by the pre-lottery human-capital-to-financial-wealth ratio, we focus on calculating the expected human capital in the year before the lottery event.

Following Calvet and Sodini (2014), we set the variance of the permanent component of the labor income process to its steady state value,

$$\sigma_p^2 = \frac{1}{2}(\sqrt{\sigma_n^4 + 4\sigma_u^2\sigma_n^2} - \sigma_n^2), \tag{25}$$

and compute the conditional mean (in the year prior to the lottery) as

$$\mu_{p,i,t-1} = h^*_{i,t-1}\frac{\sigma_p^2}{\sigma_u^2}, \tag{26}$$

where $h^*_{i,s} = \log(H^*_{i,s}) \equiv \log(H_{i,s}) - \hat{f}(\text{age}_{i,s}, m_{i,s}, e_{i,s})$ denotes the difference between the (observed) log income and its fitted value.[26]

[26] We assume that permanent income coincides with actual income in date $t-2$ (i.e., $\mu_{p,i,t-2} = 0$).

Given the age is fully predictable over time (whereas education and marital status are assumed to remain constant), it follows that

$$\begin{aligned}\mathbb{E}_{t-1}(H_{i,t-1+n}) &= \exp(f(\text{age}_{i,t-1+n}, m_i, e_i))\mathbb{E}_{t-1}(e^{p_{i,t-1+n}+u_{i,t-1+n}}) \\ &= \exp(f(\text{age}_{i,t-1+n}, m_i, e_i) + \frac{1}{2}\sigma_u^2 + \frac{1}{2}Var_{t-1}(p_{i,t-1+n})).\end{aligned} \tag{27}$$

Since the relation $p_{i,s+n} = p_{i,s} + n_{i,s+1} + \ldots + n_{i,s+n}$ implies $Var_s(p_{i,s+n}) = Var_s(p_{i,s}) + n\sigma_n^2$, it follows that

$$\mathbb{E}_{t-1}(H_{i,t-1+n}) = \exp(f(\text{age}_{i,t-1+n}, m_i, e_i) + \frac{1}{2}(\sigma_u^2 + \sigma_p^2 + n\sigma_n^2)). \tag{28}$$

Finally, the expected human capital is given by

$$\begin{aligned}HC_{i,t-1} &= \sum_{n=1}^{T_{i,t-1}} \pi_{i,t-1,t-1+n} \frac{\mathbb{E}_{t-1}(H_{i,t-1+n})}{(1+r)^n} \\ &= \sum_{n=1}^{T_{i,t-1}} \pi_{i,t-1,t-1+n} \frac{\exp(f(\text{age}_{i,t-1+n}, m_i, e_i) + \frac{1}{2}(\sigma_u^2 + \sigma_p^2 + n\sigma_n^2))}{(1+r)^n},\end{aligned} \tag{29}$$

where $T_{i,s}$ is the difference between 100 and the age at date s, and $\pi_{i,s,s+n}$ denotes the probability of survival at date $s+n$ conditional on being alive at date s.

C Bank Account Balance Imputation

C.1 Background

Between 1999 and 2007, banks were legally required to report account balances and interest payments to the Swedish Tax Authority (the relevant law is SFS 2001:1227). All accounts that earned at least 100 SEK in interest had to be reported. Moreover, during 2006 and 2007, accounts with a year-end balance of at least 10,000 SEK also had to be reported. For accounts with less than 100 SEK interest, the law did not require that the amount of interest paid be reported, regardless of the account balance.[27] During the whole

[27] One additioinal complication is that individual bank accounts are not observed during 2001 and 2002. For these years, we only observe the total account balance and the total amount of interest paid for all reported accounts at the individual level.

period, however, banks were allowed to also report accounts that did not meet these criteria. We follow Nekoei and Seim (2023) and use these voluntarily reported accounts to impute account balances for accounts with missing balances.

Column (2) of Table C.1 shows the number of individuals with at least one account in the Wealth Registry. The registry includes children and sometimes also deceased individuals (most likely because the division of the estate had not been settled yet). Over the whole period, 7,050,382 individuals are linked to a reported account at least once during the sample period, which can be compared to the total population at the time of about 9 million. Due to a lower interest rate, the number of registered accounts was particularly low during 2004-2005. The number of reported accounts increased substantially in 2006 when all accounts with an account balance of more than 10,000 SEK had to be reported.

