The Consequences of Narrow Framing for Risk-Taking: A Stress Test of Myopic Loss Aversion

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Abstract

Narrow bracketing in combination with loss aversion has been shown to reduce individual risk-taking. This is known as myopic loss aversion (MLA) and has been corroborated by many studies. Recent evidence has contested this notion indicating that MLA's applicability is confined to highly artificial settings. Given the impact of these findings, we reevaluated the evidence on MLA with substantially higher statistical power than in almost all previous studies. We systematically modified the seminal study design by Gneezy and Potters (1997) to include five key adjustments. These involved down-scaled returns, return compounding, and extended investment horizons. Our results—which are highly robust to analytical heterogeneity—consistently document the presence of MLA across all experimental conditions.

Keywords: myopic loss aversion, narrow framing, risk-taking, meta science, replication

JEL: D14, D81, G02, G11

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1 Introduction

Individuals often frame decisions narrowly, segregating outcomes or frequently evaluating them (Kahneman & Lovallo, 1993; Kahneman & Tversky, 1984; Read et al., 2000; Thaler, 1985; Thaler et al., 1997). In an investment context, this particularly applies when investors evaluate their portfolios on a short-term basis. This temporal myopia demonstrates the difficulty of people in foreseeing long-term outcomes and their implications for decisions. Coupled with a prevalent loss aversion (Kahneman & Tversky, 1979), myopia diminishes the propensity of individuals to allocate investments in riskier assets. This combination of temporal myopia and loss aversion is referred to as myopic loss aversion (MLA). Given its intuitive appeal and its explanatory power with respect to significant stock market anomalies, such as the equity premium puzzle (Benartzi & Thaler, 1995), MLA has generated considerable interest in the literature on economics and finance. For example, Benartzi and Thaler (1995)'s seminal work has received more than 4,500 citations, and Gneezy and Potters (1997)'s groundbreaking experimental verification of MLA has exceeded 1,700 citations, according to Google Scholar.

Previous studies have extensively documented behavior consistent with MLA across a wide range of demographics and settings. Most studies have provided evidence for MLA among university students (Bellemare et al., 2005; Fellner & Sutter, 2009; Gneezy & Potters, 1997; Keren & Wagenaar, 1987; Langer & Weber, 2008; Thaler et al., 1997; Wendy & Asri, 2012). Furthermore, observations of MLA extend beyond students to the general population (Van der Heijden et al., 2012), financial experts and traders (Eriksen & Kvaløy, 2010; Haigh & List, 2005; Iqbal et al., 2021; Larson et al., 2016), decision-making teams (Sutter, 2007), and private investors (Wendy & Asri, 2012). Notably, natural field experiments have revealed that financial professionals exhibit MLA behaviors within their daily work environments (Larson et al., 2016). Evidence of MLA-consistent behavior extends to retirement planning and insurance (Benartzi & Thaler, 1999; Papon, 2008) as well as to experimental markets (Gneezy et al., 2003). Together, these studies underscore the contribution of MLA to conservative decision making and its association with generally suboptimal financial outcomes (Larson et al., 2016; Looney & Hardin, 2009; Thaler et al., 1997).

Nevertheless, recent empirical evidence has increasingly called the concept of MLA into question. Several of the aforementioned studies have implemented the design by Gneezy and Potters (1997) as a benchmark. Their investment task extended across nine periods, featuring linear return calculations. Their risky asset yielded rates of return of either +250% or −100%, which resembles an "all-or-nothing" gamble. These experimental settings and parameters markedly deviate from the more realistic scenarios of annual investment returns, compound returns, and the extended investment horizons typically observed in financial markets. Although some field studies, including that of Larson et al. (2016), have featured compound return calculations and more realistic rates of return, these studies primarily focused on professional financial traders and involved alterations beyond the scaling of returns and compounding. This complexity makes it challenging to isolate and evaluate the singular effect of these characteristics on MLA, especially within the traditional, widely-used framework established by Gneezy and Potters (1997). In a substantial and resource intensive study, Beshears et al. (2017) have taken this as an impetus to examine whether MLA is robust to more realistic, scaled-down rates of return, return compounding, and extended investment horizons.[1] Their findings suggest that behaviors consistent with MLA may not be prevalent in more

[1] Moreover, Beshears et al. (2017) have introduced longer delays between periods in their post-lab conditions in order to move away from short laboratory settings toward more realistic real-world investment horizons. The study cost more than $200,000 in participant payoffs, by magnitudes more than comparable studies.

realistic investment contexts. In a series of tests they came to the conclusion that the artificial "all-or-nothing" gamble is responsible for the non-replication and that the results do not extend to settings with less extreme and more realistic risk profiles. This necessitated a thorough reevaluation of the multitude of studies utilizing Gneezy and Potters (1997)'s experimental design, thereby challenging the overarching validity of the MLA literature. By contrast, Langer and Weber (2008) found MLA-consistent behavior in a small sample of university students when applying similar modifications to the scaling of the risky asset's return, return compounding, and investment horizon. Recently, Schwaiger and Hueber (2021) found that the original protocol by Gneezy and Potters (1997) replicates only among the more attentive crowdworkers on Amazon MTurk. Additionally, a variation of the original lottery, which only differed in terms of the probabilities of winning and losing (50% each instead of 33% and 66%, respectively) did not lead to behavior consistent with MLA. Nevertheless, the vast majority of studies primarily maintained or minimally altered the original framework set by Gneezy and Potters (1997). Apart from the study by Beshears et al. (2017), no other research has undertaken a comprehensive and systematic revision of the experimental parameters.[2] The path-dependent nature of research, with the vast majority of studies applying the original parameters by Gneezy and Potters (1997), thus seriously questions the external validity of the whole research field. Furthermore, different analytical choices for testing MLA behavior in published studies, such as the choice of regression models or covariates, could (partly) explain the heterogeneity of MLA results in the literature (Holzmeister et al., 2023; Menkveld et al., 2024; Simonsohn et al., 2020).[3] Considering the divergent findings from studies deviating from Gneezy and Potters (1997)'s original protocol, a clear consensus remains elusive about whether MLA is a universal feature of investment decisions or a fragile artifact that crucially depends on stylized experimental designs and analytical choices. Furthermore, it is important to acknowledge the potential impact of publication bias when evaluating the scientific arguments for and against the relevance of MLA.

Previous studies questioning MLA's robustness to real-world financial contexts as well as studies confirming the original findings by Gneezy and Potters (1997) suffered from at least one—and most often two—of the following two issues ex ante: (i) insufficient statistical power to reliably detect small- to medium-sized standardized effects and (ii) non-isolated alterations of characteristics of the original Gneezy and Potters (1997) setting. The first issue raises the question of whether MLA's absence in more realistic contexts is genuine, or if its effects are simply diminished—rendering it unlikely to detect in studies lacking sufficient statistical power. The power curves presented in Figure 2 illustrate that even slight reductions in the true standardized effect, potentially resulting from alterations in the experimental design, lead to a notable decrease in statistical power. This would markedly hamper the ability to reliably identify diminished yet economically significant effects.[4] The second issue, that of concurrent modifications, complicates the attribution of specific design changes to MLA's observed fragility. In their study, Beshears et al. (2017) not only reduced the rates of return on the risky asset but also simultaneously transitioned from the original model with periodic endowments and linear returns to a singular initial endowment and compound returns. In this modified version, each decision impacts not only the immediate outcomes but also the available funds for investment in subsequent periods, potentially prompting participants to frame the investment decision more broadly overall. Adopting a broader perspective might inherently mitigate myopia in decision-making by

2 Table B4 offers a detailed overview of the applied modifications in studies based on the experiment by Gneezy and Potters (1997). We will elaborate below on our important extensions to Beshears et al. (2017).

3 Table B9 depicts the implemented analytical pathways in published studies that adopted the Gneezy and Potters (1997) design.

4 For the detailed power calculations see the *R* script in the project's OSF repository (9xmda).

underscoring the long-term ramifications of present choices (Langer & Weber, 2008). However, the compound nature of returns heightens the significance of each choice as it affects the capital that is available for investments for the next periods, potentially leading to more conservative investments due to loss aversion. Increased caution may offset the mitigating effects of a broader decision-making frame on MLA. The cumulative impact on MLA from transitioning from periodic endowments without compounding to a singular initial endowment with compounding has yet to be isolated, leaving its overall effect ambiguous. Specifically, compound returns, down-scaling of returns, or the combination of both including potential interaction effects, might diminish MLA-consistent behavior compared to the original setting. The discrete effects of each modification have not been distinctly isolated in the literature. It is plausible that some of the described alterations could counteract each other. Therefore, the degree to which MLA findings can be generalized to scenarios that differ from the common paradigm established by Gneezy and Potters (1997), as well as the specific factors influencing this generalizability, continue to be unclear.

To illuminate these critical gaps and assess the comprehensive MLA literature's relevance and impact, we contribute to the field through extensive, pre-registered online experiments with students from two large universities, in the Netherlands and in Austria. In light of conflicting findings, our objective was to ascertain the robustness of MLA and identify specific modifications to the Gneezy and Potters (1997) design that potentially mitigate individuals' inclination toward MLA-consistent behavior. Our methodology is distinguished by its capacity for the meticulous isolation of disparate elements of experimental design choices, such as realistic rates of return, return compounding, and investment horizons. Employing a (partial) factorial design, we were able to precisely discern the impact of each of the more realistic investment attributes on MLA tendencies. Furthermore, we conducted our study with a substantially larger number of participants compared to almost all previous studies that applied the Gneezy and Potters (1997) design (detailed in Table B4). This ensured that we were sufficiently powered to reliably identify even minor to moderate standardized effects, as illustrated in Figure 2. Lastly, acknowledging the diverse statistical analyses of MLA in existing research, we introduce innovation through the adoption of a multiverse approach complemented by specification curve analysis (Simonsohn et al., 2020). This method addressed potential variations in our results arising from analytical heterogeneity, thereby enhancing the reliability of our conclusions. We based the choice of the analytical pathways on an extensive examination of the applied analyses in the related MLA literature.

Our analysis uncovered compelling evidence for the persistence of MLA across all examined settings, including more realistic, down-scaled rates of return, a compound return scheme following a single initial endowment, and longer investment horizons. The outcomes derived from the multiverse approach underscored the consistency and reliability of our findings across more than 10,000 distinct analytical specifications, including varied sample exclusion criteria, sets of covariates, and regression methodologies. Collectively, our results present a stark contrast to prior research challenging MLA's robustness, as we found significant evidence for the relevance of MLA in all of our conditions and, thus, for more realistic investment settings.

Our results show that MLA can undermine long-term wealth accumulation, especially as individuals take on more responsibility for retirement savings with the shift from "Defined-Benefit" to "Defined-Contribution" pension schemes. Technological advancements that enable quick information processing may encourage short-term thinking, detracting from a strategic, long-term investment approach (see, e.g., Kalda et al., 2021). Policies promoting a long-term perspective—through tax incentives, loyalty programs, or educational initiatives—could enhance financial well-being. The effectiveness of such strategies depends on the presence or absence of MLA in specific investment contexts, which this research clarifies by resolving inconsistencies in more realistic settings.

2 Experimental Design

The experimental design of this study is based on the protocol by Gneezy and Potters (1997). In the original study across each of nine periods, participants allocated a financial windfall endowment between a risky asset, which had a positive expected value, and a risk-free cash option. In one of the two treatment groups, designated as HIGH, the authors introduced a higher frequency in which participants made decisions and received feedback on investment outcomes. Participants randomly assigned to treatment HIGH received outcome feedback and made decisions in each period, while the feedback and decisions in LOW always applied to three consecutive periods. Behavior aligning with MLA theory manifests when individuals in the LOW treatment group show average higher investments in the risky asset compared to those in the HIGH group. Investments with a positive expected return are characterized by an increasing (non-monotonic) likelihood of aggregate positive outcomes over time, irrespective of whether returns are compound or linear. On average, this makes the investment more attractive under infrequent evaluation for a loss-averse investor. Furthermore, the commitment to decisions across multiple periods encourages more prospective thinking in the LOW group (Redelmeier & Tversky, 1992). The risky asset in Gneezy and Potters (1997) is characterized by a binary distribution, yielding periodic outcomes where there is a one-third probability of achieving a +250% gain and a two-thirds probability of incurring a total loss (−100%).

To underpin our experimental design, we utilize cumulative prospect theory (CPT), which provides a theoretical framework for understanding behavior consistent with MLA, as discussed by Langer and Weber (2005). In an extension of MLA to myopic prospect theory, Langer and Weber (2005) emphasized the importance of additional factors, such as probability weighting and value function curvature. By integrating CPT into our experimental design, we leveraged this critical factor to isolate the effects of key variables on investment behavior, ensuring that any observed differences were not merely due to variations in CPT values. To evaluate the attractiveness of the lotteries, we modeled a CPT agent using the following parameters: $\alpha = \beta = 0.88$, $\gamma = 0.61$, $\delta = 0.69$, and $\lambda = 1.6$.[5] Based on these parameters, a myopic decision maker would reject the gamble, as the periodic CPT value is negative (−2.2 for an investment of 100). In contrast, when evaluating the aggregate outcome over three periods, the CPT value becomes positive (4.3 for an investment of 100), indicating a preference to invest in the risky asset.[6] As we will show below, a myopic decision maker rejects the gamble under all conditions in our study ($CPT_1 < 0$), while a more forward-looking decision maker opts to accept it ($CPT_3 > 0$).

2.1 Conditions

For this study, we replicated the original design and additionally modified it in relation to the following three critical dimensions. These changes enabled us to assess the robustness of MLA under more realistic conditions, including returns that closely mirror actual one-year stock market performance, return compounding, and extended investment horizons. This, in turn, allowed us to differentiate the effects across key dimensions of the design.

5 α and β are the curvature parameters of the value function for gains and losses, respectively, while γ and δ represent the probability weighting parameters for gains and losses. Value function curvature and probability weighting parameters are based on Tversky and Kahneman (1992). In line with empirical estimates, however, we assumed a lower magnitude of loss aversion (Walasek et al., 2018). Applying a higher loss aversion parameter such as Tversky and Kahneman (1992)'s $\lambda = 2.25$, the three-period lottery would still be preferred, but CPT would predict rejection of both gambles.

