Behavioral Biases in Dynamic Choice: Theory and Evidence

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Contents

Preface

While behavioral economics successfully explains many commonly observed choice patterns that remain puzzles within the neoclassical model, it poses its own challenges as well. This dissertation consists of three self-contained chapters that tackle such challenges in the domain of dynamic choice.

Dynamics are a crucial feature of many economic decision problems and a key driver of their complexity. The term "dynamic" does not only refer to so-called inter-temporal tradeoffs, i.e. a time-lag between choice and outcome that drives costs and benefits apart, but also to interactions between sequential decisions. A choice in the present may both determine what options will become available in future choices, as well as what desires will be evoked then.

A leading behavioral model of inter-temporal choice is hyperbolic discounting (e.g. Laibson 1997), which induces time-inconsistency by allowing discount rates to fade over time. Near future is discounted heavier relative to presence than distant future is discounted relative to near future. This inconsistency is well known as present bias and captures a desire for immediate gratification. It was noted early on that such time-inconsistent behavior is crucially affected by a decision maker's awareness of own present bias (e.g. Strotz 1956). Decision makers who understand that they repeatedly fail in achieving a long-run goal because of giving in to immediate desires may want to "tie their hands" and self-commit. Moreover, the attractiveness of a current option may depend on future choices, so that its perceived attractiveness depends on anticipated actions. While the interaction of present bias and anticipation mistakes, so called naïve present bias, helps understand many behavioral patterns in dynamic choice, it poses a challenge itself. Naïveté was shown to aggravate the negative effects of present bias in many domains (e.g. DellaVigna & Malmendier 2004), but feedback abounds and should easily allow for a correction of own anticipations. So how can naïveté with respect to present bias persist in face of ample learning opportunity?

In Chapter 1 of this dissertation, I tackle this question by characterizing potential upsides of naïveté. As these apply to economically important domains, such as health and education, this establishes a meaningful tradeoff between sophistication and naïveté, as both can be beneficial and harmful. Specifically, I show that naïveté helps a decision maker partly overcome present bias in situations where sequential investments exhibit sufficient complementarity and cannot be pre-committed. Intuitively, this is because a present biased decision maker invests too little from a welfare point of view, and naïveté motivates to invest more in case of complementarity.

The novel aspect of this investigation is the payoff structure of the choice environment, i.e. the focus on complementarity respectively substitutability of investments. There, the underlying model of naïve present bias in form of quasi-hyperbolic discounting directly applies and offers interesting insights.

However, dynamic inconsistency can arise also in models that were not intended to generate it in the first place. Prospect Theory (Kahneman & Tversky 1979), for example, is a static model of decision making under risk, i.e. it does not entail any dynamics. Thanks to its success, however, it was applied in dynamic decision making as well. As the Prospect Theory value function defines risk preferences locally, i.e. relative to a reference point, dynamics are apt to interfere with them. When reference point and state of world shift relative to one another, because either of the two moves, or both move non-simultaneously, a shift into the gain or loss domain occurs, where risk preferences are different than at the "kink". Such a preference shift can induce inconsistency, which in turn can make decisions sensitive to anticipation.

Chapter 2 of this dissertation, which is joint work with Maximilian Breu and Felix Peterhammer, investigates experimentally such Prospect Theory anticipation. We fixed reference points by inducing them, and found that almost half of our participants planned a sequence of investment choices poorly, relative to a simple benchmark of zero gain-loss-utility from never investing. This suggests a rather poor understanding of own future Prospect Theory preferences in general. Moreover, almost a third of our participants undervalued a commitment option, which suggests an under-appreciation of own reactions to domain shifts.

In order to identify potential drivers of between-subject differences in anticipation quality, we conducted a Cognitive Reflection Test and a Big-5 personality test, recorded planning times, calculated a stability measure of Prospect Theory preferences, and collected self-reported demographics. We found that variation in planning quality was captured well by these correlates, but commitment quality was not. Specifically, higher cognitive reflection, planning time, and stability of Prospect Theory preferences improved planning quality, whereas more agreeableness and neuroticism impaired it. Our stability measure of Prospect Theory preferences is based on a separate elicitation of loss aversion and diminishing sensitivity parameters in two subsequent weeks, and reveals inconsistency in the parameter estimates for almost a third of our participants. While this could be explained by inattention and the limited number of observations per subject, it might also point toward an additional dynamic inconsistency of decision makers with Prospect Theory preferences.

Another dynamic choice anomaly is the so called disposition effect, which has been studied extensively. It refers to a tendency of investors to sell winning assets too early and keep losing assets too long. Originally, it was viewed as a mistake as it is suboptimal with respect to taxation. Later, other potential rational explanations, such as portfolio re-balancing or differences in transaction costs, were disregarded. Behavioral economics, however, can rationalize the observed behavior even twice: On the one hand, people may have a preference against realizing losses and, therefore, defer it despite instrumental costs; On the other hand, they may hold wrong believes about their assets, i.e. view their winners overly pessimistic and their losers overly optimistic.

Both explanations are suitable to rationalize the disposition effect, but they give rise to completely different welfare implications. If it is non-standard preferences that generate the disposition effect, investors are perfectly aware of the instrumental costs of their decisions and happy to bear them in order to achieve some non-standard utility instead. If it is non-standard beliefs, however, investors may exhibit the disposition effect simply because they do not know any better. In the former case, behavior is individually rational. In the latter case, decision makers would adjust their behaviors if they had more accurate beliefs of the situation.

Chapter 3 of this dissertation, which is joint work with Johannes Maier, experimentally tests these two behavioral explanations of the disposition effect against one another. In order to do so, we decompose the disposition effect into four different mistakes relative to the standard model. Specifically, we view first-order violations as mistakes and distinguish them according to the action, i.e. keeping the own asset or switching, and according to the domain, i.e. gains or losses. Testing the explanations against each other becomes possible because they make different predictions for the specific error patterns: The leading preference based explanation, realization utility, predicts keep violations in losses and switch violations in gains, but not the opposite violations; The most prominent mechanical belief bias, belief in mean reversion, predicts all violations equally; and motivated beliefs predict only keep violations, but no switch violations. It is important to notice that usual measures of the disposition effect confound error propensities and error frequencies. However, a specific error can happen with a high propensity, e.g. whenever it is possible, but still be observed only infrequently, e.g. when it is very often simply not possible.

Doing such a decomposition required a laboratory experiment, as it requires to control for the true return process as well as for the information. Main features of our design are that only binary choices were allowed, i.e. portfolio building was precluded, and that several periods had to be observed before investment decision could be made. The former feature is necessary to establish a rational benchmark by first-order violations, the latter allows to disentangle gains from good news respectively losses from bad news, which is necessary for distinguishing preference-based and belief-based models based on subjects' investment choices alone.

We found much higher propensities for keep violations than for switch violations, both in gains and in losses, which is consistent only with the motivated beliefs explanation, not with the mechanic beliefs or non-standard preferences explanations. In a robustness treatment, we explicitly asked for our participants' beliefs in each investment decision, and found that they behaved mostly in line with their stated beliefs, but these were often non-Bayesian. This supports the view that our participants were rather not aware of instrumental consequences when they committed first-order violations, instead of being aware and actually wanting to commit them.

1. Motivation by Naïveté

1.1. Introduction

Many decisions, both minor and major ones, entail a conflict of interest between people's long-run goals and their desire for immediate gratification. Such present bias induces time-inconsistent decisions and has drawn considerable attention in the economics literature (e.g. Strotz 1956, Pollak 1968, Laibson 1997).

In path-dependent decision sequences, current choices affect a distant outcome not only directly, but also indirectly via their effects on subsequent decisions. In such situations, behavioral consequences of present bias are crucially driven by the decision makers' awareness of their bias's persistence, their so called sophistication or (partial) naïveté. Here, the appeal of a current choice heavily relies on the (potentially wrong) anticipation of its implications for future choices which, in turn, depends on the anticipation of own future preferences, in particular of own future present bias.

Such anticipation mistakes have been shown to harm decision makers in many economically relevant situations. Intuitively, naïveté with respect to present bias is overoptimism with respect to one's own future self-control and, hence, lets decision makers take up ineffective or even exploitative commitments. This poses the puzzle how naïveté can persist in face of ample learning opportunities.

I argue that many important inter-temporal decisions do not easily allow for any commitment at all, such as investments in education or health, and show that in such situations naïveté can actually help decision makers to partly overcome their present bias. Specifically, I characterize necessary and sufficient conditions of the reward structure of a choice environment for naïveté to be beneficial. The key driver of the potential benefits of naïveté is complementary of the investments in their joint reward determination.

For example, education decisions are highly path-dependent and jointly determine the distant reward of a better or worse job regarding pay, status, job-security, career prospects, et cetera. Moreover, investments in education are likely to be complementary with respect to the described outcomes, as success in one stage is typically a prerequisite for entering precisely those paths that are most conducive to a later career: One has to excel at school to be admitted to a prestigious college, where one has to stand out to become part of the Master's program, and so on; shirking in an early stage leads to a lower education path where subsequent effort pays off less.

Therefore, present biased school graduates will determine their choice of college and field of study not only by weighing their current desire for leisure against the distant reward from current effort, but also consider their future desire for exerting further effort. As naifs are overoptimistic with respect to own future effort, they believe to seize all opportunities that their current efforts will bring about and, hence, regard current investments as attractive. On the other hand, sophisticates correctly predict to be just as lazy tomorrow as they are today, such that creating future opportunities seems less worthwhile to them.

This example naturally extends to subsequent work decisions, as a higher career path offers more opportunities to excel with one's effort. For instance, a surgeon not only earns a much higher wage per hour than a nurse, but also faces much stronger career incentives, as becoming head physician is a disproportionately more dramatic step in status than becoming head nurse. Yet, becoming the former instead of the latter requires a substantially higher upfront investment in education.

The remainder of this article is structured as follows. Section 1.2 reviews the most closely related literature, Section 1.3 introduces the model, Section 1.4 provides the main analyses and results, and Section 1.5 concludes. All proofs are relegated to Appendix A.1, and Appendix A.2 provides a cross-validation for the leading example.

1.2. Literature

The importance of the anticipation of own time inconsistency for determining a decision maker's course of action has been pointed out in the literature on hyperbolic discounting right from the start (Strotz 1956, Pollak 1968). The first, however, who explicitly contrasted the differential implications of full sophistication versus full naïveté were O'Donoghue & Rabin (1999) shortly after the revival of the quasi-hyperbolic discounting model of Phelps & Pollak (1968) by Laibson (1997).

O'Donoghue & Rabin (1999) investigate the timing of a single action without pre-commitment and find that choices and welfare of decision makers crucially depend on the payoff-structure of the decision at hand. For immediate costs and delayed rewards, naïveté aggravates procrastination, i.e. the delay of a costly task against one's own long-run self's best interest; for immediate rewards and delayed costs, naïveté alleviates preproperation, i.e. the immediate consumption of a reward when it is in one's own long-run self's best interest to wait. From a hypothetical self-zero's point of view, both procrastination and preproperation are suboptimal timings of an action and reduce the decision maker's welfare. Hence, naïveté is harmful in the immediate costs case, but beneficial in the immediate rewards case.

Subsequent investigations paid particular attention to the effects of naïveté and sophistication in models with multiple actions and commitment devices. Specifically, they studied the design of (potentially exploitative) commitment devices for naïve present biased decision makers by fully rational firms. In such setups, the benefit of naïveté on timing in immediate rewards situations vanishes and naïveté becomes unambiguously harmful. These investigations span a wide range of interesting and important applications: DellaVigna & Malmendier (2004) show that naïve present biased decision makers can be exploited in market settings, e.g. in the health club and credit card industries; Eliaz & Spiegler (2006) find that unbiased principals are able to exploit naïve present biased agents, e.g. in the cable TV and casino gambling industries; Heidhues & Kőszegi (2010) show that banks can exploit naïve present biased consumers by offering credit contracts which induce over-borrowing, mostly for credit cards and mortgages.

However, many economically relevant inter-temporal decisions do not easily allow for (external) commitments, e.g. investments in education and health. There, the effects of naïveté are much more intriguing.

Carrillo & Mariotti (2000) investigate how strategic ignorance of costless information can help overcome present bias in situations where immediate consumption exerts an initially unknown negative externality on future utility. As decision makers over-consume due to their present bias, they may choose to remain uninformed, as overestimation of the externality reduces harmful over-consumption. Hence, in this immediate consumption and delayed costs model, pessimism in a risky environment helps overcome present bias. Bénabou & Tirole (2002) investigate how motivation by over-confidence can help overcome present bias when an unknown own ability determines the delayed reward of an immediate investment. In this model, decision makers can manipulate their beliefs and choose how to inform themselves.

In both models, decision makers hold wrong beliefs about their environment, i.e. about the consequences of their choices. In Carrillo & Mariotti (2000), decision makers sustain overly pessimistic beliefs with respect to the externality of their actions and in Bénabou & Tirole (2002), decision makers sustain overly optimistic, possibly manipulated beliefs with respect to their own abilities. In my model, decision makers are perfectly aware of the consequences of their choices, but fail to predict their own future preferences and, hence, their future decisions.

The most closely related paper to my investigation is Herweg & Müller (2011). They investigate how present bias and naïveté affect choices and welfare in a model where the sum of two sequential efforts determines a delayed reward. They find that present biased decision makers under-invest relative to the time-consistent benchmark and that a higher stage one investment implies a lower stage two investment, higher overall investment, and a better effort smoothing (conditional on the overall effort level). Therefore, the welfare effect of naïveté solely depends on its effect on stage one investment: Naïveté induces higher welfare (compared to sophistication) if and only if it induces a higher stage one investment.

However, they do not provide a characterization for when stage one investment does increase in naïveté.[1] They do provide a numerical example where more na-

[1] Their Proposition 3 states a sufficient criterion for sophistication to be beneficial, but this

ïveté induces higher stage one investment and, hence, higher welfare. This is an important contribution because they are the first to do so in an immediate costs and delayed rewards setting. However, the example relies on piecewise defined functions that do not meet the model assumptions. Therefore, it is not instructive for the identification of a general pattern.

I use a slightly different model specification that allows for more general utility functions, and derive a necessary and sufficient characterization for stage one investment to increase in naïveté in my Proposition 1, which helps to fill this gap in the literature. Specifically, I allow for complementarity of investments, which is not only a key driver of the effects of naïveté, but also broadens the potential applications of the model.

1.3. Model

In this section, I introduce the model and its underlying assumptions for the immediate costs and delayed rewards case. The model can be defined analogously for the reverse case of immediate rewards and delayed costs.

Suppose a present biased decision maker decides in each of two subsequent periods how much to invest in a project, e.g. how much effort to exert in school and university, and both periods' investments jointly determine a distant reward, e.g. wage and prestige of a later career. Let x and y denote (the immediate costs of) stage one and two investments, respectively, and $u(.,.)$ the (instantaneous) utility function of these investments.[2] Figure 1.3.1 illustrates the time structure of the payoffs.

criterion is in fact impossible to meet within their own assumptions. Specifically, it demands $g'''(.) \leq 0$ globally for a function $g(.)$ that is globally strictly increasing and strictly concave, which jointly implies that $g''(.)$ converges to zero, which can happen monotonically only for $g'''(.) > 0$. A necessary criterion is not offered.

[2] Unless explicitly stated otherwise, all results hold true for strictly convex cost functions as well, but notation becomes slightly cluttered.

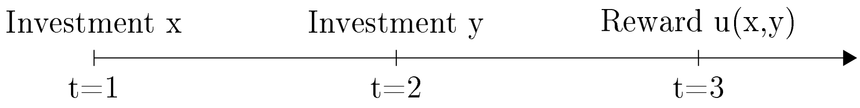


Figure 1.3.1.: Time Structure of the model.

I impose the usual smoothness, monotonicity, and curvature assumptions on the utility function.

Assumption 1.

(i) The utility function is continuously differentiable up to order three.

(ii) Utility $u(.,.)$ is non-negative, strictly increasing, and strictly concave.

A big class of utility functions that satisfy Assumption 1 are the Cobb-Douglas functions of homogeneity smaller one.

Example (Cobb-Douglas)**.**
All utility functions $u(x, y) := \alpha x^{\mu} y^{\nu}$ with $\alpha, \mu, \nu > 0$ and $\mu + \nu < 1$ satisfy Assumption 1.

Notice that Cobb-Douglas functions of homogeneity equal one do not satisfy Assumption 1, because they are not strictly concave. In fact, "cutting" a function of homogeneity equal one through the origin always yields a straight line. Appendix A.2 provides a detailed test of Assumption 1 for the Cobb-Douglas functions of homogeneity smaller one and demonstrates linearity of the Cobb-Douglas functions of homogeneity equal one.

I model present bias as quasi-hyperbolic discounting (Phelps & Pollak 1968, Laibson 1997), i.e. perceived inter-temporal utility U_t at time t of a future stream of instantaneous utilities $(u_s)_{s \geq t}$ is defined as follows.

$$U_t := u_t + \beta \sum_{s>t} \delta^{s-t} u_s$$

Here, $\delta < 1$ captures the usual time-consistent discounting and $\beta < 1$ captures the decision maker's desire for immediate gratification. As β discounts all future periods equally, relative to the present, and as presence shifts in the course of time,

β induces a time inconsistency. For simplicity and as is common in finite horizon applications of this model, I set the exponential discount factor $\delta = 1$ and focus on the effects of present bias $\beta < 1$ alone.

Naïveté with respect to present bias is defined as a misperception of how much present bias one will exhibit in the future. Following O'Donoghue & Rabin (2001), I denote a decision maker's anticipated degree of present bias as $\hat{\beta}$, with $\beta \leq \hat{\beta} \leq 1$. As $\hat{\beta} = \beta$ corresponds to full sophistication and $\hat{\beta} = 1$ to full naïveté, I refer to $\hat{\beta}$ as a decision maker's degree of naïveté.

Further, and most crucially, I assume that no (external) commitments are available.[3]

Assumption 2.
Stage two investment y cannot be pre-commited in stage one.

Clearly, this precludes all applications where commitments are readily available, such as the classical consumption-savings problem. However, many economically important inter-temporal decisions do not easily allow for commitment, such as education and health decisions, and, therefore, remain within the scope of this investigation.

As is common in the (above cited) literature, I assume the natural rationality condition that all agents follow perception-perfect strategies, i.e. they choose their stage one and two investments as to maximize their perceived inter-temporal utilities U_1 and U_2, respectively, given their anticipation of own future behavior. Crucially, they anticipate at stage one to maximize a (possibly distorted) utility $\hat{U}_2$ at stage two which reflects not their actual present bias, but their (potentially wrong) anticipation of future present bias.

$$\begin{array}{rcrrl} U_1 & = & -x & -\beta y & +\beta u(x,y) \\ U_2 & = & & -y & +\beta u(x,y) \\ \hat{U}_2 & = & & -y & +\hat{\beta} u(x,y) \end{array}$$

[3] Or alternatively, that all available commitments are perceived as non-beneficial and, therefore, never taken up.

1.4. Analysis

In this section, I analyze the effects of (partial) naïveté on investment choices and welfare of present biased decision makers. A crucial driver of these effects is complementarity, respectively substitutability, of the utility function.

Definition 1 (Complementarity).
For a joint utility function $u(x, y)$ of both periods' investments x and y, respectively, investments are called

(i) ***complements*** *iff* $u_{x,y}(.,.) > 0$;

(ii) ***independent*** *iff* $u_{x,y}(.,.) = 0$;

(iii) ***substitutes*** *iff* $u_{x,y}(.,.) < 0$.

Intuitively, investments are complements iff an increase in one period's investment raises the marginal benefit of the other period's investment, and they are substitutes iff an increase in one period's investment diminishes the marginal benefit of the other period's investment.

Solving for the perception-perfect strategy and the (potentially deviating) behavior of the decision maker requires backwards induction. The decision maker anticipates at stage one to solve the following maximization problem at stage two.

$$\begin{aligned} \max_y \hat{U}_2 &= \max_y -y + \hat{\beta} u(x, y) \\ FOC: &\qquad -1 + \hat{\beta} u_y(x, y) \stackrel{!}{=} 0 \end{aligned}$$

The second order condition holds by strict concavity of $u(.,.)$. Hence, the implicit function theorem is applicable and yields the existence of an *anticipated reaction function* $g(x, \hat{\beta})$ of how much a decision maker believes to invest at stage two conditional on stage one investment x and the degree of naïveté $\hat{\beta}$.

$$\exists\, g \ s.t. \ -1 + \hat{\beta} u_y(x, g(x, \hat{\beta})) = 0 \tag{1.1}$$

I will frequently treat $\hat{\beta}$ as a parameter of the anticipated reaction function and, hence, use the shorthand $\hat{g}(x) := g(x, \hat{\beta})$. The implicit function theorem implies the differentiability of g(.,.) with respect to both x and $\hat{\beta}$, and I denote these partial derivatives as $\hat{g}_x(.)$ and $\hat{g}_{\hat{\beta}}(.)$, respectively. Strict monotonicity everywhere implies that $\hat{g}$ exists not only locally, but globally.

The *actual reaction function* corresponds to $\hat{g}$ for $\hat{\beta} = \beta$ and is independent of the decision maker's anticipation. That is, conditional on the first period investment, naïveté does not affect stage two behavior anymore. However, it is crucial in determining stage one investment. In slight abuse of notation, I denote this actual reaction function as g.

The following first result is a direct implication of the implicit function theorem.

Lemma 1 (Reaction Function Monotonicity)**.**
$sgn(\hat{g}_x) = sgn(u_{x,y})$; in particular,

(i) complementarity of investments implies a strict increase of (both anticipated and actual) stage two investment in stage one investment;

(ii) $u_{x,y} = 0$ implies independence of stage two investment from stage one investment;

(iii) substitutability of investments implies a strict decrease of stage two investment in stage one investment.

The next result shows that overoptimism with respect to future investment is a universal feature of naïveté that holds independently of complementarity.

Lemma 2 (Overoptimism)**.**

(i) (Partially) naïve decision makers are overoptimistic with respect to their stage two investment, i.e. $\hat{g} > g$ for all $\hat{\beta} > \beta$.

(ii) Overoptimism strictly increases in the degree of naïveté, i.e. $\hat{g}$ strictly increases in its parameter $\hat{\beta}$.

For specific functions, the (implicit) perceived reaction function $\hat{g}$ can be derived explicitly. I do so for the Cobb-Douglas case and cross-validate the above statements.

Example (Cobb-Douglas, continued).
For Cobb-Douglas utility functions, investments are complements. The perceived reaction function for stage two investment and its partial derivatives read as follows.

$$\begin{aligned}
\hat{g}(x) &= \left(\hat{\beta}\alpha\nu\right)^{\frac{1}{1-\nu}} x^{\frac{\mu}{1-\nu}} \\
\hat{g}_x(x) &= \left(\hat{\beta}\alpha\nu\right)^{\frac{1}{1-\nu}} \frac{\mu}{1-\nu} x^{\frac{\mu+\nu-1}{1-\nu}} \\
\hat{g}_{\hat{\beta}}(x) &= \hat{\beta}^{\frac{\nu}{1-\nu}} (\alpha\nu)^{\frac{1}{1-\nu}} \frac{1}{1-\nu} x^{\frac{\mu}{1-\nu}}
\end{aligned}$$

The implications of Lemma 1 and Lemma 2 are immediately evident.

Unfortunately, deriving the decision maker's stage one investment x is considerably harder. It requires to solve the following maximization problem.

$$\max_x \quad -x - \beta\hat{g}(x) + \beta u(x,\hat{g}(x))$$

$$FOC: -1 - \beta\hat{g}_x(x) + \beta u_x(x,\hat{g}(x)) + \beta u_y(x,\hat{g}(x))\hat{g}_x(x) \stackrel{!}{=} 0 \tag{1.2}$$

$$SOC: +\beta\left[u_{x,x}(x,\hat{g}(x)) + u_{x,y}(x,\hat{g}(x))\hat{g}_x(x)\right] + \beta\left(\frac{1}{\hat{\beta}} - 1\right)\hat{g}_{x,x}(x) \stackrel{!}{<} 0 \tag{1.3}$$

In case of full naïveté, i.e. for $\hat{\beta} = 1$, the rightmost term of (1.3) vanishes and the second order condition holds by Assumption 1 (strict concavity of $u(.,.)$). For the general case of $\beta \leq \hat{\beta} < 1$, a sufficient condition for (1.3) to hold can be derived easily in the form of a positive upper bound for $\hat{g}_{x,x}$, which is usually strictly negative. To be on the safe side, I impose it as an assumption.

Assumption 3.
The second order condition (1.3) holds for all $\beta \leq \hat{\beta} \leq 1$, i.e.

$$\hat{g}_{x,x}(x) < \frac{-\hat{\beta}}{1-\hat{\beta}} \frac{detH_u(x,\hat{g}(x))}{u_{y,y}(x,\hat{g}(x))} > 0.^{4}$$

[4] By calculating the second derivative $\hat{g}_{x,x}$ of the perceived reaction function, Assumption 3 can be restated as follows.

$$|\nabla u|^T H_{u_y} |\nabla u| < \frac{\hat{\beta}}{1-\hat{\beta}} u_{y,y}^2 detH_u$$

The above first order condition (1.2) implicitly characterizes the perceived optimal stage one investment x, which is, therefore, actually undertaken by the decision maker. The following main result characterizes how stage one investment x is affected by naïveté $\hat{\beta}$.

Proposition 1 (Stage One Investment)**.**
Let $x_{\hat{\beta}}$ denote the change of stage one investment x in naïveté $\hat{\beta}$. Then,

$$sgn(x_{\hat{\beta}}) = sgn(\hat{g}_{x,\hat{\beta}}(x)).$$

In particular, stage one investment x increases in naïveté $\hat{\beta}$ if and only if $\hat{g}_{x,\hat{\beta}}(x) > 0$, and decreases if and only if $\hat{g}_{x,\hat{\beta}}(x) < 0$.[5]

Whether stage one investment x increases or decreases in the degree of naïveté $\hat{\beta}$ is fully determined by the sign of $\hat{g}_{x,\hat{\beta}}$. This cross-derivative of $\hat{g}$ characterizes the change in slope of the anticipated reaction function $\hat{g}$ with respect to changes in naïveté. Intuitively, this describes the sensitivity with respect to naïveté of decision makers' anticipations of the intensity of their own stage two reactions to stage one investment. Note again that the intensity of the actual stage two reaction is independent of anticipation respectively naïveté. The sign of this reaction is implied by Lemma 1. Specifically, the change of stage two investment in naïveté is of same sign as the change of stage one investment in naïveté for complements, and of opposite sign for substitutes.

By Definition 1, the marginal benefit of stage one investment increases in stage two investment for complements, and decreases in stage two investment for substi-

It is immediately evident that negative semi-definiteness of the Hessian of u_y would be a sufficient criterion for Assumption 3 to hold. However, only complementary utility functions satisfy $u_{y,x,x} < 0$, but they violate $detH_{u_y} > 0$. Therefore, this criterion would not just be demanding, but in fact impossible to meet.

[5]This result is slightly more complicated for strictly convex cost functions $c(.)$:

$$sgn(x_{\hat{\beta}}) = sgn(c_{x,x}(\hat{g}(x))\hat{g}_x(x)\hat{g}_{\hat{\beta}}(x) + c_x(\hat{g}(x))\hat{g}_{x,\hat{\beta}}(x)).$$

According to Lemma 1 and 2, the sign of the left term on the right-hand side is positive for complements and negative for substitutes. Hence, only either necessary or sufficient conditions hold. Precisely, $\hat{g}_{x,\hat{\beta}} > 0$ is sufficient for stage one investment to increase in $\hat{\beta}$ for complements and necessary for substitutes, and $\hat{g}_{x,\hat{\beta}} < 0$ is necessary for stage one investment to decrease in $\hat{\beta}$ for complements and sufficient for substitutes.

tutes. Bearing this in mind, the intuition of Proposition 1 becomes straightforward: The stronger a reaction in stage two the decision maker anticipates, the more attractive is an early investment in case of complements, and the less attractive it is in case of substitutes. Clearly, the strength of the anticipated reaction is captured in the steepness of the anticipated reaction function $\hat{g}$. For complements, $\hat{g}$ has a positive slope and, hence, becomes steeper in $\hat{\beta}$ iff $\hat{g}_{x,\hat{\beta}} > 0$; For substitutes, $\hat{g}$ has a negative slope and, hence, becomes steeper in $\hat{\beta}$ iff $\hat{g}_{x,\hat{\beta}} < 0$.

The implicit function theorem allows to rephrase Proposition 1 in terms of higher order derivatives of $u(.,.)$.

Corollary 1 (Sufficient Complementarity and Sufficient Substitutability)**.**

(i) For complementary utility functions $u(.,.)$, stage one investment x increases in naïveté $\hat{\beta}$ iff both stages investments are ***sufficiently complementary*** *in the sense that*

$$\frac{-u_{y,y,y}}{u_{y,y}} > \frac{-u_{y,x,y}}{u_{y,x}}.$$

(ii) For substitutable utility functions $u(.,.)$, stage one investment x decreases in naïveté $\hat{\beta}$ iff both stages investments are ***sufficiently substitutable*** *in the sense that*

$$\frac{-u_{y,y,y}}{u_{y,y}} > \frac{-u_{y,x,y}}{u_{y,x}}.$$

To get an intuition for the sufficiency notion in Corollary 1, we have to think of $u_{y,y}$ as (local) measure of concavity and of $u_{y,x}$ as (local) measure of complementarity respectively substitutability. Then, $u_{y,y,y}$ measures the fading of concavity, and $u_{y,x,y}$ the fading of complementarity respectively substitutability with respect to an increase in y.[6] Hence, the fraction $\frac{-u_{y,y,y}}{u_{y,y}}$ can be interpreted as relative fading of concavity, and the fraction $\frac{-u_{y,x,y}}{u_{y,x}}$ can be interpreted as relative fading of complementarity respectively substitutability of $u(.,.)$ with respect to stage two investment y.

Put together, sufficiency in the above sense means that complementarity respectively substitutability fades at a lower rate than concavity of $u(.,.)$. That

[6] Note that concavity and complementarity respectively substitutability have to fade, i.e. $u_{y,y}$ and $u_{y,x}$ have to converge to zero, as global strict monotonicity of $u(.,.)$ (Assumption 1) would be violated otherwise.

is, "sufficient" does not refer to the actual level of complementarity respectively substitutability, but to its relative rate of deterioration. By Corollary 1, such sufficiency implies an increase of stage one investment x in naïveté $\hat{\beta}$ for complements, and a decrease for substitutes.

Cobb-Douglas functions of homogeneity smaller one are sufficiently complementary in this sense.

Example (Cobb-Douglas, continued).
For Cobb-Douglas utility functions, the cross derivative of the perceived reaction function for stage two investment reads as follows.

$$\hat{g}_{x,\hat{\beta}}(x) = \hat{\beta}^{\frac{\nu}{1-\nu}} (\alpha\nu)^{\frac{1}{1-\nu}} \frac{\mu}{(1-\nu)^2} x^{\frac{\mu+\nu-1}{1-\nu}} > 0.$$

Proposition 1 implies that stage one investment x is strictly increasing in naïveté $\hat{\beta}$. We can cross-validate this finding by calculating x explicitly.

$$x = \left(\hat{\beta}^{\frac{\nu}{1-\nu}} \frac{1-\hat{\beta}\nu}{1-\nu}\right)^{\frac{1-\nu}{1-\nu-\mu}} \mu^{\frac{1-\nu}{1-\nu-\mu}} \alpha^{\frac{1}{1-\nu-\mu}} \nu^{\frac{\nu}{1-\nu-\mu}} \beta^{\frac{1-\nu}{1-\nu-\mu}}$$

In fact, $x_{\hat{\beta}} > 0$ for all $\hat{\beta} < 1$. Hence, stage one investment x is strictly increasing in naïveté $\hat{\beta}$ everywhere up until full naïveté.