Column (3) shows total bank holdings for the reported accounts in the Wealth Registry in billion SEK. These estimates can be compared to the household's total bank savings according to the national financial accounts (sum of FA2200 and FA2900 in Statistics Sweden's data) in column (4). The incomplete reporting implies about 13% of all wealth held in bank accounts during 1999-2007 is not attributed to any individual. As shown in column (5), our imputation procedure (to be described below) brings the total wealth attributed to each individual much closer to the total bank account balance in the national accounts.

Notably, the Wealth Registry does not include cash. Column (6) of Table C.1 shows the total amount of cash held by households during the period according to the national financial accounts. Because we have no information about the distribution of household cash holdings, we do not attempt to impute cash holdings. Instead, Section C.4 below shows robustness analyses where cash holdings are taken into account.

C.2 Voluntarily Reported Accounts

Column (2) of Table C.2 shows the number of accounts that were reported despite not meeting the interest payment ($R \geq 100$ SEK) or bank balance ($B \geq 10,000$ SEK) criteria. Some of these accounts had interest payments and account balances of exactly zero. For the years 2001 and 2002, however, we cannot distinguish accounts that are non-reported from voluntarily reported accounts with zero balance and therefore treat accounts with zero balance and zero interest payment as missing (although most likely some of them actually had a zero account balance). Column (4) shows the fraction of voluntarily reported accounts that belonged to an individual who had at least one voluntarily reported account in the

Table C.1: Overview of Reported Account Balances. This table shows the number of individuals with at least one observable account in the Wealth Registry, and the total bank account balances at the end of each year in the Wealth Registry, in the national financial accounts, and in our imputed measure of account balances. The last column shows the total amount of cash held by households in the national financial accounts.

	(1) Riksbank Rate (%)	(2) Individuals w/ Accounts	(3) Wealth Registry	(4) National Accounts	(5) Imputed Wealth	(6) Cash
1999	3.0	3,551,504	382	436	438	70
2000	3.7	3,675,831	375	424	427	71
2001	4.0	4,015,354	410	475	491	77
2002	4.1	3,847,879	410	519	518	77
2003	3.1	3,871,882	473	552	550	79
2004	2.2	2,754,495	424	564	555	78
2005	1.7	2,674,848	443	615	573	80
2006	2.2	5,175,615	701	708	711	81
2007	3.5	5,715,269	863	867	869	80

previous year. There is a high degree of persistence in the voluntary reporting of accounts, which implies that banks use similar rules from year to year to determine which accounts to report voluntarily. Our data do not allow us to match accounts to banks. However, Nekoei and Seim (2023) show in their Online Appendix that a couple of large banks reported the same number of accounts before and after the law change in 2006, suggesting that these banks simply decided to report all accounts regardless of their balance and interest.

The voluntarily reported accounts in our data are not entirely representative of non-reported accounts. To illustrate this, Figure C.1 shows the bank account balances for accounts with interest payments around the cutoff at 100 SEK. Note that the scale on the vertical axis changes from year to year, where the jump at the threshold is particularly large in 2004 and 2005 when the interest rates were low. As is clear from these plots, voluntarily reported accounts tend to have lower account balances. In our imputation procedure (to be described below), we use the jump at the threshold to adjust imputed bank account balances.

Table C.2: Overview of Voluntarily Reported Accounts. This table shows the number of voluntarily reported accounts, the number of accounts with zero balance and zero interest payment, and the fraction of voluntarily reported accounts that belong to an individual who had at least one voluntarily reported account in the previous year.

	(1) Legal Requirement	(2) # of Voluntarily Reported Accounts	(3) Accounts with $R = 0$ & $B = 0$	(4) Persistence (%)
1999	$R \geq 100$	525,463	3,969	
2000	$R \geq 100$	739,802	7,164	55
2001	$R \geq 100$	298,153*	0*	64
2002	$R \geq 100$	260,640*	0*	57
2003	$R \geq 100$	773,915	67,172	25
2004	$R \geq 100$	783,742	66,525	63
2005	$R \geq 100$	895,160	44,917	63
2006	$R \geq 100$ or $B \geq 10,000$	605,472	60,496	66
2007	$R \geq 100$ or $B \geq 10,000$	568,978	78,467	56

Note: * refers to the number of individuals, not the number of accounts.