6 For the detailed calculations see the *R* script in the project's OSF repository (9xmda).

(1) **Rates of Return**: The properties of the risky asset in Gneezy and Potters (1997) do not resemble those of typical retail investment products. To enhance realism, we adjusted the rates of return, scaling them down to +25% and −10%, mirroring the approach by Beshears et al. (2017). The authors came to the conclusion that MLA in the Gneezy and Potters (1997) experiment only holds true due to unrealistic return scenarios as they did not find evidence for MLA with such down-scaled rates of return. In our setting, scaling down rates of return reduces the CPT values of one- vs. three-period prospects to around −0.3 and 0.6, respectively. Theoretically, a decision-maker guided by CPT still only accepts the three-period prospect. However, the practical impact of such a slight absolute value difference between the one- and three-period prospects on empirical outcomes might be reflected in smaller true effect sizes. Such a consideration could potentially explain why previous studies without sufficient statistical power have detected no effect. Should the effect disappear or lose economic significance, determining whether the cause is the reduced overall appeal of the prospect or the manner in which returns are scaled becomes challenging. Therefore, we introduced an additional condition featuring asset rates of return of +230% and −90%. By reducing the potential periodic loss to −90% of the invested amount, we were able to examine whether MLA behavior persists when the asset profile does not follow an "all-or-nothing" return framing. Again, a narrow one-period evaluation predicts rejection by a CPT decision maker, whereas a broader and aggregate evaluation of the three-period return distribution predicts acceptance of the gamble ($CPT_1 = -1.2$; $CPT_3 = 6.1$).[7]

(2) **Compound Returns**: In the original Gneezy and Potters (1997) study, participants received a new endowment in each of the nine periods for making investment decisions specific to each period. Periodic earnings were calculated for each of these separate decisions, and total earnings equaled the sum of all independent periodic earnings. However, subsequent studies by Langer and Weber (2008) and Beshears et al. (2017) highlighted that this approach, resulting in linear returns, diverges from typical investment practices. In real investment contexts, investors benefit from the compounding of capital gains and the reinvestment of dividends. Moreover, investors usually do not invest for predetermined periodic intervals and are not forced to liquidate their position after each period. Therefore, building upon the framework of Beshears et al. (2017), to simulate compound returns, we provided participants with an initial endowment (I), set as the product of the number of periods (K) and the periodic endowment (P), that is, $I = K \times P$. This endowment was allocated at the start, and the balance was adjusted at each period's end to reflect any gains or losses, effectively carrying the balance forward through the experiment. For instance, if participants invested a fraction x of their initial endowment I in the first period with a return of r_1, the endowment for the second period I_2 would be recalculated to include the gains or losses from that investment, that is, $I_2 = I \cdot (1 - x) + I \cdot x \cdot (1 + r_1)$. Figure 1 illustrates the comparison between linear and compound return calculations across the experiment's duration. Notably, Beshears et al. (2017) altered both the return scaling

[7] We adjusted the up-scaling from 250% to 230% to align the CPT values more closely with those in Gneezy and Potters (1997). We strongly believe that variances in MLA behaviors, compared to the original study, would mainly be due to the reduced risk of total loss rather than the slight change in up-scaling. Keeping the up-scaling at 250%would have led to two simultaneous changes: a move away from the risk of total loss *and* a significant deviation in CPT values. In particular, CPT would have predicted acceptance of both the one-period and the aggregated three-period lottery ($CPT_1 = 2$; $CPT_3 = 15.1$).

and return compounding and conjectured that it is likely the former that drives their insignificant results, but that the latter is a possible reason as well.

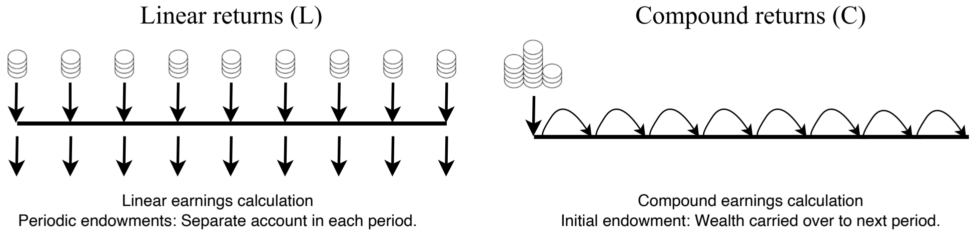

Figure 1: Linear versus compound return calculation.

(3) **Investment Horizon**: When participating in the investment task over nine periods, participants in condition LOW only made three decisions. We elevated decision-making and feedback instances—from 3 to 10 in the LOW condition, and from 9 to 30 in the HIGH condition. This is consistent with Langer and Weber (2008) and aligns our experiment with more conventional long-term investment scenarios. This modification allowed us to explore how extended decision-making frames might either mitigate or amplify MLA tendencies. Notably, only a minority of studies, including the first experiment by Beshears et al. (2017), utilized an investment horizon exceeding nine periods. In their study, Beshears et al. (2017) implemented the return histogram design from Benartzi and Thaler (1999) and extended the investment horizon to 52 periods, incorporating time delays of one week, real-world investment funds, and various other interventions and adjustments (details of which we discuss below). They did not find behavior consistent with MLA. Ponti and Tomás (2021) find that lowering feedback and decision frequency increases risk-taking (at a decreasing rate), but the effect is only significant for sufficiently long time horizons. However, they focused on shorter horizons of 3 to 12 periods. To systematically examine the investment horizon's effect on MLA, we introduced extended periods of 30 to guarantee a multiple of three based on the original design in Gneezy and Potters (1997).

2.2 Theoretical Predictions

For our non-pre-registered predictions, we build on Barberis et al. (2006) and model investment decisions as individuals assessing both the aggregate outcomes of repeated lottery plays and the outcomes of single plays. The CPT evaluation of an N-period gamble is therefore expressed as follows:

$$\mathrm{CPT} = w(N, CR) \cdot \sum_{I=1}^{N} \mathrm{CPT}_{1,i} + \big(1 - w(N, CR)\big) \cdot \mathrm{CPT}_N$$

A higher decision and feedback frequency (HIGH) increases the tendency to frame investments narrowly, resulting in a larger decision weight on single-period outcomes $w_{HIGH} > w_{LOW}$. This leads to a higher CPT evaluation in LOW if and only if $\sum_{i=1}^{N} \mathrm{CPT}_{1,i} < \mathrm{CPT}_N$. The dimension of return scaling directly enters the CPT calculations, with weights on segregated outcomes determined as follows:

$$w(N, CR) = a \cdot e^{-h \cdot N} - c \cdot CR$$

where N represents the investment horizon indicated by the number of periods, and CR is a dummy variable coded 1 for compound return conditions and 0 otherwise.[8] The term e^{-hN} describes exponential decay at a rate determined by h, illustrating how people adopt a broader frame as the horizon (N) increases, with this broadening effect diminishing as N grows further. In the original Gneezy and Potters (1997) experimental design ($N = 9$ and $CR = 0$), a stronger tendency for narrow framing was induced in HIGH, resulting in $a_{HIGH} > a_{LOW}$ and behavior consistent with MLA. Extended investment horizons ($N > 9$) might lead to a broader framing of investments as extended planning horizons are often correlated with greater investment risk-taking (see, e.g., Anderson & Settle, 1996; Dierkes et al., 2010). However, the extent of this effect could differ between the LOW and HIGH treatment groups, potentially influencing MLA. We hypothesized that extending the investment horizon will lead to more uniform investment behaviors across treatments, thus reducing MLA, as both groups, particularly HIGH, will have sufficiently prolonged decision and investment horizons. Over time, individuals gain insights into the aggregate distribution of the lottery outcomes. As the investment horizon lengthens, the greater absolute increase in the number of decisions and feedback instances in HIGH (e.g., from 9 to 12 periods = +3 decisions) compared to LOW (+1 decision) suggests a more significant reduction in narrow framing in HIGH relative to LOW: $h_{HIGH} > h_{LOW}$. This is intuitive given that, in the original design by Gneezy and Potters (1997), the outcomes in LOW are already grouped into intervals spanning one-third of the entire investment horizon.

Our model also predicts that the choice of the return calculation method impacts MLA. Compared to Gneezy and Potters (1997), providing a one-time initial endowment with compounding may influence participants' investment strategies by emphasizing the long-term consequences of their initial decisions. This could encourage a more forward-looking perspective on investments and help counteract narrow bracketing, especially in condition HIGH as we argue below. Naturally, it could also lead participants to adopt more conservative strategies initially, as they become more aware of the lasting effects of their choices. However, Klos (2013) found that emphasizing final outcome distributions can mitigate MLA. Building on this research, we hypothesized that introducing an initial endowment with compounding has a net effect of reducing MLA-consistent behavior, thus, $c_{HIGH} > c_{LOW}$. This arises from the greater relative increase in stakes per decision in HIGH compared to LOW, which is expected to decrease narrow bracketing more effectively in HIGH. For instance, in the nine-period conditions, the stakes in round 1 increase from 100 to 900 ECU in HIGH, whereas in LOW, they only increase from 300 to 900 ECU.

Table 1 depicts the characteristics of the six different conditions under which we tested the robustness of MLA. Alongside a baseline condition (250-100L9) identical to Gneezy and Potters (1997)'s design, our study was structured to isolate the effects of scaled-down rates of return and a departure from "all-or-nothing" gamble framing on MLA (top left box with conditions 250-100L9, 230-90L9, and 25-10L9). The CPT value differences between the LOW and HIGH treatments under the 230-90L9 setup closely mirrored those in the original lottery, maintaining the value relationship between the LOW versus HIGH scenarios. Thus, condition 230-90L9 primarily deviates from the

[8] Introducing compounding directly affects the returns used in the CPT calculations, but because both the sum of individual evaluations and the aggregate evaluation are based on the same series of returns, differences in CPT values arise solely from whether these values are considered separately for each period or combined as a whole.

original by not featuring the potential for a total loss. Additionally, our experiment featured a 2 (linear vs. compound returns) × 2 (short vs. long horizon) design contrasting linear versus compound returns and short versus long investment horizons, all under more realistic, scaled-down rates of return (all four boxes with conditions 25-10L9, 25-10L30, 25-10C9, and 25-10C30). In contrast to prior research on the robustness of MLA, we were thus able to disentangle the influence of the return scaling and the influence of the return calculation.

Table 1: Experimental Conditions Overview: This figure delineates the between-subjects experimental setup. Our design enabled us to examine the isolated impact of scaled-down rates of return and the shift from an "all-or-nothing" gamble framing on MLA (top left box with conditions 250-100L9, 230-90L9, and 25-10L9. Furthermore, it illustrates how the investment horizon and return calculation variables are systematically varied within a 2 × 2 factorial design, incorporating realistic return rates (all four boxes with conditions 25-10L9, 25-10L30, 25-10C9, and 25-10C30.

Return Calculation		Investment Horizon: 9 Periods	Investment Horizon: 30 Periods
	Linear	250-100L9	25-10L30
	Linear	230-90L9	25-10L30
	Linear	25-10L9	25-10L30
	Compound	25-10C9	25-10C30

2.3 Procedure

After providing informed consent to the study's terms and conditions,[9] participants viewed an elaborate description and illustration of the asset's return distribution. To maintain comparability, our instructions were identical to those of Beshears et al. (2017) except for necessary minor edits due to the online experimental setting (see Appendix C for the full set of the experimental instructions). In conditions with linear returns (250-100L9, 25-10L9, 25-10L30, and 230-90L9), participants received 100 experimental currency units (ECU) in each period, whereas in conditions with compound returns (25-10C9 or 25-10C30) participants received either 900 ECU or 3,000 ECU in Period 1 to be invested over either 9 or 30 periods. Participants in the LOW treatment were informed that each of their decisions would apply to the subsequent three periods and that their investment results would be presented in three-period blocks. In contrast, participants in the HIGH treatment were informed that they would make decisions and receive feedback on a period-by-period basis.

In each of the six conditions and each of the two treatments, HIGH and LOW, participants indicated their investment as a percentage of the endowment in ECU. This standardized approach ensured that the set of investment allocations was consistently scaled across all conditions. Furthermore, these percentages could be readily converted to their corresponding absolute amounts, ensuring comparability across different conditions. On each feedback screen, we presented the return outcome(s) of the risky asset, the amount gained or lost, and the total

9 The study has been approved by the ethics board of the University of Zurich.

earnings from the previous period (HIGH) or the previous three periods (LOW). Importantly, as in earlier studies (with the exception of Hardin & Looney, 2012), single-period outcomes and earnings were also displayed for participants in LOW. After the final period, participants received information about their final payoff. Due to the different structures and lengths of conditions, incentives varied slightly in magnitude.[10]

The experiment concluded with pre-registered survey questions on perceptions of ambiguity and risk associated with the lottery, which were aimed at uncovering potential explanations for variations in risk-taking across conditions (Venkatraman et al., 2006).[11] In addition, as pre-registered, we included three questions on the understanding of the risky asset return distribution to identify and exclude participants who were inattentive or did not understand relevant information from the sample that we used for a robustness check. Finally, we collected basic demographic data to be used for sample balancing diagnostics and to be added as control variables in our regression analyses.

3 Results

3.1 Statistical Power and Sample

All analyses presented herein, unless noted otherwise, were pre-registered on "AsPredicted".[12] We adhered to significance levels (α) of 5%, 1%, and 0.1%, respectively, for all analyses. The analyses have been carried out using *R* (the script is included in our OSF repository (9xmda)). Our final sample consists of data we collected via online experiments in three waves with students at Radboud University in the Netherlands (Wave 1 & 2) and the University of Innsbruck in Austria (Wave 3).[13] We did not establish any specific exclusion criteria for the students prior to the study. We invited students from different universities to ensure a sufficient number of participants. This enabled us to achieve a high statistical power to reliably detect small to medium-sized standardized effects.

We conducted ex-ante statistical power analyses for which we used Cohen's d as a standardized effect size. With our 2,245 participants (pre-registered: 2,200) in total, we generated on average 187 independent observations per treatment—HIGH and LOW—across all six conditions. Thus, we had a statistical power of at least 80% (90%) to reliably detect a standardized effect size equal to or larger than Cohen's $d = 0.29$ ($d = 0.34$), given a Type I error rate of $\alpha = 0.05$ in pairwise comparisons via two-sided unpaired-sample t-tests (Refer to "Present Study" in Figure 2, where we ensured that all compared studies have at least 10 citations on Google Scholar, establishing a lower bound for impact in the field).

The median duration of the experiment in the full sample was 10 minutes, with a median compensation of €2.49, corresponding to an hourly rate of €14.94. As pre-registered, we excluded the fastest and slowest 2.5% of participants in terms of total processing time from the analyses to increase the signal-to-noise ratio in our data. Clicking through quickly may be an indicator of a lack of focus and understanding of the procedure and the lottery (Chmielewski & Kucker, 2020; Downs et al., 2010). As a result, behavior consistent with MLA may not unfold as it

10 In the conditions with nine periods, the payment in Euro equalled the total ECU earnings in the experiment divided by 400. In the conditions with thirty periods, we divided the total ECU earnings by 1,200 to achieve similar payments and also to compensate participants for the slightly longer time spent on the additional investment periods.

11 Since these questions were exclusively pre-registered and asked during Wave 3, we have detailed their corresponding results in Appendix A. This approach allowed us to focus on the most relevant findings based on the combination of all waves in the paper's analysis section.

12 See the pre-registration under the following links: Wave 1, Wave 2, Wave 3.

13 Wave 1 featured only conditions 250-100L9, 25-10L9 and 25-10C9, whereas Wave 2 featured only 230-90L9, 25-10L30 and 25-10C30. Wave 3 contained all six conditions.

