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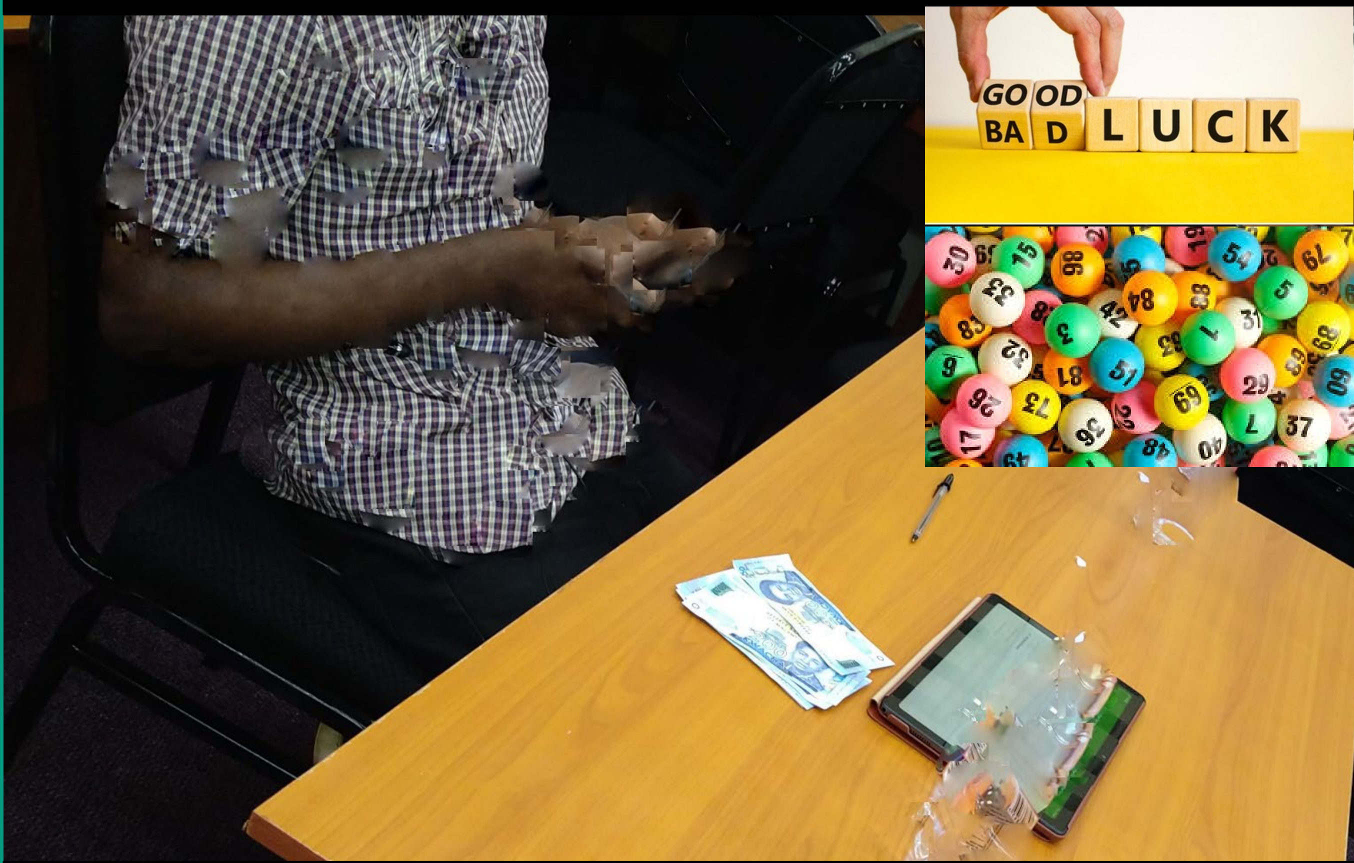

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The Predictive Power of Luck: Luck and Risk-Taking in a Repeated Risky Investment Game

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Abstract

Can luck predict risk-taking behavior in games of chance? Economists have not widely studied this issue although overconfidence, optimism-, and pessimism bias have received substantial attention in recent years. In this study, we investigate how good and bad luck outcomes in a simple repeated risky investment game affect risk-taking behavior in the following rounds of the same game where the outcome (luck) in the game is determined by the throwing of a die after each round. The outcome of the previous round's die-throw is known when the subjects decide how risky their next choice in the game will be. A sample of 718 university students is used as subjects in the game in a recursive within-subject design. The results demonstrate a strong impact of luck on risk-taking behavior that lasts not only to the next round but also into another two follow-up rounds, with cumulative effects. A time delay of 1-2 months between Round 1 and Round 2 did not wipe out the luck effect and it

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was only slightly weaker than the luck effect from Round 2 to Rounds 3 and 4 that followed immediately after Round 2. Many recent studies have shown that risk preferences respond to recent shocks. This study indicates that random shocks such as luck in previous games (states of nature) influence risk-taking behavior. Our study suggests that the causal mechanism goes through subjective beliefs in luck based on past experiences that influence expectations and thereby risk-taking behavior.

Keywords: Risky investment game, Luck, Illusion of control, Repeated game, Predictive power

JEL Classification: D8 , H51

1 Introduction

Many studies in behavioral and experimental economics have revealed that *Homo sapiens* does not behave perfectly rationally in the sense of Expected Utility Theory. However, irrational behavior is not completely random and may at least to some extent be predictable and Prospect Theory (PT) and Cumulative Prospect Theory (CPT) have been developed to better predict real behavior in situations under risk and uncertainty (Kahneman & Tversky, 1979; Tversky & Kahneman, 1992). The toolbox of behavioral and experimental economists that has been used to study behavior under risk and uncertainty has grown over time and the different tools have been scrutinized and refined. This is an ongoing process that involves interactions between alternative methods used to obtain data from heterogeneous subjects in heterogeneous environments. Two key behavioral regularities that are captured by CPT are that a) subjects tend be more willing to take risk when they are framed in a loss domain compared to a gain domain, and b) subjects are insensitive to changes in probabilities when these probabilities are in the range 0.3-0.7.

The low predictive power of theoretical models and risk preferences remains a notorious problem and the issue of measurement errors in experiments has received increasing attention. Such errors are associated with systematic biases related to the experimental design characteristics, random choice by subjects, and exposure to variable framing conditions. The recent literature on shocks and instability of risk references is an example of the latter. In this study, we focus on good and bad luck in a repeated game of chance. A perfectly rational subject should not change her/his response in the game based on random outcomes in previous rounds of the game. However, beliefs in luck that deviate from this view may be quite common. We postulate and test the hypothesis that irrational beliefs in good or bad luck are widespread and affect real behavior. It is common among people to perceive themselves as lucky or think of a day as their lucky day. It is also quite common to attempt to control luck through common rituals among people who usually are regarded as rational human beings although they may use statements such as ‘knock on wood to avoid bad luck or carry an artifact for good luck. Gamblers may blow on dice before throwing them. Athletes also commonly use certain rituals before their competitions. Langer (1975) found that people may develop an illusion of control[1] over outcomes that were determined by pure chance. Ejova, Delfabbro, and Navarro (2015) assessed gamblers' erroneous gambling-related beliefs and found them to be related to illusions of control and belief in luck that also could be religiously related or related to the gambler's fallacy.[2] The belief that luck and chance are different things does not only exist among gamblers but is also found among ordinary subjects in everyday situations (Wagenaar & Keren, 1988).

Another theoretical explanation for people's response to luck is the gambler's fallacy. LaPlace (1814) described the gambler's fallacy as the tendency to perceive independent events as negatively correlated. It would lead to less risk-taking after a win and more risk-taking after a loss. This theory may therefore predict the same outcome as CPT (higher willingness to take risk after losses) but the opposite of the illusion of control theory.

We investigate the importance of luck in previous rounds for risk-taking in later rounds in a simple risky investment game that has been used to study how myopic loss aversion may affect financial decisions and to measure risk tolerance (Charness & Viceisza, 2016; Gneezy, Leonard, & List, 2009; Gneezy & Potters, 1997). Gneezy and Potters (1997) used the risky investment game to illustrate that myopic loss aversion may explain the equity premium puzzle based on Prospect Theory (PT) by experimentally manipulating the evaluation period in the experiment. Myopic loss aversion predicts that less frequent evaluations (every third round) lead to more risky decisions than more frequent evaluations (every round) and this was what the authors found. The less frequent evaluation leads to a more favorable evaluation of the outcomes as the reference point changes less frequently. This is based on the PT assumption that utility is a function of change in wealth only and that losses are weighted more strongly than gains due to loss aversion.

Gillen, Snowberg, and Yariv (2019) found substantial 'measurement error' in the sense of unstable within-subject choices when repeated rounds of the risky investment game were played with the same subjects in a large student sample. They tried to correct such measurement errors with an instrumental variable approach where the risky investment decisions in the game were instrumented for each other. We try to dig deeper by assessing whether inconsistent choices in repeated rounds of the same game could be due to superstitious beliefs in good or bad luck and that is driven by the outcomes in earlier rounds of the game. We find robust evidence of such belief effects in terms of risk-taking decisions over repeated rounds of the risky investment game in a large sample of 718 university students.

[1]An illusion of control refers to a tendency to overestimate the extent to which one's actions influence outcomes in games of chance (Langer 1975).

[2]The gambler's fallacy is the belief that random sequences even tend to self-correct in the short run such as in coin tosses, dice throws, or other gaming instruments. Another type of gambler's fallacy is the belief that luck comes in cycles.

Economists have in the past assumed risk preferences to be stable over time (Stigler & Becker, 1977). However, recent literature demonstrates that risk preferences can be sensitive to shocks, although the evidence is mixed regarding whether negative shocks make people more or less risk tolerant. Some studies find that subjects have become more willing to take risks after negative shock exposure (Cavatorta & Groom, 2020; Hanaoka, Shigeoka, & Watanabe, 2015; Kahsay & Osberghaus, 2018; Page, Savage, & Torgler, 2014; Voors et al., 2012). Other studies find the opposite, that subjects have become less risk tolerant after exposure to shocks (Bourdeau-Brien & Kryzanowski, 2020; Brown, Montalva, Thomas, & Velásquez, 2019; Cassar, Healy, & Von Kessler, 2017; Guiso, Sapienza, & Zingales, 2018; Liebenehm, 2018). And yet other studies find that risk preferences are stable and unaffected by shocks (Brunnermeier & Nagel, 2008; Drichoutis & Nayga, 2021; Sahm, 2012). Our study uses good and bad luck outcomes in a repeated sequential real game to explore the effects of relatively small positive and negative shocks on the risk-taking responses in the following rounds of the same game. In our study, we can split our sample into subjects that respond to good (bad) luck by increasing, not changing, or decreasing their degree of risk-taking and assess how this partitioning is affected by luck outcomes in an earlier round(s) of the game. In addition to the non-parametric outcomes, we also run parametric models and assess the marginal effects of alternative outcomes from earlier game rounds taking place 1-2 months earlier and earlier on the same day. For the three game rounds taking place the same day, no payouts were implemented till after all the rounds were played to avoid any income (cash) effect in the games except through mental accounting.