C.3 Imputation Procedure

We now turn to describing our procedure for imputing account balances for non-reported accounts. Compared to previous approaches to impute non-reported bank account balances (notably Bach et al. (2020) and Nekoei and Seim (2023)), our procedure is distinguished by our using individual accounts (as opposed to aggregated information at the individual level) and using the discontinuity in Figure C.1 to adjust account balances.

We proceed in eight steps:

1. We first merge all account-level data for 1999-2000 and 2003-2007. For 2001 and 2002, we only have individual-level data and thus treat an individual's total bank account holdings as one account and add it to the panel.

2. We carry forward the number of accounts so that the number of accounts per person is non-decreasing.

3. Since the data do not contain account identifiers, we link accounts from year to year by sorting accounts by interest paid and account balance and then break ties randomly. By construction, in 2001 and 2002, there will only be one account with a

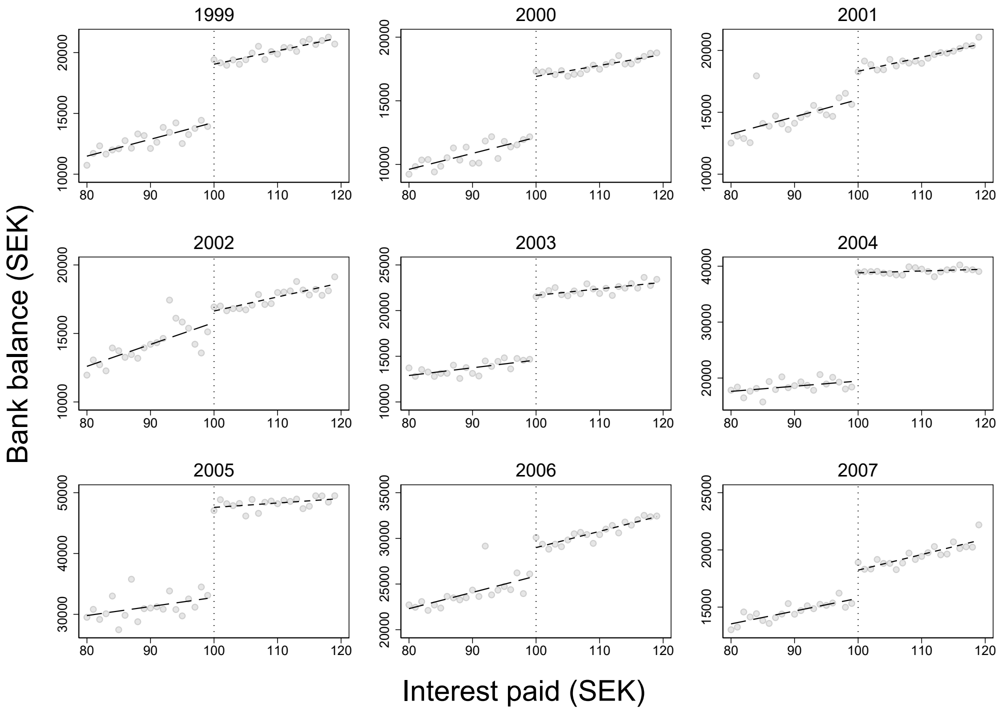


Figure C.1: Regression Discontinuity around the Reporting Threshold. This figure shows the average bank account balance around the account reporting threshold based on the amount of interest paid (i.e., annual interest payment of 100 SEK).

positive account balance per person, whereas all the other accounts will have a missing balance.

4. All individuals who are not observed in the Wealth Registry but are found in LISA are assigned one account for each year they are observed in LISA. LISA contains all individuals aged 16 or above that live in Sweden. Our assumption is that all individuals aged 16 and above have at least one bank account. Moreover, the Wealth Registry also contains accounts of younger children that had accounts meeting the reporting requirement. Bank account balances observed after a person's death are also included. We account for these accounts in order to calibrate the total amount of wealth distributed to the non-reported accounts.

5. For the years 1999-2000 and 2003-2005, we run the following regression for ac-

Table C.3: Predictors Used for Imputation. This table summarizes the definition of regression variables used in step 5 of our bank balance imputation procedure.

