Decisions under Uncertainty: Preferences, Institutions, and Social Interaction

Inauguraldissertation zur Erlangung des Doktorgrades
der Wirtschafts- und Sozialwissenschaftlichen Fakultät
der Universität zu Köln
2015

vorgelegt von
Dipl.-Kfm. Christopher Zeppenfeld
aus Wipperfürth

Referent:	Prof. Dr. Bettina Rockenbach
Korreferent:	Prof. Achim Wambach, Ph.D.
Tag der Promotion:	10. September 2015

Ich danke meiner Familie, meinen Freunden und Kollegen.
Allen, die zu dieser Arbeit beigetragen, sie durch
Kommentare und Einsichten verbessert sowie Freud und Leid
ihrer Fertigstellung geteilt haben.

Danke!

CONTENTS

Chapter 1

INTRODUCTION

Life is uncertain. From the prediction of stock prices and the behavior of market competitors to the anticipation of the behavior of other road users, decisions are often made in absence of precise knowledge of their consequences. Rather, a decision maker forms beliefs about the likelihood of an event. In addition, the outcomes of such events typically vary in their desirability. Deciding about which action to take in the face of uncertainty thus involves the joint assessment of the likelihood and the desirability of associated outcomes. This dissertation consists of four papers that analyze how individuals decide under uncertainty and focuses on determinants of such decisions: *Preferences*, *Institutions*, and *Social Interaction*.

Uncertainty is a catch-all term that comprises various reasons for the variability in outcomes (e.g., Knight 1921). Two possible sources are relevant for this dissertation. First, *Risk* refers to the case in which the randomness that the individual encounters can be described by exogenous probabilistic information that is objectively known (Machina 2008b). Typically risk results from draws of "nature" (e.g., lotteries) that have an ex-ante specified distribution. Second, *Strategic Uncertainty* results from the unpredictability of human behavior (Van Huyck, Battalio, and Beil 1990). The interaction with other people requires to form an assessment of their behavior and to anticipate how they will react to one's own decisions.

Preferences are the primitive of every decision as they rank possible consequences according to their desirability. Preferences in the canonical model of decision making under uncertainty are represented by a utility function that assigns each consequence a real number. The individual then strives to maximize the expected utility (Savage 1954; Anscombe and Aumann 1963). *Institutions* determine the general economic environment and are a set of rules, conventions, and laws that structure, constrain, and enable individual behavior because they impose rules that create stable expectations about the choice set of others (Hodgson 2006). For example, markets are institutions that govern individual interaction through prices (Manski 2000). Economic circumstances such as the opportunity to work overtime, the organization of the tax system or regulatory entities such as central banks are further examples. *Social Interaction* refers to direct interaction with other decision makers and the interdependencies of individual decisions. Social interaction is sometimes also called "non-market" interaction to emphasize that the interaction is not solely governed by the price mechanism but rather represents externalities imposed by the actions of others that affect individual preferences (Scheinkman 2008).

This dissertation focuses on how the interplay of these factors shape individual behavior and influences decisions in the face of uncertainty. Individual decisions create aggregate economic outcomes and determine how these outcomes vary with policy interventions. For example, preferences determine the impact of institutions while, in return, the institutional organization of economic environments constraints the actions that an individual can choose from to express his or her preferences. Hence, while institutions constrain individual behavior on the one hand, their effect and specific design is shaped by individual behavior (Hodgson 2006). Furthermore, social interaction itself influences the formation of preferences, e.g., by emphasizing the externalities of decisions on others and thus reinforcing concerns regarding the benefits of others.

1.1 Overview and Main Findings

This section provides an overview of each chapter and presents the main findings. A detailed account of the co-authors, financial support, and acknowledged comments and suggestions is provided in Section 1.2.

Chapter 2 entitled "*Circumstantial Risk: Impact of Future Tax-Evasion and Labor-Supply Opportunities on Risk Exposure*" is joint work with Philipp Doerrenberg and Denvil Duncan. We examine whether institutional circumstances to respond to investment outcomes ex-post changes the willingness to take risks ex-ante. In a laboratory experiment, subjects can invest earned income in a risky asset and have the opportunity to respond to the outcome of the investment through extra labor effort and/or tax evasion. These circumstances generally involve two channels: flexibility and background risk. Flexibility allows to increase income with certainty and is predicted to increase risk exposure. Background risk stems from uninsurable, risky income in addition to the main risk of the investment decision and its effect on risk-taking depends on individual risk attitudes.

We find evidence that ex-post access to labor opportunities decreases ex-ante risk-taking while access to tax evasion has no effect. Having both opportunities leads to lower risk-taking, but this effect is not statistically significant. Because labor opportunities can create both certain and risky income, we explore the channels behind these results with two additional treatments that eliminate any unintentional income variability. This allows us to disentangle the role of flexibility from the effect of background risk. We find that if labor income is risky, subjects who can respond to the lottery outcome by both providing extra effort and evade taxes, take more risks than subjects who can only respond with additional labor. However, the former group takes less risk than the latter if labor income is certain. Our results indicate that the positive effect on risk-taking due to flexibility is not strong enough the overcome the negative effect of background risk in our setting.

Chapter 3 entitled "*Dynamic On-the-Spot Consumption and Portfolio Choice in the Lab*" is co-authored with Michaela Pagel and introduces a novel experimental setting that allows to test life-cycle consumption and portfolio-choice models with real consumption outcomes. Whereas previous tests of life-cycle models typically rely on monetary incentives that are paid out at the very end of the experiment and thus do not preserve the intertemporal structure of these models, we implement on-the-spot consumption. We use our design to test several implications of the standard life-cycle model of Samuelson (1969) which is the workhorse model in macroeconomics and finance. Specifically, we design a four-period dynamic life-cycle environment and test how consumption and portfolio allocations (i) respond to fluctuations in wealth and (ii) differ from a pre-committed plan. In a within-subject design, we elicit choices under both pre-commitment (PC) and non-pre-commitment (NPC) and can thus test if and how subjects deviate from their ex-ante plan over the course of the experiment.

We find that the standard model is generally a good predictor for consumption and investment behavior. Under NPC, consumption and investment shares do not systematically vary in the investment outcome. Furthermore, investment shares do neither vary over time nor differ significantly between NPC and PC. We observe two inconsistencies for consumption behavior. First, under PC, subjects plan to increase their consumption share after a reduction in wealth. Second, subjects show a propensity to underconsume under NPC relative to their PC plan. We discuss two non-standard preference theories based on reference dependence that can explain these inconsistencies. However, these alternatives make a series of other predictions for which we do not find empirical support. We conclude that decisions in our setting are best described by the standard model.

Chapter 4 entitled "*Preferences and Decision Support in Competitive Bidding*" is joint work with Nicolas Fugger, Philippe Gillen, and Alexander Rasch. We examine bidding behavior in first-price sealed-bid and Dutch auctions which are strategically equivalent under standard preferences. Empirically, this equivalence typically breaks down. A prevalent explanation for the non-equivalence concerns asymmetric opportunity costs due to the different organization of the two formats. We analyze whether the empirical breakdown of strategic equivalence is due to non-standard preferences or due to the complexity of the two formats holding opportunity costs constant. In a first experiment, we measure risk and loss attitudes as well as Allais-type preferences. In a second experiment, we assess the predictive power of the elicited individual preferences in the two auctions. Further, we manipulate the complexity by varying the degree of decision support.

We find that strategic equivalence only breaks down in the absence of decision support. Once we provide information about the winning probability associated with one's bid, we find no statistical difference between the two formats. This indicates that complexity is more important than preferences in explaining the non-equivalence. In a third treatment, we additionally provide the expected profit of a given bid which does not alter this finding. We thus rule out probability weighting as an explanation. We assess the predictive accuracy of the non-standard preference specifications by various goodness-of-fit measures and find that expectations-based reference dependence with linear utility best explains observed bids. Furthermore, we find no significant correlation between observed bids and individual risk attitudes.

Chapter 5 entitled "*The Effect of Payoff Equality on Equilibrium Selection*" is joint work with Christoph Feldhaus and Bettina Rockenbach and analyzes the role of payoff equality as a criterion for equilibrium selection. We focus on coordination games with Pareto-rankable equilibria. Specifically, we analyze behavior in a minimum-effort game. The most-efficient equilibrium in this game is Pareto-dominant while the least-efficient one is "secure" in the sense that individual payoffs are independent of others' actions. Empirically, subjects frequently converge towards the secure equilibrium despite the presence of an achievable social optimum. We introduce unequal equilibrium payoffs into the minimum-effort game and ensure that either the Pareto-dominant or the secure equilibrium is the unique equilibrium featuring equal payoffs. We propose a social-preference model based on inequality aversion and predict that subjects select the equilibrium with equal payoffs. We test this prediction utilizing a laboratory experiment.

Our results show that payoff equality indeed serves as an important factor in equilibrium selection. Groups generally coordinate less successfully when the secure equilibrium is the one with equal payoffs. We test the robustness of this result by increasing the degree of strategic uncertainty which further amplifies the deterioration effect if the secure equilibrium has equal payoffs. In contrast, the coordination success if the Pareto-dominant equilibrium has equal payoffs is not affected by the change in strategic uncertainty.

1.2 Co-Authors and Acknowledgments

Special thanks go to Florian Gössl for helping me tame the beast called LaTeX.

Chapter 2 is published as Doerrenberg, Duncan, and Zeppenfeld (2015).[1] We gratefully acknowledge Financial support from "KölnAlumni - Freunde und Förderer der Universität zu Köln e.V." and the German Research Foundation (DFG). We would like to thank the editors of the Journal of Economic Behavior and Organization, two anonymous referees, Peter Kuhn, Alexander Klos, Max Loeffler, Andreas Peichl, Alexander Rasch, Bettina Rockenbach, Justin Ross, Christian Weyand, and participants at the

[1]**Doerrenberg**: ZEW Mannheim and IZA Bonn. **Duncan**: School of Public and Environmental Affairs (SPEA), Indiana University, and IZA Bonn.

2013 Economic Science Association World Conference, the 2013 National Tax Association conference, the 2014 IIPF conference, Colby College's department of economics seminar series, SPEA's summer seminar series, and the ZEW seminar for helpful comments and suggestions.

Chapter 3 is based on Pagel and Zeppenfeld (2015).[2] Financial support from the Institute of Business and Economic Research (IBER), University of California at Berkeley, and the German Research Foundation (DFG) is gratefully acknowledged. We would like to thank David Gill, Philippe Gillen, Vitali Gretschko, Ron Harstad, Matthew Rabin, Ulrike Malmendier, Bettina Rockenbach, Muriel Niederle, Nava Ashraf, Dirk Sliwka, Achim Wambach, and Arne Weiss for helpful comments and suggestions. We also thank seminar participants at the University of Cologne and the University of California at Berkeley, and conference participants of the Economic Science Association 2012 North America Meeting and the Experimental Finance Conference 2013.

Chapter 4 is based on Fugger, Gillen, Rasch, and Zeppenfeld (2015).[3] We gratefully acknowledge financial support from the German Research Foundation (DFG) through the research unit "Design & Behavior". We would like to thank Botond Köszegi, David Kusterer, Thomas Lauer, Bettina Rockenbach, Achim Wambach, and Arne Weiss for helpful comments and suggestions. We thank Christoph Groß-Bölting and Sebastian Schneiders for valuable research assistance.

Chapter 5 is based on Feldhaus, Rockenbach, and Zeppenfeld (2015).[4] Financial support from the German Research Foundation (DFG) through the research unit "Design & Behavior" is gratefully acknowledged. We would like to thank Nicolas Fugger, Vitali Gretschko, Johannes Mans, Andreas Pollak, Alexander Rasch, and Achim Wambach for helpful comments and suggestions.

[2]**Pagel**: Columbia University, Graduate School of Business.

[3]**Fugger and Gillen**: University of Cologne, Department of Economics. **Rasch**: Duesseldorf Institute for Competition Economics (DICE), University of Duesseldorf.

[4]**Feldhaus and Rockenbach**: University of Cologne, Department of Economics.

Chapter 2

CIRCUMSTANTIAL RISK: IMPACT OF FUTURE TAX-EVASION AND LABOR-SUPPLY OPPORTUNITIES ON RISK EXPOSURE

2.1 Introduction

Although approximately 94% of households in the United States hold some type of financial asset, there is significant variation in the type and amount of financial assets held (Bricker, Kennickell, Moore, and Sabelhaus 2012). For example, 15% of families hold stocks, 50% hold retirement accounts, 8.7% hold pooled investment accounts and 92.5% hold transaction accounts.[1] Financial asset holdings also vary with individual and household characteristics. While 24.5% of households headed by self-employed individuals have stocks in their portfolios, only 13.8% of households headed by employed individuals do. Additionally, the trends described by Bricker et al. (2012) show evidence of significant variation in risk exposure. For example, the median amount of money invested in bonds by an employed household is approximately eight times as much as that invested in stocks; the comparable ratio for self-employed households is five. An extensive literature in finance and economics has been devoted to explaining these observed variations in risk exposure. Two questions that have received a lot of attention are (Heaton and Lucas 2000): (i) how do investors decide how much of their income to invest in risky assets, and (ii) why do some individuals have greater risk exposure than others?

The theoretical finance literature has provided many insights to these questions. For example, Heaton and Lucas (2000) point out that if risk decisions are driven by utility-maximizing behavior then the drivers of risk exposure can be separated into two broad categories: preferences and 'circumstances'. It is clearly the case that some individuals are more risk-averse than others and that this variation in risk preference affects the amount of risk to which individuals voluntarily expose themselves. Circumstances generally refer to the future opportunities individuals know they will have access to at the time of making risky investment decisions. These circumstances usually have two fea-

[1] These statistics are taken from the Survey of Consumer Finances. Bricker et al. (2012) provides a detail summary of the results including definitions of the various financial assets. Transactions accounts include checking, savings, and money market deposit accounts; money market mutual funds; and call or cash accounts at brokerages. Retirement accounts include personally established individual retirement accounts (IRAs) or job-based 401(k) accounts. Pooled investment funds exclude money market mutual funds and indirectly held mutual funds and include all other types of directly held pooled investment funds, such as traditional open-end and closed-end mutual funds, real estate investment trusts, and hedge funds.

tures - *flexibility* and *background risks* - that may have opposing effects on risk exposure. Flexibility acts as a type of insurance against adverse outcomes and is therefore predicted to increase risk exposure (Bodie, Merton, and Samuelson 1992; Franke, Schlesinger, and Stapleton 2011). For example, the ability to vary labor hours or take an additional job may be used as insurance against negative investment outcomes. The opportunity to adjust future labor supply in response to the investment outcome is advantageous and therefore increases current risk exposure. On the other hand, future opportunities with risky and uninsurable income represent background risk. This future risk may cause the current risk exposure of individuals to increase, decrease or remain unchanged depending on the form of risk aversion (Gollier and Zeckhauser 2002). As a result, the impact of background risk on current risk exposure remains an empirical question. Furthermore, because future circumstances generally include both flexibility and background risk, which may have opposing effects on risk exposure, the impact of future circumstance on current risk exposure is ultimately an empirical question. Understanding the impact of the interaction of these two characteristics is important, but challenging to determine in existing theoretical models without additional assumptions.

Empirical efforts to identify the impact of circumstances produce inconclusive results. For example, Benitez-Silva (2002) tests the theory in the context of labor-supply flexibility and finds that individuals who have flexible work hours tend to hold significantly more (risky) stocks. Although this result is consistent with the theoretical work of Bodie, Merton, and Samuelson (1992), there remains some identification concerns due to self-selection. To the extent that individuals with greater labor-supply flexibility also have greater preference for risk exposure, it is not clear that the identified effect is due to labor-supply flexibility or risk preferences.[2] The empirical efforts to identify the impact of background risk is more extensive, but the results are mixed. Although a number of studies find that the presence of background risk reduces risk exposure, the magnitude of the effect varies (Heaton and Lucas 2000; Klos 2004). More importantly, the results from the existing literature suggest that the impact of background risk depends on the source of background risk: labor-income risk seems to reduce risk exposure while investment income risk seems to have little or no effect on risk exposure.

We contribute to this literature by identifying whether circumstances such as access to future labor-income and tax-evasion opportunities affect current risk exposure. In particular, our research question is: Does the opportunity to earn additional labor income

[2]To our knowledge, Benitez-Silva (2002) is the only paper to study the link between labor-supply flexibility and portfolio choice empirically. Gneezy and Potters (1997) study a different type of flexibility in a portfolio-choice experiment. Subjects in one group make their investment decisions for each period separately and receive feedback after each investment. Subjects in the other group make investment decisions for multiple periods simultaneously, and they only receive feedback for each block of investment instead of period-by-period evaluations. Their results show that the first group, which has greater rebalancing flexibility, expose their wealth to lower risk. Gneezy, Kapteyn, and Potters (2003) find similar results in an investment market.

and/or evade taxation affect risky-asset investment? By studying these two circumstances that feature both flexibility and background risk in the same setting, we are able to cleanly identify the impact of both circumstances as well as their interaction on risk exposure. We are also able to determine if the effects depend on the type of circumstance.

As indicated above, answering these questions with observational data leads to identification problems that are very difficult to overcome. Namely, it can be presumed that individuals with a high intrinsic willingness to take risks self-select into occupations with greater access to tax evasion and additional labor supply opportunities such as self-employment (Cramer et al. 2002; Hartog, Ferrer-i Carbonell, and Jonker 2002). As a result, a positive empirical correlation between self-employment and risk willingness is difficult to interpret in a causal manner, and is instead likely to be confounded by intrinsic, personality-based risk willingness. For clean identification, one would ideally like to randomly assign labor-supply and tax-evasion opportunities to different workers. Because this is not feasible in the real world, we design a laboratory experiment where each subject participates in a labor task and then makes an investment decision. Subjects are then given an opportunity to respond to the outcome of the investment before paying taxes. The opportunity to respond to the outcome of the investment depends on the group to which subjects are randomly assigned: some subjects can evade taxes; some can supply extra labor; some can both evade taxes and supply extra labor; and some can neither evade taxes nor supply extra labor.

Our results show that future labor-supply and tax-evasion opportunities have different effects on risk-taking. The baseline group, which lacks opportunities for additional labor supply or tax evasion, invests 38% of their gross income into the risky asset. Relative to the baseline group, access to extra labor opportunities reduces the investment share by approximately eleven percentage points, evasion opportunity by itself has no effect on investment shares, and access to both labor and evasion reduces the investment share by 3.5 percentage points. The large drop in the labor treatment is both economically and statistically significant, while the effect of evasion is not distinguishable from zero. These results confirm that certain circumstances matter for risk exposure; labor opportunities affect risk-taking while evasion does not. We run two additional treatments that allow us to identify the relative importance of the two features of circumstances: flexibility and background risk. In these additional treatments, we eliminate any potential variability in labor income and, hence, isolate the effect of flexibility from that of background risk. The results from these treatments indicate that the large negative treatment effect of future labor income is driven by background risk rather than flexibility. We find that the flexibility effect is positive.

The findings of our paper are in line with the theoretical and empirical literature on labor-income background risk and flexibility. There is a negative relationship between uninsurable future risks and current risk exposure and a positive relationship between flexibility and current risk exposure (Bodie, Merton, and Samuelson 1992; Heaton and Lucas 2000; Gollier 2001; Gollier and Zeckhauser 2002; Franke, Schlesinger, and Stapleton 2011). More importantly, our findings show that the effect of labor-income background risk is larger than that of flexibility; the estimated flexibility and background-risk effects are 3.4 and 14.4 percentage points, respectively. Our findings also suggest that the relative importance of background risk and flexibility depends on the type of circumstance. While the background-risk effect is larger than the flexibility effect in the labor treatment, the two effects appear to be of the same magnitude but different sign (or both zero) in the evasion treatment. The zero tax-evasion effect is in line with Klos (2004) who finds that current risk exposure is not affected by the presence of future risky investment opportunities.

In addition to identifying and decomposing the effect of circumstances on risk exposure, our paper makes two further contributions. First, this is the only study we are aware of to examine the effect of tax evasion on current risk exposure.[3] Although the decision to evade taxes is similar to that of other risky decisions, it may have a different effect due to cognitive and moral biases on the part of the investor. Furthermore, it is widely accepted that access to tax-evasion opportunities is heterogeneously distributed across individuals. For example, employees who are subject to third-party reporting have less opportunity to evade taxes than their counterparts who are not (Kleven et al. 2011). This difference in access to tax evasion has been shown to have an effect on other economic outcomes that are of interest to policy makers. For example, there is evidence that tax evasion has both income distributional implications (Alm and Sennoga 2010) and welfare implications (Chetty 2009; Gorodnichenko, Martinez-Vazquez, and Sabirianova Peter 2009). Our study contributes to this growing literature by identifying the impact of tax-evasion opportunities on risk exposure, which is itself an important economic variable for policy makers.

Second, we are also the first to identify the impact of future labor-supply opportunities on current risk exposure in a laboratory setting.[4] Existing studies of both labor flexibility and labor-income background risk use observational data, which faces identification issues. Using a laboratory setting allows us to cleanly identify the impact of future labor-supply opportunities on current risk exposure. Our results also provide some insights into the possible effect of the current incremental shift toward greater flexibility in the labor market - flexi-week work schedules and work-from-home initiatives - on

[3]Wrede (1995) theoretically analyzes the joint problem of risk-taking and tax evasion in a static economy. He, however, focuses on the effect of taxation on expected utility and does not explicitly study the dynamic effect of evasion opportunities on initial risk behavior. He concludes that eliminating evasion is likely to discourage risk-taking.

[4]Unlike Klos and Weber (2006) who analyze a portfolio-choice setting with exogenous income that was either certain or risky, we explore a case where labor-income risk is endogenous.

risk exposure. We find evidence that the added flexibility may increase risk exposure of affected workers. However, risk exposure is likely to decrease if the added flexibility is accompanied by greater income background risk. Additionally, we explicitly account for any possible interaction effects between labor-supply and evasion opportunities in our empirical design. Understanding this interaction effect is especially important given that labor flexibility is often bundled with tax-evasion opportunities.

The paper proceeds as follows. Section 2.2 presents the experimental design and the four main treatments that explores the impact of circumstances. We briefly discuss theoretical considerations in Section 2.3 and present results in Section 2.4. Section 2.5 discusses our results including the findings from two additional treatments that allow us to distinguish between flexibility and background risk. Section 2.6 concludes the paper.

2.2 The Laboratory Experiment

Considering the empirical challenges and the impracticality of the ideal field experiment, we employ a laboratory experiment to study the effect of circumstances on risky investment behavior. The experimental design used to answer our research question is based on widely accepted experimental designs in the fields of risk behavior (Gneezy and Potters 1997; Thaler et al. 1997; Klos, Weber, and Weber 2005), tax evasion (Alm 2012) and labor supply (Charness and Kuhn 2011). Although the laboratory environment is artificial, we argue that a clean and clear experimental design, such as ours, allows for causal identification of treatment effects.

2.2.1 Experimental Design

We design a one-shot experiment with between-subject variation to answer our research question. The experiment has three stages. First, subjects complete a real-effort task for which they earn experimental currency units (ECU). Second, subjects decide how much of their labor earnings to invest in a risky asset. Finally, subjects are given an opportunity to respond to the outcome of the investment before paying taxes on their income.

The first two stages are identical for all subjects and we solely vary how subjects can respond to the lottery outcome in the third stage. Depending on which of four treatment groups a subject is assigned to, she either has (i) no opportunity to respond, or she has the opportunity to respond through (ii) additional labor effort, (iii) a tax-reporting decision, or (iv) both extra labor effort and tax reporting. Hence, we cross two dichotomous factors, “evasion opportunity” and “labor-supply opportunity”, in a 2x2 fully-factorial design. The following section describes each of the three experimental stages in greater detail and highlights our identification strategy.

Labor-Task Stage. Every subject first completes a labor task that involves moving a set of sliders across a computer screen Gill and Prowse (2012).[5] The sliders are initially positioned at zero and can be repositioned to any integer between 0 and 100, inclusive. Subjects are given two and a half minutes (150 seconds) to align 48 sliders at position 50. Subjects receive instant feedback on the position of the current slider; this is indicated at the rightmost end of each slider. We disable the arrow keys on the key board to ensure the subjects only use the left mouse key to complete the task; use of the arrow keys makes the task trivial. Additionally "... no two sliders are aligned exactly one under the other". This design feature prevents subjects from positioning one slider at 50 and then visually matching the other sliders at this position. Subjects are paid an exogenously determined piece rate, which is fixed at 2.5 ECU for each correctly aligned slider. We used an exchange rate of 5 ECU to 1 EUR. Therefore, each subject earned 0.5 EUR for each correctly aligned slider.

The slider task has a number of advantages that are described in Gill and Prowse (2012). It is easy to explain and implement, does not require prior knowledge, does not allow guessing, and most importantly, is identical across treatments and subjects. Although the number of correctly aligned sliders has been used as a measure of labor effort in the labor literature, our primary objective here is to induce a sense of ownership of income and the possibility to consider labor income as a potential background risk. We argue that participants are more likely to make reasonable and "realistic" decisions in a situation with endogenous incomes, relative to a situation with an exogenous endowment. Furthermore, individual variability in outcomes suggests that labor income may be viewed as uncertain. Subjects perform one full round of the slider task as an unincentivized practice round to familiarize themselves with the task.

Investment Stage. Subjects are given an opportunity to invest a share of their labor earnings in a lottery after completing the labor task.[6] They are allowed to invest any amount between zero and their total labor earnings in the lottery. The lottery is binary and the amount invested in the lottery is either doubled or halved with equal probability. The actual outcome of the lottery is determined by the throw of a ten-sided die. One of the experimenters walks up to each subject's booth after the investment decision is complete and throws a ten-sided die. The amount invested in the lottery is doubled if

[5] See Figure 2.A.3 in the appendix for a screenshot of the task. Gill and Prowse (2013a) provide details and show how to implement the slider task. It has been used widely since its introduction: Gill and Prowse (2013b), Riener and Wiederhold (2011), Cettolin and Riedl (2011), Gill and Prowse (2012), and Hammermann, Mohnen, and Nieken (2012). Djawadi and Fahr (2012) also use the slider task in the context of tax compliance, but examine a different research question than we do and employ compliance as the dependent variable.

[6] The risky asset was framed as an investment opportunity to subjects in the experiment. However, we use *investment* and *lottery* interchangeably in the remainder of the paper.

the number on the face of the die is less than or equal to five, and halved if the number on the face of the die is greater than or equal to six. The experimenter enters the number on the face of the die on the computer and the subject verifies that the number is correct before hitting enter. The computer then reports the outcome of the lottery to the subject along with her post-lottery income.[7]

The lottery is designed such that the expected pay-off is greater than the invested amount, i.e., the expected return is positive, which implies that a risk-averse person invests a strictly positive share of his wealth into the lottery. The binary structure and the conditional investment outcomes are easy to grasp and calculate. However, we also provided a computerized calculator at every decision stage.

Response to the Lottery Outcome (Treatment Groups). The next stage of the experiment gives subjects an opportunity to respond to the outcome of the lottery. Recall that our research objective is to determine whether an individual's risk exposure today is affected by future opportunities to respond to the outcome of risky decisions via tax-evasion and/or labor-supply opportunities. Therefore, we randomly assign subjects to four groups that are identical in every way except in how they can respond to the outcome of the lottery. Following the 2x2 crossing, a subject's ability to respond to the outcome of the lottery depends on which of the following groups she is randomly assigned to:

- **Baseline:** Subjects in the *baseline* group do not have an opportunity to respond to the outcome of the lottery. After the lottery outcome is realized, their total income is taxed at a proportional rate of 30 percent, and they are simply informed of their final payoff (their net income). The tax rate is fixed exogenously, and is the same for all subjects and all groups. Therefore, subjects in the baseline treatment *cannot respond* to the lottery. This group serves as the control treatment.

- **Labor**: After the outcome of the lottery is determined, subjects in the *labor* treatment play another period (90 seconds) of the real-effort task and thereby earn additional income. After this second period of supplying labor effort, their total income is taxed and subjects are informed of their final payoff (their net income). Therefore, subjects in the labor treatment can respond to the lottery *only* through a *second labor-supply* choice.

- **Evasion:** In the *evasion* treatment, subjects have to make a tax reporting decision. After they learn the outcome of the lottery, participants are asked how much income they wish to report for tax purposes. As is standard in the experimental tax-evasion literature, there is an exogenous probability that the tax-evasion decision is audited,

[7]Note that the experimenter could at no point see the amount invested by the subject as the feedback screen was only shown after the experimenter had moved on and the subject clicked the "next" button.

and a penalty applies in the case of audit *and* underreporting. After the reporting decision has been made, the audit status is determined and the payoff (net income) is paid accordingly. Therefore, subjects in the evasion treatment can respond to the lottery *only* through their *reporting decision.*

- **Full:** Subjects in the *full* treatment group have two channels to respond to the lottery outcome. They play another period of the real effort task (90 seconds), as in the labor group, and then make an income reporting decision, as in the evasion group. Their final payoff (net income) is a function of the two periods of labor supply, the investment decision, the lottery outcome, the reporting decision, and the audit status. Therefore, subjects in this fourth treatment group have access to two channels to respond to the lottery outcome: *evasion and labor supply.*

Subjects are only informed of the set-up of the experiment in their treatment state, and they receive this information prior to the initial labor-supply and investment decisions (in the paper based instructions). Therefore, subjects know ex-ante whether and how they are able to respond to the lottery outcome, and that they can only respond ex-post. The design described above allows us to identify the impact of future labor or evasion opportunities on current risk exposure. We run two additional treatments that vary the structure of the second labor task by eliminating any variability in payoffs from this task. Hence, future labor income in these treatments is certain. This allows us to disentangle the relative importance of flexibility and background risk in our findings. These additional treatments are described in Section 2.5.1.

Tax and Audit Mechanism. As indicated above, all subjects face an exogenously determined proportional marginal tax rate of 30 percent.[8] Additionally, subjects in the evasion and full treatments face an exogenous audit probability of 10 percent and a penalty that is equal to twice any evaded taxes due (i.e., a fine rate of 2). The audit outcome is determined by the throw of a 10-sided die; subjects are audited if the number one is shown on the face of the die.[9] Audit leads to the discovery of true income, and subjects who underreport their income pay a penalty equal to twice their evaded taxes

[8]Because subjects in the baseline and the labor treatment do not make an income reporting decision, they pay 30 percent on their *total* income in taxes. On the other hand, subjects in the evasion and full treatment groups pay a tax rate of 30 percent on their *reported* income because they are able to respond to the lottery outcome by underreporting income.

[9]The procedure was as follows: After the reporting decision was complete for the evasion and full treatment groups, one of the experimenters walked up to each computer booth and threw a 10-sided die; the experimenter entered the number on the face of the die on the computer; the subject confirmed this number, which results in a screen with one of the following sentences: *You have been audited* or *You have not been audited.* The subject had to press “NEXT” again to see the screen summarizing the round’s payment. By this time the experimenter had already moved on to the next subject. Hence, the experimenter could at no point see subjects’ reported share.

(i.e., the underreported amount multiplied by twice the tax rate). All other subjects who either report honestly or underreport but throw a die number between two and ten receive a net income equal to their true gross income less the tax rate multiplied by the reported income. This audit and penalty structure is commonly used in the tax-evasion literature (Alm, Jackson, and McKee 2009). In order to make the tax-reporting decision as realistic as possible, we include an exogenous audit risk and donate all tax revenues and fines to the administrative governing body of the City of Cologne, Germany.[10]

We argue that this is a clean experimental design to answer the research question posed above: a one-shot game in a between-subject design that produces 180 independent observations (45 per treatment) on a metric dependent variable. We therefore have sufficient statistical power to answer our research question. All participants face the same labor effort, investment decision, and marginal tax rates, and therefore only differ with respect to the channel that is available to respond to the lottery outcome ex-post. This allows us to compare the share of income invested in the lottery across the four groups and attribute any differences to the difference in response opportunities. Additionally, since subjects know ex-ante whether and how they are able to respond to the lottery outcome, and that they can only respond ex-post, any treatment effect we identify must be driven purely by anticipation of the response opportunity. Methodologically, this is achieved by changing only that part of the instructions governing second-period opportunities; the first period is identical across instructions.

2.2.2 Organization

The experiment was conducted in the Cologne Laboratory for Economic Research (CLER), University of Cologne, Germany (`www.lab.uni-koeln.de`). A random sample of the laboratory's subject pool of approximately 4000 persons was invited via email – using the recruitment software *ORSEE* (Greiner 2004) – to participate in the experiment. Potential participants signed up on a first-come-first-serve basis. A total of 180 subjects, mostly undergraduate students from the University of Cologne, participated in the experiment (see Section 2.4.1 for summary statistics). Neither the content of the experiment nor the expected payoff were stated in the invitation email. The computerized experiment was programmed utilizing *z-tree* software (Fischbacher 2007).

We conducted twelve sessions over three regular school days in May and June 2013.[11] Each session included one practice round, one paying round, 15 subjects, and lasted approximately 40 minutes on average (including review of instructions and payment of

[10]Subjects received a copy of the donation receipt (stating the total amount donated) via e-mail after the experiment has been conducted. This procedure was also explicitly stated in the instructions.

[11]There are two regular semesters at the tertiary level in Germany; winter semester lasting from October to March and Summer Semester between April and July. Therefore, the experiment was implemented during the regular lecture season.

participants). The exchange rate between Experimental Currency Units (ECU) and Euro was such that five ECU corresponded to one Euro. Random assignment to computer booths was implemented by asking each subject to draw an ID number out of a box upon entering the lab. The decisions and payments of the subjects were linked to their ID and the experimenter had no way of matching this information to their names. Subjects also received a hard copy of the instructions when they entered the lab (See Appendix 2.C) and were allowed as much time as they needed to familiarize themselves with the procedure of the experiment. They were then given the opportunity to ask any clarifying questions in private.

2.3 Theoretical Background

In this section, we discuss the conditions under which the opportunity to react to the investment outcome in the future affects the current exposure to investment risks. Investors have direct utility function u which is strictly increasing and strictly concave in wealth z. Effort causes increasing and convex costs c. Investors are risk-averse and maximize the expected utility of terminal wealth z_T given disutility of effort, i.e., working.

In the baseline treatment, the investor has initial endowment w_0 and can work to gain additional income. Subsequently, he decides on his portfolio composition, i.e., how much of his total period-two wealth to invest in a risk-free storage and how much to invest in a risky asset. The risk-free rate is zero. The risky asset has a random return $\tilde{r}$ distributed according to the cumulative distribution function $F(r)$ on $\mathbb{R}$ with $E_F\tilde{r} > 0$.

In subsequent periods, the investor learns the realization of $\tilde{r}$. Then, depending on the treatment, he either has different opportunities to increase his wealth or not. Final gross wealth is subject to taxation at rate $0 < \tau < 1$. To increase his wealth, the investor can either work again for additional income, or he can evade taxes by underreporting his true final wealth, or both.

2.3.1 Portfolio Choice without Adjustment Opportunities

We begin by characterizing the investor's optimal action when he has no adjustment opportunities after the investment stage (the baseline treatment). Hence, the investor first generates labor income by choosing his effort level and subsequently decides on how much of his wealth he wants to invest. The remainder is automatically stored safely. After the realization of investment risks, the investor's gross income is automatically taxed.

Note that labor income in our experiment is not predictable with certainty. Given the real-effort task utilized in the experiment, it is difficult for the subjects to perfectly anticipate their productivity and hence their labor income. As we will show later, subjects exhibit a large individual variability in their performance. This limits their ability to forecast to what extent they will be able to offset potential losses. We capture this income risk by allowing the investor to tremble, i.e., we add some shock, $\tilde{\eta}_t \sim G(\eta_t)$, to his effort choice in periods with a working decision $t = \{1, 3\}$.[12] This productivity shock will be of special importance when we discuss those treatments that offer a second labor decision.

In the baseline treatment, the investor faces the following objective in period two:

$$\begin{aligned} v^{NoAdjust}(z_2) &= \max_a E_F u((w_0 + wl_1 + w\eta_1 - a)(1-\tau) + a(1+\tilde{r})(1-\tau)) \\ &= \max_a E_F u(z_2(1-\tau) + a(1-\tau)\tilde{r}), \end{aligned} \tag{2.3.1}$$

where $w > 0$ is the wage rate per unit of work, l_t is the number of units worked in period t, $a \geq 0$ is the absolute amount invested in the risky asset, and $\eta_t \in \mathbb{R}$ is the realization of the productivity shock in period t. We write $z_2 = w_0 + wl_1 + w\eta$ for the wealth at the beginning of period two.

It is well known that an investor invests a positive fraction of his wealth in the risky asset due the positive expectation of excess returns (e.g., Gollier 2001). Call the solution to the portfolio-choice problem without adjustment opportunities $a_{NoAdjust}$.

In the first period, the investor chooses an effort level given his optimal investment decision in period two. The investor's first-period objective reads:

$$\max_{l_1} E_G v^{NoAdjust}(w_0 + w(l_1 + \tilde{\eta}_1)). \tag{2.3.2}$$

We assume that the idiosyncratic productivity shocks are present in all treatments and orthogonal to the experimental manipulation. Hence, we argue that the randomization of subjects into treatments ensures that the average treatment effect is not affected by the noise in first-period labor. Additionally, we assume that first-period labor effort is independent of experimental manipulation. Indeed, in our main experiment, labor choice is not statistically different across treatments. We therefore take the first-period labor decision as given and, in the following, focus on how the investment level a changes across treatments.

We now turn to the conditions under which adjustment opportunities in subsequent periods lead to increased or decreased risk-taking. That is, the conditions under which adjustment opportunities increase or decrease a. For this, it is sufficient to show that these opportunities reduce (increase) the investor's risk aversion, or increase (reduce) his risk tolerance, respectively, in the portfolio problem. More specifically, the investor's objective function in the portfolio problem with adjustment opportunities has to be less (more) concave than his objective function u without adjustment opportunities in the sense of Arrow (1965) and Pratt (1964).

[12] In the experiment, this shock causes the number of correctly positioned sliders to be different from the initially chosen number of sliders.

2.3.2 Portfolio Choice with Adjustment Opportunities

Consider an investor who faces the same portfolio choice as in the baseline problem but who additionally knows that he will have an adjustment opportunity in the subsequent period. The derivation of his optimal investment results from backward induction. In period three, the investor may now respond to the current realization of wealth, i.e., to $z_3 = w_0 + wl_1 + w\eta_1 + ar$. Consequently, the investor decides on units of work, l_3 or the amount of wealth evaded, e, i.e., the amount of non-reported income, or both.

Labor-Supply Opportunity

We first examine how the opportunity to adjust labor supply in the third period affects risk-taking. The opportunity to supply additional labor represents the ability to respond to the risky investment outcome by additional effort. For each unit of work, l_3, the investor receives wage w but also suffers costs $c(l_3)$.[13]

As in the first period, current labor supply is subject to productivity shocks. Future income with positive variance may serve as an insurance against adverse current investment. The investor may base his labor supply on the realization of the investment risk. Flexibility refers to this deliberate adjustment. However, the additional variability in wealth can also be seen as a background risk relative to the current investment risk (Gollier 2001). Hence, it depends on the shape of risk aversion and risk tolerance, respectively, whether first-period risk-taking is actually increased in the presence of adjustment opportunities or whether the investor chooses to be less exposed to current investment risk.