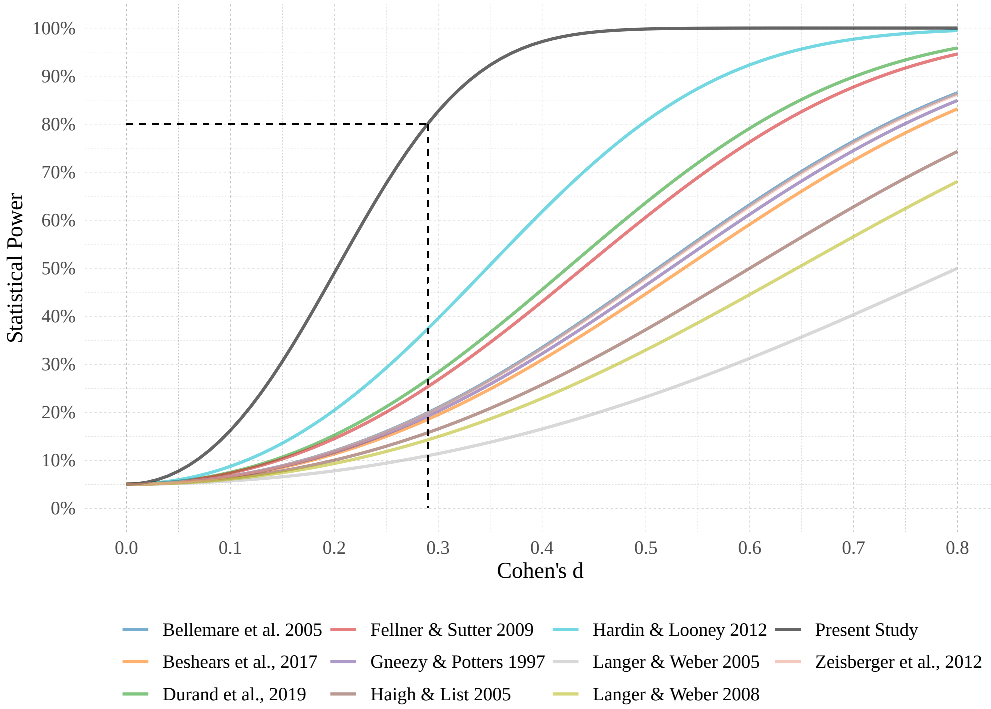

Figure 2: Statistical power calculations for the treatment comparison HIGH versus LOW based on the sample size of our study and of influential previous experimental studies (more than 10 citations on Google Scholar) in the field of MLA, applying a variant of the Gneezy and Potters (1997) design. $\alpha = 0.05$.

would in real-world investment decisions. Conducting the experiment too slowly could also be a problem as participants may take longer breaks and forget parts of the instructions (Abbey & Meloy, 2017; Downs et al., 2010). This trimming procedure led to $2,131$ observations in the final sample, which we used for the main analyses. As a robustness check, we also ran our main analysis with the full sample, for which we found consistent results, which we report in Table B7 in the Appendix. Furthermore, as pre-registered, we applied another robustness check, excluding participants who gave too many incorrect answers and those who indicated that they "did not understand at all" in a set of comprehension check questions (see Appendix C). Compared to our final sample, this robustness sample also provided qualitatively identical results.

Prior to conducting our main analyses, we assessed the balance of observable characteristics within our final sample following the trimming procedure. To accomplish this, we conducted balancing tests and examined standardized mean differences (SMDs) to identify problematic discrepancies in self-reported participant demographics and traits, such as gender, risk preferences, statistical knowledge, and university affiliations, across the two treatments and between the different cohorts. This step was taken to address endogeneity concerns by identifying potential random confounders resulting from imbalances, which should be included as control variables in our analysis (Austin, 2009). The results of the sample balancing diagnostics are presented in Table B1, Table B2 and Table B3 in the Appendix. Consistent with rules of thumb in the literature, we flagged any imbalance as problematic if it exceeded a standardized mean difference (SMD) of 0.1 (Zhang et al., 2018). Our results showed absolute SMDs exceeding 0.1 of multiple variables between HIGH and LOW and between university cohorts. Thus, for our analyses, we exercised caution and estimated econometric specifications that controlled for cohort effects and all self-reported participant characteristics. We also tested for multicollinearity among the covariates by calculating the variance inflation factors (VIFs), all of which were below 2. Thus, multicollinearity did not pose an issue.

3.2 Main Analyses

Participants' average invested amount in the lottery in percent of the periodic endowment or the current balance served as our main outcome variable. Figure 3 shows the average investment in percent in treatment HIGH and LOW across all six conditions, 250-100L9, 230-90L9, 25-10L9, 25-10L30, 25-10C9, and 25-10C30.[14] The whiskers indicate 95% confidence intervals, and the stars indicate ranges of p-values obtained by running unpaired-sample t-tests and—as robustness check—permutation tests comparing average investment amounts between treatments LOW and HIGH in each condition. As indicated by Figure 3, we found statistically significant evidence for behavior consistent with MLA in each of the six conditions (see Table B5 in the Appendix for the statistical details of the applied unpaired-sample t-tests for each condition). Consistent with MLA, we report universally higher risk-taking among participants in treatment LOW compared to participants in treatment HIGH. MLA behavior was most prevalent in condition 250-100L9—our Gneezy and Potters (1997) replication—with the standardized average investment difference (LOW − HIGH) amounting to $d = 0.45$, followed closely by condition 25-10C30 with $d = 0.42$ and condition 230-90L9 with $d = 0.36$. In conditions 25-10L30 with $d = 0.29$, 25-10C9 with $d = 0.28$, and 25-10L9 with $d = 0.27$, we observed small-to-medium standardized differences.[15] Our findings present a coherent and consistent view: MLA emerges as a behavioral phenomenon even when we scaled down returns to more realistic levels. This pattern held true not only under the traditional linear framework of Gneezy and Potters (1997) but also in settings involving more realistic compound returns. Importantly, behavior consistent with MLA under realistic returns persisted across both shorter and longer investment horizons, regardless of whether the returns were calculated on a linear or compound basis. In particular, the significant difference in condition 25-10C30—our most realistic condition featuring down-scaled and compound returns as well as a longer investment horizon—reveals important insights into the economic significance of our results. Participants who received aggregated feedback and experienced decision commitment (LOW) allocated, on average, an additional 12.03 percentage points of their balance (see Table B5 in the Appendix) compared to those with more frequent feedback and decision-making (HIGH). To accurately reflect this in monetary terms, consider that if participants in the HIGH condition invested a certain percentage of their balance in a given period, say 40% of 3,000 ECU, their starting balance, resulting in an actual investment amount of 1,200 ECU, then participants in the LOW condition would be expected to invest $40\% + 12.03pp. = 52.03\%$ of their balance. Given a balance of 3,000 ECU, this specific investment behavior would translate to an actual investment amount of 1.561 ECU, which is larger by 361 ECU compared to group HIGH. Therefore, this effect is not just statistically significant but also reflects a considerable economic impact, underscoring the importance of MLA in investment decisions.

Our results stand in contrast with those questioning MLA in broader contexts, for example, Beshears et al. (2017), who did not observe evidence of MLA in a setting with compound returns and with returns scaled down to 25% and −10% (identical to 25-10C9). Overall, our analysis revealed that MLA remains a persistent phenomenon

[14] Average period-level investments are visualized in Figure B2 in the Appendix.
[15] For a comparison of these effect sizes with those reported in the literature, see Table B4 in the Appendix.

when the risky asset return profile deviates from the original Gneezy and Potters (1997) asset, independent from altering the earnings calculation and the investment horizon. This underscores the robustness of the original results even under modified properties of the risky asset.

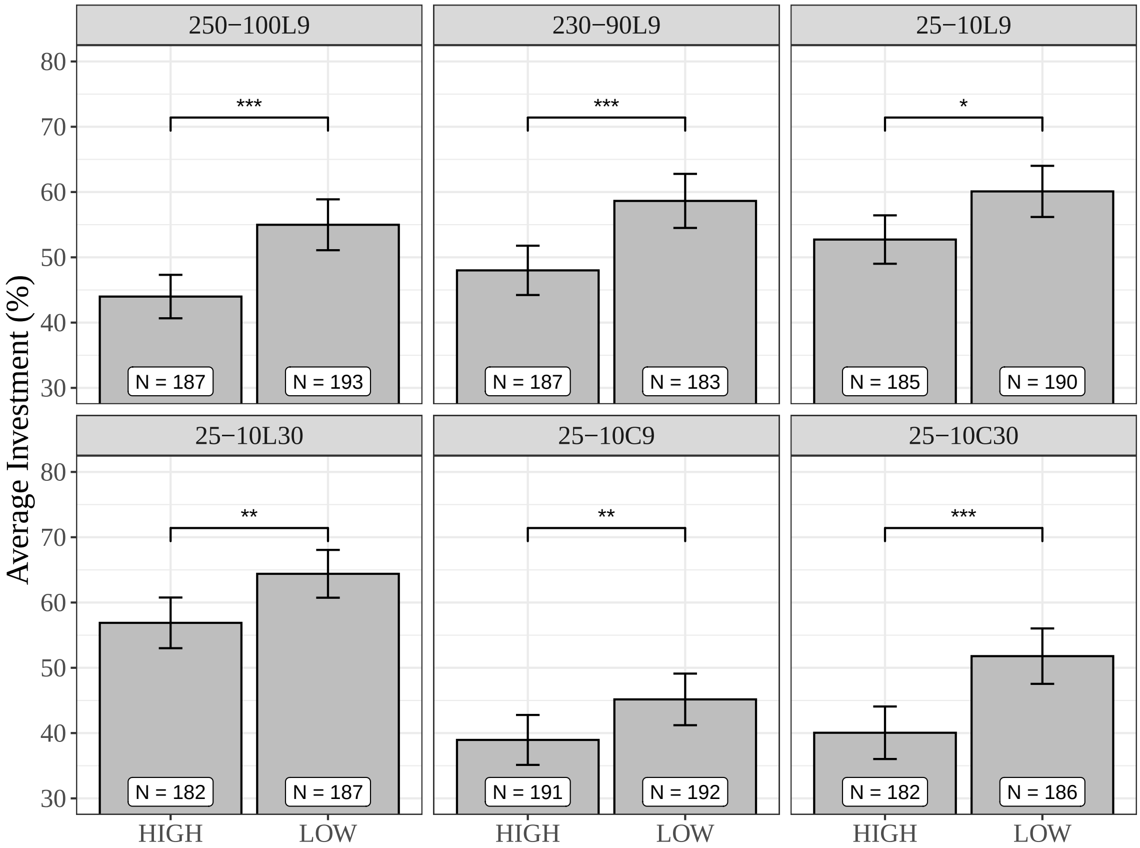

Figure 3: Average investment percentages between treatments HIGH and LOW across different conditions. HIGH features periodic feedback and decisions, whereas these are binding for three periods in LOW. 250-100L9 implements the Gneezy and Potters (1997) design, whereas the other conditions represent the different modifications (see Table 1). Error bars indicate 95% confidence intervals around mean investments in each treatment and condition. The stars indicate ranges of p-values obtained by running unpaired-sample t-tests and—as robustness check—permutation tests comparing average investment amounts between treatments LOW and HIGH in each condition (*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$). Details on differences in risk-taking and the results of pairwise unpaired-sample t-tests are provided in Table B5.

To further inform our analyses, we ran multivariate fractional response regression models with logit links and heteroskedasticity-robust standard errors for each of the six conditions with the average proportional lottery investments over all respective periods (9 or 30) as the dependent variable.[16] We report the results in Table 2, which shows average marginal effects. For each model we also included the five covariates as control variables to verify the validity of our results. The coefficient LOW represents a binary dummy that equals 0 for participants in treatment HIGH or 1 for participants in treatment LOW. FEMALE is a binary dummy variable that equals 0 for male participants or 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants stated to

[16] As the investments represent the only variable exhibiting variation across different periods, we averaged the investments for our models. Our results remained qualitatively consistent when we repeated the analyses using periodic data and applied clustered standard errors at the individual level (see Table B6).

have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants, which were measured using the German SOEP questionnaire (Dohmen et al., 2011) on Likert scales from 0 to 10. STAT.KNOWLEDGE represents participants' self-reported statistical knowledge compared to their fellow students on a seven-point scale. INNSBRUCK is a binary dummy that equals 0 for participants from Radboud University or 1 for participants from the University of Innsbruck.

Table 2: Average marginal effects fractional response models with logit links and the amount invested in percent of the endowment as dependent variables. The binary dummy variable LOW is coded 0 for participants in the HIGH treatment and 1 for those in the LOW treatment. FEMALE is a binary dummy variable that equals 0 for male participants or 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants stated to have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants, which were measured using the German SOEP questionnaire (Dohmen et al., 2011) on Likert scales from 0 to 10. STAT.KNOWLEDGE represents participants' self-reported statistical knowledge compared to their fellow students on a seven-point scale. INNSBRUCK is a binary dummy that equals 0 for participants from Radboud University or 1 for participants from the University of Innsbruck.

	Dependent variable: Investment (%)					
	Conditions:					
	250-100L9	230-90L9	25-10L9	25-10C9	25-10L30	25-10C30
	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
LOW	0.116***	0.102***	0.070*	0.078**	0.059*	0.118***
	(0.025)	(0.028)	(0.028)	(0.028)	(0.027)	(0.028)
FEMALE	−0.088**	−0.052	−0.039	−0.104**	−0.061	−0.148***
	(0.028)	(0.035)	(0.031)	(0.034)	(0.032)	(0.036)
INVESTOR	−0.017	−0.011	0.023	−0.015	0.071*	−0.027
	(0.027)	(0.033)	(0.033)	(0.033)	(0.031)	(0.034)
RISKTOLERANCE	0.040***	0.031***	0.019*	0.034***	0.026***	0.030***
	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)
STAT.KNOWLEDGE	0.014	0.011	0.030*	−0.001	0.004	0.007
	(0.011)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)
INNSBRUCK	0.090**	0.074*	0.087**	0.049	0.079**	0.074*
	(0.028)	(0.031)	(0.030)	(0.030)	(0.031)	(0.031)
Permutation p-value LOW	0.0000	0.0002	0.0096	0.0045	0.0333	0.0002
Observations	350	359	348	359	360	355

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Heteroskedasticity-robust standard errors in parentheses.

As can be seen from the coefficient LOW in each specification of Table 2, the results on MLA-consistent behavior are in line with those presented in Figure 3 and Table B5 in the Appendix when we control for all elicited covariates. Specifically, for condition 250-100L9, which corresponds to the original setting by Gneezy and Potters (1997), Model (1) predicts that participants in treatment LOW invest on average 11.60 percentage points more in the lottery compared to their counterparts in treatment HIGH. Simply scaling the rates of return in the 25-10L9 condition, our regression predicts a difference in risk-taking between LOW and HIGH of only 7.00 percentage points and, additionally, switching to 30 instead of 9 periods corresponds to a predicted investment difference between both treatments of 5.90 percentage points. 25-10C30, the condition most closely mimicking realistic settings, produced the largest average gap in risk taking between treatments LOW and HIGH. Despite the fact that behavior consistent with MLA is a robust finding in our data, it appears that MLA is not equally pronounced in all conditions. Similarly, other studies modifying the design properties of Gneezy and Potters (1997) find attenuated evidence of MLA (see, e.g., Charness & Gneezy, 2010; Schwaiger & Hueber, 2021). Because of the low statistical power of most studies (see Figure 2), smaller true MLA effect sizes that might result from deviations from the original design of Gneezy and Potters (1997) would only be detected with a relatively low likelihood.

Furthermore, in most conditions, we observed a large and statistically significant association between gender and risk-taking behavior. Averaging coefficients of FEMALE across the six different models, we found that male participants invested 8.2 percentage points more in the lottery than female participants. This finding aligns with prior research indicating that men tend to take greater risks than women, particularly in financial contexts (Charness & Gneezy, 2012). Additionally, the results demonstrate that participants who identify themselves as more risk-seeking in financial matters invested higher amounts in the lottery, which can be seen from the statistically significant coefficient RISKTOLERANCE in all models of Table 2.[17] With respect to general risk-taking behavior, we found cohort effects. In particular, participants from Innsbruck were predicted to invest more in the risky lottery compared to students from the university in the Netherlands.