Our study makes four substantial contributions to the literature on behavior under risk. First, the study provides insights on the notorious instability of responses (measurement error) in repeated rounds of the risky investment game as also found by Gillen et al. (2019) and demonstrates that the instability of responses in repeated rounds of the same game may not be purely random but may be influenced by good (bad) luck experiences. We demonstrate that the predictive power of the game can be substantially improved when the game is repeated, by including the luck outcome in earlier game rounds and this may be a more powerful tool than the use of the IV method of Gillen et al. (2019) who did not utilize such luck outcomes in their approach to deal with the measurement error problem. It is even possible that luck effects cause omitted variable bias in their predicted risky investment variable. Second, our study reveals that exposure to luck triggers more risk-taking and bad luck triggers less risk-taking on average. This finding seems to contradict PT in the sense that bad luck does not trigger more risk-taking on average. Taking luck into account may therefore enhance the predictive power of these theories that were developed to better predict behavior rather than to explain it. Third, we find heterogeneous response patterns when inspecting the response heterogeneity after good (bad) luck outcomes. Our study thus speaks to the rapidly expanding literature on how shocks affect risk tolerance and that provides mixed evidence on the risk-taking effects of such shocks. Fourth, our paper also contributes to the very limited literature on how luck affects risk-taking in different cultural contexts (Gao, Shi, & Zhao, 2021). To our knowledge, this is the first study of its kind in Africa. Beliefs in luck may vary across different cultural contexts. We should therefore also be careful about concluding the external validity of our findings. However, we think that economists who are interested in predicting behavior under risk and uncertainty should pay more attention to recent good or bad luck outcomes and possibly draw on theories from psychology on this (Darke & Freedman, 1997b).

2 Experimental design, procedure and hypotheses

We initially present the subjects with a binary (hypothetical) choice between $u(X)$ and $0.5u(3X)$. The safe amount is $X = 1000$ MKw.[3] The subjects were free to invest nothing, some, or all of the endowment$=x$ (in multiples of 200 MKw) in a 50-50 lottery with the researchers tripling the amount invested.

The real game is then presented as a choice between six alternatives with the options above as the extreme options and with four intermediate options:

1) $0.5u(3X)$
2) $0.5u(12/5X) + u(X/5)$
3) $0.5u(9/5X) + u(2X/5)$
4) $0.5u(6/5X) + u(3X/5)$
5) $0.5u(3/5X) + u(4X/5)$
6) $u(X)$

A fundamental issue concerning the theoretical basis for risk-taking decisions is how subjects set their reference point. We refrain from making any strong assumptions regarding this but note the following possible alternatives. One alternative is to use option 6) as the reference point.[4] Another option is to use the decision in the first hypothetical question as the reference point, which means option 1) or option 6). A third option is the safe amount in the chosen option among the choice options 1)-6). And a final alternative is $u(0)$ in our version of the game as no initial amount is provided to the subjects before we play the game. Holden and Tilahun (2022) alternatively used options 1) and 6) as the starting point and found that option 1) resulted in a substantially larger risky investment than when option 6) was provided upfront and which has been the standard approach with this game (Charness & Viceisza, 2016; Dasgupta, Mani, Sharma, & Singhal, 2019; Gneezy et al., 2009; Gneezy & Potters, 1997; Gong & Yang, 2012). Based on PT, the outcomes below the reference point should be weighted more heavily, possibly 2-3 times as strongly, than outcomes above the reference point. With the reference point being the

[3] MKw is Malawian Kwacha, the local currency. 1000 MKw is approximately 30% above a daily PPP income in Malawi in 2022 at the time of the survey.

[4] This is a good alternative if the safe amount X is provided upfront.

status quo before the game, all the options above are in the gains domain and the degree of loss aversion should not influence the subjects' decisions.

After the subjects have made their choices in the real game, the outcome is revealed with one independent throw of a 20-sided die for each subject, where die outcomes 11-20 imply a win and die outcomes 1-10 imply a loss. Payout is then implemented.

In a new experimental session 1-2 months later, the identical game is played with the same subjects. We investigate whether the win/loss outcome in Round 1 of the game influences risk-taking in the Round 2 first stage hypothetical choice between $u(X)$ and $0.5u(3X)$ as well as the following Round 2 real game with the same six alternatives as above. After this, the real game outcome is determined with one throw of the die for each subject like after Round 1.

Unlike in Round 1, Round 2 is followed up with two immediate additional rounds to assess potential luck effects from Round 1 as well as from Round 2. Rounds 3 and 4 are played with equal probability of becoming a real game, determined with one throw of the die for each subject after the subjects have made up their mind about the choices in Rounds 3 and 4. Rounds 3 and 4 also differ from Rounds 1 and 2 and each other in two other important aspects. First, the probability of winning is reduced from 0.5 in Rounds 1 and 2 to 0.4 in Round 3 and 0.3 in Round 4. Second, the hypothetical binary choices were dropped in Rounds 3 and 4.

After throwing the die to identify whether Round 3 or Round 4 becomes the real game, the die is used to identify the outcome in the real game with die outcomes 13-20 (win) and 1-12 (loss) if Round 3 becomes real and die outcomes 15-20 (win) and 1-14 (loss) in Round 4 if this becomes the real game before a payout is arranged. The actual payout for Rounds 2, 3, and 4, then followed after this. No cash payout was therefore provided between Round 2 and Rounds 3 and 4 although subjects knew whether they won or lost and that payment would be made after all rounds had been completed.

A fundamental question we investigate with the experiment is how good or bad luck affects risk-taking behavior in later rounds. PT may on the one hand predict that loss effects are stronger than win effects due to loss aversion. However, it is not obvious how reference points have changed from Round 1 to Round 2 and from Round 2 to Rounds 3 and 4 and therefore how the sensitivity to losses in earlier rounds affects risk-taking. With rapid adjustment of reference points, there should be no good or bad luck effects from Round 1 to Round 2 as Round 1 took place 1-2 months earlier and income effects should be very small in such a time perspective. We propose the following hypotheses:

H1) There are no good or back luck effects from Round 1 on risk-taking behavior in Round 2 as these rounds took place with a substantial time gap (1-2 months).

H2) There are good and bad luck effects from Round 2 to Rounds 3 and 4 as these took place almost immediately after each other. We base this on PT and no reference point adjustment between Round 2 and Rounds 3 and 4.

H2a) Loss in Round 2 triggers on average more risk-taking in Rounds 3 and 4. This is based on the assumption that the reference point has not changed since the beginning of Round 2 and that the value function is convex in the loss domain (PT).

H2b) Win in Round 2 has no effect on average on risk-taking in Rounds 3 and 4. This is based on the assumption that the value function in the gains domain is stable and has not changed from Round 2 to Rounds 3 and 4.

H3) Win outcomes trigger optimism bias and more risk-taking while a loss in previous rounds triggers pessimism and lower risk-taking in later rounds. This hypothesis follows from the illusion of control theory (Langer & Roth, 1975).

H4) Win in Rounds 1 and 2, trigger less risk-taking in the following rounds, and Loss trigger more risk-taking. This is based on the gambler's fallacy that implies a belief in negative correlations between the outcomes in the game when it is repeated.

3 Sampling and ethical issues

3.1 Sampling

A stratified random sample of 764 university students from Lilongwe University of Agriculture and Natural Resources (LUANAR), Malawi, was used in the first session of the experiment that also included a survey questionnaire. The follow-up rounds of the experiment took place 1-2 months after the first round and targeted the same students. We have complete data for 718 of the students from both sessions[5]. The students were randomly drawn from 48 classes that were stratified across different study programs and years of study. 16 students were randomly sampled from each class. Each class was simultaneously interviewed and experimented with in a single classroom. Each student was provided a tablet for the answering of survey questions and filling of their experimental responses. The experiments were orchestrated by a facilitator to ensure uniform framing of the experiments while students were not allowed to communicate among themselves but could ask questions for clarification. Supervisors guided them if they needed individual help and handled the individual randomization of real game identification[6] and luck outcome treatments.

3.2 Ethical issues

The experiments took place during the fourth wave of the corona pandemic (February-March 2022) when the omicron variant was dominant. Utmost care had to be taken during the execution of experiments to prevent the spread of the virus. A classroom with 16 seats and desks with tables positioned in fixed

[5]The exception is the luck treatment variables as subjects who chose the safe option did not expose themselves to any risk in the game. The luck outcome treatments are therefore missing for these subjects.

[6]Round 3 and 4 games had equal chance of becoming real.

distanced positions was used for all groups. All participants and the research team had to wear face masks throughout. All had to disinfect their hands before entering the room and before leaving the room. All tablets and other equipment (dice, cups, and boards used for randomization) were disinfected before and after use.

Prior informed consent was provided by the participants as the first question they had to answer on the tablets after having received an introduction about the experiments and the survey. The survey focused on the corona pandemic but is not part of the focus of this paper.

The experiments were all standard experiments that are part of the tool bag of behavioral and experimental economists and provided monetary incentives that were sufficiently large to motivate the students to take part in the survey and experiments and return for the second round of experiments. We see no ethical problems associated with the design and implementation of these experiments other than the importance of ensuring corona safe procedures, having obtained their prior informed consent, and protecting the anonymity of the students.

4 Descriptive analysis and estimation strategy

4.1 Descriptive analysis

Simple comparisons of the treatment (luck) effects were made by Round and luck and use of t-tests (tables and graphs). After detecting that the distributions of our risky investment share variables that are in the 0-1 range deviate significantly from normal distributions, we applied the Wilcoxon rank-sum tests and two-sample Kolmogorov-Smirnov tests for equality of means and distributions to assess the significance of the treatment (luck) effects.

First, the risky investment variables were constructed as shares of the maximum safe amount, $ri = x/X$, in each round. We constructed the subject-level change in risky investment share variables as follows; $dri2 = ri2 - ri1, dri3 = ri3 - ri2, dri4 = ri4 - ri2$.To assess the distributional heterogeneity of the treatment effects, we assess the risky investment response change distributions for winners and losers in the luck treatments by using the subject-level risky investment share before the luck treatment as the base to calculate the change. This allows us for each luck outcome in Rounds 1 and 2 to split the sample into subjects that reduced, kept constant, and increased their risky investment share after a win or loss in the previous round. Non-parametric cumulative response graphs by treatment were used to inspect the response heterogeneity.