With a labor-supply opportunity, the investor has the following value function in period three:

$$v^{Labor}(z_3) = \max_{l_3} E_G u(z_3(1-\tau) + w(l_3 + \tilde{\eta_2})(1-\tau)) - c(l_3). \tag{2.3.3}$$

The portfolio-choice problem in period two is given by

$$\max_a E_F v^{Labor}(z_2 + a\tilde{r}). \tag{2.3.4}$$

Call the solution to the portfolio choice with labor-supply opportunity a_{Labor}. The difference between the baseline portfolio choice without adjustment opportunities and the current portfolio choice is that the investor now maximizes a different objective function. He will invest more in the risky asset, i.e., $a_{Labor} > a_{NoAdjust}$, if the value function v^{Labor}, which he maximizes in (2.3.4), is less concave than the direct utility function u which he maximizes in (2.3.1). On the other hand, if the new value function is more concave than u, the investor will reduce his risk exposure, i.e., $a_{Labor} < a_{NoAdjust}$.

[13] We assume that the cost function is the same across periods.

Recall that we model the productivity shock by adding a risky non-market component to the overall income.[14] Variable non-market wealth has two effects. First, if the minimum income is bounded away from zero and the variance of this income is sufficiently small, investors should invest more in the risky asset (Franke, Schlesinger, and Stapleton 2011), i.e., $a_{Labor} > a_{NoAdjust}$. This is one likely scenario for the performance in the real-effort task. The amount of labor income available from second-period performance varies but can be considered non-negative. However, Franke, Schlesinger, and Stapleton (2011) also note that this result may not hold if the variance of income is sufficiently large.

Second, consider an investor having problems generating income in the labor task. He will not only generate very low income but he will also have highly convex costs in doing so. Combined, this may yield negative overall utility as marginal costs exceed marginal earnings. Hence, it is possible that for some investors the labor task may actually bear the risk of achieving negative utility though it has a positive expectation in general. Franke, Schlesinger, and Stapleton (2011) show that in such cases risk aversion is range dependent. The more likely a bad performance is, the more risk-averse the investor becomes as he tries to avoid negative utility.

Evasion Opportunity

We now examine how the opportunity to evade taxes in the subsequent period affects the investor's exposure to investment risks. The opportunity to evade taxes represents the ability to respond to the risky investment outcome by taking an additional exogenous risk. As such, this adjustment opportunity again features a deliberate reaction to previous investment outcomes (flexibility) but also increases variability in terminal wealth (background risk). We write the tax evasion decision as a second portfolio choice in the spirit of Allingham and Sandmo (1972). This allows us to apply the results of Gollier (2001) and Gollier and Zeckhauser (2002) to characterize the conditions under which an additional subsequent risk induces more or less contemporaneous risk-taking.

One unit of evaded taxes yields a random return $\tilde{\epsilon}$ distributed according to $(p, -\zeta; 1-p, 0)$, i.e., the tax-reporting decision is audited with probability $p > 0$ and the fine rate is $\zeta > 0$. Starting in the third period, the investor has the following value function

$$v^{Evasion}(z_3) = \max_{e} E_P u(z_3(1-\tau) + e\tilde{\xi}), \tag{2.3.5}$$

where $\tilde{\xi} \sim P(\xi) = (p, \tau - \zeta; 1-p, \tau)$ is the excess return per unit of evaded taxes and $E_P\tilde{\xi} > 0$.

[14] The productivity shock is a random component of wealth that does neither depend on the investor's choices nor on the investment return realization.

By backward induction, in period two, the investor now chooses the amount invested in the risky asset that solves the following objective

$$\max_a E_F v^{Evasion}(z_2 + a\tilde{r}). \tag{2.3.6}$$

Call the solution to the portfolio-choice problem with tax-evasion opportunity $a_{Evasion}$. Again, the investor will invest more in the risky asset, i.e., $a_{Evasion} > a_{NoAdjust}$ if his value function $v^{Evasion}$ is less concave than his direct utility function u and vice versa.

Gollier and Zeckhauser (2002) show that the opportunity to take an additional risk ex-post leads to more risk-taking ex-ante if and only if absolute risk tolerance is convex. Their proposition applies if the second risk has a binary support which holds for our assumption on the auditing mechanism. Note that it is sufficient to assume that risk aversion is concave for a tax-evasion opportunity to induce more risk-taking.[15]

Whether risk tolerance is actually convex or concave is an empirical question. Gollier and Zeckhauser (2002) list several empirical observations leading to arguments for either form. Hence, it remains an empirical question whether a subsequent risky decision leads to more initial risk-taking. Note that the standard assumption in macroeconomics and finance postulates a linear risk tolerance. Given linearity, a future investment risk does not affect the optimal exposure to initial investment risks. Hence, myopic investment decisions are optimal (Mossin 1968).

2.3.3 Labor-Supply and Evasion Opportunities

In the full treatment, subjects have both the opportunity to supply additional labor as well as to evade taxes. Note that the labor-opportunity and evasion-opportunity treatments are nested in the full treatment. Subjects are not forced to evade taxes. Hence, they can reduce the full treatment to the labor treatment. Analogously, subjects are not forced to provide additional real effort. Hence, they can reduce the full treatment to the evasion treatment. Ultimately, all treatments can, of course, be reduced to the baseline treatment.

The theoretical results discussed above suggest that the effect of 'circumstances' on risk exposure is ambiguous. This ambiguity extends to the case with both evasion and labor supply opportunities. We therefore do not write down the full four-period model formally. The analysis via backward induction proceeds in the same fashion as before and there is nothing new that provides an additional understanding of the portfolio choice. The last-period value function is plugged into the third-period objective which itself yields

[15] Concave risk aversion implies convex risk tolerance and hence increased risk-taking. However, Gollier and Zeckhauser (2002) state the proposition in terms of risk tolerance as this is more useful because they note that risk aversion cannot be decreasing, concave, and positive everywhere. As Gollier (2001) notes, the convexity of risk tolerance is rather a restriction on risk aversion not to be too concave.

a value function (v^{Full}) that the agent maximizes in period two by choosing a_{Full}. Again, whether the investor changes his investment exposure relative to the baseline treatment ($a_{Full} \gtreqless a_{NoAdjust}$) depends on the curvature of v^{Full} in relation to the curvature of u. Therefore, we rely on an empirical analysis to identify the effects of ex-post opportunities on risk exposure. The results from this analysis are reported in the next section.

2.4 Results

This section describes our results for the effect of circumstances on risk exposure. We first present summary statistics (Section 2.4.1) and simple non-parametric comparisons of the four treatment groups (2.4.2), before we proceed to regression results in Section 2.4.3. The results are discussed in Section 2.5 where we present two additional treatments and pay special attention to the relative importance of flexibility and background risk.

2.4.1 Summary Statistics

Table 2.1 presents summary statistics for demographic and attitudinal variables which were surveyed through a questionnaire at the end of the experiment. The demographic variables include age, gender, and native language. Because our experiment involves making investment and evasion decisions, we ask one question on risk aversion and one on tax morale.[16]

The summary statistics reported in Table 2.1 show that males and native German speakers make up 42% and 79% of the sample, respectively, and that the average age is 24.4 years. Randomization into treatment groups worked well as the variables are fairly balanced across treatment groups. Since the share of males is about 9 percentage points lower in the baseline treatment relative to the average share, we provide regression results that control for demographic variables to ensure that our results are not driven by differences in gender or other individual characteristics.

[16]The measure of risk aversion is obtained by asking subjects to choose between a certain pay-off of \$50 and a gamble that pays \$100 with probability of 0.5 and \$0 with probability of 0.5. The tax-morale question is adopted from the World Values Survey (Inglehart n.d.). “Please tell me for the following statement whether you think it can always be justified, never be justified, or something in between: ‘Cheating on taxes if you have the chance’.” This is the most frequently used question to measure tax morale in observational studies (e.g., Slemrod 2003, Alm and Torgler 2006, and Halla 2012).

Table 2.1: Summary Statistics by Treatment Status: Demographic and Attitudinal Variables.

	Baseline	Treatments			
		Labor	Evasion	Full	Total
Age	25.22	23.22	24.47	24.64	24.39
	(6.842)	(2.779)	(3.546)	(7.371)	(5.512)
Male	0.333	0.422	0.467	0.489	0.428
	(0.477)	(0.499)	(0.505)	(0.506)	(0.496)
German	0.711	0.756	0.889	0.800	0.789
	(0.458)	(0.435)	(0.318)	(0.405)	(0.409)
Tax Morale	7.711	7.289	6.689	6.800	7.122
	(2.427)	(2.873)	(2.670)	(3.005)	(2.760)
Risk	1.200	1.244	1.311	1.222	1.244
	(0.505)	(0.609)	(0.668)	(0.599)	(0.594)

Notes: Reported is the mean of demographic and attitudinal variables by treatment status. Standard deviations in parentheses. $N = 180$. Subjects in the *Labor* and *Evasion* treatments had the opportunity to supply extra effort and evade taxes, respectively. *Full* treatment indicates that both labor and evasion adjustments were available. Subjects in the *Baseline* group had no adjustment opportunities. All information were surveyed through a questionnaire at the end of the experiment.

2.4.2 Non-parametric Comparisons of Treatment Groups

Table 2.2 presents summary statistics for the choice variables: Labor effort, investment decision, and compliance behavior. In the following, we compare the treatment groups with respect to these variables.

Effort Decisions. On average, over all treatment groups, roughly 18 sliders were correctly positioned in the first payoff-relevant period. Table 2.2 shows that labor effort is fairly balanced across the four treatment groups. Though slightly higher in the labor treatment relative to the other groups, this difference in correctly positioned sliders in the first period is not statistically significant (Mann-Whitney test relative to baseline group: $p = 0.491$). The slight difference in period-two effort between the two groups that have the opportunity to respond to the lottery outcome by working is not significant either.

Compliance Behavior. On average, subjects in the two groups with tax evasion opportunities reported 22.5% of their income for tax purposes. However, there are large differences between the two groups. While subjects in the *evasion* treatment reported, on average, 14% of their income, subjects in the *full* treatment group were more honest and reported almost 31% of their income. This 17 percentage points difference is statistically

Table 2.2: Summary Statistics by Treatment Status: Choice Variables.

	Baseline	Treatments			
		Labor	Evasion	Full	Total
Effort Practice	13.31	13.51	12.69	13.47	13.24
	(4.547)	(5.968)	(4.828)	(4.998)	(5.081)
Effort Period 1	17.78	18.44	17.51	17.96	17.92
	(4.680)	(4.429)	(4.841)	(4.472)	(4.583)
Investment Share	0.381	0.271	0.375	0.346	0.343
	(0.297)	(0.206)	(0.267)	(0.279)	(0.266)
Effort Period 2	.	12.20	.	11.47	11.83
		(3.259)		(2.951)	(3.113)
Compliance Rate	.	.	0.141	0.309	0.225
			(0.248)	(0.401)	(0.342)

Notes: Reported is the mean of choice variables by treatment status. Standard deviations in parentheses. $N = 180$. Subjects in the *Labor* and *Evasion* treatments had the opportunity to supply extra effort and evade taxes, respectively. *Full* treatment indicates that both labor and evasion adjustments were available. Subjects in the *Baseline* group had no adjustment opportunities.

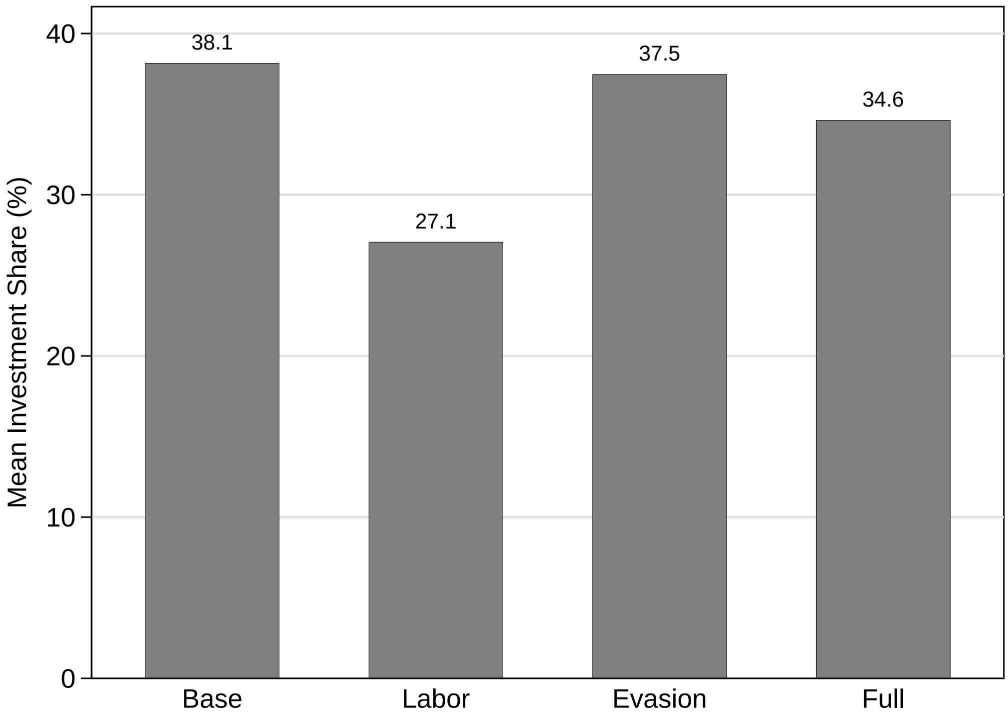
different from zero ($p = 0.0871$), and raises the possibility of large interaction effects which we explore below.

Investment Decisions. Our main variable of interest is the share of period-one income invested in the lottery, which we simply refer to as investment share. This variable is presented in the third row of Table 2.2 as well as in Figure 2.1. We observe that the share of period-one income that is invested in the lottery is 38.1% in the baseline, 27.1% in the *labor* treatment, 37.5% in the *evasion* treatment, and 34.6% in the *full* treatment. The difference in investment shares between the baseline and evasion treatments is neither economically nor statistically different from zero (Mann-Whitney test: $p = 0.878$). On the other hand, we observe a large treatment effect in the *labor* treatment; the investment share is eleven percentage points lower relative to the baseline. This treatment effect is both economically large and statistical different from zero ($p = 0.066$). Although the investment share in the *full* treatment is 3.5 percentage points lower than the baseline, this difference is not statistically different from zero ($p = 0.660$). However, this treatment effect indicates that the effect of labor opportunity on investment share is substantially smaller among subjects who also have an evasion opportunity. In other words, access to evasion reduces the responsiveness of subjects to the labor income circumstance.[17] This

[17]The interaction effect of evasion fits perfectly with the narrative of the public finance literature. It is generally accepted that *tax-shifting* responses such as tax evasion reduces the magnitude of *real* responses such as investment (Slemrod 1994; Doerrenberg and Duncan 2014).

interaction effect of 8.1%, which is economically meaningful but not statistically different from zero ($p = 0.297$), is estimated using the following formula: $(\hat{a}_{Full} - \hat{a}_{Labor}) - (\hat{a}_{Evasion} - \hat{a}_{Base}) = (0.346 - 0.271) - (0.375 - 0.381)$.[18]

Our results therefore suggest that the opportunity to provide extra labor effort after learning the outcome of the lottery (ex-post) reduces subjects' willingness to take on investment risk (ex-ante).[19] This effect is almost fully countered if these subjects also have an opportunity to evade taxes. Curiously, subjects whose only response to the investment outcome is to evade taxes are no more or less risky than subjects without response opportunities. The fact that the investment decisions of subjects in the baseline and evasion groups are close to each other (given that the choice variable is metric between 0 and 1) shows that the significant difference between the labor and baseline group is very high and reflects a causal treatment effect.



Reported is the share of period-one labor income that is invested in the lottery by treatment status. $N = 180$. Subjects in *Labor* and *Evasion* treatments had the opportunity to supply extra effort and evade taxes, respectively. *Full* treatment indicates that both labor and evasion adjustments were available. Subjects in the *Baseline* group had no adjustment opportunities.

Figure 2.1: Investment Share by Treatment Group.

[18]This interaction effect can also be estimated directly in a parametric OLS regression of investment share on indicators for evasion, labor as well as an interaction between these two indicators.

[19]Note that the investment share does not depend on period-one labor income (Spearman's rank correlation: $p = 0.6192$, $N = 180$).

2.4.3 Regression Results

This section describes parametric results based on the following regression model:

$$a_{ig} = \psi + \beta \cdot Treat_g + \phi \cdot \mathbf{X}_{ig} + \epsilon_{ig}, \tag{2.4.1}$$

where subscripts indicate a subject i who is in treatment group $g = 0, 1, 2, 3$ (with $g = 0$: baseline, $g = 1$: labor treatment, $g = 2$: evasion treatment, $g = 3$: full treatment). The dependent variable a_{ig} is the share of contemporaneous income that is invested in the lottery. $Treat_g$ is a categorical indicator variable for a subject's treatment group[20] and β is the coefficient of interest. The coefficients for each treatment group is relative to the omitted baseline group with $g = 0$. Some specifications also control for demographic and attitudinal variables that are captured in $\mathbf{X}_{ig}$. The error term is ϵ_{ig}; we use heteroscedasticity-robust standard errors.

Specification (I) of Table 2.3 shows OLS estimates of the effect of treatment status on the share of invested income. The estimated treatment effects confirm our non-parametric analysis. The investment share of subjects who have the opportunity to provide additional labor effort is 10 to 11 percentage points lower than the baseline group. This effect is large and statistically significant at the 5% level. On the other hand, the investment share of subjects in the evasion and full treatment groups is not statistically different from that of the baseline group. Again, the results from the full treatment points to large differential effects between subjects with only labor opportunity and subjects with both labor and evasion opportunities. These results are robust to the inclusion of demographic and attitudinal variables in specification (II); age, gender, a dummy indicating German as the native language, and the questionnaire answers to the risk and tax-morality questions. The results are also robust to estimating two-censored Tobit regressions (specifications (III) and (IV)), which account for the fact that subjects were restricted by borrowing constraints, i.e., they could only invest their total income but not more, and the fact that they could not short the lottery.

As expected, males and subjects who are characterized as risk-seeking invest a larger share of income in the lottery. These estimates are in line with the risk literature and therefore support our claim that the lottery in the experiment captures risk behavior well. For example, Charness and Gneezy (2012) find that males are more risk-seeking than women in laboratory experiments. We also find that older subjects invest a larger share of income in the lottery. Although this estimate is statistically different from zero, it is economically small.[21]

---

[20]Recall that we employ a between-subjects design where each subject is exclusively in one of the four treatment groups.

[21]For details on the measurement of risk and tax morale see footnote 16. The coefficients for all demographic and attitudinal variables are reported in Table 2.B.4 in the Appendix.

Table 2.3: Treatment Effects on Investment Behavior: OLS and Tobit Regressions.

Model	(I)	(II)	(III)	(IV)
Estimation	OLS	OLS	Tobit	Tobit
Labor	-0.111**	-0.102**	-0.128**	-0.114**
	(0.054)	(0.051)	(0.063)	(0.057)
Evasion	-0.007	-0.018	-0.016	-0.027
	(0.060)	(0.061)	(0.071)	(0.070)
Full	-0.035	-0.041	-0.058	-0.062
	(0.061)	(0.058)	(0.073)	(0.069)
Constant	0.381***	0.261**	0.387***	0.213
	(0.044)	(0.118)	(0.053)	(0.144)
Controls	No	Yes	No	Yes
N	180	180	180	180
R2	0.028	0.176	0.030	0.219

Notes: OLS and two-censored (at 0 and 1) Tobit regressions based on equation (2.4.1). Dependent variable is the share of contemporaneous income invested in the lottery. Treatment effects are relative to the omitted *Baseline* group without adjustment opportunities. Subjects in the *Labor* and *Evasion* treatments had the opportunity to supply extra effort and evade taxes, respectively. *Full* treatment indicates that both labor and evasion adjustments were available. Robust standard errors in parentheses. Estimations (II) and (IV) include a full set of control variables (see Table 2.B.4 for detailed results). $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

2.5 Discussion

Our empirical analysis confirms that certain circumstances matter for risk exposure. In particular, future labor-supply opportunities reduce risk exposure while access to only tax evasion does not. This section of the paper explores our findings more carefully to determine the channels through which future labor opportunities affect current risk exposure. We also discuss the internal validity of our design.

2.5.1 Disentangling the Effect of Circumstances

The existing theoretical literature suggests that the impact of circumstances such as labor opportunity on risk exposure is driven by two channels: flexibility and background risk. While flexibility induces greater risk exposure, background risks may cause current risk exposure to increase, decrease or remain unchanged depending on the shape of individual risk tolerance as discussed in Section 2.3. We highlight the relative importance of these channels in two steps. We first hypothesize about the relative impact of each channel and then provide empirical evidence that disentangles the two channels based on additional experimental treatments.

Flexibility vs. Background Risk

The future labor opportunity offers both a means of flexibly responding to the outcome of the lottery and a source of background risk. Flexibility stems from the ability to offset potential adverse return realizations with additional labor supply. Background risk stems from the additional variability in terminal wealth. Although subjects in the labor treatment knew ex-ante that they had an opportunity to supply labor after learning the outcome of the lottery, the income associated with this labor task was difficult to predict for two reasons. First, the difficulty of the slider task implies that subjects could only predict their labor income with large errors. Second, subjects had 150 seconds to complete both the practice and first labor task and 90 seconds to complete the second labor task. We argue that this 40% difference in labor time, made it even more difficult for subjects to accurately predict their future labor income based on the income earned in the first labor task. The inability to accurately predict future labor income along with the disutility of completing the labor task suggest that the future labor opportunity acted as both a source of flexibility and labor-income background risk. Because we find that subjects in the labor treatment invest a lower share of income in the lottery, our findings suggest that the background-risk effect dominates the flexibility effect.

Similarly, the opportunity to evade taxes also acts as a source of flexibility and background risk. In other words, individuals in the evasion treatment who realize an adverse lottery outcome have the opportunity to underreport income for tax purposes. This lowers their tax payment, increases their net income and thus allows them to recoup some of the income lost in the lottery. At the same time, the probability of being caught and the fine associated with evasion implies a source of background risk as well. Since the treatment effect is practically zero in the evasion treatment, this suggests that the two channels are of the same magnitude, but of different sign (or both zero).

While it is useful to draw inference about the relative role of each channel from existing theory, we also determine the relative role of each channel empirically by running two additional treatments. Because we find large labor treatment effects and zero evasion treatment effect, our empirical analysis focuses on disentangling the role of the two channels in the labor treatments only. These additional treatments and results are described in the next sections.

Additional Empirical Evidence

Design of Additional Treatments. In order to identify the relative impact of flexibility, we ran the *labor* and *full* treatments without the presence of future labor income risk. Both treatments are designed to remove background risk from the future labor income. The only difference between these treatments and their respective counterparts described in Section 2.2 is the structure of the second labor task, which we describe below. In fact, the only difference in the instructions is the boldfaced portions of the following paragraph.

> The investment outcome will be determined and displayed on your computer screen. You will then undertake the same labor task as in stage 1. However, you will not receive an additional fixed amount this time. ~~**In addition, this second labor task lasts 90 seconds.**~~ **In addition, this second labor task has no time constraint. Instead, before the start of the second labor task, you choose the number of sliders that you want to position correctly. The maximum number of sliders you can choose is 15. The slider task ends automatically once you have reached the chosen number of correctly positioned sliders.** The money that you earn in this labor task will be added to the money that you have earned so far.

Notice that instead of giving subjects 90 seconds to complete as many sliders as they intend to in the second labor task, the new treatments allow subjects to preselect the number of sliders they wish to complete in the second labor task. Subjects are allowed to select up to 15 sliders and have as much time as needed to complete the selected number of sliders. Although subjects make this labor-effort decision after learning the outcome of their investment decision, they know that the they will be able to make this decision and that they will have as much time as needed to complete their selected number of sliders before they make their investment decision. This new design of the second labor task is applied to the *labor* and *full* treatments. We henceforth refer to these treatments as *Labor NR* and *Full NR* (where NR indicates no background risk).

Theoretically, Bodie, Merton, and Samuelson (1992) and Franke, Schlesinger, and Stapleton (2011) show that under non-stochastic positive income, investors increase their risky investment. Intuitively, a certain way to increase income ex-post serves as a kind of insurance towards adverse investment outcomes. The investor knows that he will be able to offset potential losses with certainty. Non-stochastic income substitutes the risk-free storage and the investor invests a larger fraction of his wealth in the risky asset. We can model this non-risky labor income by setting $\tilde{\eta} \equiv 0$ in the respective second labor decisions in period three, e.g., in equation (2.3.3). Theoretically, this modification unambiguously increases the exposure to investment risks.

Our additional treatments allow us to identify the relative importance of flexibility and background risk by removing the variability in future labor income. We identify the role of labor-income flexibility by comparing the additional labor treatment to the baseline, and the role of background risk by comparing the additional labor treatment to the original labor treatment. We also identify the importance of labor-income flexibility and background risk for the evasion interaction effect by comparing the additional labor treatment to the additional full treatment.

In order to successfully identify these relative effects, it is important that the only difference between the original and additional treatments is background risk. We ensure comparability of the number of completed sliders between the original and additional treatments by setting the maximum number of sliders each subject can preselect to 15, which is one standard deviation higher than the mean completed sliders in the original labor and full treatments, respectively.[22]

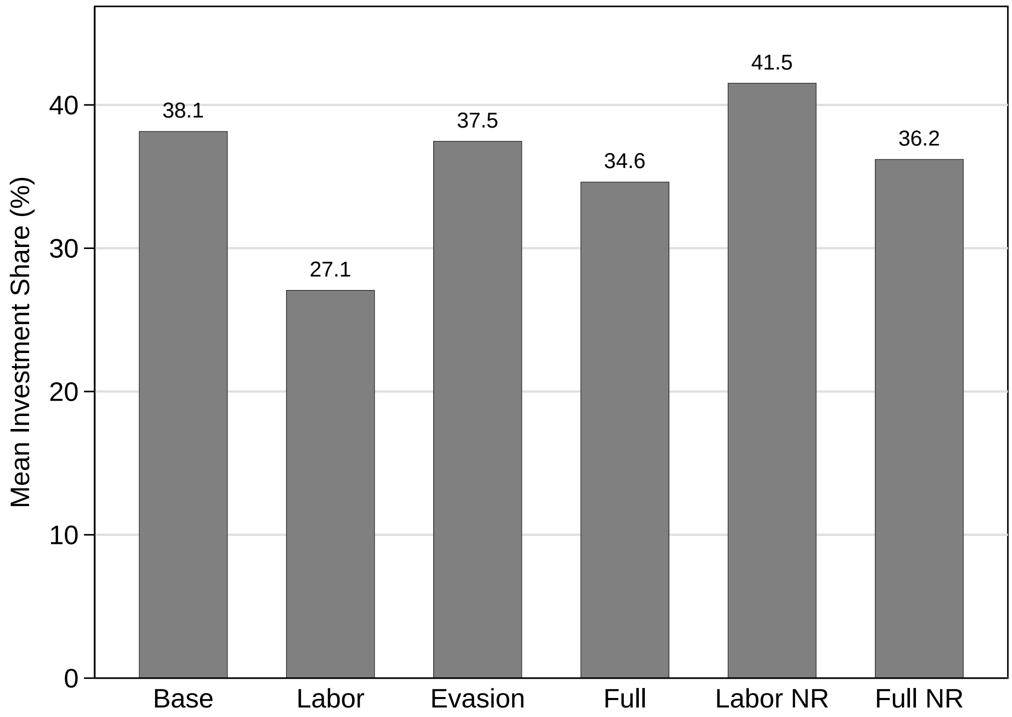
We ran two sessions of each treatment with 15 subjects per session for a total of 30 independent observations per additional treatment and 60 new observations in total. The sessions were conducted in the same lab drawing from the same subject pool (not the same subjects) as the original experiments. All other details are identical to those described in Section 2.2.

Results of Additional Treatments. The results presented in Figure 2.2 below show that the investment share is 41.5% (sd: 22.8%) in Labor NR and 36.2% (sd: 29.4%) in Full NR. This implies that removing the risk component of future labor income has an effect on investment shares in the labor treatment but not in the full treatment.[23] The investment share in the Labor-NR treatment is 3.4 percentage points higher than in the baseline and 15 percentage points higher than in the original labor treatment. Since there is no background risk in the additional labor treatment, we argue that the 3.4 percentage points difference from the baseline is the flexibility effect; access to future certain labor income increases risky investment today. Although this difference is in the expected direction, it is not statistically different from zero (Mann-Whitney test: $p = 0.24$). The 15 percentage points difference between the additional and original labor treatments reflects background risk and is both statistically and economically meaningful ($p = 0.002$). These results suggest that the background risk effect dominates the flexibility effect in the original labor treatment and thus explains the significantly smaller investment share in the original labor treatment.

Investment shares are two percentage points higher in the additional full treatment (without labor-income background risk) relative to the original full treatment (with labor-income background risk). Although this difference is not statistically distinguishable from

[22] The means and standard deviations were 12.20 and 3.26, and 11.47 and 2.95, in the labor and full treatments, respectively. We argue that the design we implement for the additional treatments removes background risk while maintaining enough similarities to the original treatments, which is critical for comparing the results.

[23] The additional and original treatments are similar in demographic characteristics. The new treatments completed approximately 3 fewer sliders, on average, in the first labor task than the old treatments. However, we find no evidence that this difference has any effect on the results that are discussed in this section.



Notes: Reported is the share of period-one labor income that is invested in the lottery by treatment status. $N = 240$. Subjects in *Labor* and *Evasion* treatments had the opportunity to supply extra effort and evade taxes, respectively. *Full* treatment indicates that both labor and evasion adjustments were available. *Labor NR* and *Full NR* are identical to the original *Labor* and *Full* treatments except for the second labor effort task. Subjects in *Labor NR* and *Full NR* preselect the number of sliders they wish to complete and have as much time as needed to complete the selected number of sliders while subjects in *Labor* and *Full* treatments have 90 seconds to position sliders. Subjects in the *Baseline* group had no adjustment opportunities.

Figure 2.2: Investment Share by Treatment Group (All Treatments).

zero, it provides suggestive evidence of a negative labor-income background-risk effect. Furthermore, the treatment effect in the additional full treatment, relative to the baseline, confirms that the effect of labor is conditional on having access to tax evasion. In other words, whereas subjects with only certain future labor income increase their investment share by 3.4 percentage points, subjects who have both labor and evasion opportunity reduce their investment share by 2 percentage points. This implies a negative interaction effect of -4.7 percentage points ($= (0.362 - 0.415) - (0.375 - 0.381)$). That is, if future labor income is certain, subjects with both evasion and future labor income invest 4.7 percentage points less than subjects who only have access to future labor income. Notice that this interaction effect is sizably different from the one obtained in Section 2.4.2; 8.1 in original treatments and -4.7 in additional treatments. This difference in estimates can be attributed to background risk and further supports our conclusion in Section 2.5.1 of a dominant negative background risk effect. Although the estimated interaction effects differ in sign and magnitude, they tell the same story; tax-shifting responses such as tax evasion reduce the magnitude of real responses such as investment.

2.5.2 Further Discussion

Overall, our findings show that the type of circumstances matter for risk exposure; future labor income is associated with a very strong negative background-risk effect, while future evasion opportunity has no effect. While we cannot comment on the relative importance of flexibility and background risk in explaining the tax-evasion results, we find evidence that future labor income has a large negative background-risk effect and a relatively small positive flexibility effect. Our finding that future labor income affects current risk exposure while evasion does not is consistent with existing empirical evidence. For example, our evasion result is similar to that of Klos (2004) who finds that having a future investment opportunity has no effect on current risk exposure. Similarly, the labor supply result is consistent with Gakidis (1997) who finds that individuals with unpredictable future labor income have lower risk exposure, and Benitez-Silva (2002) who finds that labor-income flexibility increases risk exposure.

A seemingly alternative explanation for our results in the original labor treatment is income targeting. If subjects begin the experiment with a certain target income in mind, then having an extra labor task would allow them to achieve that target with lower investment risk. However, this explanation is inconsistent with the findings in the additional labor treatment where background risk is removed from the future labor income. Income targeting would imply an even lower level of risk exposure when background risk is removed. Instead, we find that risk exposure increases. If income targeting was the explanation, risk exposure would also be lower in the full treatment, relative to the baseline, because subjects in the full treatment have access to the same labor opportunity as subjects in the labor treatment. However, risk exposure in the full treatments is not statistically different from that of the baseline group.

An interesting question raised by our results, is why does the *source* of circumstances matter? The existing theoretical literature seems to be mute on this question, and we do not explore it further here.[24]

2.5.3 Internal Validity

The results described above are based on data generated in a one-shot experiment using a between-subject design and six randomly determined groups that are identical except for

[24] Although we find this question interesting, addressing it requires additional design features that take us away from the central research question of this paper. We therefore view this question as somewhat outside the scope of the specific objective of this study, and believe it is best pursued in future work.

treatment status. Descriptive statistics of observable characteristics show that the groups are balanced along observables. This suggests that randomization into groups worked, which is crucial for identification in between-subject designed experiments (Charness, Gneezy, and Kuhn 2012). Additionally, the experimental instructions for the different treatment groups were identical in every aspect except for the opportunity to respond to the lottery. Since investment decisions in all groups are made before subjects proceed to the adjustment stage, any differences in the outcome variable of interest, risk exposure, between groups are only driven by the knowledge about and anticipation of future (ex-post) adjustment possibilities. The number of participants in the experiment is relatively large and we are able to rely on 30 to 45 independent observations per group because there is no interaction among participants. We find no evidence that the experimental design choices and parameterization induced extreme behaviors that conflict with standard theoretical modeling approaches. For example, the lottery outcomes are not clustered on extremes (0 or 1), which suggests that the resulting distribution of investment shares supports the theoretical modeling of an interior solution. Furthermore, labor supply is strictly positive but not statistically different across treatments. Additionally, the labor task yielded sufficient variation to support the idea of labor-income background risk.

2.6 Conclusion

While the literature acknowledges that circumstances are likely to affect investment outcomes, there is little empirical evidence on these relationships. Using a laboratory experiment, we examine if individuals who have the opportunity to (ex-post) respond to lottery outcomes through evasion and/or labor supply show different (ex-ante) risk exposure than individuals without any response opportunities. The experimental results show that circumstances matter, but different circumstances can have different effects. While the opportunity to earn extra labor income affects risk-taking, there is no evidence that access to evasion alone has an effect, at least not in a statistically significant way. However, we find evidence of fairly large interaction effects. Relative to subjects with only future labor income opportunities, subjects with both labor-income and evasion opportunities take more risks if labor income is accompanied by background risk and less risk if labor income is certain.

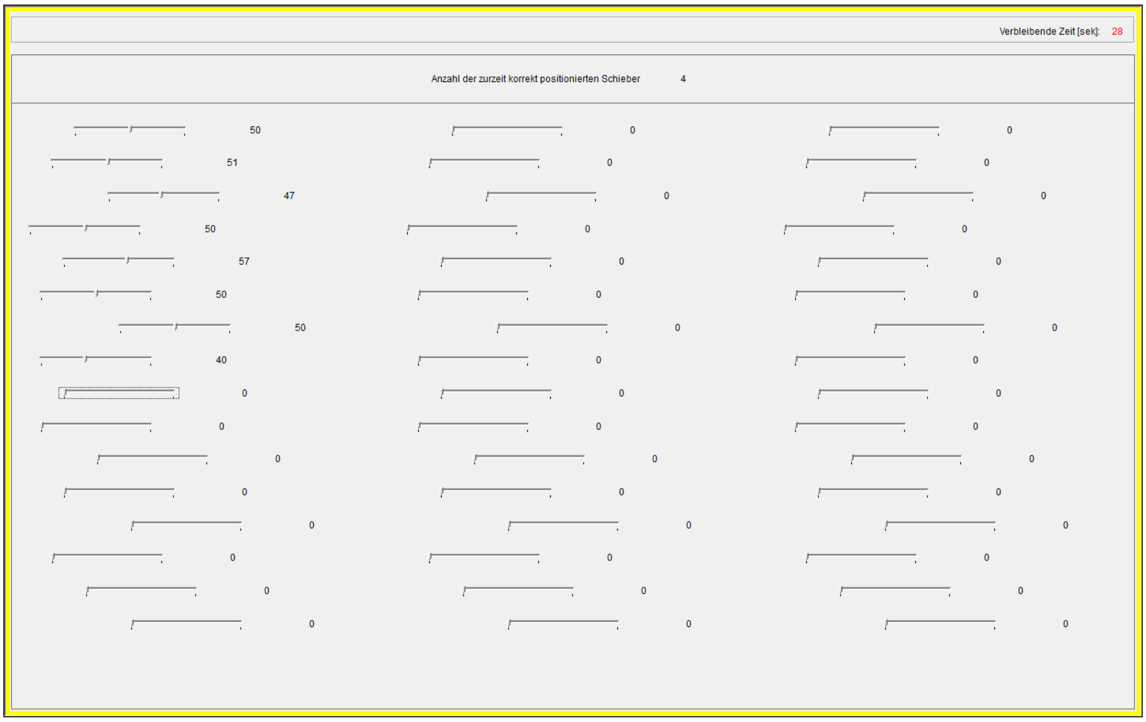
We identify the channels behind the circumstantial labor effect and find that risk-taking increases in the presence of income flexibility and decreases in the presence of background risk. Furthermore, the background-risk effect is much larger than the flexibility effect. This allows us to comment on (Gollier and Zeckhauser, 2002, p. 201) who state: "The critical question is when can the flexibility effect be assured to overcome (be weaker than) a potential negative background risk effect." In our setting, we conclude that the flexibility effect is not strong enough to overcome a negative background-risk effect.

These results contribute one possible answer to questions relating to the lower-than-expected stock market exposure of individuals. We find that heterogeneous access to additional labor income, tax-evasion opportunities and their interaction play a non-trivial role in determining risk exposure. In addition to contributing to the finance literature on portfolio choice, the paper also speaks to the literatures on tax evasion and labor economics. While heterogeneous access to tax evasion has been shown to affect labor-supply elasticities with respect to taxes, income distribution and social welfare, we know of no other study that examines the effect of tax evasion on current risk exposure.

Our results also provide insights into the possible effect of the current gradual shift towards greater flexibility in labor markets – e.g., flexi-week work schedules and work-from-home initiatives – on risk exposure. We find that risk exposure is likely to decrease if the added flexibility is accompanied by greater income background risk. Additionally, we explicitly account for any possible interaction effects between labor-supply and evasion opportunities in our empirical design. Understanding these interaction effects is especially important given that labor-income flexibility and background risk are often bundled with tax-evasion opportunities (e.g., among the self-employed). We are also able to speak to the possible implications of the increased scrutiny of offshore accounts aimed at reducing tax-evasion opportunities among high-income individuals. Our results suggest that risk-taking is not hampered by such a change in tax-compliance policy. Therefore, this change may lead to higher tax revenues without reducing risk-taking in society.