As a robustness check, we repeated the main analyses presented in Table 2 with the full sample. We show the results in Table B7 in the Appendix. The results remained robust and we observed the same qualitative patterns with respect to MLA in all conditions. In addition, we performed another pre-registered robustness check based on three questions we implemented in the experiment to test participants' comprehension of the investment task. Specifically, for the robustness check we excluded participants from the analysis who answered at least two of the three questions incorrectly and those who answered "did not understand at all" (after the decision task; see Appendix C). We present the results of this robustness check in Table B8 in the Appendix. Again, identical qualitative patterns emerged with respect to MLA-consistent behavior. In comparison to our final sample, MLA appears to be a marginally more pronounced characteristic among participants who demonstrated a better understanding of the task.

3.3 Multiverse Analysis of Main Results[18]

Typically, researchers enjoy a degree of freedom in choosing study populations, experimental designs, and analytical pathways for a given research question. Existing evidence has demonstrated marked variability in outcomes based on differences in these choices (Holzmeister et al., 2023; Landy et al., 2020; Menkveld et al., 2024; Simonsohn et al., 2020; Wicherts et al., 2016). Thus, the integrity of research findings, including those from pre-registered analyses, may still be influenced by the researcher's specific field of expertise or prior experience (Simmons et al., 2011). Such researcher degrees of freedom, particularly in the context of analytical heterogeneity, hold high relevance in empirical studies (Menkveld et al., 2024). This concept pertains to the discretion afforded to researchers in deciding upon data analysis methods, such as choosing specific statistical models, variables, or methods of interpretation. Such freedom can inadvertently introduce biases or lead to varying conclusions from the same dataset and given the same hypothesis.

To counter the challenges posed by analytical heterogeneity, multiverse analysis has emerged as a vital tool. Applying such analysis, researchers systematically explore all reasonable and non-redundant analytical choices for studying the same dataset and the same hypothesis, encompassing different combinations of statistical techniques,

[17] We conducted additional unreported and non-pre-registered exploratory analyses to test for heterogeneous treatment effects. These analyses showed a statistically significant interaction between RISKTOLERANCE and the LOW treatment in the 250-100L9 condition, indicating a stronger effect for participants with lower risk aversion. No such interaction was found in other conditions, nor was there heterogeneity based on gender, self-reported statistical knowledge, or investment experience.

[18] The analyses in this section were not pre-registered and are thus of exploratory nature.

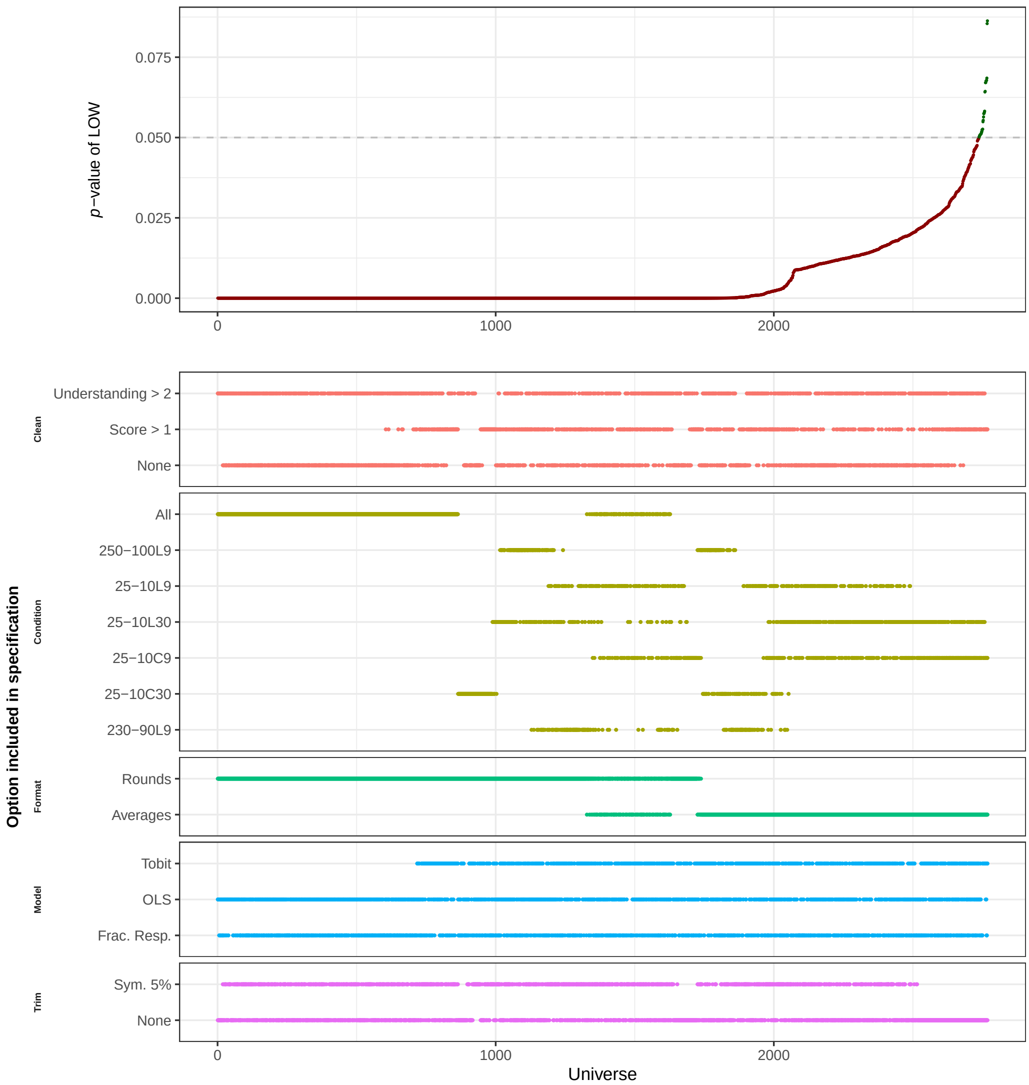

Figure 4: Multiverse analysis of main results. The upper panel illustrates the highest and lowest 5% of p-values of the coefficient LOW and a randomly drawn subset of 10% of p-values in between, out of the 13,824 analysis paths. The lower panel features the tested specification. For the purpose of illustration, in- or exclusion of each control variable has been left out in the lower panel.

variables, and model specifications. By examining the results across these numerous scenarios, researchers can identify how sensitive their findings are to different analytical decisions. This is referred to as “specification curve analysis” (Simonsohn et al., 2020). It effectively limits the flexibility in model selection that might otherwise align closely with a researcher’s preconceived hypotheses. The multiverse analysis approach not only bolsters the robustness and credibility of findings but also yields a more comprehensive understanding of the data.

This type of analysis necessitates a definition of multiple branches for both sample selection and model specification. Based on a thorough examination of the literature on MLA, we identified several key dimensions along which we varied our main analysis to ensure a comprehensive and unbiased analytical approach.[19] Next, we outline the various choices adopted in the literature linked to multiple dimensions, which we categorize as distinct branches within our multiverse analysis:

Cleaning: We considered (i) the full sample and (ii) applied exclusion criteria. To enhance data quality, we excluded participants demonstrating a lack of understanding of the task or poor response to straightforward questions about the lottery. In particular, we excluded participants with low self-stated understanding of the experiment ("Did not understand at all" or "I had quite some difficulties") and those who answered two out of three simple test questions incorrectly.[20]

Condition: We analyzed (i) pooled data from all conditions or (ii) tested MLA in each condition separately. In the pooled dataset, we also introduced a branch with dummy control variables for each condition (with 250-100L9 as the reference category).

Model: We utilized (i) the fractional response regression model when the investment was expressed in shares. Both (ii) Tobit and (iii) OLS regressions were applied for the percentage dependent variable format.

Data Format: We examined (i) individual investments averaged across all periods as well as (ii) panel (period-level) data with nine or thirty observations per participant. In panel data analyses, we additionally controlled for the period number.

Trimming: We considered (i) no trimming procedure and (ii) a symmetric cutoff below the 5^{th} and above the 95^{th} percentile of the individual time taken to complete the experiment.

Control Variables: All possible combinations of our control variables FEMALE, INVESTOR, RISKTOLERANCE, STAT.KNOWLEDGE, and INNSBRUCK, as well as a version without any control variables, were included in the analysis.

All combinations of these choices yielded a total of 13,824 specifications. To validate the robustness of our results to variations in analytical approaches, we expected at least 95% of all "LOW" coefficients to be statistically significant at the 5% level. Figure 4 displays the highest and lowest 5% of p-values among all applied analysis paths as well as an additional 10% of p-values randomly sampled from the remaining set. The upper panel demonstrates that all p-values fall beneath 0.08, encompassing a variety of different branch combinations, as portrayed in the lower panel. Specifically, our multiverse results indicated that in total, 13,793 specifications (99.78%) led to a statistically significant coefficient of LOW.[21] When we based our formal multiverse analysis of MLA on the median of all 13,824 p-values (Simonsohn et al., 2020), we found that increased risk-taking under low decision and feedback frequency (LOW) was highly statistically significant ($p < 0.001$). We thus conclude that behavior consistent with MLA withstands alterations to the sample composition and model specification within the realm of reasonable and non-redundant configurations informed by the existing MLA literature.

[19] Table B9 provides an overview of the different analytical paths in the related MLA studies adopting the Gneezy and Potters (1997) paradigm.

[20] The associated understanding and test questions are displayed in Appendix C.

[21] At a significance level of 1%, the coefficient of LOW was significant in 95.47% of all cases. Figure B3 illustrates the cumulative distribution of p-values for the coefficient LOW.

3.4 Determinants of the Magnitude of Myopic Loss Aversion[22]

Finally, we addressed the question of which factors determine the magnitude of behavior consistent with MLA, which appeared to be attenuated in most conditions compared to condition 250-100L9. Differences in MLA across conditions can be a result of either higher risk-taking in the HIGH group, lower risk-taking in the LOW group, or both. In non-pre-registered analyses, we explored this further by comparing investment allocations in groups HIGH and LOW in each condition to the behavior of their counterparts in condition 250-100L9—analagous to the original design by Gneezy and Potters (1997). The left panel of Figure 5 depicts coefficient plots based on the applied fractional response regressions (see Table 2) with treatments HIGH or LOW in condition 250-100L9 as reference groups. Participants in HIGH invested significantly higher amounts in conditions 25-10L9, 25-10C9, and 230-90L9 compared to their counterparts in 250-100L9. Although not statistically significant, in the other two conditions, we observed the opposite pattern (see 25-10L30 and 25-10C30). Investment amounts by participants in group LOW were lower only in condition 25-10L30 but higher in conditions 25-10L9 and 25-10C9 compared to group LOW in condition 250-100L9. Visible in the right panel of Figure 5, when benchmarked against the baseline setting by Gneezy and Potters (1997), the difference-in-difference MLA effect between conditions tends to be negative across all but two cases. However, these attenuations of MLA did not reach statistical significance, indicated by the 95% confidence intervals. Thus, our findings suggest that MLA was not statistically significantly less pronounced in settings different from the traditional Gneezy and Potters (1997) experiment. Given the inherent complexity in reliably detecting interaction effects, which typically necessitates a considerably larger sample size compared to detecting equivalent non-interaction effects, it is important to acknowledge that our study may lack sufficient power for small-to-medium difference-in-difference effects, despite our comparatively large sample sizes. Thus, in Figure 5 we also depict the results of equivalence tests based on the two one-sided tests (TOST) approach. This procedure provides a nuanced statistical method for establishing similarity of conditions by testing for equivalence with the null hypothesis. For our paper, we adopted a methodological approach that leverages coefficient plots of the difference-in-difference effects with 90% confidence intervals. Specifically, we used these plots to graphically indicate the effects we can rule out with high confidence—that is, any difference-in-difference effect sizes that fall outside the 90% confidence bounds of the effects can be considered statistically implausible based on our data. Comparing all conditions to the original study by Gneezy and Potters (1997), we could confidently rule out any differences exceeding a standardized effect size of approximately $d = 0.25$. However, we cannot confidently reject even smaller, possibly still economically significant, variations in MLA across conditions. Moreover, while individual conditions do not show significant differences in MLA behavior compared to 250-100L9, there may still be structural effects of investment horizon and compound returns on MLA magnitude. To test our non-pre-registered theoretical predictions of one-period versus three-period CPT evaluation of the risky asset, we conducted mixture model analyses with CPT as the underlying utility model, as shown in Table B11 in the Appendix.[23] Model (1), although approaching conventional levels of significance, indicates that the effect of narrow bracketing (w) on risk taking in treatment HIGH was not stronger than in LOW. Model (2), which aligns with the model proposed in subsection 2.2, shows that neither the investment horizon (h) nor the compounding of returns (c) significantly differ in their effect on risk taking between treatments, thus, have no effect on MLA. This is contrary to our predictions of $h_{HIGH} > h_{LOW}$ and $c_{HIGH} > c_{LOW}$. To further validate the non-significant effects of these dimensions on MLA, we conducted robustness checks by

[22] The analyses in this section are of exploratory nature and were not pre-registered.

[23] The CPT parameters used were the same as those discussed in section 2. We applied a sigmoid transformation of CPT values $\frac{1}{1+e^{-CPT}}$ to map these against average investment proportions.

examining potential interaction effects between the investment horizon and compounding of returns within the LOW treatment, specifically in conditions where the risky asset returns were reduced (25% and -10%). The main effects presented in Table B12 corroborate the results of our mixture model analysis and our main results, showing no significant effect of compounding returns or investment horizon on MLA.[24]

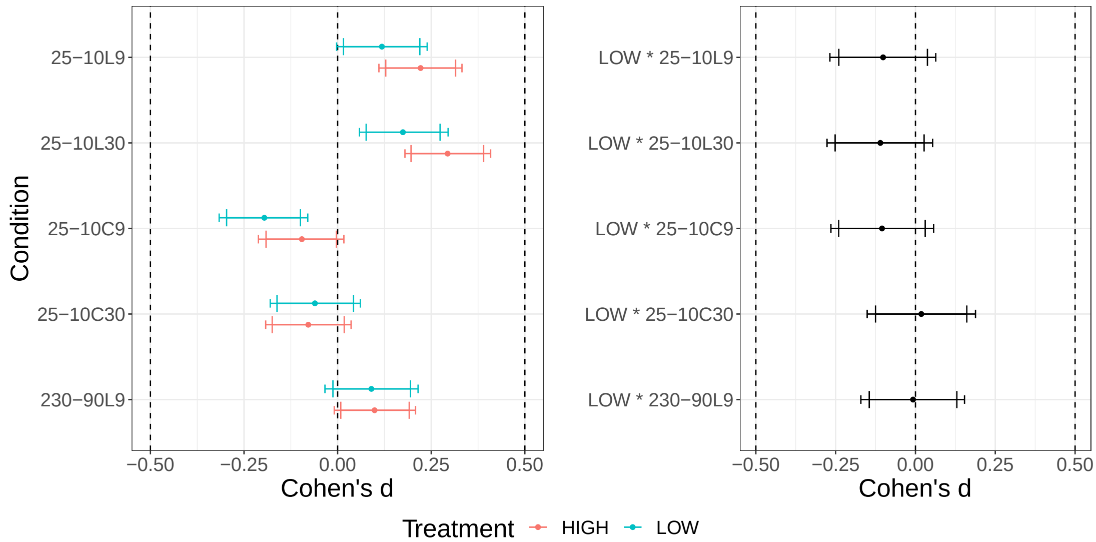

Figure 5: Forest plots of fractional response regression coefficients. Left panel: Condition effects separately in the HIGH and LOW treatments with the 250-100L9 condition as the reference category for both. Right panel: Difference-in-difference MLA effect in the full sample with 250-100L9 as the reference category. Bandwidths indicate 90% and 95% confidence intervals of estimated coefficients. The corresponding regression results are displayed in Table B10 in the Appendix.