4.2 Attrition and choice of the safe option

We had initial attrition as 43 subjects dropped out from the second session. The second attrition issue we have relates to subjects selecting the safe option in the real games. This implied that they took no risk such that the random luck treatment was not imposed on them. The experimental treatments (random luck) are therefore random treatments that are conditional that the subject chose an option that involved some risk in the risky investment game. The luck treatment variables (T1, T2) are therefore missing for subjects that chose the safe option. We, therefore, have attrition in the luck treatment data and potential attrition bias. The fact that a subject chose the safe option in one round does not necessarily imply that the safe option was also chosen in other rounds but these decisions may be correlated. Separate models were run using the choice of the safe option or not as the dependent variable over the game rounds to assess how previous choices as well as luck in previous rounds influenced the likelihood of the safe choice in later rounds.

To assess the effects of luck on the likelihood of selection of the safe choice in later rounds, we run the following linear binary choice models by game round.

Round 1:

$$r1D_{ci1} = \alpha_{10} + \alpha_{12} rh_{ci1} + \epsilon_{ci1} \tag{1}$$

Round 2:

$$r2D_{ci2} = \alpha_{20} + \alpha_{21} T1_{ci1} + \alpha_{22} r1D_{ci1} + \alpha_{23} rh_{ci1} + \alpha_{24} rh_{ci2} + \alpha_{24} C_c + \epsilon_{ci2} \tag{2}$$

Rounds 3 and 4:

$$r3D_{ci3} = \alpha_{30} + \alpha_{31} T1_{ci1} + \alpha_{32} T2_{ci2} + \alpha_{33} r2D_{ci2} + \alpha_{34} rh_{ci2} + \alpha_{34} C_c + \epsilon_{ci3} \tag{3}$$

$$r4D_{ci4} = \alpha_{40} + \alpha_{41} T1_{ci1} + \alpha_{42} T2_{ci2} + \alpha_{43} r2D_{ci2} + \alpha_{44} rh_{ci2} + \alpha_{44} C_c + \epsilon_{ci4} \tag{4}$$

where $r1D, r2D, r3D$, and $r4D$ represent the choice of the safe option in real games.[7] Subscript c represents class, subscript i represents subject, subscripts 1, 2, 3, and 4 represent game rounds, α_{2n} represents the estimated coefficients in terms of investment shares in round 2, $T1_{ci1}$ and $T2_{ci2}$ represent luck treatments in Rounds 1 and 2 for subject i in class c, rh_{ci1} and rh_{ci2} represent the binary (hypothetical) choices (dummy with risky=1) in Rounds 1 and 2 by the subjects. α_{21}, α_{31}, and α_{41} capture the average luck treatment effects ($T1$) from luck in Round 1 on the likelihood of selecting the safe option in Rounds 2, 3, and 4 of the game. α_{32} and α_{42} capture the average luck treatment effects ($T2$) from luck in Round 2 on the likelihood of selecting the safe option in Rounds 3 and 4. C_c represents a vector of class dummy variables, and ϵ_{ci*} represents the error terms by round *.

As an additional check for luck effects we investigate whether the binary (hypothetical) choice in Round 2 is affected by luck in Round 1:

Round 2:

$$rh_{ci2} = \gamma_{20} + \gamma_{21} T1_{ci1} + \gamma_{22} ri1_{ci1} + \gamma_{23} rh_{ci1} + \alpha_{24} C_c + \eta_{ci2} \tag{5}$$

4.3 Risky investment share models

We measure the share invested such that $0 \leq x/X \leq 1$. The basic issue we want to study is whether this investment share ($ri = x/X$) is influenced by

[7] Note that Rounds 3 and 4 had an equal likelihood of being randomly chosen as real.

good or bad luck in previous games and that luck, therefore, plays a role in the instability in responses over repeated rounds of the game.

As a first approach, we used linear panel data models with class fixed effects (FE) to control for academic influence (Equations (6)-(8):

Round 2:

$$ri_{ci2} = \beta_{20} + \beta_{21}T1_{ci2} + \beta_{22}rh_{ci1} + \beta_{23}ri_{ci1} + \beta_{24}C_c + \sigma_{ci2} \tag{6}$$

Rounds 3 and 4:

$$ri_{ci3} = \beta_{30} + \beta_{31}T1_{ci1} + \beta_{32}T2_{ci2} + \beta_{33}T1_{ci1} * T2_{ci2} + \beta_{34}C_c + \sigma_{ci3} \tag{7}$$

$$ri_{ci4} = \beta_{40} + \beta_{41}T1_{ci1} + \beta_{42}T2_{ci2} + \beta_{43}T1_{ci1} * T2_{ci2} + \beta_{44}C_c + \sigma_{ci4} \tag{8}$$

where ri represents the risky investment share, subscript c represents class, subscript i represents the subject, subscripts 2, 3, and 4 represent Rounds 2, 3, and 4, $\beta_{*n}, n = 1, 2, 3$, represent the estimated coefficients for the luck treatment effects $(T1, T2, T1 * T2)$ in terms of investment shares in the round * and treatment n, rh_{ci1} represents the binary choice (hypothetical) in Round 1 of the game, ri_{ci*} represents the risky investment share in the Round * real game. C_c represents a vector of class dummy variables (class FE), and σ_{ci*} represents the error terms. For robustness assessment of the luck effect, we run models without and with the Round 1 binary (hypothetical) choice dummy and the Round 1 real risky investment share as additional right-hand side (RHS) variables.

4.4 Conditional IV models

We consider two types of conditionalities in the risky investment behavior. First, in a repeated game the investment level in the previous round affects the upward and downward response freedom in the next round in our game. The higher the response level in the previous round, the lower the upward response freedom in the following round. This upward and downward response freedom in later rounds is therefore conditional on the investment level in the previous round. Second, the investment level in Round 2 does not only depend on the investment level in Round 1 but also the luck in Round 1 if there is a significant luck effect. When using the Round 2 investment level to condition for the upward and downward response freedom in Rounds 3 and 4, the Round 2 investment level can be considered as conditional on the initial investment level in Round 1 as well as luck in Round 1. This implies that we have an endogeneity problem in assessing the cumulative luck effects if we want to control for the variation in response freedom.

We suggest two approaches to deal with this issue. The first approach is handle the endogeneity of $ri2$ due to the luck effect in Round 1 as IV models where $ri2$ is instrumented for with the initial risky investment level and the luck outcome in Round 1. The T1 luck treatment effect is then observed in the first stage regression, while the T2 luck treatment effect is observed in the second stage IV regressions where $ri3$ and $ri4$ are the dependent variables, while controlling for the endogenous investment level in Round 2. This also allows us to assess the suitability of $ri1$ and T1 as instruments to predict $ri2$. We use 2SLS models where we alternatively exclude or include T1 and T2 to assess relative model and instrument performance.

We will then run the following simple linear model for Round 2 (first stage) and the 2SLS IV models for Rounds 3 and 4 (second stage):

Round 2:

$$ri_{ci2} = \eta_{20} + \eta_{21}T1_{ci1} + \eta_{22}ri_{ci1} + \eta_{24}C_c + \rho_{ci2} \tag{9}$$

Rounds 3 and 4:

$$ri_{ci3} = \eta_{30} + \eta_{31}T2_{ci2} + \eta_{32}ri_{ci2}(ri_{ci1}, T1_{ci1}) + \eta_{33}C_c + \rho_{ci3} \tag{10}$$

$$ri_{ci4} = \eta_{40} + \eta_{41}T2_{ci2} + \eta_{42}ri_{ci2}(ri_{ci1}, T1_{ci1}) + \eta_{43}C_c + \rho_{ci4} \tag{11}$$

The second approach is to split the sample based on the luck outcome in Round 1. The models for Rounds 3 and 4 are then estimated as separate models for winners and losers in Round 1 while also conditioning on the investment level in Round 2. The fundamental issue whether the investment level in Round 2 is endogenous and causing bias in the split sample models, still remains a concern. We, therefore, use 2SLS models also for the split samples but then only have $ri1$ that is used to instrument for $ri2$. The sample splitting allows us to compare the treatment effects in Rounds 3 and 4 conditional on the T1 outcomes.

Rounds 3 and 4: Conditional on T1:

$$ri_{ci3,T1} = \eta_{30,T1} + \eta_{31,T1}T2_{ci2,T1} + \eta_{32,T1}ri_{ci2}(ri_{ci1,T1}) + \eta_{33,T1}C_c + \rho_{ci3,T1} \tag{12}$$

$$ri_{ci4,T1} = \eta_{40,T1} + \eta_{41,T1}T2_{ci2,T1} + \eta_{42,T1}ri_{ci2}(ri_{ci1,T1}) + \eta_{43,T1}C_c + \rho_{ci4,T1} \tag{13}$$

We test for endogeneity of ri_{ci2} and the strength of the instrument ri_{ci1} in the four models.

4.5 Additional robustness checks

The following robustness checks were applied:

- Risky investment share models: As an alternative to the linear panel data models, we used fractional probit models which have more appropriate statistical properties for dependent variables that are distributed in the 0-1 interval, as is the case for our investment share variables. This allowed us to assess whether the significance levels and the marginal effects in the fractional probit models differ from those in the linear panel data models. We also included class fixed effects (FE) in the fractional probit models and run models without and with treatment $T1 * T2$ interactions as an additional check.

- We assessed the effects of inclusion of age, gender, and a dummy for Economics students to further inspect the potential heterogeneity of the responses in the repeated games. This allowed us to assess, among others, whether economics students behave more rationally in the game than other students.

5 Results

5.1 Descriptive results

Fig. 1 presents the distribution of choices over the four rounds of the risky investment game. Table 1 presents the distribution of choices in the first binary (hypothetical) and the follow-up (real) games in Round 1 of the experiment. As a first indication of the degree of randomness in the choices in the hypothetical versus real game questions, we see in Table 1 that 17 of those who selected the safe option in the first hypothetical question selected the riskiest option in the following real game. And 47 of those who selected the risky option in the first hypothetical game selected the safest (no risk) option in the follow-up real game.

This two-stage procedure was repeated in the second session that took place 1-2 months later. Table 2 presents the cross-tabulation of the decisions in the hypothetical and real game questions in Round 2. While the sample has been reduced by 43 subjects (attrition), the number of subjects that selected the safe option in the hypothetical game and the riskiest option in the real game was 44 and the number of subjects selecting the risky option in the hypothetical question and the safest (no risk) option in the follow-up real game was 52. The number of subjects with clearly inconsistent hypothetical versus real game choices, therefore, increased from Round 1 to Round 2.