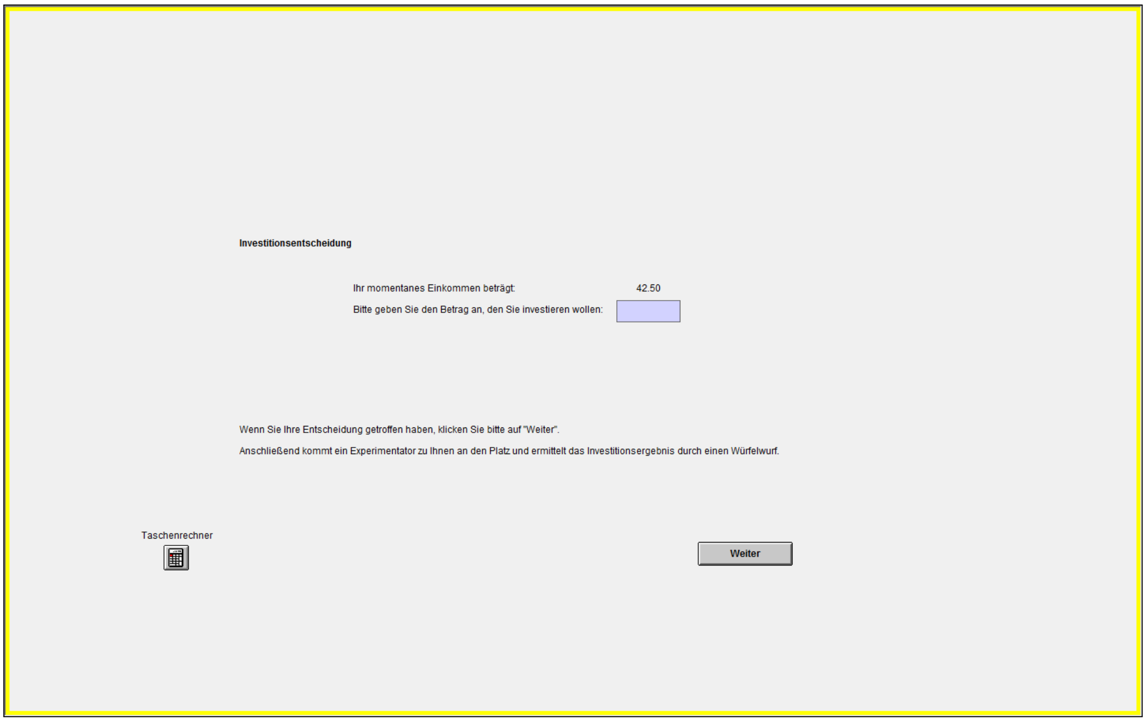
Appendices

2.A Screens in the Lab Experiment



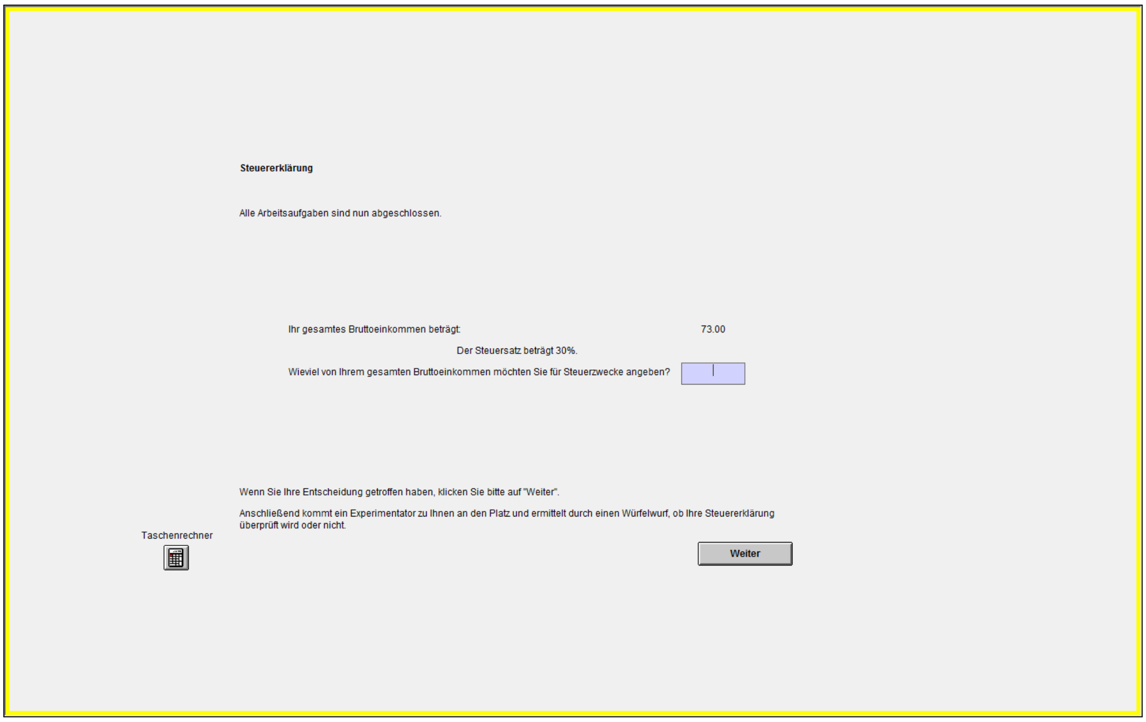
Notes: Screen showing the slider task which was designed by Gill and Prowse (2012). In the displayed screen, the subject positioned four sliders correctly and four falsely. She currently works on positioning the ninth slider. 28 seconds are left in this round.

Figure 2.A.3: Slider Task.



Notes: Subjects decide how much of their labor earnings they want to invest in the risky asset. Choices are confirmed by clicking “Next”.

Figure 2.A.4: Investment Decision.



Notes: Subjects decide how much of their gross income they want to report for tax purposes. Choices are confirmed by clicking “Next”.

Figure 2.A.5: Evasion Decision.

2.B Detailed Regression Results

Table 2.B.4: Treatment Effects on Investment Behavior: OLS and Tobit Regressions.

Model	(I)	(II)	(III)	(IV)
Estimation	OLS	OLS	Tobit	Tobit
Labor	-0.111**	-0.102**	-0.128**	-0.114**
	(0.054)	(0.051)	(0.063)	(0.057)
Evasion	-0.007	-0.018	-0.016	-0.027
	(0.060)	(0.061)	(0.071)	(0.070)
Full	-0.035	-0.041	-0.058	-0.062
	(0.061)	(0.058)	(0.073)	(0.069)
Age		0.009***		0.012***
		(0.003)		(0.004)
Male		0.098**		0.108**
		(0.040)		(0.047)
German		-0.004		-0.012
		(0.042)		(0.046)
Tax Morale		0.004		0.004
		(0.007)		(0.008)
Risk (indiff.)		0.132		0.154
		(0.087)		(0.097)
Risk (seeking)		0.202***		0.222***
		(0.061)		(0.065)
Constant	0.381***	0.261**	0.387***	0.213
	(0.044)	(0.118)	(0.053)	(0.144)
N	180	180	180	180
R2	0.028	0.176	0.030	0.219

OLS and two-censored (at 0 and 1) Tobit regressions based on equation (2.4.1) for the original treatments. Dependent variable is the share of period-one labor income invested in the lottery. Treatment effects are relative to the omitted control group without adjustment opportunities. Subjects in the *Labor* and *Evasion* treatments had the opportunity to supply extra effort and evade taxes, respectively. *Full* treatment indicates that both labor and evasion adjustments were available. Subjects in the *Baseline* group had no adjustment opportunities. Robust standard errors in parentheses. Estimations (II) and (IV) include control variables. $^{*}< 0.10$, $^{**}< 0.05$, $^{***}< 0.01$.

2.C Original Instructions

The following pages display the instructions in German (original) and English (translated) for the "Full Treatment" Group. The instructions for the other groups are the same but exclude the parts which were not relevant for the respective group (these are available upon request). The two sessions that eliminate any income variability ("Labor No Risk" and "Full No Risk") only change the paragraph regarding the second labor task. These changes are reported in parentheses under the respective section in the instructions.

Instruktionen

Herzlich willkommen und vielen Dank für Ihre Teilnahme an diesem Experiment. Bitte kommunizieren Sie ab sofort und bis zum Ende des Experimentes nicht mehr mit den anderen Teilnehmern. Sollten Sie sich nicht an diese Regel halten, müssen wir Sie von dem Experiment ausschließen.

Wir bitten Sie, die Instruktionen sehr aufmerksam zu lesen. Wenn Sie nach dem Lesen oder während des Experiments noch Fragen haben, heben Sie bitte Ihre Hand. Einer der Experimentleiter wird dann zu Ihnen kommen und Ihre Frage persönlich beantworten. Ihre Auszahlung und Ihre Entscheidungen werden vertraulich behandelt. Keiner der anderen Teilnehmer erfährt während oder nach dem Experiment, welche Entscheidungen Sie getroffen haben oder wie hoch Ihre Auszahlung war.

Sie können in diesem Experiment Geld verdienen. Wie viel Sie verdienen, hängt von Ihren Entscheidungen ab und wird nicht von den Entscheidungen anderer Teilnehmer beeinflusst. Ihre Auszahlungen werden im Laufe des Experimentes in virtuellen Geldeinheiten, den Experimental Currency Units (ECU), angegeben**. 5 ECU entsprechen 1 EUR**. Ihre Auszahlung wird nach dem Ende des Experimentes in Euro umgerechnet und in bar an Sie ausgezahlt. Zusätzlich erhalten Sie für Ihr Erscheinen eine Teilnahmepauschale in Höhe von 2,50 EUR.

Das Experiment

Überblick

Das Experiment besteht aus einer Übungsrunde und einer Auszahlungsrunde. Sie können in der Übungsrunde kein Geld verdienen.

Die Auszahlungsrunde hat vier Phasen:

> Phase 1 (Arbeitsaufgabe 1): Sie erledigen eine Arbeitsaufgabe und erhalten dafür, abhängig von Ihrer Leistung, Geldeinheiten.
>
> Phase 2 (Investitionsentscheidung): Sie können Ihr in Phase 1 verdientes Geld entweder in eine Anlage mit zufälliger Auszahlung investieren oder aufbewahren. Sie entscheiden in dieser zweiten Phase, wie viel Sie investieren möchten.
>
> Phase 3 (Arbeitsaufgabe 2): Sie wiederholen die Arbeitsaufgabe aus Phase 1 und erhalten dafür, abhängig von Ihrer Leistung, zusätzliche Geldeinheiten.
>
> Phase 4 (Steuererklärung): Auf Ihr gesamtes Einkommen aus den ersten drei Phasen fällt eine Steuer an. In dieser vierten Phase des Experiments geben Sie eine Steuererklärung ab.

Die Übungsrunde zu Beginn des Experiments dient dazu, Sie mit der Arbeitsaufgabe vertraut zu machen und umfasst daher nur die Arbeitsaufgabe. Alle Phasen des Experiments werden im Folgenden ausführlicher erklärt.

Phase 1: Arbeitsaufgabe 1

Sie erledigen an Ihrem Bildschirm eine Arbeitsaufgabe mit Hilfe der Computermaus. Die Arbeitsaufgabe dauert 150 Sekunden. Während der Arbeitsaufgabe erscheint ein Bildschirm, auf dem 48 sogenannte „Schieber“ zu sehen sind. Jeder Schieber ist zunächst auf „0“ (Null) positioniert und kann von Ihnen verschoben werden. Sie können den Schieber mit der Computermaus auf jede ganze Zahl zwischen „0“ und „100“ verschieben. Sie können jeden Schieber so oft verschieben, wie Sie möchten. Sie erhalten **2,50 ECU** für jeden Schieber, den Sie innerhalb der 150 Sekunden **exakt** auf der Nummer „**50**“ positionieren. Oben rechts am Bildschirm können Sie während der 150 Sekunden immer ablesen, wie viele Schieber Sie aktuell auf „50“ positioniert haben. Zusätzlich zu Ihrer Bezahlung für die korrekt positionierten Schieber erhalten Sie eine fixe Ausstattung von 2,50 ECU.

Phase 2: Investitionsentscheidung

Sie haben die Wahl, einen Teil Ihres Geldes oder Ihr gesamtes Geld, welches Sie in Phase 1 verdient haben, aufzubewahren oder zu investieren. Das aufbewahrte Geld steht Ihnen unverändert im weiteren Verlauf des Experiments zur Verfügung. Durch Investieren können Sie zusätzliches Geld gewinnen oder einen Teil Ihres Geldes verlieren. Das Geld, das Sie investieren, wird entweder verdoppelt oder halbiert. Beide Ergebnisse sind gleich wahrscheinlich.

Nachdem Sie entschieden haben, wie viel Sie investieren möchten, wird ein Experimentator an Ihren Platz kommen und einen 10-seitigen Würfel mitbringen. Der Experimentator wird an Ihrem Platz auswürfeln, ob Ihr investiertes Geld verdoppelt oder halbiert wird. Zeigt der 10-seitige Würfel eine Zahl zwischen 1 und 5 (also 1, 2, 3, 4 oder 5), dann wird Ihre Investition halbiert. Wird eine Zahl zwischen 6 und 10 geworfen (also 6, 7, 8, 9 oder 10), dann wird Ihre Investition verdoppelt.

Ihr Einkommen, das Ihnen im weiteren Verlauf des Experiments zur Verfügung steht, besteht also aus dem aufbewahrten Betrag zuzüglich des entweder verdoppelten oder halbierten Investitionsbetrags. Somit gilt abhängig vom Würfelergebnis für Ihr Einkommen:

- Würfel zeigt eine Zahl zwischen 1 und 5:

  Einkommen = Aufbewahrt + (0,5 x Investition)

- Würfel zeigt eine Zahl zwischen 6 und 10:

  Einkommen = Aufbewahrt + (2 x Investition)

Phase 3: Arbeitsaufgabe 2

Das Investitionsergebnis wird bestimmt und auf ihrem Bildschirm angezeigt. Sie erledigen danach die gleiche Arbeitsaufgabe wie in Phase 1. Diesmal erhalten Sie jedoch keinen zusätzlichen Fixbetrag. Außerdem dauert diese zweite Arbeitsaufgabe 90 Sekunden. Das Geld, das Sie in dieser Arbeitsaufgabe verdienen, wird zu Ihrem bisherigen Geld hinzuaddiert.

[(*No-Risk Treatments*) Das Investitionsergebnis wird bestimmt und auf ihrem Bildschirm angezeigt. Sie erledigen danach die gleiche Arbeitsaufgabe wie in Phase 1. Diesmal erhalten Sie jedoch keinen zusätzlichen Fixbetrag. Außerdem hat diese zweite Arbeitsaufgabe keine Zeitbeschränkung. Bevor die zweite Arbeitsaufgabe startet, wählen Sie die Anzahl an Schiebern, die Sie korrekt positionieren möchten. Das Maximum, das Sie auswählen können ist 15. Die zweite Arbeitsaufgabe endet automatisch, sobald Sie die von Ihnen ausgewählte Anzahl an Schiebern korrekt positioniert haben. Das Geld, das Sie in dieser Arbeitsaufgabe verdienen, wird zu Ihrem bisherigen Geld hinzuaddiert.]

Phase 4: Steuererklärung

Ihnen wird zunächst Ihr gesamtes aktuelles Einkommen angezeigt. Dieses Einkommen bildet Ihr Bruttoeinkommen, auf das eine Steuer von 30% fällig wird.

Sie sind nun aufgefordert, eine Steuererklärungsentscheidung zu treffen. Dazu benennen Sie einen Betrag, der mit dem Steuersatz von 30% besteuert werden soll. Dieser genannte Betrag kann zwischen Null und der Höhe Ihres gesamten Bruttoeinkommens liegen.

Nachdem Sie Ihre Steuererklärungsentscheidung getroffen haben, warten Sie bitte bis einer der Experimentatoren an Ihren Platz kommt. Der Experimentator wird einen 10-seitigen Würfel mitbringen und an Ihrem Platz werfen.

Basierend auf dem Ergebnis des Würfelwurfs ergibt sich eines der zwei folgenden Szenarien für Ihr Nettoeinkommen, das Ihnen am Ende des Experiments zusammen mit der Teilnahmepauschale ausgezahlt wird:

Szenario a) Der Würfel zeigt eine **Zahl zwischen 2 und 10**:

Es *wird nicht überprüft*, ob Sie in Ihrer Steuererklärungsentscheidung Ihr Bruttoeinkommen vollständig angezeigt haben. Ihre Auszahlung (das Nettoeinkommen) setzt sich in diesem Fall aus dem Bruttoeinkommen abzüglich der Steuerzahlung zusammen. Dabei ist die Steuerzahlung der Betrag, den Sie in der Steuererklärung angegeben haben, multipliziert mit dem Steuersatz von 30%. Also:

→ <u>Nettoeinkommen</u> = Bruttoeinkommen – (angegebener Betrag x 0,30)

Szenario b) Der Würfel zeigt die **Zahl 1**:

Es *wird überprüft*, ob Sie in der Steuererklärung Ihr vollständiges Bruttoeinkommen angegeben haben. Abhängig von Ihrer vorher getroffenen Steuererklärungsentscheidung, gibt es in diesem Fall für Ihr Nettoeinkommen zwei Möglichkeiten:

- Ist der von Ihnen in der Steuererklärung angegebene Betrag *gleich* Ihrem gesamten Bruttoeinkommen, dann setzt sich Ihr Nettoeinkommen aus dem Bruttoeinkommen minus der Steuerverbindlichkeit zusammen. Also:

  → <u>Nettoeinkommen</u> = Bruttoeinkommen – (Bruttoeinkommen x 0,30)

- Ist der von Ihnen in der Steuererklärung angegebene Betrag *niedriger* als Ihr gesamtes Bruttoeinkommen, dann müssen Sie die volle Steuerzahlung basierend auf Ihrem gesamten Bruttoeinkommen zahlen. Außerdem fällt eine Extrazahlung an. Diese Extrazahlung errechnet sich durch den von Ihnen *nicht* angegebenen Betrag multipliziert mit dem Steuersatz von 30%. Also:

  → <u>Nettoeinkommen</u> = Bruttoeinkommen – (Bruttoeinkommen x 0,30)

  – [(Bruttoeinkommen – angegebener Betrag) x 0,30]

Es besteht also eine Wahrscheinlichkeit von 10%, dass Ihre Steuererklärungsentscheidung überprüft wird. Dem Experimentator ist es selbstverständlich nicht möglich, einzusehen, ob Sie Ihr Bruttoeinkommen in voller Höhe angegeben haben oder nicht.

Die von allen Teilnehmern insgesamt geleisteten Steuerzahlungen werden an die **Verwaltung der Stadt Köln** als Spende überwiesen. Ein Nachweis über die Gesamtspende wird Ihnen im Laufe der nächsten Wochen per Email zugeschickt.

Schlussbemerkungen

Am Ende des Experiments bitten wir Sie, einen kurzen Fragebogen auszufüllen während wir die Auszahlungen vorbereiten. Alle dort angegebenen Informationen, sowie alle während dieses Experiments erhobenen Daten, werden selbstverständlich anonymisiert und ausschließlich für wissenschaftliche Zwecke verwendet.

Instructions

Welcome and thank you for participating in our experiment. From now on until the end of the experiment, please refrain from communicating with other participants. If you do not abide by this rule, we will have to exclude you from the experiment.

We kindly ask you to read the instructions thoroughly. If you have any questions after reading the instructions or during the experiment, please raise your hand. One of the instructors will then come to you and answer your question in person. Your payment and your decisions throughout the experiment will be treated confidentially. None of the other participants is informed, neither during nor after the experiment, about your decisions in the experiment or your payment.

You can earn money in this experiment. How much you earn depends on your decisions and is not affected by the decisions of other participants. During the experiment, your payments will be calculated in a virtual currency: Experimental Currency Units (ECU). **5 ECU corresponds to 1 EUR.** After the experiment, your pay-off will be converted to Euro and given to you in cash. Additionally, you will receive a show-up fee of 2.50 EUR.

The Experiment

Overview

The experiment consists of one practice round and one payoff round. You cannot earn money in the practice round.

The payoff round has four stages.

Stage 1 (Labor Task 1): You will complete a labor task and, depending on your performance, earn money from this labor task.

Stage 2 (Investment Decision): You can either invest the money that you earned in stage 1 in an asset with a random payoff or store it. You decide in this second stage, how much you want to invest.

Stage 3 (Labor Task 2): You will repeat the labor task from stage 1 and, depending on your performance, earn additional money from this labor task.

Stage 4 (Tax Declaration): You will have to pay taxes on your total income from the first three stages. In this fourth stage, you have to file a tax declaration.

The practice round at the beginning of the experiment is meant to acquaint yourself with the labor task and, hence, only involves the labor task. All stages of the experiment will be explained in more detail below.

Stage 1: Labor Task 1

You undertake a labor task on the computer screen using the computer mouse. The task will last 150 seconds. During the task a screen with 48 so-called “sliders” appears on the screen. Each slider is initially positioned at “0” (Zero) and can be moved by you. You can move the slider to every integer between “0” and “100” via the computer mouse. You can readjust the position of each slider as many times as you wish. For each slider that you position exactly at the number “50” during the 150 seconds, you earn 2.50 ECU. During the 150 seconds of the labor task, on the upper right of the screen you are shown how many sliders you have currently positioned at “50”. In addition to your result for the correctly positioned sliders, you receive an additional fixed endowment of 2.50 ECU.

Stage 2: Investment Decision

You will be given the choice to store or invest some or all of the money that you earned in stage 1. The money that you store will be available unchanged in the future course of the experiment. By investing, you can gain additional money or lose some of your money. The money that you invest will be either doubled or halved. Both outcomes are equally likely.

After you have decided how much to invest, an experimenter will come to your booth and bring a 10-sided die. At your booth, the experimenter will roll the die to decide if your invested money is doubled or halved. If the 10-sided die shows a number between 1 and 5 (i.e., 1, 2, 3, 4 or 5), then your investment will be halved. If it shows a number between 6 and 10 (i.e., 6, 7, 8, 9 or 10), then your investment will be doubled.

Your income that will be available in the future course of the experiment hence consists of the stored amount plus the, either halved or doubled, investment amount. Hence, depending on the die roll, your income is:

- Die shows a number between 1 and 5:

  Income = Stored + (0.5 x Investment)

- Die shows a number between 6 and 10:

  Income = Stored + (2 x Investment)

Stage 3: Labor Task 2

The investment outcome will be determined and displayed on your computer screen. You will then undertake the same labor task as in stage 1. However, you will not receive an additional fixed amount this time. In addition, this second labor task lasts 90 seconds. The money that you earn in this labor task will be added to the money that you have earned so far.

[(*No-Risk Treatments*) The investment outcome will be determined and displayed on your computer screen. You will then undertake the same labor task as in stage 1. However, you will not receive an additional fixed amount this time. In addition, this second labor task has no time constraint. Instead, before the start of the second labor task, you choose the number of sliders that you want to position correctly. The maximum number of sliders you can choose is 15. The slider task ends automatically once you have reached the chosen number of correctly positioned sliders. The money that you earn in this labor task will be added to the money that you have earned so far.]

Stage 4: Tax Declaration

You will first be shown your full current income. This income is your gross income which is subject to tax of 30%.

You will now be asked to make a tax-declaration decision. To make this decision, you specify an amount which will be taxed at the tax rate of 30%. This reported amount can be between zero and your full gross income.

After you have completed the tax-declaration decision, please wait until one of the experimenters comes up to your booth. The experimenter will bring a 10-sided die and roll it at your booth.

Depending on the outcome of the die roll, you will face one of the following two scenarios regarding your net income which will be paid to you, along with the show-up fee, at the end of the experiment.

Scenario a) The die shows a **number between 2 and 10:**

Your tax-declaration decision *will not be checked* to determine whether you specified your full gross income. Your payment (the net income), in this case, consists of your gross income less the tax payment. The tax payment is the amount that you reported multiplied with the tax rate of 30%. Hence:

→ Net Income = Gross Income – (Reported Amount x 0.30)

Scenario b) The die shows the **number 1**:

Your tax-declaration decision *will be checked* to determine whether you reported your full gross income. Depending on your previous tax-declaration decision, there are two possibilities for your net income:

- If your reported income *equals* your full gross income, then your net income consists of your gross income less your tax liability. Hence:

  → Net Income = Gross Income – (Gross Income x 0.30)

- If your reported income is *lower* than your full gross income, then you will have to pay the full tax liability based on your full gross income. In addition, you have to pay an extra amount. This extra amount is equal to the income that you *did not* report multiplied by the tax rate of 30%. Hence:

  → Net Income = Gross Income – (Gross Income * 0.30)

  – [(Gross Income – Reported Income) x 0.30]

Hence, there is a 10% probability that your tax-declaration decision will be checked. The experimenter who comes up to your booth with the die, of course, cannot see whether or not you reported your full gross income.

The total generated tax payments from all participants will be donated to the **administration of the City of Cologne**. A verification of the total donation will be sent to you via e-mail within the next weeks.

Final Remarks

At the end of the experiment, you will be asked to complete a short questionnaire while we prepare the payments. All information collected through this questionnaire, just like all data gathered during the experiment, are anonymous and exclusively used for scientific purposes.

Chapter 3

DYNAMIC ON-THE-SPOT CONSUMPTION AND PORTFOLIO CHOICE IN THE LAB

3.1 Introduction

Individual preferences determine how people allocate wealth and plan consumption between different points in time and over different states of the world. Such consumption and investment decisions determine the accumulation of capital in a society and are thus basic factors for economic growth. They are further important for the design of fiscal policy such as taxation and retirement plans and monetary policy such as interest rate determination. In the macroeconomic and finance literature following Samuelson (1937; 1969), *standard preferences* assume that agents (i) draw utility only from consumption, (ii) have constant relative risk aversion (CRRA), and (iii) have constant discount rates and are thus time-consistent. This preference specification is however inconsistent with several empirical stylized facts about consumption and portfolio choice such as stock market non-participation, high equity risk premia, and the time variation of portfolio and consumption decisions, specifically in reaction to wealth shocks.[1]

We design a laboratory experiment that replicates the structure of life-cycle consumption models and utilizes a real consumption good. Our experiment consists of four consumption and investment periods. In the first period, subjects are endowed with initial experimental wealth. Consumption corresponds to the purchase of internet time. While subjects surf the web, they do not have to perform an alternative task which corresponds to the monotone closing of pop-up windows. However, subjects need to start doing this dull task once they have consumed the internet time they purchased in the current period. Moreover, subjects decide how much experimental wealth to invest in a risky asset or store safely for consumption in future periods. The investment outcome generates fluctuations in wealth. In the very last period, subjects automatically consume all remaining wealth.

The experiment distinguishes allocation plans to which subjects wish to pre-commit to ex-ante from those that subjects actually chose on-the-fly. For this, subjects first face a *pre-commitment* stage and subsequently an *allocation-and-consumption* stage. In the pre-commitment stage, subjects have to make a consumption and portfolio plan for every possible contingency, i.e., they fill out a decision tree that captures all possible paths of

[1] See for example Heaton and Lucas (1997); Odean (1998); Fernandez-Villaverde and Krueger (2007); Jappelli and Pistaferri (2010); Frederick, Loewenstein, and O'Donoghue (2002).

the experiment. Then, we randomly determine whether this initial plan is binding for each subject in the second stage. If it is binding, a subject has to follow his or her plan. In this case, we say that a subject is pre-committed (PC). If it is not binding, subjects have to allocate their initial wealth anew in every period of the second stage. In this case, we say that a subject is non-pre-committed (NPC).

We use this novel design to explore the following research questions: (i) Do changes in wealth change consumption and investment behavior? (ii) Does behavior differ from a possible pre-committed allocation? These question are important for various reasons. The correlation of consumption with the business cycle determines the impact of counter-cyclical fiscal policies such as tax rebates and transfer payments. If people massively cut back their consumption, e.g., in a recession, more extreme measures might be necessary than if consumption response is sluggish. Furthermore, understanding the changes in societal risk exposure is important to propose effective and efficient monetary policies such as the level of interest rates or fiscal policies such as stimulation of entrepreneurship and self-employment incentives. Regarding the second research question, it is important to understand the necessity and role of commitment devices for the design of retirement plans and the decision about mandatory or voluntary enrollment. A recent trend in social and corporate retirement plans across the US and Europe is directed to grant individuals more control and responsibility (Mitchell and Utkus 2006). However, time-inconsistent behavior and the sub-optimal reaction to wealth fluctuations might prevent people from achieving sufficient retirement provision.

We contribute to the financial and macroeconomic literature in the following ways. Our main contribution is the design of a joint consumption and portfolio-choice setting with real on-the-spot consumption rather than monetary payoffs. Such a design closely mimics the theoretical modeling of life-cycle problems. We use our design to test the predictions of the workhorse model of Samuelson (1969). Hence, we are the first to analyze consumption and portfolio decisions with an actual time structure of real consumption flows. Further, we are not aware of any experimental study that assesses the impact of wealth changes on consumption choices under pre-commitment. Hence, we are also the first to analyze the endogenous formation of life-cycle consumption and investment plans and explore if and how subjects deviate from such an ex-ante plan. Our experiment provides a controlled testing ground for theories on dynamic allocation problems and the analysis of policy interventions concerning, e.g., taxation and retirement, which can easily be implemented in the current design.

Samuelson (1937) established the idea that an agent maximizes the (expected) discounted sum of future utilities. His assumption of constant discount rates has become the standard modeling assumption in life-cycle models. Furthermore, Samuelson (1969) augments this discounted-utility model and analyzes intertemporal consumption and portfolio decisions in discrete time using a functional form of utility with CRRA. Under constant investment opportunities, Samuelson (1969) shows that the optimal consumption share only depends on the remaining time horizon and is independent of the investment outcome. Further, the optimal investment share is constant. Because the agent is time-consistent, he has no positive demand for a commitment device. That is, PC and NPC choices do not differ.

Our results are generally consistent with the model of Samuelson (1969). We find that standard preferences are a good predictor for consumption and investment behavior if subjects are not committed to an ex-ante plan (NPC). We find no indication that subjects plan to systematically vary their investment behavior under PC with variations in wealth. This also holds for NPC choices which is in line with the results in Brunnermeier and Nagel (2008). In addition, subjects do not significantly vary their investment share over time. We observe two inconsistencies with standard preferences for consumption. Under precommitment (PC), subjects plan to vary their consumption share with changes in wealth due to the investment outcome. In particular, subjects plan to increase consumption in case of a bad investment outcome. Comparing PC and NPC choices, we observe no difference for the investment share but significant underconsumption relative to the ex-ante plan. However, underconsumption is not robust against the inclusion of controls in parametric regressions. Finally, the post-experimental questionnaire confirms the use of internet as a real consumption good as subjects indicate a positive willingness to pay even after three hours of experimentation.

We discuss to what extent the deviations from standard preferences can be explained by two prominent non-standard preference specifications: (i) *external-habit preferences* and (ii) *expectations-based reference-dependent preferences*. Both specifications incorporate reference points into total current utility and predict that choice variables vary with changes in wealth. External-habit preferences specify that the reference point is a level of consumption that the agent must not fall short of (Constantinides 1990; Munk 2008; Brunnermeier and Nagel 2008). Expectations-based reference dependence specifies that the reference point is the agent's rational belief about consumption streams (Kőszegi and Rabin 2006; 2007; 2009; Pagel 2012; 2014). While the subjects' plan to vary consumption with the investment outcome under PC is consistent with expectations-based reference dependence, the difference in commitment can be explained by external habits under the identifying assumptions that subjects are naive and do not anticipate the formation of habits (Loewenstein, O'Donoghue, and Rabin 2003; Acland and Levy 2013). However, both non-standard preference specifications make several other predictions for which we find no evidence. Hence, we conclude that standard preferences are the best predictor in our setting.

The use of real consumption in a life-cycle context has several advantages compared to analyzing our research questions with observational data. The difference between theoretical implications and empirical observations can be due to market frictions such as participation, rebalancing, and monitoring costs or because of non-standard preferences that rationalize the observed behavior as a result of optimal choices in a discounted expected-utility framework or a combination of frictions and preferences. In addition, empirical data sets on individual consumption and portfolio decisions are usually based on survey data which feature severe measurement and aggregation problems, e.g., in the definition of consumption and wealth shocks (Campbell and Deaton 1989). The controlled setting of a laboratory experiment, however, can account for these problems by specifying the economic environment and exactly observing individual decisions.

Empirically, preferences are typically evaluated using two major data sets: The Consumer Expenditure Survey (CEX) and the Panel Study of Income Dynamics (PSID). The CEX is a primary source in the empirical life-cycle consumption literature and consists of two surveys which include consumption information of American consumers, data on their expenditures and income as well as household characteristics collected by the U.S. Census Bureau.[2] The PSID is a longitudinal household survey used in empirical research on investment behavior but also in the life-cycle consumption and portfolio-choice literature.[3] However, both of these data sets have flaws that impair a clear identification of the response of consumption and investment behavior to wealth fluctuations. First, the CEX is no panel but a repeated cross-sectional survey without detailed financial information. Second, the PSID only has coarse consumption measures that aggregate a wide range of consumption expenditures. Of course, neither of these data sets allows to analyze the difference in commitment.

We are not aware of any other study that analyzes both consumption and portfolio decisions with real consumption. Previous research typically focuses on these two decisions in isolation. Furthermore, the use of real consumption goods in dynamic decisions is also rare as most experiments use monetary incentives and pay off only at the very end of the experiment.[4] A noteworthy exception is Brown, Chua, and Camerer (2009) who investigate life-cycle consumption with habit formation by utilizing beverages as incentives. In one condition, subjects receive the beverage in the same period in which

[2] See `http://www.bls.gov/cex`.

[3] See `http://psidonline.isr.umich.edu`.

[4] Experiments analyzing consumption choices with monetary incentives include, for example, Kotlikoff, Samuelson, and Johnson (1988); Anderhub et al. (2000); Mattei (2000); Février and Visser (2004) and Luhan, Roos, and Scharler (2011). Regarding investment choices, much work has been done on testing the portfolio-choice assumptions underlying asset pricing models (Kroll, Levy, and Rapoport 1988a,b; Kroll and Levy 1992; Bossaerts, Plott, and Zame 2007), analyzing the effects of ambiguity aversion on portfolio composition (Charness and Gneezy 2010; Bossaerts et al. 2010; Ahn et al. 2011), or introducing background risk (Klos and Weber 2006).

they make a decision, while in another condition they receive the beverage in some future period. Consistent with hyperbolic discounting, subjects in the immediate condition overconsume relative to the delayed condition.[5]

Augenblick, Niederle, and Sprenger (2013) provide further evidence for time-inconsistent behavior under real effort. They analyze both monetary and real-effort choices in a longitudinal experiment. Subjects have to complete a number of tasks over the course of seven weeks and can choose when to conduct these tasks. Besides providing real effort, subjects also have to decide how much work they want to complete at some adjacent future dates. In a within-subject design, subjects additionally allocate money across different sooner-and-later options. Augenblick, Niederle, and Sprenger (2013) find that subjects show little present bias under monetary incentives but a considerable present bias under real effort. Subjects actually reallocate "unpleasant consumption" (real effort) to the future instead of doing it today as intended in their initial allocation. Subjects were also offered a probabilistic commitment device that provides commitment with a certain probability as in our design. Augenblick, Niederle, and Sprenger (2013) report that subjects generally show no willingness to pay for commitment.

Houser et al. (2010) propose a design closely related to ours. They test whether subjects are willing to pay for a commitment device that excludes a tempting alternative from their choice set. Temptation consists of frequently offering subjects to abandon a boring task (counting numbers) and to surf the internet instead. When such a tempting choice screen appears, subjects can either continue counting, stop counting completely and surf the internet (and therewith forfeit a high payoff), or pay to get rid of the choice screen completely and therewith commit to the counting task. They find that a significant number of subjects is willing to pay to eliminate the temptation.

The rest of the paper is organized as follows. Section 3.2 presents the economic environment and our experimental implementation. Subsequently, we present the predictions based on standard preferences in Section 3.3. We then present our results (Section 3.4) and discuss the two non-standard preference specifications as alternative explanations (Section 3.5). Section 3.6 concludes.

3.2 Experiment

We first present our implementation of a multi-period allocation problem with real consumption. We vary the commitment type within-subject to test our predictions on the difference between PC and NPC choices.

[5]Sippel (1997) also uses on-the-spot consumption to analyze subjects' choices from a variety of consumption bundles to be consumed in the laboratory as an alternative to sit around and do nothing. Under the variation of prices and income, subjects frequently violate revealed-preference axioms, hence contradicting the standard model of utility maximization.

3.2.1 Dynamic Allocation with Real Consumption

Consumption. Life-cycle models are based on utility flows from consumption over multiple periods. Experimental studies generally proxy consumption via monetary payoffs. Such payoffs are, however, inappropriate to distinguish different preference theories of intertemporal consumption choice. This is because standard experimental payment procedures do not have an intertemporal structure, i.e., they do not pay out at different points in time over the course of the experiment. Usually, monetary earnings are either accumulated over all rounds or one round is randomly determined for payment. Both these procedures pay off at the very end of the experiment. Hence, money has to be converted into consumption after the completion of the experiment thus only resembling utility from terminal wealth. We therefore allow for real consumption of an actually consumable good. In addition, we wanted consumption to be on-the-spot, i.e., in the actual period for which a decision applies. Our experiment consists of four such periods and hence allows for real utility flows from consumption over time.

Subjects earn a fixed payment and are endowed with an initial wealth of an experimental currency simply named *points*. They decide each period how much experimental wealth to consume in that period or to save for consumption in future periods. Specifically, we implement real consumption as follows. There are two alternatives on how subjects can spent their time in the lab: either they (i) perform a monotone *clicking task* or they (ii) surf the internet instead.[6] For the clicking task, subjects have to close pop-up windows that appear randomly at random positions on their computer screen. When and where a window appears is independent across subjects. On appearance, subjects have ten seconds to click on a button to close the pop-up. If a subject does not close the pop-up, it will disappear automatically creating convex costs for this subject.[7] The clicking task is designed to bore subjects and to render surfing the internet as more pleasurable.[8] Because the clicking task only controls subjects attention outside the consumption phase, we do not want it to interfere with the life-cycle decisions. Hence, we incentivize the clicking task with money and not with additional consumption consequences by adding or subtracting points.

Utility. Dynamic consumption models require a certain concavity of the utility function to induce a preference for consumption smoothing. Following Smith (1976),

[6] The instructions made clear that we were neither tracking their online behavior nor saving passwords etc.

[7] The random appearance (both time and location) of the pop-ups ensures that subjects focus on the computer screen during the whole experiment. The clicking task is thus basically a pure disciplining mechanism. Without this task, subjects could just decide to take the otherwise fixed payoff and do nothing (e.g., sleep). Hence, the clicking task enforces that subjects, in a sense, actively do nothing.

[8] Comments in the post-experimental questionnaire confirm that subjects perceived this task as extremely unchallenging and boring.

it is standard in consumption experiments to induce the period utility function exogenously (e.g., Fehr and Zych 1998; Ballinger, Palumbo, and Wilcox 2003; Brown, Chua, and Camerer 2009; Carbone and Duffy 2014).[9] We follow the literature and implement concavity by transforming points into internet time according to

$$\text{Internet Time [Sec.]} = f(\text{Points}) = k_0 - k_1/(\text{Points} + \epsilon)^{\hat{\theta}} \tag{3.2.1}$$

with risk-aversion parameter $\hat{\theta} = 0.1$. Scaling factors are $k_0 = k_1 = 2400$ and we bound the utility function from below with $\epsilon = 1$. Without $\epsilon > 0$, the transformation function would be unbounded from below for $\hat{\theta} > 0$ whenever $C_t = 0$.[10] Ballinger, Palumbo, and Wilcox (2003) and Brown, Chua, and Camerer (2009) both use $\hat{\theta} = 3$ and $\epsilon = 2.7$. However, in our experiment, a too concave utility function would result in too little variation in outcomes, i.e., seconds to spend online.