4 Discussion

While previous studies have questioned the robustness of MLA in investment settings deviating from the very specific design of Gneezy and Potters (1997), they often lacked sufficient statistical power and/or did not systematically explore the underlying mechanisms that could mitigate behavior consistent with MLA. To bridge this gap, our statistically high-powered study allowed a detailed examination of factors that could influence MLA. In contrast to some earlier studies, our research revealed that MLA remains prevalent under all specifications that we tested.

Specifically, introducing scaled-down rates of return that resemble more those of annual stock return distributions did not change our findings, independent of return compounding. This result diverges from the second study in Beshears et al. (2017) who adopted identical (down-scaled) rates of return but tested return compounding simultaneously.

Furthermore, we hypothesized that return compounding in our setting may shift individuals' focus to final investment outcomes, potentially mitigating the effects of high evaluation and decision frequency. Klos (2013)'s findings, in which MLA tendencies were significantly reduced after eliciting participants' total lottery return

24 While the results suggested a general reduction in risk-taking (measured as a percentage of the current balance) under conditions with compound returns, this did not translate into an effect on MLA.

expectations, seemed to support this hypothesis. However, under return compounding, MLA persisted, consistent with Langer and Weber (2008). Our analysis indicated, however, overall lower levels of risk-taking under compounding compared to linear return scenarios (see Table B12).

Finally, our results revealed that MLA is resilient to variations in the investment horizon. Longer planning horizons are typically associated with increased investment risk-taking (see, e.g., Anderson & Settle, 1996; Dierkes et al., 2010). In our experiment, additional periods allowed participants to experience the long-term dynamics of the risky asset, enabling better understanding of the underlying return distribution. However, it remained uncertain whether this effect is stronger for participants in HIGH compared to those in LOW, as our model hypothesized. In our study, risk-taking seemed to have increased almost proportionally in HIGH and LOW compared to the corresponding conditions with 9 periods (see Figure 3). Following the relative risk argument by Looney and Hardin (2020), it seems conceivable that risk-taking over longer horizons increases further in linear return conditions, but it does not explain the similar increases in risk-taking in HIGH and LOW over time in the compound return conditions (see Figure B2). Instead, such development could potentially be explained by wealth effects.

Overall, our findings provide robust evidence that behavior consistent with MLA persists, even when returns are down-scaled, across both linear and compound returns, as well as for both shorter and extended investment periods. In contrast to many studies on MLA, we conducted our study online (participants used their home computers) and not in a lab, so that this experimental feature can also be ruled out to have caused recent insignificant results. As a consequence, the differences from the main study of Beshears et al. (2017) are likely to stem from other causes. We identify the following potential reasons to be most likely for their insignificant result: i.) the smaller changes in return probability distributions between the treatments as they tested one week versus half a year instead of the larger difference in Gneezy and Potters (1997), ii.) offering four investment options (with real names) instead of asking for an investment allocation between a risky and a safe asset, iii.) asking participants to make decisions on a weekly or semi-annual basis, rather than consecutively one after another, and iv.) interaction effects between the four different interventions they tested simultaneously. Such interaction effects could mitigate MLA, necessitating even more statistical power to reliably detect the underlying true effect. Further differences include paying higher incentives ($325 per participant), paying extra incentives for logging in to the experimental platform, and non-student participants. Although we cannot rule out whether any of these additional factors significantly drove their results, we believe that it is unlikely given the related literature.[25] In contrast, our findings rule out commonly assumed explanations and demonstrate the robustness of MLA, extending well beyond the specific research designs often replicated due to path dependence.

5 Conclusion

Following the influential work of Benartzi and Thaler (1995) and Gneezy and Potters (1997), the academic community has investigated the applicability of the theory of myopic loss aversion (MLA) in various settings. Previous research has extensively discussed the real-world implications of MLA, and its importance has been mentioned in many popular media outlets and investment websites. For example, a search within the News on the Web corpus yields 47 entries related to the concept of MLA. In contrast, other widely recognized behavioral

[25] For instance, Hackethal et al. (2023) found that monetary incentives do not significantly alter elicited risk aversion in conventional risk preference elicitation tasks.

concepts such as regret aversion yield a comparatively smaller number of results (NOW Corpus, 2024). Recently, however, as the focus on replicability (Camerer et al., 2018; Huber et al., 2023) and reproducibility (Menkveld et al., 2024) of economic experiments has intensified, there has been a growing discourse on the robustness and generalizability of MLA, notably propelled by the contribution of Beshears et al. (2017)'s work, which suggested that evidence for MLA could be confined to a narrow range of experimental designs. Previous studies attempting to modify the characteristics of the experimental design in this context often encountered challenges related to statistical power or lacked a refined design to clearly dissect the factors influencing MLA-consistent behavior. This left a void in our understanding of MLA's robustness and drivers.

Given both the number of studies and the mentioned practical consequences of MLA, our research endeavored to fill this gap by rigorously examining the resilience of MLA to broader settings. We isolated the effects of more realistic rates of return, commonly used investment procedures featuring return compounding, and a longer investment horizon in the established Gneezy and Potters (1997) experimental design across six between-subject conditions. Specifically, MLA persisted in various modified conditions: when the possibility of a total loss was reduced, when return rates were scaled down to a fraction of the original rates under both compound returns with a dynamic endowment balance, and under linear returns with a period-by-period endowment. The down-scaling of returns did not mitigate MLA under extended investment horizons either, for both compound and linear returns. We further validated our results by a multiverse analysis, which confirmed MLA's stability by effectively addressing concerns about analytical researcher degrees of freedom. We found no evidence of interaction between these alterations and the magnitude of MLA and could confidently rule out standardized differences in MLA larger than $d = 0.25$ compared to the original design. We conclude that the non-replication of MLA by Beshears et al. (2017) in their second aggregation experiment is likely the result of a false negative and insufficient statistical power. In their main (first) study, they also did not find behavior consistent with MLA, however, that study also differed from ours in many other aspects (see Discussion).

Our study provides evidence that MLA constitutes a persistent behavioral pattern with significant implications for individual investment decisions. This finding is consistent with studies utilizing field data (Larson et al., 2016) and those reevaluating MLA in less abstract lab-in-the-field environments (Iqbal et al., 2021). Our results highlight challenges in communicating financial asset risks, exacerbated by technological advancements that reinforce short-sighted decisions due to rapid information transmission and stimulus overload (see, e.g., Borsboom et al., 2022; Kalda et al., 2021). This is especially relevant in retirement and long-term savings decisions. Policies that encourage broader bracketing of investment outcomes, such as extended return horizon disclosure (Shaton, 2017) or aggregated, comprehensive performance disclosure (Gerhard et al., 2017), could mitigate the adverse effects of myopic decision-making. Financial literacy education also plays a crucial role in emphasizing long-term planning over reactive, short-term decisions. In addition, organizational and regulatory frameworks could incentivize long-term investments through tax benefits, tiered products, or loyalty bonuses, thus reducing myopia and improving market stability (Bolton & Samama, 2013; Davies et al., 2014).

While our study contributes important insights, it also opens new paths for future examination. Future research could investigate whether other variables, such as outcome probabilities of risky assets (see, e.g., Schwaiger & Hueber, 2021), higher moments (see, e.g., Haisley et al., 2008), or longer times delays between investment decisions, play a systematic role in influencing MLA tendencies. Such studies are vital to allow us to fully comprehend the factors driving MLA.

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Appendices

for Online Publication

The Consequences of Narrow Framing for Risk-Taking:

A Stress Test of Myopic Loss Aversion

A Perceptions

Aligned with the methodology of Venkatraman et al. (2006), we conducted a post-investment game survey in Wave 3 (University of Innsbruck) to explore psychological factors and perceptions related to risk-taking behaviors. While such variables are typically integral to risk-taking analysis, our study specifically aims to understand the dynamics of MLA and how variations in these factors might contribute to different levels of risk-taking between the groups HIGH and LOW across various conditions. We hypothesize that frequent evaluations and decision-making lead to a heightened perception of loss magnitude and likelihood, thus promoting a decrease in risk-taking. Furthermore, this process may also affect individual emotions, potentially increasing worry and diminishing satisfaction with investment choices.

Figure A1 displays the average scores for the key variables—risk perception, worry, satisfaction, perceived loss probability, and perceived loss magnitude—under the different experimental conditions. The results of independent-sample t-tests regarding these key variables between HIGH and LOW in each condition are outlined in Table A1. Notably, the anticipated disparities were evident primarily in the 230-90L9 condition, where worry and perceived loss probability were significantly higher in the HIGH group compared to the LOW group. Conversely, satisfaction levels were higher in the LOW group. This latter pattern also held for the entire sample, encompassing all conditions. Interestingly, despite higher investments, participants in group LOW perceived greater losses and loss likelihoods than those in HIGH under the 25-10C30 condition. This observation is particularly striking in the context of a long-horizon multiplicative setting, suggesting that participants might feel more confident in overcoming short-term losses due to the potential for cumulative gains over time leading to higher risk-taking.

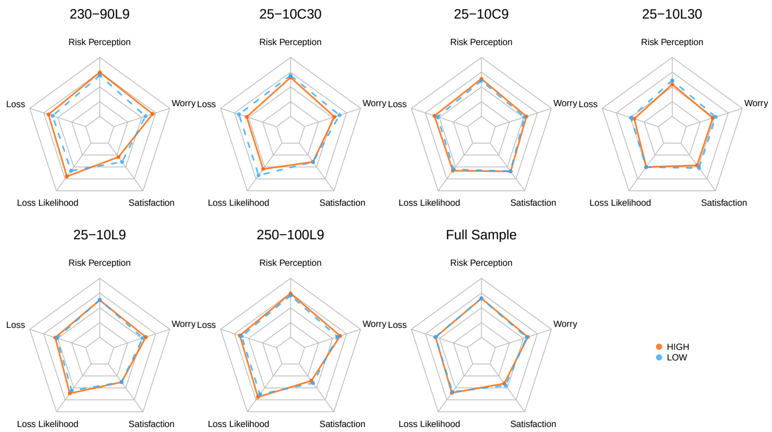


Figure A1: Radar charts of mean scores on perception variables. Each variable is measured by means of subject assessment on a seven-item Likert scale. The corresponding t-test results are displayed in Table A1.

Table A1: T-tests of perception differences between treatments. The table shows mean differences of selected perceptions variables in HIGH- LOW in each condition using independent-samples t-tests.

Condition	N	Risk Perception	Worry	Satisfaction	Loss Probability	Loss
250-100L9	160	0.21	0.30	-0.29	0.35	0.19
230-90L9	166	0.31	0.77**	-0.61*	0.70*	0.44
25-10L9	172	-0.04	0.33	0.01	0.35	0.18
25-10L30	157	-0.40	-0.31	-0.34	-0.01	-0.34
25-10C9	157	0.19	0.21	0.03	0.16	0.34
25-10C30	170	-0.20	-0.54	-0.003	-0.78**	-0.83**
Full Sample	982	0.01	0.12	-0.24**	0.12	-0.02

Note: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

B Additional Tables & Figures

Table B1: Final sample randomization checks. Each row represents a randomization metric of the distribution of covariates between HIGH and LOW treatments for a given variable within each of the six conditions. FEMALE is a binary dummy variable that takes the value of 0 for male participants and the value of 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants on Likert scales from 0 to 10. STAT.KNOWLEDGE represents self-assessed statistical knowledge from 1 to 7. INNSBRUCK is a binary dummy that takes the value of 0 for participants from the Radboud University and the value of 1 for participants from the University of Innsbruck.

Condition	Variable	Test	Value	N
250-100L9	FEMALE	chi2 Test	0.011	347
250-100L9	INVESTOR	chi2 Test	0.566	347
250-100L9	RISKTOLERANCE	Kruskal-Wallis Test	0.119	347
250-100L9	STAT.KNOWLEDGE	Kruskal-Wallis Test	6.498	347
250-100L9	INNSBRUCK	chi2 Test	1.151	347
25-10L9	FEMALE	chi2 Test	2.248	346
25-10L9	INVESTOR	chi2 Test	0.277	346
25-10L9	RISKTOLERANCE	Kruskal-Wallis Test	0.798	346
25-10L9	STAT.KNOWLEDGE	Kruskal-Wallis Test	0.43	346
25-10L9	INNSBRUCK	chi2 Test	2.307	346
25-10C9	FEMALE	chi2 Test	0.328	359
25-10C9	INVESTOR	chi2 Test	2.009	359
25-10C9	RISKTOLERANCE	Kruskal-Wallis Test	0.217	359
25-10C9	STAT.KNOWLEDGE	Kruskal-Wallis Test	0.169	359
25-10C9	INNSBRUCK	chi2 Test	2.121	359
230-90L9	FEMALE	chi2 Test	0.07	358
230-90L9	INVESTOR	chi2 Test	0.25	358
230-90L9	RISKTOLERANCE	Kruskal-Wallis Test	0.371	358
230-90L9	STAT.KNOWLEDGE	Kruskal-Wallis Test	0.051	358
230-90L9	INNSBRUCK	chi2 Test	0.224	358
25-10L30	FEMALE	chi2 Test	0.045	359
25-10L30	INVESTOR	chi2 Test	1.899	359
25-10L30	RISKTOLERANCE	Kruskal-Wallis Test	2.612	359
25-10L30	STAT.KNOWLEDGE	Kruskal-Wallis Test	0.879	359
25-10L30	INNSBRUCK	chi2 Test	0.402	359
25-10C30	FEMALE	chi2 Test	0.005	348
25-10C30	INVESTOR	chi2 Test	1.691	348
25-10C30	RISKTOLERANCE	Kruskal-Wallis Test	0.038	348
25-10C30	STAT.KNOWLEDGE	Kruskal-Wallis Test	0.041	348
25-10C30	INNSBRUCK	chi2 Test	0.002	348

Table B2: Final sample randomization checks between treatments. Standardized mean difference scores (SMD) of means in HIGH minus LOW close to zero represent balanced sample characteristics.FEMALE is a binary dummy variable that takes the value of 0 for male participants and the value of 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants on Likert scales from 0 to 10. STAT.KNOWLEDGE represents self-assessed statistical knowledge from 1 to 7. INNSBRUCK is a binary dummy that takes the value of 0 for participants from Radboud University and the value of 1 for participants from the University of Innsbruck.