Variable Name	Variable Description
B_{it}	Reported bank balance
$income_{it}$	Total earnings
$real\_assets_{it}$	Real assets
$financial\_assets_{it}$	Financial assets net of reported bank balance
$debt_{it}$	Total gross debt
$BR_{i,t-s}$	Reported bank account balance lagged s years
	(set to missing if it does not meet the reporting requirement)
$D1_{it}$, $D2_{it}$, $D3_{it}$	Dummy for whether the account is the first, second, or third largest

counts with $R < 100$ separately for each year:

$$\begin{aligned} B_{it} =& \beta_0 + \beta_1 income_{it} + \beta_2 real\_assets_{it} + \beta_3 financial\_assets_{it} + \beta_4 debt_{it} \\ & + \beta_5 BR_{i,t-1} + \beta_6 BR_{i,t-2} + \beta_7 BR_{i,t-3} + \beta_8 age_{it} + \beta_9 age_{it}^2 + \beta_{10} age_{it}^3 + \beta_{11} female_i \\ & + \beta_{12} dead_{it} + \beta_{13} born\_nordic_i + \beta_{14} D1_{it} + \beta_{15} D2_{it} + \beta_{16} D3_{it} + \mathbf{MISS}_{it}\alpha + \varepsilon_{it}, \end{aligned} \tag{30}$$

where $\mathbf{MISS}_{it}$ is a vector of dummy variables indicating whether variable j is missing. All variables are set to 0 if they are missing, and monetary variables on the right-hand side are winsorized at the 1st and 99th percentiles. Apart from the self-explanatory demographic variables, all variables included in the regression are described in Table C.3.

Note that we only include lagged bank account balances that meet the reporting requirement, because voluntarily reported account balances in preceeding years contain different information for voluntarily reported accounts and non-reported accounts. Some banks reported all accounts, so a voluntarily reported account that has a voluntarily reported account balance in preceeding years is likely to be correctly linked, whereas a non-reported account with a voluntarily reported balance in preceeding years is likely to be incorrectly matched. The dependence on previous voluntarily reported bank account balances is therefore likely to differ between voluntarily reported and non-reported accounts. For 2006 and 2007, we run the same regression, but restrict the sample to accounts with $R < 100$ and $B < 10,000$. For 2001 and 2002, we run the same regression as above, but restrict the sample to individuals with $R < 100$ who are predicted to only have one bank account (based on the number of accounts in 1999 and 2000). The R^2 from these regressions is between 0.03 (in 2007) and 0.12 (in 2001).

6. We use the regression estimates from the previous step to predict the bank balance of all non-reported accounts. We winsorize negative values. For 2001 and 2002, we impute all accounts that an individual is believed to have in those years based on the number of accounts in 1999 and 2000.

7. We repeat steps 5 and 6 with R_{it} as the dependent variable instead. We winsorize negative values and values above 100.

8. Because Figure C.1 showed evidence of bank balances being lower in voluntarily reported accounts, we estimate the jumps in Figure C.1 assuming separate linear trends on different sides of the cutoff and using a bandwidth from $R = 80$ to $R = 120$. Denoting the resulting estimate of the jump δ_t, we adjust the imputed bank balances by adding the product of δ_t and $R_{it}/100$, where R_{it} is the imputed interest rate from step 7.

Table C.1 shows total bank holdings in the registry before and after our imputation, where the latter is much closer to the aggregate balance in the national accounts (without this balance being explicitly targeted in the imputation). Table C.4 shows summary statistics for the resulting imputed bank balances and interest payments. The much smaller imputed balances in 2006 and 2007 are due to the new reporting requirement in 2006, which mandated accounts with balances above 10,000 SEK to be reported regardless of the amount of interest payment.

C.4 Robustness to Imputation Method

We now turn to evaluating the robustness of our main empirical results to different assumptions regarding non-reported account balances and cash holdings.

We first consider the results when we abstain from imputing non-reported bank account balances and simply use the bank account balances reported in the Wealth Registry when computing the risky share. Panel (a) of Figure C.2 shows that the estimated effect of lottery wealth on the risky share of financial assets becomes more negative when we abstain from bank balance imputation. For example, at $t = 0$, each \$100K won reduces the risky share