The increase or decrease of stage one investment in naïveté is important for the determination of the welfare effects of naïveté. Following O'Donoghue & Rabin (1999), I conduct welfare comparisons based on the inter-temporal utility U_0 of a hypothetical stage zero, which is meant to reflect long-run preferences, i.e. $W := U_0 = \beta\,(-x - y + u(x,y))$.

Proposition 2 (Welfare Implications).
Starting at full sophistication, i.e. $\hat{\beta} = \beta$, the marginal welfare-change of an introduction of naïveté is of same sign as the change of stage one investment in naïveté, i.e. $sgn(\frac{\partial}{\partial\hat{\beta}}W) = sgn(x_{\hat{\beta}})$.

For $x_{\hat{\beta}} > 0$, this welfare result holds in general only locally at full sophistication. This is because the sign of $\frac{\partial}{\partial\hat{\beta}}W$ depends on the product of $x_{\hat{\beta}}$ with the sum of a strictly positive term and the first order condition (1.2) of the fully sophisticated. Due to the second order condition (1.3), the latter turns negative for an increase in x, such that the sign of the sum becomes indeterminate when $\hat{\beta}$ (and thereby x) increases. So in case of $x_{\hat{\beta}} > 0$, it is a priori unclear whether full naïveté or only some partial degree of naïveté yields the highest welfare for the decision maker.

However, for $x_{\hat{\beta}} < 0$, the welfare result always holds globally. This is because an increase in naïveté induces a decrease of x and, hence, turns the first order condition (1.2) of the fully sophisticated positive, such that the sign of the sum persists. In this case, full sophistication is optimal.

For Cobb-Douglas utility functions of homogeneity smaller one, welfare does in fact increase globally in naïveté, up to $\hat{\beta} = 1$.

Example (Cobb-Douglas, continued)**.**
For Cobb-Douglas utility functions, welfare reads as follows.

$$W = (\alpha\mu^{\mu}\nu^{\nu})^{\frac{1}{1-\nu-\mu}}\, \beta^{\frac{(1-\nu)(\nu+\mu)-\mu\nu}{(1-\nu)(1-\nu-\mu)}}\, f(\hat{\beta})^{\frac{\mu}{1-\nu-\mu}} \left[1 - \mu\beta^{\frac{1-2\nu}{1-\nu}}\, f(\hat{\beta}) - \nu\beta\right]$$

with $f(\hat{\beta}) := \hat{\beta}^{\frac{\nu}{1-\nu}} \frac{1-\hat{\beta}\nu}{1-\nu}$*. As is shown in Appendix A.2,* $\frac{\partial}{\partial\hat{\beta}}W > 0$ *for all* $\hat{\beta} < 1$*. Hence, starting at* $\hat{\beta} = \beta$*, self-zero welfare strictly increases in naïveté* $\hat{\beta}$ *up until full naïveté.*

1.5. Conclusion

This article investigates the effects of complementarity of investments on choices and welfare of partially naïve present biased decision makers in a model with two uncommitted investment stages and a delayed reward stage. The main result shows that both stages' investments increase in naïveté iff they are sufficiently complementary in the sense that complementarity of the utility function fades slower than concavity. As present biased decision makers under-invest relative to their long-run preferences, such naïveté-induced investment increases are always at least marginally beneficial to them.

Shedding light on the upsides of naïveté is instructive when it comes to the puzzle of its persistence, despite ample opportunity to learn. The literature has focused mostly on the negative welfare effects of naïveté, such as procrastination, over-borrowing, or exploitation by subscriptions and other long-term contracts. As naïveté is very costly in these contexts, decision makers should face strong incentives to learn about it and become sophisticated, eventually. Accounting for the above investigated upsides of naïveté as well broadens the perspective, as a tradeoff between sophistication and naïveté arises. Sophistication, on the one hand, is beneficial in committed choices, because naïve decision makers pick up ineffective or even harmful commitments. Naïveté, on the other hand, is beneficial in situations of uncommitted investments with complementarity.

While it is hard to judge which of the two situations is more relevant today, the mechanism in my model could have arguably been dominant in the past and might, therefore, still affect us evolutionary. Whereas credit cards, installment plans, and health clubs are all rather recent inventions, most of our ancestors were farmers for many millennia, and farming is an almost prototypical example for a complementary multi-stage investment: Farmers have to seed, irrigate, dung, weed, and harvest before they can eventually eat, and shirking on one task diminishes returns from all the others.

A potential contemporary application of the above findings could be incentive contracts for naïve present biased agents when performance is not continuously measured or rewarded. For example, bonus contracts for sales people often depend on annual revenues, and observed sales patterns tend to be seasonal with strong year-end effects. Even though this could be driven by demand seasonality, it could also reflect naïve present bias of the sales force: Maybe they believe each month to generate a lot of revenue in the next, and hence relax until December. In this case, inducing complementarity between the various months' revenues in the bonus determination might help the sales force in partly overcoming their present bias in earlier months.

Another potential application is the measurement of naïveté in the laboratory. Existing methods (e.g. Hey & Lotito 2009) usually rely on the discrepancy between an early plan and a later decision. However, providing incentives both for early planning and later decisions is tricky. Moreover, cognitive dissonance, tastes for consistency, and limited memory can confound the findings. My model allows to identify naïveté without explicitly asking for plans by observing a sequence of investment decisions that jointly determine a distant reward, e.g. in a real-effort task. Both β and $\hat{\beta}$ could easily be backed out from a three-period investment game with complementary payoff function when the following three assumptions are imposed: First, utility is approximately linear in the experiment's (monetary) payoff; Second, time-consistent discounting is almost irrelevant for the time-span of the experiment; Third, costs of effort are linear. More complex designs could potentially allow to relax some or all of these assumptions.

Last but not least, my model naturally extends to the case of immediate rewards and delayed costs, which applies for example to joyful but health-hazardous behaviors. Again, complementarity is plausible, as each little peccadillo is likely to raise the marginal cost of the next. The occasional cigarette, drink, or junk food will probably affect future health only marginally, but it may well increase the marginal cost of subsequent "sins", as stamina deteriorates. Here, overoptimism with respect to future self control leads to underestimation of the negative effects of current consumption and thereby induces the naif to increase over-consumption relative to the sophisticate.

2. Anticipation of Prospect Theory Preferences in Dynamic Choice - Experimental Evidence*

2.1. Introduction

In dynamic risky choices, Prospect Theory preferences can induce time inconsistency. For a fixed reference point, Prospect Theory decision makers may play a sequence of lotteries in a way that is not equivalent to the ex ante most preferable compound lottery. In other words, they may fail to follow through their most preferred ex ante plan of sequential risk taking. This can be driven both by diminishing sensitivity or probability weighting: The resolution of an early lottery puts the decision maker into the gain or loss domain, where risk preferences are different than at the reference point, and it reduces the number of overall end-states, which non-linearly affects their probability weights (Barberis 2012).[1] Failing to anticipate such preference shifts can harm decision makers, e.g. when they plan to gamble just a little, but end up gambling heavily after a first loss (Andrade & Iyer 2009).

When anticipated correctly, however, Prospect Theory preferences may serve decision makers as an internal commitment device. If they can choose their future

*This chapter is joint work with Maximilian Breu (University of Munich) and Felix Peterhammer (University of Regensburg).

[1] Similarly, a change of the reference point relative to the current state induces different risk preferences.

reference points upfront, e.g. by setting a goal, they may employ their loss aversion as a punishment, e.g. for not meeting the goal, and thereby commit against self-control problems (Koch & Nafziger 2011).

Hence, anticipation of own Prospect Theory preferences as well as own reactions to domain shifts can distort dynamic risky choices. In order to inform the literature on dynamic Prospect Theory applications about how accurate an anticipation is to be expected from decision makers, we conducted a laboratory experiment.

Our participants had to make a complete contingent plan for a 4-stage investment game identical to the one in Imas (2016), and to actually play the game subsequently. Planning and playing were split into different sub-sessions that took place one week apart from each other in order to rule out cognitive dissonance or other preferences for consistency as drivers of choices in the actual play. Therefore, we view the actual investment decisions in the playing stage as purely instrumental for final (Prospect Theory) utility. Moreover, we elicited a (positive or negative) valuation for commitment to the plan. In both weeks, loss aversion and diminishing sensitivity parameters were elicited in a separate task according to the method of Abdellaoui, Bleichrodt & L'Haridon (2008).[2]

We assess our participants' anticipations of their Prospect Theory preferences based on the quality of their investment plans, which is measured as a certainty equivalent relative to never investing, i.e. relative to the benchmark of zero gain-loss-utility. Furthermore, we assess our participants' anticipations of their reactions to domain shifts based on the quality of their commitment choices, which is measured as a certainty equivalent of the original plan relative to the actually pursued investment scheme. These certainty equivalents are calculated based on the separately elicited loss aversion and diminishing sensitivity parameters. As actual investment decisions could be observed only in the realized contingencies of the plan, only a lower bound for the value of commitment, i.e. only underpayment for commitment, is identified.

We found that 19 of 43 participants in our main sample planned poorly relative

[2]This method does not allow for identification of probability weighting functions. As probability weighting is not necessary to induce inconsistencies and less common in applications than loss aversion and diminishing sensitivity, we abstracted away from it in this article.

to the benchmark of zero gain-loss-utility[3], and 14 under-appreciated the value of commitment. The average certainty equivalent of the poor plans was EUR -0.99, which amounted to more than 15% of the total endowment of EUR 6.40. The average under-valuation of the commitment was by EUR 1.80, based on deviations in the realized contingencies alone. Both errors appeared to be unrelated in our sample.

In order to identify drivers of these anticipation mistakes, we elicited various correlates: A Cognitive Reflection Test score, time spent on planning, time-stability of Prospect Theory preferences between weeks 1 and 2, and self-reported characteristics (age, gender, mathgrade, Likert scores of all Big-5 personality traits). Cognitive reflection, planning time, stability of risk preferences, agreeableness, and neuroticism jointly capture 60% of the variation in planning quality. The former three improve plan quality, the latter two impair it. However, only very little variation in commitment quality is captured by the correlates.

Overall, our findings suggest that our subjects' anticipations of their Prospect Theory preferences as well as of their reactions to domain shifts was often inaccurate. However, the positive impact of cognitive reflection and planning time suggests that higher stakes may help improve anticipation quality.

The remainder of this article is structured as follows. Section 2.2 provides an overview of the related literature, Section 2.3 introduces our experimental design and summarizes the conduct, Section 2.4 presents our analyses and findings, and Section 2.5 concludes.

2.2. Literature

Our investigation of naïveté with respect to Prospect Theory preferences forms a link between the literatures on anticipation mistakes and dynamic Prospect Theory applications.

Anticipation is well known for affecting choices in dynamic contexts when preferences are time-inconsistent. Loewenstein, O'Donoghue & Rabin (2003) show that

[3]Overall, 64 subjects participated in the experiment. However, 21 had to be excluded from the anticipation analyses because their Prospect Theory parameters could not be estimated with sufficient precision.

projection bias, an under-appreciation of own taste changes, distorts consumption-savings patterns and durable goods purchases, and contributes to the formation of addictions. It was experimentally identified also for taste changes based on hunger (Read & van Leeuwen 1998) and weather (Conlin, O'Donoghue & Vogelsang 2007, Busse et al. 2015, Buchheim & Kolaska 2017).

Another prominent example of anticipation mistakes is naïveté with respect to present bias, i.e. the underestimation of ones future desire for immediate gratification. The importance of anticipation for present biased decision makers was noted early on in this literature (Strotz 1956, Pollak 1968). O'Donoghue & Rabin (1999) show that naïveté with respect to present bias aggravates procrastination, i.e. the delay of a costly task when early execution would be beneficial. Moreover, it gives rise to exploitation by rational firms, which was shown in market settings (DellaVigna & Malmendier 2004, DellaVigna & Malmendier 2006) and for incentive contracts (Eliaz & Spiegler 2006) as well as credit contracts (Heidhues & Kőszegi 2010).

Due to an abundance of field evidence on naïve present bias (as described in the above cited articles), its clean identification in the laboratory received less attention. Hey & Lotito (2009) identified it by observing choice inconsistencies between a sequence of lotteries and their equivalent compound lottery, and found almost no sophisticates.[4]

Prospect Theory (Kahneman & Tversky 1979, Tversky & Kahneman 1992) is a static model and, hence, agnostic about anticipation. However, thanks to its success in explaining risky choice behaviors, it was applied to dynamic choice environments as well. There, Barberis (2012) shows that Prospect Theory can induce inconsistencies. For example, casino gamblers with Prospect Theory preferences have difficulties in sticking to their initial plans and may, therefore, have a demand for self-commitment, such as leaving their credit card at home. Andrade & Iyer (2009) provide experimental evidence that people do not follow through their plans for sequential bets, but tend to increase bets after losses relative to their initial plans. Conversely, Koch & Nafziger (2011) showed that loss aversion itself may

[4]Besides naïves, they identified so called "resolute" subjects who mistakenly abstained from the commitment, but still followed through their initial plans while suffering a cost from their taste changes.

serve as a commitment against self-control problems if future reference points can be deliberately chosen upfront, e.g. by setting a goal. In both applications, anticipation of own future feelings of gain and loss, respectively of their consequences for subsequent risky choices, drives decision making: It determines whether someone feels a need for commitment when gambling, and whether someone understands the effectiveness of goal-setting.

Further dynamic applications of Prospect Theory are stock trading and advertisement. There, it helps explain the equity premium puzzle and the disposition effect (Benartzi & Thaler 1995, Barberis & Xiong 2009), as well as why "bait-and-switch" strategies and other misleading advertisements are effective (Heidhues & Kőszegi 2014, Rosato 2016, Karle & Schumacher 2017). These applications assume perfect anticipation of future loss aversion.

However, experiments on labor contracts find suggestive evidence for naïveté with respect to loss aversion. Imas, Sadoff & Samek (2015) and de Quidt (2017) find that loss averse workers do not only exert higher effort under penalty contracts than under equivalent bonus contracts, but are more likely to pick them up and forgo higher outside options for them. Even though loss aversion can explain higher effort under loss contracts, it predicts a demand for compensation of exposure to the risk of a loss, which was not offered in the experiments. Our experiment provides additional evidence on anticipation mistakes with respect to Prospect Theory preferences.

The related question of anticipation of reference point changes was experimentally investigated based on the endowment effect. Loewenstein & Adler (1995) show that many people underestimate the magnitude of the endowment effect both in themselves and in others, which is confirmed by vanBoven, Loewenstein & Dunning (2003). However, as the endowment effect is explained by loss aversion in riskless choice (Tversky & Kahneman 1991), it is not clear how these findings translate to risky choice behaviors.

2.3. Experimental Design and Conduct

Our experiment consisted of a Prospect Theory parameter elicitation task (PTPE), an investment game (IG), and of measurements of potential correlates. Each subject had to show up for two sub-sessions that took place one week apart from each other.

In week 1, we asked our participants for a complete contingent plan for the investment game, elicited their willingness to pay for committing to the plan, conducted a Cognitive Reflection Test (CRT) and a short Big Five personality test, and elicited their loss aversion and diminishing sensitivity parameters. In week 2, we elicited these Prospect Theory parameters again, played the investment game, and conducted a brief survey required by the lab. The time-line is depicted in Figure 2.3.1.

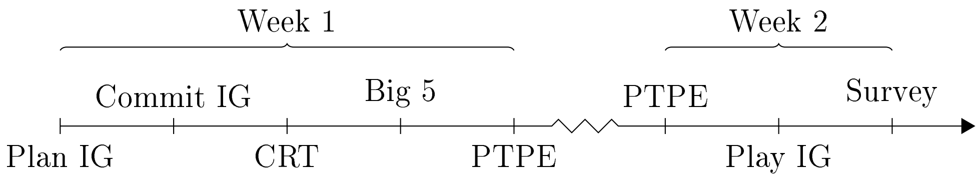


Figure 2.3.1.: Time-line of the experiment

The experiment was conducted in the Regensburg Economic Science Lab (RESL) in November 2016. We did 3 sessions with a total of 66 participants, 64 of whom showed up for both sub-sessions. Subjects were recruited from the RESL subject pool and were mostly students of the University of Regensburg from various backgrounds.

The first sub-session took on average 110 minutes, the second one 50 minutes. Our subjects earned on average EUR 28.28, including a show-up fee of EUR 5.00 per week. All payments were made at the end of the second sub-session in week 2. The experiment was programmed in z-tree version 3.4.7 (Fischbacher 2007) and organized via ORSEE (Greiner 2015).

2.3.1. Prospect Theory Parameter Elicitation

We elicited loss aversion and diminishing sensitivity according to the method of Abdellaoui, Bleichrodt & L'Haridon (2008), which relies on cognitively simple choices and requires only few observations. We abstract from probability weighting, because it is the least commonly applied feature of Prospect Theory and not necessary to generate dynamic inconsistencies. Moreover, this parameter elicitation method is not suited for estimating weighting functions due to a lack of variation in probabilities.

In line with the usual Prospect Theory notation, we write $(x, p; y)$ for a binary prospect that yields payoff x with probability p and payoff y with probability $1-p$, relative to a reference point. We assume that the reference point in our experiment is induced by the endowment and stays constant throughout. As usual, a binary prospect $(x, p; y)$ is called a gain prospect if $x > y \geq 0$, a loss prospect if $x < y \leq 0$, and a mixed prospect if $x > 0 > y$, where the ordering of x and y is without loss of generality.

Throughout this article, we assume the usual power form of the Prospect Theory value function $v(.)$,

$$v(x) = \begin{cases} x^{\alpha}, & for\, x > 0 \\ -\lambda(-x)^{\beta}, & for\, x \leq 0 \end{cases}$$

where α and β denote the curvature parameters of diminishing sensitivity in gains respectively losses, and λ the loss aversion parameter. Then, prospects $P := (x, p; y)$ are evaluated as follows.

$$U(P) = pv(x) + (1-p)v(y)$$

The parameters α, β, and λ are estimated based on simple choice data of the following three types: certainty equivalents E_G for gain prospects G, certainty equivalents E_L for loss prospects L, and offsetting "loss equivalents" E_M for mixed prospects M. The method requires that one "winning" probability p_G is fixed for all gain prospects G, also applied as probability for the positive payoff in the mixed prospects M, and equals $1 - p_L$ for the "losing" probability p_L of all loss prospects L. Together, these assumptions imply that the introduced equivalents satisfy the following equations.

$$\begin{aligned} E_G^{\alpha} &= p_G x^{\alpha} + (1-p_G)y^{\alpha} \\ -\lambda(-E_L)^{\beta} &= -\lambda p_L(-x)^{\beta} - \lambda(1-p_L)(-y)^{\beta} \end{aligned}$$

$$0 = p_G x^\alpha - \lambda p_L (-E_M)^\beta$$

We specified $p_G = p_L = 0.5$ throughout in order to reduce the cognitive burden compared to asymmetric gambles, and elicited all equivalents by a bisection method with 7 iterations.[5]

We elicited certainty equivalents for 7 gain prospects (4 in week 1), 7 loss prospects (4 in week 1), and 5 mixed prospects (3 in week 1), which yields 20 observations per subject in total. In each week, we started the elicitation with gain lotteries, continued with loss lotteries, and finished with mixed lotteries. Abdellaoui, Bleichrodt & L'Haridon (2008) found that this order is the easiest for the participants. Table B.1.1 in the appendix lists all lotteries we used.

We deviated from Abdellaoui, Bleichrodt & L'Haridon (2008) in the choice of lottery payoffs. They used *"substantial money amounts"* (multiples of EUR $1,000$), because they argued that *"for small amounts utility is approximately linear (Wakker & Deneffe 1996)"*. We used amounts in the range of EUR 1.20 to EUR 7.80, instead. The reasoning behind our design choice is twofold. First, we assess our participants' behaviors in the investment game based on these separately elicited Prospect Theory parameters and, therefore, used similar stakes to ensure comparability between the tasks. Second, the choice of a power form for the value function actually implies that curvature is biggest over small amounts. The expressed concern that utility is approximately linear for small stakes applies to subjects of Expected Utility type rather than to subjects of Prospect Theory type.

To prevent diversification and hedging, only one of all 20 lottery choices was actually played out. All inferences rely on the standard assumption of narrow bracketing, i.e. that subjects treated all decisions separately instead of viewing their choices as one big compound lottery. As the task was split across weeks,

[5] See also Abdellaoui, Bleichrodt & L'Haridon (2008) for a discussion of these two design features. The advantage of a bisection method is that participants have to make only binary choices between a risky prospect and a safe payment. In the first iteration of the certainty equivalent elicitation, the safe payment is set to the expected value of the prospect. Successively, the safe payment is increased after choices of the risky option, and decreased after choices of the safe payment. In the first iteration of the loss equivalent elicitation, the loss is set to minus the gain while the safe payment remains zero throughout. Successively, the loss is increased after choices of the risky option, and decreased after choices of the safe option.

the risk was resolved only in week 2. In order to avoid expectational spillovers to other parts of the experiment, the resolution was deferred to the very end of the experiment.[6]

2.3.2. Investment Game

The centerpiece of our experiment was a 4-round investment game borrowed from Gneezy & Potters (1997) and Imas (2016). Our subjects had to make complete contingent investment plans and could stochastically commit to them in week 1, and had to actually play the game in week 2. Eliciting plan, play, and commitment choices allows us to assess the quality of these choices, both by comparing them to one another and by evaluating them by means of separately elicited Prospect Theory parameters. This, in turn, allows us to classify our subjects as sophisticated respectively naïve.

The investment game consisted of a sequence of 4 identical decisions of how much to invest into a risky asset. Subjects received an endowment of EUR 1.60 per round and choices were tied to a EUR 0.20 grid. Following Imas (2016), the risky asset returned seven times the invested amount with probability 1/6, and zero otherwise. A subject's final payoff was the sum of investment returns and non-invested money. Hence, the risky asset yielded a higher expected return than the safe outside option. Each lottery was realized before the next decision, so that subjects were aware of their own investment record in each choice.[7]

In week 1, subjects had to make complete contingent plans for this investment game.[8] They filled in the "game-tree" line by line and each contingency's proceedings were displayed on screen. Subjects were aware that their plans were going to be implemented stochastically and how the exact probability of implementation would be determined in the subsequent commitment task. They knew that playing the investment game in week 2 could not be avoided.

We elicited our subjects' willingness to pay for commitment and determined the

[6] We pre-determined to play out the most attractive gain lottery (G6) to favor our participants. Of course, this was unknown to them.

[7] The randomization was facilitated by a die roll of a randomly selected participant per round, and subjects chose their individual success number (1-6) for themselves.

[8] Hence, the exact procedure of the game was explained in week 1 already. Understanding was ensured by three control questions.

probability of plan implementation via a choice list. In each line of the list, subjects had to choose between plan implementation plus some (positive or negative) money amount, and non-implementation. As all subjects had to both plan and play the investment game, the term implementation refers to which of the two choice-sequences became payoff-relevant.[9] The money amounts ranged from EUR -4.80 to EUR +4.80 in increasing order. Each line was drawn with equal probability, so that the share of choices in favor of plan implementation reflected its probability. Multiple switching was precluded by an error message, so that the unique switching point can be viewed as willingness to pay for commitment respectively flexibility.[10]

Whether plan or play became payoff-relevant was determined by a computer randomization at the very end of the experiment. In case of plan implementation, the winning-and-losing history of the actual play was applied to the week 1 investment plan.

A key difference between our experiment and the planning treatment in Imas (2016) is the one week gap between the stages, which reduced anchoring and related costs of deviations from the plan, such as cognitive dissonance, and allows us to view week 2 investment choices as purely instrumental for Prospect Theory utility. Furthermore, the separate elicitation of Prospect Theory parameters allows for a more detailed assessment of the observed choices.

As it was only a mild complication from what we needed anyway, we also conducted the realization treatment of Imas (2016) for the sake of replication. The analysis of this replication task is deferred to the appendix.

2.3.3. Correlates Elicitation

As our experiment classifies participants as naïve respectively sophisticated with respect to their Prospect Theory preferences, it is natural to ask for drivers of such anticipation differences. On that behalf, we elicited potential correlates that might capture some of the observed variation. These comprise a Cognitive Reflection Test (Frederick 2005), a short Big Five personality test (Schupp & Gerlitz 2014), and a brief survey which was also required by the laboratory.

[9] Only either plan or play could be paid out in order to preclude diversification and hedging.

[10] Subjects could fix plan respectively play implementation deterministically by never switching, but not avoid playing the game in week 2 when doing so.

The Cognitive Reflection Test (CRT) consisted of three short questions that involved basic calculations. Each question lent itself to an intuitive, but incorrect answer and required some reflection for uncovering the correct solution.[11] The CRT score counts the number of correct answers (0-3), each of which was incentivized by EUR 0.50.

The short Big Five personality test consisted of 16 self-assessments of how much a personal statement applied, rated on a 7-points Likert scale. It tested for the personality traits of openness, conscientiousness, extroversion, agreeableness, and neuroticism (OCEAN). Participation was rewarded with a flat payment of EUR 2.00.

2.4. Results

We measure naïveté with respect to Prospect Theory preferences and domain shifts based on the separately elicited Prospect Theory parameters. Hence, we present the result from the parameter elicitation first, and then proceed to the naïveté analysis.

2.4.1. Prospect Theory Parameters

We estimated loss aversion λ and diminishing sensitivity α and β in gains and losses, respectively, according to the usual power-specification introduced in Section 2.3.1. We followed the overall approach of Abdellaoui, Bleichrodt & L'Haridon (2008) in using a non-linear least squares estimation, but deviated slightly in its specification. Instead of estimating gain and loss curvatures α and β separately based on the certainty equivalents of the gain respectively loss lotteries alone, and instead of calculating loss aversion parameters λ deterministically for each mixed lottery and taking their median, we used all observations to estimate all parameters simultaneously. In particular, we fed the information from the mixed lottery choices also into the estimation of the curvature parameters.

[11] The most prominent example, which was also included in our experiment, is "A bat and a ball cost EUR 1.10. The bat costs EUR 1.00 more than the ball. How much is the bat?" The intuitive impulse-answer is EUR 1.00, whereas the correct answer is EUR 1.05. No time limit was imposed, but response times were tracked.

We distinguish 2×2 main specifications: estimating the parameters jointly for both weeks together or separately per week, and estimating the parameters for all participants pooled together or for each participant individually.

For each subject, we collected 20 observations of certainty and loss equivalents of the lotteries listed in Table B.1.1 in the appendix. Let E jointly denote the elicited certainty equivalents E_G and E_L of the gain and loss prospects, respectively, and the elicited loss equivalents E_M of the mixed prospects. Further, let $\mathbb{1}_j$ for $j \in \{G, L, M\}$ denote the indicator function for an observation being a gain, loss, or mixed prospect observation. Given that we set all probabilities to $p = 0.5$, our regression equation reads as follows.

$$E = \mathbb{1}_G 0.5^{\frac{1}{\alpha}} (x^\alpha + y^\alpha)^{\frac{1}{\alpha}} - \mathbb{1}_L 0.5^{\frac{1}{\beta}} ((-x)^\beta + (-y)^\beta)^{\frac{1}{\beta}} - \mathbb{1}_M \left(\frac{x^\alpha}{\lambda}\right)^{\frac{1}{\beta}}$$

In the joint estimation for both weeks together, the regression uses all observations. In the estimations of both weeks separately, the sample is restricted accordingly. The results of the pooled regressions over all participants together are summarized in Table 2.4.1 for both weeks jointly and in Table 2.4.2 for both weeks separately.

Table 2.4.1.: Pooled estimation, both weeks jointly

	Coefficient	Std.Error	t.value	p.value
α	1.455	0.035	42.07	2.311×10^{-243}
β	1.447	0.034	42.16	4.250×10^{-244}
λ	1.511	0.102	14.78	8.985×10^{-46}

In the pooled regressions, we do not find the usual S-shape of the value function, but an inverse S-shape, as our curvature parameters are bigger than 1, not smaller. However, the individual regressions reveal a lot of heterogeneity with respect to the curvature and loss aversion parameters, and show that S-shaped value functions are more common than inverse S-shaped ones. The results of the individual regressions per subject are summarized in Table 2.4.3 for both weeks jointly and in Table 2.4.4 for both weeks separately. The complete list of all participants' individual estimates is deferred to Table B.1.2 in the appendix.

Table 2.4.2.: Pooled estimation, both weeks separately

	Coefficient	Std.Error	t.value	p.value
α_1	1.358	0.075	18.06	4.401×10^{-65}
β_1	1.455	0.083	17.63	1.980×10^{-62}
λ_1	1.228	0.147	8.331	2.049×10^{-16}
α_2	1.497	0.041	36.43	9.872×10^{-200}
β_2	1.424	0.038	37.28	2.658×10^{-206}
λ_2	1.749	0.157	11.12	1.732×10^{-27}

Table 2.4.3.: Distribution of individual estimates, both weeks jointly

	Min	25%	Median	75%	Max	Average
α	0.57	0.82	0.93	1.09	3.66	1.02
	(0.00)	(0.08)	(0.13)	(0.19)	(1.55)	(0.17)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.03]	[0.00]
β	0.30	0.93	0.99	1.10	2.02	1.02
	(0.00)	(0.09)	(0.13)	(0.20)	(0.36)	(0.15)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.00]
λ	0.62	0.93	1.12	1.81	1800	30.05
	(0.00)	(0.14)	(0.25)	(0.45)	(5800)	(90.88)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.76]	[0.04]

Notes: Standard errors in parantheses, p-values in square brackets.

Even though we decided to collect more data-points than suggested in Abdellaoui, Bleichrodt & L'Haridon (2008), some of the individual level estimates do not fit satisfactory.[12] We decided to exclude these subjects in order not to corrupt the validity of further analyses.

When deciding which subjects to exclude, one could either apply a (strict) criterion on the overall model fit, or a (more lenient) criterion on each individual parameter estimate. As our further analyses heavily rely on individual parameter estimates, we did the latter. Given that we estimated the 6 individual level parameters based on 20 observations per subject only, we decided to apply the usual minimum requirement for significance of $p < 0.1$ as threshold per parameter. This

[12]Potentially, this is driven by our choice of smaller stakes.

