Portfolio Choice in the Presence of Nontradeable Income: An Experimental Analysis *

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ABSTRACT

This paper reports the results of experiments on portfolio choice in the presence of nontradeable income. The nontradeable income part could either be riskless or risky (background risk). In many cases, we observe behavior which is qualitatively consistent with the predictions of normative theory. However, correlations between financial and nontradeable wealth are neglected. The computation of aggregated risk profiles helps subjects to partly overcome the deviations from normative theory due to neglect of correlations. *(JEL G11)*

Keywords: portfolio choice, experimental economics, nontradeable income, background risk

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1. Introduction

Background risk should matter. Consider the following extreme example of two stock brokers. Broker A is paid according to market performance and broker B receives a flat salary. The benefits from their human capital differ substantially. The expected return of human capital for Broker A is risky and highly positively correlated with the local stock market returns. Let us assume that human capital is nontradeable, as for nearly all people. Assume furthermore that both brokers invest part of their financial wealth into the local stock market. From a diversification perspective, the different characteristics of their salaries, or in other words their differences in background risk, should clearly influence their asset allocation decision.

Nevertheless, background risk has been largely ignored in the literature on portfolio choice for a long time.[1] One of the reasons for this may be the lengthy preponderance of the concept of complete markets. In a complete market every wealth part - risky or not - can be traded. It is therefore possible to aggregate the value of all assets into one value: Total wealth. The total wealth can then be split into risky and riskless investments.

Recently, researchers have started addressing portfolio choice in the presence of nontradable income in more detail. Theorists analyze complex models where an investor has to decide about the asset allocation in the presence of nontradeable wealth parts (see e.g., Viceira, 2001, and Campbell and Viceira, 2002). An expected utility maximizer is usually assumed in these models. There is also a growing empirical literature considering portfolio choice in the presence of nontradeable income from a descriptive perspective (see e.g., Heaton and Lucas, 2000a, Vissing-Jørgensen, 2002, and Degeorge et al., 2004). The majority of these studies utilize data from large consumer surveys. We briefly review both parts of the literature in the next section.

This paper deals with the descriptive aspects of the decision problem. We aim to add to the existing literature with an experimental analysis. Field data studies face the problem that nontradeable wealth parts should be assessed at the individual level. Even if a sufficiently detailed data set is available, the core problem to exactly quantify the background risk and to estimate the correlation structure with tradeable assets remains. The issue therefore leads itself to an experimental investigation. In an experiment we can systematically vary the factors of interest (i.e. the amount of nontradeable wealth and the correlation structure) and analyze the responses of our subjects. Especially, this analysis leads to within-subject data. The results of experiments can therefore add to the growing empirical literature on portfolio choice in the presence of nontradeable income and enhance our understanding of this important issue. In

[1] An early exception is Mayers (1972); another example for incorporating background risk is Pratt and Zeckhauser (1987).

this sense, the results of an experiment with control of the amount of background risk and the concrete risk structure completes existing field data evidence.

Motivated by recent theoretical research, the decision-making problem of the investor considered in theoretical models is simplified to a buy-and-hold problem. Every subject had two investment possibilities, i.e. a risk-free investment opportunity and a risky asset, modelled as a discrete random variable with two possible outcomes. The wealth of our subjects consisted of two parts. The nontradeable wealth part could either be risk-free or risky (background risk). The other part was financial wealth while the decision variable was the proportion of financial wealth invested in the risky option. In many cases, we observed a behavior which is qualitatively consistent with the predictions of normative theory. However, correlations between financial and nontradeable wealth are neglected. In a second treatment, subjects had to invest in one investment opportunity. Explicit asset allocation decisions were not considered, but they could change the risk profile of the only available investment by varying the position of a slider. The trick is that changing the risk profile via the slider was equivalent to choosing the proportion of financial wealth in the first treatment in terms of final wealth. The program computed the "aggregated risk profile", i.e. the joint probability, the resulting total wealth, the absolute change in wealth, and the percentage change in wealth for every possible state of the world. The computation of aggregated risk profiles helps subjects to partly overcome the deviations from normative theory due to negligence of correlations.

Common caveats against portfolio choice experiments address the subject pool and the relatively low monetary incentives. We will discuss these general objections against an experimental investigation after the presentation of our study.

The paper is structured as follows. In Section 2 we review related literature on portfolio choice and briefly discuss the usefulness of an experimental approach. Section 3 describes our experimental design in detail and Section 4 reports the results. We discuss the two mentioned major caveats against the experimental investigation of portfolio choice problems in Section 5 and Section 6 draws conclusions.

2. Portfolio Choice Literature

Much of the work on portfolio choice is inspired by the classic papers of Samuelson (1969) and Merton (1969). They analyze dynamic multiperiod models with an investor optimizing his consumption stream. There are a risky asset with i.i.d. returns and a riskless investment opportunity. In complete market models Samuelson (1969) and Merton (1969) show that the optimal asset allocation does not depend on the length of the investment horizon. A multiperiod investor chooses the same allocation as a single period investor with the same risk preference.

However, relaxing the rigid assumption of the above models results in very different conclusions (see Campbell and Viceira, 2002, for a comprehensive survey). We review the important insights concerning portfolio choice in the presence of nontradeable labor income which motivated our study in the next subsection.

2.1. The Adjustment Effect, the Horizon Effect, and the Correlation Effect

The Adjustment Effect

A natural starting point when thinking about portfolio choice in the presence of nontradeable labor income is a decision problem where the investor is certain to receive nontradeable future payments. Bodie et al. (1992) examine this situation and their main argument may best be explained by giving an example.

Consider an investor A with a total wealth of $800,000. Let us first assume that this investor lives in a world with a complete capital market, i.e. every part of her wealth is tradeable. Let us further assume that she prefers to invest fifty percent of her wealth ($400,000) in the risky asset. We call this the total or desired risk position (trp). Next, consider an investor B with identical risk preferences and identical total wealth. The only difference between A and B is that part of B's wealth is nontradeable and riskless. Bodie et al. (1992) show that this part of his wealth is then equivalent to holding the riskless asset. In other words, the nontradeable wealth part is economically an implicit investment in the riskless asset in the amount of the present value of the future payments. How do A and B differ in their investment policy? Both desire the same total risk position. If investor B is implicitly invested in the riskless asset, he must allocate his remaining financial wealth more heavily to the risky asset to ensure that he achieves the desired risk position. Assume that B's total wealth consists of $400,000 of a tradeable and a nontradeable part, respectively. Then he has to invest his entire financial wealth into the risky asset (see table 1 below).

Table 1: *Possible Preference and Wealth Position of a Hypothetical Investor.* The table shows the possible preference and wealth position of a hypothetical investor B.

Investor B	Total Wealth	Nontradeable	Financial Wealth
Total	$800,000	$400,000	$400,000
Riskless Asset	$400,000	$400,000	$0
Risky Asset	$400,000	$0	$400,000

The only difference between investors A and B is that B cannot trade part of his wealth. As a result A invests fifty percent of her *financial* wealth in the risky asset, while B invests one hundred percent of his *financial* wealth in the risky asset. Higher nontradeable riskless wealth parts lead to a higher proportion of financial wealth invested in the risky asset provided that total wealth is left constant. We will call this the "adjustment effect" throughout the paper.

The Horizon Effect

Viceira (2001) studies a dynamic model of optimal consumption and portfolio choice in the presence of background risk. He shows that in the important case of uncorrelated background risk, investors with constant relative risk aversion (CRRA)[2] preferences invest a larger fraction of their savings in the risky asset compared to a decision situation without background risk. A "one percent negative shock in their financial wealth does not fully translate into a one percent decrease in their consumption growth" (Viceira, 2001, page 445), whereas it does so for investors without an uncorrelated second source of income. We will call this result the "horizon effect" because it has some relevance for the ongoing discussion if investors with long horizons should allocate more heavily to risky assets. Younger investors have to carry more (uncorrelated) background risk and own lower financial wealth than older people. The bulk of their background risk is in most cases the uncertain benefits of human capital. The result therefore provides a rational argument for younger people with a longer investment horizon to hold a riskier portfolio.

In some sense, the horizon effect is also a risk effect. It addresses a situation where the investor has to carry relatively more background risk. Note, that this increase in risk has to be distinguished from a mean preserving spread in the sense of Rothschild and Stiglitz (1970). Gollier and Pratt (1996) and Gollier (2001) discuss such issues in the presence of background risk theoretically.

The Correlation Effect

Viceira (2001) further examines the influence of the correlation between the background risk and the risky asset on the asset allocation decision. All things being equal, a negative correlation leads to a higher exposure to the risky asset. In such a situation holding more of the risky asset serves as a hedge against negative background income realizations. Above average realizations of the risky asset tendentiously arise with below average realizations of the background risk. Holding a given amount of the risky asset results in a relatively less volatile time path of consumption opportunities in the case of a negative correlation. Therefore the investor would like to act in a less risk averse fashion. The opposite is true for a positive correlation. Under this condition the extra demand is negative, i.e. the investor reduces his

[2]The solution of the problem is ambiguous for some other utility functions and depends on the exact specification of the decision problem. See Gollier (2001) for a brief discussion and two numeric examples on background risk and time horizon in a dynamic context without a consumption/saving decision.

holdings of the risky asset. These extra demands are usually called hedging demands in the literature. As a result, the optimal proportion of the risky asset is negatively related to the correlation between the background risk and the risky asset. We will call this the "correlation effect".

The experiments reported in this paper aim to address the three described effects descriptively. Before moving to the details of our experimental design in Section 3, we briefly mention other closely related literature.

2.2. Other Related Literature

Portfolio choice in the presence of nontradeable income has been discussed from a descriptive point of view on the basis of large consumer surveys.[3] The most prominent example is the Survey of Consumer Finance (SCF) in the USA. Among other things, people were asked about their asset allocation decisions. Researchers then construct a variable which approximately measures the exposure to a nontradeable risk and they analyze the relationship between the variable and the asset allocation decisions. Examples for variables of interest are the variability of income (Guiso et al., 1996), entrepreneurial risk (Heaton and Lucas, 2000) and housing (Faig and Shum, 2002). Heaton and Lucas (2000b) survey this literature. The results are in many cases in line with theoretical predictions, for example Guiso et al. (1996) find that households reporting a large income variability hold less risky portfolios. However, finding the "correct" proxy to quantify background risk is one core problem of this literature. Some studies also try to estimate the impact of a non-zero correlation between the asset and the exogenous risk. The results are mixed. Heaton and Lucas (2000a) find evidence for a qualitatively rational response and justify the result with classic economic theory. In contrast, Vissing-Jørgensen (2002) does not detect a similar correlation effect and Massa and Simonov (2002) find a reverse correlation effect, i.e. they document the opposite behavior compared to the normative prediction. Further research is needed in this field to reconcile the conflicting evidence.