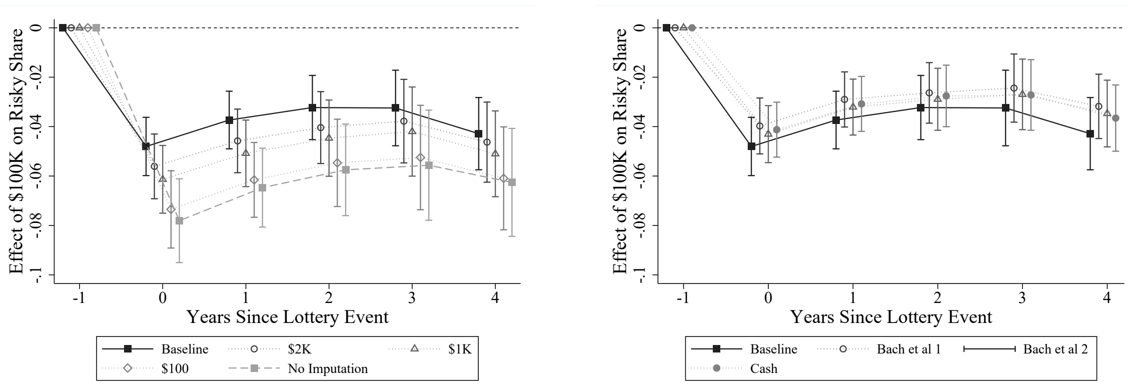
Table C.4: Summary Statistics of Imputed Bank Account Balances. This table shows the summary statistics of imputed bank balances and interest payments under our baseline imputation procedure described in Section C.3. The imputed bank balances and interest payments are denoted in SEK.

	Imputed bank balance (SEK)			Imputed interest payment (SEK)	
	Mean	Median	Max	Mean	Median
	(1)	(2)	(3)	(4)	(5)
1999	14,661	11,948	92,501	30.6	31.6
2000	12,860	8,978	324,956	28.8	30.1
2001	14,637	8,534	467,375	32.3	31.6
2002	18,751	11,433	412,937	27.9	29.2
2003	16,220	11,292	573,427	19.5	19.5
2004	20,147	15,024	544,352	12.3	10.3
2005	19,301	14,689	607,422	12.2	10.0
2006	2,303	2,132	6,247	14.0	11.9
2007	1,702	1,652	4,137	17.2	15.9

by 7.6 percentage points, compared to 4.8 percentage points in our baseline case (under which bank account balances are imputed as described above). When using non-imputed account balances, the effect is most likely overstated because the risky share is overstated for individuals whose balances are not reported (since non-imputed balances are set to 0). Moreover, as some lottery wealth ends up in bank accounts (see Figure 7), winning the lottery reduces the likelihood that bank account balances are not reported (and consequently that the risky share is overstated). To illustrate, Panel (a) of Figure C.2 shows that winsorizing non-imputed bank account balances from below at \$100, \$1,000 or \$2,000 reduces the discrepancy between the results based on imputed and non-imputed bank balances.

We next consider the alternative procedure for imputing bank balances from Bach et al. (2020). Their procedure differs from ours in several respects: they impute balances at the individual level rather than the account level, they only use information on assets in the current period, and they use a logaritmic specification rather than a linear specification. Moreover, they impute balances based on data from all reported accounts, not just using the voluntarily reported accounts. In addition, after the regression-based imputation, they adjust the imputed amounts so that the difference in bank holdings between the National Financial Accounts and the Wealth Registry is distributed to those with missing accounts.

In order to assess the robustness of our results to using Bach et al. (2020)'s alternative



(a) Non-Imputed Measures **(b) Alternative Approaches**

Figure C.2: Robustness to Alternative Imputations of Bank Account Balances. This figure shows the robustness of our main results to alternative procedures used for imputing bank account balances. Panel (a) shows the effect of wealth on the risky share when using non-imputed bank balances and when using non-imputed bank balances that are winsorized from below at $100, $1,000, and $2,000. Panel (b) shows the effect of wealth on the risky share when using imputation procedures similar to those in Bach et al. (2020) and when accounting for average cash holdings not reported in the Wealth Registry.

approach, we replicate their procedure as closely as possible. Due to data limitations, however, we deviate from their method in two ways. First, we use data on assets from the Wealth Registry as predictors rather than data from the underlying asset-level KURU registry. The classification of assets is somewhat different in the Wealth Registry compared to Bach et al. (2020)'s classification of assets in the KURU registry. For example, we use interest funds rather than money market funds as a predictor variable. Second, because we do not have demographic variables for the full population, we base the imputation on those aged 16 and above who appear in the LISA registry. We construct two measures of imputed bank balances in the spirit of Bach et al. (2020). The first measure ("Bach et al. 1") distributes the "missing bank holdings" (i.e., the difference between the national financial accounts and bank holdings in the Wealth Registry) only to people above age 16. In this case, the average imputed amount varies between SEK 14,596 (in 2006) and SEK 38,972 (in 2005). The second measure ("Bach et al. 2") assumes that the missing balance is distributed to everybody in the population, implying the average imputed amount is between SEK 8,951 (in 2006) and SEK 28,924 (in 2005). Panel (b) of Figure C.2 shows our results are robust to using these alternative imputation methods.