Table 2.4.4.: Distribution of individual estimates, both weeks separately

	Min	25%	Median	75%	Max	Average
α_1	0.48	0.78	0.99	1.20	2.79	1.04
	(0.00)	(0.15)	(0.24)	(0.38)	(1.64)	(0.29)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.14]	[0.02]
β_1	0.42	0.93	1.04	1.37	3.03	1.17
	(0.00)	(0.17)	(0.28)	(0.46)	(1.18)	(0.33)
	[0.00]	[0.00]	[0.00]	[0.03]	[0.12]	[0.02]
λ_1	0.40	0.83	1.13	2.06	71.99	2.69
	(0.00)	(0.22)	(0.40)	(0.84)	(377.74)	(6.92)
	[0.00]	[0.00]	[0.02]	[0.14]	[0.85]	[0.10]
α_2	0.51	0.82	0.95	1.08	6.94	1.16
	(0.00)	(0.09)	(0.15)	(0.20)	(4.81)	(0.31)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.17]	[0.01]
β_2	0.25	0.88	0.96	1.04	1.86	0.96
	(0.00)	(0.10)	(0.14)	(0.19)	(0.42)	(0.16)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.06]	[0.00]
λ_2	0.60	0.96	1.13	1.90	1.5×10^6	2.4×10^4
	(0.00)	(0.15)	(0.29)	(0.83)	(1.5×10^7)	(2.4×10^5)
	[0.00]	[0.00]	[0.00]	[0.03]	[0.92]	[0.08]

Notes: Standard errors in parantheses, p-values in square brackets.

leads to an exclusion of 21 of our 64 subjects. Notice that this threshold is almost never binding for the curvature estimates α and β, and that no subject is excluded based on a curvature parameter alone.[13]

2.4.2. Stability of Prospect Theory Preferences

Before proceeding to the actual naïveté analysis, we investigate the time-stability of our Prospect Theory parameter estimates. We do so because we expect stability of own Prospect Theory preferences to be a driver of the quality of anticipation. Intuitively, predicting a "moving target" seems particularly hard.

Testing for the time-stability of all participants' Prospect Theory parameters

[13]In fact, only in 6 of 256 estimated curvature parameters, the p-value exceeds 0.1, and it never exceeds 0.2.

cannot be done by standard tests for coefficient (in-)equality, such as the Wald Test. The reason is that these tests do not allow for a meaningful comparison between subjects, i.e. between different "samples". They test for statistically significant differences between coefficients while neglecting their magnitudes. When investigating a single sample, strictness respectively lenience of such a test can be calibrated by choice of the confidence level. In our case, however, applying the same test (i.e. the same confidence level) to all subjects would result in rejecting stability for the subjects with the most precise estimates due to miniscule differences in their point estimates, but not rejecting stability for subjects with completely different point estimates, as long as they are sufficiently noisy.

Clearly, we need to use a notion of stability that reflects consistency of behaviors. In particular, we would not regard a subject with a very precise parameter difference of ε as inconsistent, as very small parameter differences do not translate into any behavioral differences. Also, we would not regard a subject as consistent if there was too much noise in their estimates, because a lot of noise essentially means that a subject does not behave consistently even within one week.

Hence, we designed our own test for consistency by asking how much probability weight of any week's estimate falls within a fixed band around the joint estimate for both weeks together. We impose the usual normality assumption and set the band for each of the three parameters α, β, and λ as the length of the 95% confidence interval of their respective pooled estimation.[14] This allows to account for different levels of noise in different parameters. Using the same bandwidth for all subjects guarantees that they are classified on an equal footing, whereas setting the mid-point of the band to each individual's estimate warrants that we test for consistency with oneself, rather than the population average.

We reject stability if there is at least one parameter estimate with less than 5% probability weight within this band. So if we reject stability, there exists a parameter with a 95% chance of both weeks' estimates to be more than 1.96 times the standard error of the pooled estimation apart from each other. That is, our test is lenient in the sense that we reject stability only if there is a very high chance for a behaviorally meaningful parameter difference. Furthermore, we use the minimum probability weight within this band of all 6 parameter estimates as

[14] I.e., at +/- 1.96 times the standard error of the pooled estimation.

a continuous measure of stability.[15]

We classify 11 of the 43 subjects in our main specification as non-stable. The non-stable subjects exhibited 1.9 violations of the above stability criterion on average, with an average minimum probability weight within the band of 1.6%. The stable subjects, in comparison, had an average minimum weight of 23.7%. Table B.1.3 in the appendix reports the exact probability weights per parameter for all subjects as well as the resulting minimum weights and stability classifications.

A discussion of the finding that a quarter of our subjects did not exhibit time-stable Prospect Theory preferences is deferred to Section 2.5.

2.4.3. Naïveté with respect to Prospect Theory

We assess the quality of our subjects' anticipations of their future risky choice behaviors by establishing two measures of naïveté: the quality of the investment plan and the quality of the commitment decision. The former is a measure of anticipation of Prospect Theory preferences in general, whereas the latter measures the anticipation of own reactions to domains shifts.

To assess the quality of our subjects' complete contingent plans, we interpret them as compound lotteries and calculate their certainty equivalents based on the individual Prospect Theory parameters. We use week 2 parameters, because they reflect preferences at the time at which the lottery is played. A plan is classified as decent if it has a non-negative certainty equivalent, and as poor otherwise. Clearly, this threshold is conservative as any subject could achieve zero utility by never investing, regardless of their individual Prospect Theory value function.[16]

To assess the quality of our subjects' commitment decisions, we compare their plan and play. In order not to assess the quality of the actual play based on outcome luck, we have to define an ex ante measure for the expected utility of play. We do so by viewing our subjects' play as the implementation of an alternated plan. As we can observe our subjects' behaviors only in the realized contingencies, we have to impose an assumption on their counter-factual play in all other

[15] The order of Prospect Theory parameter elicitation and investment game was reversed between weeks 1 and 2, so our stability measure could be affected by order effects.

[16] Computing a counter-factually optimal plan would be too demanding, as there are 9^{15} possible plans per subject. The zero utility plan of never investing, however, is easily conceivable.

contingencies. The most agnostic way of doing so is to assume that they would have stuck to their initial plans in all non-realized contingencies. This allows to assess whether a subject paid too little for commitment, but not whether they paid too much, because the assumption artificially limits the potential benefits of commitment. We classify a commitment decision as poor if the utility of the original week 1 plan exceeds the utility of the actual play. In this case, the certain money amount that equates utilities of committed plan and play is a continuous measure of underpayment for commitment.[17]

Table B.1.4 in the appendix summarizes all subjects' certainty equivalents of their plans relative to a utility of zero (from not investing) and, accordingly, whether a subject was a decent planner. Furthermore, it reports the difference between willingness to pay for commitment and its counter-factual minimum value (which is the certainty equivalent relative to the actual play) and, accordingly, whether a subject paid too little for commitment. Table 2.4.5 provides the most important summary statistics for our main sample.

Table 2.4.5.: Planning and commitment quality.

	Commitment			
	Decent	Poor	Total	∅ CE Plan
Plan Decent	14	10	24	0.52
Plan Poor	15	4	19	-0.99
Total	29	14	43	-0.15

Notes: Number of subjects per quality combination, CE in EUR, restricted to main sample (43 of 64).

Almost 45% of our main sample were poor planners in the sense that their plans yielded lower expected utility than never investing, based on own loss aversion and diminishing sensitivity in week 2. On average, the certainty equivalent of these poor plans was EUR 0.99 below the initial endowment. That is, these subjects would have been equally well off when paying a Euro for walking away from the

[17]In the calculation of this certainty equivalent, all payoffs are reduced by the willingness to pay for commitment (respectively increased by the willingness to accept).

experiment rather than having implemented their own plans.

Almost 33% of the main sample paid too little for commitment (respectively demanded too much), given their Prospect Theory parameters and their actual play of the investment game. Moreover, poor committers were not only slightly off, but paid on average EUR 1.80 too little.[18]

2.4.4. Drivers of Naïveté

We collected a rich set of correlates in order to tackle the question of what drives the above characterized naïveté. Specifically, we defined a stability measure of Prospect Theory preferences in Subsection 2.4.2 (ranging from 0 to 1), counted correct answers to the Cognitive Reflection Test (0 to 3), recorded planning duration (in minutes), asked for age, gender, and math-grade, and measured all Big-5 personality dimensions (on a Likert scale).

Stability of Prospect Theory preferences, correct CRT answers, and planning duration jointly capture 33% of overall variation in plan-quality (measured as certainty equivalent), and they are all individually significant. Self-reported characteristics (age, gender, math-grade) jointly capture 11% of the variation, but only gender is significant. Big-5 personality traits capture an amazing 46% of the variation, but only agreeableness and neuroticism are significant. In fact, the two alone still capture 45%.

Combining these categories reveals that PT stability, CRT score, planning time, agreeableness, and neuroticism jointly capture 60% of overall variation in plan quality, and no other correlate (combination) contributes much in addition or is even significant when added to that combination. Table 2.4.6 summarizes the regression results.

The quantitative interpretation is straight forward, as the dependent variable is measured in money. In particular, increasing PT stability from 0 to 1 increases the value of the plan, relative to never investing, by EUR 0.94, answering one

[18] Our subjects could commit deterministically to their plans by never switching in the choice list, and 11 of 64 subjects did so. As our assessment of commitment quality only accounts for underpayment, this does not affect our results. However, 3 subjects did deterministically commit to their play, and 2 of them are included in our main sample. Excluding them reduces the average underpay to EUR 1.30.

Table 2.4.6.: Regression of Plan quality on Correlates

	Coefficient	Std.Error	t.value	p.value
Intercept	-0.1877	0.4756	-0.39	0.6954
PT stable	0.9428	0.4532	2.08	0.0445
CRT	0.2291	0.1110	2.06	0.0462
Time	0.1024	0.0410	2.50	0.0170
Agreeableness	-0.1003	0.0305	-3.29	0.0022
Neuroticism	-0.1108	0.0283	-3.91	0.0004

Notes: Main sample (43 observations), $R^2 = 0.6014$. *PT stable as defined in Subsection 2.4.2 (0-1), CRT as number of correct answers (0-3), Time in minutes, Agreeableness and Neuroticism in Likert points.*

additional CRT question correct increases it by EUR 0.23, and spending another minute on planning by EUR 0.10. On the downside, an additional Likert point in a Big-5 question on agreeableness (normalized, such that higher values reflect higher agreeableness) reduces the plan value by EUR 0.10, and an additional Likert point on more neuroticism does so by EUR 0.11.

The first three effects seem intuitive. Being more consistent in one's own Prospect Theory preferences makes it easier to predict future tastes and, hence, to plan accordingly. Scoring higher on the CRT suggests a more thorough handling of cognitive tasks, in particular of our planning stage. More time spent on planning probably reflects higher effort, which should improve plan quality as well.

The effects of the personality dimensions, however, are less clear. Neuroticism points toward a higher vulnerability in general, so more neurotic subjects may face smaller upsides and bigger downsides from the investment game in general. The negative effect of neuroticism on plan quality suggests that the neurotic subjects did not sufficiently account for this vulnerability in their planning decisions. Agreeableness is foremost a social trait, so its effect on individual planning capability remains opaque.[19]

[19] A potential channel could be a distaste for non-interactive tasks in general and an accordingly lower experience or effort level for such tasks.

A similar analysis of the various correlates' effects on the quality of commitment is inconclusive. The main takeaway from the observed commitment decisions is that they do not significantly correlate with plan quality.

2.5. Conclusion

This article experimentally investigates quality of anticipation of Prospect Theory preferences in general and of reactions to shifts into the gain or loss domain in particular. Almost half of our subjects were poor planners in a sequential investment game which points to a wrong anticipation of own future Prospect Theory preferences in general. Almost a third of our subjects undervalued a commitment option, which points to an under-appreciation of own reactions to domain shifts.

Our measures of anticipation quality rely on separately elicited loss aversion and diminishing sensitivity parameters. We interpret our data as if these parameters reflected true preferences, and deviations thereof in the investment game were mistakes. However, the opposite interpretation that decisions in the investment game reflected true preferences does not qualitatively change the results. Then, the measured inconsistencies between investment game and Prospect Theory parameter elicitation task imply a mistake in the latter, which was incentivized as well. Furthermore, inconsistencies between the tasks can never be rationalized by hedging, as their payoffs were determined via independent randomization. If none of the tasks reflected true preferences, the mistake would be even larger than suggested.

Besides cognitive ability and planning effort, our measure of Prospect Theory preferences' time-stability between weeks 1 and 2 was a strong positive predictor for plan quality, i.e. of risk preference anticipation. This measure may capture either an awareness of immediate risk preferences when confronted with a risky tradeoff, or simply the attention paid to the experiment. However, it may also point to actual changes in the underlying loss aversion and diminishing sensitivity. This interpretation poses the question whether Prospect Theory, which is a descriptive model of behavior, does actually reflect non-standard preferences, which is a common view and also assumed in our article. Alternatively, Prospect Theory could reflect impulses or heuristics that are driven by current moods, emotions, and depletion, i.e. some type of self-control problem. This, of course, would give rise to a different scope of commitment than just avoiding own reactions to domain shifts.

Surprisingly, plan quality was no predictor of commitment decisions whatsoever. In particular, some subjects paid a lot to commit themselves to poor plans. This at least suggests that overpayment for commitment could have been an issue for some subjects, but unfortunately it cannot be identified with our measure of commitment quality.

An interesting question for future research is how preferences and the quality of their anticipation correlate, e.g. whether more loss averse people are also more naïve.[20] Our data cannot address this question, because the Prospect Theory parameters directly enter our naïveté measure, such that all observed correlations mechanically reflect its calculation. Another interesting question is whether anticipation mistakes with respect to Prospect Theory preferences give rise to exploitation by rational firms, similar to what has been shown for naïveté with respect to present bias.

[20] The question of correlation between preference and anticipation is still open for present bias as well, as far as the authors know.

3. Decomposing the Disposition Effect*

3.1. Introduction

The disposition effect, defined as investors' "general disposition to sell winners too early and hold losers too long" (Shefrin & Statman 1985), has become one of the most important and persistent empirical puzzles on financial markets. Although the effect is typically seen as a mistake (e.g., because "momentum" causes prices to drift over short and medium time horizons), the literature has solely identified it through a positive difference between the proportion of gains realized and the proportion of losses realized (Odean 1998). While this empirical measure shows that investors tend to realize gains more readily than losses, it remains unclear whether and to what extent gains are realized "too soon" and losses "too late" without a rational benchmark postulating what *should* be done. Surprisingly, such a benchmark has never been provided despite its immense potential to inform us not only about the nature of the disposition effect, but also about the validity of various theoretical approaches (preference or belief-based) that were proposed as explanations.

In this paper, we propose a novel experimental design that generates such a rational benchmark for investment decisions. We use this benchmark to identify whether and to what extent disposition-prone behavior is caused by actual mistakes, and to what extent this mistaken behavior occurs in the loss and gain domain, respectively. In a second step, we use this information to discriminate between various theoretical models that were proposed to explain the disposition

*This chapter is joint work with Johannes Maier (University of Munich).

effect. In particular, we speak to the long-standing debate whether such behavior roots in non-standard preferences or non-standard beliefs. Importantly, our experimental design allows us to discriminate solely based on individuals' *choices*, which directly implies that the distinction between preferences and beliefs as underlying cause *is* behaviorally relevant.

Our experimental design is distinct from previous ones in several respects, but three of them appear to be crucial. First, we require subjects to make *binary* choices between investing in one out of two risky assets and not investing. This binary nature of investments allows us to derive an individual-specific rational benchmark based on the property of first-order stochastic dominance. Although such binary choices are somewhat artificial and preclude the use of field data, they are essential in identifying the extent of *erroneous* disposition effects in gains and losses *separately* (see Shefrin & Statman's verbal definition above). Second, subjects can trade at *two* points in time only, with several periods in between. This feature allows us to separate gains from good news and losses from bad news, which is a pre-requisite for distinguishing preference-based and belief-based explanations solely based on observed behavior. Third, we use a (staircase) strategy method. Although this adds to the complexity of our design, it allows us to derive *subject-based* measures, which would have required a myriad of participants otherwise. Since theoretical models usually explain the disposition effect with individual biases, it is exactly these subject-based measures that speak to the theoretical predictions.

Our analysis decomposes the disposition effect along two dimensions. First, we split it into separate effects for gains and losses. This first decomposition yields a novel *domain-specific* measure of an erroneous disposition effect, which resembles its common interpretation much closer than the usual aggregate measure. However, since it still represents an aggregate measure within each domain, an important caveat remains: aggregate measures are too crude to draw conclusions about the prevalence of decision errors. The reason is that there are two types of potential errors investors could make, namely the error to realize when they should not (switch violations) and the error to keep an asset when they should realize it (keep violations). In both domains, one of these errors contributes to the aggregate measure of the disposition effect, while the other one counteracts it. Hence, both errors may prevail, but cancel each other in the aggregate and thus appear non-existent. It may also be the case that certain errors are more prevalent than others in a given domain, but the aggregate measure shows the opposite, simply because the frequency with which certain errors *can* be made systematically differs.[1] To address this problem, we further decompose the erroneous disposition effect within each domain according to the two types of errors that investors can make. This second decomposition yields novel domain-specific measures of subjects' propensities to make each specific error, given it is possible. Since theoretical models make predictions exactly along these lines, it is these error propensities that are essential in informing us about underlying mechanisms driving the disposition effect.

With respect to these error propensities, we find that subjects are considerably more prone to make keep rather than switch violations. These propensities are not affected by the domain in which the violation occurs, i.e., whether violations occur in gains or losses. This result is consistent with belief-based explanations, but inconsistent with prominent preference-based explanations such as realization utility (Barberis & Xiong 2009, Barberis & Xiong 2012).[2] As mentioned above, these observed error propensities may or may not induce a disposition effect in the aggregate, depending on the frequencies with which the violations are possible. In our experiment, keep violations are often possible in losses, but rarely in gains. In contrast, switch violations are often possible in gains, but rarely in losses. Hence, we should observe a disposition effect in losses as both the error propensities and frequencies work in the same direction. However, frequencies and propensities work in opposite directions in gains, so that it depends on their relative strength whether we observe a disposition effect in gains. Indeed, we find a disposition effect at the aggregate level only in losses.

[1] For instance, it may be the case that subjects' propensity for keep violations is larger than for switch violations in losses and vice versa in gains, but we still observe the opposite of a disposition effect, simply because switch violations are more often possible than keep violations in losses and vice versa in gains. Of course, the flip-side is also possible: We may observe a disposition effect, but subjects' propensity for switch violations is larger than for keep violations in losses and vice versa in gains.

[2] We further distinguish between motivated and mechanical belief distortions. In particular, our propensity results above as well as the result that subjects behave mostly rational at initial asset purchase are both consistent with models of motivated belief choice (Brunnermeier & Parker 2005, König & Maier 2017), but inconsistent with a mechanical distortion through an irrational belief in mean reversion (Odean 1998).

While our experimental design allows to discriminate between potential theoretical explanations based on choice behavior alone, we cross-validated our findings with a robustness treatment where subjects had to state their beliefs as well. There, we find that these stated beliefs diverge from the Bayesian belief, and that subjects' choices are aligned with their subjective beliefs rather than the Bayesian. This is true for observed first-order stochastic dominance violations in particular, which implies that neither keep nor switch violations are typically perceived as such. Therefore, not only subjects' choice behaviors, but also their stated beliefs are consistent with a belief-based explanation, but inconsistent with a preference-based explanation of the disposition effect.[3]

The remainder of this chapter is structured as follows. Section 3.2 introduces our benchmark and predictions, Section 3.3 describes the design and conduct of the experiment, Section 3.4 presents our results, and Section 3.5 concludes.

3.2. Benchmark and Predictions

The disposition effect was introduced by Shefrin & Statman (1985) as a tendency to hold on to losing assets too long and to sell winning assets too soon.[4] In empirical studies, it is usually measured as a difference between the proportion of gains realized (PGR) and the proportion of losses realized (PLR). Interpreting this statistic as a measure of error imposes the implicit benchmark that losses should be realized at least as readily as gains. This benchmark reflects both the original tax argument as well as momentum in asset prices. Moreover, rational explanations for selling winners rather than losers were tested and refused by Odean (1998), namely portfolio re-balancing, differences in transaction costs, and subsequent performance of kept versus sold assets.

While being intuitive, this implicit benchmark is clearly crude. A more detailed

[3]Notice that subjects' stated beliefs are closer to the Bayesian belief at initial investment decisions than at later choices, which is again consistent with a motivated but inconsistent with a mechanical belief distortion.

[4]Ever since, the disposition effect was confirmed by multiple studies in various domains, e.g. in retail brokerage accounts (Odean 1998, Ben-David & Hirshleifer 2012, Kaustia 2010), professionally managed funds (Frazzini 2006), housing markets (Genesove & Mayer 2001), and in laboratory experiments (Weber & Camerer 1998, Weber & Welfens 2008, Jiao 2017, Fischbacher, Hoffmann & Schudy 2017, Kuhnen, Rudorf & Weber 2017).

benchmark would distinguish assets that should be kept from assets that should be sold separately for gains and losses. Doing so requires relative performance measures. Intuitively, a stock that went up 1% in a strong bull market is a "winner", but still under-performed, whereas a stock that went down 1% in a strong bear market is a "loser", but still out-performed the market. Methodologically, however, it requires a forward-looking measure of expected relative performance, which is hardly obtainable in the field.[5]

Therefore, we conducted a laboratory experiment where we controlled both the objective return processes of our experimental assets and the information structure. Our experiment is designed in a way that allows for a specific benchmark on the individual choice level. Surprisingly, this is achieved not by a complication of previous designs, but by the simplifications of imposing binary choices over assets, which allows to use a first order stochastic dominance criterion of rationality, and limiting trade to two periods only, which allows to partly separate gains from good news and losses from bad news.[6]

Our benchmark allows us to compare the proportion of gains realized to the proportion of gains that should be realized (PGSR), as well as the proportion of losses realized to the proportion of losses that should be realized (PLSR). Thereby, we can test for the disposition effect in gains and losses separately. Or put differently, we can test the two original conjectures of holding losers too long and selling winners too early individually.

Prediction 1 (Disposition Effect in Gains)**.** $DEG := PGR - PGSR > 0$.

Prediction 2 (Disposition Effect in Losses)**.** $DEL := PLSR - PLR > 0$.

However, these domain specific disposition effects are still not fully informative as error measures. Suppose PGSR was 50%, and PGR as well. Then, the disposition effect in gains would be zero, but still every individual investment choice may be mistaken. Even when the exact right number of winning assets is sold, it may comprise exactly those assets that should have been kept, while the kept assets

[5] Allegedly, most real-world trade occurs precisely because market participants disagree about the prospects of an asset.

[6] Which is not to say that our experiment was simpler than others overall.

are the ones that should have been sold.[7]

As our rational benchmark is choice specific, we are able to delve deeper and distinguish four independent disposition errors, based on violations of first-order stochastic dominance: First, keep violations in gains (KVG) refer to keeping the own asset when it is in gains but should be sold. Second, switch violations in gains (SVG) refer to switching to the other asset when the own one is in gains and should be kept. Third, keep violations in losses (KVL) refer to keeping the own asset when it is in losses and should be sold. And fourth, switch violations in losses (SVL) refer to switching to the other asset when the own one is in losses but should be kept.

According to its original definition, the disposition effect refers to SVG and KVL errors only. Moreover, these errors are the drivers of our domain-specific disposition effects as well, as SVG implies gain realizations, and KVL non-realizations of losses. In fact, the other two errors point in the opposite directions and, hence, reduce the domain specific disposition effects as well as the usual empirical measure. Consequentially, both our domain-specific disposition effects as well as the usual empirical one are suitable measures of decision errors only if KVG and SVL are small compared to SVG and KVL.

The specific pattern of these four individual disposition errors is particularly informative for the theory as well. Due to its robust identification, the disposition effect received a lot of attention, so that several behavioral models were proposed as explanations. These models utilize either of two channels: non-standard preferences, or non-standard beliefs. This distinction, however, is not merely technical, but gives rise to different welfare judgments of the disposition effect. If preferences drove the effect, investors would be aware of, and willing to bear, the instrumental costs of their trading behavior. If non-standard beliefs drove the effect, investors would be unaware of, and probably unwilling to bear, these instrumental costs. In the latter case, investors would exhibit disposition errors due to a lack of understanding of the consequences of their choices, and could potentially be de-biased by information or education, and would appreciate it.

While models of both types were designed to explain the aggregate disposi-

[7] Clearly, the 50% case is the uninformative extreme. For all other values of PGSR, DEG carries at least some information.

tion effect and, hence, predict it, they differ in their predictions with respect to the above introduced individual disposition errors. Moreover, the models speak to individual decision making. Hence, their predictions refer to individual error propensities, i.e. to conditional probabilities of making an error, conditional on the possibility of the error. Therefore, the short-hand notations KVG, SVG, KVL, SVL refer in the following predictions to population averages of the individual error propensities.

The leading preference-based explanation combines reference dependent risk preferences with mental accounting and realization utility (Barberis & Xiong 2009, Barberis & Xiong 2012). The reference point is assumed to be the original purchase price, so that gains and losses carry the utility. However, this utility is felt only when its respective mental account is closed, which is assumed to be asset-specific. Hence, the pleasure of a gain is only enjoyed when the gain is realized, and the pain from a loss is only suffered when the loss is realized. By its finite nature, our experiment enforces realization at its end, but subjects can opt for an interim realization in their second trading period by switching to the other asset. If the reference dependent value function exhibits diminishing sensitivity as usually assumed (Kahneman & Tversky 1979), tradeoffs between maximizing interim realization utility and maximizing final realization utility can arise. When holding a loser that should be sold according to the rational benchmark, it may be worthwhile to keep it even if its prospects are stochastically dominated, as keeping it allows to avoid the dis-utility from interim realization, while diminishing sensitivity induces an asymmetry in the perceived up and downsides of keeping for the final realization. Analogously, when holding a winner that should be kept for instrumental reasons, reaping the interim realization utility may be worth the cost of switching to a worse asset for the remaining periods.

Hence, realization utility systematically allows for SVG and KVL mistakes, but not for KVG and SVL mistakes, because the instrumental costs of the former may be worthwhile to bear in order to enjoy interim realization utility or avoid interim realization dis-utility, while the latter mistakes would go along with avoiding interim realization utility or bearing interim realization dis-utility. Allowing for uniform noise, the following prediction obtains.

Prediction 3 (Preference Based Disposition Effect).
$KVG = SVL < SVG$ and $KVG = SVL < KVL$.

A common belief-based explanation for the disposition effect assumes an irrational belief in mean reversion, i.e. the belief that losers will recover and winners will come down again. In our experiment, price increases relative to the other asset are positive signals of asset quality. Hence, belief in mean reversion can be modeled as inverse (Bayesian) updating in our setup. While the Bayesian posterior for an asset adhering to the higher return process increases if the asset's price increases relative to the other asset's price, the distorted belief of the mean reverter decreases. Hence, mean reverters' decisions are guided by the flip-side of the rational benchmark, as they perceive first-order dominated assets to be dominant, and first-order dominant assets to be dominated. This predicts all four disposition errors with 100% or, when accounting for uniform noise, with equal propensity.

Prediction 4 (Mean Reversion Based Disposition Effect).
$KVG = SVG = KVL = SVL > 0$.

Beyond these "classic" preference-based and belief-based explanations, it was noted in the literature that the two could interact. Specifically, investors could have motivated beliefs, either because they derive anticipatory utility from the beliefs themselves (Brunnermeier & Parker 2005), or because their asset ownership induces them to under-react especially to unfavorable news (König & Maier 2017). Either way, this induces investors to view their assets overly rosy after bad news. Therefore, motivated beliefs decision makers only exhibit KVG and KVL mistakes, but no SVG or SVL mistakes, as they sometimes keep their asset when they should not, but always keep it when they should. When controlling for informativeness in gain and loss states, KVG and KVL should be equally likely, so that under uniform noise the following prediction obtains.

Prediction 5 (Motivated Beliefs Based Disposition Effect).
$KVG = KVL > SVG = SVL$.

It is important to note that this prediction is perfectly consistent not only with the above introduced disposition effect in the loss domain, but also with the usual empirical measure of the disposition effect, PGR-PLR>0. Moreover, this is not a peculiarity of our experimental design, but holds true in general. Intuitively, when prices carry at least some information about asset quality, the likelihood that keeping a loser is a mistake is systematically higher than the likelihood that switching is a mistake. And vice versa, switching is more likely a mistake for winners. Hence, the same propensity in gains and losses of making a keep mistake translates into a bigger share of losers that are mistakenly kept than of winners that are mistakenly kept.

While the disposition effect refers to selling decisions of own assets, behavioral models speak to the original purchase decisions as well. The above described non-standard preferences of the realization utility model clearly affect risk preferences at initial purchase if decision makers are foresighted, but do not allow for irrationality. Hence, realization utility types do not exhibit first-order violations at the initial purchase. As motivated belief types' belief distortion depends on their asset ownership, they also behave according to the standard model and do not exhibit first-order violations at the initial purchase. By contrast, an irrational belief in mean reversion does not depend on asset ownership and affects the initial asset purchase the same way as later selling decisions. Hence, a mean reverter exhibits all first-order violations already at the initial purchase. Moreover, initial purchase decisions allow to identify information unresponsive behaviors as well, i.e. heuristics that imply the purchase of a specific asset regardless of information. The most prominent heuristics in this context are price heuristics, i.e consistently choosing the more or less expensive asset. Since we calibrated our experiment such that prices never cross, we can identify heuristic types as those who always choose the same asset, which is precluded for all other types. Taken together, the following predictions for the initial asset purchase obtain under the usual uniform noise assumption.

Prediction 6 (Rational Purchase)**.**
Choice frequency of an asset increases in its Bayesian posterior of adhering to the higher return process.

Prediction 7 (Mean Reversion Purchase)**.**
Choice frequency of an asset decreases in its Bayesian posterior.

Prediction 8 (Heuristic Purchase).
Choice frequency of an asset is flat across its Bayesian posteriors.

Our experiment is designed for testing all above predictions.

3.3. Experimental Design

In our experiment, subjects faced a series of investment decisions between two risky assets or not investing. The prices of the risky assets were determined exogenously by independent binomial processes with known distributions. However, it was unknown to the subjects which of the assets adhered to which of the processes. The subjects received an initial endowment for investing and informed their decisions by observing price realizations of the processes.

Asset Prices

The prices of the two risky assets A and B were simulated for 10 periods $t \in \{1, ..., 10\}$ according to independent binomial processes H and L (high and low). Which asset adhered to which process was randomly assigned on a per subject level at the start of the experiment, but subjects learned the outcome of this randomization only at the end.

From period to period, each process either appreciated 30% or depreciated 20% of its current value. Process H appreciated with 55% probability, process L with 45%, and they depreciated with the respective counter-probabilities. Figure 3.3.1 illustrates the binomial tree of the price "lottery" of asset A for one period from the perspective of $t = 0$.

Our participants received an initial endowment of 20,000 EMU for their investment decisions at an exchange rate of 2,500 EMU $\widehat{=}$ EUR 1. The asset prices were initiated in period $t = 0$ at $p_A^0 = 200$ EMU and $p_B^0 = 5$ EMU.

All subjects were informed that they could not avoid observing the asset prices for all 10 periods as well as learning which asset adhered to which process eventually, regardless of their investment decisions.