Experimental investigation of portfolio choice behavior has been started at the latest with Gordon et al. (1972). Kroll et al. (1988), Kroll and Levy (1992) and Levy (1997) report the results from experimental tests of the Capital Asset Pricing Model (CAPM). Among other things, they investigate responses to different correlations between assets. We are not aware of any other experimental study dealing with strategic asset allocation decisions in the presence of two wealth sources and the consideration of background payments is therefore the unique contribution of this paper.

[3] A recent exception is Degeorge et al. (2004). They use a database of current and former employees of France Telecom.

3. Experimental Design

Obviously we have to simplify the dynamic decision problem faced by an investor in theoretical studies for our experiments. First, we consider a finite time horizon and abstract from retirement and bequest. Second, we are not interested in saving/consumption decisions. In most cases, the portfolio decision in the presence of nontradeable income should not be independent from the saving decision. This issue is beyond the scope of this paper. Third, we consider buy-and-hold decisions. We therefore factor out any effects driven by the dynamic nature of the investment problem. Campbell and Viceira (2002) provide a formal treatment of the buy-and-hold case which shows that the qualitative intuitions concerning the effects considered here are captured by our design. We now turn to the details of our design. The experiments consist mainly of two treatments. We start with treatment A.

Every subject had two investment opportunities. The risky asset was a discrete random variable with two possible outcomes. The amount invested in the risky asset, which we also call lottery, could increase in value by 20% or decrease in value by 10%. The amount assigned to the riskless asset increased in value by 3%. No explicit investment horizon was specified.

Wealth consisted of two parts. We called one part "exogenous income". This part could be risky or riskless. In any case, it was nontradeable. Subjects could not trade or insure against this wealth part. The other part was financial wealth. The decision variable was the proportion α of financial wealth invested in the lottery. Borrowing and short sales were allowed as long as $-100\% \leq \alpha \leq 400\%$ holds. The restrictions for α were chosen after some simulations which showed that an investor with a plausible degree of risk aversion frequently chooses highly leveraged positions. Short sales were also possible under plausible assumptions. In every decision situation[4] total wealth, the sum of financial wealth and exogenous income, was 1,000,000 DM (abbreviation: 1,000 TDM). Note that a risk-seeking or risk-neutral investor will chose $\alpha = 400\%$, because the expected value of the lottery is 5% and the riskless investment opportunity guarantees only 3%.

We next describe the situations with a riskless exogenous income relating to the adjustment effect. Every possible outcome of the lottery could occur with a probability of 1/2. Subjects were confronted with this situation for exogenous incomes of 0 TDM, 400 TDM, and 800 TDM. Exogenous income increased in value by 3% over the investment period. Financial wealth was then 1,000 TDM, 600 TDM, and 200 TDM, respectively. Because of the segregated description of the decision problem, we call the situations seg0:0:0, seg1:400:0, and seg2:800:0. The number

[4]We use the terms decision situation and decisions task synonymously throughout the paper to mean one specific decision problem within one treatment.

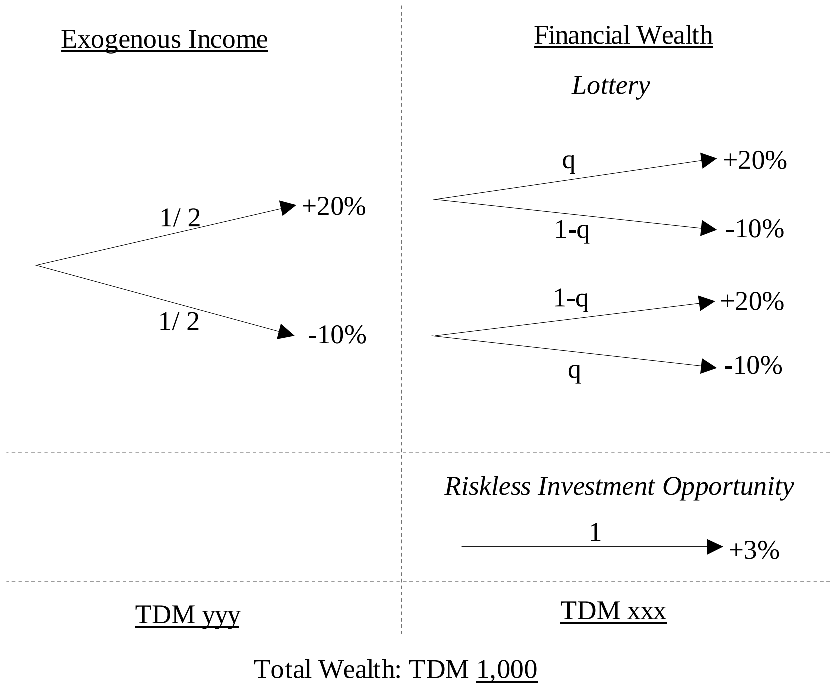
after the first colon gives the amount of exogenous income and the zero after the second colon indicates that the background payments are riskless. We define further $\alpha_{segi:y:q}$ as the response of one specific subject and $\hat{\alpha}_{segi:y:q}$ as the median response of all subjects to decision situation segi:y:q.

The riskless exogenous income is economically an implicit investment in the riskless asset. The higher the exogenous income, the higher α must be to reach the desired risk position (Bodie et al., 1992). This brings us to our first hypothesis.

Hypothesis 1: $\hat{\alpha}_{seg0:0:0} < \hat{\alpha}_{seg1:400:0} < \hat{\alpha}_{seg2:800:0}$

We now turn to the case of a risky decision situation. Figure 1 illustrates the problem.

Figure 1: *Risky Exogenous Income.* The diagram demonstrates the decision problem in the case of a risky exogenous income.



The nontradeable wealth part could either increase in value by 20% or decrease in value by 10%. Every possibility could occur with a probability of 1/2. The amount assigned to the lottery could again increase in value by 20% or decrease in value by 10%. One important aspect of our design is the conditional probability q. q denominates the probability of a gain (loss) in the lottery given that a gain (loss) in the exogenous income has occurred. 1-q equals the probability of a gain in the lottery given that a loss in the exogenous income has occurred.

Let us first consider the case of q being equal to 1/2. This is the case of two independent risks. The realization of the exogenous income does not influence the probability distribution of the lottery.

The independent case is of relevance for the horizon effect. If we increase the amount of background risk, subjects should invest a higher proportion of financial wealth into the lottery. A ten percent drop in financial wealth does on average not fully translate into a ten percent drop in final wealth. This is the basic intuition transferred from Viceira (2001) to our buy-and-hold problem. We again use the amounts of 400 TDM and 800 TDM for the exogenous income. The two new decision situations are denominated as seg4:400:1/2 and seg7:800:1/2.[5] The fraction after the second colon gives the value of q and indicates in this case the zero correlation. The horizon effect predicts the behavior stated in hypothesis 2.

$$\textit{Hypothesis 2: } \hat{\alpha}_{seg0:0:0} < \hat{\alpha}_{seg4:400:1/2} < \hat{\alpha}_{seg7:800:1/2}$$

We now turn to the correlation effect. Consider the independent case (q=1/2). A change of the conditional probability q introduces a correlation between the two risk sources.[6]

For $q < 1/2$ it is more likely that a gain in the background risk occurs with a loss in the lottery and that a loss in the background risk occurs with a gain in the lottery. This is the case of a negative correlation. Because of the negative correlation a larger amount invested in the lottery serves as a hedge against undesirable realizations in the background risk. A positive hedging demand results. The opposite is true for a positive correlation ($q > 1/2$). The two realizations of the random variables tend to move together. This induces a negative hedging demand. We use $q = 1/4$, $q = 1/2$, and $q = 3/4$ for a background risk of 400 TDM (seg3:400:1/4, seg4:400:1/2, seg5:400:3/4) and for a background risk of 800 TDM (seg6:800:1/4, seg7:800:1/2, seg8:800:3/4).

Note that the *unconditional* probability of a gain in the lottery is the same for every *conditional* probability q and that the two sources of wealth are additively linked. This excludes a wealth effect similar to Samuelson (1991)'s study of portfolio choice in the presence of non i.i.d. returns.

The basic intuition, again transferred from Viceira (2001), is that a negative correlation reduces ceteris paribus the variance of final wealth and leads therefore to a higher acceptance of lottery risk. The reverse result should hold for a positive correlation.

Our design allows us to test the correlation effect for two different amounts of background

[5]The reason for the not consecutively numbered decision situations will be obvious after the description of the correlation effect.

[6]Samuelson (1991) also uses conditional probabilities to model correlations in a portfolio choice context.

risk. The theoretical literature predicts $\hat{\alpha}_{seg3:400:1/4} > \hat{\alpha}_{seg4:400:1/2} > \hat{\alpha}_{seg5:400:3/4}$ and $\hat{\alpha}_{seg6:800:1/4} > \hat{\alpha}_{seg7:800:1/2} > \hat{\alpha}_{seg8:800:3/4}$ under common conditions on the utility function. However, experimental studies have shown that correlations are neglected under various circumstances (Kroll et al., 1988, Kroll and Levy, 1992, Lipe, 1998, Siebenmorgen and Weber, 2003). Although we use a different communication of correlations, we have no a priori reason to assume that different correlations should lead to different decisions in contrast to the existing results. We therefore formulate hypothesis 3.

Hypothesis 3: no difference between $\hat{\alpha}_{seg3:400:1/4}/\hat{\alpha}_{seg4:400:1/2}/\hat{\alpha}_{seg5:400:3/4}$
and $\hat{\alpha}_{seg6:800:1/4}/\hat{\alpha}_{seg7:800:1/2}/\hat{\alpha}_{seg8:800:3/4}$.