Condition	Variable	SMD	N
250-100L9	FEMALE	-0.011	347
250-100L9	INVESTOR	0.08	347
250-100L9	RISKTOLERANCE	0.026	347
250-100L9	STAT.KNOWLEDGE	0.245	347
250-100L9	INNSBRUCK	-0.115	347
25-10L9	FEMALE	0.161	346
25-10L9	INVESTOR	0.056	346
25-10L9	RISKTOLERANCE	-0.099	346
25-10L9	STAT.KNOWLEDGE	-0.049	346
25-10L9	INNSBRUCK	0.163	346
25-10C9	FEMALE	-0.061	359
25-10C9	INVESTOR	0.15	359
25-10C9	RISKTOLERANCE	0.037	359
25-10C9	STAT.KNOWLEDGE	0.07	359
25-10C9	INNSBRUCK	-0.154	359
230-90L9	FEMALE	0.028	358
230-90L9	INVESTOR	0.053	358
230-90L9	RISKTOLERANCE	0.089	358
230-90L9	STAT.KNOWLEDGE	-0.004	358
230-90L9	INNSBRUCK	-0.05	358
25-10L30	FEMALE	0.022	359
25-10L30	INVESTOR	0.146	359
25-10L30	RISKTOLERANCE	-0.157	359
25-10L30	STAT.KNOWLEDGE	-0.079	359
25-10L30	INNSBRUCK	-0.067	359
25-10C30	FEMALE	0.007	348
25-10C30	INVESTOR	0.138	348
25-10C30	RISKTOLERANCE	-0.002	348
25-10C30	STAT.KNOWLEDGE	0.014	348
25-10C30	INNSBRUCK	0.004	348

Table B3: Final sample randomization checks between waves. Standardized mean difference scores (SMD) of means close to zero represent balanced sample characteristics. FEMALE is a binary dummy variable that takes the value of 0 for male participants and the value of 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants on Likert scales from 0 to 10. STAT.KNOWLEDGE represents self-assessed statistical knowledge from 1 to 7. INNSBRUCK is a binary dummy that takes the value of 0 for participants from Radboud University and the value of 1 for participants from the University of Innsbruck.

Difference	Variable	SMD	N
Wave 1 - Wave 2	FEMALE	0.079	1149
Wave 1 - Wave 2	INVESTOR	0.098	1149
Wave 1 - Wave 2	RISKTOLERANCE	-0.047	1149
Wave 1 - Wave 2	STAT.KNOWLEDGE	-0.021	1149
Wave 1 - Wave 3	FEMALE	-0.573	1565
Wave 1 - Wave 3	INVESTOR	0.073	1565
Wave 1 - Wave 3	RISKTOLERANCE	0.504	1565
Wave 1 - Wave 3	STAT.KNOWLEDGE	0.021	1565
Wave 2 - Wave 3	FEMALE	-0.66	1548
Wave 2 - Wave 3	INVESTOR	0.025	1548
Wave 2 - Wave 3	RISKTOLERANCE	0.53	1548
Wave 2 - Wave 3	STAT.KNOWLEDGE	0.04	1548
RU - UoI	FEMALE	-0.615	2131
RU - UoI	INVESTOR	0.025	2131
RU - UoI	RISKTOLERANCE	0.517	2131
RU - UoI	STAT.KNOWLEDGE	0.03	2131

Table B4: Overview of previous MLA studies in chronological order. Selected studies use the between-subjects Gneezy and Potters (1997) investment experiment with a binary-outcome risky asset and the same exogenous manipulation of myopia between HIGH and LOW. Columns 1–4 indicate how the original experiment is modified (blank indicates no modification compared to Gneezy and Potters (1997)). Sample Size Per Cond. indicates the rounded average number of participants in each condition HIGH/LOW. Total Sample Size refers to the total number of recruited participants in exogenous MLA conditions.

Study	Rates of Return	Probabilities	Earnings	Horizon	Sample Size Per Cond.	Total Sample Size	Cohen's d
Gneezy and Potters (1997)	+250% / -100%	0.33 / 0.67	Linear	9 Periods	42	84	0.64
Langer and Weber (2005)	+30(15)% / -100%	0.1 / 0.9			17	35	0.57[a]
Bellemare et al. (2005)					29	88	0.35
Haigh and List (2005)					30	118	0.4
Langer and Weber (2008)	+7% / -3%	0.4 / 0.6	Compound	30 Periods	27	54	NA[b]
Fellner and Sutter (2009)				18 Periods	30	118[c]	0.72[d]
Hardin and Looney (2012)				30 Periods	31	622	0.67[e]
Zeisberger et al. (2012)	+230(190)% / -100%	0.4 / 0.6	Compound	36 Periods	48	190	NA[f]
Beshears et al. (2017)	+25% / -10%		Compound		40	320	0.14
Durand et al. (2019)					64	128	0.43
Schwaiger and Hueber (2021)		0.5 / 0.5			234	937	0.15
Hueber and Schwaiger (2022)					224	894	0.28
This study	+25% / -10%		Compound	30 Periods	187	2,245	0.45

[a] Cohen's d was estimated based on the study's reported one-sided p-value of < 0.05. A conservative p-value of 0.049 was assumed to calculate the t-value, which was then used to estimate the pooled standard deviation. This cautious approach ensures the effect size is not overestimated.

[b] Coming up with a comparable treatment effect was not possible. There was no direct comparison between low versus high feedback and decision frequency; instead, a 2×2 analysis along these dimensions was conducted.

[c] Including endogenous-choice treatments, their study comprises a total sample of 444 participants.

[d] Cohen's d was estimated by first converting the a p-value of 0.009 (conservative approach as the paper reported $p < 0.01$) from a two-sided Mann-Whitney U test to a z-score. We then calculated the rank-biserial correlation r_b and transformed it into Cohen's d using the formula $d = \frac{2r_b}{\sqrt{1-r_b^2}}$. This allows for a comparable effect size measure from the non-parametric test.

[e] Cohen's d was calculated by first deriving the standard deviations from the reported standard errors of the means (SEMs) using the formula $\text{SD} = \text{SEM} \times \sqrt{n}$. These standard deviations were then pooled to come up with Cohen's d.

[f] Coming up with a comparable treatment effect was not possible. Results were reported for 36 rounds but not for 9 rounds

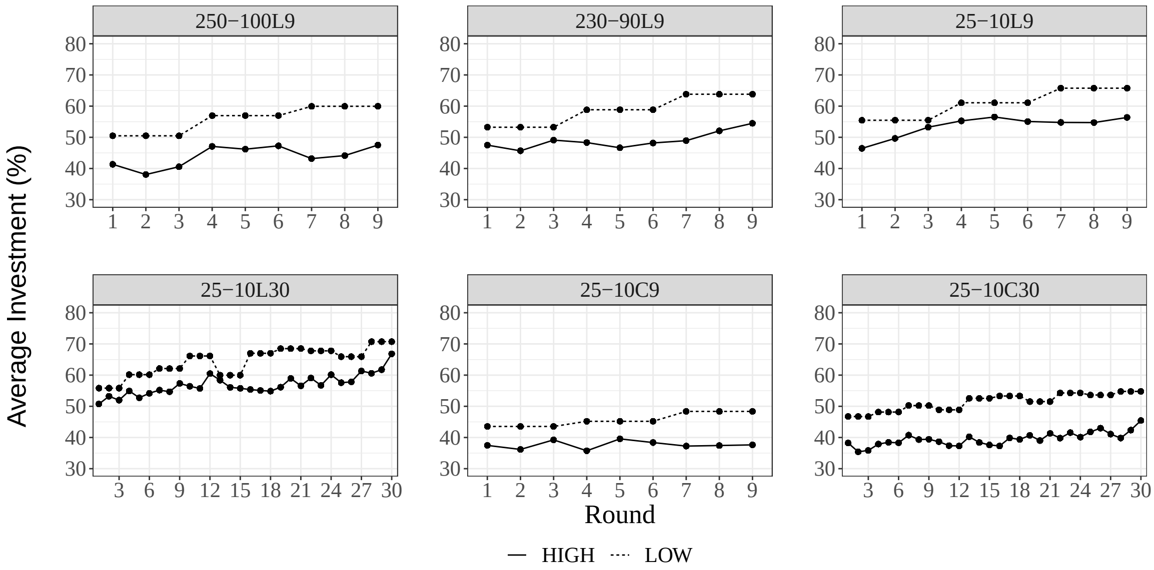


Figure B2: Round-level average investment percentages between treatments HIGH and LOW across different conditions. Conditions 250-100L9, 230-90L9, 25-10L9, and 25-10C9 display the development of average investments over nine periods. 25-10L30 and 25-10C30 display thirty-period developments of average investments.

Table B5: T-tests of differences between treatments. The table shows mean differences of investment amounts in HIGH and LOW in each condition using independent-samples t-tests. The last column indicates the p-value results of a permutation (asymptotic general independence) test.

Condition	N	Mean diff. (H - L)	lower 95% conf. int.	upper 95% conf. int.	t-stat	Std. Error	p	Perm. test p
250-100L9	350	-11.505***	−16.855	−6.156	−4.231	2.719	0.000	0.000
230-90L9	360	-9.913***	−15.615	−4.211	−3.419	2.899	0.001	0.001
25-10L9	359	-7.218*	−12.842	−1.594	−2.525	2.859	0.012	0.012
25-10C9	348	-7.753**	−13.420	−2.085	−2.690	2.882	0.007	0.008
25-10L30	359	-7.640**	−13.105	−2.174	−2.749	2.779	0.006	0.006
25-10C30	355	-12.029***	−17.960	−6.099	−3.990	3.015	0.000	0.000

Note: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. 95%-confidence interval in parentheses.

Table B6: Average marginal effects fractional response models with logit links and the amount invested per period in percent of the endowment as dependent variables. The binary dummy variable LOW is coded 0 for participants in the HIGH treatment and 1 for those in the LOW treatment. FEMALE is a binary dummy variable that takes the value of 0 for male participants and the value of 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants on Likert scales from 0 to 10. STAT.KNOWLEDGE represents participants' self reported statistical knowledge compared to their fellow students on a 7-point scale. INNSBRUCK is a binary dummy that takes the value of 0 for participants from Radboud University and the value of 1 for participants from the University of Innsbruck.

	Dependent variable: Investment (%)					
	Conditions:					
	250-100L9	230-90L9	25-10L9	25-10C9	25-10L30	25-10C30
	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
LOW	0.121***	0.100***	0.070***	0.081***	0.058***	0.117***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.007)	(0.006)
FEMALE	−0.091***	−0.055***	−0.042***	−0.106***	−0.061***	−0.143***
	(0.012)	(0.014)	(0.012)	(0.013)	(0.008)	(0.008)
INVESTOR	−0.014	−0.013	0.016	−0.023	0.070***	−0.025***
	(0.012)	(0.013)	(0.013)	(0.012)	(0.007)	(0.008)
RISKTOLERANCE	0.040***	0.033***	0.019***	0.033***	0.026***	0.031***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
STAT.KNOWLEDGE	0.013**	0.008	0.031***	−0.0004	0.003	0.007**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
INNSBRUCK	0.081***	0.082***	0.086***	0.050***	0.080***	0.076***
	(0.012)	(0.012)	(0.012)	(0.011)	(0.007)	(0.007)
Observations	3,042	3,159	3,096	3,177	10,680	10,590

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Clustered standard errors at the subject level in parentheses.

Table B7: Full Sample average marginal effects fractional response models with logit links and the amount invested in percent of the endowment as dependent variables. The binary dummy variable LOW is coded 0 for participants in the HIGH treatment and 1 for those in the LOW treatment. FEMALE is a binary dummy variable that takes the value of 0 for male participants and the value of 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants on Likert scales from 0 to 10. STAT.KNOWLEDGE represents participants' self reported statistical knowledge compared to their fellow students on a 7-point scale. INNSBRUCK is a binary dummy that takes the value of 0 for participants from Radboud University and the value of 1 for participants from the University of Innsbruck.

	Dependent variable: Investment (%)					
	Conditions:					
	250-100L9	230-90L9	25-10L9	25-10C9	25-10L30	25-10C30
	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
LOW	0.111***	0.108***	0.072**	0.066*	0.058*	0.117***
	(0.025)	(0.027)	(0.027)	(0.027)	(0.026)	(0.028)
FEMALE	−0.100***	−0.059	−0.023	−0.081*	−0.056	−0.153***
	(0.027)	(0.034)	(0.029)	(0.033)	(0.031)	(0.035)
INVESTOR	−0.014	−0.015	0.005	−0.014	0.068*	−0.025
	(0.027)	(0.033)	(0.031)	(0.032)	(0.030)	(0.033)
RISKTOLERANCE	0.038***	0.033***	0.020*	0.038***	0.027***	0.032***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)
STAT.KNOWLEDGE	0.012	0.009	0.031*	−0.002	0.005	0.008
	(0.011)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)
INNSBRUCK	0.101***	0.080**	0.093**	0.040	0.077*	0.070*
	(0.027)	(0.030)	(0.030)	(0.029)	(0.030)	(0.030)
Observations	380	370	375	383	369	368

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Heteroskedasticity-robust standard errors in parentheses.

Table B8: Robustness sample average marginal effects fractional response models with logit links and the amount invested in percent of the endowment as dependent variables. The binary dummy variable LOW is coded 0 for participants in the HIGH treatment and 1 for those in the LOW treatment. FEMALE is a binary dummy variable that takes the value of 0 for male participants and the value of 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants on Likert scales from 0 to 10. STAT.KNOWLEDGE represents participants' self reported statistical knowledge compared to their fellow students on a 7-point scale. INNSBRUCK is a binary dummy that takes the value of 0 for participants from Radboud University and the value of 1 for participants from the University of Innsbruck.

	Dependent variable: Investment (%)					
	Conditions:					
	250-100L9	230-90L9	25-10L9	25-10C9	25-10L30	25-10C30
	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
LOW	0.128***	0.117***	0.102**	0.097**	0.064*	0.117***
	(0.029)	(0.031)	(0.032)	(0.035)	(0.032)	(0.034)
FEMALE	−0.102**	−0.014	−0.069*	−0.122**	−0.055	−0.152***
	(0.034)	(0.039)	(0.034)	(0.044)	(0.038)	(0.042)
INVESTOR	−0.011	−0.012	0.050	−0.041	0.062	−0.058
	(0.030)	(0.036)	(0.037)	(0.040)	(0.035)	(0.039)
RISKTOLERANCE	0.040***	0.039***	0.019*	0.032**	0.029***	0.035***
	(0.009)	(0.008)	(0.009)	(0.010)	(0.009)	(0.009)
STAT.KNOWLEDGE	0.008	0.018	0.029	0.009	0.0003	0.014
	(0.014)	(0.015)	(0.015)	(0.017)	(0.014)	(0.015)
INNSBRUCK	0.095**	0.078*	0.098**	0.020	0.058	0.089*
	(0.032)	(0.035)	(0.034)	(0.040)	(0.037)	(0.038)
Observations	288	286	258	239	281	262

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Heteroskedasticity-robust standard errors in parentheses.

Table B9: Overview of analytical heterogeneity across previous MLA studies in chronological order. Selected studies use the between-subjects Gneezy and Potters (1997) investment experiment with a binary-outcome risky asset and the same exogenous manipulation of myopia between HIGH and LOW. For an overview of the different conditions in the experimental design, see Table B4.