Investment. Subjects can save wealth by keeping it in a risk-free asset or investing it in a risky asset. The risk-free asset yields the gross return $R^f = 1$ and the risky asset's gross return is binary with equal probability to either triple the invested points, $R_t = 3$, or dividing them by three, $R_t = 1/3$. These investment opportunities are constant throughout the experiment.

Closed Life-Cycle Design. All remaining wealth is automatically transformed into internet time in the very last period. This is done because subjects may hold an ex-ante payoff target when participating in an experiment. This may serve as a reference point from the mere participation in the experiment and run against the endogenous expectations formed within the experiment and confound our results. We thus ensure that the expectations which influence subjects' decisions are formed within the experiment. That is, we control subjects' expectations regarding the experiment itself and the monetary payments, respectively. We do so by disentangling monetary payments from the consumption decisions. The instructions make very clear that points used for consumption are of no value outside the lab.

3.2.2 The Course of the Experiment

The experiment consists of two parts. In the first part, subjects have to complete the clicking task for ten minutes. This serves to familiarize them with the task and eliminate choices out of curiosity in the second part. The second part consists of two stages: The (2a) *pre-commitment* stage and the (2b) *allocation-and-consumption* stage. In each of

[9] Inducing utility depends on the requirements of *monotonicity*, *salience*, and *dominance* (Smith 1976). Monotonicity says that the real but unknown utility is strictly monotonically increasing in the incentive (e.g., money or real consumption). Salience means that changes in the incentive stem from subjects' actions. Dominance says that changes in subjects' utility come predominantly from changes in the incentive.

[10] See Ballinger, Palumbo, and Wilcox (2003) for a discussion on the role of ϵ.

these stages, subjects have to make decisions for a total of four periods of which each is 19 minutes long.[11] In the pre-commitment stage, subjects have to determine their consumption and portfolio choices for the entire course of the experiment. It is randomly determined whether the conditional plan is binding (PC) or not (NPC). In the allocation-and-consumption stage, PC subjects follow their plan while NPC subjects decide anew in each period how much to consume and how much to invest. In the following, the two stages are described in more detail.

The Pre-Commitment Stage

In this initial stage, subjects determine their consumption and investment choices for every possible contingency. We present a horizontal decision tree in which subjects have to enter all choices conditional on the realization from the risky asset. We framed this decision tree neutrally as *point manager*.

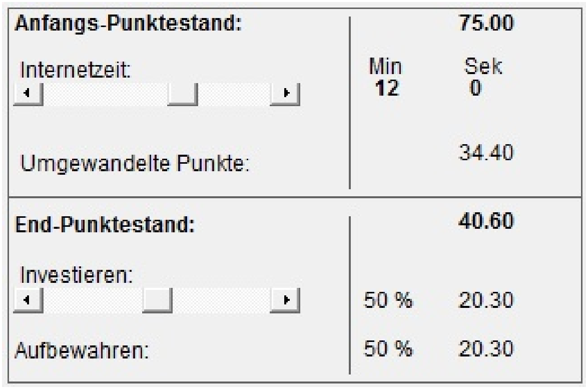
The point manager displays all four rounds and all possible paths resulting from the investment outcome (see Figure 3.A.5). Choices during the experiment were input to so called *decision boxes* (Figure 3.1). In the very first round, subjects are endowed with an initial wealth of 75 points. Subject have to choose via scrollbars how many points to transform into internet time and the percentage of remaining wealth to invest in the risky asset. The remaining points are automatically stored and the resulting internet time is automatically calculated and displayed according to the transformation function (3.2.1). No initial values were displayed to avoid framing.[12]

The risky asset yields two possible paths in round two: *Up* (U) or *Down* (D). In round three, there are four possible paths: (1, U, UU); (1, U, UD); (1, D, DU); and (1, D, DD) where UU denotes *Up Up*, UD denotes *Up Down* etc. Hence, subjects have to fill out seven decision boxes in total as period four involves no choice. In the following, we abbreviate paths by their period-three realization, e.g., UD denotes the path (1, U, UD) starting in period one, going up in period two ($R_2 = 3$) and down in period three ($R_3 = 1/3$).

The point manager automatically enforces the budget constraint. In addition, the point manager also updates the wealth at subsequent nodes depending on the investment decision. If a subject changes a decision in an early decision box, the point manager

[11] We chose this length such that subjects had a real incentive to fill out the point manager as it may be binding for 4 x 19 = 76 minutes. In addition, if we had chosen a period length of, e.g., 20 minutes, subjects may have had the ten-minutes mark as a focal point for their decisions. Thus, we wanted to make the "half time" less salient.

[12] Subjects were free to use the built-in computer calculator that is accessible in all decision screens throughout the entire experiment.



Notes: Depicted is the decision box used in the point manager to elicit choices under pre-commitment (PC). The first line displays current wealth W_t^{PC} (**"starting wealth"**) at the beginning of the period. In line two, subjects choose their current level of consumption C_t^{PC} (**"internet time"**). The third line displays the amount of transformed points needed to purchase the chosen internet time (**"transformed points"**). Points are transformed according to Points $= (2400/(2400 - \text{internet time [sec.]}))^{10} - 1$. The fourth line displays the remaining wealth after consumption $W_t^{\mathrm{PC}} - C_t^{\mathrm{PC}}$ (**"end wealth"**). In line five, subjects choose their investment share α_t^{PC} (**"investing"**). The sixth line displays the share kept in the risky asset $1 - \alpha^{\mathrm{PC}}$ (**"storing"**).

Figure 3.1: PC Decision Box.

resets wealth and internet time at later branches to emphasize the dynamic structure of the decision problem and to prevent unintended changes at later nodes.[13]

At the end of the pre-commitment stage, after the point manager was completely filled out and saved, we roll a die for each individual to determine whether the plan is binding for the entire allocation-and-consumption stage. If a one comes up, the subject is bound to his or her plan, i.e., pre-committed (PC). If a number between two and six comes up, the subject is not bound to his or her plan, i.e., non-pre-committed (NPC).[14]

The Allocation-and-Consumption Stage

This stage consists of four consecutive periods. For the PC subjects, in each period, the computer displays the conditional choice taken from their point manager. NPC subjects, on the other hand, decide in each period anew, how much they want to consume. As in the point manager, the computer transforms all remaining points into internet time for the NPC subjects in period four.

PC Subjects. If subjects are pre-committed, the computer automatically displays their choice made in the point manager conditional on the path that has been reached.

[13]Investment and storing shares were not reset as these are percentages of remaining wealth $W_t^{\mathrm{PC}} - C_t^{\mathrm{PC}}$. However, their absolute values were, of course, also reset if the according wealth levels were reset.

[14]We framed pre-commitment (PC) by stating that the point manager "remains active". We framed non-pre-commitment (NPC) by stating that the point manager "has been deactivated".

The internet browser opens and closes automatically and the clicking task starts automatically after the chosen internet time has been consumed.

NPC Subjects. If subjects are non-pre-committed, they decide anew, in each period, how much they want to consume in that given period. First, at the beginning of period t, NPC subjects see a choice box similar to the one used in the pre-commitment stage but without internet time, transformed points or remaining wealth (see Figure 3.A.6). There, they choose their investment share α_t^{NPC} without knowing their remaining wealth $(W_t^{\mathrm{NPC}} - C_t^{\mathrm{NPC}})$ in that period because this will only be determined at the end of the period. NPC subjects have an internet time "test calculator" on their decision screen which transformed internet time into points. In addition, the instructions include a table with several examples of time-point pairs.

Second, the internet browser opens and they can surf the internet. After they do not want to consume internet time any more, they close the internet browser and press a button to start the clicking task again.[15] The time that they spent online corresponds to C_t^{NPC} and is automatically transformed into points according to the transformation function (3.2.1).[16] After the period, subjects receive feedback about their allocation and are shown the absolute amount invested, i.e., $\alpha_t^{\mathrm{NPC}}(W_t^{\mathrm{NPC}} - C_t^{\mathrm{NPC}})$.

Feedback and Investment Outcomes. At the beginning of a new period, PC and NPC subjects are both reminded of their previous-period allocation and the possible (two) consequences from that decision. Then we determine the investment outcome for each subject individually via a die roll. Subsequently, PC subjects are informed which decision node has been reached and are displayed their according decision box from the point manager while NPC subjects have to choose their investment share for the current period anew as described before.

For all subjects, the internet browser was slightly shorter than full screen. This scaling allowed the subjects to see the experiment screen in the background which displayed a counter showing their current level of consumption, i.e., how much internet time has already been consumed (see Figure 3.A.7).[17] As in the PC stage, subjects hence choose consumption C_t, i.e., internet time, directly. After the 19 minutes had passed, a notification tells the subjects to wait for the start of the next period and for an experimenter to realize their individual investment outcome via a die roll: if a number between one and three comes up, the low outcome ($R_t = 1/3$) is realized and if a number between four and six comes up, the high outcome ($R_t = 3$) is realized.

[15] On the same screen where subjects choose their investment shares, a note reminded them how to close the internet and start the clicking task.

[16] If a subject consumed all her wealth, the internet browser closed automatically.

[17] Tabbing and several other hotkeys had been disabled and the task bar has been hidden. Hence, subjects did only see the internet browser and the experimental screen in the background.

3.2.3 Procedural Details

The experiment has been conducted at the Cologne Laboratory for Economic Research (CLER), University of Cologne, Germany.[18] Subjects were recruited via the online recruitment system *ORSEE* (Greiner 2004). Subjects could sign up on a first-come-first-serve basis. In total, 60 subjects from all faculties participated. Neither content nor expected payments were stated in the invitation e-mail. Decisions were inputs to an interface computerized via *z-tree* (Fischbacher 2007). We utilized Mozilla Firefox as the internet browser with a customized user profile.[19] We conducted six sessions in October 2014. Each session lasted for around three hours including payment.

Upon entering the lab, subjects draw a number from an urn determining their computer booth. A set of general instructions was already placed in each booth.[20] It was not possible for subjects to see each other's computer screens. Each booth was equipped with an air-cushion envelope. Subjects had to put their mobile phones and other electronic devices (mp3 players etc.) into this envelope which was then sealed by an experimenter. This ensured that subjects had no way to substitute the provided internet during the experiment. In addition, subjects had to deposit their personal belongings (backpacks etc.) behind them outside their computer booth. Each computer was equipped with a pair of headphones.

Subsequently, subjects were informed that the experiment consisted of two parts and received the instructions for part one. After the completion of part one, subjects received the instructions for part two. At all stages of the experiment, subjects were allowed as much time as they needed to familiarize themselves with the procedure of the experiment, to ask questions, and make their choices. After all questions were answered in private, subjects had to answer some control questions. Subsequently, part two started with the pre-commitment stage followed by the allocation-and-consumption stage. After the experiment, subject had to answer a questionnaire in which we elicited demographical and attitudinal variables as well as a willingness-to-pay (WTP) measure for a hypothetical additional period of experimentation. Subjects were paid in private. Payoffs consisted of 30 EUR from which the total costs accumulated from the clicking task were deducted. In addition, subjects received a show-up fee of 2.50 EUR. All subjects received the full 32.50 EUR (approx. 41.39 USD at the time of the experiment).

---

[18] `http://www.cler.uni-koeln.de`.

[19] `http://www.mozilla.org`.

[20] Appendix 3.B contains instructions in both German (original) and English (translated).

3.3 Theory: Standard Preferences

This section describes the benchmark prediction based on standard preferences and we call an agent with standard preferences a *standard agent*. We define standard preferences in the way that the agent derives utility from the absolute level of current consumption, C_t, according to the power-utility function

$$u(C_t) = \frac{C_t^{1-\theta}}{1-\theta}, \tag{3.3.1}$$

with constant relative risk aversion (CRRA) of $\theta > 1$. In addition, the standard agent is time-consistent and discounts future utility exponentially with the discount factor $\delta \in [0, 1)$. Thus, the agent has the same preferences about future plans at every point in time.

Time is discrete and the agent lives for $t = \{1, \dots, T\}$ periods and is endowed with initial wealth $W_1 > 0$. At period t, the agent's wealth before consumption, is denoted by W_t. Each period, the agent decides how much to consume out of his wealth and how to save the remaining wealth, $W_t - C_t$. The agent has access to a risk-free asset with gross return $R^f > 0$ and a risky asset with gross return $R_t > 0$ that is identically and independently distributed across periods according to F_R with $E_{F_R} R_t > R^f$. These investment opportunities are constant throughout the agent's life. We will refer to the realization of R_t as the investment outcome.

We denote the agent's choice variables in relative terms, i.e., as consumption and investment shares. The consumption share $\rho \in [0, 1]$ is the ratio of absolute consumption to current wealth and given by $\rho_t := C_t/W_t$. The investment share $\alpha \in [0, 1]$ is the amount of remaining wealth that is invested in the risky asset and given by $\alpha_t := I_t/(W_t - C_t)$ where $I_t \geq 0$ is the absolute amount invested. Hence, the agent's portfolio (PF) gross return in period t is a weighted average of the risk-free and the risky gross returns and given by $R_t^{\text{PF}} = \alpha_{t-1}(R_t - R^f) + R^f$ where $R_t - R^f$ is the excess return over the risk-free rate. Accordingly, the agent's intertemporal budget constraint at time t can be written as

$$W_{t+1} = (W_t - C_t) R_{t+1}^{\text{PF}} = \big(W_t(1 - \rho_t)\big) R_{t+1}^{\text{PF}}. \tag{3.3.2}$$

Samuelson (1969) and Merton (1969) derive the optimal consumption and portfolio share in discrete and continuous time, respectively. In each period t, the standard agent maximizes his remaining life-time expected utility. He conditions his decision on information available at time t, denoted by Ω_t.[21] The associated indirect utility function

[21] The state variable Ω_t contains all information available in period t. It does thus consist of the time horizon, current wealth, and the (constant) investment opportunities: $\Omega_t = (t, T, W_t, F_R, R^f)$. However, current information does not include current or future choice variables because these are assumed to be chosen optimally via backward induction, i.e., to maximize expected lifetime utility (Brandt 2010).

given current wealth W_t and remaining horizon $T-t$ conditional on information at time t contained in the state variable Ω_t reads:

$$V_t = \max_{\{\rho_s,\alpha_s\}_{s=t}^{T-1}} \left\{ E_t \left[u(C_t) + \sum_{\tau=1}^{T-t} \delta^\tau u(C_{t+\tau}) \right] \right\} \tag{3.3.3}$$

with $C_t = \rho_t W_t$ and subject to the budget constraint (3.3.2), the no-bankruptcy requirement $W_s \geq 0$ and terminal conditions $\rho_T = 1$ and $\alpha_T = 0$. Hence, there is no bequest motive in this model.

Under CRRA utility, the choice variables are selected sequentially, i.e., the first-order condition for the investment share α_t is independent of the consumption share ρ_t (Brandt 2010). Thus, the agent first derives the optimal investment share and subsequently derives the optimal consumption share given the investment share. In period t, the first-order condition for the investment share is given by

$$E_t \left[\left(\alpha_t (R_{t+1} - R^f) + R^f \right)^{-\theta} (R_t - R^f) \right] = 0. \tag{3.3.4}$$

The investment share characterized by (3.3.4) is constant, i.e., $\alpha_t \equiv \alpha$ for every $t = 1, \ldots, T-1$ (Samuelson 1969; Brunnermeier and Nagel 2008). Specifically, the investment share does not depend on the investment outcome, i.e., $\partial\alpha_t/\partial R_t = 0$. The reason for the independence of wealth fluctuations induced by the realization of the investment outcome is the homotheticity of the power-utility function. Hence, the investment outcome just scales the agent's wealth and thus changes the absolute amount invested but it has no impact on the optimal investment share (Brandt 2010). The time invariance of α_t stems from the independent realization of the investment outcome across time, i.e., the constant investment opportunities. This independence eliminates any hedging demand in the first-order condition (3.3.4) which is thus identical to the one-period model.

Given the optimal investment share, the agent now derives the optimal consumption share in period t. The first-order condition equalizes marginal utility from current consumption and discounted expected marginal utility from the stream of future consumption and yields

$$\rho_t = \frac{1}{1 + (\psi_t)^{\frac{1}{\theta}}} \tag{3.3.5}$$

with $\psi_t = \delta E_t \big[R_{t+1}^{\mathrm{PF}} \big((\rho_{t+1})^{1-\theta} + (1-\rho_{t+1})^{1-\theta} \psi_{t+1} \big) \big]$.[22] The function ψ_t is the expected discounted value of future consumption flows and depends on period t, the investment share α_t, and future investment outcomes. Specifically, it does not depend on the current investment outcome. Hence, $\partial\rho_t/\partial R_t = 0$ for every $t = 1, \ldots, T-1$ and ρ_t varies with time t only. Samuelson (1969) shows that $\rho_t < \rho_{t+1} < \ldots < \rho_T = 1$.

Because the standard agent is time-consistent, he is not willing to pay for a commitment device. In particular, the agent would not benefit from such a device because

[22] See, for example, Pagel (2012).

neither his consumption nor his investment share differs from a possible pre-committed contingency-based plan that he could form in some period $t = 0$. Furthermore, denote the difference between NPC and PC choice variables by

$$\Delta\rho_t := \rho_t^{NPC} - \rho_t^{PC} \qquad \text{and} \qquad \Delta\alpha_t := \alpha_t^{NPC} - \alpha_t^{PC}. \tag{3.3.6}$$

Hence, under standard preferences, it holds that $\Delta\rho_t = 0$ and $\Delta\alpha_t = 0$. Furthermore, $\partial\Delta\rho_t/\partial R_t = 0$ and $\partial\Delta\alpha_t/\partial R_t = 0$, i.e., the commitment difference does not depend on the investment outcome.

3.4 Empirical Analysis

We begin with a more detailed description of the data set. Subsequently, we present the results in the same order in which subjects made their decisions. First, we analyze PC choices and subsequently evaluate NPC choices. Second, we compare PC and NPC choices and differences in commitment.

3.4.1 Data and Descriptives

In total, $N = 60$ subjects participated in the experiment each making seven choices under pre-commitment yielding 420 PC choices. $N = 52$ subjects were not committed to their consumption and investment plans. These NPC subjects faced only one (realized) path yielding three observed choices per subjects. However, one of the NPC subjects consumed her entire wealth in period one.[23] This leaves us with 154 NPC choices in total of which 102 are in reaction to realized investment outcomes (rounds two and three).[24] Hence, for the NPC choices, we have a micro-panel data set with a large cross-sectional dimension (large N) and few time periods (small T).

The post-experimental questionnaire asked demographics and other individual characteristics (age, sex, faculty, native language, etc.) as well as subjects' (hypothetical) willingness to pay (WTP) for internet time if there was a further round of experimentation. We discuss the results of the WTP elicitation in Section 3.5.2. Subjects consisted of 26 (43%) men, were on average approximately 26 years old, and the vast majority (83.3%) of the sample indicated German as their first language. Subjects were enrolled in all faculties with a majority of 20 (33%) from management, economics and social sciences. Most subjects were undergraduates (48%).

[23] We do not consider the investment choices of this subject in the analyses because they are zero per definition.

[24] Of the 51 NPC subjects, 27 got a good investment outcome in round two and 24 got a bad investment outcome. In round three, 25 got a good outcome and 26 got a bad outcome. The full distribution of subjects over the seven paths of interest is given in Table 3.C.4.

3.4.2 Pre-Committed Choices

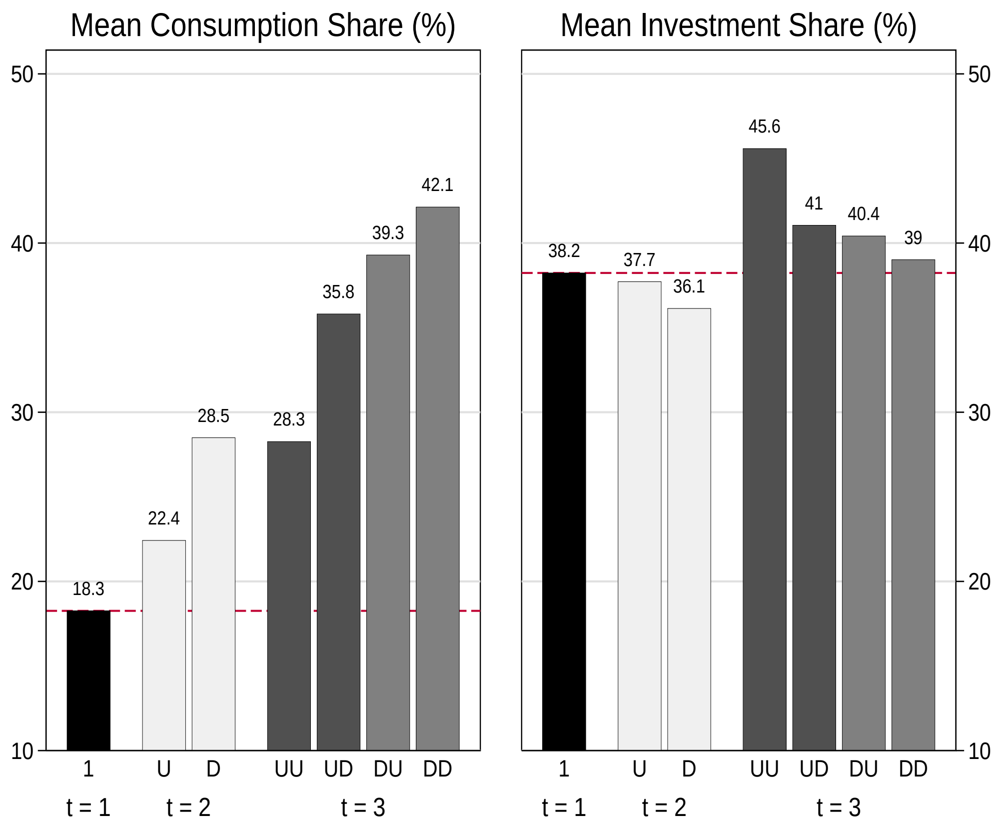
Figure 3.2 displays the mean for the two choice variables consumption share, ρ^{PC}, and investment share, α^{PC}, for each possible path.[25] We analyze how these variables change with respect to the expected investment outcome. A high realization of the gross return ($R_t = 3$) denotes a "good" investment outcome and a low realization ($R_t = 1/3$) denotes a "bad" investment outcome.

Consumption. The consumption share is given by the fraction of current wealth that is transformed into internet time. On average, over all possible paths, subjects pre-commit to consume 30.67% of their current wealth. As is standard in the optimal solution of life-cycle problems, subjects plan to increase the consumption share over time. However, this is also implied by the closed life-cycle design because subjects have to consume their entire wealth by the end of the last period. In general, the correlation between average per-period consumption shares and period is statistically different from zero (Spearman's rank correlation = 0.4763, $p = 0.0000$). In period one, the consumption share starts at 18.3%. In period two, subjects plan to increase the consumption share more strongly after a bad investment outcome (28.5%) than after a good outcome (22.4%). This corresponds to a difference of 27% which is economically meaningful and statistically different from zero (Wilcoxon signed-rank (SR) test, $p = 0.0001$). In period three, we have to compare consumption on the UU path (28.3%) with consumption on the UD path (35.8%) and consumption on the DU path (39.3%) with consumption on the DD path (42.1%). The 26.5% difference between UU and UD is again economically and statistically significant (SR test $p = 0.0056$). The 7.1% difference between DU and DD is comparatively small in magnitude and not significantly different from zero (SR test $p = 0.3971$).

On all paths, the direction in which subjects plan to adjust their consumption shares depends on the realization of the investment outcome. A bad investment outcome leads to an increase in relative consumption. This effect is economically large and statistically different in two of three possible path-wise comparisons. We therefore reject that the consumption share does not react to the realization of investment outcomes as implied by standard preferences.

Investment. The investment share is given by the fraction of remaining wealth after consumption that is invested in the risky asset. While our research question focuses on the response to wealth fluctuations, we also control for any time effect because standard preferences imply that investment shares are constant.

[25] Table 3.C.1 presents the according standard deviations. We note that there is less heterogeneity in the consumption share than in the investment share, i.e., for a given period, the consumption share always has a smaller standard deviation than the consumption share.



Notes: Reported is the mean of choice variables under pre-commitment (PC). $N = 60$. Consumption share is the fraction of current wealth transformed into internet time. Investment share is the fraction of remaining wealth after consumption that is invested in the risky asset. Paths that are compared to each other have the same color. The dashed line corresponds to the reference level of period one.

Figure 3.2: Choice Variables under Pre-Commitment.

On average, over all possible paths, subjects pre-commit to invest 39.74% of their remaining wealth in the risky asset. In period one, the investment share amounts to 38.23%. In period two, this share is not affected by the possible realizations of the investment outcome. The difference between path U and path D is not significantly different from zero (SR test, $p = 0.1141$). In period three, there is a statistical difference between path UU and path UD (SR test, $p = 0.0592$) but no statistical difference between path DU and path DD (SR test, $p = 0.3918$).

Concerning the prediction of constant investment shares under standard preferences, we do not find an economically or statistically meaningful trend between periods. The average investment share in period two over both realized outcomes is 36.93% and not statistically distinguishable from 38.23% in period one (SR test, $p = 0.5310$). Further, the average investment in period three over all four paths is 41.52% which is also not statistically different form period-one or period-two choices (SR test, $p = 0.3861$). In general, there is no statistically significant correlation between average per-period investment shares and period under PC (Spearman's rank correlation = 0.0941, $p = 0.2088$).[26]

We find no indication that the realization of the investment outcomes seems to influence how subjects plan to invest their remaining wealth. Hence, we cannot reject that investment shares under PC are constant both over time and investment outcomes as implied by standard preferences.

Regression Analyses. For robustness, we run parametric regressions of the consumption and investment shares on the investment outcome and additional controls. The regressions utilize the PC choices for period two and three where subjects anticipate to experience the realization of investment outcomes when making their decisions in the point manager. Hence, only the data from these rounds is suitable to test our research question of the dependence of choice variables on investment outcomes. The variable *Investment Outcome* is a dummy taking the value one if $R_t = 3$ and zero if $R_t = 1/3$. As controls we include the demographic variables *Age*, a dummy indicating male subjects (*Male*), and a dummy indicating German as the native language as a proxy for the comprehension of the instructions (*German Native*). The variable *Round 3* is a dummy taking the value one if the period for which a decision was made is $t = 3$ and zero if it is $t = 2$.

We run the fully specified model both with and without random effects on subjects. The use of random effects is valid due to the strict exogeneity of controls and the random sampling of subjects. In addition to OLS specifications, we also estimate two-censored Tobit models, because subjects in our experiment could neither borrow additional wealth nor could they short the risky asset. Our results are generally very robust across the different specifications and estimation methods.

Table 3.C.2 reports the results of these analyses for the consumption share. The negative effect of the investment outcome on consumption shares is not affected by the inclusion of control variables. In addition, we find that men consume around 8.5 percentage points less than women. This effect is significantly different from zero ($p < 0.05$).[27] Regarding the estimation of Tobit models, the results are virtually unaffected because only three choices (less than 1%) are clustered on the left boundary of zero and none are clustered on the right boundary of 100. Table 3.C.3 reports the same regressions for the investment share. As in the non-parametric analysis, we do not find any effect of investment outcome on investment shares. In addition, we find no convincing indication

[26] We observe a positive trend only in period three for the very top path UU. While there is no statistical difference between path U and period one (SR test, p=0.7679), the 19% increase on path UU compared to period one, is both economically large and statistically distinguishable from zero (SR test $p = 0.0290$). In addition, the investment share increases from U to UU by 20.95% (SR test, $p = 0.0369$).

[27] The p-value is based on the t-statistic in OLS and the z-statistic in GLS and Tobit estimation.

that the PC investment share varies systematically over time (*Round 3* is in all but one specification statistically insignificant). Overall, less than 8% of the observations are censored at zero and less than 6.4% at 100. We conclude that our non-parametric results are robust to parametric analyses and controlling for demographics.

Result 1 *[PC Choices] Under pre-commitment, subjects plan to increase their consumption shares after a bad investment outcome but do not plan to vary their investment shares. Further, subjects do not plan to systematically change their investment behavior over time. These results are robust against different parametric estimations and controlling for demographics.*

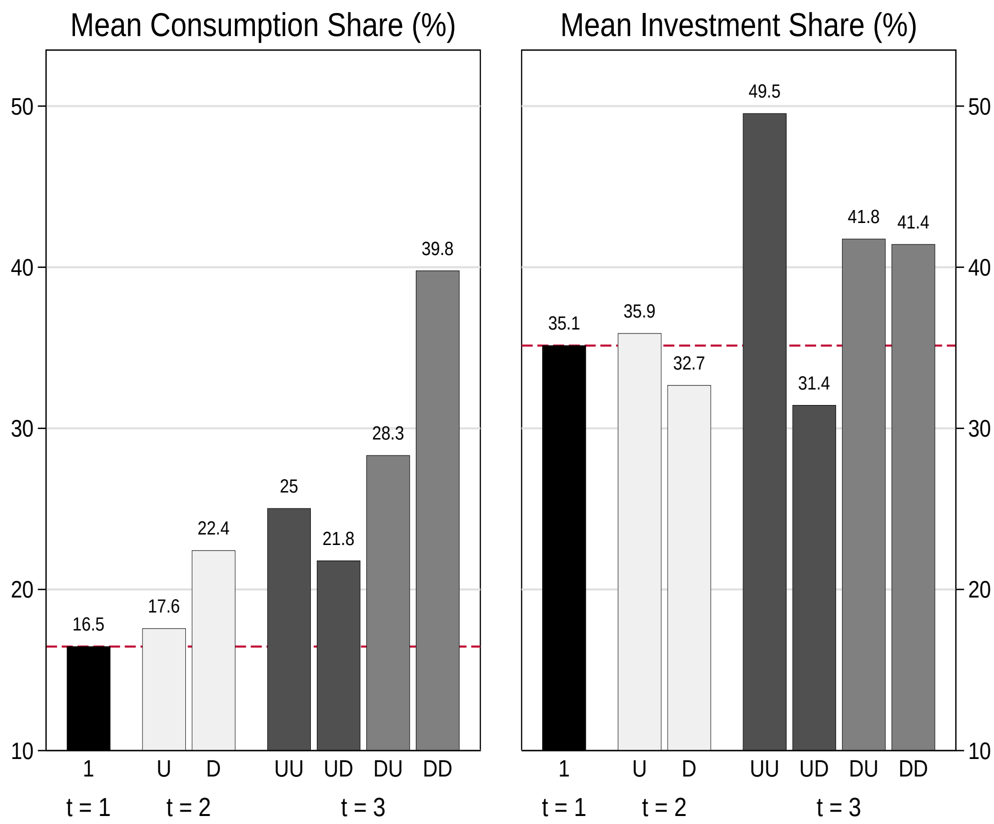
3.4.3 Non-Pre-Committed Choices

Figure 3.3 displays the mean for the two choice variables consumption share, ρ^{NPC}, and investment share, α^{NPC}, for each realized path under NPC.[28] We analyze how these variables change with respect to the realized investment outcome. Each subject only has one realized path in the experiment and thus, in contrast to pre-commitment, the number of observations per path decreases each round. The use of rank tests such as the SR for paired samples and the Wilcoxon-Mann-Whitney U-test for independent samples does not account for the fact that our choice variables are metric and thus these tests generally have lower statistical power than tests who use this information. The non-parametric Fisher-Pitman Permutation (FPP) test accounts for the metric scaling of our variables (Kaiser 2007).[29] However, if variables are quantitatively small but show a strong separation, the SR and U-test may detect a significant difference that is not reported under the FPP test. We therefore base our results on both the SR or U-test, respectively, and the FPP test. As in the analyses of PC choices, we complement the non-parametric analysis by assessing the robustness of our results with parametric regression models.

Consumption. On average, over all realized paths, subjects actually consume 21.53% of their current wealth. As under pre-commitment, consumption shares increase over time (Spearman's rank correlation = 0.2968, $p = 0.0002$). In period one subjects consume 16.5%. In period two, those subjects who experience a bad outcome consume 27.27% more than subjects who experience a good investment outcome. Although economically large, this effect is significantly not distinguishable from zero (U-test, $p = 0.1462$, FPP test, $p = 0.2038$). In period three, mean consumption shares on path UU are 14.68% larger than

[28] Table 3.C.5 presents the according standard deviations. As under PC, investment shares have larger standard deviations than consumption shares.

[29] For paired samples, we report the one-sample FPP test while we use the two-sample FPP test if we compare independent observations (Kaiser 2007). In both situations, the FPP test has more statistical power than either the SR test or the U-test (Siegel and Castellan 1988; Kaiser 2007).



Notes: Reported is the mean of choice variables under non-pre-commitment (NPC). For the consumptions share: $N = 52$ in round one and $N = 51$ in rounds two and three, respectively. For the investment share: $N = 51$. Consumption share is the fraction of current wealth transformed into internet time. Investment share is the fraction of remaining wealth after consumption that is invested in the risky asset. Paths that are compared to each other have the same color. The dashed line corresponds to the reference level of period one.

Figure 3.3: Choice Variables under Non-Pre-Committment.

on path UD (U-test, $p = 0.7709$, FPP test, $p = 0.6251$). In period three, subjects on path DD consume 38.87% more than subjects who experienced a good investment outcome (path DU). This difference is economically very large but not statistically significant (U-test, $p = 0.2482$, FPP test, $p = 0.1233$).

Investment. On average, over all realized paths, subjects invest 36.78% of their remaining wealth in the risky asset. In period one, subjects invest 35.14% and do not systematically adjust their investment based on the investment outcome in period two (U-test, $p = 0.5131$, FPP test, $p = 0.5971$). In period three, subjects largely increase their investment share on the up path after experiencing a good investment outcome. On path UU, the average investment share is 57.64% larger than on path UD (U-test, $p = 0.0682$, FPP test, $p = 0.0944$). On paths DU and DD, however, investment shares are virtually identical and not statistically different from each other (U-test, $p = 0.8843$, FPP test, $p = 0.9792$).

Though subjects, on average, seem to increase consumption shares on average after a bad investment outcome in two out of three pathwise comparisons, we cannot reject that consumption shares are unaffected by the investment outcome as predicted by standard preferences. Regarding the investment share, we can also not convincingly reject standard preferences under NPC. Furthermore, despite the comparatively large variation in investment shares over time, we find no significant trend across periods (Spearman's rank correlation = 0.0698, $p = 0.3910$). However, as we will see below, this result changes in the parametric regressions once we control for demographics.

Regression Analyses. Subjects make three decisions under NPC. As with PC choices, we effectively use the observations of periods two and three. Only in these rounds is it possible to react to the investment outcome. Tables 3.C.6 and 3.C.7 show that our results are generally robust against the inclusion of control variables in the parametric regression models and against different estimation methods. We run the same specifications as for the PC data.

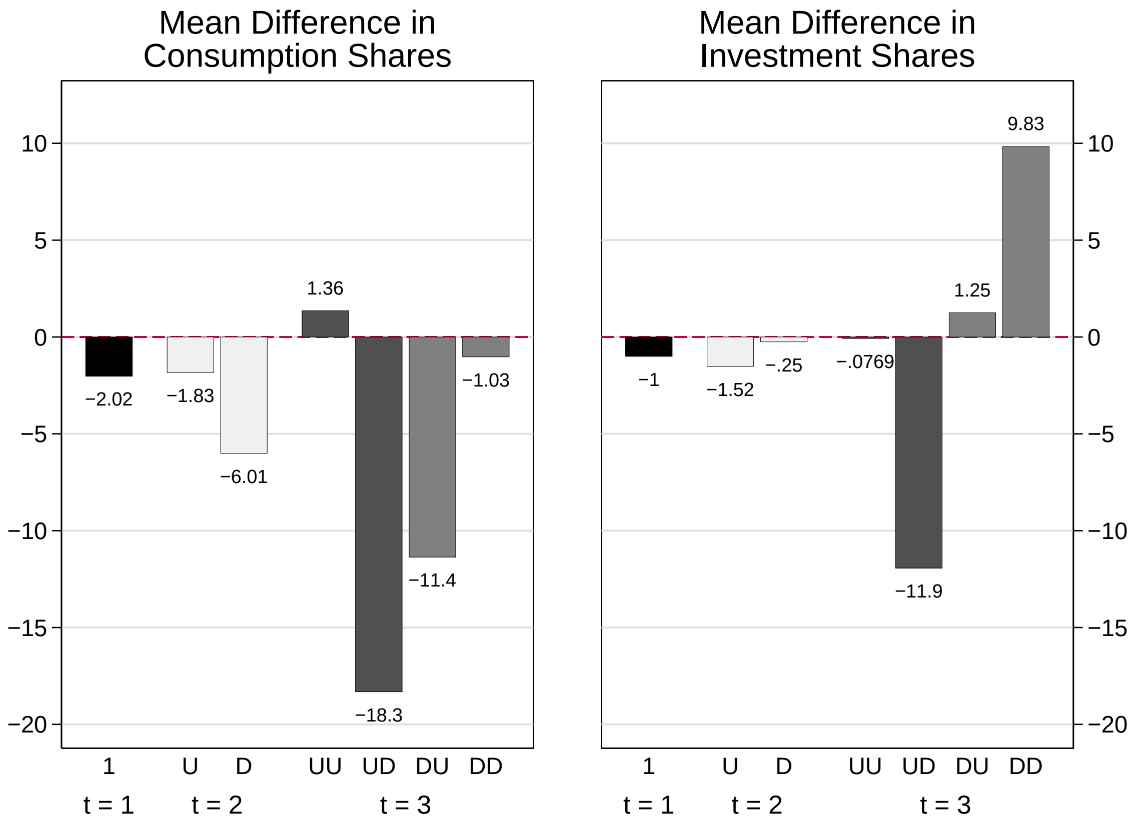
Consumption shares do not systematically depend on age or speaking German natively. However, men consume significantly less than women in period two and three ($p < 0.01$). A pairwise comparison shows that men consume more in later rounds while women prefer to consume more out of their current wealth in the beginning of the experiment. In period four, men on average have more than twice the wealth that women have in that final period (U-test, $p = 0.0042$, FPP test, $p = 0.0184$).

Regarding the investment share, age, gender, and indicating German as the native language have no statistically significant effects. As mentioned above, we find that the investment share increases between period two and period three by approximately seven percentage points in both OLS and Tobit models ($p < 0.01$). This leads us to reject the hypothesis that investment shares are constant over time after controlling for demographics.

Result 2 *[NPC Choices] Under non-pre-commitment, subjects do not significantly vary their consumption or investment shares with the investment outcome. After controlling for demographics, subjects increase their investment shares over time.*

3.4.4 Differences in Commitment

We analyze the difference of commitment status by comparing the choices of those subjects that have been observed under both conditions, i.e., we utilize the same subjects as in the NPC analyses in Section 3.4.3. For each subject, we generate the difference



Notes: Reported is the mean difference of choice variables between non-pre-commitment (NPC) and pre-commitment (PC) in percentage points. For the consumption share: $N = 52$ in period one and $N = 51$ in periods two and three, respectively. For the investment share: $N = 51$. Consumption Share is the fraction of current wealth transformed into internet time. Investment share is the fraction of remaining wealth after consumption that is invested in the risky asset. Differences are computed by subtracting PC values from NPC values for each subject. Paths that are compared to each other have the same color. The dashed line corresponds to the reference level of zero (no difference).