We had a second treatment to investigate the behavior of our subjects further. Treatment B put the subjects in nine decision situations which are equivalent in terms of final wealth changes to the nine decision problems in treatment A. The framing was different.

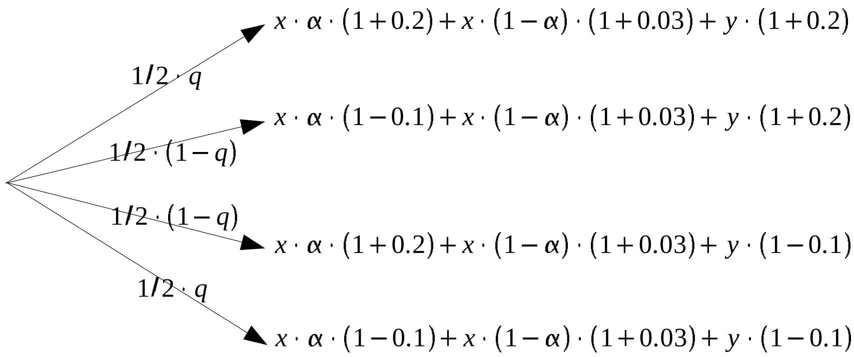
Two major considerations motivate the second treatment. First, we principally aim to replicate the effects using a different framing which would show the robustness of the results. The second reason deals with the correlation effect. As stated above, experimental studies document neglect of correlations. One explanation may be that people are not able to correctly take into account the correlation structure due to cognitive limitations. The main idea of treatment B is therefore to compute "aggregated risk profiles". The program used for conducting the experiments explicitly computed the joint probability and the total wealth position for every state of the world and therefore clearly showed the impact of the correlation. Our hypothesis is that people will correctly take the correlation structure into account under such circumstances. The second treatment may at best be explained by giving an example.

Consider the case of background risk. Now assume an arbitrary $\alpha \in [-100; 400]$[7]. Then there are four possible states of the world. The probability and the final wealth position for every state of the world can be easily computed and are shown in figure 2. Subjects in treatment B were told that they have to invest 1,000 TDM in one lottery and they could change the risk profile by varying the position of a slider. The trick was that one position of the slider equals one possible α in treatment A. The computer program computed the final wealth positions for this α. Subjects varied the slider, the program updated the final wealth positions and so on. This procedure continued until subjects found their preferred risk profile. As a result, we obtained a slider position which matches exactly to one α in treatment A. Put differently, treatment A and treatment B are equivalent in terms of final wealth. We denominate the nine decision

[7]When we talk about α-allocations in the rest of the chapter, we will drop the "%"-sign for the sake of convenience. For example, we write $\alpha_{seg0:0:0} = 80$ instead of $\alpha_{seg0:0:0} = 80\%$.

situations in treatment B as agg0:0:0 to agg8:800:3/4. A fully rational response is therefore $\alpha_{segj:y:q} = \alpha_{aggj:y:q}\ \forall j$. Screen shots for treatment B can be found in Appendix A.2.[8]

Figure 2: *General Design - Treatment B.* The diagram illustrates the general experimental design for treatment B. y stands for the amount of background risk and x for the amount of financial wealth.



We believe that people react qualitatively according to the theory by the use of aggregated risk profiles and thus formulate hypothesis 3a.

$$\textit{Hypothesis 3a: } \hat{\alpha}_{agg5:400:3/4} < \hat{\alpha}_{agg4:400:1/2} < \hat{\alpha}_{agg3:400:1/4} \textit{ and}$$
$$\hat{\alpha}_{agg8:800:3/4} < \hat{\alpha}_{agg7:800:1/2} < \hat{\alpha}_{agg6:800:1/4}$$

For the other two effects of interest, we have no reason to believe that the aggregated description of the decision problem would change the results.

$$\textit{Hypothesis 1a: } \hat{\alpha}_{agg0:0:0} < \hat{\alpha}_{agg1:400:0} < \hat{\alpha}_{agg2:800:0}$$
$$\textit{Hypothesis 2a: } \hat{\alpha}_{agg0:0:0} < \hat{\alpha}_{agg4:400:1/2} < \hat{\alpha}_{agg7:800:1/2}$$

Table 2 provides a summary of all decision situations of the described experiment.

One important empirical question, which is not addressed in this paper, is the reaction to an increase in the exogenous and endogenous risk. We conducted a second experiment to investigate this important question, where we define an increase in risk as a substitution of the two possible outcomes 20% and -10% by 40% and -30% for the exogenous or the endogenous income part. A detailed analysis of the results of experiment 2 can be found in Klos and Weber (2004). However, the parameters used in experiment 2 are helpful to gather further evidence concerning the correlation effect. This experiment uses exactly the same experimental setup including the two treatments A and B as described above. Only the amount of background risk and the risk structure of the the two sources of income are varied, and the risk free rate is increased to 5%. Table 3 describes the decision situations in the second experiment.

[8] This slider technique was first used by Langer and Fox (2004) in a different context.

Table 2: *Every Considered Decision Situation in Experiment 1.* This table describes every considered decision situation in experiment 1. Exogenous income could be riskless or risky, the amount of background risk varies between 0 TDM and 800 TDM and the possible values for q are 1/4, 1/2, and 3/4.

decision situation		exogenous income	in the amount of	q
seg0:0:0	agg0:0:0	-	0 TDM	-
seg1:400:0	agg1:400:0	riskless	400 TDM	-
seg2:800:0	agg2:800:0	riskless	800 TDM	-
seg3:400:1/4	agg3:400:1/4	risky	400 TDM	1/4
seg4:400:1/2	agg4:400:1/2	risky	400 TDM	1/2
seg5:400:3/4	agg5:400:3/4	risky	400 TDM	3/4
seg6:800:1/4	agg6:800:1/4	risky	800 TDM	1/4
seg7:800:1/2	agg7:800:1/2	risky	800 TDM	1/2
seg8:800:3/4	agg8:800:3/4	risky	800 TDM	3/4

Important for the issues discussed in this paper is that we can investigate three decision situations where all parameters are exactly the same with the exception of the conditional parameter q. These decision situations are seg10:500:1/4 to seg12:500:3/4 (agg10:500:1/4 to agg12:500:3/4), seg13:500:1/4:exo to seg15:500:3/4:exo (agg13:500:1/4:exo to agg15:500:3/4:exo), and seg16:500:1/4:end to seg18:500:3/4:end (agg16:500:1/4:end to agg18:500:3/4:end). We can therefore test our hypotheses about the correlation effect out of sample for experiment 2 with a different subject pool.

We now formulate our additional hypotheses for the second experiment. Again, we expect that correlations are largely neglected in treatment A and that a qualitatively rational response is observable in treatment B.

Hypothesis $\widetilde{3}$: no difference between $\hat{\alpha}_{seg10:500:1/4}/\hat{\alpha}_{seg11:500:1/2}/\hat{\alpha}_{seg12:500:3/4}$ *and*
$\hat{\alpha}_{seg13:500:1/4:exo}/\hat{\alpha}_{seg14:500:1/2:exo}/\hat{\alpha}_{seg15:500:3/4:exo}$ *and*
$\hat{\alpha}_{seg16:500:1/4:end}/\hat{\alpha}_{seg17:500:1/2:end}/\hat{\alpha}_{seg18:500:3/4:end}$

Hypothesis $\widetilde{3a}$: $\hat{\alpha}_{agg12:500:3/4} < \hat{\alpha}_{agg11:500:1/2} < \hat{\alpha}_{agg10:500:1/4}$ *and*
$\hat{\alpha}_{agg15:500:3/4:exo} < \hat{\alpha}_{agg14:500:1/2:exo} < \hat{\alpha}_{agg13:500:1/4:exo}$ *and*
$\hat{\alpha}_{agg18:500:3/4:end} < \hat{\alpha}_{agg17:500:1/2:end} < \hat{\alpha}_{agg16:500:1/4:end}$

We used the following randomization procedure to prevent order effects. In a first step, we drew a random sequence for the occurrence of the possible treatments, so that some subjects had to think about treatment B before the decision problems in treatment A were presented, while for other subjects the ordering was the other way around.[9] Next, we randomized the

[9]Both experiments consisted not only of the two described treatments. We had one additional task in experi-

Table 3: *Every Considered Decision Situation in Experiment 2.* This table describes every considered decision situation in experiment 2. Exogenous income could be riskless or risky, the amount of background risk is €500,000 and the possible values for q are 1/4, 1/2, and 3/4. In some decisions situations we consider an increase in risk. An increase in risk is defined as a substitution of the two possible outcomes 20% and -10% by 40% and -30%. We used € instead of DM in the second experiment as a currency unit.

decision situation		increase in risk	exogenous income	q
seg9:500:0	agg9:500:0	riskless exogenous income in the amount of €500,000 riskfree rate on exogenous income: 5%		
seg10:500:1/4	agg10:500:1/4	no	€500,000	1/4
seg11:500:1/2	agg11:500:1/2	no	€500,000	1/2
seg12:500:3/4	agg12:500:3/4	no	€500,000	3/4
seg13:500:1/4:exo	agg13:500:1/4:exo	yes (exogenous)	€500,000	1/4
seg14:500:1/2:exo	agg14:500:1/2:exo	yes (exogenous)	€500,000	1/2
seg15:500:3/4:exo	agg15:500:3/4:exo	yes (exogenous)	€500,000	3/4
seg16:500:1/4:end	agg16:500:1/4:end	yes (endogenous)	€500,000	1/4
seg17:500:1/2:end	agg17:500:1/2:end	yes (endogenous)	€500,000	1/2
seg18:500:3/4:end	agg18:500:3/4:end	yes (endogenous)	€500,000	3/4

sequence of the different decision tasks within every treatment, so that every decision situation could appear at any position with equal probability. This randomization procedure was applied to every subject at the start of the computerized experiment to exclude a systematic bias due to a special ordering of the decision situations. We also explicitly verified if the ordering of the treatments influenced behavior. There is no systematic difference between subjects which started with treatment A compared to participants which started with treatment B.

4. Experimental Results

We conducted experiment 1 at the University of Mannheim in December 2001. Participants were 38 voluntary graduate students from the course "Aktienmarkt & Börse" (stock market and stock exchange) and it took approximately 40 minutes to complete the experiment. They were well-educated in finance and they were familiar with the concepts of short sales, conditional probabilities, and correlations.