Study	Dependent Variable	Model	Data Format	Cleaning/Trimming	Controls
Gneezy and Potters (1997)	Absolute & relative	Mann-Whitney test	Periods & averages	No	No
Langer and Weber (2005)	Relative	Mann-Whitney test	Averages	No	No
Bellemare et al. (2005)	Absolute & relative	Mann-Whitney test	Periods & averages	No	No
Haigh and List (2005)	Absolute	Tobit regression	Periods	No	No
Langer and Weber (2008)	Relative	Mann-Whitney test	Averages	No	No
Fellner and Sutter (2009)	Absolute	Tobit Regression	Periods	No	Yes
Hardin and Looney (2012)	Relative	Linear mixed modelling	Averages	No	Yes
Zeisberger et al. (2012)	Absolute	Mann-Whitney test	Periods & averages	No	No
Beshears et al. (2017)	Relative	OLS regression	Averages	No	No
Durand et al. (2019)	Absolute	Tobit regression	Periods	No	Yes
Schwaiger and Hueber (2021)	Relative	Tobit regression	Averages	Yes	Yes
Hueber and Schwaiger (2022)	Relative	Tobit regression	Averages	Yes	Yes

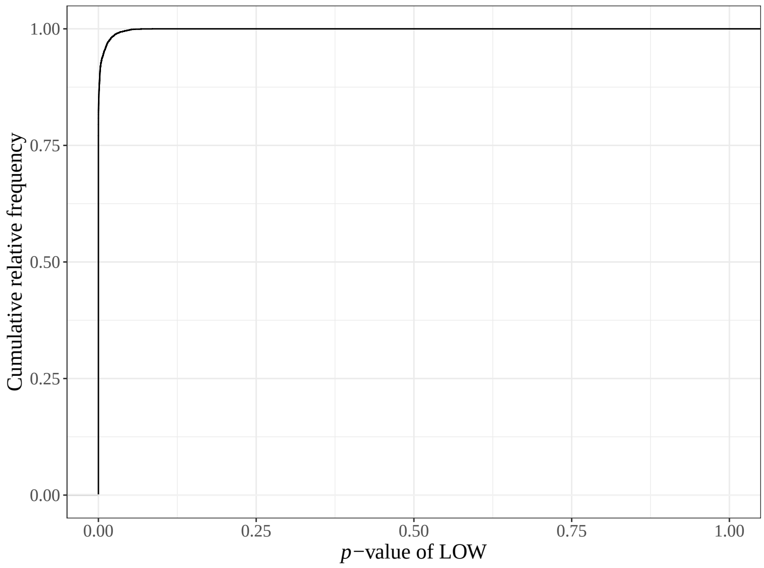


Figure B3: Cumulative distribution of p-values in multiverse analysis. The figure displays the cumulative relative frequency of p-values of LOW from all regressions in our multiverse analysis. The multiverse analysis is based on 13,824 regressions featuring different analytical choices as outlined in section 3.

Table B10: Condition effects fractional response models with logit links and the amount invested in percent of the endowment as dependent variables. The binary dummy variable LOW is coded 0 for participants in the HIGH treatment and 1 for those in the LOW treatment. FEMALE is a binary dummy variable that takes the value of 0 for male participants and the value of 1 for female participants. INVESTOR is a dummy variable that equals 1 if participants have already invested in financial products. RISKTOLERANCE indicates the self-reported risk preferences of participants on Likert scales from 0 to 10. STAT.KNOWLEDGE represents participants' self reported statistical knowledge compared to their fellow students on a 7-point scale. INNSBRUCK is a binary dummy that takes the value of 0 for participants from Radboud University and the value of 1 for participants from the University of Innsbruck.

	Dependent variable: Investment (%)		
	HIGH	LOW	Full
LOW			0.484***
			(0.115)
230-90L9	0.211	0.161	0.207
	(0.110)	(0.119)	(0.114)
25-10L9	0.453***	0.247*	0.447***
	(0.111)	(0.121)	(0.115)
25-10L30	0.542***	0.313**	0.536***
	(0.110)	(0.121)	(0.114)
25-10C9	−0.197	−0.341**	−0.195
	(0.111)	(0.119)	(0.116)
25-10C30	−0.148	−0.115	−0.152
	(0.111)	(0.118)	(0.116)
FEMALE	−0.277***	−0.410***	−0.344***
	(0.075)	(0.081)	(0.055)
INVESTOR	−0.022	0.052	0.014
	(0.071)	(0.078)	(0.053)
RISKTOLERANCE	0.113***	0.152***	0.131***
	(0.017)	(0.019)	(0.013)
STAT.KNOWLEDGE	0.068*	0.012	0.043*
	(0.027)	(0.031)	(0.020)
INNSBRUCK	0.190**	0.461***	0.321***
	(0.069)	(0.076)	(0.051)
LOW * 230-90L9			−0.056
			(0.162)
LOW * 25-10L9			−0.208
			(0.163)
LOW * 25-10L30			−0.216
			(0.163)
LOW * 25-10C9			−0.152
			(0.162)
LOW * 25-10C30			0.025
			(0.162)
Observations	1,063	1,068	2,131

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses.

Table B11: Mixture model results of exogenous (Model (1)) and endogenous (Model (2)) narrow bracketing tendency and their effects on MLA. Columns HIGH and LOW depict parameter estimates of the model. The standardized parameter difference Z (HIGH minus LOW) and their associated p-values are displayed in Columns 3 and 4.

		Dependent variable: Investment Fraction (0-1)			
		HIGH	LOW	Wald test Z-value	p-value
Model (1)	Narrow bracketing (w)	0.790	0.694	1.840	0.066
		(0.035)	(0.039)		
	Log-Likelihood	-1,017.453	-1,029.348		
Model (2)	Horizon (h)	0.027	0.083	−0.472	0.637
		(0.031)	(0.115)		
	Compound returns (c)	−0.590	−0.315	−0.638	0.523
		(0.311)	(0.296)		
	Constant (a)	1.000	1.478	−0.307	0.759
		(0.287)	(1.529)		
	Log-Likelihood	-1,015.496	-1,027.358		

Standard errors in parentheses.

Table B12: Horizon and compounding effects fractional response models with logit links and the amount invested in percent of the endowment as dependent variables. The sample includes only conditions with reduced lottery returns of 25% and -10%. The binary dummy variable LOW is coded 0 for participants in the HIGH treatment and 1 for those in the LOW treatment. Long Horizon (LONG HORIZON) is a dummy indicating whether the condition features a short (0) or long investment horizon (1). COMPOUND RETURNS is coded 1 for participants in conditions with compound returns, and 0 otherwise.

	Dependent variable: Investment (%)			
	Model (1)	Model (2)	Model (3)	Model (4)
LOW	0.391***	0.308***	0.357***	0.321**
	(0.086)	(0.083)	(0.102)	(0.118)
LONG HORIZON	−0.107		−0.102	−0.139
	(0.084)		(0.083)	(0.113)
LOW * LONG HORIZON	−0.095		−0.098	−0.026
	(0.121)		(0.120)	(0.167)
COMPOUND RETURNS		−0.667***	−0.667***	−0.704***
		(0.083)	(0.083)	(0.119)
LOW * COMPOUND RETURNS		0.095	0.095	0.166
		(0.119)	(0.120)	(0.171)
LONG HORIZON * COMPOUND RETURNS				0.075
				(0.166)
LOW * LONG HORIZON * COMPOUND RETURNS				−0.142
				(0.239)
Observations	1,422	1,422	1,422	1,422

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Heteroskedasticity-robust standard errors in parentheses.

C Instructions

The complete experimental instructions for our baseline condition 250-100L9 are shown in subsection C.1. Headings distinguish alternative pages for participants in HIGH or LOW (instructions, decisions and feedback). subsection C.2 displays the experimental instructions in all other conditions for HIGH and LOW, respectively.

C.1 250-100L9

Thank you for participating in this study.

This is part of a research investigation conducted by the University of Innsbruck (Austria), the University of Zurich (Switzerland) and Radboud University (the Netherlands), and was reviewed and approved by the University of Zurich's institutional review board for ethical issues. For any questions contact stefan.zeisberger@ru.nl.

As part of this study, we will ask you to play an investment game and answer some questions. We expect this to take you approximately 12 minutes. **Depending on your decisions in the experiment you will be financially compensated for taking part.** We will provide further details and instructions in the relevant sections.

We ask you to complete all the sections and not to close the browser before reaching the end. Please read the instructions carefully. **Note that you will only be paid for your participation if you complete the task and survey in full.** Information about your final payoff will be provided at the end of the task.

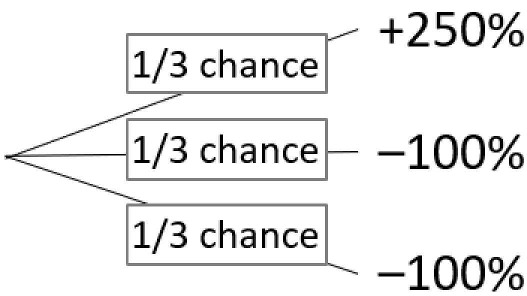
By taking part in this study, you acknowledge that your participation is voluntary and that your responses, including basic demographic information will be saved, but no identifiable personal data will be stored, except for payment purposes. This participation and payment relevant data will be deleted afterwards. All data will be anonymized and used for scientific research purposes only. Your data will not be passed on to third parties.

HIGH

This study consists of nine successive rounds. You may take as much time as you need to make your choices.

In each round, you will start with an amount of 100 Experimental Currency Units (ECU). You must decide which part of this amount (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 100% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of getting the amount that you bet back, plus an additional 250% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds.

Your total earnings for the round are equal to 100 ECU (your starting amount) plus your gains or losses in the gamble.

At the end of each round, you will learn how much money you gained or lost from the gamble and your total earnings for the round.

After that, you will make your choice for the next round. Again you start with 100 ECU, a part of which (between 0% and 100%) you can bet in the gamble. You may not bet money from previous rounds in the gamble. The same procedure as described above determines

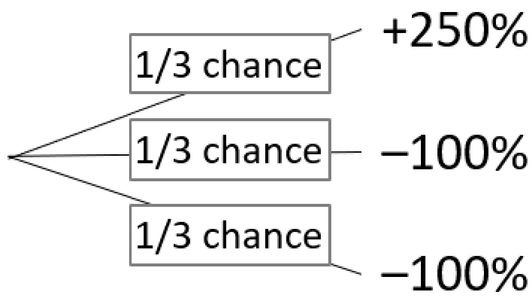
your earnings for this round. All subsequent rounds will also proceed in the same manner.

Your total earnings for this study in real euros are equal to your total ECU earnings (summed over all nine rounds) divided by 400. As an example, 1000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

This study consists of nine successive rounds. You may take as much time as you need to make your choices.

In each round, you will start with an amount of 100 Experimental Currency Units (ECU). You must decide which part of this amount (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 100% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of getting the amount that you bet back, plus an additional 250% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds.

In round 1, you will decide how much to bet in the gamble for rounds 1, 2, and 3. You must bet the same amount in all three rounds. Thus, if you decide to bet X% in the gamble for round 1, then you will also bet X% in the gamble for rounds 2 and 3. Therefore, three consecutive rounds are joined together.

After making your bet choice, you will learn how much money you gained or lost from the three gambles simultaneously, as well as your total earnings for the three rounds. Your total earnings for the three rounds are equal to 300 ECU (three times your starting amount of 100 ECU per round) plus your gains and losses in the three gambles.

After that, you will make your choice for the next three rounds (4-6). For each of the three rounds you again start with 100 ECU, a part of which (between 0% and 100%) you can bet in the gamble. You may not bet money from previous rounds in the gamble. The same procedure as described above determines your earnings for these three rounds, and you will again learn the outcomes of the three gambles simultaneously. The subsequent three rounds (7-9) will also proceed in the same manner.

Your total earnings for this study in real euros are equal to your total ECU earnings (summed over all nine rounds) divided by 400. As an example, 1000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

HIGH

Round 1:

What percentage of your endowment of 100 ECU do you bet in the following gamble in this round?

• You have a 2 out of 3 chance (67%) of losing 100% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of getting the amount that you bet back, plus an additional 250% of the amount that you bet.

0 10 20 30 40 50 60 70 80 90 100

LOW

Round 1:

What percentage of your endowment of 100 ECU do you bet in the following gamble in the next three rounds?

• You have a 2 out of 3 chance (67%) of losing 100% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of getting the amount that you bet back, plus an additional 250% of the amount that you bet.

0 10 20 30 40 50 60 70 80 90 100

HIGH

Round	Realization of Gamble	Your Gain/Loss Amount (Last Round)	Your Total Earnings (Last Round)
1	LOSS	-50 ECU	50 ECU

You **lost -50 ECU** from the gamble in the last round.
Your total earnings of the last round are 50 ECU.

LOW

Round	Realization of Gamble	Your Gain/Loss Amount (Last 3 Rounds)	Your Total Earnings (Last 3 Rounds)
1	WIN	+200 ECU	500 ECU
2	WIN		
3	LOSS		

You **gained +200 ECU** from the gamble in the last three rounds.
Your total earnings of the last three rounds are 500 ECU.

Your total earnings are 800 ECU, which amounts to 2€.

How satisfied are you with your return?

1 - Completely unsatisfied	2	3	4	5	6	7 - Completely satisfied
○	○	○	○	○	○	○

How well did you understand what to do and how to answer in this study?

- ○ Did not understand at all
- ○ I had quite some difficulties
- ○ Understood somehow what to do
- ○ Understood well
- ○ Everything was clear

Next, we would like to ask you some questions about the gamble from your previous task.

How certain are you about the distribution of outcomes of this gamble?

Absolutely uncertain	Moderately uncertain	Somewhat uncertain	Neither certain nor uncertain	Somewhat certain	Moderately certain	Absolutely certain
○	○	○	○	○	○	○

Please indicate to what extent you agree or disagree with the following statements:

I fully understand the distribution of outcomes of this gamble.

Strongly disagree	Moderately disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Moderately agree	Strongly agree
○	○	○	○	○	○	○

I am confident that I know the expected outcomes of the gamble.

Strongly disagree	Moderately disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Moderately agree	Strongly agree
○	○	○	○	○	○	○

I have sufficient information to play this gamble.

Strongly disagree	Moderately disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Moderately agree	Strongly agree
○	○	○	○	○	○	○

How risky is this gamble?

Very safe	Moderately safe	Somewhat safe	Neither risky nor safe	Somewhat risky	Moderately risky	Very risky
○	○	○	○	○	○	○

Please indicate to what extent you agree or disagree with the following statements:

It is very likely that I will lose money if I decide to play this gamble.

Strongly disagree	Moderately disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Moderately agree	Strongly agree
○	○	○	○	○	○	○

If I decide to play this gamble, I would worry about the consequences.

Strongly disagree	Moderately disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Moderately agree	Strongly agree
○	○	○	○	○	○	○

I could incur a great loss if I decide to play this gamble.

Strongly disagree	Moderately disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Moderately agree	Strongly agree
○	○	○	○	○	○	○

What was the probability to win the gamble in each round?

- ◯ 25%
- ◯ 33%
- ◯ 10%
- ◯ 67%

What was the amount you could **win** from the gamble in each round?

- ◯ 0.33 times (=33%) of the amount that you bet
- ◯ 0.25 times (=25%) of the amount that you bet
- ◯ 2.5 times (=250%) of the amount that you bet
- ◯ 2.3 times (=230%) of the amount that you bet

What was the amount you could **lose** from the gamble in each round?

- ◯ 0.10 times (=10%) of the amount that you bet
- ◯ 0.25 times (=25%) of the amount that you bet
- ◯ 1 time (=100%) of the amount that you bet
- ◯ 0.90 times (=90%) of the amount that you bet

Imagine you had bet 100% of your ECU in each round. What final wealth would you expect to have at the end of the nine rounds?