Two other points are worth noting from Fig.1 which shows the distribution of choices in all four rounds. First, the aggregated distributions are fairly stable across rounds and with a tendency that the corner solutions are preferred to interior "mixed" solutions. Second, the fact that the probability of winning was reduced from 0.5 in Rounds 1 and 2 to 0.4 in Round 3 and 0.3 in Round 4 had modest effects on the likelihood of subjects choosing more risky or safe options. The share of respondents choosing the riskiest option even increased from Round 2 to Round 3. More than 30% of the subjects preferred the riskiest alternative in Round 4 even though a risk-neutral person should prefer the safest option in this game.

Table 3 presents summary statistics for the key variables of interest for the subjects that participated in both experimental sessions (all four rounds of the experiment). Two additional variables of interest in Table 3 are the subject-specific average risky investment level (riavg) across the four rounds and the subject-specific variance in risky investment choices (rivariance) over the four rounds. The distributions of these two variables are presented in Fig. 2. Fig.2 reveals some interesting insights. First, the average distribution is concave while the distributions in each game round (Fig. 1) had a convex

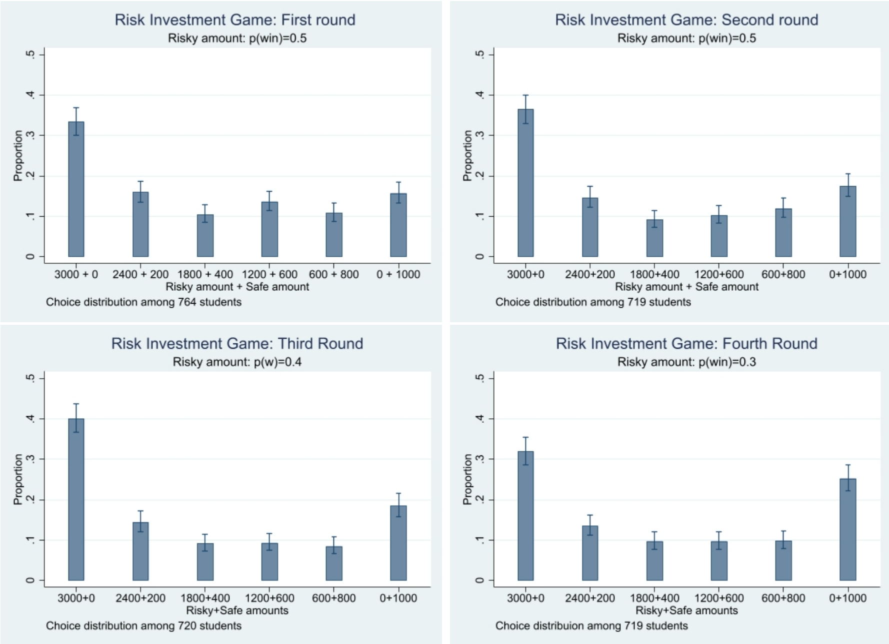

Fig. 1 Risky investment distribution by students in four rounds

Table 1 Round 1 Risky Investment Game: First binary choice versus real game choice

Real game choice Options	First binary choice: Safe amount	Number selecting Risky Amount	Total
3000 Risky + 0 Safe	17	239	256
2400 Risky + 200 Safe	9	112	121
1800 Risky + 400 Safe	12	68	80
1200 Risky + 600 Safe	28	77	105
600 Risky + 800 Safe	31	51	82
0 Risky + 1000 Safe	73	47	120
Total	170	594	764

Table 2 Round 2 Risky Investment Game: First binary choice versus real game choice

Real game choice Options	First binary choice: Safe amount	Number selecting Risky Amount	Total
3000 Risky + 0 Safe	44	221	265
2400 Risky + 200 Safe	25	79	104
1800 Risky + 400 Safe	15	50	65
1200 Risky + 600 Safe	28	46	74
600 Risky + 800 Safe	48	38	86
0 Risky + 1000 Safe	75	52	127
Total	235	486	721

Table 3 Experimental and socioeconomic control variables

	Mean	Median	SD	Min	Max	N
ri1 (Round 1)	0.60	0.60	0.37	0	1	718
ri2 (Round 2)	0.60	0.80	0.39	0	1	718
ri3 (Round 3)	0.63	0.80	0.39	0	1	718
ri4 (Round 4)	0.54	0.60	0.41	0	1	718
riavg (average share)	0.59	0.60	0.27	0	1	718
rivariance	0.32	0.24	0.30	0	1	718
T1 (Win-Loss Round 1)	0.50	1.00	0.50	0	1	611
T2 (Win-Loss Round 2)	0.55	1.00	0.50	0	1	606
Sex (Female dummy)	0.37	0.00	0.48	0	1	718
Age (Years)	23.01	22.00	3.59	17	48	718
Econ (Economics student)	0.32	0.00	0.47	0	1	718

Note: Choice in each round measured as risk share (sacrificed risky amount out of safe amount). riavg is average subject-level investment share across the four rounds.

pattern across risky investment shares. Second, the subject-level variance is low for a large share of the sample but is much larger for a smaller share of the sample. This indicates that the subject-level consistency of responses across game rounds varies and that a substantial share of the sample may make more random choices or may have responded to the treatment (luck) in previous game rounds.

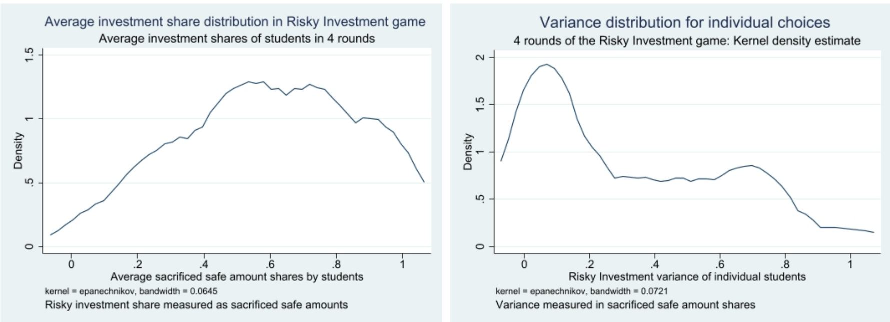

Fig. 2 Average Choices and Variance distributions over 4 rounds of the risky investment game

5.2 Treatment (Luck) effects

Table 4 presents average single-round treatment (luck) effects in terms of average and median risky investment shares in Rounds 2, 3, and 4 by luck treatments in Rounds 1 and 2. This gives the first insight into whether luck in a game played one to two months earlier or earlier the same day influences the risk-taking behavior in the following real game rounds.

Fig.3 presents histograms of the luck treatment effects from the previous game round in terms of the histogram distribution of responses across the 6 choice options in each round and with 95% confidence intervals for the frequencies for each choice category.[8], We see particularly large and significant treatment effects for options 1 and 6 in each round. However, these graphs do not reveal the extent of within-subject randomness across experimental rounds.

Fig. 4, which presents the change in risky investment shares (dri) by round and luck in the previous round, provides more insights into the subject heterogeneity and variation in responses to luck in the game. The graphs reveal that 30-50% of the subjects did not change their risky investment shares, 20-30% reduced their investment share after a win in the previous round of the game, against 40-55% after a loss in the previous round. 22-38% increased their investment share after a win, against 18-20% after a loss.

Table 5 presents conditional treatment effects in terms of average risky investment shares over two rounds of luck treatment in the game and Fig. 5 presents histograms of the combined (cumulative) treatment effects from the two previous game rounds. Table 6 presents Wilcoxon rank-sum and Kolmogorov-Smirnov equality of distributions tests to further scrutinize the significance of these treatment effects. Overall, the treatment effects on the risky investment shares are highly significant and indicate that good and bad luck have strong impacts on the behavior in the following round(s) of the game, not only when the earlier round of the game was played earlier the same day but also when it was played 1-2 months ago. Fig. 4 and 5 give some indications that double bad luck has a relatively stronger effect than double good luck compared to the one lucky and one unlucky treatment outcome when it comes to the selection of the riskiest option 1.

Fig. 6 presents the subject-level change in risky investment shares in Rounds 3 and 4 conditional on luck in Round 1. The effect of good luck shows that it reduced the likelihood that subjects reduced their investment shares and increased the likelihood that they kept their investment shares constant compared to those that experienced bad luck in Round 2. By comparing two and two horizontal graphs we also see the systematic effect of luck from Round 1 as the cumulative graphs are more elevated after luck in Round 1. The lower probability of winning in Round 4 had a surprisingly small effect on the decision distribution pattern although the T2 luck effect in Round 2 has been reduced. This indicates probabilistic insensitivity in the p(Win) range of 0.3-0.5. A rational risk-neutral person should invest the whole amount in Rounds 1, 2, and 3, and nothing in Round 4. The fairly limited variation across game rounds in terms of change in risky investment share responses is also shown in parametric models by comparing the estimated parameters in Tables B1 and B2 in Appendix B.

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[8]Round 2 is considered to be the previous round for both Rounds 3 and 4 as the outcome in Rounds 3 and 4 were revealed after decisions were made in both these.