Every subject was paid for participation. After the experiment we randomly selected one decision and played out the lottery based on the asset allocation the subject had chosen in this decision situation. Payments ranged from 9 DM to 21 DM with the average payment about 14.50 DM ($1 was about 2.20 DM at the time of the experiment).

ment 1 (elicitation of certainty equivalents) and two additional tasks in experiment 2 (binary lottery choices and one additional asset allocation task). The purpose of these additions is not directly related to the issue of this paper.

The median age was 24, and 7 out of 38 subjects were female. All subjects were asked to rate the factor of understandability of the instructions on a scale from 1 to 9. The median response is 7.5, so we are certain that the subjects understood the instructions and the decision situations.

The second experiment was also conducted at the University of Mannheim. Subjects were again recruited from the course "Aktienmarkt & Börse" and 51 agreed voluntary to participate. The experiment took place in January 2003 and payments ranged from €5 to €15 with a mean of about €10. The arrangement of payments to the subjects was analogous to the first experiment. 11 of 51 subjects were female and the median ranking for the factor of understandability was 8.

As was described in Section 3, we have two treatments each with nine decision situations in experiment 1. Every decision situation in treatment A has its equivalent complement in treatment B. As one might expect, allocations in both treatments differ. Therefore, we mainly report on separate tests and analyze differences between the two treatments in Section 4.4.

To test the hypotheses 1 to 4, we perform a nonparametric test for ordered hypothesis as introduced by Page (1963). This statistical method tests the null hypothesis

$$H_0 : \hat{\alpha}_{decision\_situation\_x} = \hat{\alpha}_{decision\_situation\_y} = \hat{\alpha}_{decision\_situation\_z}$$

against the ordered alternative

$$H_1 : \hat{\alpha}_{decision\_situation\_x} < \hat{\alpha}_{decision\_situation\_y} < \hat{\alpha}_{decision\_situation\_z},$$

with decision_situation_x, decision_situation_y, and decision_situation_z denominating a specific decision situation in the experiment (e.g., seg0:0:0, seg1:400:0, and seg2:800:0 to test hypothesis 1). The Page test is therefore especially suitable for our experimental design. We report the value of the test statistic L and compute corresponding approximate p-values based on the χ^2_L-statistic given in Page (1963).[10] For pairwise comparisons, we use the Wilcoxon matched pairs signed rank test. The reported tests use within-subject data. However, for the sake of completeness we report the arithmetic means, medians, and standard deviations for every decision situation in Appendix B.

[10] Page (1963) shows that this approximation works very well, especially in cases like ours. See Wellek (1989) for computing exact p-values.

4.1. The Adjustment Effect

We start our analysis with treatment A. A Page test strongly rejects the null hypothesis in favor of hypothesis 1 (L = 505; p = 0.0000***).[11] A look at all possible Wilcoxon tests further supports our hypothesis (see table 4).

Every pairwise comparison is highly significant in the predicted direction. So, we conclude that hypothesis 1 is strongly confirmed in treatment A.

For treatment B the null hypothesis of a Page-test can also be rejected at a high significance level (L = 487; p = 0.0002***). But the Wilcoxon tests given in table 4 show that this effect is mainly driven by the responses to decision situation agg2:800:0. There is virtually no difference between the responses under the condition agg0:0:0 and agg1:400:0.

Our design made it possible for every subject to choose approximately the same desired risk position under conditions seg0:0:0 to seg2:800:0 and agg0:0:0 to agg2:800:0. This has the advantage that on the basis of one response we can calculate the two other optimal allocations in this treatment for all decision tasks under consideration. Note that the calculation can be done without knowledge of a specific preference calculus. Whatever it is that leads to a specific desired risk position in decision situation seg0:0:0, the same mechanisms should be at work under decision situation seg1:400:0 as long as the final wealth positions are the relevant parameters for the decision maker.[12] We are then able to calculate the necessary $\alpha_{seg1:400:0}$ which leads to the same desired risk position as the chosen $\alpha_{seg0:0:0}$. This is a special feature of the adjustment effect, which does not apply to any other effect in the paper.

How many subjects - if any - chose consistently in the described sense? This question is not as easy to answer as it might seem at first glance. We must know which allocation the subject truly prefers. If the response to seg0:0:0 is the true preference, we will be able to compute the correct response to seg1:400:0 and seg2:800:0 directly. But why shouldn't be the response to seg1:400:0 be the true preference? Working within a deterministic framework obviously doesn't allow a rigid analysis of this issue.

[11] We use the symbols * for significant at the 5%-level, ** for significant at the 1%-level, and *** for significant at the 0.1%-level throughout the paper.

[12] Mental accounting (Thaler, 1999) is one descriptive concept which could prevent subjects from the final wealth perspective. However, mental accounting would predict $\hat{\alpha}_{seg0:0:0} = \hat{\alpha}_{seg1:400:0} = \hat{\alpha}_{seg2:800:0}$, which is clearly not supported in our data for treatment A. In treatment B, the aggregate description of the decision problem rules out any kind of influence of mental accounting.

Table 4: *Wilcoxon-Tests for Both Experiments.* Shown are the z-value of a within Wilcoxon test for differences in the median and the corresponding p-values in parenthesizes. All reported Wilcoxon tests are two-sided tests. A negative test statistic, e.g. between seg0:0:0 and seg1:400:0, indicates that $\hat{\alpha}_{seg0:0:0} < \hat{\alpha}_{seg1:400:0}$. A positive test statistic indicates the opposite. For each effect and treatment or (in the case of the correlation effect) for each amount of background risk, the upper left number belongs to a test between the left and the middle decision situation and the upper right number to a test between the middle and the right decision situation. The number in the middle gives the test statistic for a test between the left and the right decision situation (e.g., the test statistic for the Wilcoxon test $H_0 : \hat{\alpha}_{seg0:0:0} = \hat{\alpha}_{seg2:800:0}$ is -5.030, while the test statistic for $H_0 : \hat{\alpha}_{agg1:400:0} = \hat{\alpha}_{agg2:800:0}$ is -3.894).

Adjustment Effect						
	Treatment A – Experiment 1			*Treatment B – Experiment 1*		
	$\alpha_{seg0:0:0}$	$\alpha_{seg1:400:0}$	$\alpha_{seg2:800:0}$	$\alpha_{agg0:0:0}$	$\alpha_{agg1:400:0}$	$\alpha_{agg2:800:0}$
Wilcoxon z	-3.283		-4.154	-0.182		-3.894
(p-value)	(0.0010)***		(0.0000)***	(0.8559)		(0.0001)***
		-5.030			-3.937	
		(0.0000)***			(0.0001)***	
Horizon Effect						
	Treatment A – Experiment 1			*Treatment B – Experiment 1*		
	$\alpha_{seg0:0:0}$	$\alpha_{seg4:400:1/2}$	$\alpha_{seg7:800:1/2}$	$\alpha_{agg0:0:0}$	$\alpha_{agg4:400:1/2}$	$\alpha_{agg7:800:1/2}$
Wilcoxon z	0.320		-2.176	0.392		-3.693
(p-value)	(0.7487)		(0.0296)*	(0.6949)		(0.0002)***
		-1.568			-3.053	
		(0.1169)			(0.0023)**	
Correlation Effect						
	Treatment A – Experiment 1			*Treatment B – Experiment 1*		
	$\alpha_{seg3:400:1/4}$	$\alpha_{seg4:400:1/2}$	$\alpha_{seg5:400:3/4}$	$\alpha_{agg3:400:1/4}$	$\alpha_{agg4:400:1/2}$	$\alpha_{agg5:400:3/4}$
Wilcoxon z	-0.386		2.887	2.009		1.249
(p-value)	(0.6992)		(0.0039)**	(0.0445)*		(0.2115)
		1.077			2.747	
		(0.2814)			(0.0060)**	

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	Treatment A – Experiment 1			*Treatment B – Experiment 1*		
	$\alpha_{seg6:800:1/4}$	$\alpha_{seg7:800:1/2}$	$\alpha_{seg8:800:3/4}$	$\alpha_{agg6:800:1/4}$	$\alpha_{agg7:800:1/2}$	$\alpha_{agg8:800:3/4}$
Wilcoxon z	0.124		2.206	0.709		2.142
(p-value)	(0.9011)		(0.0274)*	(0.4785)		(0.0322)*
		1.062			2.166	
		(0.2882)			(0.0303)*	

	Treatment A – Experiment 2			*Treatment B – Experiment 2*		
	$\alpha_{seg10:500:1/4}$	$\alpha_{seg11:500:1/2}$	$\alpha_{seg12:500:3/4}$	$\alpha_{agg10:500:1/4}$	$\alpha_{agg11:500:1/2}$	$\alpha_{agg12:500:3/4}$
Wilcoxon z	-0.099		2.608	2.120		2.440
(p-value)	(0.9215)		(0.0091)**	(0.0340)*		(0.0147)*
		2.391			3.241	
		(0.0168)*			(0.0012)**	

	Treatment A – Experiment 2			*Treatment B – Experiment 2*		
	$\alpha_{seg13:500:1/4:exo}$	$\alpha_{seg14:500:1/2:exo}$	$\alpha_{seg15:500:3/4:exo}$	$\alpha_{agg13:500:1/4:exo}$	$\alpha_{agg14:500:1/2:exo}$	$\alpha_{agg15:500:3/4:exo}$
Wilcoxon z	0.019		1.267	3.287		1.374
(p-value)	(0.9850)		(0.2050)	(0.0010)***		(0.1696)
		0.966			2.569	
		(0.3339)			(0.0102)*	

	Treatment A – Experiment 2			*Treatment B – Experiment 2*		
	$\alpha_{seg16:500:1/4:end}$	$\alpha_{seg17:500:1/2:end}$	$\alpha_{seg18:500:3/4:end}$	$\alpha_{agg16:500:1/4:end}$	$\alpha_{agg17:500:1/2:end}$	$\alpha_{agg18:500:3/4:end}$
Wilcoxon z	-0.591		1.641	-0.394		2.604
(p-value)	(0.5542)		(0.1008)	(0.6936)		(0.0092)**
		0.788			2.081	
		(0.4305)			(0.0374)*	

Recently, researchers have started dealing with closely related problems by assuming that decisions makers act according to a specific decision rule plus an error term (see e.g., Harless and Camerer, 1994 and Hey and Orne, 1994). Although the methodology used in these studies is not transferable to our problem, we employ a data analysis method which is motivated by this literature.