We used relative instead of absolute price changes in order to make sure that the asset that followed process H was always the more attractive one for a rational

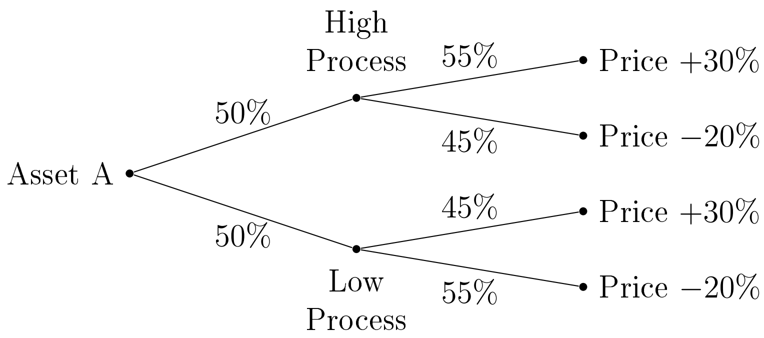


Figure 3.3.1.: Binomial tree of "lottery" A for one period at $t = 0$.

investor, regardless of the absolute price level.[8] Moreover, relative price changes guaranteed that asset prices always remained positive, regardless of the realizations of the processes and regardless of the number of periods.

The choice of appreciation and depreciation probabilities close to 50% guaranteed that Bayesian updating with respect to incoming information was moderate and almost linear for the first couple of periods. Furthermore, it corresponded to the medium calibration (i.e., + and -) of the seminal experiment by Weber & Camerer (1998).

The high percentage price changes, on the other hand, made sure that investment outcomes represented a substantial fraction of our subjects' earnings, so that picking the right asset was of considerable relevance for them. Even though the difference between process H and process L may seem faint at first, it is in fact substantial: The high process yielded three times the expected return of the low one per round, and compounding amplified this effect in the course of time. In order to make this difference clear to our subjects, they had to compute the expected returns of both processes themselves in a control question.[9]

[8] With absolute price increments, the asset following the low process would be more attractive if it was only cheap enough. For example, assume the assets would appreciate 30 EMU instead of 30% and depreciate 20 EMU instead of 20%. If the high asset traded at $p = 400$ EMU and the low one at $p = 100$ EMU, investing 400 EMU in the former would yield an expected return of 7.5 EMU per round, whereas the latter would yield 10 EMU.

[9] Precisely, the better process yielded an expected return of $0.55 \times 0.30 - 0.45 \times 0.20 = 7.5\%$ per round, whereas the worse process yielded only $0.45 \times 0.30 - 0.55 \times 0.20 = 2.5\%$. Most of the subjects managed to calculate these returns, the others got some hints by the instructors until they came up with the correct solutions.

The asymmetry of gains and losses warranted attractiveness of risky investments also for risk averse participants and for uninformative priors, which was important for our experiment as only asset holders could exhibit a disposition effect.[10]

The substantial difference in initial prices p_A^0 and p_B^0 made sure that our subjects reflected on the (ir-)relevance of absolute prices. Moreover, it guaranteed that $p_A^t > p_B^t$ for all $t \leq 6$ in all possible price realizations, which allowed to test for irrational price heuristics.

Investment Choices

Our participants could invest only in periods $t = 2$ (Choice I) and $t = 6$ (Choice II). Both choices were "all or nothing", i.e., subjects had to invest their entire wealth either in asset A, in asset B, or not at all, but could not build a portfolio. To inform their decisions, they observed asset prices for two pre-periods before Choice I and another four interim-periods before Choice II. Their earnings from the experiment were determined by another four end-periods as the value of their investments at $t = 10$. Figure 3.3.2 depicts the time-line of investment choices.

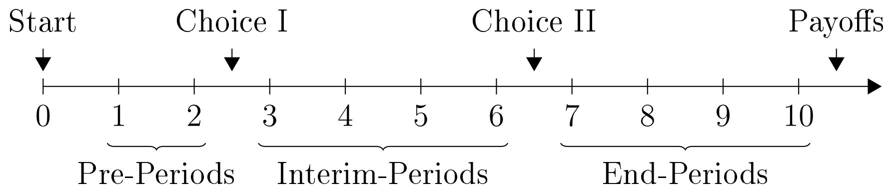


Figure 3.3.2.: Timing of investment choices

When subjects decided not to invest, they kept their current wealth at zero interest. Allowing them not to invest came at the cost of losing some observations for the disposition effect, but improved comparability with the literature as well as realism. Furthermore, it probably reduced noise as subjects who did not want to invest would most likely have behaved in a more random, less systematic way if they were forced to than subjects who actually wanted to invest.

Demanding "all or nothing" decisions precluded diversification motives and allowed us to use first-order stochastic dominance arguments in the analysis of the experiment. As a side-effect, it also simplified the decisions.

[10] Almost 90% of our subjects ended up holding a risky asset at their second choice.

Strategy Method

Even though there were only two trading periods, our subjects were not done with merely two decisions. Instead, they were asked for Choice I and Choice II multiple times via a strategy method. That is, they had to make their investment decisions conditional on various possible contingencies of the choice environment. Only after they decided, one contingency was randomly drawn and their respective choice was implemented.

The possible contingencies of our investment choices were the realizations of price paths that could be observed when making a decision. For each asset, there were 2^t possible price paths within t periods and, hence, $2^t \times 2^t = 2^{2t}$ price paths combinations for both assets. In particular, there were $2^2 = 4$ possible price paths per asset at $t = 2$ and, hence, 16 possible price paths combinations for both assets in Choice I. Similarly, there were $2^6 = 64$ possible price paths per asset at $t = 6$ and, hence, 4,096 possible price paths combinations for both assets in Choice II.[11]

For the disposition effect, however, only prices at times of trade were relevant, as they determined whether and by how much an investment was in gains or losses, and what the outside options of the investors were. Fortunately, multiple price paths could result in the same final price, which significantly reduced the number of relevant contingencies. In general, the number of ups and downs uniquely determined an asset's final price, regardless of the order of ups and downs.[12] In particular, there were $t + 1$ possible final prices per asset after t periods and, therefore, $(t + 1) \times (t + 1)$ price combinations, or "states". Specifically, there were $3 \times 3 = 9$ possible states in Choice I and $7 \times 7 = 49$ possible states in Choice II.[13]

Furthermore, multiple states could result in the same Bayesian posterior with respect to which asset adhered to which process. In fact, the Bayesian posterior depended solely on the difference in the number of ups between asset A and asset B, which we call Δ. In particular, if both assets had the same number of ups, i.e., if

[11] As initial prices were fixed, each price path can be expresses as an ordered sequence of ups U and downs D of the price. So for example, the 4 possible price paths per asset in Choice I were UU, UD, DU, and DD.

[12] Due to commutativity of the multiplication. For example, UD and DU both result in a final price that is $1.3 \times 0.8 - 1 = 0.8 \times 1.3 - 1 = 4\%$ above the initial price.

[13] For a fixed number of periods t, each state can be expressed as a tuple of ups per asset, e.g. (2U,1U) for the state where asset A appreciated twice and B once.

$\Delta = 0$, the Bayesian posterior was equal to the prior of 50%. Tables 3.3.1 and 3.3.2 depict the "state matrix" of Choice I, the former with the Bayesian posteriors for asset A adhering to the better process, the latter with the equivalent $\Delta's$. Notice that states on the same diagonal have the same Bayesian posterior, respectively Δ.

Table 3.3.1.: Bayesian posteriors per state, Choice I.

		Asset B		
		2U	1U	0U
Asset A	2U	50%	60%	69%
	1U	40%	50%	60%
	0U	31%	40%	50%

Table 3.3.2.: Δ per state, Choice I.

		Asset B		
		2U	1U	0U
Asset A	2U	0	1	2
	1U	-1	0	1
	0U	-2	-1	0

Each subject had to report their investment decisions for 5 contingencies (i.e., price paths combinations) in Choice I and up to 13 contingencies in Choice II. In Choice I, they faced one contingency for each possible posterior, i.e., for each diagonal of the state matrix. The exact price paths combination that was presented for a specific diagonal was randomly determined according to the true (conditional) distribution. The diagonals were asked in the order $\Delta = 0$, -1, -2, 1, 2.

After reporting all 5 decisions of Choice I and before proceeding to Choice II, one of the 5 diagonals was randomly drawn for each subject individually according to the true (unconditional) distribution. Then, each subject was informed about the outcome of this draw, and their respective investment decision was implemented on their behalf.

Therefore, only those contingencies at $t = 6$ that were consistent with a subject's Choice I realization could still materialize in Choice II. These were $2^4 \times 2^4 = 256$ price paths combinations and $(4+1)\times(4+1) = 25$ states with a total of $2\times4+1 = 9$ different Bayesian posteriors. Due to our calibration, an asset was in gains when it increased at least twice within four periods. Hence, there were 15 gain states with 7 different Bayesian posteriors and 10 loss states with 6 different Bayesian posteriors among these 25 states for an asset holder at $t = 6$.[14] Table 3.3.3 illustrates the

[14] Clearly, there are no gains or losses for non-asset holders.

Choice II state matrix of a subject for whom state (2U,1U) was realized in Choice I and who held asset A according to their first choice. The percentage numbers in the matrix refer to the Bayesian posteriors again and the double line separates gain states from loss states.

Table 3.3.3.: Bayesian posteriors per state in Choice II for Choice I realization of (2U,1U) and holding asset A.

		Asset B					
		5U	4U	3U	2U	1U	
Asset A	6U	60%	69%	77%	83%	88%	Gain States
	5U	50%	60%	69%	77%	83%	
	4U	40%	50%	60%	69%	77%	
	3U	31%	40%	50%	60%	69%	Loss States
	2U	23%	31%	40%	50%	60%	

A couple of remarks is in order. First, the bottom right cell of the Choice II state matrix is precisely the state that was realized in Choice I. Clearly, the number of ups per asset could remain constant if no asset would appreciate any further, but it could never decrease. Second, the uninformative diagonal (i.e., with a Bayesian posterior of 50%) is not the main diagonal of the state matrix, because Δ in Choice I was not zero. Third, for subjects who held asset B, the split in gain and loss states would be vertical, not horizontal, but the number of gain and loss states and the variation in posteriors would remain the same.

In Choice II, subjects were asked for a contingency per Δ again, but separately for a gain and a loss state whenever applicable.[15] Again, the uninformative diagonal $\Delta = 0$ was asked first, followed by the negative Δ's in descending order and, then, the positive Δ's in ascending order. If both gain and loss states existed on a diagonal, a contingency of a gain state was asked first. As in Choice I, the exact price paths combination that was presented for a specific diagonal was randomly determined according to the true (conditional) distribution.

In order to reduce the number of decisions per subject, we applied "stopping

[15] A separation of gain and loss states only existed for asset holders, and some diagonals are completely in either the gain or loss domain, see Table 3.3.3.

rules". Specifically, as soon as a subject chose a first-order stochastically dominant asset in a contingency, no further contingencies with more extreme posteriors were asked within that domain (i.e., within gains or losses). For example, if the decision maker in Table 3.3.3 switched to asset B at state (3U,4U), the bottom left corner of the state matrix was not explored any further. That is, no loss states on the diagonals $\Delta = -2$ and $\Delta = -3$ were asked.

In order not to discriminate in Choice II between mean reversion and heuristic types versus subjects that were rational in Choice I, we applied history specific stopping rules. Subjects who revealed to be mean reverters in Choice I faced the exact opposite stopping rule as subjects who were rational in Choice I. Specifically, as soon as such a subject chose a first-order stochastically dominated asset in a contingency, no further contingencies with more extreme posteriors were asked within that domain. Subjects who revealed to be heuristic types in Choice I were stopped at informative posteriors as soon as they picked the asset of their Choice I heuristic.

After reporting all Choice II decisions (however many), one of the actually asked contingencies was randomly drawn for each subject according to the true (conditional) distribution of their respective (half-)diagonals. Each subject was informed about the outcome of their draw, and the respective investment decision was implemented on their behalf. Then, another four periods of the price processes were simulated and final wealth at $t = 10$ determined the subjects' payoffs from the experiment.

The realizations of Choice I and Choice II according to the true distributions of the diagonals guarantees incentive compatibility. Even though the "better" asset was randomly pre-specified for each subject before the experiment, it made sense to respond to the information that was contained in a contingency's price history, because more likely histories were drawn with higher probabilities.

There were several reasons to use a strategy method instead of letting subjects trade in each period. First, it increased comparability between subjects as they faced more similar decisions despite an individual level outcome randomization. Second, it gave us more control over the type of states that subjects faced. In particular, it made sure that we could observe investment decisions in the states that were relevant in our disposition error analysis for each subject. Third, the fact that subjects observed price paths that span several periods instead of only one whenever they made a decision allowed us to disentangle "good news" from "gains". For example, a subject's own asset could have experienced two ups and two downs between periods $t = 2$ and $t = 6$ and, therefore, be in gains. At the same time, the other asset could have had four ups, which is "bad news" in the sense that the Bayesian posterior with respect to how good one's own asset is must have declined. Fourth, observing decisions for various states allowed us to calculate an "empirical" disposition effect per subject by weighting these states with their respective probabilities of occurrence. Furthermore, this weighting allowed us to distinguish empirical error frequencies from individual error propensities.

As the choices per contingency differed only in their respective price histories, they must have looked very similar to our subjects. Hence, we faced a tradeoff between the number of choices per subject and the amount of concentration and effort our subjects would put into each. Our stopping rules were designed to reduce the number of decisions per subject without losing relevant observations by eliminating only those contingencies where there was little doubt about how a subject would have behaved, given their Choice I.

Using an interim realization of Choice I before proceeding to Choice II instead of a strategy method with only one realization at the very end guaranteed that each subject knew for sure which asset they held when it came to Choice II, and that each subject held an asset that they picked for themselves deliberately. Both features are crucial properties of real world situations where disposition effects are observed and are, therefore, incorporated in our experiment. Moreover, it had the practical advantage of substantially reducing the number of possible contingencies in Choice II.

Belief Treatments

In our two belief treatments, the baseline experiment was augmented with a belief elicitation. At each investment decision, subjects had to report a probability estimate of which asset was following which process. More precisely, they had to report a probability estimate of how likely it was that asset A adhered to process H. One belief treatment was incentivized, the other one not.

The incentivization of the belief elicitation was conducted according to the method of Karni (2009), which is a revelation mechanism for subjective beliefs. Each participant had to report their subjective probability estimate $\mu \in [0,1]$ for asset A adhering to process H. Then, a random number r was drawn according to a uniform distribution on $[0,1]$. If $\mu \geq r$, the decision maker received a positive price $P > 0$ if A did in fact adhere to process H, and no price otherwise. If instead $\mu < r$, the decision maker was awarded a lottery with chance r to win P and chance $1-r$ to win nothing. We set $P =$ EUR 2 and elicited probabilities on a 5% grid. The non-incentivized belief elicitation allowed us to control for cross-hedging between investment choices and probability estimates.

Experimental Conduct

Participants received detailed instructions (see appendix), could start a test-run of the experiment only after carefully reading the instructions[16], and had to complete 9 control questions that were individually checked by the instructors before the actual experiment was started simultaneously for all participants. After the experiment, a questionnaire asked for basic demographics, a self-assessment of risk-preferences, and a simple probability calculation to test for understanding of Bayes' Rule.

The experiment was conducted in the *Munich Experimental Laboratory for Economic and Social Sciences (MELESSA)* in July 2017. We did 10 sessions with 218 subjects in total: 6 sessions of the baseline treatment with 137 subjects, 2 sessions of the incentivized belief treatment with 40 subjects, and 2 sessions of the non-incentivized belief treatment with 41 subjects. Most participants were university students of various backgrounds, 62.8% were female. Each session took 50 to 60 minutes and our subjects earned EUR 14.69 on average, including a show-up fee of EUR 4.00. The experiment was programmed in oTree (Chen, Schonger & Wickens 2016) and organized via ORSEE (Greiner 2015).

[16] A code was hidden in the instructions which was required by the program-interface for proceeding to the test-run.

3.4. Results

3.4.1. First Choice - Initial Asset Purchase

As the disposition effect refers to selling decisions with respect to own assets, it requires holding an asset in the first place. Moreover, as it refers to "winning" and "losing" assets, it requires that own assets have a reference value. Since own assets are typically acquired by a previous purchase, the purchase price usually serves as reference value. Our experiment conforms to this convention by letting subjects purchase an asset in Choice I of the experiment, and allowing them to sell it off again in Choice II.

Besides being a prerequisite for the Choice II analysis, Choice I decisions are interesting to investigate by themselves. As subjects do not yet hold an asset in Choice I, they should not have any motivated attitudes with respect to any of the assets. This allows us to test for an irrational belief in mean reversion. Moreover, Choice I decisions allow for identification of price heuristic types, as asset A was more expensive for all possible histories until $t = 2$. Table 3.4.1 reports the buying frequency of each asset in Choice I per Bayesian posterior of asset A adhering to the high return process, respectively per Δ.

Table 3.4.1.: Buying decisions per Bayesian posterior in Choice I.

Bayesian	30.9%	40.1%	50.0%	59.9%	69.1%
Δ	-2	-1	0	1	2
Asset A	29.8%	34.9%	45.9%	63.8%	72.9%
Asset B	66.5%	58.7%	38.5%	28.4%	22.0%
No Asset	3.7%	6.4%	15.6%	7.8%	5.0%

Notes: Percentage share of subjects per asset choice, 218 subjects in total; Δ as difference of ups between assets A and B, Bayesian posterior for asset A adhering to the higher return process.

Table 3.4.1 shows that buying decisions in Choice I closely track the Bayesian posterior. The percentage shares of subjects investing in asset A resemble the Bayesian posteriors of asset A being the good asset. Thus, when starting with an uninformative prior, subjects invest more in an asset if its signal to be the good asset becomes stronger. This shows that subjects buy the asset that has seen more ups (the more so, the larger the difference in ups). The decision not to invest is most frequent when the signal, and hence the Bayesian posterior, is uninformative. As expected, these frequencies decrease with the informativeness of the signal.

Table 3.4.2 reports the buying frequency of each asset in Choice I per Bayesian posterior, conditional on realization. Realization frequencies per Δ are reported as well. When looking only at realized decisions (which are exactly the Choice I decisions our Choice II analysis is based on), the qualitative pattern remains the same. In fact, picking the wrong asset is even less common when we only consider realized (instead of all) decisions.

Table 3.4.2.: Realized asset choices per Bayesian posterior, Choice I.

Bayesian Δ	30.9% -2	40.1% -1	50.0% 0	59.9% 1	69.1% 2
Asset A	28.1%	28.6%	48.0%	70.6%	72.4%
Asset B	68.8%	60.7%	38.0%	17.6%	20.7%
No Asset	3.1%	10.7%	14.0%	11.8%	6.9%
Rlz. Frq.	14.7%	25.7%	22.9%	23.4%	13.3%

Notes: Percentage share of subjects per realized asset choice, conditional on Δ, for 218 subjects in total; Realization frequencies per Δ; Δ as difference of ups between assets A and B, Bayesian posterior for asset A adhering to the higher return process.

Conditional on investing in an asset, a binomial test per Δ reveals that subjects systematically buy the asset that has seen more ups for any informative posterior ($\Delta = -2$: $p = 0.0000$, $\Delta = -1$: $p = 0.0004$, $\Delta = 1$: $p = 0.0000$, $\Delta = 2$: $p = 0.0000$). Moreover, buying behavior cannot be distinguished from random choice when the Bayesian posterior is uninformative ($\Delta = 0$: $p = 0.2688$).

Result 1 (Rational Behavior in Choice I)**.** *If signals are informative, subjects buy the asset that is more likely to adhere to the higher return process, according to the Bayesian posterior. The frequency of subjects buying a given asset monotonically increases in its Bayesian posterior of being the good one. The frequency of subjects not investing is highest when the Bayesian posterior is uninformative and decreases with the informativeness of the Bayesian posterior. If signals (i.e. the Bayesian* *posteriors) are uninformative, subjects' buying decisions cannot be distinguished from random choice.*

Result 1 shows that Choice I behavior is mostly rational, i.e. in line with Prediction 6. Per reverse conclusion, it is inconsistent with two potential other explanations. First, Result 1 is inconsistent with a price heuristic, which would predict a flat relationship between investments in a given asset and the Bayesian posteriors (Prediction 8). Second, Result 1 is inconsistent with an irrational belief in mean reversion, which would predict that subjects buy the asset that has seen more *downs*, and the more so the larger the difference in downs (Prediction 7). As this is the exact opposite of the Bayesian prediction, it is inconsistent with our Choice I finding.

Notice that Result 1 rejects an irrational belief in mean reversion as a potential explanation solely based on *behavior*. So far, the literature has mostly tried to disentangle this prominent belief-based explanation through stated beliefs, despite the frequently discussed problems that are associated with belief elicitation. An exception is Weber & Camerer (1998), who also try to identify it by investigating buying decisions. However, as they look at averages of buying decisions over all investment periods (and because their investments do not have a binary nature), their measure is confounded by subjects' motivated attitudes towards assets they already hold. In contrast, by considering the first investment decision only, we isolate buying decisions that are not yet motivated by previous behavior. This feature allows us to cleanly reject a belief in mean reversion as potential explanation.

3.4.2. Second Choice - Keep or Switch

The analysis of this section is based on all subjects who invested in one of the risky assets in their realized Choice I contingency, and thus $n = 196$. Table 3.4.3 presents the empirical distribution of subjects' Choice II decisions conditional on whether these choices violate the first-order stochastic dominance property. For instance, a_G expresses the average subject's probability that a gain state realizes, where she keeps her asset and keeping it is first-order stochastically dominated.

Table 3.4.3 shows that subjects typically invest in one of the assets: across both domains, the proportion of no investments taking place is only 4.93%. Table 3.4.3

Table 3.4.3.: Empirical distribution of asset choices per benchmark

Gains (G) Losses (L)	First-order violation if keep	First-order violation if switch	No first-order violation
Choice: keep asset	$a_G = 0.0705$ $a_L = 0.0673$	$b_G = 0.3870$ $b_L = 0.0553$	$c_G = 0.0636$ $c_L = 0.0351$
Choice: switch asset	$d_G = 0.0437$ $d_L = 0.0999$	$e_G = 0.0534$ $e_L = 0.0101$	$f_G = 0.0369$ $f_L = 0.0279$
Choice: don't invest	$g_G = 0.0061$ $g_L = 0.0024$	$h_G = 0.0152$ $h_L = 0.0059$	$i_G = 0.0141$ $i_L = 0.0056$

Notes: The stated proportions approximate the empirical distribution when abstracting away from the strategy method, meaning they incorporate the frequency with which certain events happen. For example, a_G expresses the average subject's probability that a gain state realizes, where she keeps her asset when keeping it violates the first-order stochastic dominance property. Likewise, b_L measures the average subject's probability that a loss state realizes, where she keeps her asset and keeping it does not violate first-order dominance. The fourth column ("No first-order violation") represents all choices made for uninformative Bayesian posteriors, where first-order dominance violations do not exist. The table excludes all subjects who did not invest in their first choice realization, so that $n = 196$. Let domain $i \in \{G, L\}$, then $\sum_i a_i + b_i + c_i + d_i + e_i + f_i + g_i + h_i + i_i = 1$.

also shows that across both domains, the proportion of states in which first-order violations are not possible is 21.28%. These are all the states in which the Bayesian posterior is uninformative ($\Delta = 0$). While these states will become important for the discrimination of subjects' underlying motives, states in which no action is erroneous cannot contribute to our error analysis of this section. Similarly, the action of not investing is never a first-order violation, and cannot contribute to our error analysis. Therefore, we exclude the last column and the last row of Table 3.4.3 from the subsequent analysis.

Table 3.4.4 normalizes the proportions of Table 3.4.3 separately for gains and losses. In each domain, assets are either realized (i.e., switched) or not realized (i.e., kept). Whether or not these assets *should* be realized is fully determined by the property of first-order stochastic dominance. Thus, whenever an action contradicts what should be done, we identify it as erroneous behavior. Notice that such erroneous behavior constitutes the lower bound of possible mistakes that can be made as it only requires monotonicity and allows for any risk attitudes.

Table 3.4.4.: Asset choice per benchmark, normalized in gains and losses

Gains (G)	Asset *should* be realized	Asset *should not* be realized
Asset *is* realized	$\alpha_G = 0.0788$	$\beta_G = 0.0963$
Asset *is not* realized	$\gamma_G = 0.1271$	$\delta_G = 0.6978$

Losses (L)	Asset *should* be realized	Asset *should not* be realized
Asset *is* realized	$\alpha_L = 0.4295$	$\beta_L = 0.0434$
Asset *is not* realized	$\gamma_L = 0.2893$	$\delta_G = 0.2378$

Notes: Table 3.4.4 normalizes the proportions of Table 3.4.3 separately for gains and losses and without uninformative states or riskless choices.

There are two ways to interpret the definition of the disposition effect. First, an asset based view that refers to the disposition of winning *assets* to be sold too early and of losing *assets* to be held too long. Second, an investor based view that refers to the disposition of *investors* to sell their winning assets too early and to hold their losing assets too long. The following analysis considers both.

In order to determine the erroneous disposition effect, we first need to look at what should be done, i.e., the rational benchmark. Under the asset based interpretation, the proportions of gains and losses that should be realized can be directly inferred from Table 3.4.4:

$$PGSR = \alpha_G + \gamma_G = 0.2059 \quad \text{and} \quad PLSR = \alpha_L + \gamma_L = 0.7188.$$

As stated in Predictions 1 and 2, erroneous disposition-prone behavior in both domains requires PGR to be larger than PGSR and PLR to be lower than PLSR. However, we find that

$$PGR = \alpha_G + \beta_G = 0.1751 \quad \text{and} \quad PLR = \alpha_L + \beta_L = 0.4729.$$

Under the investor based interpretation, we cannot use Table 3.4.4 as it already shows averages. Here, we need to construct Tables 3.4.3 and 3.4.4 separately for each subject in order to calculate individual-specific measures of PGSR/PLSR and PGR/PLR. When doing so, we find that the average subject's proportions of gains and losses that should be realized are

$$PGSR = 0.2534 \quad \text{and} \quad PLSR = 0.6570,$$

whereas the average subject's proportions of gains and losses that are realized are

$$PGR = 0.1946 \quad \text{and} \quad PLR = 0.4691.$$

The corresponding differences are in line with the asset based interpretation: We find that $DEL = PLSR - PLR = 0.1879$ still represents a substantial and highly significant difference (Wilcoxon signed rank test, $p = 0.0000$), while $DEG = PGR - PGSR = -0.0588$ becomes slightly more negative and is significant as well (Wilcoxon signed rank test, $p = 0.0277$).[17] Thus, using the average of each subject's individual-specific disposition effect allows us to draw the same conclusion as under the asset based interpretation.

Result 2 (Disposition Effect within Domains)**.** *At an aggregate level, the disposition effect exists in losses, but not in gains, i.e.* $DEL > 0$ *and* $DEG < 0$.

Interestingly, we observe the disposition effect only in losses. That is, Prediction 2 is confirmed, but Prediction 1 is neglected. However, since Result 2 is derived at an aggregate level, we can neither conclude that subjects have a larger propensity

[17] By the commutative and distributive law, it becomes irrelevant whether we first calculate each subject's disposition effect and then take the average over all subjects or whether we first average over all subjects' PGSR/PLSR and PGR/PLR and then calculate the disposition effect.

for keep than switch violations in losses, nor can we say that subjects make less violations in gains. The reason is that keep (switch) violations contribute to the disposition effect in losses (gains), whereas switch (keep) violations counteract it. Hence, the aggregate level is too crude to draw conclusions on individual error propensities. However, our individual choice data allows us to derive these individual errors, which we do next. They will inform us about underlying mechanisms of the disposition effect.

For each subject, we can calculate an individual propensity to make a specific error in a given domain. In doing so, we draw on each subject's individual Table 3.4.4 entries again, and denote these entries correspondingly by $\alpha^i_{G,L}$, $\beta^i_{G,L}$, $\gamma^i_{G,L}$, and $\delta^i_{G,L}$ for subject $i = 1, 2, \ldots, n$. In the gain domain, we find that the average subject's propensity for switch violations is 19.13% ($\frac{1}{n}\sum_i \frac{\beta^i_G}{\beta^i_G+\delta^i_G}$), whereas the propensity for keep violations is 46.34% ($\frac{1}{n}\sum_i \frac{\gamma^i_G}{\gamma^i_G+\alpha^i_G}$). This difference is highly significant (Mann-Whitney-U test, $p = 0.0000$). With respect to losses, a similar picture emerges. Here, the propensity for switch violations is 16.44% ($\frac{1}{n}\sum_i \frac{\beta^i_L}{\beta^i_L+\delta^i_L}$) and that for keep violations 34.66% ($\frac{1}{n}\sum_i \frac{\gamma^i_L}{\gamma^i_L+\alpha^i_L}$), again a difference that is highly significant (Mann-Whitney-U test, $p = 0.00002$).[18]

Result 3 (Error Propensities within Domains)**.** *Subjects' propensity not to realize when they should (keep violations) is substantially higher than their propensity to realize when they should not (switch violations), both in gains and losses, i.e.* $KVG > SVG$ *and* $KVL > SVL$.

Result 3 is consistent with Prediction 5, i.e. with a motivated beliefs explanation. It rejects Prediction 4, i.e. a mechanical belief in mean reversion. Moreover, it is only partly consistent with Prediction 3, i.e. with realization utility. Precisely, it is consistent with $SVL < KVL$, but not with $KVG < SVG$.

[18]Rather than looking at subjects' error propensities, we can also investigate the empirical likelihood of certain errors in a given domain. Table 3.4.4 shows that in gains the likelihood for switch violations is 12.13% ($\frac{\beta_G}{\beta_G+\delta_G}$), whereas the likelihood for keep violations is 61.73% ($\frac{\gamma_G}{\gamma_G+\alpha_G}$). With respect to losses, Table 3.4.4 shows that the likelihood for switch violations is 15.43% ($\frac{\beta_L}{\beta_L+\delta_L}$) and that for keep violations 40.25% ($\frac{\gamma_L}{\gamma_L+\alpha_L}$). While this finding shows the robustness of Result 3, it cannot inform us about underlying mechanisms of why subjects behave this way. Therefore, we restrict our attention to subjects' error propensities in the following analysis.

Result 3 is surprising. It shows that holding the asset for too long (rather than realizing too soon) is the predominant error subjects make in *both* domains. Notice, however, that there is a subtle property of our experimental design that mechanically increases the propensity of keep violations versus switch violations in gains, and of switch violations versus keep violations in losses. This mechanical effect amplified the observed difference of Result 3 in gains, but attenuated it in losses. The reason is the following: There are more loss than gain states where holding on to the asset violates first-order stochastic dominance. Since these additional states are more informative, holding on implies a more severe violation than in the other states. As a result, even if a subject's bias for keep violations was the same across gains and losses (when holding informativeness constant), we would observe a larger propensity for keep violations in gains than losses. The reverse argument can be made for switch violations, since there are more gain than loss states where realizing the asset violates first-order stochastic dominance. The fact that switch violations are more severe in these states dampens the propensity measure in gains compared to losses. These mechanical forces may explain why, contrary to intuition, we observe a somewhat larger propensity for keep violations in gains than losses.