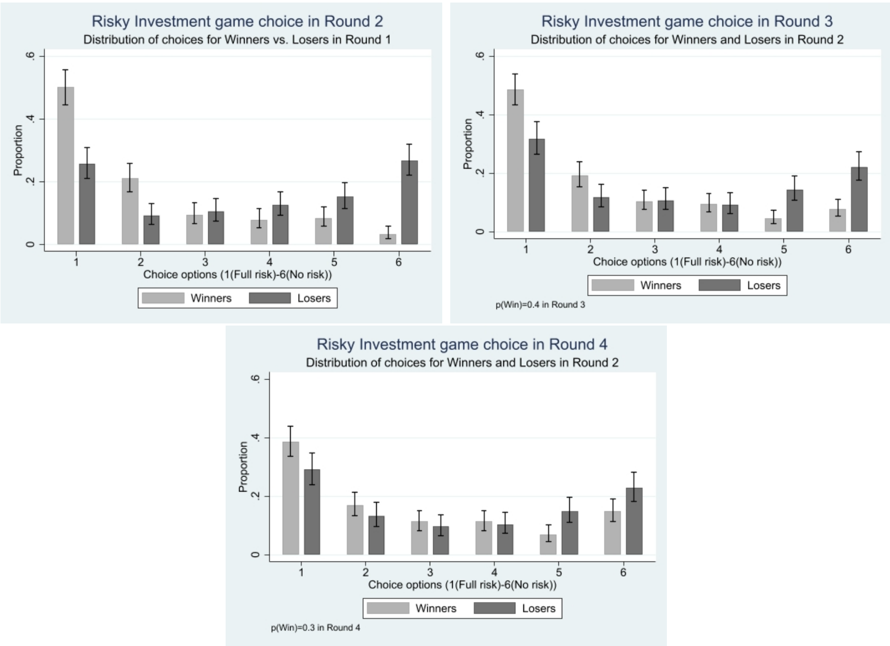

Fig. 3 Risky Investment distributions in Rounds 2, 3 and 4 for winners vs losers in the previous round

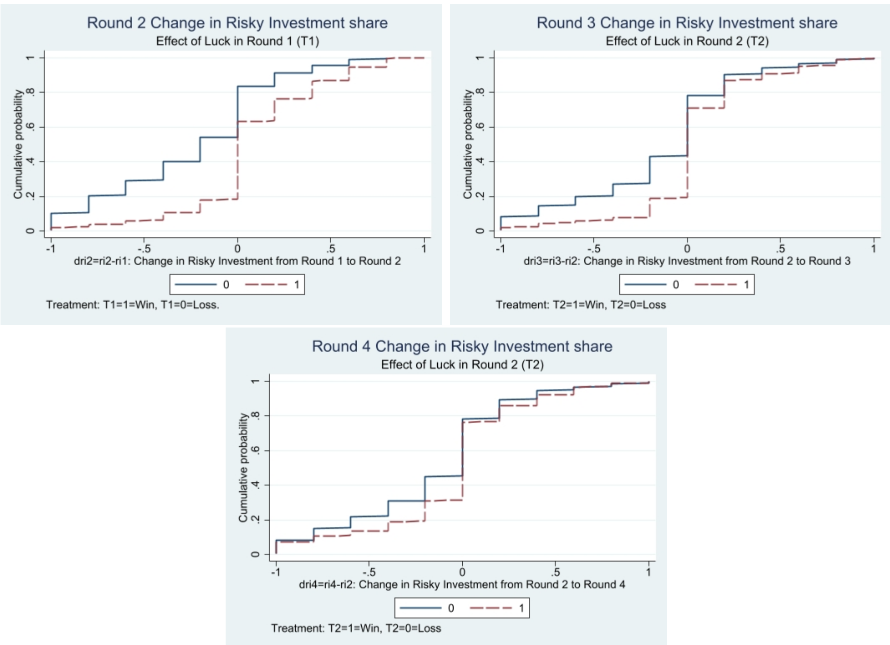

Fig. 4 Risky Investment Change distributions in Rounds 2, 3 and 4 for winners vs losers in previous round

Table 4 Single-round random luck treatment effects in the Risky Investment game

Outcome	Stats	ri2-T1	ri3-T2	ri4-T2	ri3-T1	ri4-T1
0=Loss	Mean	0.48	0.54	0.53	0.57	0.50
	Median	0.40	0.60	0.60	0.60	0.40
	St.Err.	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
	N	303	271	271	304	304
1=Win	Mean	0.77	0.75	0.65	0.74	0.65
	Median	1.00	0.80	0.80	0.80	0.80
	St.Err.	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
	N	309	337	336	309	308
Total	Mean	0.63	0.66	0.59	0.65	0.57
	Median	0.80	0.80	0.60	0.80	0.60
	St.Err.	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
	N	612	608	607	613	612

Note: T1=Outcome in Round 1, T2=Outcome in Round 2.

Table 5 Conditional luck effects over two rounds in the Risky Investment game

T2 Outcome	Stats	ri3 if T1=1	ri3 if T1=0	ri4 if T1=1	ri4 if T1=0
0=Loss	Mean	0.659	0.441	0.598	0.470
	Median	0.80	0.40	0.60	0.40
	St.Err.	(0.034)	(0.038)	(0.034)	(0.038)
	N	128	103	128	103
1=Win	Mean	0.807	0.727	0.712	0.628
	Median	1.00	0.80	0.80	0.80
	St.Err.	(0.020)	(0.029)	(0.026)	(0.034)
	N	174	129	173	129
Total	Mean	0.744	0.600	0.664	0.558
	Median	0.80	0.60	0.80	0.60
	St.Err.	(0.019)	(0.025)	(0.021)	(0.026)
	N	302	232	301	232

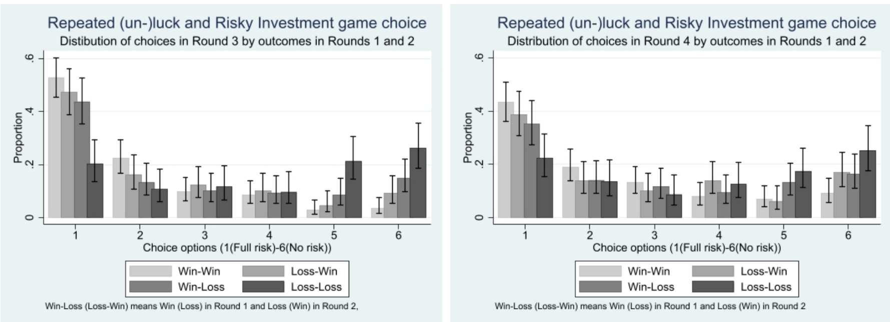

Fig. 5 Risky Investment distributions in Rounds 3 and 4 for winners vs losers in previous rounds

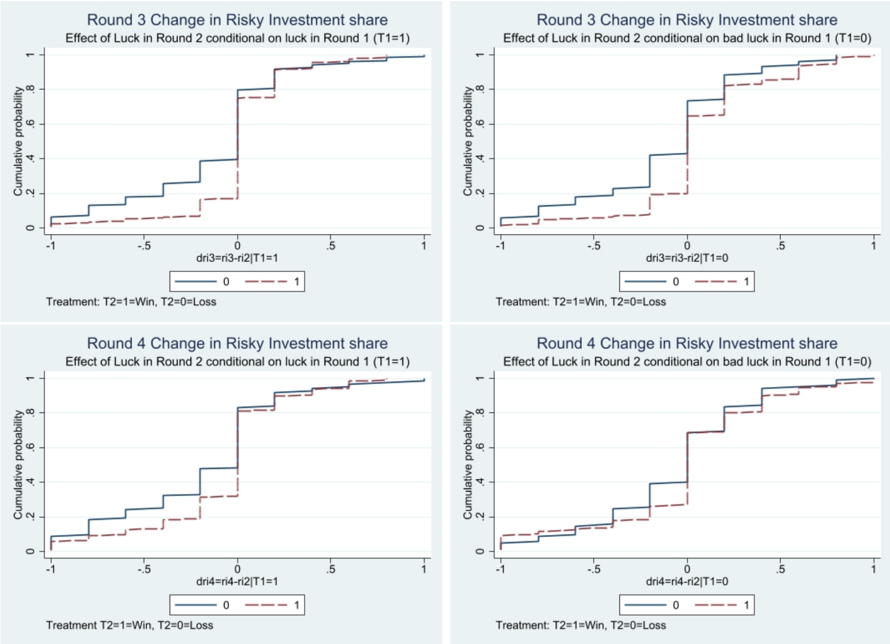

Fig. 6 Risky Investment Change distributions in Rounds 3 and 4 for winners vs losers in previous rounds

Table 6 Two-sample Kolmogorov-Smirnov tests for equality of distribution functions and Wilcoxon rank-sum tests

Comparison	Combined Kolmogorov-Smirnov: D	K-S P-value	Wilcoxon Ranksum P-value
ri2, by(T1)	0.362	0.000	0.000
ri3, by(T2)	0.244	0.000	0.000
ri4, by(T2)	0.159	0.001	0.000
ri3, by(T1)	0.198	0.000	0.000
ri4, by(T1)	0.195	0.000	0.000
ri3 if T1=1, by(T2)	0.183	0.015	0.003
ri3 if T1=0, by(T2)	0.336	0.000	0.000
ri4 if T1=1, by(T2)	0.148	0.080	0.015
ri4 if T1=0, by(T2)	0.195	0.026	0.002

The significance of (un-)luck in previous Rounds of the game.

5.3 Regression results

5.3.1 Binary choice models: Choice of safe option

Table 7 presents the results for the models with the choice of the safe option (Safe option dummy=1) in the real games as the dependent variable and how it is affected by luck in previous rounds and correlated with previous hypothetical and real game choices in previous rounds.

Table 7 demonstrates that there was a significant positive correlation between the choice of the safe option in the real game in Round 1 and the first hypothetical choice of the safe option. The initial hypothetical choice of the safe option was associated with a 33% points higher likelihood of selecting the safe option in the following real game. However, the R-squared is not very high (0.145) and therefore demonstrates substantial randomness in the choices in line with the initial descriptive findings. The selection of the safe option in the Round 2 real game was not significantly associated with selecting the safe options in the Round 1 real game. However, it was significantly positively correlated with the selection of the safe option in the binary (hypothetical) Round 2 game. The safe choice in the hypothetical game was associated with an 11% points higher likelihood of selecting the safe option in the real Round 2 game. More interestingly, luck in the Round 1 game reduced the likelihood of selecting the safe option in the Round 2 real game by 22% points, and this effect is highly significant.

In the third and fourth rounds of the game, Table 7 shows that the choice of the safe options was significantly and negatively affected by luck in Round 2 while luck in Round 1 had no significant effects. Surprisingly, the effects in Rounds 3 and 4 from luck in Round 2 played earlier the same day were weaker than the luck effect from Round 1 on Round 2 given the much longer time difference between these two rounds. A safe (no risk) choice in Round 2 was strongly correlated with a safe choice in Round 3 but not in Round 4. The safe hypothetical choice in Round 2 was positively correlated with the likelihood of the safe choice in Rounds 3 and 4. Overall, the included variables explained less of the outcome variance in Round 4 than in Round 3. This may be related to the lower probability (0.3) of winning in Round 4.