We first give the basic intuition for the following analysis. Assume that subjects act according to a specific preference calculus, which results in an optimal total risk position. Assume furthermore that subjects are not able to express their true preference. They make a mistake. In our experiment, a mistake means they deviate a little bit from their optimal $\alpha_{segi:y:0}$ in decision situation segi:y:0 or from their optimal $\alpha_{aggi:y:0}$ in decision situation aggi:y:0 in terms of percentage points.[13] In other words, the subject chooses the $\alpha_{segi:y:0}$ ($\alpha_{aggi:y:0}$) $\forall i \in \{0,1,2\}$, which lead to her desired risk profile being disturbed by a small error term ϵ. We then ask for each decision situation if the subject chooses consistently such an α. We next introduce helpful definitions to make the idea more precise.

Consider three α-values in decision situation 0 to 2 leading to the same total risk position called trp ($\alpha^{trp}_{seg0:0:0}$; $\alpha^{trp}_{seg1:400:0}$; $\alpha^{trp}_{seg2:800:0}$). Consider now the three intervals $[\alpha^{trp}_{seg0:0:0} - \epsilon; \alpha^{trp}_{seg0:0:0} + \epsilon]$, $[\alpha^{trp}_{seg1:400:0} - \epsilon; \alpha^{trp}_{seg1:400:0} + \epsilon]$, and $[\alpha^{trp}_{seg2:800:0} - \epsilon; \alpha^{trp}_{seg2:800:0} + \epsilon]$, which define a corridor. Note that only one α-value is needed to fully determine a corridor. We use the $\alpha^{trp}_{seg0:0:0}$-value as the parameter to define the corridor. One $\alpha^{trp}_{seg0:0:0}$-value corresponds exactly to one corridor.

If we assume that a subject chooses according to her true preference, disturbed by a small error term in every three decision situations, we should find a corridor which contains the chosen α's of the subject. In this sense, we will say that this subject acts according to the adjustment effect.

To express it more mathematically, a corridor fits the data of one subject if an integer $\alpha^{trp}_{seg0:0:0} \in [\alpha_{seg0:0:0} - \epsilon; \alpha_{seg0:0:0} + \epsilon]$ exists such that

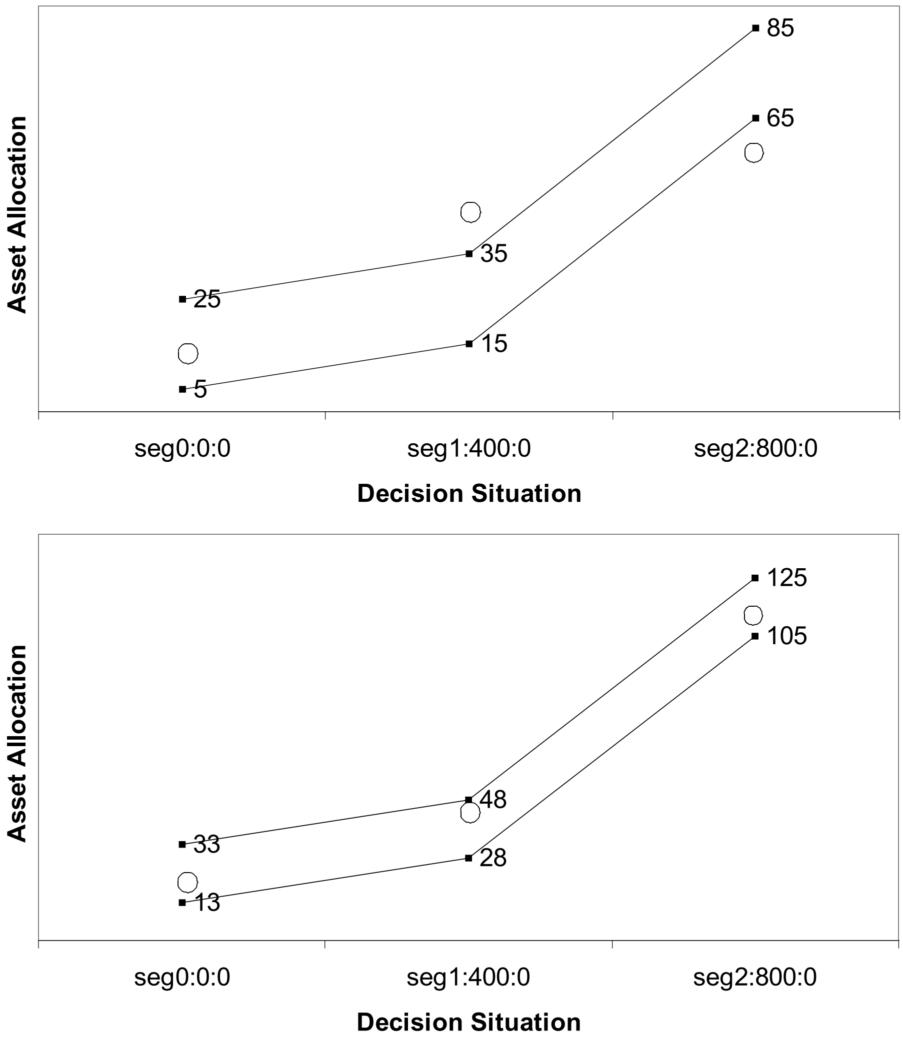
$$\alpha^{trp}_{seg1:400:0} - \epsilon \leq \alpha_{seg1:400:0} \leq \alpha^{trp}_{seg1:400:0} + \epsilon \ \textit{and}$$
$$\alpha^{trp}_{seg2:800:0} - \epsilon \leq \alpha_{seg2:800:0} \leq \alpha^{trp}_{seg2:800:0} + \epsilon$$

with ϵ being a constant, $\alpha_{seg0:0:0}$, $\alpha_{seg1:400:0}$, and $\alpha_{seg2:800:0}$ being the responses of a specific subject in our experiment, and $\alpha^{trp}_{seg0:0:0} \pm \epsilon \in [-100; 400]$, $\alpha^{trp}_{seg1:400:0} \pm \epsilon \in [-100; 400]$, $\alpha^{trp}_{seg2:800:0} \pm \epsilon \in [-100; 400]$.

[13] A second opportunity is to define an error as a deviation in terms of DM amounts, see Appendix C.2.

Figure 3 illustrates this idea. The three points in both graphs are responses of two hypothetical subjects to decision situations seg0:0:0, seg1:400:0, and seg2:800:0 (or agg0:0:0, agg1:400:0, and agg2:800:0). For a given ϵ, the upper example shows a corridor which doesn't fit the data and the lower example shows a corridor which fits.

Figure 3: *Illustration of a Corridor.* The upper example shows a corridor which doesn't fit the data, i.e. two responses are lying outside the corridor. The lower example shows three different responses. Every response lies inside the corridor.



For sufficiently risk averse subjects, the method can be used in a meaningful way. But assume a risk seeking or risk neutral person who chooses 400 in every decision situation due to the borrowing constraint. Using our method, we classify this subject as someone who acts according to the adjustment effect. However, the adjustment effect is meaningless for such a person, as she would decide to buy as much of the lottery as possible, no matter how large the exogenous income is. A second concern is our scale from -100 to 400. It is possible that a risk averse subject prefers a risk profile that leads to $\alpha^{trp}_{seg2:800:0} > 400$. We couldn't elicit the exact value, but our algorithm judges such a subject as consistent with the adjustment effect. A subject faces the borrowing constraint in seg2:800:0 if $\alpha^{trp}_{seg0:0:0} \geq 80$, and in seg1:400:0 if $\alpha^{trp}_{seg0:0:0} \geq 240$. The short sale constraint is binding for seg2:800:0 if $\alpha^{trp}_{seg0:0:0} \leq -20$, and for seg1:400:0 if $\alpha^{trp}_{seg0:0:0} \leq -60$. The results of the analysis should be interpreted in this light.

Figure 4 shows the smallest possible error rate, so that we can find a fitting corridor for one subject. For example, if the responses of one subject are $\alpha_{seg0:0:0} = 14$, $\alpha_{seg1:400:0} = 26$, and $\alpha_{seg2:800:0} = 75$, the smallest possible error rate is 1 with $\alpha^{trp}_{seg0:0:0} = 15$ defining the best fitting corridor. Figure 4 shows the smallest possible error rate for every subject and the $\alpha^{trp}_{seg0:0:0}$ which defines the best fitting corridor.

The data presented in figure 4 reveals that a notable number of subjects chooses according to the adjustment effect given reasonable error rates.[14] Especially interesting is the remarkable consistency of a relatively high number of subjects. Remember, that subjects were confronted with a series of complex decision situations and that the occurrence of the different tasks were randomized. Clearly, our criterium depends on the definition of a "reasonable" error term. However, using a relatively restrictive error term of 10 percentage points, 50% of all subjects choose at least in one of the two treatments according to the adjustment effect.[15]

It also seems noteworthy that we can identify more consistent people in treatment B than in treatment A for low error rates (see figure 4 and Appendix C.1). The computation of the aggregate risk profiles leads to a stronger tendency to choose the same total risk position in all three relevant decision situations 0:0:0 to 2:800:0 for a group of subjects. It is also evident that the variance in treatment B is higher. Another group of subjects seemed to have more problems with the decision tasks in treatment B compared to treatment A.