Figure 3.4: Commitment Differences of Choice Variables.

in consumption shares, $\Delta\rho_t := \rho_t^{\text{NPC}} - \rho_t^{\text{PC}}$, and the difference in investment shares, $\Delta\alpha_t := \alpha_t^{\text{NPC}} - \alpha_t^{\text{PC}}$. We then perform the same analyses as before where we analyzed the choices under PC and NPC separately.

Figure 3.4 displays, for each realized path, the mean differences for the two choice variables.[30] We first analyze whether the difference between commitment status is statistically different from zero. We account for the small sample size, specifically in period three, by using the SR test and the FPP test.

[30] Table 3.C.8 presents the according standard deviations. In contrast to the separate analyses of PC and NPC choices, investment shares do not always vary more strongly than consumption shares.

Consumption. We observe a general pattern of underconsumption relative to the plan made in the PC stage. Collapsing the data by subjects yields 52 independent observations and an average difference of -3.55 which is significantly different from zero (SR test, $p = 0.0004$, FPP test, $p = 0.2303$). In period one, subjects consume around two percentage points less under NPC (SR test, $p = 0.0200$, FPP test, $p = 0.4529$). In period two, underconsumption is not significant on the up path (SR test, $p = 0.4004$, FPP test, $p = 0.5824$) but on the down path (SR test, $p = 0.0593$, FPP test, $p = 0.0433$). The slight positive deviation on path UU is not significant (SR test, $p = 0.3109$, FPP test, $p = 0.9001$). On path UD, average underconsumption is large and amounts to an average of around 18 percentage points (SR test, $p = 0.1094$, FPP test, $p = 0.0359$). Similarly, subjects underconsume by around eleven percentage points on path DU (SR test, $p = 0.1361$, FPP test, $p = 0.0981$). We find no statistical difference on path DD (SR test, $p = 0.3078$, FPP test, $p = 0.7646$). These results indicate that subjects underconsume if they are not committed to their PC choices.

We analyze whether the commitment difference depends on the investment outcome by pathwise comparisons between subjects as in Section 3.4.3. Generally, commitment differences in consumption shares do not vary with the investment outcome. Underconsumption is not significantly different in period two. In period three, only the seven percentage point difference between UD and DU is significant under the FPP test ($p = 0.0572$) but not under the U-test ($p = 0.4969$).[31] Overall, we cannot reject that differences between PC and NPC consumption behavior is independent of the investment outcome. The difference in commitment does further not significantly change over time (Spearman's rank correlation = -0.0590, $p = 0.4675$).

Investment. We find no general pattern in the commitment difference regarding investment behavior. Collapsing over all paths by subjects, the average investment difference is -0.869 and we cannot reject that investment shares are the same under PC and under NPC (SR test, $p = 0.8539$, FPP test, $p = 0.5609$). In period one and two, we find no statistical difference in commitment.[32] In period three, the only difference that shows an effect that can be statistically distinguished from zero is the approximately ten percentage-point overinvestment in DD (SR test, $p = 0.1293$, FPP test, $p = 0.0938$).[33]

[31] U vs D: U-test, $p = 0.2821$, FPP test, $p = 0.3523$. DU vs DD: U-test, $p = 0.5637$, FPP test, $p = 0.1772$.

[32] Period one: SR test, $p = 0.3885$, FPP test, $p = 0.5098$. Path U: SR test, $p = 0.2658$, FPP test, $p = 0.4824$. Path D: SR test, $p = 0.5815$, FPP test, $p = 0.9158$.

[33] UU: SR test, $p = 0.8885$, FP test, $p = 1.000$. UD: SR test, $p = 0.1965$, FPP test $p = 0.1303$. DU: SR test, $p = 0.6658$, FPP test, $p = 0.8759$.

As with consumption behavior, we also find no indication that commitment differences in investment depend systematically on the investment outcome.[34] Further, the difference in investment behavior between NPC and PC does not systematically vary over time (Spearman's rank correlation = 0.0484, $p = 0.5523$).

Tables 3.C.9 and 3.C.10 report the results of the parametric regressions. For the consumption share, we see that the commitment difference (given by the constant) is significant when we only control for the investment outcome ($p < 0.01$). However, if we include the other control variables, the underconsumption vanishes. This effect does not differ across estimation techniques. For the investment share, we find no significant impact of any of the independent variables.

Result 3 *[Difference in Commitment] Overall, subjects underconsume under NPC relative to their PC choices but this effect is not robust against parametric estimation under the inclusion of control variables. Investment behavior is not distinguishable between PC and NPC. Commitment differences for both consumption and investment behavior do neither systematically vary in the investment outcome nor over time.*

3.5 Discussion

Standard preferences generally predict behavior well in the experiment. However, the significantly negative correlation between consumption shares and investment outcomes under PC and the significant underconsumption relative to the PC path are inconsistent with the model of Samuelson (1969). Hence, we present two alternative preference specifications to explain these results. We further discuss the internal validity of our design and our implementation of real consumption.

3.5.1 Non-Standard Preferences

The theoretical literature on consumption and portfolio choice is vast and there exist several alternative theories (*non-standard preferences*, DellaVigna 2009). Two of these preference specifications are noteworthy as they create variability of the choice variables with changes in wealth and may explain the two inconsistencies in our data. Expectations-based reference dependence as proposed by Köszegi and Rabin (2006; 2007; 2009) predict the negative correlation that we find under PC. External-habit preferences (e.g., Constantinides 1990; Brunnermeier and Nagel 2008) may help understand the slight underconsumption relative to the PC plan. A more detailed presentation of these preferences is provided in Appendix 3.D.

[34] U vs D: U-test, $p = 0.7147$, FPP test, $p = 0.6682$. UU vs UD: U-test, $p = 0.3782$, FPP test, $p = 0.3277$. DU vs DD: U-test, $p = 0.5804$, FPP test $p = 0.3361$.

Expectations-Based Reference Dependence. Expectations-based reference-dependent preferences have been developed by Köszegi and Rabin (2006; 2007; 2009) (henceforth, KR preferences). The preferences consist of two components: consumption utility and gain-loss utility. Consumption utility is based on the level of consumption, as with standard preferences. Gain-loss utility is based on a comparison of consumption with a reference point, as in prospect theory, and the agent is loss averse, i.e., losses hurt more than gains please (Kahneman and Tversky 1979). Köszegi and Rabin (2006) introduce a stochastic reference point that corresponds to the agent's rational beliefs formed in the previous period about the entire stream of future consumption. When the agent learns the investment outcome, he updates his beliefs and compares his previous and updated beliefs about present and future consumption experiencing gain-loss utility over what he has just learned in this comparison. Because all modifications from standard preferences involve psychological factors, KR preferences are directly testable with our experimental design.

KR preferences have been applied to consumption and asset pricing by Pagel (2012; 2014) who finds that consumption and investment shares are negatively correlated with the investment outcome. Hence, consumption adjusts only insufficiently to wealth fluctuations. The KR agent is more willing to lower future consumption than to fully adjust current consumption. This is because, learning that future consumption will be lower than previously expected is less painful than learning that current consumption is lower than expected. Furthermore, the reference point will have adjusted in the future, i.e., if the agent actually decreases his consumption share in some future period, he will by then have expected to lower it. Thus, expectations will have adjusted "downwards" (Barberis 2013).

Pagel (2012) shows that the agent has a positive demand for pre-commitment although to a different plan than under standard preferences. Because the KR agent's beliefs are endogenous, he considers how his choices determine his beliefs and thus his gain-loss utility when forming a pre-committed plan. Pagel (2012) shows that the agent's choice variables are negatively correlated with changes in wealth. In addition, relative to his pre-committed plan, the KR agent both overconsumes and overinvests and is thus time-inconsistent.

External Habit. Habit formation assumes that agents compare current absolute consumption to a certain reference level of consumption. Habit formation can be either external or internal, i.e., based on own past consumption. We follow Brunnermeier and Nagel (2008) and assume a simple preference specification with a constant external habit level, $X > 0$. The period utility function is given by $u(C_t, X) = (C_t - X)^{1-\theta}/(1-\theta)$.

The habit level can be interpreted as a reservation consumption level (Brunnermeier and Nagel 2008) or as cash flow that is put aside for future committed consumption (Chetty and Szeidl 2014). Both these interpretations are consistent with the use of internet as our consumption good. In contrast to experiments that induce habit formation directly (e.g., Brown, Chua, and Camerer 2009; Carbone and Duffy 2014), this is an ex-post explanation in our setting. However, if we reinterpret the transformation function (3.2.1) as generating consumption level $\tilde{C}_t = f(\text{Points})$, the habit agent's period utility would be given by $u(\tilde{C}_t - X)$.

External-Habit (EH) formation implies that relative risk aversion depends on wealth by introducing an additional precautionary savings motive. The EH agent has to make sure that he can always meet the habit level of consumption. Hence, he invests the present value of the habit into the risk free rate (Brunnermeier and Nagel 2008). This lowers the amount of "surplus wealth" that he can invest in the risky asset. On the one hand, if wealth is close to the habit level, the agent becomes more risk averse. On the other hand, if current wealth is much larger than the habit level, the agent becomes less risk averse. Hence, the investment share is positively correlated with wealth changes while the reaction of the consumption share depends on how close wealth is to the habit level (Munk 2008).

EH preferences can explain underconsumption if we make the additional assumptions that the agent has a "projection bias" and does not anticipate the formation of a habit when making a PC plan in period zero (Loewenstein, O'Donoghue, and Rabin 2003; Acland and Levy 2013). Relative to the pre-committed plan that coincides with the standard agent's plan, the EH agent underconsumes and underinvests if he is not pre-committed because now he has to ensure the unanticipated habit level in each period and has to cut consumption and investment in the risky asset in favor of storing funds in the risk-free asset.

Assessment. While the two non-standard preference specifications can explain the two inconsistencies ex-post, they make a series of further predictions for which we find no evidence in our data. Regarding KR preferences, the result on PC consumption is very interesting. We are not aware of any other experiment that tests KR preferences in a life-cycle context. However, Pagel (2012) provides a series of other predictions, e.g., that consumption and investment shares are decreasing in the investment outcome both under PC and NPC, that subjects are time-inconsistent, and that the commitment difference varies with changes in wealth. We find no evidence for these implications. Because our design allows to jointly test these predictions, we conclude that KR preferences poorly describe consumption and investment behavior in our setting.

Regarding EH preferences, we also find no evidence for its main implication that investment shares vary with the investment outcome. According to Brunnermeier and Nagel (2008, p. 713), habit formation is the "most popular approach" to model time-varying relative risk aversion which can explain some "stylized facts about asset returns and the business cycle" such as the level of risk premia and their negative correlation with the business cycle (Constantinides 1990; Campbell and Cochrane 1999). However, as Brunnermeier and Nagel (2008) who test the positive correlation of investment behavior and wealth changes with PSID data, we find no significant correlation between investment shares and the investment outcome. Furthermore, the assumption of a prediction bias is rather ad-hoc and we are not aware of any study that tests this idea in an investment context.

Although not robust, the observed underconsumption is in contrast to a large literature that shows that people have self-control problems and behave inconsistently over time in the sense that they prefer immediate gratification (see Frederick, Loewenstein, and O'Donoghue 2002 for a survey). We are not aware of any other experiment that compares PC and NPC life-cycle choices. Carbone and Infante (2014) provide a different explanation for the difference in commitment and argue that ambiguity about changes in wealth explains underconsumption in a life-cycle experiment. However, in our experiment, probabilities were objectively given both under PC and under NPC. Hence, we also rule out probability weighting as an alternative explanation.

3.5.2 Internal Validity

Our data stem from the observed choices of two basically metric choice variables in a within-subject design. Within-subject designs generally have more power than between-subject designs in which subjects only participate in one condition. This is because each subject acts as his or her own control. The number of subjects in our setting is also relatively large such that we can utilize between 51 and 60 independent observations per period as there is no interaction among subjects. The fact that choice variables were rarely clustered on the boundaries supports the theoretical result of an interior solution.

We base our experimental design on the assumption that internet is a good consumption proxy which means that subject's draw utility from internet and that utility is monotonically increasing in internet time.[35] We do not observe subject's actual utility but rather induce it in a way that is standard in life-cycle experiments. This section presents the result of the willingness-to-pay (WTP) measure that we elicited in the post-experimental questionnaire. We argue that internet is a valid consumption proxy for two reasons.

[35] See also footnote 9.

First, we asked subjects to imagine another 19-minute period of experimentation in the WTP elicitation. Subjects had to indicate how much of an initial 10-Euro endowment they were willing to spend to buy a certain amount of internet time. Figure 3.C.8 presents an aggregate demand curve for internet time derived from the hypothetical WTP elicitation. As expected, WTP is significantly increasing in internet time (Spearman's rank correlation = 0.2057, $p = 0.0000$). Although choices are hypothetical, this increase indicates that internet is a suitable consumption good fulfilling the requirements of monotonicity and dominance as required by Smith (1976). We find no indication for a non-linear relationship by regressing WTP on minutes, squared minutes or cubed minutes even after controlling for demographics. Note that the questionnaire was asked after approximately three hours of experimentation and thus marginal utility could have been different at the beginning of the experiment. However, we see this as a justification of our design choice to use internet as an actual consumption good.

Second, in the experiment, internet is always more pleasurable than the alternative clicking task. At least, subjects do not risk losing money while surfing the web as the clicking task is deactivated during that time. In addition, comments in the post-experimental questionnaire pointed out that subjects perceived the task as extremely boring. Furthermore, we think that our design gains additional reliability from the experiment of Houser et al. (2010) as they find that a significant number of subjects choose to pay to eliminate the temptation to abandon a boring task and surf the internet instead. Using the internet as a mean to simulate a pleasurable good is a reasonable choice given the various opportunities it provides and the student population we sample from.

3.6 Conclusion

How people vary their consumption and investment behavior with fluctuations in wealth and how they vary their decisions with the passage of time is important for economic policy and fiscal intervention, e.g., to prevent or recuperate from a recession. Furthermore, there is a global trend towards granting more self determination in financial planning and a discussion about how policymakers should help people to achieve economic security throughout their lives. Proposals include default enrollment in saving plans, default investment options, and managed institutional investment to achieve optimal portfolio allocations (Mitchell and Utkus 2006).

However, analyzing individual choices with observational data sets is subject to measurement and aggregation errors, problems in the definition of dependent and independent variables, and (in terms of panel data) attrition rates. In this paper, we design an experiment to test the consumption and investment behavior in a life-cycle environment with on-the-spot consumption of a real consumption good. This allows a direct test of the implications of life-cycle theories that model utility from consumption rather than wealth and that assume that current utility is actually realized in a given period rather than paid out at the end of the "life cycle". While our main contribution is primarily methodological, we use our design to analyze the predictive power of the standard life-cycle consumption and portfolio-choice model of Samuelson (1969).

In the experiment, we differentiate behavior under pre-commitment (PC) and under non-pre-commitment (NPC). Overall, standard preferences predict consumption and investment behavior well. Under PC, investment plans are unaffected by anticipated changes in wealth or the passage of time. Actual choices under NPC are also consistent with these implications based on standard preferences, e.g., investment shares do not systematically vary over time. These findings are in line with the results in Brunnermeier and Nagel (2008) who test investment behavior with PSID data. However, as Liu, Yang, and Cai (2014) point out, empirical investigation may suffer from misidentification problems if additional income shocks are ignored or misspecified. Hence, we complement the findings of Brunnermeier and Nagel (2008) with the controlled environment of the lab. However, we report two findings for consumption behavior that are inconsistent with standard preferences. First, subjects under PC show a significantly negative correlation between consumption shares and investment outcomes. Second, subjects underconsume under NPC relative to their previously formed PC plan.

We present two alternative non-standard preference specifications to explain these findings. Expectations-based reference dependence (Köszegi and Rabin 2006; 2007; 2009; Pagel 2012; 2014; KR preferences) and external habit (Constantinides 1990; Brunnermeier and Nagel 2008; EH preferences). While KR preferences can explain the PC consumption finding, EH preferences can explain underconsumption if we assume that subjects are naive and do not anticipate the formation of habits. However, this effect is not robust against controlling for demographics and the naiveté rests on the assumption of projection bias (Loewenstein, O'Donoghue, and Rabin 2003). Further research is needed to evaluate if and how subjects predict habit formation in life-cycle contexts. Furthermore, both alternative preferences also imply a range of behavioral responses particularly to wealth changes. We find no evidence for these other predictions and hence conclude that choices in the experiment are overall best described by standard preferences.

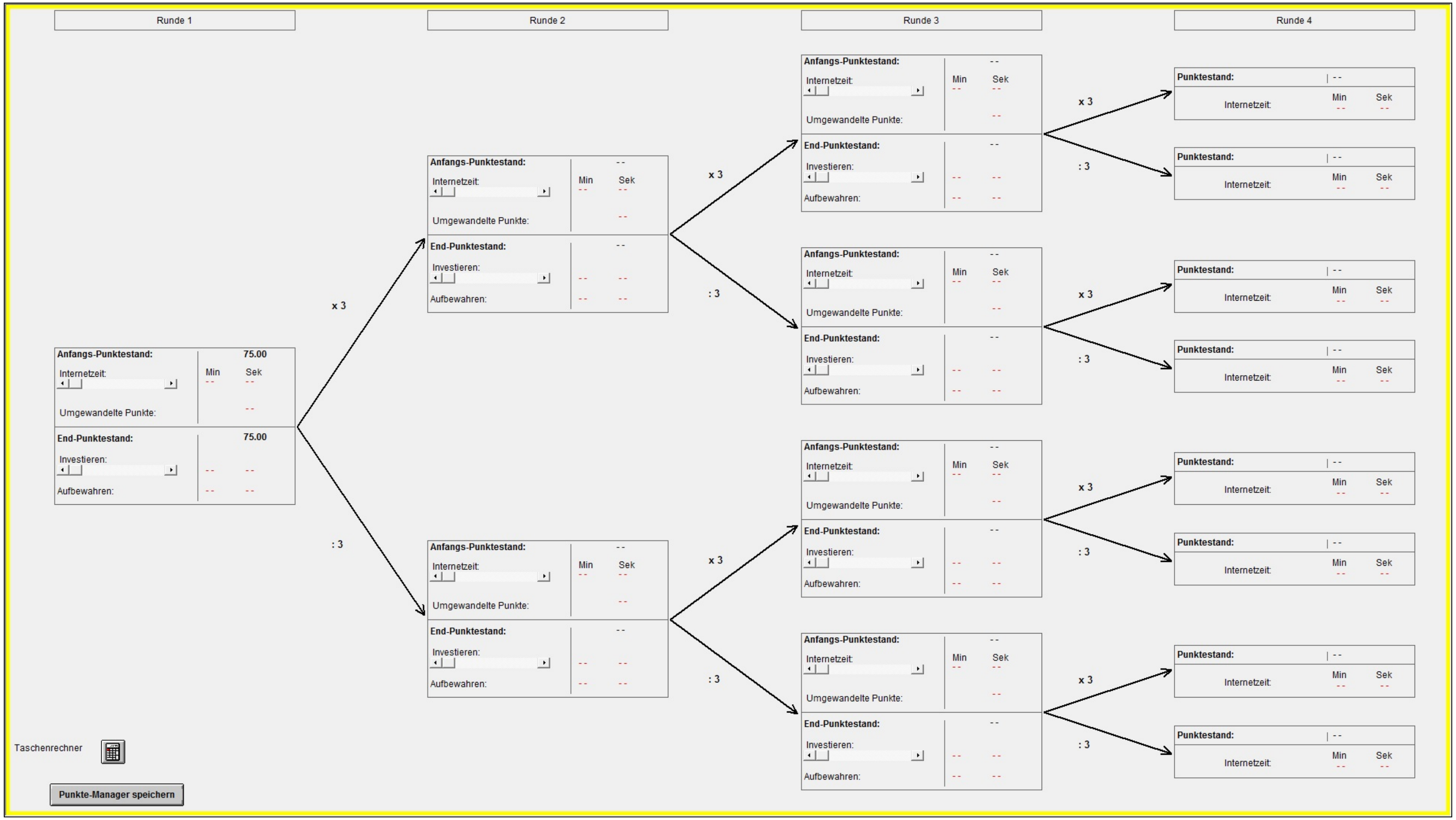
Our results should be interpreted with some caution. We only consider a four-period environment while our main data is generated by periods two and three where subjects react to the investment outcome. This is a caveat in our implementation of on-the-spot consumption and the resulting time restrictions. We think a longer time of experimentation cannot be imposed on the subjects due to cognitive and physical depletion. Furthermore, while our within-subject design has more power than a between-subjects design, it may be worthwhile to consider a separation of pre-committed to non-pre-committed choices between treatment groups to prevent any carry-over effects. Nevertheless, we think that the fact that all subjects had to form a possibly pre-committing plan ex-ante greatly increased the understanding of the dynamic structure of the decision problem. Subjects learned how their choices map into utility in future rounds and our implementation automatically enforced the budget constraint. We think that this design greatly reduces possible decision errors due to myopic behavior (Ballinger, Palumbo, and Wilcox 2003; Carbone and Hey 2004) and the misunderstanding or ignorance of the intertemporal dependence of choices (Fehr and Zych 1998; Brown, Chua, and Camerer 2009; Carbone and Hey 2004).

Our design is an important step towards controlled research of dynamic theories about consumption and portfolio choice. We proxy consumption by utilizing the internet because the web is likely to generate immediate utility and to fulfill the requirement of monotonicity, salience, and dominance necessary to be used effectively in economic experiments (Smith 1976). Further, experiments frequently sample from a student population who generally consume internet time on a daily basis. Our design is easy to implement and needs little explanation while offering the intertemporal structure of life-cycle models. Further modifications to test policy interventions such as taxation or nudging ideas (e.g., automatic enrollment and saving plans) can easily be implemented and tested in our experimental setting. In addition, the PC stage allows to test proposals such as delegated portfolio choice by letting subjects choose from a set of pre-specified plans.

Appendices

3.A Screens in the Lab Experiment

This section shows screen shots of the implementation of the experiment. The point manager is part of the pre-commitment stage (see Section 3.2.2). The NPC decision box is part of the consumption-and-allocation stage for those subjects that are not pre-committed to their decisions made in the point manager (see Section 3.2.2). The browser size is adjusted such that the consumed internet time is visible.

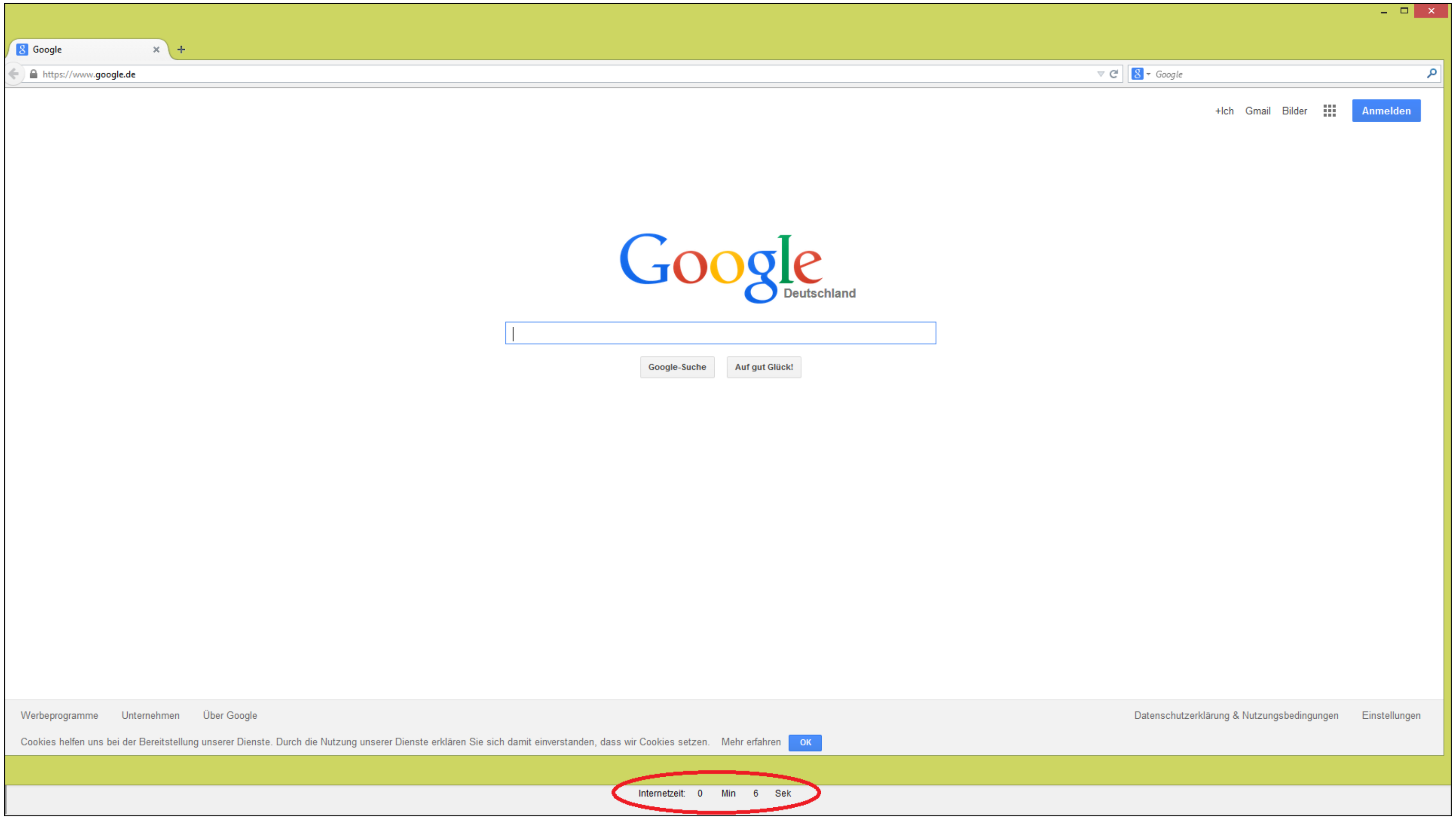


Notes: Depicted is the point manager used in the pre-commitment (PC) stage. Subjects choose their consumption and investment shares by positioning scroll bars. The point manager elicits a complete consumption plan for every possible path that could realize in the experiment. For an enlarged picture of an individual decision box along with a detailed discussion see Figure 3.1 and Section 3.2.2. All remaining wealth is automatically consumed in period four. Screen size and resolution: 23", Full HD 1920x1080.

Figure 3.A.5: Point Manager.

Notes: Depicted is the decision box used at the beginning of period one to three to elicit choices under non-pre-commitment (NPC). The first line displays current wealth W_t^{NPC} (**"starting wealth"**) at the beginning of the period. In contrast to the decision box under pre-commitment (PC), line two (**"internet time"**), line three (**"transformed points"**), and line four (**"end wealth"**) are left blank because this information depends on the consumption that the subject chooses after she made her decisions regarding the investment share. However, to ease handling for the subjects, PC and NPC decision boxes are designed to look as similar as possible. In line five, subjects choose their investment share α_t^{NPC} (**"investing"**). The sixth line displays the share kept in the risk-free asset $1 - \alpha^{\text{NPC}}$ (**"storing"**).

Figure 3.A.6: NPC Decision Box.



Notes: Depicted is the internet explorer used in the allocation-and-consumption stage for both pre-committed (PC) and non-pre-committed (NPC) subjects. Because the browser did not cover the entire screen, subjects were able to see how much internet time they had already consumed at the bottom (marked area). Screen size and resolution: 23”, Full HD 1920x1080.

Figure 3.A.7: Internet Browser.

3.B Instructions

The following pages report the instructions in both German (original) and English (translated). Upon their arrival subjects received the general instructions. Subsequently, they received the instructions for part one. After part one was finished, they received the instructions for part two and some comprehension questions.

Allgemeine Instruktionen

Herzlich willkommen und vielen Dank für die Teilnahme an diesem Experiment. Bitte lesen Sie die Instruktionen sorgfältig durch. Stellen Sie ab jetzt bitte jegliche Kommunikation ein. Falls Sie Fragen haben, melden Sie sich per Handzeichen. Wir kommen dann zu Ihnen und helfen Ihnen weiter.

Es ist bei dem heutigen Experiment nicht erlaubt private Unterlagen (Bücher, Vorlesungsskripte etc.) am Platz zu haben oder ein Mobiltelefon oder Ähnliches zu benutzen. Stellen Sie bitte Ihre Taschen und andere Gegenstände hinter sich. Schalten Sie Ihr Mobiltelefon aus und achten Sie darauf, dass kein Weckruf eingestellt ist. Bitte legen Sie Ihr Mobiltelefon und ähnliche Geräte in den Umschlag, der zu Ihrem Platz gehört. Kleben Sie den Umschlag nicht zu. Dies übernimmt ein Experimentator. **Der Umschlag bleibt während des gesamten Experiments bei Ihnen am Platz und darf nicht geöffnet werden**. Bei einem Verstoß gegen diese Regel werden Sie von diesem Experiment und allen Auszahlungen ausgeschlossen.

In diesem Experiment können Sie Geld erhalten. Die Höhe Ihrer Auszahlung hängt nur von Ihren eigenen Entscheidungen ab und wird **nicht** durch die Entscheidungen anderer Teilnehmer beeinflusst.

Alle Entscheidungen, die Sie während des Experiments treffen, sind anonym. Ihre Auszahlung wird vertraulich behandelt.

Das Experiment besteht aus **zwei Teilen**. Sie erhalten zunächst die Instruktionen für Teil 1. Danach startet Teil 1. Nachdem dieser abgeschlossen ist, erhalten Sie die Instruktionen für Teil 2. Im Anschluss erhalten Sie einige Verständnisfragen und Teil 2 startet.

Als Pauschale für Ihr Erscheinen erhalten Sie am Ende des Experiments 2.50 EUR zu Ihrer Auszahlung hinzu. Zu Beginn des Experiments erhalten Sie außerdem eine **Anfangsausstattung** von **30 EUR**. Ihre Entscheidungen in diesem Experiment bestimmen, wie viel Ihnen am Ende von diesen 30 EUR ausbezahlt wird. In beiden Teilen (Teil 1 und Teil 2) kann Ihnen Geld von der Anfangsausstattung abgezogen werden.

Im Anschluss an Teil 2 des Experiments bitten wir Sie, einen Fragebogen auszufüllen, während wir die Auszahlungen vorbereiten. Alle dort angegebenen Informationen sowie alle während dieses Experiments erhobenen Daten werden anonymisiert und ausschließlich für wissenschaftliche Zwecke verwendet. Nachdem Sie den Fragebogen ausgefüllt haben, warten Sie bitte an Ihrem Platz bis Ihre Platznummer zur Auszahlung aufgerufen wird.

Instruktionen – Teil 1

In Teil 1 absolvieren Sie eine „**Klick-Aufgabe“**. Ihre Aufgabe ist es, über eine Dauer von **10 Minuten** zufällig erscheinende Pop-up-Fenster durch Klicken zu schließen.

Ablauf der Klick-Aufgabe

Während der Klick-Aufgabe erscheinen auf dem Bildschirm nacheinander Pop-up-Fenster. Wann und wo auf dem Bildschirm die Fenster erscheinen ist **zufällig**. Nach Erscheinen eines Fensters haben Sie **10 Sekunden** Zeit, das Fenster zu schließen. Sollten Sie es nicht schließen, verschwindet das Fenster nach Ablauf der 10 Sekunden von alleine.

Die Klick-Aufgabe ist **Bestandteil von Teil 1 und Teil 2** des Experiments. Alle Fenster, die Sie in Teil 1 und Teil 2 zusammen nicht geschlossen haben, ergeben **Kosten**. Diese Kosten verringern Ihre Auszahlung. Ihre Auszahlung hängt also von der Gesamtzahl der nicht geschlossenen Fenster ab. Am Ende des Experiments erfahren Sie, wie viele Fenster Sie insgesamt nicht geschlossen haben.

Der Zusammenhang zwischen der Gesamtzahl nicht geschlossener Fenster und den zugehörigen Kosten ist in der folgenden Tabelle aufgelistet:

Gesamtzahl nicht geschlossener Fenster (Teil 1 und 2 zusammen)	**Kosten in EUR**	**Gesamtauszahlung am Ende des Experiments in EUR**
0	0	32.50
1	1	31.50
2	4	28.50
3	9	23.50
4	16	16.50
5	25	7.50
ab 6	30	2.50

Instruktionen – Teil 2

2.1 Allgemeiner Ablauf

In Teil 2 gibt es **4 Runden**. In jeder dieser Runden gibt es einen Zeitraum von **19 Minuten**. Zu Beginn dieser 19 Minuten können Sie im **Internet** surfen. Die restliche Zeit müssen Sie die **Klick-Aufgabe** absolvieren. Die Klick-Aufgabe startet erst, nachdem Sie nicht mehr im Internet sind. Das heißt, während Sie im Internet surfen erscheinen **keine** Fenster und es kann Ihnen kein Geld abgezogen werden. Wie viel Internetzeit Sie zur Verfügung haben, hängt von Ihren Entscheidungen in diesem Teil ab.

Zu Beginn von Runde 1 erhalten sie einmalig eine Ausstattung von 75 Punkten. Mit diesen Punkten können Sie Internetzeit kaufen. Das bedeutet, Internetzeit wird in Punkte umgewandelt und von Ihrem Punktestand abgezogen.

Punkte, die Sie in einer Runde nicht für Internetzeit nutzen, stehen Ihnen in späteren Runden zur Verfügung. Sie können diese Punkte in die nächste Runde überführen, indem Sie die Punkte entweder **investieren** oder **aufbewahren**. Durch Investieren können Sie mehr Punkte hinzu gewinnen, aber auch Punkte verlieren. Wie genau dies funktioniert, wird weiter unten erklärt. Punkte, die Sie aufbewahren, stehen Ihnen unverändert in der nächsten Runde zur Verfügung.

Vor dem Start von Runde 1 erscheint auf dem Computer ein **Punkte-Manager**. Mit diesem können Sie Ihre Punkteaufteilung **für alle vier Runden planen**. Mit dem Punkte-Manager legen Sie also Ihre Aufteilung für die gesamten **76 Minuten** (= 4 Runden x 19 Minuten) vor dem Start von Runde 1 fest.

Nachdem alle Teilnehmer ihren Punkte-Manager vollständig ausgefüllt haben, startet Runde 1 von 4.

Beachten Sie:

- Die Internetverbindung läuft über das Netzwerk der Universität zu Köln. Wir werden den Verlauf der von Ihnen online besuchten Seiten **nicht** speichern oder auswerten. Es werden **keine** Passwörter oder Ähnliches gespeichert.
- Es kann aus technischen Gründen zu Ladezeiten kommen (z.B. beim Öffnen/Schließen des Internet-Browsers).
- Die Punkte können **nicht** in Geld umgewandelt werden, sondern dienen dazu, Zeit im Internet zu verbringen. Bei der Klick-Aufgabe kann Ihnen **weiterhin Geld abgezogen werden**, wenn Sie die Fenster nicht schließen.
- Auf Ihrem Bildschirm werden Zahlen bis auf zwei Nachkommastellen angezeigt. Der Computer rechnet jedoch mit den exakten Werten.

2.2 Der Inhalt des Punkte-Managers

Der Punkte-Manager besteht aus einzelnen **Entscheidungs-Boxen**, in denen Sie Ihre Entscheidung für eine Runde treffen. Eine Entscheidungs-Box sieht wie folgt aus:

Anfangs-Punktestand:

Dies sind die Punkte, die Sie am Anfang der Runde zur Verfügung haben. Maximal diese Punkte können Sie als Internetzeit nutzen.

Internetzeit und Umgewandelte Punkte

Hier legen Sie fest, wie viel Zeit Sie in einer Runde im Internet verbringen möchten. Die gewählte Zeit wird vom Computer automatisch in Punkte umgerechnet. Die Anzahl umgewandelter Punkte steigt dabei in der Höhe der Internetzeit überproportional. Das bedeutet, doppelt so viel Zeit erfordert mehr als doppelt so viele Punkte.[1] Sie können nicht länger als 19 Minuten pro Runde im Internet verbringen.

Einige Beispielwerte:

Internetzeit (Min:Sek)	Umgewandelte Punkte
0:00	0
2:00	0.67
4:00	1.87
6:00	4.08
8:00	8.31
10:00	16.76
12:00	34.40
14:00	73.28
16:00	164.38
18:00	393.80

End-Punktestand:

Dies sind die Punkte, die Sie am Ende einer Runde noch übrig haben. Diese Punkte errechnen sich wie folgt:

$$\text{End-Punktestand} = \text{Anfangs-Punktestand} - \text{Umgewandelte Punkte}.$$

Ihren End-Punktestand können Sie nun Investieren und/ oder Aufbewahren.

[1] Der Computer verwendet zur Umrechnung: $\text{Punkte} = \left(\frac{2400}{2400 - \text{Internetzeit (in Sek.)}}\right)^{10} - 1.$

Investieren

Hier wählen Sie den Anteil (in %) der Punkte Ihres End-Punktestands, den Sie investieren möchten. Durch Investieren können Sie Punkte hinzugewinnen oder verlieren. Die Punkte, die Sie investieren, werden zu Beginn der darauffolgenden Runde entweder **gedrittelt** (**: 3**) oder **verdreifacht** (**x 3**). Dies wird für jeden Teilnehmer am Platz ausgewürfelt. Beide Möglichkeiten sind dabei **gleich wahrscheinlich**.

Würfelergebnis	**Investierte Punkte werden**
1, 2, 3	gedrittelt
4, 5, 6	verdreifacht

Aufbewahren

Der Anteil (in %) Ihres End-Punktestands, den Sie nicht investieren, wird automatisch aufbewahrt. Die aufbewahrten Punkte stehen Ihnen in der nachfolgenden Runde **unverändert** zur Verfügung.

2.3 Punkte-Manager ausfüllen

Investierte Punkte werden entweder gedrittelt oder verdreifacht. Somit gibt es in der nachfolgenden Runde mehrere mögliche Punktestände. Der Punkte-Manager stellt all diese möglichen Rundenverläufe und die jeweils dazu gehörenden Entscheidungs-Boxen dar. **In Runde 4 wandelt der Punkte-Manager alle verbleibenden Punkte automatisch in Internetzeit um**. Im Punkte-Manager teilen Sie also – bevor Runde 1 startet – die Punkte für jeden möglichen Verlauf des Experiments auf.

Bitte nehmen Sie Abbildung 1 (Punkte-Manager) auf dem Zusatzblatt zur Hand.