Table 8 presents the results for the impact of luck in Round 1 on the binary hypothetical choice (1=Risky) in Round 2. To assess the robustness of the luck effect we alternatively run models with and without the hypothetical game binary choice in Round 1 and the risky investment share in the real game in Round 1 to assess their relative importance and eventual influence on the estimated luck effect. Table 8 demonstrates a strong and stable treatment (luck) effect from Round 1 on the hypothetical choice in Round 2. Luck in Round 1 is associated with a 29% point higher likelihood of selecting the safe choice in the Round 2 hypothetical binary game. This effect is very robust to the inclusion or removal of the other two Round 1 variables. The hypothetical binary choice in Round 1 is also strongly positively related to the Round 2 binary hypothetical choice. Choice of the risky option in the Round 1 hypothetical game is associated with an 18-20% point higher likelihood of selecting the risky choice in the Round 2 hypothetical game. The risky investment share in the Round 1 real game is significantly positively correlated with the likelihood of choosing the risky option in the Round 2 hypothetical game when the Round 1 hypothetical binary choice variable is excluded. The risky investment

Table 7 Luck and Choice of the Safe option in the Risky Investment game

VARIABLES	(1) r1D	(2) r2D	(3) r3D	(4) r4D
r1D-Safe choice (real) Round 1		0.120		
		(0.208)		
r2D-Safe choice (real) Round 2			0.361***	0.127
			(0.119)	(0.093)
Safe choice (hypothetical) Round 1	0.333***	0.035		
	(0.045)	(0.040)		
Safe choice (hypothetical) Round 2		0.113**	0.171***	0.116**
		(0.046)	(0.036)	(0.048)
T1-Luck Round 1		-0.216***	-0.023	-0.030
		(0.031)	(0.031)	(0.040)
T2-Luck Round 2			-0.131***	-0.096***
			(0.029)	(0.030)
Constant	0.416***	0.366***	0.321***	0.309***
	(0.035)	(0.043)	(0.036)	(0.035)
Observations	764	613	532	532
R-squared	0.145	0.148	0.167	0.051
Number of ClassID	48	48	48	48

Note. r*D=Choice of safe option in real game in Round *. Linear panel data models with class FE. Cluster-robust standard errors in parentheses, clustering on class. Sign. levels: *** p<0.01, ** p<0.05, * p<0.1

share variable becomes insignificant when the Round 1 hypothetical choice variable is included. The real game risky investment share variable, therefore, explains less of the variation in the data than the two other variables, luck, and hypothetical choice, do.

As a further robustness check of the luck effect on the choice of the safe option in Rounds 3 and 4, we used the instrumental variable (IV) method to control for the endogeneity associated with the inclusion of the one-round lagged dependent variable (Round 2 dummy variable for selection of the safe option in the real game). Two-stage least squares (2SLS) models were used with T1, Round 1 hypothetical binary choice, and Round 2 binary hypothetical choice as instruments. The results are presented in Table 9. The model results for the choice of the safe option in Rounds 3 and 4 demonstrate that the previous round's safe choice variable was highly endogenous (Wu-Hausman test), and that the statistical validity (Sargan overidentification) test was satisfactory (no significant correlation between the instruments and the outcome error), and that the instruments were quite strong (F-value=9.4). However, this may not be sufficient to have removed all of the endogeneity bias in the coefficients. We see though that the treatment effect from luck in Round 2 remains significant in both models and is slightly lower than in Table 7. These results do not change any of our conclusions.

Table 8 Binary choice (hypothetical) in round 2 (1=Risky) and luck in round 1

VARIABLES	(1) rh2	(2) rh2	(3) rh2
T1-Luck in Round 1	0.292***	0.290***	0.292***
	(0.039)	(0.039)	(0.039)
rh1-Binary choice (hypothetical) Round 1: 1=Risky	0.198***		0.177***
	(0.058)		(0.058)
ri1-Risky investment share Round 1		0.148**	0.082
		(0.058)	(0.057)
Constant	0.392***	0.454***	0.352***
	(0.048)	(0.039)	(0.057)
Observations	613	613	613
R-squared	0.123	0.109	0.126
Number of ClassID	48	48	48

Note: rh2=Dummy for binary choice in Round 2 with Risky=1. Linear probability models with class FE. Cluster-robust standard errors in parentheses, clustering on class. Sign. levels: *** p<0.01, ** p<0.05, * p<0.1

Table 9 2SLS-IV Models: Choice of the Safe option in the Risky Investment game Rounds 3 and 4

VARIABLES	(1) r3D	(2) r4D
r2D-Safe choice (real) Round 2, predicted	1.911***	1.302***
	(0.418)	(0.425)
T2-Luck dummy Round 2	-0.127***	-0.081**
	(0.036)	(0.036)
Constant	0.123***	0.156***
	(0.032)	(0.032)
Observations	532	532
Wu-Hausman endogeneity test, p-value	0.000	0.003
Sargan overidentification test, p-value	0.174	0.102
Instruments, F-test	9.43	9.43

Note: Instrumented: r2D, instruments: T1-luck in round 1, round 1 binary choice, round 2 binary choice. Standard errors in parentheses. Sign. levels: *** p<0.01, ** p<0.05, * p<0.1

5.3.2 Risky investment share models

The linear panel data models with the risky investment shares in Rounds 2, 3, and 4 are presented in Table 10. In Rounds 3 and 4, we have tested whether the treatment effects over Rounds 1 and 2 are cumulative by including the interaction effect for the two treatment variables. First, we see that the luck treatment in Round 1 has a highly significant effect on the risky investment share in Round 2 as the risky investment share increases from 0.47 for the

Table 10 Luck effects in the Risky Investment game: Risky investment share effects

VARIABLES	(1) ri2	(2) ri3	(3) ri4
T1-Luck dummy Round 1	0.311***	0.230***	0.142**
	(0.037)	(0.052)	(0.060)
T2-Luck dummy Round 2		0.289***	0.177***
		(0.053)	(0.057)
T1*T2-Luck in Round 1 and 2 interaction		-0.141**	-0.050
		(0.066)	(0.080)
Constant	0.469***	0.433***	0.453***
	(0.019)	(0.037)	(0.038)
Observations	612	534	533
R-squared	0.168	0.138	0.063
Number of ClassID	48	48	48

Note. ri*= Risky investment share in round *. Linear panel data models with class FE. Cluster-robust standard errors in parentheses, clustering on class. Sign. levels: *** p<0.01, ** p<0.05, * p<0.1

unlucky to 0.78 for the lucky, which is a 31% point increase.[9] Next, we see that the luck treatment in Round 1 (T1) remains significant also in Rounds 3 and 4 when we include the luck treatment effect (T2) from Round 2 and the interaction effect for luck in both Rounds 1 and 2. The interaction effect is significant and negative in the Round 3 risky investment share model and negative but insignificant in the Round 4 model. The coefficients for the T2 luck effects are higher than for the T1 luck but they are not significantly different as can easily be observed from the sizes of the standard errors for the coefficients. The results indicate substantial cumulative luck effects although they are not fully additive. This is surprising given that the Round 1 T1 treatment took place one to two months before the Round 2 T2 treatment. It is interesting to note that the T1 effects on the risky investment shares are stronger than the T1 effects on the likelihood of selecting the safe option in the game in Rounds 3 and 4[10]. This may be because the risky investment shares capture more variation than the dependent dummy variable for the selection of one of the six options in the game.

5.4 Conditional IV models

Table 11 presents the conditional 2SLS-IV models that assess how luck in Round 2 affected the risky investment shares in Rounds 3 and 4 when conditioning on the endogenous risky investment shares in Round 2. For each of the Round 3 and Round 4 investment variables three alternative models were specified to assess the added value of including the luck effect (T1) as an extra instrument in the first stage regression that estimates the endogenous Round

[9] We note that we had to drop the part of the sample that chose the safe option in the Round 1 real investment game as the T1 luck treatment did not apply to them.

[10] To the extent that shares and probability changes are comparable

Table 11 IV models with lagged endogenous risky investment shares

VARIABLES	(1) ri3	(2) ri3	(3) ri3	(4) ri4	(5) ri4	(6) ri4
ri2, predicted	1.039***	0.875***	0.844***	0.774***	0.619***	0.596***
	(0.219)	(0.111)	(0.109)	(0.238)	(0.122)	(0.121)
T2			0.161***			0.115***
			(0.032)			(0.034)
Constant	-0.120	0.025	-0.016	0.143	0.280*	0.251*
	(0.231)	(0.157)	(0.142)	(0.233)	(0.146)	(0.139)
Observations	532	532	532	531	531	531
R-squared		0.104	0.169		0.080	0.111
Number of ClassID	48	48	48	48	48	48
First stage:						
Instruments						
ri1	0.258***	0.260***	0.257***	0.259***	0.261***	0.258***
	(0.052)	(0.047)	(0.047)	(0.052)	(0.047)	(0.047)
T1		0.211***	0.209***		0.210***	0.208***
		(0.028)	(0.028)		(0.028)	(0.028)
Endog. test, p	0.003	0.000	0.000	0.028	0.006	0.006
Sargan overid., p		0.339	0.529		0.400	0.418
Instruments, F	24.6	50.2	49.2	24.9	50.2	49.2

Note: Dependent variables: Subject level risky investment shares by round. 2SLS IV Models with robust standard errors in parentheses. Endogeneity test: Robust score. Models with Class FE. Sign. levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

2 risky investment variable using the risky investment share in Round 1 as an instrument. For $ri3$ the results are presented in models (1) and (2) in Table 11. We see that both models detect strong endogeneity, and both models indicate that the instruments are very strong but much stronger when T1 was added as an extra instrument. With two instruments it is also possible to use the Sargan overidentification test for statistical validity and the results indicate no influence on the outcome error term. We also see that the effect of the predicted lagged risky investment ($ri2$) variable is reduced and the precision of the effect is increased substantially by cutting its standard error nearly by 50%. When we also add the luck treatment effect in the second stage, we see highly significant luck effects in both stages with luck stimulating more risky choices in Rounds 2 and 3, after we have controlled for the conditional lagged endogenous investment levels. The results for $ri4$ are similar but the effects of the predicted lagged variable and T2 are weaker as could be expected with the reduced probability of winning. It is interesting to note that the intercept becomes positive and significant in these models. This indicates a sluggish "business-as-usual" response among part of the sample as the responses in the earlier rounds may have cemented their responses even though a risk-neutral person should invest nothing in the fourth round.