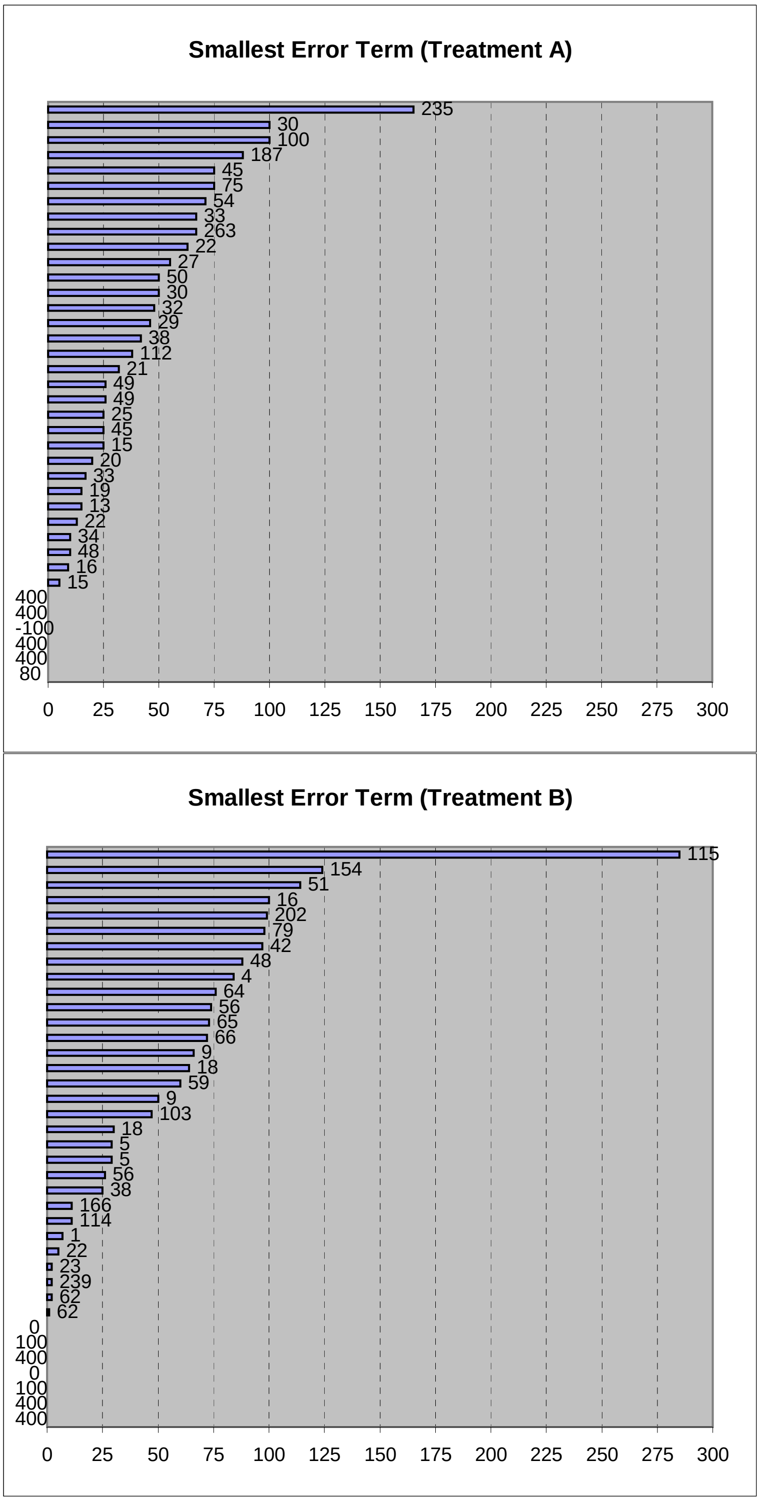
4.2. The Horizon Effect

We now turn to the horizon effect. Only weak support for hypothesis 2 can be found at a satisfying significance level (L = 470; p = 0.0541). In contrast, the null hypothesis can be rejected in treatment B (L = 478.5; p=0.0049**). A closer look at the Wilicoxon tests reveals a systematic pattern (see table 4). We notice virtually no difference between $\hat{\alpha}_{seg0:0:0}$ and $\hat{\alpha}_{seg4:400:1/2}$ ($\hat{\alpha}_{agg0:0:0}$ and $\hat{\alpha}_{agg4:400:1/2}$) and a significant effect between $\hat{\alpha}_{seg4:400:1/2}$ and $\hat{\alpha}_{seg7:800:1/2}$ ($\hat{\alpha}_{agg4:400:1/2}$ and $\hat{\alpha}_{agg7:800:1/2}$).

[14] Brennan and Torous (1999) estimate utility losses due to suboptimal equity participation and therefore a decision problem similar to our decision situations seg0:0:0 to seg2:800:0. They use a constant relative risk aversion utility function and consider the wealth level required with a suboptimal allocation, to reach the same level of expected utility under the optimal allocation. The costs of a small deviation from the optimal allocation are modest, e.g. a 10% error requires approximately a 1% higher wealth level depending on the assumed degree of risk aversion.

[15] The table in Appendix C.1 reports on more details for every subject.

Figure 4: *Smallest Possible Error Term for Each Subject in Experiment 1.* This figure reports on details concerning the adjustment effect. The smallest possible error rate for every subject and the $\alpha^{trp}_{seg0:0:0}$-value which defines the best fitting corridor are given. The smallest possible error rates are presented in ascending order.



The results are relevant for the ongoing discussion if investors with long time horizons (should) invest more heavily in risky assets. A riskier portfolio for longer horizons is the accepted opinion at least outside the academic area. Uncorrelated background risk provides a rational argument for this (Jagannathan and Kocherlakota 1996). One question that remains unanswered is whether the salience of the described opinion is mainly due to rational considerations (for example the stated background risk argument) or due to behavioral biases or bounded rationality. An example which belongs to the latter explanation is a misinterpretation of the law of large numbers (Samuelson 1963). Our results show that the (rational) argument, investors with longer time horizons should choose riskier portfolios due to larger (uncorrelated) background risk, seems for our subjects only to be valid for a high proportion of background risk to total wealth.

4.3. The Correlation Effect

The reader will recall that hypothesis 3 states that we expect no differences in consequence of the correlation variations in treatment A. In conformance with this hypothesis, it is not possible to reject the null hypothesis of a Page test for both proportions of background risk to total wealth in experiment 1 (BR 400 TDM: L = 470; p = 0.0541 and BR 800 TDM: L = 466.5; p = 0.1142). Only one of three Page tests is significant in experiment 2 (no increase in risk: L = 637.5; p = 0.0058**; increase in the exogenous risk: L = 627; p = 0.0687; increase in the endogenous risk: L = 623; p = 0.1380). In sum, no or at best weak support for a correlation effect can be found in treatment A.

However, the Wilcoxon tests reported in table 4 reveal a systematic difference. In the case of a positive correlation (seg5:400:3/4, seg8:800:3/4, seg12:500:3/4, seg15:500:3/4:exo, seg18:500:3/4:end) subjects choose systematically lower allocations compared to a zero correlation. The difference between a positive and a negative correlation goes against our hypothesis and is with the exception of one comparison not significant.

A first possibility is that this result is due to loss aversion (Kahneman and Tversky 1979). The risk communication with the arrow diagrams calls special attention to the possible serve loss in the decision situations with a positive correlation. The problem with this explanation is that it should also have an effect on the differences between allocations in the negative and the zero correlation decision tasks. In the decision situation with the negative correlation the arrow diagram draws special attention to the fact that the possible serve loss can occur only with a low probability. However, such an effect is not observable for both amounts of background risk. Loss aversion can therefore not explain the results in treatment A.

The following is a second possibility. Imagine that subjects do indeed react according to the correlation effect. Assume, in addition, that the zero correlation decision tasks are easier to grasp. Subjects feel more competent in making their decision in the zero correlation situations and choose therefore a higher allocation. If we assume that both effects overlap, the observed pattern will result, i.e. no effect between a negative and a zero correlation and a strong difference for a positive correlation. However, this reasoning is speculative and ex post at best. The significant drop in the positive correlation decision situations in treatment A remains puzzling.

Using the data from treatment B, we find highly significant support for hypothesis 3a (*Experiment 1:* BR 400 TDM: L = 477; p = 0.0080** and BR 800 TDM: L = 478; p = 0.0058**; *Experiment 2:* no increase in risk: L = 647.5; p = 0.0002***; increase in the exogenous risk: L = 601.5; p = 0.1493; increase in the endogenous risk: L = 634.6; p = 0.0129*). The Wilicoxon tests are also in line with the hypothesis (see table 4). With only one exception, all possible comparisons differ in the predicted direction and they are in most cases significant.

The qualitative differences between treatments A and B are obvious for the correlation effect. In treatment A we replicate the well-known fact that correlations are largely neglected in financial decision making. However, the picture changes for treatment B. To our knowledge this is one of the first experimental scenarios which documents a qualitative correct processing of correlations in a financial context. The comparison with the results of treatment A shows that the explicit computation of aggregate risk profiles is necessary to obtain this result. Decision situations are equivalent in terms of final wealth changes, but the overall perspective in treatment B is different and seemingly helpful. The analysis supports our view that explicit computation of the aggregated risk profiles helps subjects to partly overcome the deviations from normative theories due to neglect of correlations, or put differently, helps them to make "better" decisions.

4.4. Differences Between the Treatments

Up to now, we have mainly presented results separately for both treatments. A self-evident question is whether there are any systematic differences between the responses in treatment A and treatment B. The following table 5 reports the results of Wilcoxon tests between every equivalent decision situation.

We observe a systematic effect for a risky exogenous income of 800 TDM (decision situations 6:800:1/4 to 8:800:3/4). Subjects invest a larger fraction of financial wealth in treatment B. Maybe this is due to underestimating the high impact of the exogenous income part. Necessary or desired high adjustments in the split of financial wealth may be more obvious in treatment

Table 5: *Differences Between the Treatments A and B.* The table addresses the question of whether there are any systematic differences between treatments A and B. The reported numbers belong to Wilcoxon tests between every equivalent decision situations.

Wilcoxon-Tests		
Decision Situation	Test Statistic	p-value
0 (seg0:0:0 vs. agg0:0:0)	-0.087	0.9306
1 (seg1:400:0 vs. agg1:400:0)	1.661	0.0967
2 (seg2:800:0 vs. agg2:800:0)	-0.916	0.3597
3 (seg3:400:1/4 vs. agg3:400:1/4)	-1.182	0.2371
4 (seg4:400:1/2 vs. agg4:400:1/2)	0.595	0.5519
5 (seg5:400:3/4 vs. agg5:400:3/4)	-0.602	0.5472
6 (seg6:800:1/4 vs. agg6:800:1/4)	-2.988	0.0028**
7 (seg7:800:1/2 vs. agg7:800:1/2)	-2.188	0.0287*
8 (seg8:800:3/4 vs. agg8:800:3/4)	-1.703	0.0885

B. Besides this, no systematic effect is observed. An analogous analysis for experiment 2 also reveals no result which may be of relevance for the three effects considered in this paper. The test statistics are given in Appendix D.