Der Punkte-Manager ist wie folgt zu lesen:

- Runde 1: Dies ist die erste Runde. Somit gibt es nur eine Entscheidungs-Box.
- Runde 2: Für die investierten Punkte in Entscheidungs-Box 1 gibt es zwei Möglichkeiten: dritteln oder verdreifachen. Somit gibt es in Runde 2 insgesamt zwei Entscheidungs-Boxen.
- Runde 3: Jede Entscheidungs-Box aus Runde 2 hat wieder zwei Möglichkeiten für die dort investierten Punkte. Somit gibt es in Runde 3 insgesamt vier Entscheidungs-Boxen.
- Runde 4: Nach dem gleichen Prinzip gibt es in Runde 4 insgesamt acht Möglichkeiten. Da dies die letzte Runde ist, müssen Sie keine Entscheidung mehr treffen. Der Computer wandelt automatisch alle verbleibenden Punkte in Internetzeit um.

Wenn Sie den Punkte-Manager vollständig ausgefüllt haben und nichts mehr ändern möchten, bestätigen Sie Ihre Wahl mit einem Klick auf „**Punkte-Manager speichern**".

Beachten Sie: Wenn Sie die Aufteilung der Punkte in einer Entscheidungs-Box ändern, so ändern sich auch die Aufteilungen in den nachfolgenden Entscheidungs-Boxen. Überprüfen Sie daher bitte, bevor Sie auf „Punkte-Manager speichern" klicken, dass die Punkte an allen Stellen des Punkte-Managers so verteilt sind, wie Sie es wollen. Sie können in einer Runde nicht mehr Punkte aufteilen, als zur Verfügung stehen.

2.4 Auswürfeln, ob Punkte-Manager aktiv bleibt

Nachdem Sie den Punkte-Manager ausgefüllt haben, wird ausgewürfelt, ob dieser für alle vier Runden aktiv bleibt, oder ob er deaktiviert wird. Ob der Punkte-Manager aktiv bleibt, wird für jeden Teilnehmer am Platz ausgewürfelt.

Würfelergebnis	Punkte-Manager
1	aktiv
2, 3, 4, 5, 6	deaktiviert

a) Punkte-Manager bleibt aktiv:

Bleibt Ihr Punkte-Manager **aktiv**, durchlaufen Sie das gesamte Experiment nach dem dort von Ihnen gewählten Schema. Sie können den Punkte-Manager im Nachhinein **nicht mehr verändern**. Er gilt für die vollen 76 Minuten.

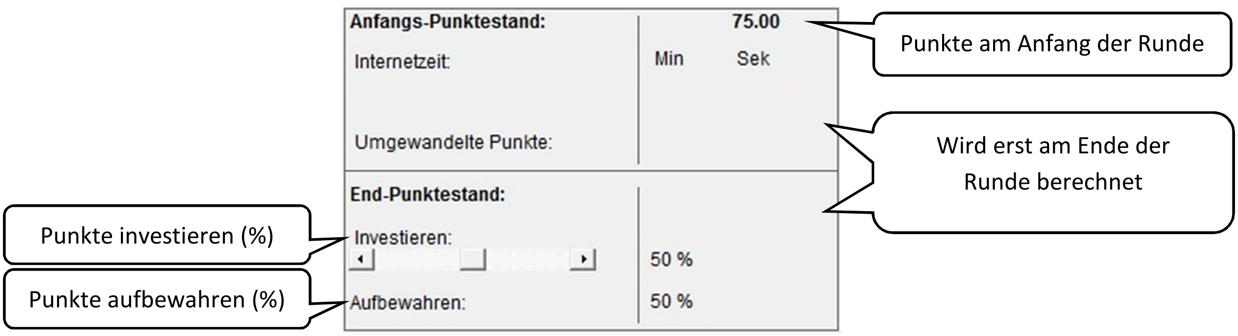
Je nach Investitionsergebnis (dritteln/ verdreifachen) wählt der Computer die entsprechende Entscheidungs-Box aus Ihrem Punkte-Manager automatisch aus. Zuerst verbringen Sie die von Ihnen gewählte Zeit im Internet, anschließend startet für den Rest der 19 Minuten die Klick-Aufgabe. Der Computer öffnet und schließt den Internet-Browser automatisch und startet die Klick-Aufgabe ebenfalls automatisch.

b) Punkte-Manager wird deaktiviert:

Wird der Punkte-Manager **deaktiviert**, treffen Sie in jeder Runde eine neue Aufteilung. Zu Beginn jeder Runde sehen Sie eine Entscheidungs-Box. In dieser sehen Sie Ihren Anfangs-Punktestand mit dem Sie in dieser Runde starten. Allerdings wählen Sie in der Entscheidungs-Box keine Internetzeit. Stattdessen startet nach der Entscheidungs-Box der Internet-Browser und Sie entscheiden, wie viel Zeit Sie im Internet verbringen möchten.

Wenn Sie das Internet beenden möchten, schließen Sie einfach den Internet-Browser. Sie gelangen dann zurück auf den Experimentbildschirm. Ihre Internetzeit wird jedoch erst dann gestoppt, wenn Sie dort auf **"Internetzeit stoppen/Klick-Aufgabe starten"** klicken. Anschließend machen Sie für den Rest der 19 Minuten mit der Klick-Aufgabe weiter. Der Computer wandelt die gestoppte Internetzeit automatisch in Punkte um. Sollte Ihr Anfangs-Punktestand zwischendurch aufgebraucht sein, wird der Internet-Browser automatisch geschlossen.

Da Sie erst nach der Entscheidungs-Box im Internet surfen, ist zu Beginn einer Runde also noch nicht bekannt, wie viele Punkte umgewandelt werden. Deshalb werden in der Entscheidungs-Box auch keine Internetzeit oder umgewandelten Punkte angezeigt. Ebenso kann auch noch kein End-Punktestand berechnet werden. Sie wählen in der Entscheidungs-Box also nur den Anteil (in %) Ihres End-Punktestands, den Sie investieren möchten.



Zusammenfassung/ Ablauf

- Punkte-Manager für alle möglichen Verläufe ausfüllen
- Auswürfeln, ob Punkte-Manager aktiv bleibt
  a. Punkte-Manager **aktiv**:
  Ihre Punkte werden in jeder Runde automatisch so aufgeteilt, wie Sie dies im Punkte-Manager angegeben haben. Der Internet-Browser öffnet und schließt automatisch. Die Klick-Aufgabe startet automatisch.
  b. Punkte-Manager **deaktiviert**:
  Zu Beginn jeder Runde erscheint eine neue Entscheidungs-Box und Sie teilen Ihren (zu dem Zeitpunkt noch nicht berechneten) End-Punktestand für diese Runde auf. Der Internet-Browser öffnet automatisch. Um die Internetzeit zu stoppen und mit der Klick-Aufgabe weiter zu machen, schließen Sie den Internet-Browser und klicken auf „Internetzeit stoppen/Klick-Aufgabe starten".

- Runde 1
  a. Punkte Manager aktiv: Entscheidungs-Box 1 gilt
  Punkte-Manager deaktiviert: Entscheidungs-Box für Runde 1 ausfüllen
  b. 19 Minuten starten (zuerst Internet, danach Klick-Aufgabe)

- Runde 2
  a. In Runde 1 investierte Punkte werden entweder gedrittelt oder verdreifacht (Würfel)
  b. Punkte-Manager aktiv: Entscheidungs-Box 2a oder 2b gilt (siehe Abbildung 1)
  Punkte-Manager deaktiviert: Entscheidungs-Box für Runde 2 ausfüllen
  c. 19 Minuten starten (zuerst Internet, danach Klick-Aufgabe)

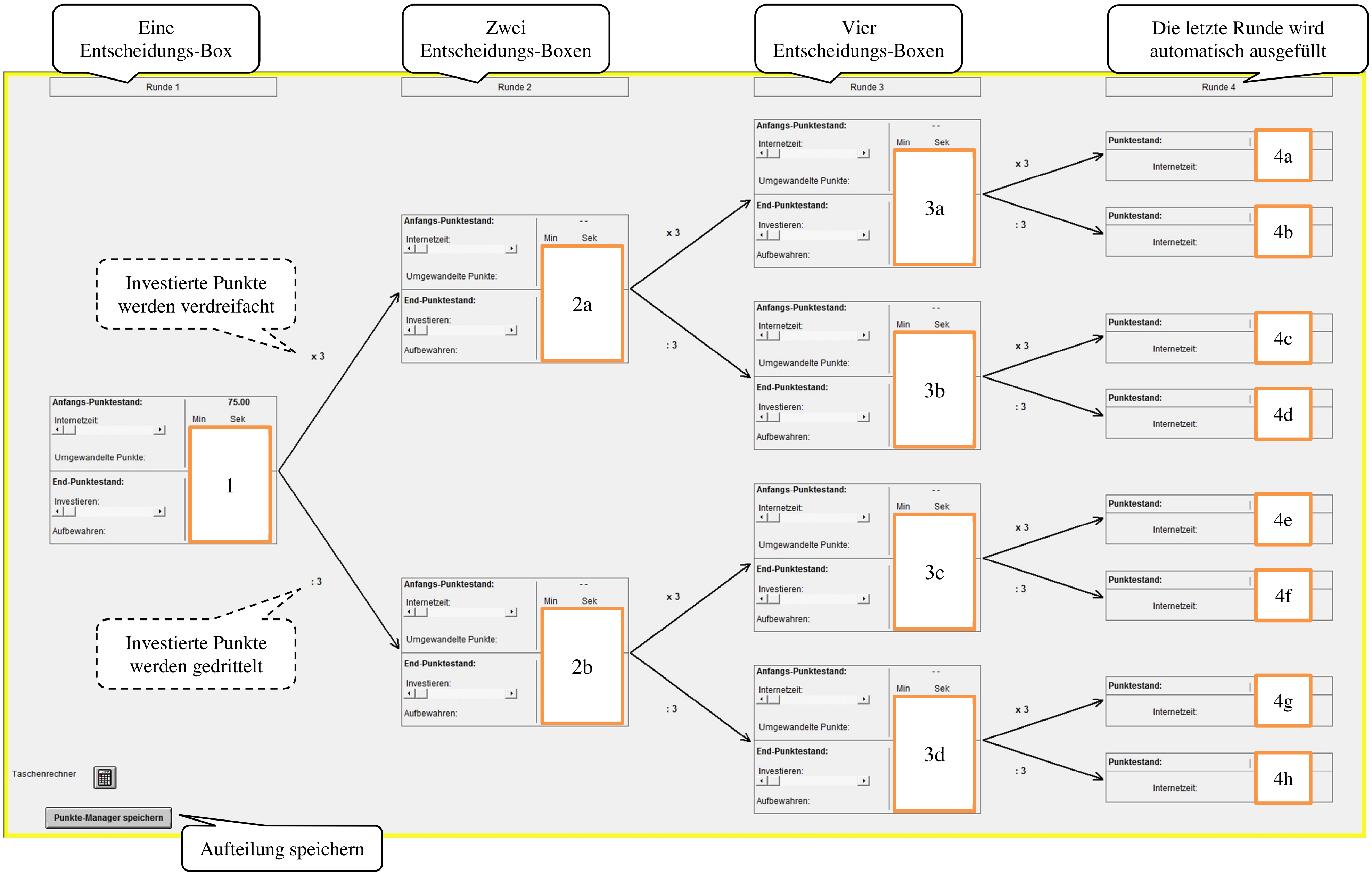
- Runde 3
  a. In Runde 2 investierte Punkte werden entweder gedrittelt oder verdreifacht (Würfel)
  b. Punkte-Manager aktiv: Entscheidungs-Box 3a, 3b, 3c oder 3d gilt (siehe Abbildung 1)
  Punkte-Manager deaktiviert: Entscheidungs-Box für Runde 3 ausfüllen
  c. 19 Minuten starten (zuerst Internet, danach Klick-Aufgabe)

- Runde 4
  a. In Runde 3 investierte Punkte werden entweder gedrittelt oder verdreifacht (Würfel)
  b. Punkte-Manager aktiv: Entscheidungs-Box 4a, 4b, 4c, 4d, 4e, 4f, 4g oder 4h gilt (siehe Abbildung 1)
  Punkte-Manager deaktiviert: Verbleibender Punktestand wird automatisch in Internetzeit umgewandelt. Internet-Browser öffnet und schließt in dieser Runde automatisch. Die Klick-Aufgabe startet in dieser Runde automatisch.
  c. 19 Minuten starten (zuerst Internet, danach Klick-Aufgabe)

Zur Erinnerung: Punkte werden nicht in Geld umgewandelt. Bei der Klick-Aufgabe kann Ihnen jedoch weiterhin Geld abgezogen werden.

Zusatzblatt

Abbildung 1: Punkte-Manager



Verständnisfragen

Bitte füllen Sie die folgenden neun Verständnisfragen aus. Diese sollen Ihnen helfen, die Instruktionen richtig zu verstehen. Nachdem alle Teilnehmer die Verständnisfragen richtig beantwortet haben, startet der Punkte-Manager. Sollten Sie Fragen haben, heben Sie bitte die Hand.

1. Fenster, die Sie im Laufe des gesamten Experiments nicht geschlossen haben, werden addiert und verursachen Kosten, die Ihre Auszahlung verringern.
   Nehmen Sie an, Sie haben in Teil 1 des Experiments ein Fenster nicht geschlossen. In Teil 2 haben Sie in Runde 2 ebenfalls ein Fenster nicht geschlossen und in Runde 4 haben Sie zwei Fenster nicht geschlossen.
   Wie viele Fenster haben Sie insgesamt nicht geschlossen?
   _______________ Fenster
   Wie hoch sind die Kosten, die dadurch entstehen?
   _______________ EUR
   Wie viel Geld wird Ihnen abgezogen, wenn Sie insgesamt fünf Fenster nicht geschlossen haben?
   _______________ EUR

2. Die Punkte, die Sie investieren, werden in der nächsten Runde gedrittelt oder verdreifacht.
   Wenn eine 3 gewürfelt wird, werden die investierten Punkte _______________________
   Wenn eine 4 gewürfelt wird, werden die investierten Punkte _______________________

3. Wenn Sie 12:00 Minuten im Internet verbringen möchten, wie viele Punkte würde der Computer dann von Ihrem Anfangs-Punktestand in dieser Runde abziehen?
   _______________ Punkte
   Nachdem diese Internetzeit aufgebraucht wurde, absolvieren Sie für die restliche Zeit die Klick-Aufgabe. Wie lange dauert diese nun an?
   _____ Min _____ Sek.

4. Nehmen Sie an, Sie haben in einer Runde einen Anfangs-Punktestand von 100 Punkten und Sie verbrauchen 40 Punkte für Internetzeit. Wie hoch ist Ihr End-Punktestand in dieser Runde?
   _______________Punkte
   Von diesem End-Punktestand investieren Sie 50% und bewahren 50% auf.
   Wie viele Punkte investieren Sie somit? _______________Punkte
   Wie viele Punkte bewahren Sie somit auf? _______________Punkte
   Wie hoch ist Ihr Anfangs-Punktestand in der nächsten Runde, wenn eine 2 gewürfelt wird?
   _______________Punkte
   Wie hoch ist Ihr Anfangs-Punktestand in der nächsten Runde, wenn eine 5 gewürfelt wird?
   _______________Punkte

5. Wie viele Möglichkeiten und damit Entscheidungs-Boxen ergeben sich in Runde 3?
   _____ Entscheidungs-Boxen

6. Nehmen Sie an, Ihr Punkte-Manager bleibt **aktiv**.
   Sie befinden sich in Runde 3. Die investierten Punkte aus Runde 1 wurden verdreifacht. Die investieren Punkte aus Runde 2 wurden gedrittelt.
   Welche Entscheidungs-Box ist nun erreicht worden? (siehe Abbildung 1: Punkte-Manager)
   Nr. _____

7. Nehmen Sie an, Ihr Punkte-Manager bleibt **aktiv**.
   Sie befinden sich in Runde 4. Die investierten Punkte aus Runde 1 wurden gedrittelt. Die investieren Punkte aus Runde 2 wurden verdreifacht. Die investierten Punkte aus Runde 3 wurden gedrittelt.
   Welche Entscheidungs-Box ist nun erreicht worden? (siehe Abbildung 1: Punkte-Manager)
   Nr. _____

8. Nehmen Sie an, Ihr Punkte-Manager wurde **deaktiviert**.
   Sie befinden sich in Runde 3 und haben einen Anfangs-Punktestand von 50 Punkten. Zu Beginn müssen Sie die Entscheidungs-Box für diese Runde ausfüllen. Sie investieren 50% Ihres End-Punktestands und bewahren 50% auf. Anschließend klicken Sie auf „Weiter“ und der Internet-Browser wird geöffnet. Nachdem Sie den Internet-Browser geschlossen haben, klicken Sie auf „Internetzeit stoppen/Klick-Aufgabe starten“. Die Internetzeit wird dann gestoppt. Nehmen Sie an, die gestoppte Internetzeit würde umgerechnet 20 Punkte betragen.
   Wie hoch wäre Ihr End-Punktestand am Ende der Runde 3?
   _______________Punkte
   Wie viele Punkte hätten Sie nun investiert und wie viele aufbewahrt?
   _______________Punkte investiert und _______________Punkte aufbewahrt.
   Wie lautet Ihr Anfangs-Punktestand in Runde 4, wenn eine 1 gewürfelt wird?
   _______________Punkte
   Wie lautet Ihr Anfangs-Punktestand in Runde 4, wenn eine 6 gewürfelt wird?
   _______________Punkte

9. Nehmen Sie an, Ihr Punkte Manager wurde **deaktiviert**.
   Sie befinden sich in Runde 3 und müssen die Entscheidungs-Box für diese Runde ausfüllen. In Runde 4 werden alle verbleibenden Punkte automatisch in Internetzeit umgewandelt. Ihr End-Punktestand am Ende von Runde 3 beträgt 73.28 Punkte.
   Wie viel Internetzeit ergibt Ihre Wahl in Runde 4?
   _____ Min _____ Sek.
   Wie lange dauert dann die Klick-Aufgabe in Runde 4?
   _____ Min _____ Sek

General Instructions

Welcome and thank you for participating in this experiment. Please read these instructions thoroughly. Please stop any conversations now. If you have any questions please raise your hand. We will then come to help you.

In today's experiment, it is not allowed to have private reading materials (books, lecture notes etc.) close by or to use your cell phone or similar devices. Please put all your personal belongings behind you. Please switch off your cell phone and please check that no alarm is set. Please put your cell phone and similar devices into the envelope that belongs to your desk. Please do not seal the envelope. This will be done by an experimenter. **The envelope will stay on your desk throughout the experiment but you are not allowed to open it**. If you do not abide by this rule you will be excluded from this experiment and all payments.

You can earn money in this experiment. How much you earn depends on your decisions and is **not** affected by the decisions of other participants.

All decisions made by you during the experiments are anonymous. Your payment will be treated confidentially.

The experiment consists of **two parts**. You first receive the instructions for part 1. Afterwards part 1 will start. After this is completed, you will receive the instructions for part 2. In turn you will receive a couple of comprehension questions and part 2 will start.

After the experiment, you will receive a show-up fee of 2.50 EUR in addition to your payoff. In the beginning of the experiment, you receive an **endowment** of **30 EUR**. Your decisions in the experiment determine how much of these 30 EUR you will actually receive at the end of the experiment. In both parts (part 1 and part 2), money can be subtracted from your endowment.

After completion of part 2 of the experiment, we would kindly ask you to fill out a questionnaire while we prepare your payments. All information in the questionnaire and all the data collected in the experiment will be de-identified and used for scientific purposes exclusively. After filling-out the questionnaire, please wait at your desk until your number is called to receive your payment.

Instructions – Part 1

In part 1, you complete a **„clicking task".** Your task is to click and close randomly appearing pop-up windows for **10 minutes**.

Course of the clicking task

During the clicking task, pop-up-windows appear subsequently on the computer screen. When and where on the screen the windows appear is **randomly determined**. After a window appears, you have **10 seconds** to close the window. If you happen to not close it, the window will disappear on its own after the ten seconds.

The clicking task is **part of both part 1 and part 2** of the experiment. All windows that you have not closed in both part 1 and part 2 of the experiment create **costs**. These costs reduce your payment. Your payment thus depends on the total number of windows that you have not closed. In the end of the experiment, you will learn how many windows you have not closed in total.

The relationship of the total number of windows you have not closed and the corresponding costs are listed in the following table:

Total number of windows you have not closed (part 1 and part 2 together)	**Costs in EUR**	**Total payment at the end of the experiment in EUR**
0	0	32.50
1	1	31.50
2	4	28.50
3	9	23.50
4	16	16.50
5	25	7.50
6 and more	30	2.50

Instructions – Part 2

2.1 General Course

In part 2, there are **4 rounds**. In each of these rounds, there is a period of **19 minutes**. At the beginning of these 19 minutes, you can surf the internet. The remaining time, you have to perform the **clicking task**. The clicking task just starts, after you are no longer surfing the internet. That means that while you are surfing the internet, there appear **no** windows and no money can be subtracted from your account. How much internet time you have available, depends on your decisions in this part.

At the beginning of round 1, you receive a one-time endowment of 75 points. You can use these points to buy internet time. That means, internet time is transformed into point and subtracted from your points.
Points that you do not use for internet time in a given round are available in later rounds. You can carry these points over to the next round by either **investing** the points or **storing** them. By investing, you can gain additional points but you can also lose points. How this works exactly will be explained below. Points that you store are available unchanged in the next round.
Before round 1 starts, you will see a **point manager** on your computer. You can use this to **plan** your point allocation **for all four rounds**. With the point manager, you thus determine your allocation for the whole **76 minutes** (= 4 rounds x 19 minutes) before round 1 starts.
After all participants completely filled out their point manager, round 1 of 4 starts.

Please note:

- The internet connection uses the network of the University of Cologne. We will **not** save or evaluate those pages you decide to visit online. **No** Passwords or the like will be saved.
- Due to technical reasons, there may be loading times (e.g., when opening/closing the internet browser).
- The points **cannot** be transformed into money but serve to spend time in the internet. During the clicking task, **money can still be subtracted** from your account if you do not close the windows.
- On your screen, numbers are shown up to two decimal places. The computer, however, calculates with the exact values.

2.2 Content of the Point Manager

The point manager consists of several **decision boxes** in which you can make your decision for a given round. A decision box looks as follows:

Starting Points:

These are the points that you have available at the beginning of the round. You can use at most these points for internet time.

Internet Time and Transformed Points

Here you determine how much time you want to spend in the internet in a given round. The chosen time is automatically transformed into points by the computer. The number of transformed points increases more than proportionally in the amount of internet time. This means that double the amount of time requires more than double the amount of points.[1] You cannot spend more than 19 minutes per round in the internet.

Some Examples:

Internet time (min:sec)	Transformed points
0:00	0
2:00	0.67
4:00	1.87
6:00	4.08
8:00	8.31
10:00	16.76
12:00	34.40
14:00	73.28
16:00	164.38
18:00	393.80

End points:

These are the points that remain at the end of a round. These points are calculated as follows:

$$\text{End Points} = \text{Starting Points} - \text{Transformed Points.}$$

You can now invest and/or store your end points.

[1] The computer uses for transformation: $\text{Points} = \left(\frac{2400}{2400 - \text{internet time (in sec.)}}\right)^{10} - 1.$

Investing

Here you choose the share (in %) of your end points that you want to invest. By investing, you can gain additional points or lose points. Those points that you invest will be either **divided by three (:3)** or **multiplied by three (x3)** at the beginning of the next round. This will be determined via a die roll for each participant at his disk. Both outcomes are **equally probable**.

Result of the die roll	Invested points will be
1, 2, 3	divided by three
4, 5, 6	multiplied by three

Storing

The share (in %) of your end points that you do not invest is automatically stored. Stored points are available to you in the next round **unchanged**.

2.3 Filling out the Point Manager

Invested points will be either divided by three or multiplied by three. Hence, there are several possible amounts of points in the subsequent round. The point manager displays all these possible round outcomes and the according decision boxes. **In round 4, the point manager automatically transforms all remaining points into internet time**. In the point manager, you thus allocate – before round 1 starts – the points for each possible course of the experiment.

Please see Figure 1 (point manager) on the supplementary sheet.

You read the point manager as follows:

- Round 1: This is the first round. Hence, there is only one decision box.
- Round 2: There are two possible outcomes for the invested points in decision box 1: dividing by three or multiplying by three. Hence, there are two decision boxes in total in round 2.
- Round 3: Every decision box from round 2 has again two possible outcomes for the invested points. Hence, there are four decision boxes in total in round three.
- Round 4: Following the same principle, there are eight possible outcomes in round 4. Because this is the last round, you do not have to make a decision. The computer automatically transforms all remaining points into internet time.

When you completely filled out the point manager and do not want to change anything anymore, confirm your decision by clicking on "**save point manager**".

Please note: If you change the allocation of points in one decision box, the allocations in all subsequent decision boxes also change. Hence, please make sure that all points at all nodes of the point manager are allocated as you want them to be before you click on "save point manager". You cannot allocate more points in a round than you have available.

2.4 Determining whether Point Manager remains active

After you filled out the point manager, a die roll determines whether it remains active for all four periods or whether it will be deactivated. Whether the point manager remains active will be determined via a die roll for each participant at his desk.

Result of the die roll	Point manager
1	active
2, 3, 4, 5, 6	deactivated

a) Point manager remains active:

If your point manager remains **active**, you will run through the experiment according to the scheme that you chose there. You **cannot change** the point manager afterwards. It holds for the entire 76 minutes.

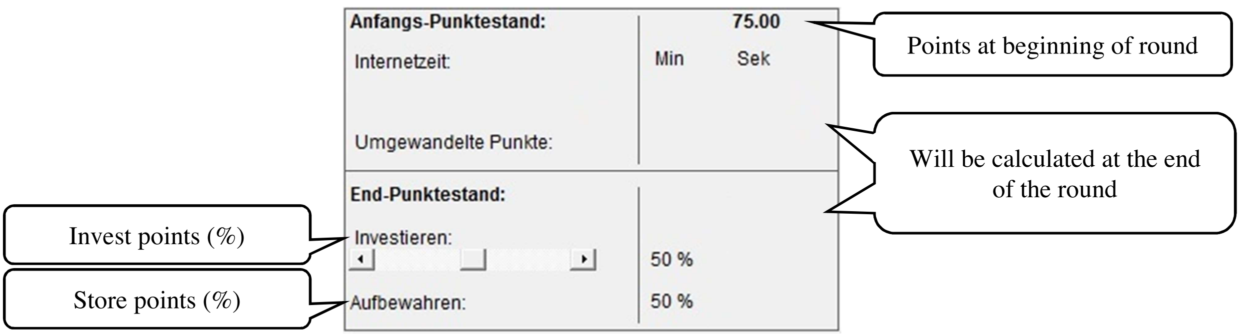
Depending on the investment outcome (dividing by three/ multiplying by three), the computer automatically chooses the according decision box from your point manager. First, you spend your chosen time in the internet. Subsequently, the clicking task starts for the remainder of the 19 minutes. The computer automatically opens and closes the internet browser and also starts the clicking task automatically.

b) Point manager gets deactivated:

If the point manager gets deactivated, you choose a new allocation in every round. At the beginning of each round, you see a decision box. There you see the starting points of this round. However, you do not choose internet time in this decision box. Rather the internet browser starts after the decision box and you decide how much time you want to spend in the internet.

When you want to close the internet, just close the internet browser. You will then return to the experiment screen. However, your internet time will only then be stopped, when you click on "**Stop internet time/Start clicking task**". Subsequently, you continue with the clicking task for the remainder of the 19 minutes. The computer automatically transforms the stopped internet time into points. If your starting points are depleted in the meantime, the internet browser closes automatically.

Because you surf the internet only after the decision box, you do not know at the beginning of a round how many points will be transformed. Hence, the decision box does not display internet time or transformed points. Further, no end points can be calculated by then. In the decision box, you thus only choose the share (in %) of your end points that you want to invest.



Summary/ Course of the Experiment

- Fill out point manager for all possible courses of the experiment
- Determining whether point manager remains active
  a. Point manager **active**:
  Your points are allocated the way you chose in the point manager in every period. The internet browser opens and closes automatically. The clicking task starts automatically.
  b. Point manager **deactivated**:
  At the beginning of each period, there is a decision box and you allocate your (by then not yet calculated) end points for this round. The internet browser opens automatically. To stop the internet time and to continue with the clicking task, close the internet browser and click on "Stop internet time/Start clicking task".

- Round 1
  a. Point manager active: Decision box 1 holds
  Point manager deactivated: Fill out decision box for round 1
  b. 19 minutes start (Internet first, then clicking task)

- Round 2
  a. Points invested in round 1 are either divided by three or multiplied by three (die roll)
  b. Point manager active: Decision box 2a or 2b holds (see Figure 1)
  Point manager deactivated: fill out decision box for round 2
  c. 19 minutes start (Internet first, then clicking task)

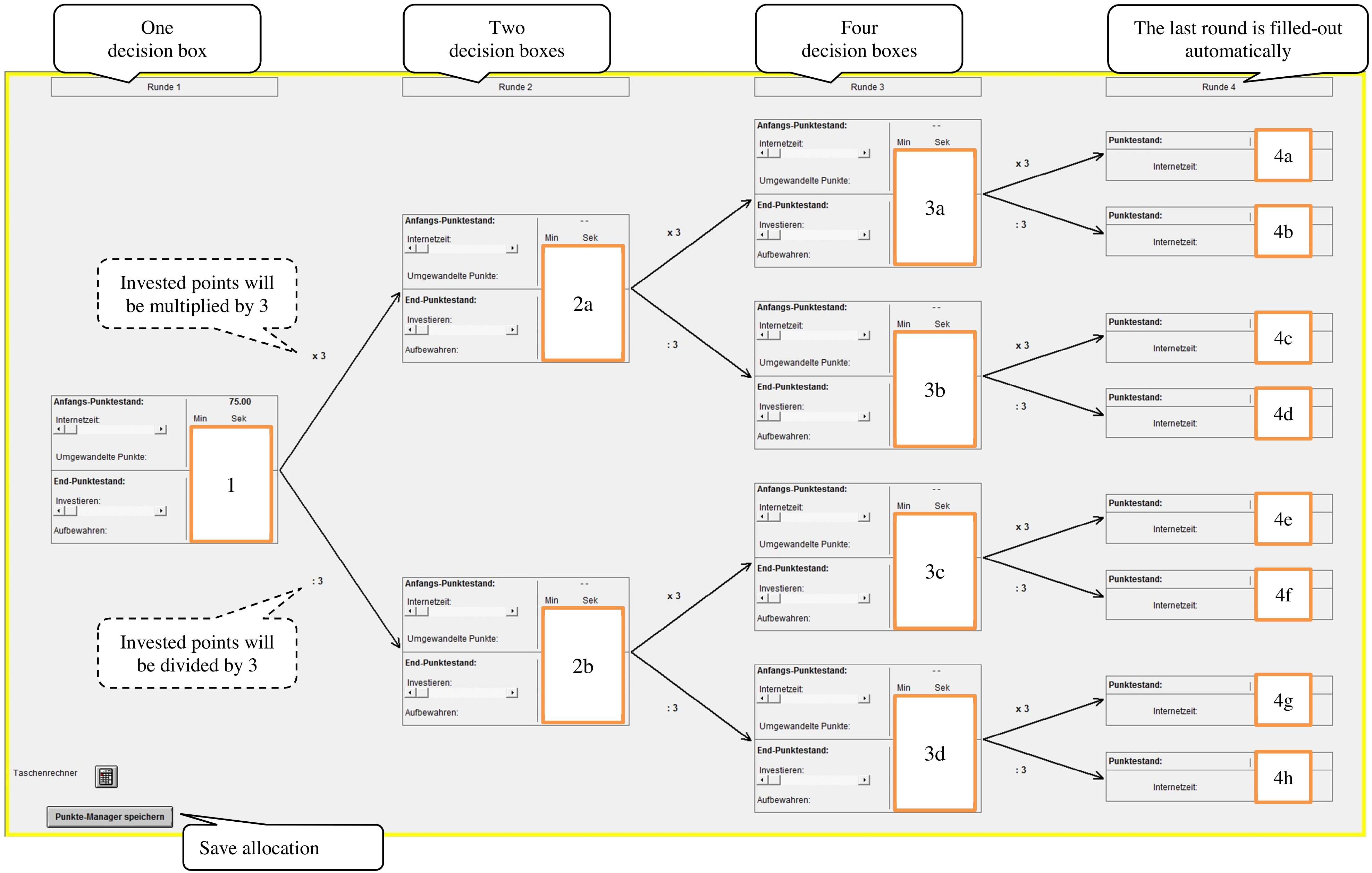
- Round 3
  a. Points invested in round 2 are either divided by three or multiplied by three (die roll)
  b. Point manager active: Decision box 3a, 3b, 3c or 3d holds (see Figure 1)
  Point manager deactivated: fill out decision box for round 3
  c. 19 minutes start (Internet first, then clicking task)

- Runde 4
  a. Points invested in round 3 are either divided by three or multiplied by three (die roll)
  b. Point manager active: Decision box 4a, 4b, 4c, 4d, 4e, 4f, 4g or 4h holds (see Figure 1)
  Point manager deactivated: Remaining points are automatically transformed into internet time. The internet browser opens and closes automatically in this round. The clicking task starts automatically in this round.
  c. 19 minutes start (Internet first, then clicking task)

As a reminder: Points cannot be transformed into money. However, during the clicking task, money can still be subtracted from your account.

Supplementary Sheet

Figure 1: Point Manager



Comprehension Questions

Please answer the following nine comprehension questions. These shall help you to understand the instructions correctly. After all participants have answered the comprehension questions correctly, the point manager will start. If you have any questions, please raise your hand.

1. Windows that you did not close during the entire experiment will be summed up and create costs that will reduce your payment.
   Please assume that you have not closed one window in part 1 of the experiment. In part 2 you then also missed to close one window in round 2 and you missed to close two windows in round 4.
   How many windows did you not close in total?
   ________________ windows
   How high are the costs that you incur?
   ________________ EUR
   How much money will be subtracted from your final payment if you happen to not close five windows in total?
   ________________ EUR

2. The points you invest will be either divided by three or multiplied by three in the next round.
   If the die comes up as a 3, the invested points will be ________________________
   If the die comes up as a 4, the invested points will be ________________________

3. If you want to spend 12:00 minutes in the internet, how many points would the computer subtract from your starting points in this round?
   ________________ points
   After this internet time has run out, you perform the clicking task for the remaining time. How long does this last?
   _____ min _____ sec.

4. Please assume that you have 100 starting points and you use 40 points for internet time. How many end points do you have this round?
   ________________points
   From these end points, you invest 50% and store 50%.
   How many points do you thus invest?________________points
   How many points do you thus store?________________points
   How many starting points do you have in the next round if the die comes up a 2?
   ________________points
   How many starting points do you have in the next round if the die comes up a 5?
   ________________points

5. How many outcomes and thus decision boxes are there in round 3?

   _____ decision boxes

6. Please assume that your point manager remains **active**.

   You are currently in round 3. The invested points from round 1 have been multiplied by three. The invested points from round 2 have been divided by three.

   Which decision box has now been reached? (See Figure 1: Point Manager)

   Nr. _____

7. Please assume that your point manager remains **active**.

   You are currently in round 4. The invested points from round 1 have been divided by three. The invested points from round 2 have been multiplied by three. The invested points from round 3 have been divided by three.

   Which decision box has now been reached? (See Figure 1: Point Manager)

   Nr. _____

8. Please assume that your point manager has been **deactivated**.

   You are currently in round 3 and have 50 starting points. At the beginning, you have to fill out the decision box for this round. You invest 50% of your end points and store 50%. Subsequently, you click on “next” and the internet browser starts. After you closed the internet browser, you click on “Stop internet time/Start clicking task”. The internet time will be stopped then. Assume that the stopped internet time would be transformed into 20 points.

   How much end points would you have at the end of round 3?

   _______________points

   How many points would you thus have invested and how many stored?

   _______________points invested and _______________points stored.

   How many starting points do you have in round 4 if the die comes up a 1?

   _______________points

   How many starting points do you have in round 4 if the die comes up a 6?

   _______________points

9. Please assume that your point manager has been **deactivated**.

   You are currently in round 3 and have to fill out the decision box for this round. In round 4, all remaining points will be automatically transformed into internet time. You have 73.28 end points at the end of round 3.

   How much internet time does your choice yield in round 4?

   _____ min _____ sec.

   How long is the clicking task in round 4?

   _____ min _____ sec

3.C Empirical Appendix

Summary Statistics of Choices under Pre-Commitment

Table 3.C.1: Summary Statistics: Choice Variables under Pre-Commitment.

	1	U	D	UU	UU	DD	DD	Total
Consumption Share	18.26	22.42	28.49	28.26	35.81	39.29	42.13	30.67
	(12.52)	(17.33)	(18.77)	(19.87)	(20.53)	(21.09)	(21.25)	(20.54)
Investment Share	38.23	37.72	36.13	45.58	41.05	40.42	39.02	39.74
	(24.51)	(22.65)	(24.79)	(27.90)	(27.26)	(24.88)	(28.80)	(25.88)

Notes: Reported is the mean of choice variables under pre-commitment in percent. Standard deviations in parentheses. $N = 60$. Consumption Share is the fraction of current wealth transformed into internet time. Investment share is the fraction of remaining wealth after consumption that is invested in the risky asset.

Detailed Regression Analyses: Pre-Commitment

Table 3.C.2: Consumption Choice under Pre-Commitment: Regressions.

Model	(I)	(II)	(III)	(IV)	(V)	(VI)
Estimation	OLS	OLS	GLS	Tobit	Tobit	Tobit
Investment Outcome	-5.484***	-5.484***	-5.484***	-5.441***	-5.438***	-5.455***
	(1.363)	(1.371)	(1.371)	(1.366)	(1.367)	(1.622)
Round 3		10.916***	10.916***		11.168***	11.110***
		(2.111)	(2.111)		(2.163)	(1.722)
Age		0.438	0.438		0.446	0.444
		(0.362)	(0.362)		(0.359)	(0.425)
Male		-8.483**	-8.483**		-8.685***	-8.640**
		(3.302)	(3.302)		(3.296)	(3.793)
German Native		1.909	1.909		1.868	1.878
		(5.779)	(5.779)		(5.736)	(4.918)
Constant	35.477***	18.976*	18.976*	35.382***	18.627*	18.696
	(2.033)	(10.667)	(10.667)	(2.034)	(10.590)	(11.891)
Random Effects	No	No	Yes	No	No	Yes
Clustered Std. Errors	Yes	Yes	Yes	Yes	Yes	No
Observations	360	360	360	360	360	360
Clusters	60	60	60	60	60	60
Left-censored Obs.				3	3	3
Right-censored Obs.				0	0	0

Notes: Reported are OLS/GLS and two-censored (at 0 and 100) Tobit regressions. Dependent variable is the consumption share under pre-commitment in percent. Standard errors in parentheses. $N = 60$ subjects (cluster) and $T = 7$ decisions per subject. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

Table 3.C.3: Investment Choice under Pre-Commitment: Regressions.