A final issue is that our imputation method does not take cash holdings into account.

Table C.1 shows that total cash holdings amount to 70-80 billion SEK for the period we consider. With approximately 7 million Swedish adults, average cash holdings are thus about 10,000 SEK ($1500) per individual. As a rough way to take cash holdings into account, we add $1500 to our baseline measure of imputed bank holdings. As shown in Panel (b) of Figure C.2, the results are robust to changing our baseline imputation to either of these alternative measures.

D Comparison to Prior Work

Here, we briefly discuss two studies whose conclusions conflict with our baseline finding that wealth effects on the risky share are negative.

Calvet and Sodini (2014) Calvet and Sodini (2014) analyzed Swedish longitudinal twin data and found a positive within-family association between financial wealth and the risky share. While there is a long history of relying on twin comparisons to improve causal inference, the key identifying assumption of so-called co-twin studies is quite strong. To many researchers, the intuitive appeal of the method is that twins, especially monozygotic twins, tend to be very similar, both genetically and in terms of their environmental backgrounds. Therefore, twin studies are effective at controlling for a number of factors, both genetic and environmental ones, that are hard to measure reliably and could lead to omitted-variable biases in conventional cross-sectional analyses. But as noted by Griliches (1979), the fact that twins' outcomes are usually highly correlated need not imply that any remaining differences are plausibly exogenous. *Something* must have caused the differences observed between the twins, and the critical identifying assumption is that the differences are as-good-as randomly assigned.

In practice, there is a body of literature that makes a compelling argument - both theoretically and empirically - that twin studies only recover causal estimates under strong assumptions and may well be vulnerable to exactly the same set of concerns about omitted-variable biases that are often voiced about cross-sectional analyses (Griliches, 1979; Neumark, 1999; Bound and Solon, 1999). Therefore, Calvet and Sodini (2014)'s identification strategy implicitly comes down to assuming that within-pair differences in financial wealth are unrelated to within-pair differences in other determinants of the risky share (conditional on some controls for within-pair differences that can be reliably measured). In contrast, the source of identifying variation in our study is transparent and comes from the randomized assignment of lottery wealth. Our identfication strategy is valid provided the lottery prizes were assigned according to the rules of the lottery.

Calvet, Campbell, and Sodini (2009) Calvet et al. (2009), also using the Swedish administrative data, investigate how portfolio-return-induced variation in financial wealth affects portfolio decisions among individual investors. Specifically, they estimate a portfolio adjustment model in which the change in financial wealth and a wide array of characteristics are used as predictors of the risky share. To account for the endogeneity of financial wealth, they use the zero-rebalancing portfolio return as an instrument for the contemporaneous change in financial wealth and find that an increase in financial wealth leads to a higher risky share. While the idiosyncratic component of portfolio returns can induce an exogenous variation in financial wealth, the results of this prior study likely reflect household inertia in portfolio rebalancing or what is known as the "flypaper effect" (Hines and Thaler, 1995; Choi, Laibson and Madrian, 2009). When households fail to rebalance promptly in response to yearly variations in financial wealth induced by portfolio returns, higher financial wealth can lead to a mechanical increase in the risky share. Although our results are not entirely immune to such inertial behavior, our analyses in Section 4.3 suggest the lottery winners' portfolio responses likely reflect an active decision, as only 24% (12%) of the winnings are retained in bank accounts in the initial year (after one year).

Appendix References

Bach, Laurent, Laurent E Calvet, and Paolo Sodini, "Rich Pickings? Risk, Return, and Skill in Household Wealth," *American Economic Review*, 2020, *110* (9), 2703–2747.

Barillas, Francisco and Jesús Fernández-Villaverde, "A Generalization of the Endogenous Grid Method," *Journal of Economic Dynamics and Control*, 2007, *31* (8), 2698–2712.