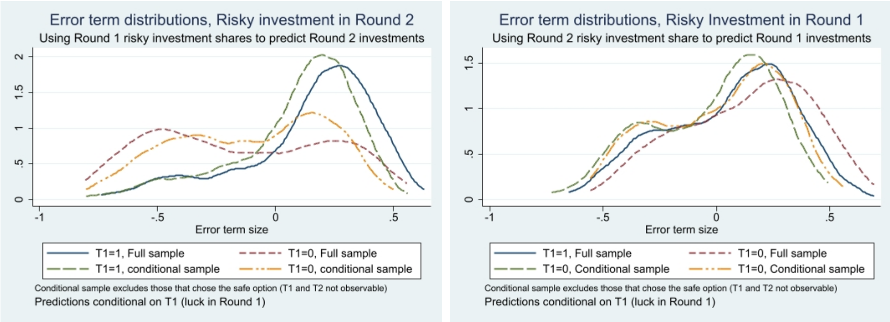

Fig. 7 Prediction errors in Rounds 1 and 2 conditional on T1 outcomes

5.5 Prediction power and bias

First we assess the reciprocal prediction power of the Round 1 and Round 2 risky investment shares and how these may vary with the luck outcome in Round 1. While this luck effect only influences the Round 2 investment level, it can cause bias not only in the prediction of $ri2$ based on $ri1$, but also when $ri1$ is predicted based on $ri2$ due to omitted variable bias due to the correlation between $ri2$ and T1. This is illustrated in Figure 7 by splitting the sample by T1 outcome. As T1 is not observable for those that chose the safe option and the same is the case for T2 in Round 2, we also assess the error distributions for the reduced sample where the subjects selecting the safe option are dropped [11]. We see from Figure 7 that the omitted luck variable causes bias in predictions not only in Round 2 but also in Round 1. The bias is somewhat reduced in the conditional sample where those that chose the safe option were dropped.

Next, we assess the predicted errors for the risky investment shares in Rounds 1 and 2 for models without and with the T1 variable as an additional RHS variable. Does adding the T1 variable reduce bias and enhance precision? This is likely to work better for the prediction of $ri2$ due to the recursive nature of the experiments. Figure 8 shows the predicted error distributions for the two rounds without and with T1. We see a clear tendency that the distribution has become more narrow for $ri2$ while for $ri1$ it has shifted to the left.

Figure 9 presents the prediction errors from the IV models (3) and (6) in Table 11. The graph to the left is for the whole sample where T1 and T2 are observed. The graph to the right has split the sample for T2=1 vs. T2=0 for $ri3$ and $ri4$. We recall that the probability of winning was reduced from 0.5 in Rounds 1 and 2 to 0.4 in Round 3 and to 0.3 in Round 4. This may explain the lower prediction power for $ri4$ that for $ri3$. We also see that the prediction power is weaker for T2=0 than for T2=1. But this difference is smaller than it was for T1=0 vs. T1=1 in Figure 7 in Rounds 1 and 2. The IV approach to handle the luck and endogenous previous round investments gives more unimodal error distributions in Rounds 3 and 4.

[11] This is done also to enable a comparison with the errors in Rounds 3 and 4 when T1 and T2 are taken into account

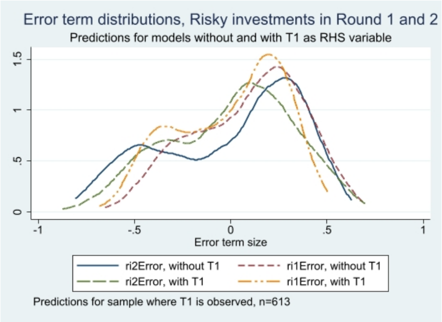

Fig. 8 Predicted error distributions in Rounds 1 and 2 without and with T1 as RHS variable

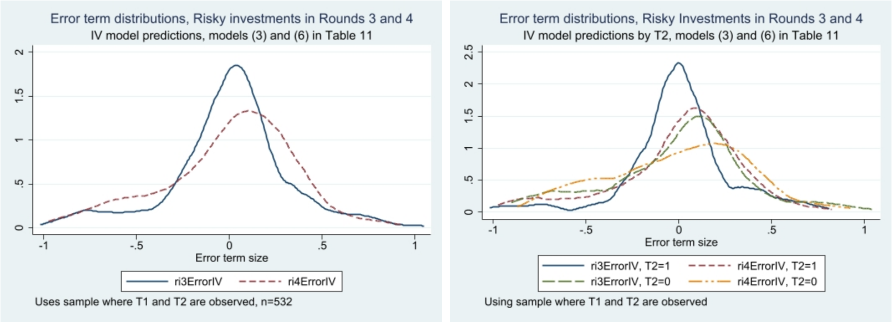

Fig. 9 Predicted error distributions in Rounds 3 and 4 from pooled IV models

5.5.1 Additional robustness checks

The robustness check with fractional probit models is presented in Appendix B Table B1 with the average marginal treatment effects without and with the T1*T2 treatment interaction effects. The results in Table B1 can be compared with the results in Table 10. Overall, the results for the treatment effects in Tables 10 and B1 are very similar. The luck effects over the two rounds are highly significant and robust. The treatment effects in Rounds 3 and 4 are even stronger in the fractional probit models than in the linear models after deducting the negative interaction effects there. The inclusion of the interaction effects in the fractional probit models has a very small effect on the overall marginal effects. This indicates that luck effects can accumulate over several game rounds and trigger more (if lucky) or less (if unlucky) risky choices.

We also investigated the effect of splitting the sample in Rounds 3 and 4 by the luck outcome in Round 2 and do separate conditional IV models to control for the endogeneity of the investment level in Round 2 ($ri2$). To further scrutinize the responses in Rounds 3 and 4 while taking the response in Round 2 into account, we ran separate models for winners and losers in Round 1 to assess whether this random sample splitting influenced the effects of the T2 treatment and the lagged variable ($ri2$), while assessing whether this variable still was causing an endogeneity bias. The results are presented in Table B2.

The results indicate that endogeneity is primarily a problem for those that won in Round 1 (T1=1). We also see a tendency that the luck effect of T2 is lower for winners than losers.

As another robustness check, we investigated whether economics students behaved more rationally in the game as they should be more familiar with risky investment situations and perhaps, therefore, behave more rationally. However, we found that economics students responded as much to good and bad luck as other students. The results are available from the authors upon request.

In yet another robustness test we investigated whether women responded differently to luck than men. From other studies using variants of the risky investment game, we know that women on average invest less in the game than men (Charness & Gneezy, 2012; Dasgupta et al., 2019; Filippin & Crosetto, 2016; Gong & Yang, 2012; Holden & Tilahun, 2022). In Round 2 of the game we found women to invest significantly less (at 5 percent level) than men but the interaction effect between gender and outcome in Round 1 of the game was insignificant. In Rounds 1, 3, and 4, women did not invest significantly less than men and there were no significant interaction effects between luck outcomes in the previous round and gender on the risky investment choices in later rounds. We, therefore, conclude that the luck effect is independent of gender. The results from these models are also available from the authors upon request.

As a final robustness check, we investigated whether age influenced the risky choices and the responses to luck in the game. In Round 2 we found age to be positively related to risk-taking but this tendency did not persist in Rounds 3 and 4 and age and luck interactions were insignificant. We also tested for age and gender interactions but found no such significant interactions. We conclude that the luck effects are robust and not influenced in any strong way by academic programs, gender, or age in our study.

6 Discussion

We first discuss our findings concerning our hypotheses before we relate our findings to the existing literature and propose ideas for future research. Our H1 hypothesis states that there are no good or back luck effects from Round 1 on risk-taking behavior in Round 2 as these rounds took place with a substantial time difference. Our findings demonstrate very clearly that we have to reject this hypothesis. Luck in Round 1 has a strong and significant positive effect on the risky investment share in Round 2. It also reduces significantly the likelihood of selecting the safe option in Round 2.

Hypothesis H2 states that there are good and bad luck effects from Round 2 to Rounds 3 and 4 as these took place almost immediately after each other. Our results show that we cannot reject this hypothesis. Luck in Round 2 has strong and significant positive effects on risk-taking in Rounds 3 and 4 of the game.

Hypothesis H2a states that loss in Round 2 triggers more risk-taking in Rounds 3 and 4 as the value function is convex in the loss domain according to PT. Our results demonstrate very clearly an effect of loss in Round 2 that goes in THE opposite direction of our hypothesis. Subjects invested significantly less in Rounds 3 and 4 after a loss in Round 2. The hypothesis must therefore be rejected. A convex value function in the loss domain, based on PT cannot, therefore, explain the observed behavior in the game. This does not mean that PT can be rejected as the subjects may have adjusted their reference point between Round 2 and Rounds 3 and 4. Alternatively, it is their subjective probability weighting rather than their value function that has been affected by the outcome in Round 2. However, the repeated risky investment game experiment does not allow us to investigate this. This requires further research.

Our hypothesis H2b states that winning (luck) in Round 2 does not affect risk-taking in Rounds 3 and 4. The results demonstrate that this hypothesis also has to be rejected. However, we cannot verify whether it is the value function or the probability weighting (optimism bias) that explains our result. This also requires further research.

Our hypothesis H3 states that good luck outcomes trigger optimism bias and more risk-taking, while a loss in previous rounds triggers pessimism and lower risk-taking in later rounds. Our findings support this hypothesis which seems to give a simpler explanation than PT which predicts adjustment through changes in reference points and value functions. However, this does not mean that the latter types of changes did not take place, just that our experiment only provides limited information about such changes.

Finally our H4 hypothesis states that win in Rounds 1 and 2, trigger less risk-taking in the following rounds, and loss trigger more risk-taking based on the gambler's fallacy. The majority of the subjects did not behave in accordance with this hypothesis. Less than 20% of the sample invested less than in the following round if they had won in the previous round. Likewise, less than 20% of the sample invested more in the following round if they had lost in the previous round.

Our assessment of prediction power and possible prediction bias associated with luck outcomes revealed that our repeated within-subject incentivized game created reciprocal prediction biases when two rounds of the game were played with the same players and these two rounds were played 1-2 months apart. When Round 1 investment is used as instrument the effect of luck in Round 1 is an omitted variable that causes bias in Round 2. When Round 2 is used as an instrument to predict Round 1, the Round 2 instrument is endogenous and causes bias in the predicted Round 1 investment level and the biases depend on the luck outcomes in Round 1.

Next, we scrutinize our study findings concerning previous literature. The possible prediction bias issue was not considered by Gillen et al. (2019) when they used two rounds of the game with their ORIV approach. We do not know whether they have access to the luck outcomes from the Caltech student experiments but it could be worth while revisiting their data to inspect for such potential bias. However, it is possible that the students taking part in such experiments there have participated in many experiments and that they therefore are less influenced by the outcome in earlier experiments.

Holden and Tilahun (2022) found that the provision of an initial safe amount in the risky investment game created an endowment effect which reduced initial investment compared to the provision of an initial risky amount which enhanced risk-taking behavior. In a follow-up paper, they show that the provision of no initial amount resulted in an intermediate investment level or degree of risk-taking. In this study, we use the same type of risky investment game but without providing an initial safe or risky amount in this way to avoid creating an initial endowment effect (Holden & Tilahun, 2021).