In order to control for these mechanical forces, we need to compare states with the same informativeness. On that behalf, we restrict attention to a sub-sample of subjects that invested in one of the assets in both gain and loss states that have an informativeness of $|\Delta| = 1$. This sub-sample consists of 98 subjects. Using this sub-sample, we find that in gains, subjects' propensity for switch violations is 22.45% and for keep violations 38.78%. In losses, the propensity for switch violations is 18.37% and the propensity for keep violations is 34.69%. Notice first that Result 3 still holds: In gains, the propensity for keep violations is 1.70 times larger than the propensity for switch violations, which is a significant difference (Wilcoxon signed rank test, $p = 0.0048$). In losses, keep violations have a propensity that is 1.88 times larger than the propensity of switch violations. This difference is again significant (Wilcoxon signed rank test, $p = 0.0063$). The factor with which the propensity for keep violations exceeds that for switch violations is similar in gains and losses. Therefore, Result 3 is not due to a mechanical design feature of our experiment. Second, notice that we can neither find a significant difference between keep violations in gains and losses (Wilcoxon signed rank test, $p = 0.4236$), nor between switch violations in gains and losses (Wilcoxon signed rank test, $p = 0.2669$).

Result 4 (Domain Parity of Error Propensities)**.** *The propensity neither of keep nor of switch violations is different between gains and losses, i.e.* $KVG = KVL$ *and* $SVG = SVL$*.*

Result 4 is consistent with Prediction 5, i.e. with a motivated beliefs explanation, as well as with Prediction 4, i.e. a mechanical belief in mean reversion. However, it is inconsistent with Prediction 3, i.e. with realization utility. Precisely, it is inconsistent with $SVL < SVG$ and with $KVG < KVL$.

Results 3 and 4 show that the propensity for keep violations is larger than the propensity for switch violations and that these propensities are equal across domains. This poses the question of how this pattern in propensities is consistent with Result 2, which identified a sizable disposition effect in losses, but a much smaller negative disposition effect in gains. However, as mentioned above, the aggregate disposition effect is not only driven by subjects' propensities to make certain errors in given domains, but also by the frequencies with which these errors are possible. These frequencies have the potential to generate a disposition effect even without subjects' propensity for disposition-prone behavior.

In our experiment, keeping the asset is often a violation in losses, but rarely in gains. Switching is often a violation in gains, but rarely in losses.[19] This reflects the fact that gains and good news respectively losses and bad news are not fully orthogonal, i.e. that asset prices carry at least some information. Regardless of the fact that error propensities are indistinguishable across domains, these frequency differences promote keep violations in losses relative to gains and switch violations in gains relative to losses. And as keep violations have a larger propensity than switch violations, Result 2 is in fact consistent with Results 3 and 4.

[19] In our experiment, keeping the asset is a violation in 20.59% ($\alpha_G + \gamma_G$) of the gains and in 71.88% ($\alpha_L + \gamma_L$) of the losses. By contrast, switching is a violation in 79.41% ($\beta_G + \delta_G$) of the gains and in 28.12% ($\beta_L + \delta_L$) of the losses. The same results obtain using our subject- rather than state-based measure. Here, keep violations occur in 25.34% ($\frac{1}{n}\sum_i \alpha_G^i + \gamma_G^i$) of the gains and in 65.70% ($\frac{1}{n}\sum_i \alpha_L^i + \gamma_L^i$) of the losses, and switch violations occur in 74.66% ($\frac{1}{n}\sum_i \beta_G^i + \delta_G^i$) of the gains and 34.30% ($\frac{1}{n}\sum_i \beta_L^i + \delta_L^i$) of the losses.

This becomes even more apparent by two other observations. First, the likelihood that an error of any type is made is higher in losses ($33.28\% = \beta_L + \gamma_L$) than in gains ($22.34\% = \beta_G + \gamma_G$).[20] This reflects the fact that keep violations have a higher propensity than switch violations, while they are possible more often in losses than in gains. Second, conditional on making an error, it is likely to contribute to the disposition effect in losses, because keep violations contribute to the disposition effect in losses and are frequently possible. In contrast, we would expect a more balanced picture in gains, as keep violations contradict the disposition effect, but are rarely possible. This is exactly what we find: In losses, 86.95% ($\frac{\gamma_L}{\beta_L+\gamma_L}$) of all errors contribute to the disposition effect and only 13.05% ($\frac{\beta_L}{\beta_L+\gamma_L}$) counteract it, whereas in gains, 43.1% ($\frac{\beta_G}{\beta_G+\gamma_G}$) of all errors contribute to the disposition effect and 56.9% ($\frac{\gamma_G}{\beta_G+\gamma_G}$) counteract it.[21]

3.4.3. Robustness Treatment - Belief Elicitation

Our beliefs treatment serves as a robustness check for our conjecture that the disposition errors are more consistent with a biased beliefs explanation than with non-standard preferences. We show that our participants' subjective beliefs deviated from the Bayesian benchmark, and that actual investment decisions were mostly in line with these distorted beliefs. In particular, we find that most disposition errors were not perceived as such.

Choice I Beliefs and Decisions

All participants were informed in the instructions that the assignment of processes H and L to assets A and B was randomly determined by the toss of a fair coin. Therefore, we assume that they had a prior belief of 50% for asset A to adhere to process H in period $t = 0$. In Choice I, i.e. after observing price paths for two

[20] The population average of the overall error propensity per investor is 29.24% in gains and 25.55% in losses. However, this difference is not significant according to a Mann-Whitney-U test ($p = 0.4916$).

[21] The investor based interpretation of the disposition effect is in line with these findings: The population average of the share in errors that contribute to the disposition effect is 33.83% in gains and 79.75% in losses. Both are significantly different from 50% (Wilcoxon signed rank test, $p = 0.0024$ respectively $p = 0.0063$).

periods, each subject had to report their beliefs for all 5 Bayesian posteriors that could possibly materialize after these two periods.

The average of all subjective beliefs was less than 1% below the average of all Bayesian posteriors, so that the subjective beliefs seem to be almost accurate at first glance. However, a closer look reveals that information was under-appreciated on average, i.e. subjective beliefs were tilted towards 50% relative to the Bayesian benchmark. This implies that our subjects under-estimated the better asset and overestimated the worse asset on average. Table 3.4.5 summarizes the average subjective beliefs per Bayesian posterior (for asset A adhering to process H). The big standard deviations illustrate that individual beliefs were often far off, even though the averages were reasonably close to the Bayesian benchmark.

Table 3.4.5.: Average subjective belief per Bayesian posterior in Choice I.

Bayesian posterior (in %)	30.9	40.1	50.0	59.9	69.1	Any
∅ Subjective belief (in %)	34.4	41.9	49.9	55.9	63.6	49.1
(Std. Deviation)	19.6	16.4	14.5	15.6	17.2	19.6

Notes: 405 observations from 81 subjects

Investment decisions in Choice I were mostly aligned with subjective beliefs: only 15.4% of the decisions for a risky asset in contingencies with non-50% subjective belief were contradicting the belief.[22] On the contrary, 30.1% of the decisions for a risky asset in contingencies with informative Bayesian posterior were contradicting the Bayesian.

A regression analysis confirms that subjective beliefs explain the observed decisions very well, whereas the Bayesian posteriors do less so. We estimated a linear probability model for the likelihood to choose asset A instead of B based on the subjective beliefs and Bayesian posteriors, and we find that the former had a highly significant effect, whereas the latter did not.[23] Hence, it suffices to restrict the analysis to subjective beliefs alone, which neither affects the marginal effects

[22] A risky asset was chosen in 376 of 405 observed Choice I decisions in the beliefs treatment, subjects had non-50% beliefs in 311 thereof, and chose the asset they perceived as better in 263 of these.

[23] Surprisingly, multi-collinearity is not an issue by usual VIF tests.

nor the explanatory power of the model.[24] Table 3.4.6 summarizes the regression results.

Table 3.4.6.: Regression: choice of asset A (likelihood) on subjective beliefs dummies

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	16.3399	3.1078	5.26	0.0000
Belief > 50%	69.1032	4.3602	15.85	0.0000
Belief = 50%	49.8140	5.6915	8.75	0.0000

Notes: Belief smaller 50% is the omitted category.

The omitted category of this estimation are subjective beliefs smaller 50%. Hence, when subjective beliefs for asset A to be the better one were smaller 50%, subjects chose asset A with only 16.3% probability; when subjective beliefs were equal 50%, they chose asset A with 16.3%+49.8%=66.1% probability; and when beliefs were higher than 50%, they chose asset A with 16.3%+69.1%=85.4% probability. That is, they chose the subjectively worse asset in 16.3% of the situations where A was subjectively worse, and in 14.6% of the situations where asset B was subjectively worse. A Wald test shows that these are not significantly different (p-value 0.56).

On average, our subjects overestimated the asset they chose by 6.3%. This is surprising at first, as conservatism implies that they underestimated the better asset on average, and alignment of choices and beliefs implies that they typically chose the asset that they regarded higher. However, in 60 out of 324 informative states, our subjects updated their beliefs in the wrong direction and it is mostly these decisions that drove this counter-intuitive finding.[25]

Subjective beliefs in Choice I were not significantly affected by whether they were incentivized or not (p-value Kolmogorov Smirnov test 0.16, p-value Wilcoxon rank

[24] The opposite restriction on Bayesian posteriors alone establishes significant coefficients as well, but has a much lower explanatory power: The R^2 of the beliefs regression is 0.4067, the R^2 of the Bayesian posteriors regression is 0.1284.

[25] The subjects who updated in the wrong direction and then chose the subjectively better asset overestimated their asset a lot (approximately twice the information), whereas the usual conservative subjects underestimated their asset only a little bit (approximately one quarter the information).

sum test 0.50), and neither were the investment decisions (p-value Kolmogorov Smirnov test 1.00, p-value Wilcoxon rank sum test 0.90). Restricting the sample to the actually realized decisions does not qualitatively affect any of the above findings.

Choice II Beliefs and Decisions

In our analysis of Choice II beliefs, we restrict the sample to the subjects who actually held a risky asset after the Choice I realization. These were 75 of 81 subjects in the beliefs treatment. Moreover, we re-normalize our subjects' beliefs from asset A to their own asset adhering to process H.

As in Choice I, the average of all subjective beliefs was less than 1% below the average of all Bayesian beliefs in Choice II. However, this is only coincidental as Choice II beliefs resulted both from biased priors and mistaken updating. Table 3.4.7 summarizes the average self-reported subjective beliefs per (objective) Bayesian posterior in Choice II as well as the average *subjective Bayesian posteriors* (for the own asset adhering to process H) that were derived by Bayesian updating of the subjective priors. The latter are considerably above the true posteriors, which reflects the fact that the subjective priors were upward-biased on average. Moreover, they substantially deviate from the subjective beliefs, which shows that information was not processed correctly. The big standard deviations illustrate that individual beliefs were often far off, even though the averages were reasonably close to the Bayesian benchmark.

Investment decisions in Choice II were mostly aligned with subjective beliefs, but less so than in Choice I: 23.0% of the decisions for a risky asset in contingencies with non-50% subjective belief were contradicting the belief.[26] On the contrary, 35.1% of the decisions for a risky asset in contingencies with informative Bayesian posterior were contradicting the Bayesian.

A regression analysis confirms that subjective beliefs explain the observed decisions very well, whereas the Bayesian posteriors do less so. We estimated a linear probability model for the likelihood to keep one's own asset instead of switching

[26] A risky asset was chosen in 484 of 526 observed Choice II decisions of asset holders in the beliefs treatment, subjects had non-50% beliefs in 396 thereof, and chose the asset they perceived as better in 305 of these.

Table 3.4.7.: Average subjective belief and subjective Bayesian posterior per objective Bayesian posterior in Choice II.

Bayesian posterior (in %)	<23	30.9	40.1	50.0	59.9	>69	Any
∅ Subjective belief (in %)	24.1	39.9	39.8	48.4	57.0	65.9	47.2
(Std. Deviation)	23.1	23.1	19.0	15.1	16.5	18.1	20.6
∅ Subjective Bayesian (in %)	33.4	45.3	50.2	57.7	65.4	75.3	56.2
(Std. Deviation)	24.4	19.9	16.4	14.9	13.7	9.9	18.8
# Observations	35	45	135	144	132	35	526

Notes: 526 observations from 75 subjects.

to the other based on the "information" in the subjective beliefs and Bayesian posteriors, and we find that the former had a highly significant effect, whereas the latter did not.[27] Hence, it suffices to restrict the analysis to subjective beliefs alone, which neither affects the marginal effects nor the explanatory power of the model.[28] Table 3.4.8 summarizes the regression results.

Table 3.4.8.: Regression: choice of own asset (likelihood) on subjective beliefs dummies for own asset

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	33.1839	2.7833	11.92	0.0000
Belief > 50%	56.9896	4.2109	13.53	0.0000
Belief = 50%	37.2707	5.2323	7.12	0.0000

Notes: Belief smaller 50% is the omitted category.

The omitted category of this estimation are subjective beliefs smaller 50%. Hence, when subjective beliefs for the own asset to be the better one were smaller 50%, subjects chose asset A with 33.2% probability; when subjective beliefs were equal 50%, they chose asset A with 33.2%+37.3%=70.5% probability; and when

[27] Again, multi-collinearity is not an issue by usual VIF tests.

[28] The opposite restriction on Bayesian posteriors alone establishes significant coefficients as well, but has a much lower explanatory power: The R^2 of the beliefs regression is 0.2797, the R^2 of the Bayesian posteriors regression is 0.0840.

beliefs were higher than 50%, they chose asset A with 33.2%+57.0%=90.2% probability. That is, they kept their own asset in 33.2% of the situations where they perceived it to be the worse one, and switched to the other asset in 9.8% of the situations where they perceived their own asset to be the better one. A Wald test shows that the former likelihood is significantly bigger than the latter (p-value 0.00).

Again, subjective beliefs were not significantly affected by whether they were incentivized or not (p-value Kolmogorov Smirnov test 0.38, p-value Wilcoxon rank sum test 0.26), and neither were the investment decisions (p-value Kolmogorov Smirnov test 0.68, p-value Wilcoxon rank sum test 0.14).

Last but not least, we investigate those decisions that entailed disposition errors, i.e. first-order violations. In order to do so, we distinguish *actual* first-order violation, i.e. choices that contradicted the Bayesian, from *perceived* first-order violations, i.e. choices that contradicted the subjective belief. Table 3.4.9 summarizes the actual and perceived first-order violations for keep and switch errors in gains and losses.

Table 3.4.9.: Actual, perceived, and both actual and perceived disposition errors.

	Actual	Perceived	Both	Both/Actual
Keep Violation in Gains	52	27	18	34.6%
Keep Violation in Losses	72	40	29	40.3%
Switch Violation in Gains	38	15	4	10.5%
Switch Violation in Losses	28	9	5	17.9%
Any Violation	190	91	56	29.5%

Overall, we observed 190 actual first-order violations among the 526 decisions in Choice II of the beliefs treatment, but only 91 perceived violations. Moreover, only 56 violations were both perceived and actual, i.e. only 29.5% of all actual violations were perceived as such. This means that our subjects were mostly unaware of their disposition errors, which suggests that these were driven by distorted beliefs rather than non-standard preferences. Specifically, only 33 of the 56 perceived actual violations were keep errors in losses or switch errors in gains, which are the violations that are consistent with realization utility. On the other hand, 77 of the non-perceived actual violations were keep violations in gains or losses, which are the violations that are consistent with motivated beliefs. Hence, more than twice as many violations can be rationalized by motivated beliefs than by realization utility. (40.5% respectively 17.4% of all observed violations.)

3.5. Conclusion

This paper uses a novel experimental design to decompose the disposition effect along two dimensions. The first decomposition allows us to identify an erroneous disposition effect separately for gains and losses. Here, we find that a disposition effect exists in losses but not in gains. However, this aggregate measure is still too crude to inform us about subjects' propensities to make certain errors. And because these error propensities are crucial for the identification of the underlying mechanisms at work, we further decompose the erroneous disposition effect in its two opposing error types (i.e. switch and keep violations). Here, we find that subjects have a larger propensity for keep than switch violations in both gains and losses. Moreover, subjects' propensity for a given violation is the same across domains.

These findings are consistent with models of motivated beliefs, but inconsistent with either a model of realization utility or an irrational belief in mean reversion. Our theoretical conclusions are further supported when investigating assets' initial purchase decision as well as by a robustness treatment that additionally elicits subjects' beliefs.

A. Appendix to Chapter 1: Motivation by Naïveté

A.1. Proofs

Proof of Lemma 1.
The implicit function theorem yields

$$\hat{g}_x(x) = \frac{-u_{y,x}(x, \hat{g}(x))}{u_{y,y}(x, \hat{g}(x))}.$$

By strict concavity of $u(.,.)$ (in its second component), the denominator is always strictly negative. Hence, $sgn(\hat{g}_x) = sgn(u_{x,y})$. □

Proof of Lemma 2.

(i) Follows from $\hat{g} = g$ for $\hat{\beta} = \beta$ and (ii).

(ii) Differentiating (1.1) with respect to $\hat{\beta}$ yields

$$u_y(x, \hat{g}(x)) + \hat{\beta} u_{y,y}(x, \hat{g}(x)) \hat{g}_{\hat{\beta}}(x) = 0.$$

Rearranging yields

$$\hat{g}_{\hat{\beta}}(x) = \frac{-u_y(x, \hat{g}(x))}{\hat{\beta} u_{y,y}(x, \hat{g}(x))}.$$

Assumption 1 implies $\hat{g}_{\hat{\beta}}(x) > 0$. □

Proof of Proposition 1.
By the second order condition (1.3), perceived marginal utility of stage one investment x is decreasing in x. Hence, for an increase in the degree of naïveté $\hat{\beta}$,

the first order condition (1.2) is balanced by an increase/decrease in x if and only if perceived marginal utility of stage one investment x is increasing/decreasing in naïveté $\hat{\beta}$.

$$
\begin{aligned}
&\frac{\partial}{\partial \hat{\beta}}\left[-1-\beta \hat{g}_x(x)+\beta u_x(x,\hat{g}(x))+\beta u_y(x,\hat{g}(x))\hat{g}_x(x)\right] \\
=&-\beta \hat{g}_{x,\hat{\beta}}(x)+\beta u_{x,y}(x,\hat{g}(x))\hat{g}_{\hat{\beta}}(x)+\beta u_{y,y}(x,\hat{g}(x))\hat{g}_{\hat{\beta}}(x)\hat{g}_x(x) \\
&+\beta u_y(x,\hat{g}(x))\hat{g}_{x,\hat{\beta}}(x) \\
=&\beta\left[\left(\frac{1}{\hat{\beta}}-1\right)\hat{g}_{x,\hat{\beta}}(x)+u_{x,y}(x,\hat{g}(x))\hat{g}_{\hat{\beta}}(x)+u_{y,y}(x,\hat{g}(x))\hat{g}_{\hat{\beta}}(x)\hat{g}_x(x)\right] \\
=&\beta\left(\frac{1}{\hat{\beta}}-1\right)\hat{g}_{x,\hat{\beta}}(x)
\end{aligned}
$$

The last equality uses $\hat{g}_x(x)=\frac{-u_{y,x}(x,\hat{g}(x))}{u_{y,y}(x,\hat{g}(x))}$. □

Proof of Corollary 1.
We can calculate $\hat{g}_{x,\hat{\beta}}$ as follows.

$$
\begin{aligned}
\hat{g}_{x,\hat{\beta}}(x)&=\frac{\partial}{\partial \hat{\beta}}\hat{g}_x(x) \\
&=\frac{u_{y,x}(x,\hat{g}(x))u_{y,y,y}(x,\hat{g}(x))\hat{g}_{\hat{\beta}}(x)-u_{y,x,y}(x,\hat{g}(x))\hat{g}_{\hat{\beta}}(x)u_{y,y}(x,\hat{g}(x))}{u_{y,y}(x,\hat{g}(x))^2}
\end{aligned}
$$

Lemma 2 and Assumption 1, immediately imply $sgn(\hat{g}_{x,\hat{\beta}})=sgn(u_{y,y,y}u_{y,x}-u_{y,x,y}u_{y,y})$. Rearranging the expression yields the result. □

Proof of Proposition 2.

$$
\begin{aligned}
\frac{\partial}{\partial \hat{\beta}}W&=\frac{\partial}{\partial \hat{\beta}}\beta\left[-x-g(x)+u(x,g(x))\right] \\
&=\beta\left[-x_{\hat{\beta}}-g_x(x)x_{\hat{\beta}}+u_x(x,g(x))x_{\hat{\beta}}+u_y(x,g(x))g_x(x)x_{\hat{\beta}}\right] \\
&=\beta x_{\hat{\beta}}\left[-1-g_x(x)+u_x(x,g(x))+u_y(x,g(x))g_x(x)\right] \\
&=x_{\hat{\beta}}\left\{(1-\beta)+\left[-1+(1-\beta)g_x(x)+\beta u_x(x,g(x))\right]\right\}
\end{aligned}
\tag{A.1}
$$

As the term in square brackets denotes the first order condition of the fully sophisticated individual, it vanishes for $x=x^{FS}$ and the remaining term $(1-\beta)$ is strictly larger than zero for $\beta < 1$. The result obtains. $\square$

A.2. Cross-Validation of the Example (Cobb-Douglas Functions)

Let $u(x, y) := \alpha x^\mu y^\nu$ be a Cobb-Douglas utility function of homogeneity smaller one, i.e. $\alpha, \mu, \nu > 0$ and $\mu + \nu < 1$. First, we check Assumption 1. As $u(x, y)$ is a power-function both in x and y, it is infinitely often continuously differentiable with respect to x and y for all $x, y > 0$. Also, it is non-negative for all $x, y \geq 0$, because $\alpha > 0$.

The partial derivatives of $u(x, y)$ are strictly positive for all $x, y > 0$. In fact, $u_x(x, y) = \alpha\mu x^{\mu-1} y^\nu > 0$ and $u_y(x, y) = \alpha\nu x^\mu y^{\nu-1} > 0$ for all $x, y > 0$. Hence, all directional derivatives (in positive directions) in the interior of the positive quadrant are strictly positive as well. In particular, for $v := (a, b)$, the directional derivative at (x, y) with $x, y > 0$ in the direction of v is

$$\begin{aligned}
\frac{d}{dv} u(x, y) &= \frac{d}{dt} \frac{u(x + ta, y + tb)}{||(a, b)||}\bigg|_{t=0} \\
&= \frac{a u_x(x + ta, y + tb)|_{t=0} + b u_y(x + ta, y + tb)|_{t=0}}{\sqrt{a^2 + b^2}} \\
&= \frac{a u_x(x, y) + b u_y(x, y)}{\sqrt{a^2 + b^2}} \\
&= \alpha x^{\mu-1} y^{\nu-1} \frac{a\mu y + b\nu x}{\sqrt{a^2 + b^2}},
\end{aligned}$$

which is larger zero for all $a, b > 0$, i.e. in all positive directions. Here, it is easy to see why $u(x, y)$ for $\mu + \nu = 1$ is linear. In fact, when taking the derivative at (x, y) in the direction of $v := (x, y)$, i.e. when taking a cut of $u(.,.)$ across the origin, and denoting $\lambda := \frac{y}{x}$, we get

$$\begin{aligned}
\frac{d}{dv} u(x, y) &= \alpha x^{\mu-1} y^{\nu-1} \frac{x\mu y + y\nu x}{\sqrt{x^2 + y^2}} \\
&= \alpha x^\mu y^\nu \frac{\mu + \nu}{\sqrt{x^2 + y^2}}
\end{aligned}$$

$$= \alpha\lambda^{\nu}x^{\mu+\nu}\frac{\mu+\nu}{x\sqrt{1+\lambda^2}}$$
$$= \frac{\alpha\lambda^{\nu}}{\sqrt{1+\lambda^2}},$$

which is independent of x (and, hence, of y), i.e. constant, and depends solely on the angle of the cut and the function parameters. Notice that for $\mu = \nu = \frac{1}{2}$, the slope of the curve in direction of (x, y) is equal to the slope in direction of (y, x), i.e., that the above expression remains unchanged when λ is replaced by $\frac{1}{\lambda}$. That is, $u(.,)$ is symmetric with respect to the bisection line of the positive quadrant for $\mu = \nu = \frac{1}{2}$. Further, notice that $\frac{d}{dv}u(1,1) = \frac{\alpha}{\sqrt{2}}$ for all $\mu, \nu > 0$ with $\mu + \nu = 1$.

To see strict concavity of $u(x, y)$, we need the second partial derivatives.

$$\begin{aligned} u_{x,x}(x,y) &= \alpha\mu(\mu-1)x^{\mu-2}y^{\nu} < 0 \\ u_{x,y}(x,y) = u_{y,x}(x,y) &= \alpha\mu\nu x^{\mu-1}y^{\nu-1} > 0 \\ u_{y,y}(x,y) &= \alpha\nu(\nu-1)x^{\mu}y^{\nu-2} < 0 \end{aligned}$$

Notice that $u_{x,y} = u_{y,x}$ always holds by equality of mixed partials (Schwarz' Theorem). By definition, $u(x, y)$ is strictly concave when its Hessian is negative definite, i.e. its first minor $u_{x,x}(x, y)$ is negative, and its second minor, the determinant, is positive. The former is already checked. For the latter, see the following.

$$\begin{aligned} detH_u &= \begin{vmatrix} u_{1,1}(x,y) & u_{1,2}(x,y) \\ u_{2,1}(x,y) & u_{2,2}(x,y) \end{vmatrix} \\ &= u_{1,1}(x,y)u_{2,2}(x,y) - u_{1,2}(x,y)u_{2,1}(x,y) \\ &= \alpha^2\mu\nu x^{2\mu-2}y^{2\nu-2}\left[(\mu-1)(\nu-1)-\mu\nu\right] \\ &= \alpha^2\mu\nu x^{2\mu-2}y^{2\nu-2}\left[1-\mu-\nu\right] > 0 \end{aligned}$$

To explicitly calculate the perceived reaction function $\hat{g}(x)$ of stage two investment conditional on stage one investment x, we have to solve the stage two optimization problem.

$$\max_y \hat{U}_2 \qquad = \max_y -y + \hat{\beta}u(x,y) = \max_y -y + \hat{\beta}\alpha x^{\mu}y^{\nu}$$

$$
\begin{aligned}
FOC: &\qquad -1+\hat{\beta}\alpha\nu x^{\mu}y^{\nu-1} \stackrel{!}{=} 0 \\
\Leftrightarrow &\qquad \hat{g}(x) := y = \left(\hat{\beta}\alpha\nu x^{\mu}\right)^{\frac{1}{1-\nu}}
\end{aligned}
$$

The partial derivatives with respect to x and $\hat{\beta}$ as well as the cross-derivative $\hat{g}_{x,\hat{\beta}}$ readily obtain. In order to calculate the actual stage one investment x, we have to solve the stage one optimization problem.

$$
\begin{aligned}
\max_x U_1 &= \max_x -x-\beta\hat{g}(x)+\beta u(x,\hat{g}(x)) \\
&= \max_x -x-\beta\left(\hat{\beta}\alpha\nu x^{\mu}\right)^{\frac{1}{1-\nu}}+\beta\alpha x^{\mu}\left(\hat{\beta}\alpha\nu x^{\mu}\right)^{\frac{\nu}{1-\nu}} \\
&= \max_x -x+\beta\left(\hat{\beta}\nu\right)^{\frac{\nu}{1-\nu}}\alpha^{\frac{1}{1-\nu}}x^{\frac{\mu}{1-\nu}}\left(1-\hat{\beta}\nu\right) \\
FOC: \qquad 1 &\stackrel{!}{=} \beta\left(\hat{\beta}\nu\right)^{\frac{\nu}{1-\nu}}\alpha^{\frac{1}{1-\nu}}\frac{\mu}{1-\nu}x^{\frac{\mu+\nu-1}{1-\nu}}\left(1-\hat{\beta}\nu\right) \\
\Leftrightarrow \qquad x &= \left(\hat{\beta}^{\frac{\nu}{1-\nu}}\frac{1-\hat{\beta}\nu}{1-\nu}\right)^{\frac{1-\nu}{1-\nu-\mu}}\mu^{\frac{1-\nu}{1-\nu-\mu}}\alpha^{\frac{1}{1-\nu-\mu}}\nu^{\frac{\nu}{1-\nu-\mu}}\beta^{\frac{1-\nu}{1-\nu-\mu}}
\end{aligned}
$$

To see that x is increasing in $\hat{\beta}$, we could calculate the partial derivative $x_{\hat{\beta}}$. In fact, it suffices to calculate the partial derivative of the left bracket alone, as the right terms are only constant, positive multipliers, i.e. independent of $\hat{\beta}$, and its exponent is positive. Therefore, I denote $f(\hat{\beta}) := \hat{\beta}^{\frac{\nu}{1-\nu}}\frac{1-\hat{\beta}\nu}{1-\nu}$.

$$
\begin{aligned}
\frac{\partial}{\partial\hat{\beta}}f(\hat{\beta}) &= \frac{\partial}{\partial\hat{\beta}}\hat{\beta}^{\frac{\nu}{1-\nu}}\frac{1-\hat{\beta}\nu}{1-\nu} \\
&= \frac{\nu}{1-\nu}\hat{\beta}^{\frac{2\nu-1}{1-\nu}}\frac{1-\hat{\beta}\nu}{1-\nu}+\hat{\beta}^{\frac{\nu}{1-\nu}}\frac{-\nu}{1-\nu} \\
&= \frac{\nu}{1-\nu}\hat{\beta}^{\frac{\nu}{1-\nu}}\left(\frac{1-\hat{\beta}\nu}{\hat{\beta}-\hat{\beta}\nu}-1\right) > 0 \; \forall \; \hat{\beta}<1
\end{aligned}
$$

For $\hat{\beta}=1$, $f_{\hat{\beta}}(\hat{\beta})=0$ holds. Hence, stage one investment x is strictly increasing in naïveté $\hat{\beta}$ up until full naïveté.

To calculate the decision maker's welfare, we plug in x and $g(x)$ in the definition of $W := U_0$.

$$\begin{aligned}
W =& (\alpha\mu^\mu\nu^\nu)^{\frac{1}{1-\nu-\mu}} \beta^{\frac{(1-\nu)(\nu+\mu)-\mu\nu}{(1-\nu)(1-\nu-\mu)}} \cdot \\
& \left(\hat{\beta}^{\frac{\nu}{1-\nu}} \frac{1-\hat{\beta}\nu}{1-\nu}\right)^{\frac{\mu}{1-\nu-\mu}} \left[1 - \mu\beta^{\frac{1-2\nu}{1-\nu}} \left(\hat{\beta}^{\frac{\nu}{1-\nu}} \frac{1-\hat{\beta}\nu}{1-\nu}\right) - \nu\beta\right] \\
=& (\alpha\mu^\mu\nu^\nu)^{\frac{1}{1-\nu-\mu}} \beta^{\frac{(1-\nu)(\nu+\mu)-\mu\nu}{(1-\nu)(1-\nu-\mu)}} f(\hat{\beta})^{\frac{\mu}{1-\nu-\mu}} \left[1 - \mu\beta^{\frac{1-2\nu}{1-\nu}} f(\hat{\beta}) - \nu\beta\right]
\end{aligned}$$

Again, the first multipliers are strictly positive and can be ignored in determining the sign of the partial derivative.

$$\begin{aligned}
& \frac{\partial}{\partial\hat{\beta}} f(\hat{\beta})^{\frac{\mu}{1-\nu-\mu}} \left[1 - \mu\beta^{\frac{1-2\nu}{1-\nu}} f(\hat{\beta}) - \nu\beta\right] \\
=& \frac{\mu}{1-\nu-\mu} f(\hat{\beta})^{\frac{\mu}{1-\nu-\mu}} f_{\hat{\beta}}(\hat{\beta}) \left[\frac{1-\nu\beta}{f(\hat{\beta})} - (1-\nu)\beta^{\frac{1-2\nu}{1-\nu}}\right] \overset{!}{>} 0 \\
\Leftrightarrow & \frac{1-\nu\beta}{f(\hat{\beta})} - (1-\nu)\beta^{\frac{1-2\nu}{1-\nu}} \overset{!}{>} 0 \\
\Leftrightarrow & \frac{1-\nu\beta}{1-\nu} \overset{!}{>} \beta^{\frac{1-2\nu}{1-\nu}} f(\hat{\beta}) = \beta^{\frac{1-2\nu}{1-\nu}} \hat{\beta}^{\frac{\nu}{1-\nu}} \frac{1-\hat{\beta}\nu}{1-\nu}
\end{aligned}$$

As $\beta^{\frac{1-2\nu}{1-\nu}} \hat{\beta}^{\frac{\nu}{1-\nu}} \leq \hat{\beta} \leq 1$ with at least one strict inequality (as $\beta < 1$), and $\frac{1-\hat{\beta}\nu}{1-\nu} \leq \frac{1-\beta\nu}{1-\nu}$ hold, the partial derivative is strictly positive for all $\hat{\beta} < 1$ and zero for $\hat{\beta} = 1$. Hence, starting at $\hat{\beta} = \beta$, self-zero welfare strictly increases in naïveté $\hat{\beta}$ up until full naïveté.