Model	(I)	(II)	(III)	(IV)	(V)	(VI)
Estimation	OLS	OLS	GLS	Tobit	Tobit	Tobit
Investment Outcome	2.506	2.506	2.506	2.401	2.390	2.475
	(2.173)	(2.185)	(2.185)	(2.489)	(2.488)	(2.253)
Round 3		4.592	4.592		4.822	4.683**
		(3.043)	(3.043)		(3.628)	(2.385)
Age		0.324	0.324		0.434	0.490
		(0.890)	(0.890)		(1.107)	(0.728)
Male		8.223	8.223		9.202	10.226
		(5.320)	(5.320)		(6.312)	(6.425)
German Native		0.310	0.310		-0.234	-0.722
		(7.036)	(7.036)		(7.528)	(8.297)
Constant	38.733***	23.486	23.486	38.408***	20.272	18.582
	(2.916)	(23.693)	(23.693)	(3.365)	(29.178)	(20.296)
Random Effects	No	No	Yes	No	No	Yes
Clustered Std. Errors	Yes	Yes	Yes	Yes	Yes	No
Observations	360	360	360	360	360	360
Clusters	60	60	60	60	60	60
Left-censored Obs.				28	28	28
Right-censored Obs.				23	23	23

Notes: Reported are OLS/GLS and two-censored (at 0 and 100) Tobit regressions. Dependent variable is the investment share under pre-commitment in percent. Standard errors in parentheses. $N = 60$ subjects (cluster) and $T = 7$ decisions per subject. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

Summary Statistics of Choices under Non-Pre-Commitment

Table 3.C.4: Distribution of NPC Subjects.

Path	$t = 1$	$t = 2$		$t = 3$			
		U	D	UU	UD	DU	DD
Subjects	52	27	24	13	14	12	12

Notes: Reported is the distribution of subjects over paths under non-pre-commitment. One subject consumed all her wealth in period one.

Table 3.C.5: Summary Statistics: Choice Variables under Non-Pre-Committment.

	1	U	D	UU	UD	DU	DD	Total
Consumption Share	16.46	17.57	22.41	25.03	21.77	28.31	39.78	21.53
	(15.23)	(13.02)	(13.66)	(18.31)	(15.49)	(20.65)	(13.77)	(16.36)
N	52	27	24	13	14	12	12	
Investment Share	35.14	35.89	32.67	49.54	31.43	41.75	41.42	36.78
	(20.17)	(20.40)	(21.47)	(26.17)	(27.39)	(21.83)	(28.75)	(22.61)
N	51	27	24	13	14	12	12	

Notes: Reported is the mean of choice variables under non-pre-commitment in percent. Standard deviations in parentheses. Consumption Share is the fraction of current wealth transformed into internet time. Investment share is the fraction of remaining wealth after consumption that is invested in the risky asset. One subject consumed all her wealth in period one and we thus discard the investment choices for this subject.

Detailed Regression Analyses: Non-Pre-Commitment

Table 3.C.6: Consumption Choice under Non-Pre-Commitment: Regressions.

Model	(I)	(II)	(III)	(IV)	(V)	(VI)
Estimation	OLS	OLS	GLS	Tobit	Tobit	Tobit
Investment Outcome	-4.487	-2.595	-0.237	-4.487	-2.595	-0.278
	(3.306)	(2.963)	(2.483)	(3.290)	(2.889)	(2.202)
Round 3		8.426***	8.518***		8.426***	8.517***
		(1.787)	(1.751)		(1.742)	(1.695)
Age		-0.173	-0.176		-0.173	-0.176
		(0.437)	(0.443)		(0.426)	(0.425)
Male		-10.192***	-10.592***		-10.192***	-10.585***
		(3.641)	(3.689)		(3.550)	(4.047)
German Native		0.618	0.987		0.618	0.980
		(5.465)	(5.517)		(5.328)	(4.891)
Constant	26.402***	29.405**	28.101**	26.402***	29.405**	28.123**
	(2.437)	(12.409)	(12.582)	(2.425)	(12.098)	(11.856)
Random Effects	No	No	Yes	No	No	Yes
Clustered Std. Errors	Yes	Yes	Yes	Yes	Yes	No
Observations	102	102	102	102	102	102
Clusters	51	51	51	51	51	51
Left-censored Obs.				0	0	0
Right-censored Obs.				0	0	0

Notes: Reported are OLS/GLS and two-censored (at 0 and 100) Tobit regressions. Dependent variable is the consumption share under non-pre-commitment in percent. Standard errors in parentheses. $N = 51$ subjects (cluster) and $T = 4$ decisions per subject. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

Table 3.C.7: Investment Choice under Non-Pre-Commitment: Regressions.

Model	(I)	(II)	(III)	(IV)	(V)	(VI)
Estimation	OLS	OLS	GLS	Tobit	Tobit	Tobit
Investment Outcome	6.234	4.848	4.620*	6.942	5.343	5.038
	(4.295)	(4.293)	(2.745)	(4.602)	(4.520)	(3.416)
Round 3		6.641***	6.632***		6.740**	6.743***
		(2.417)	(2.410)		(2.700)	(2.568)
Age		0.271	0.272		0.261	0.277
		(0.955)	(0.955)		(1.109)	(0.761)
Male		7.600	7.638		8.675	9.157
		(6.777)	(6.815)		(7.567)	(7.174)
German Native		-6.322	-6.358		-6.375	-6.493
		(6.665)	(6.748)		(6.971)	(8.635)
Constant	34.420***	26.738	26.864	34.189***	26.461	26.120
	(3.968)	(25.179)	(25.464)	(4.360)	(29.001)	(21.160)
Random Effects	No	No	Yes	No	No	Yes
Clustered Std. Errors	Yes	Yes	Yes	Yes	Yes	No
Observations	102	102	102	102	102	102
Clusters	51	51	51	51	51	51
Left-censored Obs.				4	4	4
Right-censored Obs.				7	7	7

Notes: Reported are OLS/GLS and two-censored (at 0 and 100) Tobit regressions. Dependent variable is the investment share under non-pre-commitment in perecent. Standard errors in parentheses. $N = 51$ subjects (cluster) and $T = 4$ decisions per subject. * < 0.10, ** < 0.05, *** < 0.01.

Summary Statistics of Differences between Non-Pre-Commitment and Pre-Commitment

Table 3.C.8: Summary Statistics: Differences between Commitment.

	1	U	D	UU	UD	DU	DD	Total
Consumption Share	-2.02	-1.83	-6.01	1.36	-18.32	-11.36	-1.03	-4.46
	(16.89)	(17.10)	(13.74)	(21.43)	(29.52)	(22.81)	(11.04)	(18.92)
Investment Share	-1.00	-1.52	-0.25	-0.08	-11.93	1.25	9.83	-0.87
	(9.55)	(10.71)	(9.52)	(35.39)	(24.97)	(24.47)	(17.28)	(17.36)

Notes: Reported are the mean differences of the choice variables between non-pre-commitment (NPC) and pre-commitment (PC). Standard deviation in parentheses. For the consumption share: $N = 52$ in period one and $N = 51$ in periods two and three, respectively. For the investment share: $N = 51$. Consumption Share is the fraction of current wealth transformed into internet time. Investment share is the fraction of remaining wealth after consumption that is invested in the risky asset. Differences are computed by subtracting PC values from NPC values for each subject.

Detailed Regression Analyses: Difference in Commitment

Table 3.C.9: Difference in Commitment on Consumption Choice: Regressions.

Model	(I)	(II)	(III)	(IV)	(V)	(VI)
Estimation	OLS	OLS	GLS	Tobit	Tobit	Tobit
Investment Outcome	5.025	5.188	5.365	5.025	5.188	5.347
	(4.296)	(4.032)	(4.106)	(4.274)	(3.931)	(3.753)
Round 3		-3.596	-3.589		-3.596	-3.590
		(3.398)	(3.390)		(3.312)	(3.313)
Age		-0.509	-0.509		-0.509	-0.509
		(0.460)	(0.461)		(0.449)	(0.483)
Male		-0.289	-0.319		-0.289	-0.316
		(3.638)	(3.589)		(3.547)	(4.632)
German Native		0.285	0.312		0.285	0.310
		(8.203)	(8.273)		(7.997)	(5.587)
Constant	-8.260***	6.514	6.416	-8.260***	6.514	6.426
	(2.642)	(12.780)	(13.001)	(2.629)	(12.459)	(13.655)
Random Effects	No	No	Yes	No	No	Yes
Clustered Std. Errors	Yes	Yes	Yes	Yes	Yes	No
Observations	102	102	102	102	102	102
Clusters	51	51	51	51	51	51
Left-censored Obs.				0	0	0
Right-censored Obs.				0	0	0

Notes: Reported are OLS/GLS and two-censored (at -100 and 100) Tobit regressions. Dependent variable is the difference between consumption shares under non-pre-commitment (NPC) and under pre-commitment (PC). Standard errors in parentheses. $N = 51$ subjects (cluster) and $T = 4$ decisions per subject. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

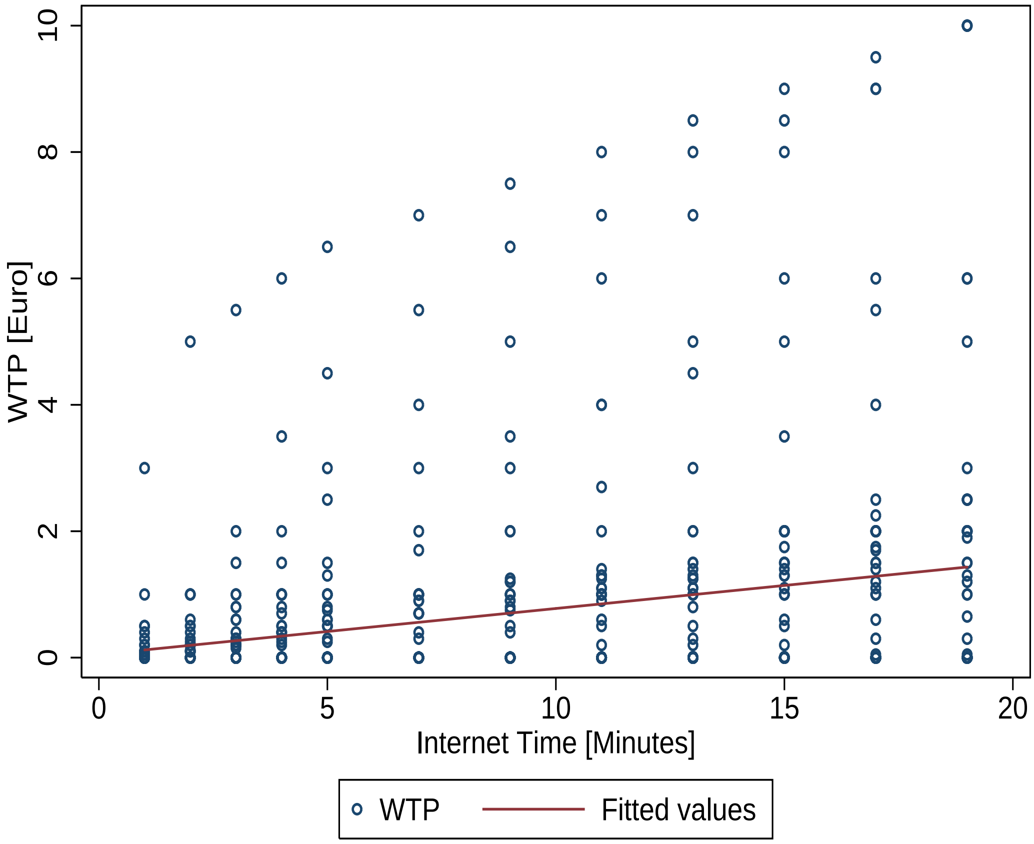
Table 3.C.10: Difference in Commitment on Investment Choice: Regressions.

Model	(I)	(II)	(III)	(IV)	(V)	(VI)
Estimation	OLS	OLS	GLS	Tobit	Tobit	Tobit
Investment Outcome	0.581	0.091	0.972	0.581	0.091	0.874
	(3.824)	(3.624)	(3.512)	(3.805)	(3.533)	(3.936)
Round 3		0.239	0.273		0.239	0.270
		(3.680)	(3.667)		(3.588)	(3.498)
Age		0.005	0.004		0.005	0.004
		(0.444)	(0.444)		(0.433)	(0.491)
Male		0.329	0.180		0.329	0.197
		(5.407)	(5.424)		(5.272)	(4.708)
German Native		-6.361	-6.223		-6.361	-6.239
		(6.782)	(6.802)		(6.612)	(5.676)
Constant	-1.100	3.883	3.395	-1.100	3.883	3.450
	(2.506)	(13.031)	(13.341)	(2.493)	(12.704)	(13.887)
Random Effects	No	No	Yes	No	No	Yes
Clustered Std. Errors	Yes	Yes	Yes	Yes	Yes	No
Observations	102	102	102	102	102	102
Clusters	51	51	51	51	51	51
Left-censored Obs.				0	0	0
Right-censored Obs.				0	0	0

Notes: Reported are OLS/GLS and two-censored (at -100 and 100) Tobit regressions. Dependent variable is the difference between investment shares under non-pre-commitment (NPC) and under pre-commitment (PC). Standard errors in parentheses. $N = 51$ subjects (cluster) and $T = 4$ decisions per subject. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

Willingness-To-Pay for Internet Time

Figure 3.C.8: Aggregated Willingness-To-Pay (WTP) Curve.



Notes: Reported is the willingness to pay (WTP) for a hypothetical additional period of experimentation. The solid line represents a linear fit. We do not find any indication for a non-linear realtionship by regressing WTP on minutes, squared minutes, or cubed minutes and clustering standard errors on the subject level. The effect is robust to the inclusion of demographics.
Instructions: *Please imagine the following situation. There exists another period which again lasts for 19 minutes. During this time you can, as before, surf the internet or perform the clicking task. If you spend the full 19 minutes performing the clicking task, you receive 10 EUR. However, you can spend some of these 10 EUR to acquire internet time. While you are surfing the internet, the clicking task is deactivated as before. Please declare in the following table, how much you are willing to dispense in order to spend the according number of minutes in the internet.*

3.D Theoretical Appendix

Following the classification of DellaVigna (2009), we contrast standard preferences with two *non-standard* preference specifications: (i) *expectations-based reference-dependent preferences* and (ii) *external-habit preferences.*

3.D.1 Expectations-Based Reference-Dependent Preferences

Expectations-based reference-dependent preferences have been developed by Köszegi and Rabin (KR; 2006; 2007; 2009) and have since been shown to be consistent with real-world behavior in a variety of domains, including taxicab drivers labor supply, expectations-driven endowment effects, and real-effort experiments.[36] KR preferences have been applied to consumption and asset pricing by Pagel (2012; 2014) and we follow her presentation of KR preferences and accordingly call an agent with such preferences a *KR agent.* The KR agent's period utility is given by

$$U_t^{\mathrm{KR}} = u(C_t) + n(C_t, F_{C_t}^{t-1}) + \gamma \sum_{\tau=1}^{\infty} \delta^\tau \boldsymbol{n}(F_{C_{t+\tau}}^{t,t-1}). \tag{3.D.1}$$

First, *consumption utility,* $u(C_t)$, corresponds to the utility from absolute consumption as under standard preferences. Second, *contemporaneous gain-loss utility,* $n(C_t, F_{C_t}^{t-1})$, corresponds to the idea of prospect theory comparing current consumption, C_t, with a reference point (Kahneman and Tversky 1979). Köszegi and Rabin (2006) make the reference point stochastic and Köszegi and Rabin (2009) assume that in life-cycle decisions, it corresponds to the rational expectations, $F_{C_t}^{t-1}$, that the agent forms in the previous period $t-1$ about current consumption in period t.[37] In the comparison with his reference point, the agent is loss averse, i.e., losses hurt him more than equally sized gains please him.

Finally, the third term corresponds to *prospective gain-loss utility* and accounts for the gain-loss feelings over the entire stream of future consumption. That is, in the previous period $t-1$, the KR agent forms beliefs about the entire future consumption stream. Then, in period t, he learns the investment outcome and experiences gains-loss feelings about future consumption prospects relative to what he has expected in the previous

[36] Mas (2006), Pope and Schweitzer (2011), and Card and Dahl (2011) provide field-data evidence for adjustment of police performance after wage arbitration, expectation-based performance of golfers, and increase in domestic violence after unexpected football outcomes, respectively. Taxi drivers' daily income and work load targeting is shown to be accordant to a forward looking reference point by Doran (2010) and Crawford and Meng (2011).

[37] Specifically, $n(C_t, F_{C_t}^{t-1}) = \int_0^\infty \mu\big(u(C_t) - u(c)\big) dF_{C_t}^{t-1}(c)$. Gain-loss utility is given by a two-piece linear function $\mu(x)$ with $\eta > 0$ and a coefficient of loss aversion $\lambda > 1$. Hence, for gains, we have $x > 0$ and thus $\mu(x) = \eta x$. For losses, we have $x \leq 0$ and thus $\mu(x) = \eta\lambda x$.

period.[38] $F^{t,t-1}_{C_{t+\tau}}$ corresponds to the updated distribution of future consumption $C_{t+\tau}$ based on the prior beliefs formed in period $t-1$ and updated by the learned investment outcome in period t. Furthermore, contemporaneous gain-loss feelings are more important than prospective gain-loss feelings which are discounted by $\gamma \leq 1$.

Pagel (2012) derives the optimal choice variables in a standard asset-pricing and life-cycle consumption model which can be adapted to our economic environment. The KR agent's consumption share has the same structure as with standard preferences. However, the expected discounted value of future consumption, ψ_t^{KR} now varies with the investment outcome. The KR agent's response is negatively correlated with fluctuations in wealth. That is, the consumption share decreases in the return realization. Hence, in the event of a bad investment outcome, the agent consumes relatively more out of his wealth than the standard agent does, i.e., $\partial\rho_t^{\text{KR}}/\partial R_t < 0$. This is because decreasing consumption below the expected level is more painful than decreasing consumption in future periods at which point the reference level will have adjusted to the decrease in wealth and hence a lower consumption level is expected by that time (Barberis 2013). The negative correlation with wealth innovations also holds for the investment share. In case of a bad investment outcome, the KR agent experiences loss feelings over the entire stream of future consumption. The agent is thus willing to increase the investment share to not encounter all of the loss feelings, i.e., $\partial\alpha_t^{\text{KR}}/\partial R_t < 0$.

Pagel (2012; 2014) shows that the KR agent has a positive demand for pre-commitment because he has a self-control problem. However, the PC plan that the KR agent wants to pre-commit to differs from the one under standard preferences because the agent considers that his rational expectations depend on his optimal choice variables.[39] Pagel (2012) shows that under NPC, the KR agent is inclined to increase consumption above his beliefs and thus overconsumes relative to the pre-commitment plan, i.e., $\rho_t^{\text{KR,PC}} < \rho_t^{\text{KR,NPC}}$. Analogously, the KR agent is inclined to increase his risk exposure by choosing a higher investment share, i.e., $\alpha_t^{\text{KR,PC}} < \alpha_t^{\text{KR,NPC}}$. Hereby, he enjoys the gain sensations of higher consumption prospects, which outweigh the increase in consumption volatility. In addition, the choice variables under pre-commitment respond stronger in magnitude to wealth fluctuations than under NPC. Hence, the amount of overconsumption and overinvestment relative to the PC path is reference-dependent and increasing in the return realization: $\partial\Delta\rho_t^{\text{KR}}/\partial R_t > 0$ and $\partial\Delta\alpha_t^{\text{KR}}/\partial R_t > 0$

[38]Specifically, $\boldsymbol{n}(F^{t,t-1}_{C_{t+\tau}}) = \int_0^\infty \int_0^\infty \mu\big(u(c) - u(r)\big) F^{t,t-1}_{C_{t+\tau}}(c,r)$, where generally $F^{t,t-1}_{C_{t+\tau}} \neq F^{t}_{C_{t+\tau}} \cdot F^{t-1}_{C_{t+\tau}}$ because the realization of future investment outcomes is contained in both the lagged beliefs and the current beliefs.

[39]Under NPC, the KR agent is assumed to take his expectations formed in the previous period as given in each period and to optimize relative to these expectations. This idea corresponds to the preferred personal equilibrium (PPE) concept of Köszegi and Rabin (2009). However, under PC, the KR agent considers how his plan determines his beliefs instead of taking his expectations as given in any period. This idea corresponds to the choice-acclimating personal equilibrium (CPE) of Köszegi and Rabin (2009).

3.D.2 External-Habit Preferences

The habit agent's period utility function is given by $u(C_t, X) = (C_t - X)^{1-\theta}/(1-\theta)$. If $X = 0$, EH preferences coincide with standard preferences and particularly have CRRA. If $X > 0$, the agent's relative risk aversion is given by $\theta C_t/(C_t - X)$ and thus decreasing in the "surplus consumption" $S_t := (C_t - X)$ (Campbell and Cochrane 1999).[40] The variation of relative risk aversion with variations in wealth generates the prediction that the two choice variables are wealth dependent (Munk 2008, Brunnermeier and Nagel 2008).[41]

The habit agent's increased risk aversion makes him invest less in the risky asset than under standard preferences (Brunnermeier and Nagel 2008). A bad investment outcome decreases wealth and the increase in risk aversion makes the habit agent reduce his investment share, i.e., $\partial\alpha^{\mathrm{EH}}/\partial R > 0$. The effect of wealth fluctuations on the consumption share depends on the level of surplus consumption. A bad investment outcome brings current wealth closer to the habit level and increases the local concavity of the utility function thus increasing marginal utility from consumption. Hence, the agent is likely to increase his consumption share to stay above his habit. However, if surplus consumption is large and there is no risk of falling short of the habit level, the agent consumes relatively less in case of a bad investment outcome because local marginal utility of consumption is low.

How the habit agent differs his choice variables based on the commitment status depends on whether or not he anticipates the formation of a habit. If the habit agent is naive in the sense that he does not anticipate his habit formation in period $t = 0$, he has the same PC plan as under standard preferences. If the habit agent is sophisticated, his behavior does not differ between PC and NPC. Whether the agent anticipates his level of habit or not is arguable. Loewenstein, O'Donoghue, and Rabin (2003) argue that people have a *projection bias* and believe that future preferences resemble current preferences. Specifically, people fail to predict how consumption habits form. Charness and Gneezy (2009) conduct a field experiment on gym attendance and find that participants form a habit to exercise that persists even after the completion of the experiment. Acland and Levy (2013) conduct a similar field experiment and find that such habit formation is not anticipated, i.e., participants indeed exhibit a projection bias. In our experiment, subjects make a plan for the entire experiment before the very first round but after having experienced the alternative monotone clicking task. Hence, we think that both the idea of sophisticated anticipation as well as a projection bias is valid in our setting.

[40] The modeling of habit preferences in terms of differences between current consumption and a reference level is crucial for the time variation of relative risk aversion. Alternative models based on "ratio habits" imply CRRA (e.g., Abel 1990). External ratio habits can be modeled as $u(C_t, X_t) = (C_t/X_t)^{1-\theta}/(1-\theta)$ yielding a coefficient of relative risk aversion of $\theta X > 0$.

[41] See also Constantinides (1990), Detemple and Zapatero (1991), and de Jong and Zhou (2013).

Under the assumption of a projection bias, the habit agent has a time-inconsistency problem because he builds up a habit that forces him to put some of his wealth aside in order to sustain the habit level that he has not anticipated in his PC plan. Because the habit agent invests most of his wealth in the risk-free asset to self-insure the stream of future habit, he has a lower investment share under NPC than under PC (Brunnermeier and Nagel 2008). In addition, his expected wealth is smaller under external habit than under standard preferences. If the naive habit agent wants to accumulate the same expected wealth as the standard agent, he has to have a lower consumption share. Hence, under naive habit formation, the habit agent consumes less and has a lower investment share under NPC than under PC because he has to make sure that he stays above his habit level over all periods. Hence, $\Delta\rho^{\mathrm{EH}} < 0$ and $\Delta\alpha^{\mathrm{EH}} < 0$.[42]

[42] However, if the projection-bias assumption does not hold and the habit agent is sophisticated in the sense that he anticipates his habit level, there is no difference between pre-committed and non-pre-committed choices.

Chapter 4

PREFERENCES AND DECISION SUPPORT IN COMPETITIVE BIDDING

4.1 Introduction

First-price sealed-bid auctions (FPSBA) and Dutch auctions (DA) yield the same revenue as both formats are strategically equivalent. However, this strong theoretical result breaks down empirically. Previous research suggests three possible explanations: opportunity costs (Carare and Rothkopf 2005; Katok and Kwasnica 2007), preferences (Weber 1982; Nakajima 2011; Lange and Ratan 2010; Belica and Ehrhart 2013; Ehrhart and Ott 2014), and complexity of the decision (Cox, Smith, and Walker 1983). We analyze the role of preferences and complexity while controlling for opportunity costs. Our results indicate that the non-equivalence is driven by the complexity of competitive bidding rather than by preferences.

Both the FPSBA and the DA, with slight variations, generate billions of dollar in revenue each year.[1] Governments and private firms frequently use the FPSBA for procurement in construction and to subcontract with suppliers. Federal banks and firms use variants of the DA to sell securities and refinance credit. The DA is also used in initial public offerings (e.g., Google Inc.) as an alternative to classical valuation by investment banks.[2] Furthermore, the DA can be found on fish and fresh-produce markets (e.g., Cassady 1967). Thus, auctions as a mean to sell or procure goods and services have seen a drastic increase in the last two decades.

The breakdown of strategic equivalence is a robust observation but the direction of the deviation is non-conclusive. On the one hand, Coppinger, Smith, and Titus (1980) and Cox, Roberson, and Smith (1982) find that the FPSBA yields higher revenue than the DA in a controlled laboratory setting. On the other hand, in a field experiment on an internet auction platform, Lucking-Reiley (1999) finds that the DA generates higher revenue than the FPSBA. Differences in opportunity costs can explain these findings. A DA arguably increases bidders' opportunity costs as they have to frequently monitor the price clock or even have to physically return to the auction site to check for updates in

[1] In an FPSBA, bidders simultaneously submit "sealed" bids to the seller and the highest bidder receives the object and pays his bid. In a DA, the seller starts at a high initial ask price and gradually decreases the ask price until the first bidder stops the auction, receives the item, and pays the stop price.

[2] Note, however, that these examples typically auction off multiple units and that the auctions are then modified such that they usually do not discriminate between different bidders but apply a uniform-pricing rule.

prices. An FPSBA, on the other hand, can be ended immediately after the simultaneous submission of bids.

Carare and Rothkopf (2005) show theoretically that such increased opportunity costs increase the optimal bid. In a DA, Cox, Smith, and Walker (1983) and Katok and Kwasnica (2007) analyze the trade-off between opportunity costs and additional utility from suspense, i.e., from a joy to gamble. Both articles provide evidence that increasing opportunity costs by increasing payoffs or by increasing the clock time, respectively, increases bids in a DA. Our goal is to assess the predictive power of different preference-based theories for observed bidding and to analyze the effect of complexity. Hence, we eliminate this confound by holding the time per auction format and thus the opportunity costs from participation constant. In addition, we hold the action set, i.e., the set of feasible bids, constant across the two formats which allows a direct comparison of the two auctions.

Preference theories assume Bayesian rationality in the sense that bidders derive and process probabilities correctly. However, bidding in auctions is a demanding problem. In deriving the optimal bid, the bidder faces a trade-off between increasing his winning probability by submitting a higher bid and increasing his winning profit by submitting a lower bid. Individual preferences determine the optimal bid that balances these diametric effects. However, this optimization requires a certain level of mathematical sophistication. It is thus possible that the complexity of bidding interferes with the effect of preferences. In other words, bidders can make mistakes, e.g., in deriving the winning probability associated with their bid. Therefore, we design a decision support system (DSS) to reduce the complexity and assist bidders in deriving the optimal bid that corresponds to their preferences.

The increase in the use of auctions has led to a rise in the demand for expert services. While our implementation of decision support is primarily a mean of reducing measurement noise, the design of such DSS is of also of interest in itself. Several patents have been filed for (automated) bid-advising systems that account for, e.g., the auction structure and risk attitudes of rival bidders based on historical data.[3] Our DSS implementation resembles such automated bidding advice that estimates competitors' bidding behavior in a given auction format. In addition, there are more and more consulting firms specializing in auctions (e.g., Market Design Inc.) and major economic consulting companies offer services regarding auctions and bidding (e.g., The Brattle Group, NERA). These services typically include all aspects relevant for setting up and participating in auctions (e.g., bid tracking, bidding strategy, auction rules and design, training, provision of input to regulators).

[3] See, for example, Guler et al. (2002), Guler, Liu, and Tang (2003; 2009), Zhang and Guler (2013).

Strategic equivalence rests on the assumption that bidders have standard preferences, i.e., they derive utility only from realized payoffs. Regarding the departures from standard preferences, we study expectations-based reference-dependent and Allais-type preferences. We focus on these two specifications because they are frequently used to explain decision making under uncertainty.[4] Under reference dependence, the bidder compares gains and losses in wealth relative to a reference point (Kahneman and Tversky 1979). In this comparison, the bidder is assumed to be loss averse and puts more weight on negative deviations from this reference point (losses) than on equivalent positive deviations (gains). Loss aversion contradicts the global-utility assumption of standard preferences because the bidder considers changes in wealth with respect to a local reference point. However, the specification of the reference point is subject to debate. Köszegi and Rabin (2006) propose expectations-based reference dependence, i.e., the reference point is stochastic and given by the rational expectations that the individual holds over the outcomes of a risky decision.[5] In the following, we will denote expectations-based reference-dependent preferences as *KR preferences*.

Individuals with *Allais-type preferences* prefer outcomes that are generated with certainty to the same outcomes that are generated by a risky lottery (e.g., Andreoni and Sprenger 2010). This difference is most prevalent in the Allais paradox (Allais 1953). Here, subjects prefer a degenerate lottery over a risky one with a higher expected value but reverse their choice if both lotteries are monotonically transformed and become both risky. This reversal is inconsistent with standard preferences as it violates the crucial independence axiom of EUT (Savage 1954; Anscombe and Aumann 1963). According to this axiom, decisions between lotteries should not depend on consequences that do not differ between the lotteries.

Our experiment consists of two stages: (i) preference elicitation and (ii) competitive bidding. First, we elicit individuals' preferences in a fully non-parametric procedure, i.e., without imposing any assumption on the functional form of utility (Abdellaoui, Bleichrodt, and Paraschiv 2007). We elicit the utility function for both the gain domain and the loss domain. This gives us a measure for risk and loss attitudes, respectively. Risk attitudes correspond to the curvature of the utility function in the gain domain. Loss attitudes correspond to the ratio of the slope of the utility function in the gain relative to the slope in the loss domain. Subsequently, we measure to what extent participants exhibit Allais-type preferences. For this, we measure the disposition toward the common-ratio

[4]Reference dependence as proposed by Kahneman and Tversky (1979) is the most cited theory on risky decision making (Kim, Morse, and Zingales 2006). Allais-type preferences are an early critique of expected utility theory (EUT) (Allais 1953) and are empirically very robust in explaining deviations from predictions under standard preferences (Kahneman and Tversky 1979; Camerer 1989; Weber 2007).

[5]This modification has recently been successful in describing various empirical observations (e.g., Sprenger 2010; Ericson and Fuster 2011; Crawford and Meng 2011).

effect, i.e., if subjects reverse their preference order if the probabilities of two prospects are scaled by the same ratio.

Second, we record bidding behavior under the two mechanisms and alter the decision support. We vary the mechanism within-subject and the level of decision support between-subject. Further, we randomly assign participants to one of three treatments. Either they have medium (*Medium DSS*) or full decision support (*Full DSS*) to assist bidding, or they do not have such a system (*No DSS*). The decision support system is an overlay displaying additional information. Medium DSS shows the winning probability while Full DSS additionally provides expected profits. Although this information is redundant for fully rational decision makers, it is non-trivial to derive and providing such information greatly reduces the complexity of optimal bidding.

In line with the literature, we find significant differences between auction formats if decision support is absent. However, differences vanish between participants once we provide decision support regarding the winning probability. The additional provision of expected profits does not change this result which indicates that probability weighting is not an appropriate explanation (e.g., Goeree, Holt, and Palfrey 2002). Regarding the prediction accuracy of preferences, expectations-based reference-dependent preferences with linear utility (Linear KR) best predict individual bidding behavior. The prediction accuracy is neither affected by the level of decision support nor by controlling for demographics or numeracy.

Our contributions are twofold. First, we test individual bidding predictions under both standard and non-standard preferences based on a non-parametric elicitation of utility. Second, we design and test a decision support system (DSS) to mitigate the complexity of bidding decisions using different levels of decision support. It is important to note that we predict behavior based on actually measured individual parameters and do not merely fit model parameters to ex-post rationalize bidding behavior as calibration procedures do. We are not aware of any other work that either elicits preferences to assess bidding predictions in the two auction formats or analyzes how bidding varies with the availability of decision support.

The paper proceeds as follows. The next section introduces the model environment and theoretically analyzes the effect of preferences on optimal bidding in both formats. Section 4.3 presents our experimental elicitation of preferences. We discuss the results of this elicitation in Section 4.4. Subsequently, we present our auction environment and the implementation of the DSS and further derive individual bidding predictions (Section 4.5). Section 4.6 reports the observed bidding behavior. We discuss the validity of the preference elicitation in Section 4.7 and Section 4.8 concludes.

4.2 Theory

In this section, we first describe the two auction mechanisms. We then characterize the equilibria in both setups for standard preferences (SP), Köszegi-Rabin (KR) preferences, and Allais-type (AT) preferences.

4.2.1 Auction Formats

In each auction, there are three players: one seller and two buyers. The seller has one indivisible item for sale. Bidder i has a valuation v^i for the item where valuations are private information and independently and identically drawn according to a distribution function $F(v)$ on $[\underline{v}, \overline{v}]$. Without loss of generality, we normalize $\underline{v} = 0$ and $\overline{v} = 1$. Moreover, we normalize $u(0) = 0$. We define the following terms. *Bids* are bidders' price offers in an FPSBA at which they are willing to buy the item. *Asks* are seller's price offers in a DA for which she is willing to sell the item. *Prices*, in both formats, correspond to the accepted price offer. Price offers (bids and asks) are discrete. In the FPSBA, bidders can choose bids from the set of possible prices $B = \{b_1, \ldots, b_{n-1}, b_n\}$ where $0 = b_1 < \ldots < b_{n-1} < b_n$. In the DA, the seller's ask starts at the highest possible price p_1 and is subsequently replaced by the next smaller price from the set $P = \{p_1, \ldots, p_{n-1}, p_n\}$ where $p_1 > \ldots > p_{n-1} > p_n = 0$ until the auction ends.[6]

In the FPSBA, the bidder who places the highest bid wins the auction. Similarly, in the DA, the bidder who is the first to accept a standing ask wins the auction. The winning bidder receives the item and pays the price. If the bidder does not win the auction, he does not receive the item and pays nothing. Ties are broken at random with equal probability to receive the item.

4.2.2 Standard Preferences

This section derives the equilibrium bidding strategy under standard preferences. Standard preferences are outcome-based, i.e., they consider only realized payoff outcomes. Furthermore, bidders only consider their own payoff and are consistent with EUT, implying Bayesian rationality. In addition, under standard preferences, the bidder has a global utility function (DellaVigna 2009). Hence, standard preferences are represented by the following monotone utility function

$$u^{\mathrm{SP}}(x) = u(x) \tag{4.2.1}$$

for every $x \in \mathbb{R}$. A bidder's risk attitude is characterized by the curvature of his utility function. A bidder is risk-averse if and only if his utility function is concave; he is risk-

[6] Note that B and P have the same elements where $b_1 = p_n, b_2 = p_{n-1}, \ldots, b_n = p_1$.

seeking if and only if his utility function is convex (Gollier 2001). The specific bidding function depends on the functional form of expression (4.2.1). We focus on standard preferences represented by a power-utility function of the form $u(x) = x^{\beta}$ in deriving the bidding function.

First-Price Sealed-Bid Auction

In the FPSBA, bidder i decides on his bid $b^i \in P$ facing the following trade-off. On the one hand, a higher bid makes winning more likely as it increases the chance to exceed the other bidder's bid b^j. On the other hand, the payoff in case of winning is smaller the higher the bid. The optimal bid $b^{i,*}$ that balances this trade-off and maximizes the bidder's expected utility is given by

$$b^{i,*} = \underset{b^i \in P}{\arg\max}\ u^{\mathrm{SP}}(v^i - b^i)\ \Pr\{b^j < b^i\} + u^{\mathrm{SP}}(v^i - b^i)\ \tfrac{1}{2}\ \Pr\{b^j = b^i\}. \tag{4.2.2}$$

The first term on the right-hand side corresponds to the utility in case that bidder i wins. The second term on the right-hand side is the expected utility in case of a tie, which is broken with equal probability. In case that bidder i loses the auction, he does not receive the item and pays nothing. In Proposition 2, FP denotes the first-price sealed-bid auction.

Proposition 1 *[Equilibrium FPSBA – SP] There exists a sequence* $\{z_k^{\mathrm{SP}}\}_{k\in\{1,\dots,n\}}$ *such that*

$$\beta^{\mathrm{SP,FP}}(v) = \begin{cases} b_1 & \textit{for } v^i \in [0, z_1^{\mathrm{SP}}] \\ b_k & \textit{for } v^i \in (z_{k-1}^{\mathrm{SP}}, z_k^{\mathrm{SP}}] \textit{ with } k \geq 2, \end{cases} \tag{4.2.3}$$

where $b_{k+1} = b_k + \delta$ *and* $b_1 = 0$*, constitutes an equilibrium bidding strategy.*

The proof is relegated to Appendix 4.A.1. The outline of the proof is as follows. Following Chwe (1989) and Cai, Wurman, and Gong (2010), we first construct the sequence $\{z_k^{\mathrm{SP}}\}_{k\in\mathbb{N}} \subset [0, 1]$ that partitions the type space into intervals. We then use this sequence to apply the bidding strategy (4.2.6). The sequence $\{z_k^{\mathrm{SP}}\}_{k\in\mathbb{N}}$ is derived by assuming that the bidder bids b_k in equilibrium, i.e., no other bid should be a better choice for the bidder. Since the winning probability and the utility function are both monotonic in b, it suffices to compare b_{k-1} and b_{k+1} with b_k. This gives us the inequalities needed to recursively compute the sequence $\{z_k^{\mathrm{SP}}\}_{k\in\mathbb{N}}$. With these, the bidding strategy from Proposition 2 constitutes an equilibrium bidding strategy.

Dutch Auction

For the dynamic course of the DA, we adopt the modeling approach of Bose and Daripa (2009). In the DA, the seller starts the auction with the highest ask p_1. She then approaches each bidder sequentially asking whether or not the bidder accepts that ask. Which bidder is asked first is randomly determined at the beginning of each offer. Each bidder has the same chance to be asked first. In case that the bidder who is asked first rejects the offer, the seller offers the same ask to the other bidder.

Facing the current ask, bidder i has the following trade-off. On the one hand, he can accept the offer and stop the auction. In this case, he receives the item with certainty. On the other hand, he can reject the offer hoping for a better one. In this case, he could make a greater payoff but also faces the risk that the other bidder stops the auction before he is asked again. At each ask p_k, bidder i decides whether to accept or to wait for the next offer.

We begin by comparing the utility the bidder earns if he accepts now in period k, i.e.,

$$u^{\mathrm{SP}}(v^i - p_k), \tag{4.2.4}$$

to the expected utility if he waits for the next price, that is,

$$E[u^{\mathrm{SP}}(v^i - p_{k+1})] = H_k^i \cdot u^{\mathrm{SP}}(v^i - p_{k+1}), \tag{4.2.5}$$

where H_k^i is the probability given distribution $F(v)$ that bidder i receives the item at price p_{k+1} given that he refuses the price p_k. The probability H_k^i consists of two parts: the probability ϕ_k^i under F that i obtains the item at the next price p_{k+1} given that it is still available at that price, i.e., that it has not been sold at price p_k; and the probability ρ_k^i under F that the item is actually available at price p_{k+1} given that bidder i refused price p_k. Consequently, we have $H_k^i = \phi_k^i \cdot \rho_k^i$.