If the winning outcome were an income effect, an initial endowment provided in the game should make persons willing to invest more while Holden and Tilahun (2022) provided evidence of the opposite. This gives good reasons to question whether the good and bad luck effects in our study are income effects. First, a fairly small additional income 1-2 months earlier in Round 1 of the game is unlikely to result in such a strong income effect in Rounds 2, 3, and 4 of the game. Second, the income from Round 2 is not paid out before Rounds 3 and 4 as we did not pay the subjects till after all these three rounds have been completed.[12]

Gneezy and Potters (1997) also examined the effect of luck in their experiment with the 41 and 42 subjects in their two treatment arms. They found no significant luck effects but this could be due to their small sample. It is also possible that the initial provision of an endowment in their original experiment induces a stronger loss aversion effect and a different change in reference point than our experiment does.

Some studies in psychology have also found that belief in luck can affect risk-taking behavior in games. Darke and Freedman (1997b) found in an experiment that subjects who have just experienced a lucky event bet more than those who did not have the same lucky experience. This is in line with our findings. They found that the effect of luck was strongest for those that believed in luck in terms of them believing this was a stable personal attribute. Darke and Freedman (1997a) found that belief in good luck was not related to general optimism, academic pessimism, self-esteem, desire for control, or achievement motivation. They found that Asian-Americans were more likely to endorse superstitious beliefs about luck than non-Asians. Similarly, Gao et al. (2021) found that luck made Chinese investors more willing to take risks in the stock market in China based on a natural experiment. It is, therefore, possible that belief in good or bad luck is a cultural and psychological phenomenon and this makes it important to be cautious when judging the external validity of our findings. Our findings in a large student sample in an African context (Malawi)

[12] We may not rule out that mental accounting may contribute to unpaid income effects in the minds of the subjects. The issue is whether such a mental accounting income effect is stronger than the endowment effect due to mental accounting, unlike in the study by Holden and Tilahun (2022), otherwise, the mental income/endowment should result in less investment after a good luck outcome. We, therefore, lean more towards optimism/pessimism bias associated with the good and bad luck outcomes as the main explanation for the strong treatment effects.

indicate that beliefs in luck, like in Asian countries, are having a strong influence on risk-taking behavior. However, more studies are needed to assess the external validity of our findings within Africa as well as outside Africa. Africa is also highly diverse in a cultural sense.

We also investigated whether economics students were more rational in the game than other students but found no evidence of that. Further work is also needed to assess whether the findings in our large student sample can be generalized to a more representative sample of the Malawian population. Preliminary findings from a sample of 835 rural respondents are to a large extent consistent with the findings in this study.

Darke and Freedman (1997b) found that subjects with a belief in luck as a causal mechanism and personal property responded more to lucky events in terms of risk-taking in a game of chance than subjects without such a belief. However, they also found some evidence of gambler's fallacy in form of a decrease in expectancy after lucky outcomes. Our assessment of the heterogeneity in responses may also point towards a sub-group of subjects that respond to good luck by investing less in the next round and to a subgroup of subjects responding to bad luck by investing more in the next round. However, such responses may also be a sign of the randomness of the choices in the game and also depend on the pre-treatment investment level as we have shown.

Wohl and Enzle (2003) provided additional evidence related to the illusion of control theory of Langer (1975). They detected perceptions of personal luck as a potential source of misperceived skillful influence on non-controllable events. We did not investigate the cognitive reasoning associated with the luck responses we found. This is also a potential avenue for further research.

7 Conclusion

The belief in luck has so far been a topic of more interest to psychologists than to economists due to its irrational nature. However, if belief in luck is important for risk-taking behavior, it is highly relevant to better understand and predict economic behavior. Systematic irrational behavior has become a popular topic among behavioral and experimental economists (Ariely & Jones, 2008). Economists interested in predicting and understanding behavior may benefit from taking systematic irrational beliefs on account that influence behavior such as the effect of luck that we have studied in this paper. We may then to a larger extent have to draw on theories from psychology and possibly expand on our economic theories.

Our study utilizes a large random sample of university students and finds that there are strong luck effects in a repeated game of chance. This systematic irrational behavior is not predicted by the standard value function assumptions in Prospect Theory that bad luck stimulates more risk-taking. The systematic response patterns seem better explained by the illusion of control theory of Langer (1975).

We suggest that our finding of strong and to some extent cumulative luck effects in the risky investment game can contribute to a deeper understanding of the empirical findings of so-called measurement errors concerning the measurement of risk preferences. We have shown that luck effects contribute to the instability of the responses in the risky investment game. If ignored they may contribute to weak predictive power. We suggest that by taking luck into account in repeated games of chance we can enhance the predictive power of such simple experiments beyond what Gillen et al. (2019) achieved with their ORIV instrumental variable approach that did not take into account the luck outcomes in previous game rounds. Their approach may also lead to prediction bias if there are significant luck effects. Further research is needed to investigate the extent to which the luck effects carry over into other experiments and can help better explain real-world behavior. Further research should also investigate the cross-cultural variation in the predictive power of luck and identify cultural settings where it may hold more potential as an instrument for predicting behavior under risk.

Further work is needed to dig deeper into the mechanisms of these luck effects to help us to better understand the cognitive logic and thereby design theoretical models that are better at predicting behavior under risk and uncertainty. We have to accept that variation in beliefs possibly adds a level of uncertainty on top of objective risks from the experimenters' perspective, although beliefs from the decision-makers perspective may even make subjective risks lower or higher than the objective risks. More work is needed a) to dis-aggregate or separate the luck effects in terms of how they affect expectations (optimism/pessimism bias), subjective probability weighting, reference points, and value functions; b) to assess respondent heterogeneity concerning these factors in different contexts.

We conclude that the predictive power of Prospect Theory (PT) was limited in the recursive risky investment game as the predictive power of luck is not captured rigorously. One option is to complement PT with psychological belief-based theories to better explain the psychological causal mechanisms associated with luck and that affect the behavior in dynamic settings. Such psychological theories may also contribute to enhancing the understanding of subjective probability weighting and its stability over time and that is an important component of Cumulative Prospect Theory (Tversky & Kahneman, 1992).

Supplementary information. Upon the publication of the paper we can provide the data and codes used for the analysis.

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Declarations

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- Conflict of interest/Competing interests: The authors declare no conflicts of interest.
- Ethics approval: Norwegian Agency for Development Cooperation (NORAD) approved the project that aims to enhance collaborative research between the Norwegian University of Life Sciences (NMBU) and Lilongwe University of Agriculture and Natural Resources (LUANAR) and to contribute to capacity-building within Behavioral and Experimental economics in LUANAR. At the time of the establishment of the project, the two universities did not have Review Boards for the ethical assessment of experimental protocols in Experimental Economics. The experiments used in the project are standard incentivized experiments used in many research projects in behavioral and experimental economics. The researchers in the project push for the establishment of such Review Boards in both universities and have used general guidelines to meet all ethical requirements associated with the types of experiments used in the project, including prior informed consent, and ensuring the anonymity of all respondents in all shared data and publications. Special care was taken as the experiments took place during the fourth round of the corona pandemic in Malawi to satisfy all safety measures needed to avoid contributing to the spread of the virus.
- Consent to participate: All subjects were explicitly asked at the beginning of each round, after receiving an introduction, about their consent to participate.
- Consent for publication: All authors are project members and have participated in the project and have agreed to publish the work jointly.
- Availability of data and materials: Experimental protocols and data will be made available upon the publication of the paper and can be made available for reviewers upon request.
- Code availability: Codes for data analyses will be made available upon the publication of the paper and can be made available for reviewers upon request.
- Authors' contributions: Stein T. Holden (First author). The initial design of experimental protocols, conceptual ideas, data checking and cleaning, variable construction, statistical analysis, and write-up of the paper. Sarah

Table B1 Robustness check: Fractional probit models: Marginal effects without and with luck interactions

VARIABLES	(1) ri2	(2) ri3	(3) ri3 +T1*T2	(4) ri4	(5) ri4 +T1*T2
T1	0.310***	0.149***	0.150***	0.114**	0.114***
	(0.037)	(0.031)	(0.031)	(0.044)	(0.044)
T2		0.208***	0.209***	0.149***	0.149***
		(0.032)	(0.032)	(0.027)	(0.027)
Observations	612	534	534	533	533

Note: Dependent variables: Risky investment shares by round. Models with class FE. Cluster-robust standard errors in parentheses. Sign. levels: *** p<0.01, ** p<0.05, * p<0.1

Tione. Comments on experimental protocol, training of enumerators, implementation of experiments, data checking and corrections. Mesfin Tilahun. Comments on experimental protocol, training of enumerators, piloting and implementation of experiments, commenting on drafts. Samson Katengeza. Comments on experimental protocol, recruitment of enumerators, implementation of experiments.

Appendix A Experimental protocols

Experimental protocol is found in a separate file.

Appendix B Luck outcomes and change in Risky Investment shares by Round: Parametric models

Table B1 presents the marginal effects from the fractional probit models for investment shares without and with the T1*T2 interaction effects.

Table B2 presents model results for conditional IV models for Rounds 3 and 4 for split samples where sample splitting was based on luck outcome in Round 1 (T1). The models use the Round 1 investment level as an instrument to predict the Round 2 investment level to assess whether it had a different effect on the investment levels in Rounds 3 and 4, as well as to test whether the Round 2 luck effect (T2) is different for winners versus losers in Round 1.

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Table B2 Conditional on luck (split sample) IV models with lagged endogenous risky investment shares

VARIABLES T1 outcome	(1) ri3 Win	(2) ri3 Loss	(3) ri4 Win	(4) ri4 Loss
ri2, predicted	1.631***	0.656**	1.691***	-0.146
	(0.503)	(0.259)	(0.612)	(0.310)
T2	0.142***	0.215***	0.147**	0.234***
	(0.054)	(0.063)	(0.063)	(0.070)
Constant	-0.749	0.260	-0.771	0.882***
	(0.469)	(0.167)	(0.558)	(0.207)
Observations	300	232	299	232
R-squared		0.394		0.196
Number of ClassID	48	48	48	48
First stage:				
Instrument				
ri1	0.183***	0.279***	0.184***	0.279***
	(0.059)	(0.084)	(0.059)	(0.084)
Endog. test, p	0.000	0.312	0.000	0.184
Instrument strength, F	9.6	11.3	9.7	11.3

Note: Dependent variables: Subject level risky investment shares by round. 2SLS IV Models with robust standard errors in parentheses. Endogeneity test: Robust score. Models with Class FE. Sign. levels: *** p<0.01, ** p<0.05, * p<0.1

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