B. Appendix to Chapter 2: Anticipation of Prospect Theory Preferences

B.1. Tables

Table B.1.1.: Lotteries for Prospect Theory parameter elicitation

Lottery	Payoff x	Payoff y	Week
G1	1.20	0.00	1
G2	2.20	0.00	2
G3	2.80	0.00	1
G4	4.40	0.00	1
G5	7.80	0.00	2
G6	7.80	2.80	2
G7	2.80	1.20	1
L1	−1.20	0.00	1
L2	−2.20	0.00	2
L3	−2.80	0.00	1
L4	−4.40	0.00	1
L5	−7.80	0.00	2
L6	−7.80	−2.80	2
L7	−2.80	−1.20	1
M1	−1.20	1.20	1
M2	−2.20	2.20	2
M3	−2.80	2.80	1 & 2

M4	−4.40	4.40	1
M5	−7.80	7.80	2

Notes: Payoffs x and y in EUR, all lotteries are 50:50.

Table B.1.2.: Individual Prospect Theory parameter estimates

	Both Weeks Jointly			Week 1			Week 2		
	α	β	λ	α_1	β_1	λ_1	α_2	β_2	λ_2
1	0.83	0.87	10.43	1.21	1.38	71.99	0.72	0.74	6.68
	(0.18)	(0.19)	(11.48)	(0.52)	(0.64)	(377.74)	(0.18)	(0.18)	(6.46)
	[0.00]	[0.00]	[0.38]	[0.04]	[0.05]	[0.85]	[0.00]	[0.00]	[0.32]
2	0.67	1.16	1.24	0.58	0.78	0.85	0.90	1.08	4.15
	(0.15)	(0.29)	(0.45)	(0.15)	(0.20)	(0.22)	(0.17)	(0.21)	(1.99)
	[0.00]	[0.00]	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.06]
3	0.84	0.87	1.39	0.74	1.27	0.91	0.85	0.78	1.59
	(0.08)	(0.09)	(0.21)	(0.14)	(0.26)	(0.25)	(0.09)	(0.09)	(0.29)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
4	0.89	0.99	0.85	0.70	1.28	0.53	0.91	0.98	0.86
	(0.11)	(0.13)	(0.16)	(0.17)	(0.35)	(0.22)	(0.13)	(0.14)	(0.20)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.03]	[0.00]	[0.00]	[0.00]
5	0.90	1.05	0.71	0.87	1.18	0.51	0.97	0.95	1.03
	(0.12)	(0.14)	(0.14)	(0.22)	(0.31)	(0.20)	(0.15)	(0.15)	(0.26)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.03]	[0.00]	[0.00]	[0.00]
6	0.92	0.98	0.65	1.06	1.18	0.56	0.87	0.92	0.67
	(0.10)	(0.11)	(0.09)	(0.25)	(0.28)	(0.20)	(0.12)	(0.13)	(0.13)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.02]	[0.00]	[0.00]	[0.00]
7	1.56	2.02	1.06	1.19	2.30	0.40	1.83	1.86	2.49
	(0.25)	(0.35)	(0.48)	(0.34)	(0.83)	(0.33)	(0.36)	(0.36)	(1.60)
	[0.00]	[0.00]	[0.04]	[0.00]	[0.02]	[0.26]	[0.00]	[0.00]	[0.14]
8	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
9	0.78	0.93	2.06	0.96	1.03	2.09	0.74	0.86	2.19
	(0.08)	(0.09)	(0.34)	(0.18)	(0.20)	(0.59)	(0.09)	(0.10)	(0.45)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
10	1.01	1.01	1.01	0.98	0.99	0.99	1.02	1.02	1.02
	(0.03)	(0.03)	(0.04)	(0.06)	(0.06)	(0.08)	(0.04)	(0.04)	(0.06)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]

Table B.1.2.: Individual Prospect Theory parameter estimates

	Both Weeks Jointly			Week 1			Week 2		
	α	β	λ	α_1	β_1	λ_1	α_2	β_2	λ_2
11	1.27	0.74	6.95	0.68	1.07	1.33	2.91	0.49	286.10
	(0.42)	(0.22)	(5.97)	(0.34)	(0.57)	(0.84)	(1.60)	(0.18)	(945.50)
	[0.01]	[0.00]	[0.26]	[0.06]	[0.08]	[0.13]	[0.09]	[0.02]	[0.77]
12	1.15	0.88	1.83	1.02	0.98	2.25	1.00	1.00	1.00
	(0.14)	(0.10)	(0.37)	(0.04)	(0.04)	(0.14)	(0.02)	(0.02)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
13	1.05	1.39	0.96	1.20	0.96	1.25	1.17	1.31	1.90
	(0.22)	(0.30)	(0.40)	(0.30)	(0.23)	(0.46)	(0.21)	(0.24)	(0.83)
	[0.00]	[0.00]	[0.03]	[0.00]	[0.00]	[0.02]	[0.00]	[0.00]	[0.04]
14	3.66	0.30	$1.8E+03$	1.13	0.42	6.55	6.94	0.25	$1.5E+06$
	(1.55)	(0.11)	$(5.8E+03)$	(0.44)	(0.17)	(5.18)	(4.81)	(0.12)	$(1.5E+07)$
	[0.03]	[0.01]	[0.76]	[0.02]	[0.03]	[0.23]	[0.17]	[0.06]	[0.92]
15	0.77	1.10	0.77	1.17	1.14	1.29	0.64	1.09	0.60
	(0.13)	(0.20)	(0.21)	(0.41)	(0.40)	(0.74)	(0.14)	(0.24)	(0.22)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.10]	[0.00]	[0.00]	[0.02]
16	1.07	0.99	1.06	1.10	1.33	0.57	1.15	0.82	1.85
	(0.23)	(0.21)	(0.32)	(0.46)	(0.57)	(0.41)	(0.30)	(0.20)	(0.87)
	[0.00]	[0.00]	[0.00]	[0.03]	[0.04]	[0.19]	[0.00]	[0.00]	[0.05]
17	0.61	1.11	0.73	0.86	1.95	0.51	0.51	0.93	0.74
	(0.14)	(0.27)	(0.25)	(0.39)	(1.18)	(0.57)	(0.15)	(0.27)	(0.29)
	[0.00]	[0.00]	[0.01]	[0.05]	[0.12]	[0.39]	[0.00]	[0.00]	[0.02]
18	0.62	0.83	1.44	0.54	1.26	2.22	0.60	0.77	1.24
	(0.14)	(0.18)	(0.41)	(0.23)	(0.61)	(1.75)	(0.15)	(0.19)	(0.41)
	[0.00]	[0.00]	[0.00]	[0.04]	[0.06]	[0.23]	[0.00]	[0.00]	[0.01]
19	0.67	1.08	0.81	0.73	0.82	0.88	0.74	1.05	1.16
	(0.14)	(0.23)	(0.25)	(0.24)	(0.28)	(0.33)	(0.17)	(0.26)	(0.48)
	[0.00]	[0.00]	[0.01]	[0.01]	[0.01]	[0.02]	[0.00]	[0.00]	[0.03]
20	0.82	0.98	0.74	0.78	0.88	0.72	0.89	0.95	0.95
	(0.08)	(0.09)	(0.10)	(0.12)	(0.13)	(0.13)	(0.09)	(0.09)	(0.15)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
21	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
22	1.74	1.41	1.81	2.79	1.53	5.92	1.57	1.29	1.71
	(0.41)	(0.33)	(0.90)	(1.64)	(0.80)	(10.72)	(0.46)	(0.36)	(1.15)
	[0.00]	[0.00]	[0.06]	[0.11]	[0.08]	[0.59]	[0.00]	[0.00]	[0.16]

Table B.1.2.: Individual Prospect Theory parameter estimates

	Both Weeks Jointly			Week 1			Week 2		
	α	β	λ	α_1	β_1	λ_1	α_2	β_2	λ_2
23	0.99	0.99	0.99	1.00	1.00	0.98	0.99	0.99	0.99
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
24	1.11	1.12	0.89	1.52	1.47	0.84	1.02	0.98	1.02
	(0.14)	(0.14)	(0.17)	(0.39)	(0.38)	(0.40)	(0.14)	(0.14)	(0.24)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.05]	[0.00]	[0.00]	[0.00]
25	1.08	0.93	2.20	1.22	0.74	2.81	1.08	0.97	2.22
	(0.31)	(0.26)	(1.20)	(0.77)	(0.43)	(3.02)	(0.42)	(0.37)	(1.82)
	[0.00]	[0.00]	[0.08]	[0.14]	[0.11]	[0.37]	[0.02]	[0.02]	[0.24]
26	0.62	1.03	1.57	1.01	1.30	2.11	0.51	0.94	1.43
	(0.09)	(0.16)	(0.34)	(0.26)	(0.36)	(0.93)	(0.09)	(0.16)	(0.34)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.04]	[0.00]	[0.00]	[0.00]
27	0.93	0.85	0.84	1.13	1.46	0.44	0.83	0.74	0.88
	(0.15)	(0.13)	(0.14)	(0.36)	(0.49)	(0.27)	(0.15)	(0.13)	(0.17)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.13]	[0.00]	[0.00]	[0.00]
28	0.80	1.34	0.62	0.96	1.37	0.65	0.77	1.27	0.67
	(0.11)	(0.19)	(0.16)	(0.25)	(0.39)	(0.31)	(0.13)	(0.23)	(0.23)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.05]	[0.00]	[0.00]	[0.01]
29	0.79	1.04	0.74	0.88	1.04	0.71	0.82	0.96	0.96
	(0.08)	(0.10)	(0.11)	(0.14)	(0.16)	(0.15)	(0.08)	(0.10)	(0.16)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
30	0.87	0.91	0.92	0.60	0.80	0.78	0.98	1.00	0.99
	(0.06)	(0.06)	(0.09)	(0.06)	(0.08)	(0.08)	(0.06)	(0.06)	(0.10)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
31	1.16	0.81	1.93	0.71	0.82	1.28	1.21	0.94	1.56
	(0.15)	(0.10)	(0.41)	(0.15)	(0.18)	(0.31)	(0.16)	(0.12)	(0.36)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
32	1.11	1.39	1.57	1.21	1.70	2.00	1.02	1.36	1.23
	(0.09)	(0.12)	(0.27)	(0.17)	(0.27)	(0.57)	(0.08)	(0.12)	(0.22)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
33	0.90	0.95	0.94	0.79	0.95	0.83	0.94	0.96	0.98
	(0.04)	(0.04)	(0.06)	(0.07)	(0.09)	(0.10)	(0.05)	(0.06)	(0.09)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
34	0.85	1.11	0.93	1.09	0.73	1.63	0.84	1.19	0.94
	(0.14)	(0.19)	(0.25)	(0.24)	(0.15)	(0.49)	(0.13)	(0.19)	(0.27)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]

Table B.1.2.: Individual Prospect Theory parameter estimates

	Both Weeks Jointly			Week 1			Week 2		
	α	β	λ	α_1	β_1	λ_1	α_2	β_2	λ_2
35	0.78	1.22	1.50	0.90	1.43	1.28	0.76	1.11	1.75
	(0.08)	(0.13)	(0.26)	(0.16)	(0.29)	(0.38)	(0.09)	(0.14)	(0.38)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
36	0.93	0.95	1.20	0.83	0.93	1.22	0.93	1.02	1.00
	(0.09)	(0.09)	(0.17)	(0.15)	(0.17)	(0.28)	(0.10)	(0.11)	(0.19)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
37	1.05	0.94	1.26	1.29	0.81	2.11	0.98	1.00	0.97
	(0.08)	(0.07)	(0.14)	(0.16)	(0.09)	(0.42)	(0.07)	(0.07)	(0.11)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
38	0.94	0.99	1.12	0.73	1.03	0.90	0.98	1.03	1.10
	(0.06)	(0.07)	(0.12)	(0.09)	(0.13)	(0.14)	(0.07)	(0.08)	(0.15)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
39	0.57	1.06	0.92	0.55	1.36	0.79	0.57	0.98	0.96
	(0.08)	(0.15)	(0.17)	(0.15)	(0.42)	(0.30)	(0.10)	(0.17)	(0.23)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.02]	[0.00]	[0.00]	[0.00]
40	1.10	1.17	4.89	1.34	1.92	2.67	1.10	0.93	7.64
	(0.19)	(0.21)	(2.17)	(0.41)	(0.68)	(1.85)	(0.20)	(0.17)	(4.08)
	[0.00]	[0.00]	[0.04]	[0.01]	[0.01]	[0.17]	[0.00]	[0.00]	[0.08]
41	0.82	0.60	1.50	1.03	0.61	1.77	0.83	0.53	1.81
	(0.18)	(0.13)	(0.37)	(0.48)	(0.28)	(1.06)	(0.24)	(0.16)	(0.73)
	[0.00]	[0.00]	[0.00]	[0.05]	[0.05]	[0.12]	[0.00]	[0.00]	[0.03]
42	0.77	0.97	0.94	0.84	0.94	0.98	0.77	0.95	1.02
	(0.11)	(0.14)	(0.19)	(0.23)	(0.27)	(0.36)	(0.14)	(0.17)	(0.30)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.02]	[0.00]	[0.00]	[0.00]
43	1.29	1.25	0.92	1.47	0.85	1.75	1.42	1.20	1.46
	(0.38)	(0.36)	(0.44)	(0.81)	(0.44)	(1.45)	(0.51)	(0.42)	(1.10)
	[0.00]	[0.00]	[0.05]	[0.09]	[0.07]	[0.25]	[0.02]	[0.01]	[0.21]
44	1.03	0.93	1.08	0.73	1.62	0.40	1.01	0.90	1.04
	(0.19)	(0.17)	(0.27)	(0.24)	(0.65)	(0.29)	(0.19)	(0.17)	(0.29)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.03]	[0.19]	[0.00]	[0.00]	[0.00]
45	0.80	0.71	1.94	0.75	1.16	1.57	0.79	0.61	2.09
	(0.14)	(0.12)	(0.51)	(0.27)	(0.46)	(0.79)	(0.17)	(0.13)	(0.71)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.02]	[0.07]	[0.00]	[0.00]	[0.01]
46	0.84	1.29	0.93	0.97	1.26	1.11	0.79	1.30	0.85
	(0.05)	(0.09)	(0.11)	(0.12)	(0.17)	(0.23)	(0.06)	(0.11)	(0.14)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]

Table B.1.2.: Individual Prospect Theory parameter estimates

	Both Weeks Jointly			Week 1			Week 2		
	α	β	λ	α_1	β_1	λ_1	α_2	β_2	λ_2
47	1.25	1.10	1.11	0.86	1.59	0.55	1.19	1.17	0.77
	(0.27)	(0.23)	(0.36)	(0.33)	(0.72)	(0.43)	(0.27)	(0.26)	(0.30)
	[0.00]	[0.00]	[0.01]	[0.02]	[0.04]	[0.22]	[0.00]	[0.00]	[0.02]
48	0.93	1.08	1.28	1.02	1.37	0.74	1.01	0.89	2.25
	(0.17)	(0.20)	(0.40)	(0.33)	(0.47)	(0.42)	(0.21)	(0.18)	(0.94)
	[0.00]	[0.00]	[0.01]	[0.01]	[0.01]	[0.10]	[0.00]	[0.00]	[0.03]
49	1.31	0.66	6.92	0.48	0.57	1.53	3.99	0.54	$2.1E+03$
	(0.44)	(0.20)	(6.11)	(0.26)	(0.29)	(0.66)	(2.65)	(0.19)	$(1.2E+04)$
	[0.01]	[0.00]	[0.27]	[0.08]	[0.07]	[0.04]	[0.15]	[0.01]	[0.86]
50	0.80	0.95	1.42	0.99	0.94	2.49	0.69	1.02	0.95
	(0.07)	(0.09)	(0.20)	(0.12)	(0.11)	(0.46)	(0.05)	(0.07)	(0.11)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
51	1.21	1.08	5.75	0.81	1.13	1.59	1.81	0.88	37.63
	(0.35)	(0.31)	(4.34)	(0.38)	(0.57)	(1.07)	(0.74)	(0.29)	(63.84)
	[0.00]	[0.00]	[0.20]	[0.05]	[0.07]	[0.16]	[0.03]	[0.01]	[0.56]
52	0.77	1.22	0.88	0.74	0.93	0.97	0.81	1.29	0.95
	(0.12)	(0.21)	(0.25)	(0.23)	(0.29)	(0.36)	(0.17)	(0.29)	(0.40)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.02]	[0.00]	[0.00]	[0.03]
53	0.96	1.04	1.21	0.70	0.98	0.85	1.12	1.04	1.64
	(0.11)	(0.12)	(0.23)	(0.14)	(0.21)	(0.22)	(0.15)	(0.14)	(0.45)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
54	1.06	1.05	1.03	1.64	1.65	1.15	0.87	0.92	0.89
	(0.11)	(0.11)	(0.16)	(0.29)	(0.29)	(0.43)	(0.08)	(0.08)	(0.12)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.02]	[0.00]	[0.00]	[0.00]
55	0.96	0.70	1.40	0.91	0.59	1.49	1.01	0.73	1.44
	(0.19)	(0.14)	(0.33)	(0.39)	(0.25)	(0.71)	(0.27)	(0.19)	(0.56)
	[0.00]	[0.00]	[0.00]	[0.04]	[0.03]	[0.06]	[0.00]	[0.00]	[0.02]
56	1.05	0.96	2.87	1.26	1.19	3.01	1.01	0.87	3.01
	(0.11)	(0.10)	(0.61)	(0.27)	(0.25)	(1.23)	(0.13)	(0.11)	(0.80)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.03]	[0.00]	[0.00]	[0.00]
57	0.84	0.68	0.87	0.72	0.66	0.77	0.88	0.71	0.88
	(0.11)	(0.09)	(0.09)	(0.19)	(0.18)	(0.18)	(0.15)	(0.12)	(0.15)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
58	0.96	0.97	0.98	1.02	1.01	1.02	0.94	0.96	0.96
	(0.07)	(0.07)	(0.10)	(0.16)	(0.16)	(0.23)	(0.09)	(0.09)	(0.15)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]

Table B.1.2.: Individual Prospect Theory parameter estimates

	Both Weeks Jointly			Week 1			Week 2		
	α	β	λ	α_1	β_1	λ_1	α_2	β_2	λ_2
59	0.92	0.94	0.97	0.99	0.99	1.01	0.89	0.93	0.94
	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)	(0.06)	(0.02)	(0.02)	(0.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
60	1.50	0.77	6.96	1.96	0.81	9.56	2.92	0.48	210.69
	(0.36)	(0.16)	(4.73)	(1.07)	(0.33)	(14.70)	(1.34)	(0.15)	(581.38)
	[0.00]	[0.00]	[0.16]	[0.09]	[0.03]	[0.53]	[0.05]	[0.01]	[0.72]
61	0.83	1.08	1.46	1.43	1.75	1.77	0.69	0.90	1.42
	(0.10)	(0.13)	(0.29)	(0.24)	(0.32)	(0.64)	(0.06)	(0.08)	(0.20)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.02]	[0.00]	[0.00]	[0.00]
62	1.70	1.87	2.71	2.36	3.03	3.34	1.50	1.59	2.53
	(0.21)	(0.24)	(0.98)	(0.65)	(0.94)	(3.03)	(0.19)	(0.21)	(0.97)
	[0.00]	[0.00]	[0.01]	[0.00]	[0.01]	[0.29]	[0.00]	[0.00]	[0.02]
63	1.09	0.89	2.01	1.38	1.58	2.06	0.94	0.77	1.72
	(0.16)	(0.13)	(0.52)	(0.42)	(0.51)	(1.30)	(0.15)	(0.12)	(0.46)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.14]	[0.00]	[0.00]	[0.00]
64	1.05	1.03	1.81	1.28	0.96	2.28	1.01	1.01	1.85
	(0.15)	(0.14)	(0.47)	(0.37)	(0.26)	(1.15)	(0.18)	(0.18)	(0.65)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.07]	[0.00]	[0.00]	[0.01]

Notes: Standard errors in parantheses, p-values in square brackets.

Table B.1.3.: Probability weight within stability band per PT parameter

	α_1	β_1	λ_1	α_2	β_2	λ_2	Min	Stable
1	0.07	0.05	0.00	0.20	0.19	0.01	0.00	0
2	0.25	0.04	0.10	0.11	0.20	0.02	0.02	0
3	0.25	0.06	0.07	0.45	0.32	0.25	0.06	1
4	0.14	0.10	0.15	0.33	0.32	0.43	0.10	1
5	0.20	0.14	0.27	0.26	0.25	0.17	0.14	1
6	0.15	0.13	0.39	0.32	0.31	0.61	0.13	1
7	0.07	0.05	0.04	0.09	0.12	0.04	0.04	0
8	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1
9	0.15	0.20	0.15	0.44	0.35	0.19	0.15	1
10	0.62	0.68	0.84	0.85	0.89	0.93	0.62	1

Table B.1.3.: Probability weight within stability band per PT parameter

	α_1	β_1	λ_1	α_2	β_2	λ_2	Min	Stable
11	0.03	0.07	0.00	0.02	0.11	0.00	0.00	0
12	0.03	0.15	0.02	0.00	0.00	0.00	0.00	0
13	0.13	0.04	0.16	0.18	0.18	0.06	0.04	0
14	0.00	0.21	0.00	0.01	0.34	0.00	0.00	0
15	0.07	0.12	0.10	0.21	0.19	0.31	0.07	1
16	0.10	0.07	0.11	0.14	0.16	0.07	0.07	1
17	0.09	0.03	0.15	0.24	0.14	0.30	0.03	0
18	0.18	0.06	0.05	0.29	0.23	0.20	0.05	0
19	0.18	0.11	0.26	0.24	0.18	0.14	0.11	1
20	0.35	0.27	0.62	0.36	0.45	0.26	0.26	1
21	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1
22	0.02	0.06	0.01	0.09	0.12	0.08	0.01	0
23	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1
24	0.07	0.08	0.22	0.25	0.20	0.32	0.07	1
25	0.06	0.10	0.03	0.11	0.13	0.05	0.03	0
26	0.06	0.10	0.08	0.23	0.25	0.24	0.06	1
27	0.11	0.04	0.11	0.24	0.25	0.48	0.04	0
28	0.14	0.12	0.29	0.32	0.19	0.37	0.12	1
29	0.27	0.28	0.53	0.48	0.34	0.24	0.24	1
30	0.00	0.24	0.34	0.15	0.35	0.64	0.00	0
31	0.00	0.26	0.03	0.27	0.23	0.15	0.00	0
32	0.22	0.09	0.12	0.30	0.38	0.14	0.09	1
33	0.23	0.49	0.50	0.59	0.70	0.73	0.23	1
34	0.11	0.02	0.07	0.34	0.22	0.32	0.02	0
35	0.21	0.13	0.20	0.47	0.25	0.19	0.13	1
36	0.23	0.27	0.32	0.42	0.34	0.28	0.23	1
37	0.09	0.21	0.03	0.39	0.45	0.06	0.03	0
38	0.05	0.33	0.23	0.47	0.48	0.55	0.05	0
39	0.29	0.09	0.28	0.44	0.25	0.37	0.09	1

Table B.1.3.: Probability weight within stability band per PT parameter

	α_1	β_1	λ_1	α_2	β_2	λ_2	Min	Stable
40	0.09	0.04	0.02	0.22	0.10	0.02	0.02	0
41	0.08	0.17	0.08	0.18	0.27	0.11	0.08	1
42	0.18	0.17	0.25	0.31	0.26	0.29	0.17	1
43	0.05	0.07	0.05	0.08	0.11	0.07	0.05	1
44	0.09	0.04	0.02	0.23	0.27	0.30	0.02	0
45	0.16	0.06	0.10	0.26	0.27	0.12	0.06	1
46	0.20	0.26	0.28	0.51	0.39	0.52	0.20	1
47	0.07	0.05	0.09	0.16	0.17	0.16	0.05	1
48	0.13	0.08	0.10	0.20	0.15	0.06	0.06	1
49	0.00	0.15	0.00	0.01	0.20	0.00	0.00	0
50	0.10	0.41	0.01	0.13	0.40	0.00	0.00	0
51	0.07	0.08	0.00	0.04	0.13	0.00	0.00	0
52	0.20	0.10	0.24	0.26	0.15	0.22	0.10	1
53	0.06	0.21	0.12	0.18	0.33	0.13	0.06	1
54	0.02	0.02	0.20	0.03	0.18	0.39	0.02	0
55	0.11	0.17	0.13	0.16	0.24	0.16	0.11	1
56	0.12	0.12	0.07	0.32	0.29	0.11	0.07	1
57	0.19	0.26	0.41	0.29	0.37	0.57	0.19	1
58	0.25	0.28	0.38	0.45	0.47	0.56	0.25	1
59	0.40	0.64	0.92	0.90	0.98	0.99	0.40	1
60	0.04	0.14	0.01	0.02	0.05	0.00	0.00	0
61	0.01	0.02	0.13	0.08	0.07	0.42	0.01	0
62	0.04	0.02	0.03	0.14	0.09	0.09	0.02	0
63	0.08	0.04	0.07	0.17	0.25	0.16	0.04	0
64	0.10	0.17	0.07	0.24	0.26	0.14	0.07	1

Notes: Stability band defined as +/-1.96 standard errors of the pooled estimation of the respective parameter around the participant's individual joint estimate. See Subsection 2.4.2 for further details.

Table B.1.4.: Plan and commitment quality per subject

		Plan	Commit	Commit
	CE Plan	Decent	WTP-CE	Decent
1	−3.83	0	0.05	1
2	−3.43	0	0.80	1
3	−0.19	0	−0.60	0
4	0.46	1	0.00	1
5	0.24	1	0.50	1
6	0.76	1	−1.55	0
7	1.55	1	0.60	1
8	0.96	1	4.85	1
9	−1.74	0	−1.45	0
10	0.96	1	4.95	1
11	3.06	1	5.35	1
12	0.49	1	4.85	1
13	−1.55	0	0.70	1
14	10.88	1	7.15	1
15	0.07	1	−0.25	0
16	0.65	1	−1.10	0
17	0.00	1	−1.90	0
18	−0.28	0	0.05	1
19	−0.48	0	0.00	1
20	0.43	1	−2.65	0
21	1.03	1	4.75	1
22	3.65	1	4.75	1
23	1.05	1	4.75	1
24	1.29	1	−3.65	0
25	−1.47	0	0.00	1
26	−0.87	0	0.60	1
27	0.55	1	−1.90	0
28	0.16	1	−1.10	0

Table B.1.4.: Plan and commitment quality per subject

🯅	CE Plan	Plan Decent	Commit WTP-CE	Commit Decent
29	0.04	1	−4.75	0
30	0.46	1	0.35	1
31	0.58	1	0.00	1
32	−1.47	0	0.00	1
33	0.23	1	1.75	1
34	−1.23	0	4.75	1
35	−1.26	0	5.05	1
36	0.26	1	−0.15	0
37	0.13	1	4.95	1
38	0.21	1	−1.50	0
39	−0.13	0	−0.05	0
40	−5.97	0	1.10	1
41	0.00	0	0.60	1
42	−0.01	0	0.00	1
43	0.79	1	4.80	1
44	1.26	1	−0.15	0
45	−0.41	0	0.00	1
46	0.12	1	−0.05	0
47	2.30	1	−0.55	0
48	−1.05	0	1.90	1
49	8.94	1	−0.50	0
50	−0.36	0	0.00	1
51	−122.64	0	−2.15	0
52	−1.28	0	0.90	1
53	−0.07	0	−1.15	0
54	0.22	1	−1.75	0
55	0.60	1	3.10	1
56	−2.07	0	0.20	1

Table B.1.4.: Plan and commitment quality per subject

⚲	CE Plan	Plan Decent	Commit WTP-CE	Commit Decent
57	0.71	1	2.50	1
58	0.45	1	−4.80	0
59	0.57	1	0.00	1
60	−23131.44	0	0.15	1
61	−0.53	0	3.05	1
62	−0.13	0	0.00	1
63	−0.13	0	−3.50	0
64	−0.72	0	0.05	1

Notes: Certainty equivalent plan is relative to never investing, plan is decent if $CE > 0$; CE commit is plan relative to play, commitment is decent if $WTP > CE$.

B.2. Replication Realization Treatment

The design of our investment game over four rounds is borrowed from Imas (2016). In particular, we replicated his realization treatment. That is, we have both a "paper" and a "realization" group among our subjects and can estimate the realization treatment effect. We find significant differences relative to Imas (2016) and discuss them in the following.

Imas compares the difference of round 4 and round 3 investments between the treatment and control groups by means of a t-test. If a subject invests more in round 4 than in round 3, the difference is positive. Therefore, positive signs correspond to an increase in risk taking, while negative signs indicate a reduction of risk taking.

As Imas (2016) makes predictions only for the loss domain, he restricts the analysis to those subjects who were in losses after the third round.

In Table B.2.1, we report the investment change between rounds 4 and 3 of the realization and control groups. The first column reports the estimates of Imas (2016) for comparison, the second column reports our replication analysis based on his data, and the third column reports the respective analysis of our data.

Table B.2.1.: Comparing realization effects

	Imas (2016)	Replication	Our Data
Paper Treatment	+0.23***	0.28***	0.16
Realization Treatment	−0.15***	−0.15***	0.01
Total Effect	−0.38***	−0.42***	−0.18
N (Paper/Realized)	53	54(28/26)	41(13/30)

*** 1% significance level; ** 5% significance level; * 10% significance level;

In the fourth row we display the number of observations and distinguish between subjects in the paper and the realization group. The first number in brackets denotes the observations in the paper group, the second the observations in the realization group.

Although our control group, i.e. the paper treatment, has only 13 observations, the effect has a p-value of 0.13 and, therefore, barely misses the 10% significance level. Compared to the results of Imas (2016), our effect is of same sign and also of same magnitude when taking the relation of endowments into account (USD 2 vs. EUR 1.60). Therefore, the effect of paper losses is robust between both experiments. Subjects invest more in round four than in round three if they only experienced losses so far.