Differences exist between equivalent decision situations, but the results are not sufficient to establish a systematic framing effect. Differences seem to appear randomly, apart from the qualitative discrepancies discussed in Sections 4.1 and 4.3.

5. Incentives and Subject Pool

One general caveat against the experimental investigation of portfolio choice behavior is that the incentives are too low, so that the observed behavior has no relevance for the behavior in markets. However, our results should still hold if we take the experimental evidence on the effect of incentives into account. Binswanger (1980), Binswanger (1981), Kachelmeier and Shehata (1992) and Holt and Laury (2002) report that subjects behave more risk averse if all payoffs are scaled up. In our experiment, the hypotheses are not affected by a greater degree of risk aversion. If somebody is more risk averse when higher stakes are at hand, she will choose a lower exposure to the risky asset on average. But the qualitative relationships tested here ("the overall decision pattern") should still hold. This conjecture is consistent with previous research. For example, Kachelmeier and Shehata (1992) elicit certainty equivalents for a couple of different lotteries and they report the same general dependence between the average ratio of the certainty equivalent to expected value and the percentage of winning for high and low monetary incentives. The degree of risk aversion changes in their experiments as they increase the stakes, but they observe no difference in the overall decision pattern. Further experimental results have also shown that subjects seem to behave more in line with normative theories (more "rational") when the payments are high.[16] Note, that the subjects in our experiments already behave in line with the normative theory. If we increase the monetary incentives, this picture should emerge even stronger and therefore strengthen our results. The only exception may be the neglect of correlations in treatment A. But for this particular question our results may be interpreted as in line with field data studies, which document ambiguous results. One possible interpretation of the field data results is that people do not take the correlation structure into account and that therefore the ambiguous picture observed in field data sets emerges. Also note that the comparison of the two described treatments is most important for our experiment. We have shown that the computation of aggregated risk profiles helps people to take the correlation structure into account. From our point of view, it is possible (but not likely) that on average a correct processing of correlations in treatment A may be observed if we increase the stakes. However, there is no reason to believe that the computation of aggregated risk profiles would be useless for higher incentives.

A second common caveat addresses the subject pool. It is often argued in two completely different ways. The first line of reasoning says that finance students are too clever. They know about at least some basic portfolio choice concepts and therefore it is not surprising that the results are in line with the theory. The second line of reasoning says that finance students are not clever enough. Professional institutional investors would never make the mistakes that students do (for example neglect correlations). There exists some evidence that the last statement may not be true. For example, Haigh and List (2004) show that professional traders exhibit a greater degree of myopic loss aversion than a student control group. Furthermore, Sarin and Weber (1993) investigate the effects of ambiguity in market experiments with students and experienced bond traders. They find that ambiguity aversion does not vanish in a market setting for both subject groups and they especially observe no significant difference in behavior between students and traders. There is also evidence that the first statement may not be true. Finance students can indeed make systematic mistakes in portfolio choice experiments and the observed rational behavior in this experiment is in no way tautological.[17]

Potential differences between students, traders, and other subgroups of the population is an interesting and active research field. In any case, we consider the finance student subject pool as suitable for the questions at hand. They are highly motivated to think about such problems and they shortly will have to decide about their strategic asset allocation or have already had to

[16] Kroll et. al. (1988) present experimental evidence in a portfolio choice context.

[17] See for this particular decision context the portfolio choice experiments cited above or in general the review from Camerer (1995) of the individual decision making literature. For example, students make systematic mistakes in Baysian updating even if they know about Bayes Rule.

decide about their strategic asset allocation. From our point of view, their intrinsic motivation,[18] which may not necessarily be expected by professional traders or non-finance students, is the best argument for using finance students in this relatively difficult and newly-made experiment.

6. Summary

This paper adds to the empirical literature on portfolio choice in the presence of nontradeable income by reporting the results of experiments. The experimental approach makes a direct causal interpretation possible and leads to within-subject data.

In most cases, the theoretical literature predicts the behavior of our subjects in the complex decision situations quite well. Subjects adjust their proportion of financial wealth invested in the risky asset as a result of higher and certain nontradeable payments in the predicted direction. Higher amounts of uncorrelated background risk lead to higher exposures to the risky asset.

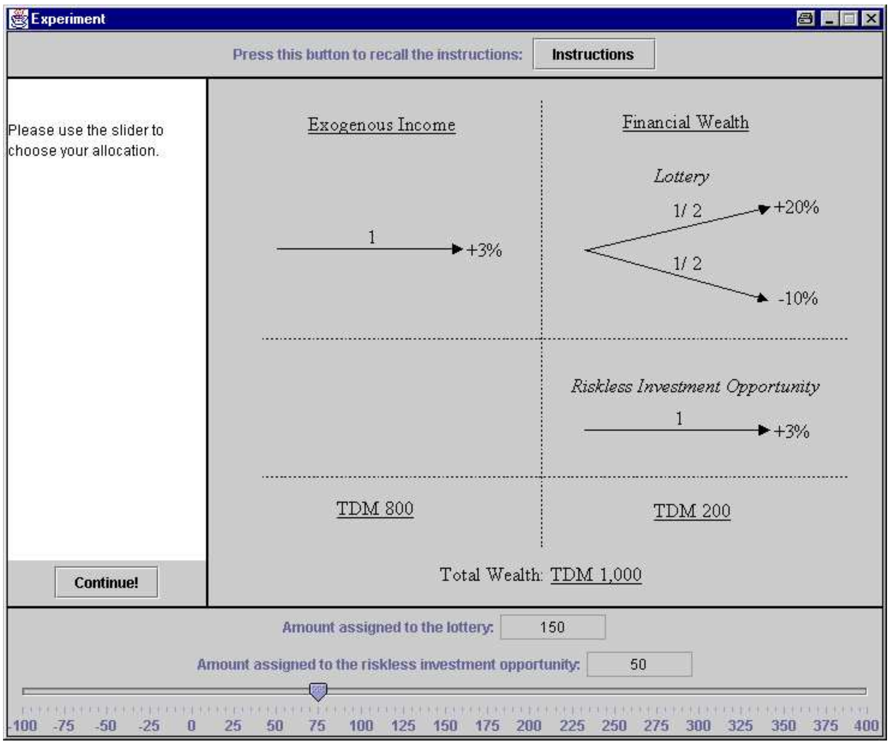
We communicate correlations by the use of conditional probabilities. Correlations are largely ignored using a segregated description of the decision problem. However, the computation of aggregated risk profiles helps subjects to partly overcome the deviations from normative theory due to neglect of correlations. Further research has to clarify under which circumstances the computation of aggregate risk profiles helps people to behave "more rationally". This seems to be especially interesting in a financial context.

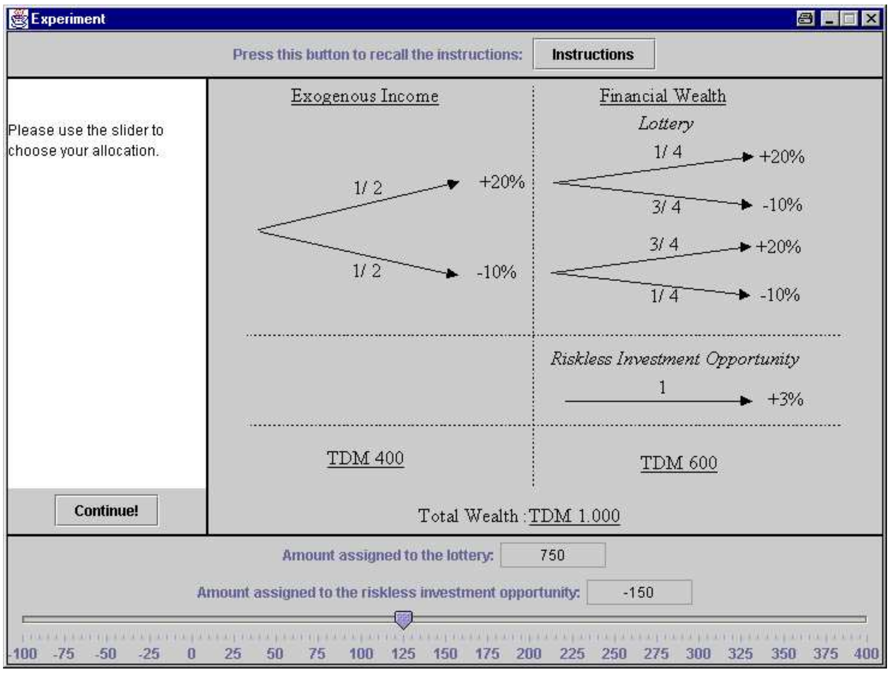
We only considered the data for the adjustment effect with respect to quantitative differences, since this is the only effect we could analyze concerning point predictions without assuming a specific preference calculus. Utilizing a small error, we can classify a relatively high proportion of our subjects as choosing consistent with the adjustment effect.

[18]Note furthermore, that all participants agreed voluntary to participate in an experiment announced as an experiment on individual decision making.