Bound, John and Gary Solon, "Double Trouble: On the Value of Twins-Based Estimation of the Return to Schooling," *Economics of Education Review*, 1999, *18*, 169–182.

Brunnermeier, Markus K and Stefan Nagel, "Do Wealth Fluctuations Generate Time-Varying Risk Aversion? Micro-Evidence on Individuals' Asset Allocation," *American Economic Review*, 2008, *98* (3), 713–736.

Calvet, Laurent, Claire Célérier, Paolo Sodini, and Boris Vallée, "Can Security Design Foster Household Risk-Taking?," *Journal of Finance*, August 2023, *78* (4), 1917–1966.

Calvet, Laurent E and Paolo Sodini, "Twin Picks: Disentangling the Determinants of Risk-Taking in Household Portfolios," *Journal of Finance*, 2014, *69* (2), 867–906.

_ **, John Y Campbell, and Paolo Sodini**, "Down or Out: Assessing the Welfare Costs of Household Investment Mistakes," *Journal of Political Economy*, 2007, *115* (5), 707–747.

Calvet, Laurent, John Campbell, Francisco Gomes, and Paolo Sodini, "The Cross-Section of Household Preferences," *NBER Working Paper 28788*, 2021.

_ **, John Y Campbell, and Paolo Sodini**, "Fight or Flight: Portfolio Rebalancing by Individual Investors," *Quarterly Journal of Economics*, 2009, *124* (1), 301–348.

Campbell, John Y, Joao F Cocco, Francisco J Gomes, and Pascal J Maenhout, "Investing Retirement Wealth: A Life-Cycle Model," in "Risk Aspects of Investment-Based Social Security Reform," University of Chicago Press, 2001, pp. 439–482.

Carroll, Christopher D, "Buffer-Stock Saving and the Life Cycle/Permanent Income Hypothesis," *Quarterly Journal of Economics*, 1997, *112* (1), 1–55.

_ , "The Method of Endogenous Gridpoints for Solving Dynamic Stochastic Optimization Problems," *Economics Letters*, 2006, *91* (3), 312–320.

— **and Andrew A Samwick**, “The Nature of Precautionary Wealth,” *Journal of Monetary Economics*, 1997, *40* (1), 41–71.

Choi, James, David Laibson, and Brigitte Madrian, “Mental Accounting in Portfolio Choice: Evidence from a Flypaper Effect,” *American Economic Review*, 2009, *99*, 2085–95.

Cocco, Joao F, Francisco J Gomes, and Pascal J Maenhout, “Consumption and Portfolio Choice Over the Life Cycle,” *Review of Financial Studies*, 2005, *18* (2), 491–533.

Gomes, Francisco and Alexander Michaelides, “Optimal Life Cycle Asset Allocation: Understanding the Empirical Evidence,” *Journal of Finance*, 2005, *60* (2), 869–904.

Griliches, Zvi, “Sibling Models and Data in Economics: Beginnings of a Survey,” *Journal of Political Economy*, 1979, *87*, 37–64.

Hines, James and Richard Thaler, “The Flypaper Effect,” *Journal of Economic Perspectives*, 1995, *9*, 217–226.

Laun, Tobias and Johanna Wallenius, “A Life Cycle Model of Health and Retirement:The Case of Swedish Pension Reform,” *Journal of Public Economics*, 2015, *127*, 127–136.

Nekoei, Arash and David Seim, “How Do Inheritances Shape Wealth Inequality? Theory and Evidence from Sweden,” *The Review of Economic Studies*, January 2023, *90* (1), 463–498.

Neumark, David, “Biases in Twin Estimates of the Return to Schooling,” *Economics of Education Review*, April 1999, *18* (2), 143–148.

Storesletten, Kjetil, Chris Telmer, and Amir Yaron, “Asset Pricing with Idiosyncratic Risk and Overlapping Generations,” *Review of Economic Dynamics*, 2007, *10*, 519–548.

Waldenström, Daniel, “Swedish Stock and Bond Returns, 1856-2012,” in Rodney Edvinsson, Tor Jacobson, and Daniel Waldenström, eds., *Historical Monetary and Financial Statistics for Sweden, Volume II:: House Prices, Stock Returns, National Accounts, and the Riksbank Balance Sheet, 1620-2012*, Ekerlids Förlag, 2014.