However, when we compare the results of the realization treatment, we find huge differences between Imas (2016) and our experiment. Imas has a strong negative effect, that is significant at the 1% significance level, whereas we have a null effect. It is important to note that our result is not driven by lack of statistical power, as our realization treatment has more observations than Imas. Our results strongly suggest that there is no effect in our data at all.

Our experiment differed to Imas (2016) in two dimensions. First, our subjects were asked beforehand to make a complete contingent plan. The plan forced them to anticipate how they would feel when they experience losses. Perhaps this anticipation on its own made them aware on how to close the mental frame, thereby reducing the effect of realization. Although this explanation is appealing, it cannot account for the fact that we still find a strong effect of the paper treatment. Additionally, it is far fetched to assume that the plan is able to influence the mental frame of decisions made one week later. Second, our subjects had to do the Prospect Theory elicitation task, before they could actually choose their investments. The elicitation task could have interfered with the investment decisions as both entailed decisions in the loss domain. However, one would expect that all investment decisions becomes noisier and not only the realization treatment.

Thus, it remains unclear as to why the results of Imas (2016) and our experiment differ significantly in the realization treatment.

In order to discuss how robust the results of Imas (2016) are without the inclusion of covariables, we can take a look if his predictions are backed by his own data. Although it is not explicitly stated, in all rounds of the investment game previous to the fourth round decision makers are in the paper loss control group. Since their losses are not realized subjects who experience losses according to Imas' theory are supposed to increase their invested shares.

We test this hypothesis for each round separately in columns (1) and (3). In columns (2) and (4) we condition the results in the subject being in the treatment group. Being in the treatment group should not influence investment decisions in previous rounds, because Imas hypothesized that only the act of taking away money closes the mental account. Table B.2.2 presents our results.

Although conditioning on the realization treatment results in different effect signs in round 2, all effects remain insignificant. Therefore, we know that randomization in treatment and control group worked. However, while the effect of being in the losses should lead to increased risk taking in the subsequent round, the effects in Imas' data set are insignificant at best. It is important to note, that almost all effects point in the opposite direction. The effect in column (1) has a p-value of 0.13, barely missing the 10% significance. With an effect size of -0.134 it indicates that subjects reduced their investment after a loss in the second round by USD 0.13.

The missing effects in the previous rounds are troubling for the theory of Imas. Although his treatment effect is significant and goes in the predicted direction,

Table B.2.2.: Effect of losses on risk taking in previous rounds

	(1) $\Delta_{3,2}$	(2) $\Delta_{3,2}\vert_{\text{real}}$	(3) $\Delta_{2,1}$	(4) $\Delta_{2,1}\vert_{\text{real}}$
Loss	-0.134	-0.0666	0.0111	-0.0666
	(0.0878)	(0.111)	(0.0949)	(0.129)
Constant	0.0741	-0.0227	-0.0926	-0.0227
	(0.0717)	(0.0938)	(0.0775)	(0.109)
Observations	81	39	81	39
R^2	0.029	0.010	0.000	0.007

*** 1% significance level; ** 5% significance level; * 10% significance level;

theory also has clear predictions for the previous investment rounds. That there are null results at best, that point in the opposite direction casts serious doubt if the results for the treatment effect could have a different cause than loss aversion and mental accounts. Therefore it is inevitable for further research to also establish a link between investment decisions and Prospect Theory parameters of subjects. Only through this link it can be unambiguously established that increasing risk seeking is driven by Prospect Theory behavior.

B.3. Instructions

In the following, I provide the original instructions (in German language) as used in the experiment. The typeset closely mimics the one of the actual printouts, but page-breaks were skipped and figures are labeled and numbered according to this dissertation.

B.3.1. Week 1

Herzlich willkommen im Regensburg Economic Science Lab RESL und vielen Dank für Ihre Teilnahme am Experiment!

Bitte sprechen Sie ab jetzt nicht mehr mit anderen Teilnehmern und schalten Sie Ihr Mobiltelefon aus. Verhalten Sie sich während des gesamten Experiments ruhig.

Allgemeines zum Ablauf

Dieses Experiment dient der Untersuchung ökonomischen Entscheidungsverhaltens. Sie können dabei Geld verdienen, das Ihnen im Anschluss an das Experiment privat in bar ausbezahlt wird.
Das gesamte Experiment besteht aus zwei zeitlich getrennten Sitzungen. Die erste Sitzung wird etwa 100 Minuten dauern und besteht aus drei Teilen. Die zweite Sitzung wird etwa 80 Minuten dauern und besteht aus zwei Teilen. Zu Beginn jedes Teils erhalten Sie detaillierte Instruktionen. Die Summe Ihres Verdienstes aus allen Teilen ergibt Ihren Gesamtverdienst aus dem Experiment. Dieser wird Ihnen nach Abschluss des zweiten Teils mitgeteilt und am Ende des Experiments einzeln und in bar ausbezahlt.
Während des Experiments werden Sie darum gebeten, Entscheidungen zu treffen. Ihre Entscheidungen haben keinen Einfluss auf die Auszahlungen der anderen Teilnehmer, nur auf Ihre eigene Auszahlung.
Sie erhalten in der ersten Sitzung eine Platzkarte, damit Sie in der zweiten Sitzung denselben Platz einnehmen können. Bringen Sie diese Karte unbedingt zur zweiten Sitzung mit!

Bezahlung
Während des Experiments berechnen sich Verdienste direkt in Euro. Zusätzlich zu dem Einkommen, das Sie während des Experiments verdienen können, erhalten Sie 5 Euro für Ihr pünktliches Erscheinen je Sitzung. Bitte berücksichtigen Sie, dass der Gesamtbetrag erst nach der zweiten Sitzung ausgezahlt wird. Daher ist es unbedingt notwendig, dass Sie auch zur zweiten Sitzung erscheinen und Ihre Platzkarte mitbringen.

Anonymität
Keiner der anderen Teilnehmer wird Ihre Entscheidungen im Experiment nachvollziehen können. Darüber hinaus werden die Daten aus dem Experiment ausschließlich anonym ausgewertet. Am Ende des Experiments müssen Sie eine Quittung

über den Erhalt des Verdienstes unterschreiben. Diese dient nur der Abrechnung und wird nicht dazu verwendet, Ihre persönlichen Daten mit Ihren Entscheidungen zu verknüpfen. Ihr Name wird zu keinem Zeitpunkt mit Ihrem Verhalten im Experiment kombiniert. Die verteilten Platzkarten enthalten Ihren Namen, um sicherzustellen, dass auch tatsächlich Sie und niemand anderes an der zweiten Sitzung teilnehmen. Die Platzkarten verbleiben sowohl während, als auch nach dem Experiment, in Ihrem Besitz.

Hilfsmittel
An Ihrem Platz finden Sie einen Kugelschreiber und einen Taschenrechner. Bitte lassen Sie beide nach dem Experiment auf dem Tisch liegen. Bitte lassen Sie auch Ihre Notizen auf dem Tisch liegen, diese werden direkt im Anschluss vernichtet.
Sollten Sie nach den Instruktionen oder während des Experiments Fragen haben, heben Sie bitte die Hand. Einer der Experimentleiter wird dann zu Ihnen kommen und Ihre Fragen unter vier Augen beantworten.

Sonstiges
Bitte verwenden Sie als Trennzeichen bei Kommazahlen einen Punkt anstelle des Kommas. Beispielsweise verwende Sie “6.40” für den Betrag “6 Euro und 40 Cent”.

Teil 1

Ablauf

Nächste Woche werden Sie hintereinander vier voneinander unabhängige Investitionsentscheidungen treffen. Sie bekommen für jede der vier Runden ein Startkapital von je 1,60 Euro. Das Geld wird Ihnen zu Beginn des Experiments nächste Woche in einem Umschlag in bar ausgehändigt.

In jeder Runde müssen Sie entscheiden, welchen Teilbetrag (in 20-Cent-Schritten) Ihres Startkapitals Sie investieren möchten. Der nichtinvestierte Teil Ihres Startkapitals wird Ihrem Vermögen eins-zu-eins gutgeschrieben.

Die Auszahlung Ihres Investments ist im Durchschnitt höher, hängt allerdings vom Zufall ab: Sie wählen in jeder Runde Ihre Erfolgszahl zwischen 1 und 6. Ein zufällig bestimmter Teilnehmer würfelt dann die gültige Erfolgszahl aus. Stimmt Ihre selbstgewählte Erfolgszahl mit der anschließend ausgewürfelten Erfolgszahl überein (dies geschieht mit einer Wahrscheinlichkeit von 16,67%), wird Ihnen der siebenfache Investitionsbetrag gutgeschrieben; andernfalls erhalten Sie den Investitionsbetrag nicht zurück.

Die Eingabemaske der Investitionsentscheidungen sieht dabei wie folgt aus:

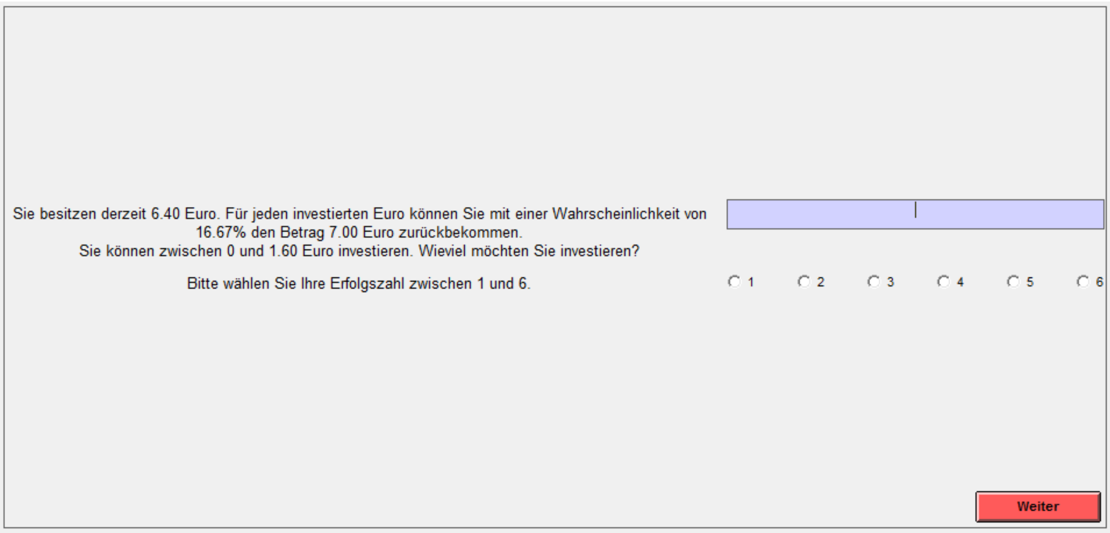


Abbildung B.3.1.: Investitionsentscheidung mit Glückszahl

Beispiel:
Sie investieren 80 Cent und entscheiden sich für die "4" als Erfolgszahl. Wenn der zufällig bestimmte Teilnehmer die "4" würfelt, erhalten Sie 5,60 Euro aus dem Investment zurück. Zusätzlich erhalten Sie den nichtinvestierten Betrag von 80 Cent. Insgesamt werden Ihnen also 6,40 Euro für diese Runde gutgeschrieben.
Würfelt der Teilnehmer eine andere Zahl, erhalten Sie lediglich den nichtinvestierten Betrag von 80 Cent zurück.
Sie treffen diese Entscheidung für jede der vier Runden neu und können maximal Ihr jeweiliges Startkapital investieren.

Heute sollen Sie planen, welche Entscheidungen Sie in der kommenden Woche treffen möchten.

In der Planungsphase können Sie für jede Runde eingeben, wieviel Sie investieren sollten. Sie finden jeweils links auf Ihrem Bildschirm die Auszahlung im Gewinnfall und rechts für den Verlustfall. Für jeden der Fälle können Sie dann individuell weiterplanen.

Ihr Bildschirm sieht dabei wie folgt aus:

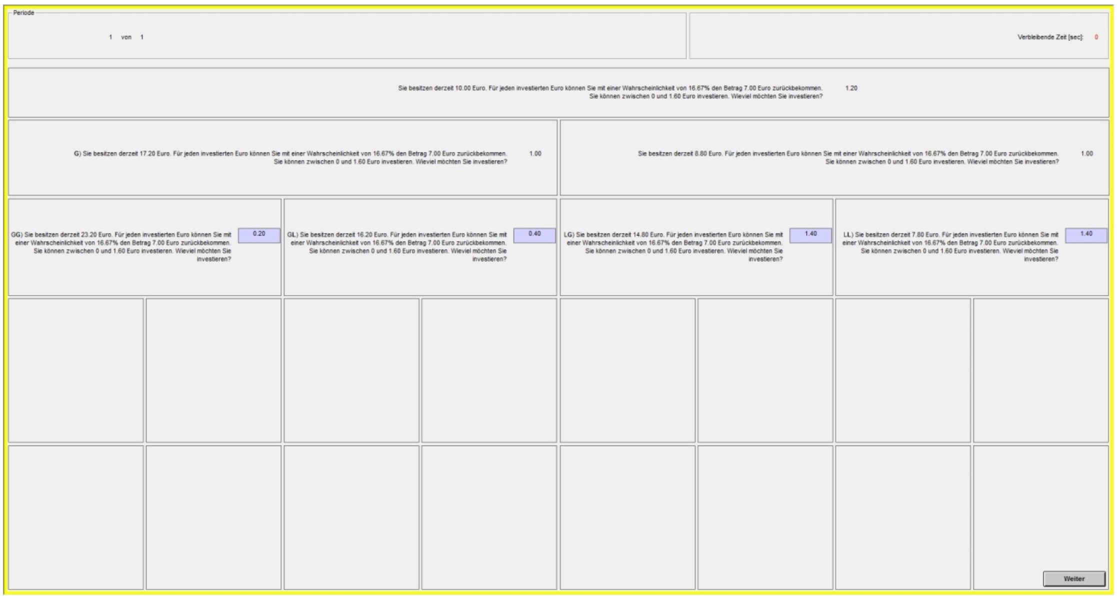


Abbildung B.3.2.: Beispiel für Ihre Vorhersage

Sie treffen die Investitionsentscheidungen also zweimal: Einmal heute als Plan und einmal nächste Woche tatsächlich.

Es wird allerdings nur entweder ihr heutiger Plan oder ihre Investitionsentscheidungen von nächster Woche umgesetzt.

Sie entscheiden 45-mal, ob Sie die Umsetzung Ihres heutigen Planes und einen Geldbetrag möchten, oder die Umsetzung Ihrer Investitionsentscheidungen von nächster Woche. Die 45 Entscheidungen variieren im Geldbetrag, der sowohl positiv (Gutschrift, +), als auch negativ (Abzug vom Guthaben, -) sein kann.

Eine dieser Entscheidungen wird nächste Woche ganz zum Schluss zufällig ausgewählt und umgesetzt. Unabhängig davon, wie Sie sich bei diesen 45 Entscheidungen entscheiden, müssen Sie nächste Woche in jedem Fall die tatsächlichen Investitionsentscheidungen treffen.

Ihr Bildschirm sieht dabei aus wie folgt:

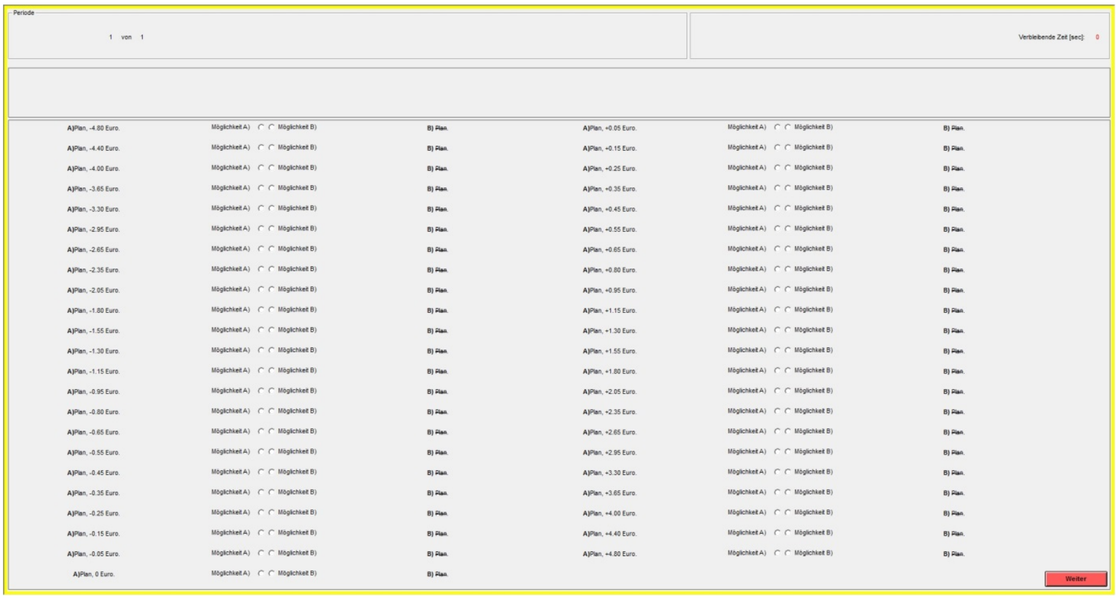


Abbildung B.3.3.: Soll Ihre Vorhersage umgesetzt werden?

Sobald Sie diese Informationen gelesen und verstanden haben, können Sie auf „Weiter“ klicken.

Teil 2

Ablauf

In diesem Teil bekommen Sie zwei Aufgaben gestellt.

Als erstes sollen Sie drei Rechenaufgaben lösen. Für jede richtig gelöste Aufgabe werden Ihnen 0,50 Euro gutgeschrieben.

Als zweites bitten wir Sie, einen kurzen Fragebogen auszufüllen. Dieser enthält Aussagen zu Ihrer Person, die Sie auf einer Sieben-Punkte-Skala von „trifft nicht auf mich zu“ bis „trifft sehr auf mich zu“ qualifizieren sollen. Wir bitten Sie, diese wahrheitsgemäß zu beantworten. Für das Ausfüllen des Fragebogens schreiben wir Ihrem Vermögen 2 Euro gut. Ihr Bildschirm sieht dabei aus wie folgt:

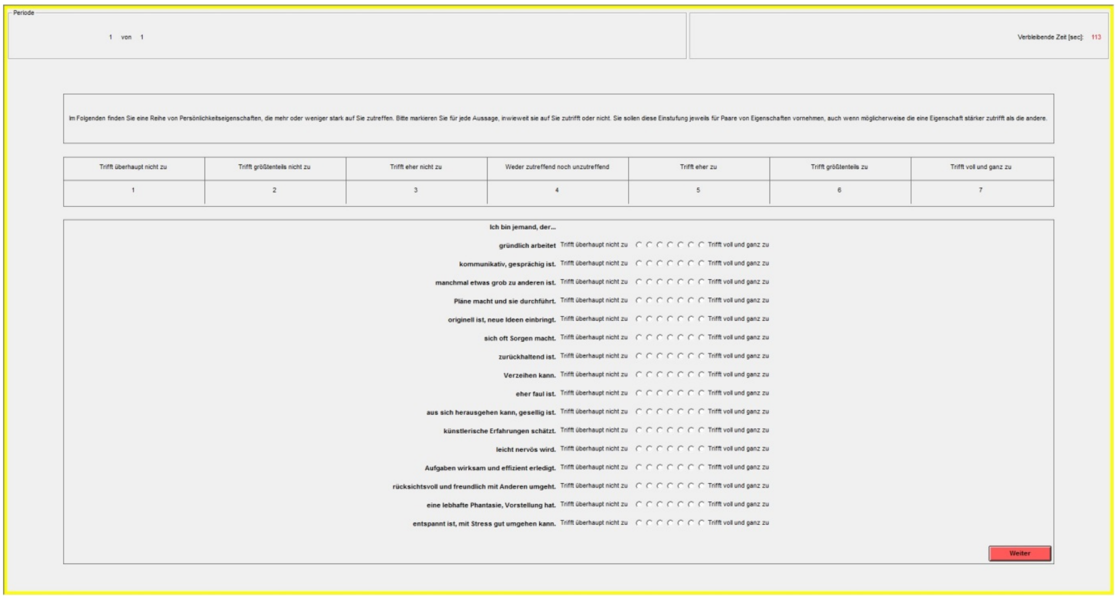


Abbildung B.3.4.: Fragebogen

Teil 3

Ablauf

In diesem Teil treffen Sie eine Reihe an Entscheidungen zwischen einem sicheren Geldbetrag (links) und einer Lotterie (rechts). Die Lotterie führt zu einer zufälligen Auszahlung von einem von zwei Beträgen, die sowohl positiv als auch negativ sein können. Jeder Betrag kommt mit 50% Wahrscheinlichkeit zur Auszahlung.

Ihr Bildschirm sieht dabei aus wie folgt:

Abbildung B.3.5.: Beispiel Sichere Option vs. Lotterie

Nächste Woche werden Sie u.a. eine Reihe ähnlicher Entscheidungen treffen. Am Ende des Experiments werden wir eine Ihrer Entscheidungen (aus beiden Wochen) zufällig ermitteln und ausführen. Positive Beträge werden Ihrem Einkommen gutgeschrieben, negative davon abgezogen.

Ende dieser Sitzung
Die Auszahlung für die Teilnahme gibt es erst nach der Teilnahme an der zweiten Sitzung. Dennoch bitten wir Sie, auf Ihrem Platz sitzen zu bleiben, bis ein Experimentator Bescheid gibt, dass das Labor verlassen werden kann. Vergessen Sie nicht, Ihre Platzkarte in einer Woche wieder mitzubringen!

B.3.2. Week 2

Herzlich willkommen im Regensburg Economic Science Lab RESL und vielen Dank für Ihre Teilnahme am Experiment!

Bitte sprechen Sie ab jetzt nicht mehr mit anderen Teilnehmern und schalten Sie Ihr Mobiltelefon aus. Verhalten Sie sich während des gesamten Experiments ruhig.

Allgemeines zum Ablauf

Diese Sitzung ist der zweite Teil des in der vergangenen Woche gestarteten Experiments. Bitte stellen Sie sicher, dass Sie an dem Rechner derselben Platznummer sitzen, wie auf Ihrer Platzkarte vermerkt ist. Diese Sitzung dauert voraussichtlich 80 Minuten und besteht aus zwei Teilen.

Bezahlung

Am Ende dieser Sitzung wird Ihnen Ihr Verdienst aus beiden Sitzungen in bar ausbezahlt. Wir kommen dazu zu Ihnen an den Platz. Um die Anonymität zu wahren, bitten wir Sie, während der Auszahlung weiter an Ihrem Platz zu bleiben. Sobald Sie Ihren Verdienst erhalten und quittiert haben, bitten wir Sie, den Raum leise zu verlassen.

Hilfsmittel

An Ihrem Platz finden Sie einen Kugelschreiber und einen Taschenrechner. Bitte lassen Sie beide nach dem Experiment auf dem Tisch liegen. Bitte lassen Sie auch Ihre Notizen auf dem Tisch liegen, diese werden direkt im Anschluss vernichtet.
Sollten Sie nach den Instruktionen oder während des Experiments Fragen haben, heben Sie bitte die Hand. Einer der Experimentleiter wird dann zu Ihnen kommen und Ihre Fragen unter vier Augen beantworten.

Sonstiges

Bitte verwenden Sie als Trennzeichen bei Kommazahlen einen Punkt anstelle des Kommas. Beispielsweise verwenden Sie "6.40" für den Betrag "6 Euro und 40 Cent".

Teil 1

Ablauf

In diesem Teil treffen Sie eine Reihe von Entscheidungen zwischen einem sicheren Geldbetrag (links) und einer Lotterie (rechts). Die Lotterie führt zu einer zufälligen Auszahlung von einem von zwei Beträgen, die sowohl positiv als auch negativ sein können. Jeder Betrag kommt mit 50% Wahrscheinlichkeit zur Auszahlung.

Ihr Bildschirm sieht dabei aus wie folgt:

Abbildung B.3.6.: Beispiel Sichere Option vs. Lotterie

Am Ende dieser Sitzung werden wir eine Ihrer Entscheidungen (aus beiden Wochen) ermitteln und ausführen. Positive Beträge werden Ihrem Einkommen gutgeschrieben, negative davon abgezogen.

Teil 2

Ablauf

In diesem Teil treffen Sie die Investitionsentscheidungen, die Sie vergangene Woche geplant haben. Die Wahrscheinlichkeiten und Auszahlungen sind dabei dieselben wie letzte Woche. Der wesentliche Unterschied gegenüber letzter Woche besteht darin, dass Sie diesmal nach jeder der vier Investitionsentscheidungen direkt erfahren, ob Ihre Investition erfolgreich gewesen ist oder nicht. Sie müssen daher nicht mehr für jede Kombination aus Gewinnen und Verlusten entscheiden, sondern nur noch für Ihren tatsächlichen Verlauf.

Ihnen wurde ein Umschlag mit 6,40 Euro ausgehändigt; dies ist Ihr gesamtes Startkapital (jeweils 1,60 Euro für jede Investitionsrunde). Die Stückelung beträgt 2 Euro, 2 Euro, 1 Euro, 50 Cent, 50 Cent, 20 Cent, 10 Cent, 10 Cent. Bitte öffnen Sie den Umschlag und vergewissern Sie sich, dass er die genannten Münzen enthält. Belassen Sie das Geld im Umschlag.

In jeder Runde müssen Sie entscheiden, welchen Teilbetrag (in 20-Cent-Schritten) Ihres Startkapitals Sie investieren möchten. Der nichtinvestierte Teil Ihres Startkapitals wird Ihrem Vermögen eins-zu-eins gutgeschrieben.

Die Auszahlung Ihres Investments ist im Durchschnitt höher, hängt allerdings vom Zufall ab: Sie wählen in jeder Runde Ihre Erfolgszahl zwischen 1 und 6. Ein zufällig bestimmter Teilnehmer würfelt dann die gültige Erfolgszahl aus. Stimmt Ihre selbstgewählte Erfolgszahl mit der anschließend ausgewürfelten Erfolgszahl überein (dies geschieht mit einer Wahrscheinlichkeit von 16,67%), wird Ihnen der siebenfache Investitionsbetrag gutgeschrieben; andernfalls erhalten Sie den Investitionsbetrag nicht zurück.

Ihr Bildschirm sieht dabei aus wie folgt:

Beispiel:
Sie investieren 80 Cent und entscheiden sich für die "4" als Erfolgszahl. Wenn der zufällig bestimmte Teilnehmer die "4" würfelt, erhalten Sie 5,60 Euro aus dem Investment zurück. Zusätzlich erhalten Sie den nichtinvestierten Betrag von 80 Cent. Insgesamt werden Ihnen also 6,40 Euro für diese Runde gutgeschrieben. Würfelt der Teilnehmer eine andere Zahl, erhalten Sie lediglich den nichtinvestierten Betrag von 80 Cent zurück.

Sie treffen diese Entscheidung für jede der vier Runden neu und können maximal Ihr jeweiliges Startkapital investieren.

Bezahlung
Während wir die Auszahlung vorbereiten, bitten wir Sie, noch einen Fragebogen

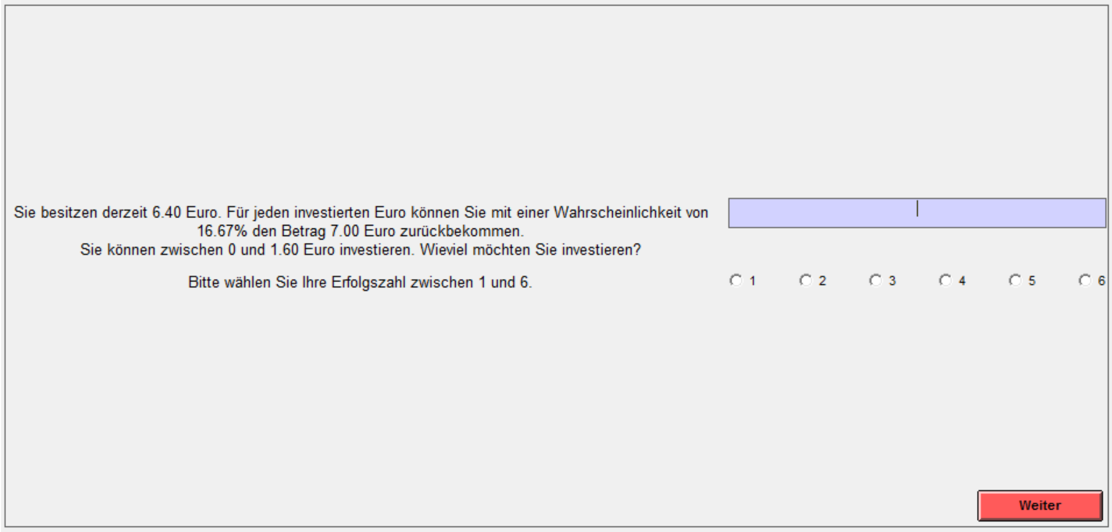


Abbildung B.3.7.: Investitionsentscheidung mit Erfolgszahl

auszufüllen. Sobald alle Teilnehmer diesen abgeschlossen haben, werden wir zu Ihnen an den Platz kommen. Sie erhalten Ihr Einkommen aus beiden Sitzungen ausbezahlt. Um die Anonymität zu wahren, bitten wir Sie, während der Auszahlung weiter an Ihrem Platz zu bleiben. Sobald Sie Ihr Verdienst erhalten und quittiert haben, bitten wir Sie, den Raum leise zu verlassen.

C. Appendix to Chapter 3: Decomposing the Disposition Effect

C.1. Instructions

In the following, I provide the original instructions (in German language) as used in the experiment. The typeset closely mimics the one of the actual printouts, but page-breaks were skipped and figures are labeled and number according to this dissertation. I provide the instructions of the robustness treatment with incentivized beliefs elicitation, as the other instructions are subsets of these, either without incentives for beliefs, or without beliefs altogether.

Herzlich Willkommen im MELESSA, dem *Munich Experimental Laboratory for Economic and Social Sciences*, und vielen Dank für Ihre Teilnahme am Experiment!

Bitte sprechen Sie ab jetzt nicht mehr mit anderen Teilnehmern und ***schalten Sie Ihr Mobiltelefon aus****. Verhalten Sie sich während des gesamten Experiments ruhig.*

Allgemeines zum Ablauf

Dieses Experiment dient der Untersuchung ökonomischen Entscheidungsverhaltens. Sie können dabei Geld verdienen, das Ihnen im Anschluss an das Experiment privat in bar ausbezahlt wird.

Während des Experiments werden Sie eine Reihe von Entscheidungen treffen. Diese beeinflussen ausschließlich Ihre eigene Auszahlung, nicht die Auszahlung der anderen Teilnehmer. Im Folgenden erhalten Sie detaillierte Instruktionen über diese Entscheidungen und auf welche Weise sie Ihre Auszahlung beeinflussen.

Bezahlung

Während des Experiments berechnen sich Verdienste in experimentellen Geldeinheiten (EG), die sich im Verhältnis **2.500 EG = 1 EUR** umrechnen. Zusätzlich zu dem Einkommen, das Sie während des Experiments verdienen, erhalten Sie 10.000 EG (also 4 EUR) für das Ausfüllen eines Fragebogens am Ende des Experiments. Sie können keinesfalls weniger als 5 EUR in diesem Experiment verdienen. Abhängig von Ihren Entscheidungen und dem Zufall kann Ihr Einkommen aber auch auf über 73 EUR steigen. Daher sollten Sie sich Ihre Entscheidungen genau überlegen!