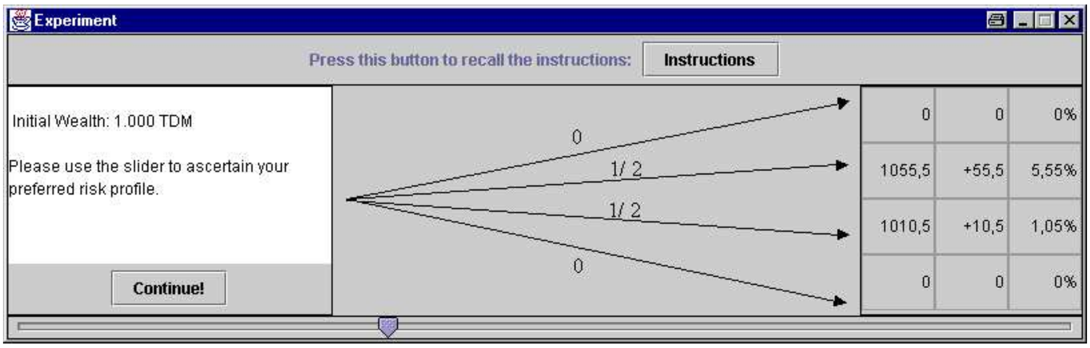
A. Screenshots

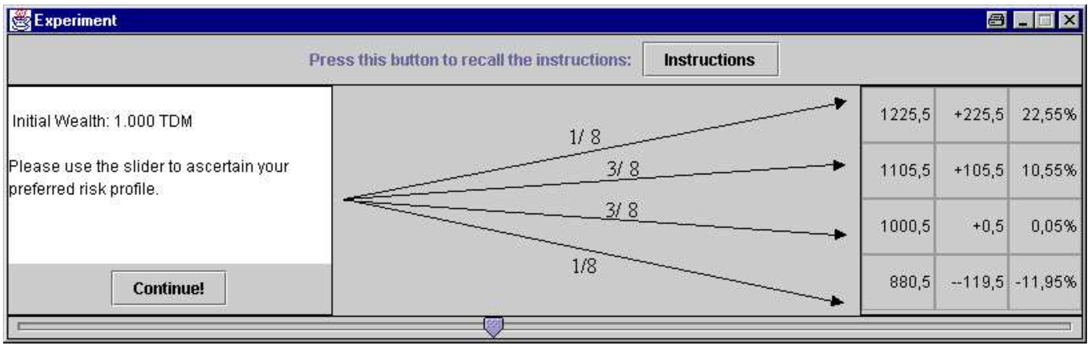
A.1. Treatment A





A.2. Treatment B





B. Means, Medians, and Standard Deviations of Chosen Allocations

B.1. Experiment 1

The following table contains the median, mean, and standard deviation of the chosen allocations for every decision situation in treatments A and B for experiment 1.

Descriptive Statistics - Experiment 1			
decision situation	median	mean	standard deviation
seg0:0:0	75	112	131
seg1:400:0	92.5	131	126
seg2:800:0	150	194	139
seg3:400:1/4	66.5	100	139
seg4:400:1/2	64.5	117	137
seg5:400:3/4	36.5	84	125
seg6:800:1/4	100	137	142
seg7:800:1/2	95	139	155
seg8:800:3/4	67.5	106	145
agg0:0:0	100	120	123
agg1:400:0	51.5	112	130
agg2:800:0	241	226	154
agg3:400:1/4	70.5	131	132
agg4:400:1/2	68.5	109	148
agg5:400:3/4	30	98	161
agg6:800:1/4	244.5	214	169
agg7:800:1/2	219.5	216	172
agg8:800:3/4	223	160	209

B.2. Experiment 2

The following table contains the median, mean, and standard deviation of the chosen allocations for every decision situation in treatments A and B for experiment 2.

Descriptive Statistics - Experiment 2			
decision situation	median	mean	standard deviation
$seg9 : 500 : 0$	100	148	127
$seg10 : 500 : 1/4$	100	132	118
$seg11 : 500 : 1/2$	98	128	120
$seg12 : 500 : 3/4$	70	105	115
$seg13 : 500 : 1/4 : exo$	75	111	116
$seg14 : 500 : 1/2 : exo$	80	102	102
$seg15 : 500 : 3/4 : exo$	50	105	128
$seg16 : 500 : 1/4 : end$	88	128	132
$seg17 : 500 : 1/2 : end$	100	133	111
$seg18 : 500 : 3/4 : end$	80	115	117
$agg9 : 500 : 0$	95	154	154
$agg10 : 500 : 1/4$	161	171	120
$agg11 : 500 : 1/2$	108	153	142
$agg12 : 500 : 3/4$	101	105	152
$agg13 : 500 : 1/4 : exo$	233	218	121
$agg14 : 500 : 1/2 : exo$	195	171	137
$agg15 : 500 : 3/4 : exo$	170	157	166
$agg16 : 500 : 1/4 : end$	58	84	99
$agg17 : 500 : 1/2 : end$	69	104	128
$agg18 : 500 : 3/4 : end$	40	74	123

C. The Error Term Analysis

C.1. Further Details on the Error Term Analysis

This table reports on details of every subject concerning the error analysis of adjustment effect. The left column contains an identification number for every subject. For treatment A, we report the smallest possible error term (SPET) and the $\alpha^{trp}_{seg:0:0}$-value, which defines the fitting corridor in the second and third column. The forth and fifth columns contain the same type of information for treatment B.

Nr.	*Treatment A*		*Treatment B*		Nr.	*Treatment A*		*Treatment B*	
	SPET	$\alpha^{trp}_{seg:0:0}$	SPET	$\alpha^{trp}_{agg:0:0}$		SPET	$\alpha^{trp}_{seg:0:0}$	SPET	$\alpha^{trp}_{agg:0:0}$
1	50	30	26	56	20	71	54	2	23
2	48	32	2	62	21	75	75	47	103
3	0	80	25	38	22	38	112	0	100
4	0	400	99	202	23	13	22	0	0
5	15	13	100	16	24	25	15	5	22
6	67	263	0	400	25	100	30	29	5
7	42	38	50	9	26	25	45	7	1
8	100	100	97	42	27	25	25	84	4
9	67	33	30	18	28	0	400	0	400
10	17	33	66	9	29	26	49	11	114
11	10	48	1	62	30	88	187	285	115
12	46	29	124	154	31	20	20	29	5
13	26	49	73	65	32	15	19	98	79
14	63	22	60	59	33	75	45	0	100
15	5	15	76	64	34	55	27	72	66
16	9	16	74	56	35	32	21	0	0
17	0	400	2	239	36	10	34	64	18
18	0	-100	88	48	37	50	50	11	166
19	0	400	0	400	38	165	235	114	51

C.2. An Alternative Specification of the Error Term

Denominate $\beta_{segi:y:q}$ the amount of money invested in the lottery. (The rest of the denomination is analogous to section 4.1.) We assume 50 TDM and 100 TDM to be reasonable error rates.

A corridor fits the data of one subject if there exist an integer $\beta^{trp}_{seg0:0:0} \in [\beta_{seg0:0:0} - \epsilon; \beta_{seg0:0:0} + \epsilon]$ such that

$$\beta^{trp}_{seg1:400:0} - \epsilon \leq \beta_{seg1:400:0} \leq \beta^{trp}_{seg1:400:0} + \epsilon \ and$$
$$\beta^{trp}_{seg2:800:0} - \epsilon \leq \beta_{seg2:800:0} \leq \beta^{trp}_{seg2:800:0} + \epsilon$$

with ϵ being a constant, $\beta_{seg0:0:0}$, $\beta_{seg1:400:0}$, and $\beta_{seg2:800:0}$ being the responses of a specific subject in our experiment, and $\beta^{trp}_{seg0:0:0} \pm \epsilon \in [-1000; 4000]$, $\beta^{trp}_{seg1:400:0} \pm \epsilon \in [-600; 2400]$, $\beta^{trp}_{seg2:800:0} \pm \epsilon \in [-200; 800]$.

Using the amount of money invested in the lottery as a measure, a completely rational response is $\beta_{seg0:0:0}$=$\beta_{seg1:400:0}$=$\beta_{seg2:800:0}$.

This table reports on details of every subject concerning the error analysis of adjustment effect. The left column contains an identification number for every subject. For treatment A, we report the smallest possible error term (SPET) and the $\beta^{trp}_{seg:0:0}$-value, which defines the fitting corridor in the second and third column. The forth and fifth columns contain the same type of information for treatment B.

Nr.	*Treatment A*		*Treatment B*		Nr.	*Treatment A*		*Treatment B*	
	SPET	$\beta^{trp}_{seg:0:0}$	SPET	$\beta^{trp}_{agg:0:0}$		SPET	$\beta^{trp}_{seg:0:0}$	SPET	$\beta^{trp}_{agg:0:0}$
1	275	475	99	512	20	550	700	11	239
2	194	418	2	618	21	300	1200	375	1125
3	2	802	191	419	22	300	1200	1	1001
4	0	4000	606	2404	23	50	250	0	0
5	58	157	595	565	24	150	150	32	218
6	402	2898	0	4000	25	395	505	114	114
7	250	550	211	209	26	100	500	40	40
8	400	1600	386	616	27	150	350	335	-135
9	400	600	116	116	28	0	4000	0	4000
10	100	400	398	92	29	151	391	85	1165
11	40	460	4	616	30	700	1700	1886	2114
12	275	475	988	1792	31	120	120	115	115
13	189	561	579	801	32	85	255	780	990
14	325	425	239	467	33	300	600	1	1001
15	28	128	604	796	34	290	460	286	514
16	33	183	299	711	35	126	276	0	0
17	0	4000	15	2385	36	44	366	256	52
18	0	-1000	350	660	37	300	700	90	1680
19	0	4000	0	4000	38	1044	2956	451	743

D. Differences between the two treatments in experiment 2

Wilcoxon-Tests		
Decision Situation	Test Statistic	p-value
9 (seg9:500:0 vs. agg9:500:0)	0.141	0.8882
10 (seg10:500:1/4 vs. agg10:500:1/4)	-2.161	0.0307*
11 (seg11:500:1/2 vs. agg11:500:1/2)	-1.144	0.2526
12 (seg12:500:3/4 vs. agg12:500:3/4)	0.023	0.9813
13 (seg13:500:1/4:exo vs. agg13:500:1/4:exo)	-4.748	0.0000***
14 (seg14:500:1/2:exo vs. agg14:500:1/2:exo)	-3.126	0.0018**
15 (seg15:500:3/4:exo vs. agg15:500:3/4:exo)	-1.898	0.0576
16 (seg16:500:1/4:end vs. agg16:500:1/4:end)	1.589	0.1120
17 (seg17:500:1/2:end vs. agg17:500:1/2:end)	2.095	0.0361*
18 (seg18:500:3/4:end vs. agg18:500:3/4:end)	2.273	0.0230*

Note, that the tests above document lower investments in treatment A for an increase in the exogenous risk and higher investments for an increase in the endogenous risk. This and further results of our experiment 2 are issues of ongoing research.

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