Imagine you had bet 100% of your ECU in each round. In how many out of 100 cases would you expect a negative return at the end of the nine rounds?

What is your gender?

- Male
- Female
- Other

Do you own financial products (like stocks, mutual/exchange-traded fund shares, etc.)?

- Yes
- No

How would you describe your financial risk appetite on a scale from 1 to 10?

	1	2	3	4	5	6	7	8	9	10	
I am not willing to take financial risks at all.	○	○	○	○	○	○	○	○	○	○	I am very willing to take financial risks.

How do you assess your statistical knowledge compared to the average of your fellow students?

	1	2	3	4	5	6	7	
much worse	○	○	○	○	○	○	○	much better

We pay you by bank transfer. For this, please provide us with your IBAN of the account you would like to receive the payment on. We will delete your payment details completely one month after the payment.

Your IBAN contains a country code at the beginning, e.g. "AT", "DE" or "IT".

IBAN:

Please state your IBAN again:

Lastly: Do you have any comments for us? (optional)

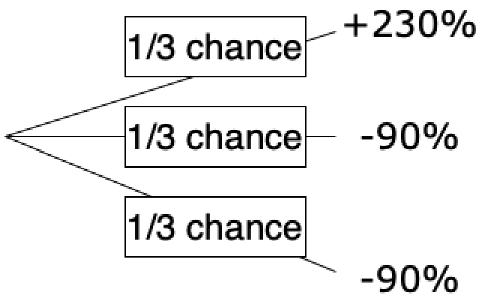
C.2 Instructions in other conditions

230-90L9 HIGH

This study consists of nine successive rounds. You may take as much time as you need to make your choices.

In each round, you will start with an amount of 100 Experimental Currency Units (ECU). You must decide which part of this amount (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 90% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of getting the amount that you bet back, plus an additional 230% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds.

Your total earnings for the round are equal to 100 ECU (your starting amount) plus your gains or losses in the gamble.

At the end of each round, you will learn how much money you gained or lost from the gamble and your total earnings for the round.

After that, you will make your choice for the next round. Again you start with 100 ECU, a part of which (between 0% and 100%) you can bet in the gamble. You may not bet money from previous rounds in the gamble. The same procedure as described above determines

your earnings for this round. All subsequent rounds will also proceed in the same manner.

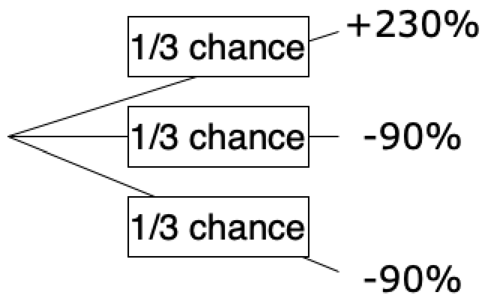
Your total earnings for this study in real euros are equal to your total ECU earnings (summed over all nine rounds) divided by 400. As an example, 1000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

230-90L9 LOW

This study consists of nine successive rounds. You may take as much time as you need to make your choices.

In each round, you will start with an amount of 100 Experimental Currency Units (ECU). You must decide which part of this amount (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 90% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of getting the amount that you bet back, plus an additional 230% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds.

In round 1, you will decide how much to bet in the gamble for rounds 1, 2, and 3. You must bet the same amount in all three rounds. Thus, if you decide to bet X% in the gamble for round 1, then you will also bet X% in the gamble for rounds 2 and 3. Therefore, three consecutive rounds are joined together.

After making your bet choice, you will learn how much money you gained or lost from the three gambles simultaneously, as well as your total earnings for the three rounds. Your total earnings for the three rounds are equal to 300 ECU (three times your starting

amount of 100 ECU per round) plus your gains and losses in the three gambles.

After that, you will make your choice for the next three rounds (4-6). For each of the three rounds you again start with 100 ECU, a part of which (between 0% and 100%) you can bet in the gamble. You may not bet money from previous rounds in the gamble. The same procedure as described above determines your earnings for these three rounds, and you will again learn the outcomes of the three gambles simultaneously. The subsequent three rounds (7-9) will also proceed in the same manner.

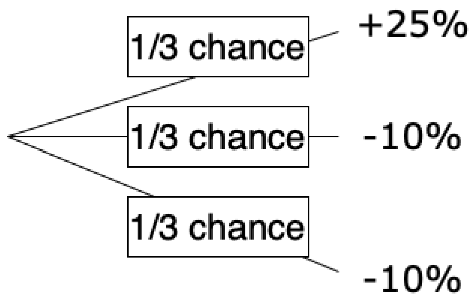
Your total earnings for this study in real euros are equal to your total ECU earnings (summed over all nine rounds) divided by 400. As an example, 1000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

25-10L9 HIGH

This study consists of nine successive rounds. You may take as much time as you need to make your choices.

In each round, you will start with an amount of 100 Experimental Currency Units (ECU). You must decide which part of this amount (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 10% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of winning 25% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds.

Your total earnings for the round are equal to 100 ECU (your starting amount) plus your gains or losses in the gamble.

At the end of each round, you will learn how much money you gained or lost from the gamble and your total earnings for the round.

After that, you will make your choice for the next round. Again you start with 100 ECU, a part of which (between 0% and 100%) you can bet in the gamble. You may not bet money from previous rounds in the gamble. The same procedure as described above determines

your earnings for this round. All subsequent rounds will also proceed in the same manner.

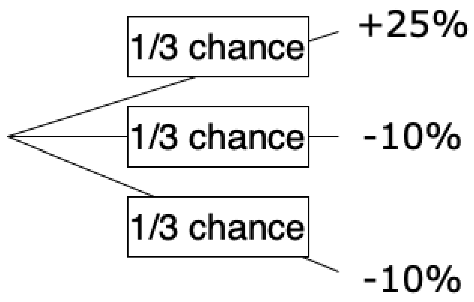
Your total earnings for this study in real euros are equal to your total ECU earnings (summed over all nine rounds) divided by 400. As an example, 1000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

25-10L9 LOW

This study consists of nine successive rounds. You may take as much time as you need to make your choices.

In each round, you will start with an amount of 100 Experimental Currency Units (ECU). You must decide which part of this amount (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 10% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of winning 25% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds.

In round 1, you will decide how much to bet in the gamble for rounds 1, 2, and 3. You must bet the same amount in all three rounds. Thus, if you decide to bet X% in the gamble for round 1, then you will also bet X% in the gamble for rounds 2 and 3. Therefore, three consecutive rounds are joined together.

After making your bet choice, you will learn how much money you gained or lost from the three gambles simultaneously, as well as your total earnings for the three rounds. Your total earnings for the three rounds are equal to 300 ECU (three times your starting

amount of 100 ECU per round) plus your gains and losses in the three gambles.

After that, you will make your choice for the next three rounds (4-6). For each of the three rounds you again start with 100 ECU, a part of which (between 0% and 100%) you can bet in the gamble. You may not bet money from previous rounds in the gamble. The same procedure as described above determines your earnings for these three rounds, and you will again learn the outcomes of the three gambles simultaneously. The subsequent rounds (7-9) will also proceed in the same manner.

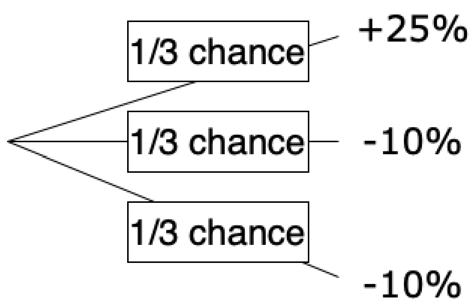
Your total earnings for this study in real euros are equal to your total ECU earnings (summed over all nine rounds) divided by 400. As an example, 1000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

25-10C9 HIGH

This study consists of nine successive rounds. You may take as much time as you need to make your choices.

You will start with a balance of 900 Experimental Currency Units (ECU). You must decide which part of your balance (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 10% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of winning 25% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds. Your balance at the end of a round is equal to your balance at the beginning of the round plus your gains or losses in the gamble.

At the end of each round, you will learn how much money you gained or lost from the gamble and your resulting balance.

After that, you will make your choice for the next round. Again you may choose to bet between 0% and 100% of your balance in the gamble. The same procedure as described above determines your earnings for this round. All subsequent rounds will also proceed in the same manner.

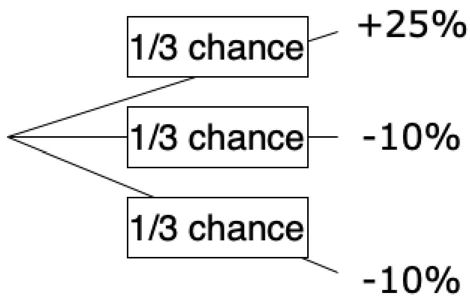
Your total earnings for this study in real euros are equal to your balance divided by 400 at the end of the last round. Hence, 1000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

25-10C9 LOW

This study consists of nine successive rounds. You may take as much time as you need to make your choices.

You will start with a balance of 900 Experimental Currency Units (ECU). You must decide which part of your balance (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 10% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of winning 25% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds. Your balance at the end of a round is equal to your balance at the beginning of the round plus your gains or losses in the gamble.

In round 1, you will decide what percent of your balance to bet in the gamble for rounds 1, 2, and 3. You must bet the same percent in all three rounds. Thus, if you decide to bet X% of your round 1 balance in the round 1 gamble, then you will also bet X% of your round 2 balance in the round 2 gamble and X% of your round 3 balance in the round 3 gamble. Therefore, three consecutive rounds are joined together.

After making your choice, you will learn how much money you gained or lost from the three gambles simultaneously, as well as your resulting balance at the end of the three rounds.

After that, you will make your choice for the next three rounds (4-6). The same procedure as described above determines your gains and losses for these three rounds, and you will again learn the outcomes of the three gambles simultaneously. The subsequent rounds (7-9) will also proceed in the same manner.

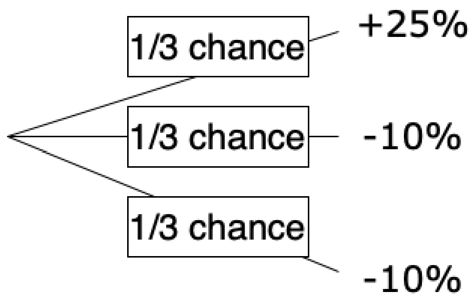
Your total earnings in real euros are equal to your balance divided by 400 at the end of the last round. Hence, 1000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

25-10L30 HIGH

This study consists of thirty successive rounds. You may take as much time as you need to make your choices.

In each round, you will start with an amount of 100 Experimental Currency Units (ECU). You must decide which part of this amount (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 10% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of winning 25% of the amount that you bet.



The lottery results of previous rounds have no influence on the probability of winning or losing in the current or in future rounds.

Your total earnings for the round are equal to 100 ECU (your starting amount) plus your gains or losses in the gamble.

At the end of each round, you will learn how much money you gained or lost from the gamble and your total earnings for the round.

After that, you will make your choice for the next round. Again you start with 100 ECU, a part of which (between 0% and 100%) you can bet in the gamble. You may not bet money from previous rounds in the gamble. The same procedure as described above determines

your earnings for this round. All subsequent rounds will also proceed in the same manner.

Your total earnings for this study in real euros are equal to your total ECU earnings (summed over all thirty rounds) divided by 1200. As an example, 3000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

25-10L30 LOW

This study consists of thirty successive rounds. You may take as much time as you need to make your choices.

In each round, you will start with an amount of 100 Experimental Currency Units (ECU). You must decide which part of this amount (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 10% of the amount that you bet.
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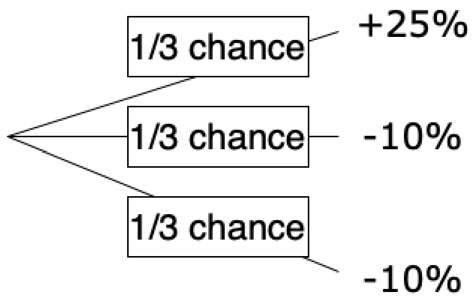

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After making your bet choice, you will learn how much money you gained or lost from the three gambles simultaneously, as well as your total earnings for the three rounds. Your total earnings for the three rounds are equal to 300 ECU (three times your starting

amount of 100 ECU per round) plus your gains and losses in the three gambles.

After that, you will make your choice for the next three rounds (4-6). For each of the three rounds you again start with 100 ECU, a part of which (between 0% and 100%) you can bet in the gamble. You may not bet money from previous rounds in the gamble. The same procedure as described above determines your earnings for these three rounds, and you will again learn the outcomes of the three gambles simultaneously. The subsequent rounds (7-30) will also proceed in the same manner.

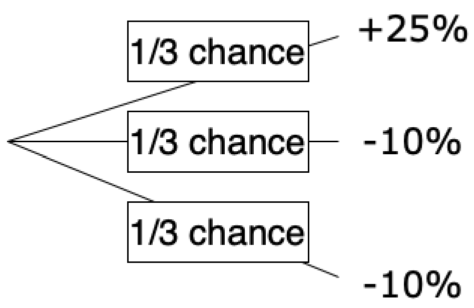
Your total earnings for this study in real euros are equal to your total ECU earnings (summed over all thirty rounds) divided by 1200. As an example, 3000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

25-10C30 HIGH

This study consists of thirty successive rounds. You may take as much time as you need to make your choices.

You will start with a balance of 3000 Experimental Currency Units (ECU). You must decide which part of your balance (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

• You have a 2 out of 3 chance (67%) of losing 10% of the amount that you bet.
• You have a 1 out of 3 chance (33%) of winning 25% of the amount that you bet.



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At the end of each round, you will learn how much money you gained or lost from the gamble and your resulting balance.

After that, you will make your choice for the next round. Again you may choose to bet between 0% and 100% of your balance in the gamble. The same procedure as described above determines your earnings for this round. All subsequent rounds will also proceed in the same manner.

Your total earnings for this study in real euros are equal to your balance divided by 1200 at the end of the last round. Hence, 3000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.

25-10C30 LOW

This study consists of thirty successive rounds. You may take as much time as you need to make your choices.

You will start with a balance of 3000 Experimental Currency Units (ECU). You must decide which part of your balance (between 0% and 100%) you wish to bet in the following gamble whose outcome will be randomly determined by the computer in each round.

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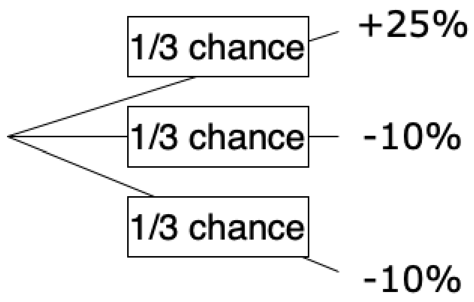

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After making your choice, you will learn how much money you gained or lost from the three gambles simultaneously, as well as your resulting balance at the end of the three rounds.

After that, you will make your choice for the next three rounds (4-6). The same procedure as described above determines your gains and losses for these three rounds, and you will again learn the outcomes of the three gambles simultaneously. The subsequent rounds (7-30) will also proceed in the same manner.

Your total earnings in real euros are equal to your balance divided by 1200 at the end of the last round. Hence, 3000 ECU equal 2.50€ in real money. You will be paid automatically by bank transfer very shortly after the study.