Anonymität

Keiner der anderen Teilnehmer wird Ihre Entscheidungen im Experiment nachvollziehen können. Darüber hinaus werden die Daten aus dem Experiment ausschließlich anonym ausgewertet. Ihr Name wird zu keinem Zeitpunkt mit Ihrem Verhalten im Experiment in Verbindung gebracht.

Hilfsmittel

An Ihrem Platz finden Sie einen Kugelschreiber und einen Taschenrechner. Bitte lassen Sie beide nach dem Experiment auf dem Tisch liegen. Bitte lassen Sie auch

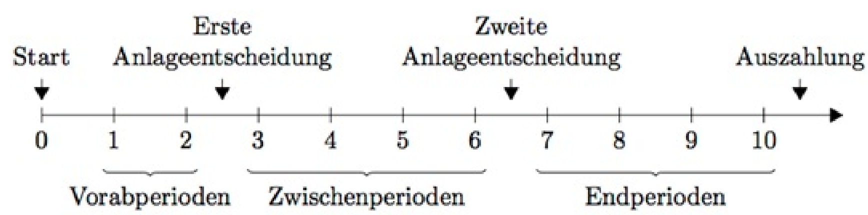
Ihre Instruktionen auf dem Tisch liegen, diese werden direkt im Anschluss an das Experiment vernichtet.

Sollten Sie zu irgendeiner Zeit Fragen haben, drücken Sie bitte die rote Taste (F11) auf Ihrer Tastatur. Einer der Experimentleiter wird dann zu Ihnen kommen und Ihre Fragen unter vier Augen beantworten.

Instruktionen

In diesem Experiment sollen Sie eine Reihe von Anlageentscheidungen treffen. Dazu wird Ihrem Experimentalkonto ein **Startkapital** in Höhe von **20.000 EG** gutgeschrieben.

In jeder der nun folgenden Anlageentscheidungen haben Sie die Möglichkeit, Ihr gesamtes Kapital entweder in Anlage A, Anlage B, oder gar nicht zu investieren. Unabhängig von Ihren Anlageentscheidungen werden Preisverläufe beider Anlagen für **10 Perioden** simuliert und Sie erfahren in jedem Fall, wie sich die beiden Anlagen entwickeln. Hier sehen Sie den zeitlichen Ablauf, der nachfolgend ausführlicher beschrieben wird:



Preisentwicklung

Anlage A startet in Periode 0 bei einem Preis von 200 EG, Anlage B bei einem Preis von 5 EG. Danach verändern sich die Preise beider Anlagen von Periode zu Periode. Dabei **steigt** der Preis einer Anlage entweder **um 30%**, oder er **fällt um 20%**, jeweils gegenüber dem Preis der vorherigen Periode.

Ob der Preis einer Anlage in einer Periode steigt oder fällt hängt ausschließlich von den folgenden Wahrscheinlichkeiten ab: Die **bessere** der beiden Anlagen steigt mit einer Wahrscheinlichkeit von **55%** und fällt mit einer Wahrscheinlichkeit von 45%. Die **schlechtere** der beiden Anlagen **steigt** mit einer Wahrscheinlichkeit von **45%** und fällt mit einer Wahrscheinlichkeit von 55%. Welche der beiden Anlagen die bessere und welche die schlechtere ist, wurde bereits vor Beginn des Experiments (und zufällig für Ihre Teilnehmernummer) festgelegt und bleibt während des gesamten Experiments gleich. Erst ganz am Ende des Experiments werden Sie erfahren, welche Anlage tatsächlich die bessere war.

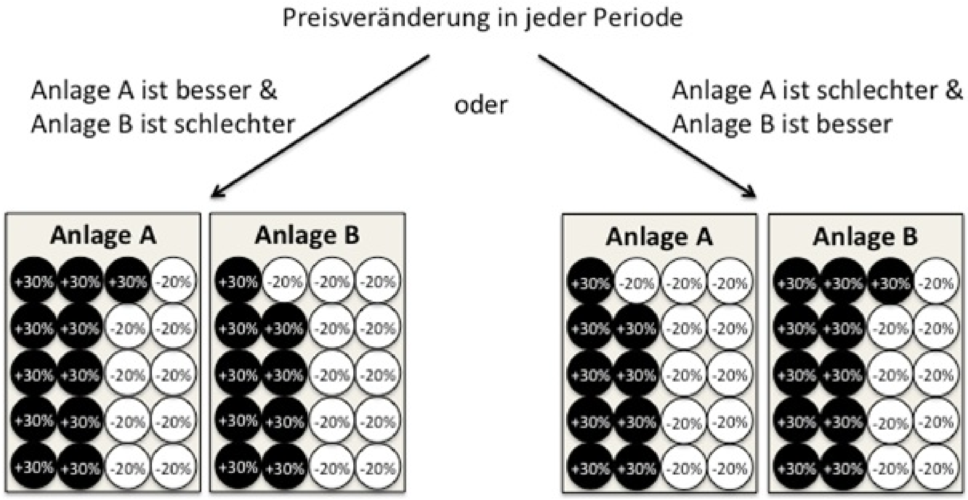
Somit wissen Sie, dass die bessere der beiden Anlagen eine höhere zu erwartende Preisentwicklung aufweist. Sie wissen jedoch nicht, welche der beiden Anlagen die bessere ist. Sie können aber im Laufe des Experiments anhand der beobachteten Preisverläufe Rückschlüsse darauf ziehen welche Anlage wahrscheinlich die bessere ist.

Beachten Sie, dass die Preisentwicklung jeder Anlage unabhängig ist, sowohl von der Preisentwicklung der jeweils anderen Anlage, als auch von Ihren Entscheidungen (sowie denen anderer Teilnehmer).

Beide Anlagen haben eine positive zu erwartende Rendite gegenüber dem unverzinsten Experimentalkonto, können aber im Einzelfall – je nach Realisierung des Zufallsgenerators – auch zu einer geringeren Auszahlung führen. Die Wahl der tatsächlich besseren Anlage führt zu einer 3,5 mal so großen, also **mehr als 250% größeren**, erwarteten Rendite als die Wahl der tatsächlich schlechteren Anlage. Daher sollten Sie sich Ihre Anlageentscheidungen sehr genau überlegen!

Im Folgenden wird die Preisentwicklung nochmal mit anderen Worten und mit Hilfe eines Diagramms erläutert:
Preisveränderungen werden dadurch bestimmt, dass in jeder Periode aus zwei Urnen (eine für Anlage A, eine für Anlage B) jeweils ein Ball zufällig gezogen wird. Sie beobachten also in jeder Periode entweder
für jede Anlage.

Sie wissen jedoch nicht, ob aus den beiden linken oder aus den beiden rechten Urnen gezogen wird. Dies wurde vorab für Ihre Teilnehmernummer zufällig bestimmt und bleibt über alle Perioden hinweg gleich.



Anhand der Preisverläufe erhalten Sie also Signale darüber, welche der beiden Anlagen die bessere ist, also ob die Bälle aus den beiden linken oder aus den beiden rechten Urnen gezogen werden. Diese Signale sind natürlich nicht präzise, sondern mit Unsicherheit behaftet. Sie können aber anhand der Preisverläufe Rückschlüsse darauf ziehen, welche Anlage **wahrscheinlich** die bessere ist und wie hoch diese Wahrscheinlichkeit ist.

Erste Anlageentscheidung

In Ihrer ersten Anlageentscheidung müssen Sie wählen, ob Sie Ihr Startkapital in Höhe von 20.000 EG entweder in Anlage A, in Anlage B, oder gar nicht investieren möchten. Außerdem werden Sie nach Ihrer Einschätzung der Wahrscheinlichkeit gefragt, welche Anlage die tatsächlich bessere ist. Sie sollten sich jede Ihrer Wahrscheinlichkeitseinschätzungen genau überlegen, denn Sie werden für deren

Richtigkeit mit bis zu 4 EUR zusätzlich entlohnt (wie genau, wird am Ende der Instruktionen erklärt). Erst ganz am Ende des Experiments werden sie erfahren, welche der beiden Anlagen tatsächlich die bessere war.

Wenn Sie sich für eine der beiden Anlagen entscheiden, wird Ihr gesamtes Startkapital unabhängig von den aktuellen Preisen vollständig in die gewählte Anlage investiert. Sie erhalten somit – abhängig von den Preisen – mehr oder weniger Anteile einer Anlage (da der Preis einer Anlage immer den Preis eines Anteils dieser Anlage wiederspiegelt). Anteilige Investitionen in eine oder mehrere Anlagen sind nicht möglich.

Entscheiden Sie sich für eine der beiden Anlagen, so entwickelt sich der Wert Ihrer Investition bis zur nächsten Anlageentscheidung mit dem Preis dieser Anlage mit. Dadurch steigt oder fällt Ihr Vermögen mit dem gleichen Prozentsatz wie der Preis der Anlage, in die Sie investiert haben. Der absolute Preis einer Anlage spielt also keine Rolle für Ihr Vermögen, sondern nur dessen prozentuale Veränderungen. Entscheiden Sie sich gegen eine Investition, so verbleibt Ihr Startkapital bis zur nächsten Anlageentscheidung auf Ihrem Experimentalkonto und wird zwischenzeitlich nicht verzinst.

Sie treffen Ihre erste Anlageentscheidung nicht für die Startperiode 0, sondern für Periode 2. Allerdings müssen Sie Ihre Entscheidung bereits vorab treffen, bevor Sie erfahren, wie sich die beiden Anlagen bis zur Periode 2 entwickelt haben.

Um Ihnen dennoch die Möglichkeit zu geben, Ihre Entscheidung von den Preisverläufen der Anlagen bis Periode 2 abhängig zu machen, werden 5 mögliche Preisverläufe der beiden Anlagen zufällig für Sie gezogen und Ihnen nacheinander separat zur Entscheidung vorgelegt.

Nachdem Sie für jeden dieser 5 Preisverläufe eine Anlageentscheidung getroffen sowie alle dazugehörigen Wahrscheinlichkeitseinschätzungen abgegeben haben, wird anschließend ein Preisverlauf zufällig gemäß den **tatsächlichen** Wahrscheinlichkeiten gezogen. Sie werden über den Ausgang dieser Ziehung informiert und Ihre entsprechende Entscheidung wird umgesetzt.

Sie wissen nicht, welche Ihrer 5 Entscheidungen umgesetzt wird. Treffen Sie also jede dieser Entscheidungen so sorgfältig, als würde sie mit Sicherheit umgesetzt werden. Mit anderen Worten, **betrachten Sie jede Ihrer Entscheidungen so, als ob sich der dargestellte Preisverlauf bereits realisiert hätte**.

Beispiel 1

In folgendem Screenshot sehen Sie einen der möglichen Preisverläufe bis Periode 2, der für Sie abgefragt und realisiert werden könnte.

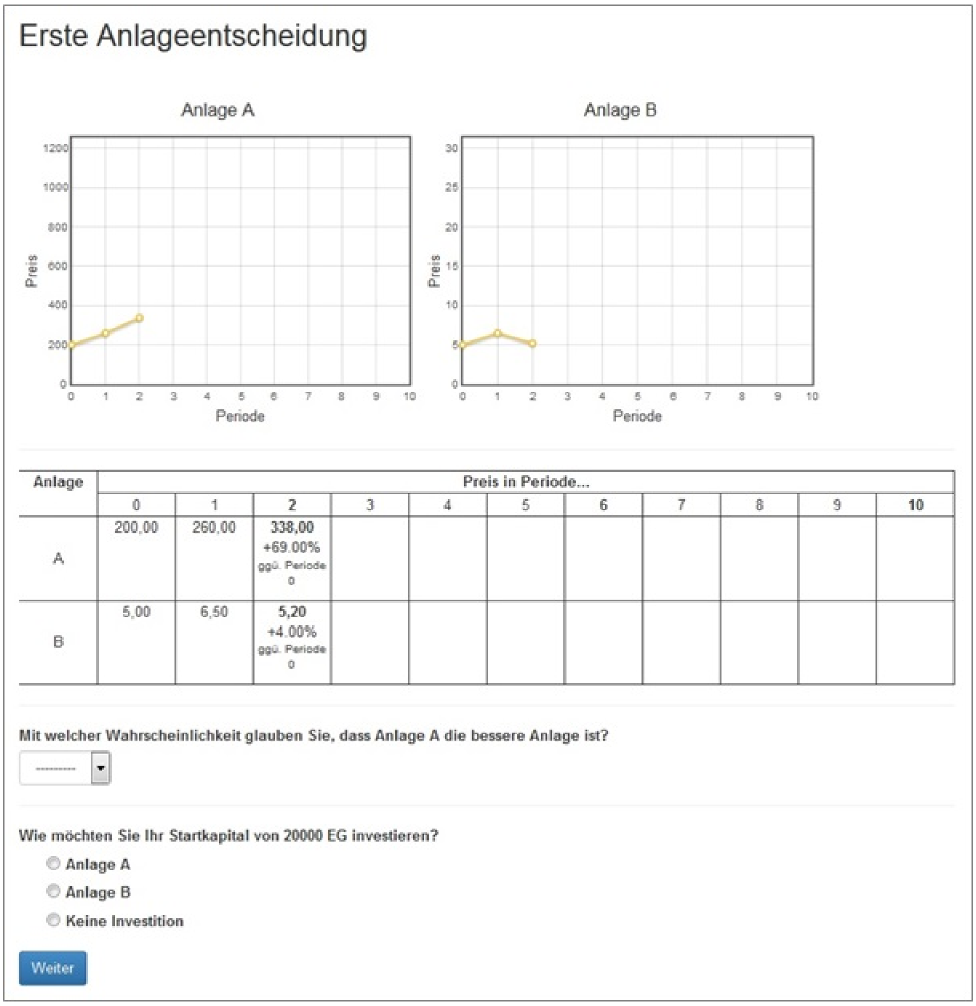
In diesem Beispiel hat Anlage A in Periode 2 einen Preis von 338,00 EG und Anlage B einen Preis von 5,20 EG. Die Diagramme zeigen den kompletten Preisverlauf bis Periode 2 je Anlage. Darüber hinaus sehen Sie in der Tabelle die prozentuale Preisveränderung jeder Anlage gegenüber Periode 0.

Unabhängig davon, für welche der Anlagen Sie sich entscheiden, wird immer Ihr gesamtes Startkapital vollständig in die gewählte Anlage investiert. Anteilige Investitionen sind nicht möglich.

Wie in Beispiel 1 zu sehen, müssen Sie für jeden Preisverlauf eine Wahrscheinlichkeitseinschätzung abgeben. Bei dieser Wahrscheinlichkeitsabfrage werden Sie immer nach der Wahrscheinlichkeit gefragt, mit der **Sie glauben**, dass **Anlage A** die bessere Anlage ist. Wenn Sie also glauben würden, dass Anlage B die bessere ist, z.B. mit 70% Wahrscheinlichkeit, müssten Sie aus dem Dropdown-Menü die Gegenwahrscheinlichkeit 30% auswählen (denn dann wäre Anlage A mit Wahrscheinlichkeit 100% - 70% = 30% die bessere Anlage).

Zweite Anlageentscheidung

In Ihrer zweiten (und letzten) Anlageentscheidung müssen Sie erneut wählen, ob Sie Ihr aktuelles Vermögen entweder in Anlage A, in Anlage B, oder gar nicht



investieren möchten. Außerdem müssen Sie wieder Ihre Einschätzung der Wahrscheinlichkeit abgeben, mit der Sie glauben, dass Anlage A die bessere Anlage ist. Auch hier sollten Sie sich jede Ihrer Wahrscheinlichkeitseinschätzungen genau überlegen, da Sie für deren Richtigkeit mit bis zu 4 EUR zusätzlich entlohnt werden (wie genau, wird am Ende der Instruktionen erklärt).

Ihre zweite Anlageentscheidung treffen Sie für Periode 6. Wiederum müssen Sie Ihre Entscheidung vorab treffen, bevor Sie erfahren, wie sich die beiden Anlagen zwischen Periode 2 und Periode 6 entwickelt haben.

Wie bei Ihrer ersten Anlageentscheidung besteht auch hier die Möglichkeit, Ihre Entscheidung von den Preisverläufen der Anlagen abhängig zu machen. Dazu werden wieder einige mögliche Preisverläufe der beiden Anlagen zufällig für Sie gezogen (diesmal 3 bis 13) und Ihnen nacheinander separat zur Entscheidung vorgelegt.

Auch hier wissen Sie nicht, welche Ihrer Entscheidungen umgesetzt wird. Treffen Sie also jede der Entscheidungen so sorgfältig, als würde sie mit Sicherheit umgesetzt werden. Mit anderen Worten, **betrachten Sie jede Ihrer Entscheidungen so, als ob sich der dargestellte Preisverlauf bereits realisiert hätte**.

Sobald Sie alle Anlageentscheidungen getroffen sowie alle dazugehörigen Wahrscheinlichkeitseinschätzungen abgegeben haben, wird anschließend einer der abgefragten Preisverläufe zufällig gemäß den **tatsächlichen** Wahrscheinlichkeiten gezogen, Sie werden über den Ausgang dieser Ziehung informiert und Ihre entsprechende Entscheidung wird umgesetzt.

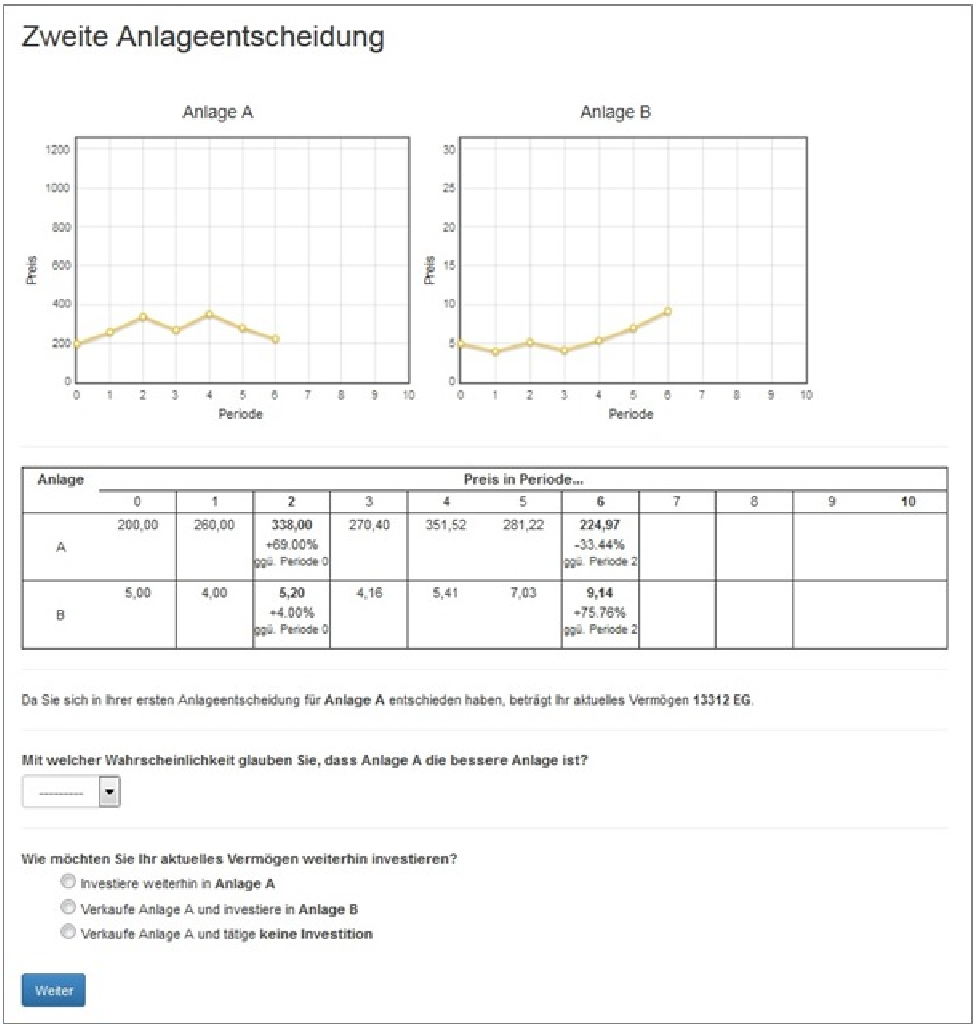
Der Wert Ihrer Investition entwickelt sich dann bis zur letzten Periode (Periode 10) mit dem Preis der gewählten Anlage mit. Erst in Periode 10 werden automatisch alle Anteile in Ihrem Besitz zu den in Periode 10 geltenden Preisen verkauft und der Erlös wird Ihrem Experimentalkonto gutgeschrieben.

Beachten Sie, dass Sie nach Periode 6 keine weiteren Anlageentscheidungen mehr treffen können, sich die Preise der Anlagen aber bis Periode 10 weiter verändern. Daher ist es wichtig zu überlegen, wie sich die Preise der beiden Anlagen für den jeweils gegebenen Preisverlauf weiter entwickeln können.

Beispiel 2

In folgendem Screenshot sehen Sie einen der möglichen Preisverläufe bis Periode 6, der für Sie abgefragt und realisiert werden könnte.

In diesem Beispiel hat sich der Preisverlauf von Beispiel 1 nach Periode 2 realisiert. Die Preise der beiden Anlagen in Periode 6 betragen 224,97 EG für Anlage A


Zweite Anlageentscheidung

Anlage	Preis in Periode...									
	0	1	2	3	4	5	6	7	8	9 10
A	200,00	260,00	**338,00** +69.00% ggü. Periode 0	270,40	351,52	281,22	**224,97** -33.44% ggü. Periode 2			
B	5,00	4,00	**5,20** +4.00% ggü. Periode 0	4,16	5,41	7,03	**9,14** +75.76% ggü. Periode 2			

Da Sie sich in Ihrer ersten Anlageentscheidung für **Anlage A** entschieden haben, beträgt Ihr aktuelles Vermögen **13312** EG.

Mit welcher Wahrscheinlichkeit glauben Sie, dass Anlage A die bessere Anlage ist?

Wie möchten Sie Ihr aktuelles Vermögen weiterhin investieren?

- Investiere weiterhin in **Anlage A**
- Verkaufe Anlage A und investiere in **Anlage B**
- Verkaufe Anlage A und tätige **keine Investition**

Weiter

und 9,14 EG für Anlage B. Die Diagramme zeigen den kompletten Preisverlauf bis Periode 6 je Anlage. Darüber hinaus sehen Sie in der Tabelle prozentuale Preisveränderungen.

Hätten Sie in Ihrer ersten Anlageentscheidung Anlage A gewählt, so würde sich Ihr aktuelles Vermögen in Periode 6 auf 13.312 EG belaufen (Startkapital –33,44%). In Beispiel 2 ist genau dieser Fall abgebildet. Hätten Sie dagegen Anlage B gewählt, betrüge Ihr aktuelles Vermögen 35.152 EG (Startkapital +75,76%). Ihr aktuelles Vermögen kann entweder in der bisherigen Anlage belassen, in die andere Anlage umgeschichtet, oder auf das zinslose Experimentalkonto eingezahlt werden. Sie haben also wieder die Wahl zwischen Anlage A, Anlage B und dem unverzinsten Experimentalkonto.

Wenn Sie sich in Ihrer ersten Anlageentscheidung für Anlage A entschieden hätten (wie in Beispiel 2 dargestellt) und nun bei Anlage A bleiben würden, würden Sie alle zuvor erworbenen Anteile von Anlage A behalten, d.h. Sie würden weder Anteile kaufen noch verkaufen. Wenn Sie dagegen nun Anlage B oder keine Investition wählen würden, würden Sie alle zuvor erworbenen Anteile von Anlage A zuerst verkaufen, bevor Ihre neue Investitionsentscheidung umgesetzt würde.

Entlohnung Ihrer Wahrscheinlichkeitsschätzungen

Eine Ihrer getroffenen Wahrscheinlichkeitseinschätzungen wird mit bis zu 4 EUR entlohnt. Ein Münzwurf wird entscheiden, ob es die Einschätzung des realisierten Preisverlaufs aus Ihrer ersten oder zweiten Anlageentscheidung ist.

Sie sollten sich jede Ihrer Wahrscheinlichkeitseinschätzungen **genau überlegen**, denn Sie werden für deren Richtigkeit entlohnt. Im Folgenden sehen Sie ein Beispiel, wie diese Entlohnung genau funktioniert: Nehmen Sie an, Sie würden bei dem auszahlungsrelevanten Preisverlauf glauben, dass Anlage B mit 60% Wahrscheinlichkeit die bessere ist (also dass dementsprechend Anlage A mit 40% Wahrscheinlichkeit die bessere ist). Wenn Sie nun 40% bei diesem Preisverlauf eingeben, wird in der folgenden Tabelle in den ersten 8 Zeilen Alternative L (Links) und in den letzten 12 Zeilen Alternative R (Rechts) automatisch ausgewählt. Eine der Zeilen wird dann zufällig gezogen (alle Zeilen sind gleich wahrscheinlich) und gemäß der ausgewählten Alternative umgesetzt (bzw. ausgespielt). Bei diesem Anreizsystem wird **garantiert**, dass Sie Ihre **Gewinnwahrscheinlichkeit** für die **4 EUR** Entlohnung **maximieren**, wenn Sie exakt Ihre **wahre Einschätzung** von 40% auswählen (Würden Sie z.B. stattdessen 20% eingeben, dann würde Alternative L in den ersten 4 Zeilen und Alternative R in den letzten 16 Zeilen ausgewählt. Sie würden aber besser gestellt, wenn Alternative L in den Zeilen 5 bis 8 ebenfalls ausgewählt würde, da es Ihnen eine höhere Gewinnwahrscheinlichkeit für die 4 EUR Entlohnung garantiert).

Zeile	**Alternative L (Links)**	**Alternative R (Rechts)**	**Ihre Wahl**
1	4 €falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 5%, 4 € mit Wahrscheinlichkeit 95%,	L
2	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 10%, 4 € mit Wahrscheinlichkeit 90%,	L
3	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 15%, 4 € mit Wahrscheinlichkeit 85%,	L
4	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 20%, 4 € mit Wahrscheinlichkeit 80%,	L
5	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 25%, 4 € mit Wahrscheinlichkeit 75%,	L
6	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 30%, 4 € mit Wahrscheinlichkeit 70%,	L
7	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 35%, 4 € mit Wahrscheinlichkeit 65%,	L
8	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 40%, 4 € mit Wahrscheinlichkeit 60%,	R
9	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 45%, 4 € mit Wahrscheinlichkeit 55%,	R
10	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 50%, 4 € mit Wahrscheinlichkeit 50%,	R
11	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 55%, 4 € mit Wahrscheinlichkeit 45%,	R
12	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 60%, 4 € mit Wahrscheinlichkeit 40%,	R
13	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 65%, 4 € mit Wahrscheinlichkeit 35%,	R
14	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 70%, 4 € mit Wahrscheinlichkeit 30%,	R
15	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 75%, 4 € mit Wahrscheinlichkeit 25%,	R
16	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 80%, 4 € mit Wahrscheinlichkeit 20%,	R
17	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 85%, 4 € mit Wahrscheinlichkeit 15%,	R
18	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 90%, 4 € mit Wahrscheinlichkeit 10%,	R
19	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 95%, 4 € mit Wahrscheinlichkeit 5%,	R
20	4 € falls Anlage A besser ist, 0 € falls Anlage A besser ist,	4 € mit Wahrscheinlichkeit 100%, 4 € mit Wahrscheinlichkeit 0%,	R

Testlauf

Um Sie mit dem Computer-Interface und dem Ablauf des Experiments noch besser vertraut zu machen, führen wir nun vor Beginn des eigentlichen Experiments einen Testlauf durch. Dieser Testlauf hat **keinen Einfluss** auf Ihre Auszahlung aus dem Experiment und soll lediglich dazu beitragen, mögliche Missverständnisse zu erkennen und zu klären.

Im Testlauf wird nicht das eigentliche Experiment durchgespielt, sondern lediglich eine vereinfachte und verkürzte Version davon. **Anders als im eigentlichen Experiment**, treffen Sie im Testlauf beide Anlageentscheidungen für lediglich 2 von den Experimentleitern ausgewählte (und nicht zufällig gezogene) Preisverläufe, von denen jeder mit 50% Wahrscheinlichkeit eintritt.

Beachten Sie, dass die Preisverläufe und Realisierungen des Testlaufs keinerlei Rückschlüsse auf das eigentliche Experiment erlauben!

Falls nach dem Testlauf noch Unklarheiten über den Ablauf des Experiments bestehen sollten, drücken Sie bitte die rote Taste (F11) auf Ihrer Tastatur und ein Experimentleiter wird zu Ihnen an den Platz kommen und Ihre Fragen beantworten.

Bitte wenden Sie sich nun dem Computer zu und geben Sie den Code 1234 in das Feld ein.

Kontrollfragen

Bevor als nächstes das eigentliche Experiment beginnt, möchten wir Sie bitten, noch kurz die folgenden Verständnisfragen zu beantworten um jegliche Missverständnisse auszuschließen. Ihre Antworten haben **keinen Einfluss** auf Ihre Auszahlung aus dem Experiment.

Sobald Sie die Fragen beantwortet haben, heben Sie bitte Ihre Hand und ein Experimentleiter wird zu Ihnen an den Platz kommen um Ihre Antworten zu kontrollieren. Das Experiment startet erst, wenn alle Teilnehmer die Kontrollfragen beantwortet haben. Bei Unklarheiten heben Sie bitte ebenfalls Ihre Hand.

1. Können Sie durch einen Kauf oder Verkauf die Preise beeinflussen?
   - ☐ Ja
   - ☐ Nein
2. Sind die Preise der beiden Anlagen voneinander abhängig?
   - ☐ Ja
   - ☐ Nein
3. Mit welcher Wahrscheinlichkeit steigt der Preis der besseren Anlage?
   ____ %
4. Mit welcher Wahrscheinlichkeit sinkt der Preis der schlechteren Anlage?
   ____ %
5. Um wieviel Prozent steigt der Preis einer Anlage in einer Periode, wenn er steigt?
   ____ %
6. Um wieviel Prozent sinkt der Preis einer Anlage in einer Periode, wenn er sinkt?
   ____ %
7. Wie hoch ist die zu erwartende Wertveränderung der besseren Anlage innerhalb einer Periode?
   - ☐ 0,45 * 0,30 - 0,55*0,20 = +2,5 %
   - ☐ 0,55*0,30 – 0,45*0,20 = + 7,5%
   - ☐ 0,45*0,20 – 0,55*0,30 = - 7,5%
   - ☐ 0,55*0,20 – 0,45*0,30 = - 2,5%
8. Wie hoch ist die zu erwartende Wertveränderung der schlechteren Anlage innerhalb einer Periode?

- ☐ 0,45*0,30 – 0,55*0,20 = + 2,5%
- ☐ 0,55*0,30 – 0,45*0,20 = + 7,5%
- ☐ 0,45*0,20 – 0,55*0,30 = - 7,5%
- ☐ 0,55*0,20 – 0,45*0,30 = - 2,5%

9. Werden Sie während des Experiments aufgefordert Anlageentscheidungen zu treffen, deren Realisierung von vornherein ausgeschlossen ist?

    - ☐ Ja
    - ☐ Nein

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