Proceeding with this comparison for all prices, we obtain the inequalities needed to construct the same sequence $\{z_k^{\mathrm{SP}}\}_{k\in\mathbb{N}} \subset [0,1]$ as in the FPSBA that determines the following equilibrium bidding strategy.

Proposition 2 *[Equilibrium DA– SP] There exists a sequence* $\{z_k^{\mathrm{SP}}\}_{k\in\{1,\dots,n\}}$ *such that*

$$\beta^{\mathrm{SP,DA}}(v) = \begin{cases} p_1 & \text{for } v^i \in [z_1^{\mathrm{SP}}, 1] \\ p_k & \text{for } v^i \in [z_k^{\mathrm{SP}}, z_{k-1}^{\mathrm{SP}}) \text{ with } k \geq 2, \end{cases} \tag{4.2.6}$$

where $p_{k+1} = p_k - \delta$ *and* $p_n = 0$*, constitutes an equilibrium bidding strategy.*

The detailed proof is relegated to Appendix 4.A.2. While the bidding strategies of the FPSBA and the DA look a bit different, they yield the same equilibrium bids for all valuations under standard preferences. Hence, the two formats are strategically equivalent (Vickrey 1961). This implies that both formats yield the same realized revenue.[7]

[7]Note that the revenue equivalence theorem only yields the same expected revenue; and this is true only under very restrictive assumptions. Hence, strategic equivalence is stronger because not only expected but actually realized revenues are the same.

4.2.3 Expectations-Based Reference Dependence: KR Preferences

This section derives the equilibrium bidding strategy under expectations-based reference-dependent preferences as proposed by Köszegi and Rabin (2006, KR). KR preferences are given by a utility function u^{KR} consisting of the following two parts. First, the term $u(x)$ corresponds to utility of payoff x as under standard preferences. Second, the term $n(x,r) = \mu(u(x) - u(r))$ corresponds to gain-loss utility that evaluates wealth against a reference level r (Kahneman and Tversky 1979). Gain-loss utility is defined piecewise as

$$n(x,r) = \begin{cases} \eta(u(x) - u(r)) & \text{if} \quad x > r \\ \eta\lambda(u(x) - u(r)) & \text{if} \quad x \leq r, \end{cases} \tag{4.2.7}$$

where $\eta > 0$ determines how important the gain-loss utility component is relative to the standard utility. Furthermore, λ represents the level of loss aversion which weighs negative deviation from the reference point (losses) relative to positive deviations (gains). If $\lambda > 1$, the bidder is loss averse, i.e., losses hurt him more than equally sized gains please him. If $\lambda = 1$, the agent is loss-neutral and if $\lambda < 1$ the agent is gain-seeking. Total utility is the sum of both parts and given by

$$u^{\mathrm{KR}}(x,r) = u(x) + n(x,r). \tag{4.2.8}$$

Kahneman and Tversky (1979) propose the piecewise specification of gain-loss utility. Köszegi and Rabin (2006) augment this idea by assuming that the reference point is stochastic and formed by the rational expectations of the bidder. Specifically, they propose that the bidder evaluates each possible outcome x under the winning probability $\Pr\{x|b\}$ against all other possible outcomes under this distribution. In our auction environment, x can take two values: $v - b$ in case of winning and 0 else. Thus, stochastic gain-loss utility is given by

$$n(x,r) = \sum_{x \in \{v-b,0\}} \sum_{r \in \{v-b,0\}} \Pr\{x|b\} \; \Pr\{r|b\} \; \mu(u(x) - u(r)). \tag{4.2.9}$$

We follow the literature and focus on the effect of loss aversion by assuming that utility of payoff $u(x)$ is linear and set $\eta = 1$. Hence, gain-loss utility $n(x,r)$ is a two-piece linear function.

First-Price Sealed-Bid Auction

The optimization problem is equal to maximizing the expected total utility given by

$$\begin{aligned} E[u^{\mathrm{KR}}(v^i, b^i)] = \Pr\{\text{win}|b^i\}(v^i - b^i) + \Pr\{\text{win}|b^i\}(v^i - b^i)\big(1 - \Pr\{\text{win}|b^i\}\big) \\ - \lambda\big(1 - \Pr\{\text{win}|b^i\}\big)(v^i - b^i)\Pr\{\text{win}|b^i\}. \end{aligned} \tag{4.2.10}$$

In contrast to the optimization problem under standard preferences given in (4.2.2), the bidder now considers two more terms as he compares the actual outcome to all possible outcomes induced by his bid. The bidder experiences gain-loss utility in each of the two possible comparisons: winning the auction but having expected to lose it and losing the auction but having expected to win it. Because the agent is rational, $\Pr\{\text{win}|b^i\}$ is his belief to win the auction generating the reference point $r^{\text{win}} = v^i - b^i$. With the converse probability $1 - \Pr\{\text{win}|b^i\}$, he expects to lose the auction generating the reference point $r^{\text{lose}} = 0$.

Hence, in case of winning, his total utility is given by

$$u^{\text{KR,win}} = v^i - b^i + \Pr\{\text{win}|b^i\}\lambda \overbrace{(v^i - b^i - (v^i - b^i))}^{=0} + (1 - \Pr\{\text{win}|b^i\})(v^i - b^i - 0) \tag{4.2.11}$$

consisting of payoff utility $v^i - p^i$ and the gain feeling of having won the auction although he expected to lose it. Gain-loss utility in case of winning is zero because the agent expected to win the auction with probability $\Pr\{\text{win}|b^i\}$ in which case the outcome coincides with the reference point. The third term is the gain feeling against the reference point $r^{\text{lose}} = 0$ weighted with the rational belief of losing the auction.

If the bidder actually loses the auction, total utility is given by

$$u^{\text{KR,lose}} = 0 + \Pr\{\text{win}|b^i\}\lambda(0 - (v^i - b^i)) + (1 - \Pr\{\text{win}|b^i\})(0 - 0). \tag{4.2.12}$$

Now, payoff utility is augmented by the loss feeling of actually receiving zero although expecting $r^{\text{win}} = v^i - b^i$ with the probability of winning the auction. Because the agent is loss averse, the loss feeling is additionally weighted by $\lambda > 1$.

The derivation of the equilibrium bidding strategy under KR preferences has the same structure as under standard preferences.

Proposition 3 *[Equilibrium FPSBA – KR] For a sufficiently fine price grid, there exists an equilibrium bidding strategy under KR preferences in the FPSBA. For $\lambda > 1$, the bidder bids more aggressively whereas for $\lambda < 1$, he bids less aggressively than with linear standard preferences.*

The proof of Proposition 3 is relegated to Appendix 4.A.1.

Dutch Auction

Deriving the equilibrium bidding strategy in the DA with KR preferences follows the same logic as with standard preferences. As accepting the current price p_k yields a certain payoff, total current utility is given by

$$u_k^{i,\text{KR}} = v^i - p_k. \tag{4.2.13}$$

The expected payoff from waiting for the next better price p_{k+1} incorporates the anticipation of gain-loss feelings following the same logic as in the FPSBA and is given by

$$E[u_{k+1}^{i,\mathrm{KR}}] = H_k^i\ (v^i - p_{k+1}) + H_k^i(1-H_k^i)\ (v^i - p_{k+1} - 0) + (1-H_k^i)H_k^i\lambda(0 - v^i + p_{k+1}). \quad (4.2.14)$$

With probability H_k^i, the bidder expects to be offered and to accept the next price p_{k+1} generating the reference point $v - p_{k+1}$. With the converse probability $1 - H_k^i$, he expects that the other bidder stops the auction before he is offered p_{k+1} generating the reference point zero. The equilibrium bidding function under KR preferences is stated in the following proposition.

Proposition 4 *[Equilibrium DA – KR] There exists an equilibrium bidding strategy for KR preferences in the DA. For $\lambda > 1$, the bidder bids more aggressively whereas for $\lambda < 1$, he bids less aggressively than with linear standard preferences.*

The proof of Proposition 4 is relegated to Appendix 4.A.2.

4.2.4 Allais-Type Preferences

Allais-type preferences violate the independence (or substitution) axiom, which is essential for EUT (Allais 1953; Savage 1954; Anscombe and Aumann 1963). We illustrate Allais-type preferences by the common-ratio effect (CRE). Let $\mathscr{L} = (x, \xi; 0)$ denote a lottery that yields a payoff of $x > 0$ with probability ξ and with probability $1 - \xi$, the payoff is zero. Participants face two pairs of lotteries: a *scaled-up* pair and a *scaled-down* pair (Cubitt, Starmer, and Sugden 1998). In the scaled-up pair, individuals compare a degenerate lottery $\mathscr{L}_1^D = (x_1, 1; 0)$ and a risky lottery $\mathscr{L}_2^R = (x_2, \xi_2; 0)$ with $x_2 > x_1 > 0$. In the scaled-down pair, the probabilities are scaled by a common ratio $\rho < 1$ which turns the first lottery risky. The individual now compares $\mathscr{L}_1^{D,\rho} = \mathscr{L}_1^R = (x_1, \rho; 0)$ and lottery $\mathscr{L}_2^{R,\rho} = (x_2, \rho \cdot \xi_2; 0)$. The independence axiom states that an individual who is indifferent between the scaled-up lotteries should also be indifferent between the scaled-down lotteries for any $\rho \in (0, 1]$. That is, if one scales the probabilities of both lotteries by a common ratio, the preference ordering is not affected under EUT.

However, Kahneman and Tversky (1979) report that subjects have a preference for certainty, i.e., outcomes in a degenerate lottery. In their experiment, a majority of individuals reveals $\mathscr{L}_1^D \succ \mathscr{L}_2^R$ but $\mathscr{L}_1^R \prec \mathscr{L}_2^R$ thus violating independence. This so called "Allais paradox" (Allais 1953) is empirically very robust although reverse Allais-type preferences, i.e., a preference for risky outcomes if a certain outcome is available, have also been observed experimentally (Camerer 1989; Weber 2007).

Similar to Andreoni and Sprenger (2010) and Nakajima (2011), we argue that Allais-type preferences can be modeled by assigning a different utility function for certain outcomes than for risky outcomes:

$$u^{\mathrm{AT}}(x) = \begin{cases} u^D(x) & \text{if } x \in X^D \\ u^R(x) & \text{if } x \in X^R, \end{cases} \tag{4.2.15}$$

where X^D is the set of outcomes of degenerate lotteries and X^R is the set of outcomes of risky lotteries. In EUT, it holds that $u^D(x) = u^R(x)$. If $u^D(x) > u^R(x)$, the individual has Allais-type preferences; if $u^D(x) < u^R(x)$, the individual has reverse Allais-type preferences.

Bidding Behavior

For the FPSBA, Allais-type preferences do not change bidding behavior compared to standard preferences since all outcomes are risky. Therefore, the same bidding function applies.

In the DA, the situation changes because accepting the current price yields a certain payoff which is evaluated by u^D whereas waiting for the next price is risky and outcomes are thus evaluated by u^R. We can derive the bidding function in the same way as under standard and KR preferences if we assume that

$$u^{\mathrm{AT}}(x) = \begin{cases} u(x) & \text{if } x \in X^D \\ \alpha^{\mathrm{AT}} \cdot u(x) & \text{if } x \in X^R, \end{cases} \tag{4.2.16}$$

where $\alpha \in \mathbb{R}^+$.

Proposition 5 *[Equilibrium DA - AT] There exists an equilibrium bidding strategy for AT preferences in the DA. For $\alpha^{AT} > 1$, the bidders bid more aggressively than with standard preferences.*

The proof is relegated to Appendix 4.A.2

Proposition 6 *[Bidding Allais-type preferences] Allais-type preferences lead to overbidding in the DA relative to the FPSBA if and only if $\alpha^{AT} > 1$.*

The proof directly follows from the proof of Proposition 5. Intuitively, the current price p_k is augmented by a psychological premium for certainty. Thus, in the DA, a buyer accepts higher prices since he overvalues a certain outcome in comparison to the risky expected outcome if he waits for the next price. The certainty premium, however, is absent in the FPSBA.[8]

[8]We note that the overbidding only works given our organization of the DA as we resolve the order in which the seller approaches the two bidders at the beginning of a period. If we had broken ties at random after each round, which is frequently done in DA implementations, the current price would actually be risky as well and Allais-type preferences would coincide with standard preferences.

The CRE violates the independence axiom which is a necessary and sufficient condition for strategic equivalence between the FPSBA and the DA (Grimm and Schmidt 2000). Note that the direction of bidding differences between the two formats depends on the direction of Allais-type preferences. Weber (1982) discusses a particular class of reverse Allais-type preferences and theoretically shows that the FPSBA yields higher revenues than the DA. In a more general setup, Nakajima (2011) considers both Allais-type and reverse Allais-type preferences and theoretically shows that Allais-type preferences imply overbidding in the DA compared to the FPSBA. Our model of Allais-type preferences represented by utility function (4.2.16) is flexible enough to account for both Allais-type and reverse Allais-type preferences.

4.3 Experiment 1: Preferences and Characteristics

We start by presenting the elicitation of the utility function and the measurement of decision biases. Subsequently, we present our results for Experiment 1. Appendix 4.D provides screenshots of the experimental implementation.

4.3.1 Experimental Design

In Experiment 1, we measure (i) risk aversion, (ii) loss aversion, (iii) Allais-type preferences, and (iv) numeracy. We first discuss the general measurement procedure for the first three aspects. We then present the respective measures for risk and loss aversion as well as for Allais-type preferences. Last, we discuss how we measure individual numeracy.

Pairwise Comparison of Lotteries

The measurement of risk and loss aversion as well as Allais-type preferences is based on pairwise comparisons of lotteries. This lottery trade-off method was developed by Wakker and Deneffe (1996) and estimates the utility function via a sequence of indifference values.[9] Participants are presented with two lotteries generically named $\mathscr{A}$ and $\mathscr{B}$, respectively, and have to decide which lottery they prefer. For example, consider the lotteries $\mathscr{A}_{x_1} = (x_1^*, p; x_A)$ and $\mathscr{B}_{x_0} = (x_0, p; x_B)$ with $x_A < x_B < x_0$ and a fixed probability $p > 0$. The value x_1^* is elicited such that the participant shows $\mathscr{A}_{x_1} \sim \mathscr{B}_{x_0}$. Once an indifferent value has been elicited, it is used as an input for later lotteries to elicit further indifferent points. In the example, the participant now chooses between the lotteries $\mathscr{A}_{x_2} = (x_2^*, p; x_A)$ and $\mathscr{B}_{x_1} = (x_1^*, p; x_B)$ to determine x_2^* yielding $u(x_2^*) - u(x_1^*) = u(x_1^*) - u(x_0)$ (Wakker and Deneffe 1996). In the experiment, sub-

[9] See Harrison and Rutström (2008) for a survey on different measurement procedures.

jects had to wait for ten seconds before they were allowed to choose a lottery to prevent accidental misclicks.

Ultimately, the trade-off method derives a sequence of monetary values that are equally spaced in terms of utility. Using lotteries with negative outcomes, we get a sequence of losses $\{l_r\}_{r=0,\ldots,r_L}$ with $l_0 < \ldots < l_{r_L}$ such that $u(l_r) - u(l_{r+1}) = u(l_{r+1}) - u(l_{r+2})$. Analogously, using lotteries with positive outcomes, we get a sequence of gains $\{g_r\}_{r=0,\ldots,r_G}$ with $g_0 < \ldots < g_{r_G}$ such that $u(g_r) - u(g_{r+1}) = u(g_{r+1}) - u(g_{r+2})$. For a pre-specified range, we can thus approximate the actual utility function by interpolating between these values. That is, while the vertical utility axis is partitioned equally, the according values on the horizontal money axis vary depending on individual preferences. Finally, we can normalize the entire utility function by assuming that $u(0) = 0$ and setting $u(l_0) = -1$.

As in Abdellaoui (2000), we derive indifference values as midpoints between choices using an iterative bisection algorithm. Hence, directly stating indifference between lottery $\mathscr{A}$ and lottery $\mathscr{B}$ is not allowed/possible. Subsequent lotteries are rather adjusted in the direction of the previous choice to narrow down the interval in which the indifference value lies. The trade-off method does not make any assumptions concerning the functional form of utility or probability weighting and is hence robust against any deviation from linear probability weighting. In contrast, the certainty-equivalent method or the probability-equivalent method, which are frequently used in experimental work, crucially assume that participants do not distort probabilities (Harrison and Rutström 2008). The former method elicits a certain amount that makes the participant indifferent to a lottery while the latter asks for a probability of a lottery that makes the participant indifferent to a certain amount.[10]

While the trade-off method can measure utility non-parametrically both on the gain and on the loss domain, it cannot measure loss aversion, i.e., the relation of the slope of utility in gains to the slope of utility in losses, without assumptions about the probability-weighting function (Abdellaoui et al. 2008). The coefficient of loss aversion λ crucially depends on the way an individual weighs probability.[11] We therefore elicit risk and loss aversion via the procedure of Abdellaoui, Bleichrodt, and Paraschiv (2007) who augment

[10] In later steps of the procedure by Abdellaoui, Bleichrodt, and Paraschiv (2007), one lottery is degenerate and certainty equivalents are elicited. However, these steps allow for arbitrary probability weighting because the first step of the procedure measures the degree of probability weighting non-parametrically.

[11] To see this, consider two lotteries $\mathscr{A} = (x, p; 0)$ and $\mathscr{B} = (0, p; y)$ with $x > 0 > y$. Under prospect theory, an individual i is indifferent between $\mathscr{A}$ and $\mathscr{B}$ if and only if $Eu(\mathscr{A}) = Eu(\mathscr{B})$ which is equivalent to $w^G(p)u(x) = w^L(1-p)\lambda u(y)$. Solving for the coefficient of loss aversion, we get $\lambda = w^G(p)/w^L(1-p) \cdot u(x)/u(y)$. Hence, λ depends on the degree of probability weighting. We underline the strength of the non-parametric measurement as we do not have to make functional assumptions regarding $w(\cdot)$ or $u(\cdot)$.

the trade-off method by a non-parametric elicitation of loss aversion.[12] Specifically, Abdellaoui, Bleichrodt, and Paraschiv (2007) construct a sequence of monetary values that runs through the reference point of zero, i.e., $\{l_0, l_1, ..., l_{k_L}, 0, g_0, g_1, ...g_{k_G}\}$. They achieve this by linking the loss and gain domain with mixed gambles, i.e., gambles that involve both gains and losses.

Structure and Incentives

In general, the measurement of utility functions over gains and losses is performed using hypothetical payoffs or, if real incentives are used, only lotteries in the gain domain are played out due to potential ethical issues with real monetary losses (Abdellaoui 2000; Harrison and Rutström 2008). Cerroni, Notaro, and Shaw (2012) show that real monetary incentives yield better estimates under chained elicitation than hypothetical payoffs do. Etchart-Vincent and l'Haridon (2011) find that hypothetical and real monetary incentives yield significant differences in the gain domain but not on the loss domain. We use actual monetary incentives for each participant in all elicitations by randomly choosing one pairwise lottery comparison and playing out the chosen alternative for real.

As subjects could win and lose substantial amounts of money in our lottery stage, we protected our participants in two ways. First, each subject was endowed with a large initial amount in that stage. This way we could guarantee that conservative subjects could select lotteries in a way that the highest possible loss was still smaller than the endowment. Second, participants knew that they could declare the lottery stage as non-payoff-relevant after they made all lottery choices but before they knew the outcome of the lottery stage.[13] We include this no-pay option for three reasons. First, in the instructions, subjects have only limited information about the magnitude of the lotteries subsequently presented to them. Hence, from an ethical point of view, we cannot force them to ex-ante agree to bear monetary losses whose magnitude would only be revealed during the elicitation itself. Second, subjects could doubt that we actually enforce the payment of losses beyond the initial endowment. This would increase the noise of measurements based on lotteries involving high-magnitude losses. By declaring their choices to be payoff-relevant, we gain additional credibility in enforcing financial liability. Third, the no-pay option works as a check for response errors. In case subjects accidentally click on the non-preferred lottery, the bisection algorithm yields a biased estimate. However, error checks that rely on re-running parts of the elicitation procedure (as in Abdellaoui, Bleichrodt, and Paraschiv 2007) suffer from other problems: they give away the chained

[12]Furthermore, Abdellaoui, Bleichrodt, and Paraschiv (2007) note that their procedure satisfies the theoretical criteria for optimal efficiency derived by Blavatskyy (2006) in the sense that, relative to other procedures, it has the smallest effect of error on the inferred utility function.

[13]In case that a participant decides that the lottery stage is not payoff relevant, he also loses the lottery-stage endowment. However, that participant still takes part in all other parts of the experiment.

structure of the elicitation and the instructions cannot state how many choices have to be made in total. The latter point, however, is a relevant payoff information necessary to evaluate the expected payoff per individual choice. Conclusively, subjects who find themselves on a lottery path they are not comfortable with can use the no-pay option. This leaves us with estimates that actually represent a subject's preferences. In total, only one subject chose the no-pay option and did not return to Experiment 2.

4.3.2 Preferences

In this section, we discuss for each preference specification how we classify subjects based on non-parametric measures. We use this classification to describe our sample in Section 4.4. We also state our parametric implementation of the respective utility functions that we will use as input in Section 4.5.3 to predict bidding behavior in Experiment 2.

Risk Attitudes

We follow Abdellaoui et al. (2008) and measure utility curvature both non-parametrically and parametrically. The non-parametric measure is the *area under the curve* (AUC), i.e., the integral of the utility function on the gain domain (AUC_G) and the loss domain (AUC_L), respectively. We normalize the domain of utility to $[0, 1]$ by dividing each elicited gain by the maximum gain and each elicited loss by the maximum loss. We interpolate linearly between the elicited points and use a geometric approach to calculate the area. On the gain domain, an individual is risk-averse if $AUC_G > 0.5$, risk-neutral if $AUC_G = 0.5$, and risk-seeking if $AUC_G < 0.5$. On the loss domain, risk aversion corresponds to $AUC_L < 0.5$, risk neutrality to $AUC_L = 0.5$, and risk seeking to $AUC_L > 0.5$.

As a parametric measure, we fit a power utility function with constant relative risk aversion (CRRA) of the form αx^β to the elicited monetary values for each individual via a non-linear least-squares estimation. The parameter $\alpha \in \mathbb{R}$ scales the utility and the parameter $\beta \in \mathbb{R}$ is a direct estimate of the utility curvature. When estimating the CRRA utility, we distinguish between gains and losses, i.e., we fit a two-piece power function given by

$$u^{CRRA}(x) = \begin{cases} \alpha_G \cdot x^{\beta_G} & \text{if } x \geq 0 \\ \alpha_L \cdot (-x)^{\beta_L} & \text{if } x < 0. \end{cases} \tag{4.3.1}$$

On the gain domain, $\beta_G < 1$ implies a concave utility and thus risk aversion. Risk neutrality and risk seeking correspond to $\beta_G = 1$ and $\beta_G > 1$, respectively. On the loss domain, $\beta_L > 1$ corresponds to risk aversion whereas $\beta_L = 1$ and $\beta_L < 1$ imply risk neutrality and risk seeking.

Loss Aversion

Loss aversion relates the slope of utility in the gain domain to its slope in the loss domain. At what point the slopes are evaluated is subject to debate and there exist several competing definitions. Abdellaoui, Bleichrodt, and Paraschiv (2007) discuss several alternative measures and while their method allows to estimate each of them, they conclude that the definitions by Kahneman and Tversky (1979) and Köbberling and Wakker (2005) were empirically most useful in classifying subjects.

Kahneman and Tversky (1979) define loss aversion by $-u(-x) > u(x)$ for every $x > 0$. We measure the coefficient of loss aversion as the mean of $-u(-x)/u(x)$ for all elicited values $x \in \{l_0, l_1, \ldots, l_{k_L}, g_0, g_1, \ldots, g_{k_G}\}$:

$$\lambda_{KT79} = \text{mean}\left(\frac{-u(-x)}{u(x)}\right). \tag{4.3.2}$$

In general, however, we do not observe $u(-l_r)$ and $u(-g_r)$ and use linear interpolation to derive these values (Abdellaoui, Bleichrodt, and L'Haridon 2008). Given the definition of Kahneman and Tversky (1979), an individual is loss averse if the coefficient of loss aversion is greater than one.

As proposed by Benartzi and Thaler (1995), Köbberling and Wakker (2005) define the coefficient of loss aversion as

$$\lambda = \frac{u'_{\uparrow}(0)}{u'_{\downarrow}(0)}, \tag{4.3.3}$$

where $u'_{\uparrow}(0)$ is the right derivative and $u'_{\downarrow}(0)$ is the left derivative of u at the reference point of zero. In the statistical analyses, we estimate this value for individual i by the ratio $u(l_{k_L})/u(g_0) \cdot g_0/l_{k_L}$ (Abdellaoui, Bleichrodt, and Paraschiv 2007; Booij and Van de Kuilen 2009). Note that $u(l_r) = -u(g_r)$ by construction which means that the ratio reduces to $-g_0/l_{k_L}$.

Allais-Type Preferences

We utilize a metric measure of the common-ratio effect (CRE) to assess the preference reversal due to violations of the independence axiom. Our design is standard in eliciting the degree to which participants exhibit the Allais paradox (e.g., Beattie and Loomes 1997; Dean and Ortoleva 2014; Schmidt and Seidl 2014). We elicit indifference points via the same bisection algorithm as with risk and loss aversion.

Participants face two pairs of lotteries. With the scaled-up pair, we find the indifference point $c^{i,D}$ such that participant i reveals $\mathscr{A}^D = (x, 1; 0) \sim \mathscr{B}^R = (c^{i,D}, 0.8; 0)$. With the scaled-down pair, we find the indifference point $c^{i,R}$ such that the participant reveals $\mathscr{A}^R = (x, 0.25; 0) \sim \mathscr{B}^{R,\rho} = (c^{i,R}, 0.2; 0)$. Note that $\mathscr{A}^R$ and $\mathscr{B}^{R,\rho}$ are based on $\mathscr{A}^D$ and $\mathscr{B}^R$ where the probabilities are scaled by the common ratio $\rho = 1/4$. In other words, in both comparisons, the participant chooses a value in a risky lottery. In the first comparison, the alternative is degenerate while it is risky in the second comparison. Hence, $c^{i,D}$ is the compensation that the participant demands to be indifferent between a risky and a degenerate lottery. Further, $c^{i,R}$ is the compensation that the participant demands to be indifferent between two risky lotteries.

Under expected utility theory, the first indifference implies $Eu(\mathscr{A}) = Eu(\mathscr{B}) \Leftrightarrow u(x)/0.8 = u(c^{i,D})$. The second indifference implies $Eu(\mathscr{A}^R) = Eu(\mathscr{B}^{R,\rho}) \Leftrightarrow u(x)/0.8 = u(c^{i,R})$. Taken together, we obtain $c^{i,D} = c^{i,R}$. Participants exhibiting the common-ratio effect show a preference reversal such that $c^{i,D} > c^{i,R}$ (Dean and Ortoleva 2014). That is, they have a preference for certain outcomes. Conclusively, we measure the strength of the Allais paradox by

$$CRE^i = c^{i,D} - c^{i,R}. \tag{4.3.4}$$

Participants who show $CRE^i = 0$ are consistent with expected utility theory. Participants with $CRE^i > 0$ show Allais-type preferences and are classified as such (Allais 1953; Kahneman and Tversky 1979). These subjects demand more compensation for the risky lottery $\mathscr{B}^R$ in the scaled-up comparison than they demand for $\mathscr{B}^{R,\rho}$ in the scaled-down comparison. Participants with $CRE^i < 0$ are analogously classified as having reverse Allais-type preferences (Weber 1982; Camerer 1989; Weber 2007).

We model Allais-type preferences by the two-piece utility function (4.3.5) that assigns larger utility for certain outcomes than for outcomes from risky lotteries. Based on the indifference conditions between the scaled-up pair and the scaled-down pair, respectively, we estimate a two-piece power utility with CRRA of the form

$$u^{\mathrm{AT}}(x) = \begin{cases} \frac{1}{\beta_{AT}} \cdot x^{\beta_{AT}} & \text{if } x \in X^D \\ \frac{\alpha_{AT}}{\beta_{AT}} \cdot x^{\beta_{AT}} & \text{if } x \in X^R, \end{cases} \tag{4.3.5}$$

where X^D is the set of outcomes of degenerate lotteries and X^R is the set of outcomes of risky lotteries. If $\alpha_{AT} = 1$, the utility function (4.3.5) expresses standard preferences. If $CRE^i > 0$, then $\alpha_{AT} < 1$ and the individual has Allais-type preferences.

4.3.3 Numeracy

In Section 4.6.2, we relate the predictive power of preferences to individual numeracy. In deriving the optimal bid, it is crucial to have a certain mathematical sophistication. We will thus consider statistical numeracy and probabilistic literacy (*Numeracy*) as a proxy for a decision bias in terms of errors due to deficiencies in mathematical sophistication. That is, participants lack some abilities that are crucial to derive the optimal bid such as the derivation and processing of (conditional) probabilities.

We assess numeracy via an incentivized open-ended seven-item test. This test is a combination of the *Schwartz et al. (1997) Numeracy Test* (*SNT* 1 to 3) and the *Berlin*

Numeracy Test (*BNT* 4 to 7). The SNT items are standard in the numeracy literature and assess the understanding of fundamental concepts of probability, e.g., the toss of a fair coin or the conversion of percentages into absolute numbers. The BNT items assess advanced concepts of statistical computation and conditional probability, i.e., the ability to apply Bayes' theorem. These harder items were specifically created for the assessment of highly educated populations such as college students who are generally more familiar with advanced mathematical concepts (Cokely et al. 2012).

Given that we sample from a student population, we think that the BNT is the preferred choice. However, depending on the specific sample, the test may be too hard resulting in a positively skewed distribution of correct answers. Cokely et al. (2012) note that a combination of both the BNT and the SNT, on the other hand, resulted in a normal distribution of correct answers with no indication of skewness.[14] We therefore follow this approach and use the combined test (SNT + BNT). We measure *Numeracy* by the test score, i.e., the number of correct answers across all seven items. A list of all items and respective answers is provided in Appendix 4.B.

For each item, participants have at most 120 seconds to provide their answer. If they do not enter their answer in time, the item counts as falsely answered and the next question comes up. We incentivize all seven items by paying 0.50 EUR per correct answer.

4.3.4 Organization

Both parts of the experiment were conducted in the Cologne Laboratory for Economic Research (CLER) at the University of Cologne, Germany.[15] Using the recruiting system *ORSEE* (Greiner 2004), we invited a random sample of the CLER's subject pool via email. The email did neither mention the content of the experiment nor the expected compensation. However, it made clear that payment was conditional on the participation in both experiments and that payment occurred after the completion of Experiment 2. Participants signed up on a first-come-first-serve basis. The whole experiment was computerized using the programming environment *z-tree* (Fischbacher 2007). In both experiments, participants received a hard copy of the instructions (see Appendix 4.C) and an additional blank sheet of paper for notes. Furthermore, in Experiment 1, participants had access to a calculator. Participants were given as much time as they needed to familiarize themselves with the respective part and the experimental procedure. Clarifying questions were answered in private. Upon their arrival for Experiment 1, participants were randomly assigned to computer terminals by drawing an ID number from a box.

[14] Cokely et al. (2012) used the combined test (SNT + BNT) on Amazon.com's Mechanical Turk with a sample of $n = 206$ participants. They cannot reject a normal distribution of correct answers and find no evidence of skew.

[15] `www.lab.uni-koeln.de`.

All decisions and payments over the course of the entire experiment were linked to this ID number, which was the same for Experiment 1 and Experiment 2.

Experiment 1 was conducted on Friday, December 5, 2014, and consisted of two sessions distributed over three rooms. Each session lasted approximately 1.5 hours on average including the distribution and review of instructions and a post-experimental questionnaire. Experiment 1 was the same for all participants. At the end of part one, one lottery (in the gain or loss domain) was randomly selected for payment. However, participants were neither told which lottery was selected nor how many correct answers they had in the numeracy test until the very end of the entire experiment, i.e., after they completed Experiment 2.

Experiment 2 was conducted on Friday, December 12, 2014, at the same time slots and locations. We utilized three separate rooms in the laboratory simultaneously. This allowed us to run all three DSS conditions simultaneously. Participants in room A faced the *No DSS* condition, participants in room B faced the *Medium DSS* condition, and participants in room C faced the *Full DSS* condition. Hence, we hold all exogenous factors between the three DSS conditions constant. Upon their arrival for Experiment 2, participants were randomly assigned to these treatments depending on their ID number. Each session lasted approximately two hours. One round of each mechanism was randomly selected for payoff.

In both experiments, payoffs were stated in Euros (EUR). Participants were paid out in private for the entire course of experimentation after Experiment 2. This was made clear in the invitation email as well as at the beginning of Experiment 1. At the very end of Experiment 2, participants learned which lottery outcome had been realized from Experiment 1. Participants were further informed about their earnings from the numeracy test. All participants were paid their total net earnings, i.e., their earnings from the auctions and their earnings from the numeracy test increased or decreased by the lottery realization. Average payoff for the entire experiment was 36.63 EUR corresponding to approx. 45.54 USD at the time of the payment. Payoffs range from −3.00 EUR (−3.73 USD) to 98.45 EUR (122.41 USD). The one subject who accumulated negative payoffs paid in cash at the end of Experiment 2.

4.4 Results: Experiment 1

From the initial 90 participants in Experiment 1, 83 participants also showed up for Experiment 2.[16] However, one participant dropped out of Experiment 2 because of computer problems. She was paid her earnings from Experiment 1 but was dropped from Experi-

[16]We do not find any indication that these seven participants where systematically different from the remaining participants based on the non-parametric Wilcoxon-Mann-Whitney U-test.

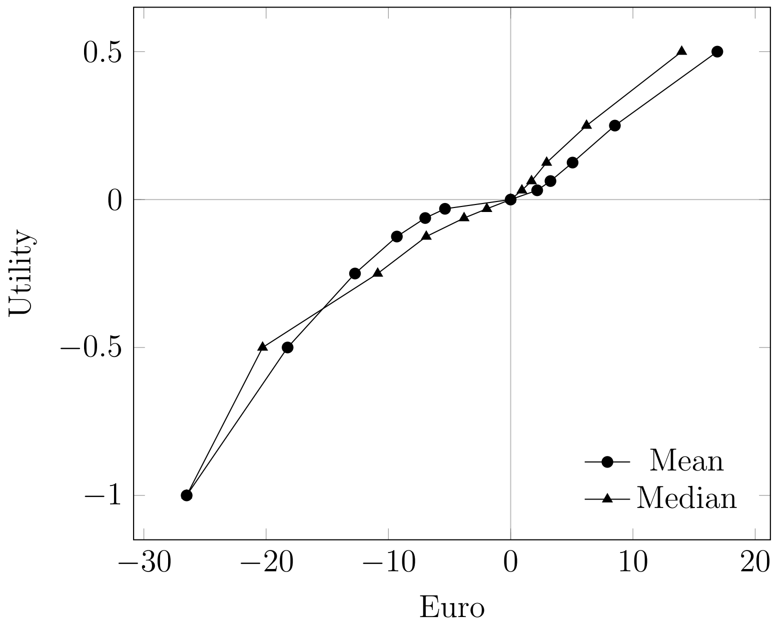
ment 2. Hence, our data set consists of $N = 83$ independent and incentivized observations for Experiment 1.[17] The average age is 24.16 years and 45.8% of the participants were male. Accordingly, Experiment 2 consists of $N = 82$ independent observations.

4.4.1 Preferences

We start with an analysis of standard preferences characterized by the shape of utility. Subsequently, we report our measurements for the behavioral theories.

Utility Curvature

Figure 4.1 shows the elicited utility function for the aggregate data.[18] For robustness and comparability with Abdellaoui, Bleichrodt, and Paraschiv (2007), we report both mean and median of our results. Per construction, utility is equally separated and individual curvature comes from variations of money for a given level of utility. In the aggregate, the figure shows no indication of loss aversion around the reference point zero. Both mean and median are concave in losses and slightly convex to linear in gains. Hence, we see a first indication of a kink around zero which is more pronounced with the mean data.



Note: Depicted is the aggregate utility for gains and losses based on the mean and median data. $N = 83$.

Figure 4.1: Shape of Utility.

[17] Table 4.E.6 reports summary statistics for each treatment.

[18] Individual utility functions are depicted in Appendix 4.E.2.

Risk Attitudes

First, we analyze risk attitudes with the non-parametric area under the curve (AUC). On average, we measure AUC_G to be 0.49 (std. dev. 0.13, median 0.49) which is not significantly different from 0.5 (Wilcoxon signed-rank [SR] test, $p = 0.5550$). On the loss domain, we measure an average AUC_L of 0.3891 (std. dev. 0.182, median 0.3811) which is significantly different from 0.5 (SR test, $p = 0.0000$). Hence, in the aggregate, based on our non-parametric measure, we cannot reject risk neutrality on the gain domain but we can reject risk neutrality on the loss domain in favor of risk aversion. Regarding our parametric estimation of the utility function (4.3.1), we estimate an average β_G of 1.74 (std. dev. 4.76, median 1.04) which is significantly different from 1 (SR test, $p = 0.0708$). We estimate an average β_L of 3.97 (std. dev. 7.21, median 1.74). These estimates are statistically different (SR test, $p = 0.0000$).

If we look at the individual data, it is impossible for a subject to be exactly risk-neutral according to AUC due to the precision of the bisection algorithm and the clearcut definition of the AUC measure. We also classify subjects according to our parametric power-utility estimation. Table 4.1 shows the classification of utility based on AUC and, in parentheses, based on β_G and β_L. We find that 29% (37%) of our sample are risk-averse both in gains and losses. Only 19% (12%) show a shape of utility that corresponds to prospect-theory utility, i.e., concave utility for gains and convex utility for losses. The majority of subjects, 42% (47%), are risk-seeking in gains but risk-averse in losses. Only a small proportion of 10% (4%) are risk-seeking in both domains. The non-parametric classification is significantly different for both domains (Fisher's exact test, $p = 0.000$).

Table 4.1: Classification of Risk Attitudes.

	Losses		
Gains	Concave	Convex	Total
Concave	24 (31)	16 (10)	40 (41)
Convex	35 (39)	8 (3)	43 (42)
Total	59 (70)	24 (13)	83

Notes: Reported is the classification of participants based on the area under the curve of the elicited utility function for gains and losses. In parentheses: classification based on parametric estimates of β_G and β_L.

Compared to Abdellaoui, Bleichrodt, and Paraschiv (2007), we find a larger proportion of subjects with standard preferences of concave utility for both gains and losses and a smaller proportion of subjects with prospect-theory preferences of concave utility in gains and convex utility in losses. However, the idea that utility is convex for losses is less empirically established than the concavity for gains. Abdellaoui, Bleichrodt, and Paraschiv (2007, p. 1661) discuss previous studies which found that around 40% of the participants exhibit standard preferences. Furthermore, they argue that these studies overestimate the proportion of subjects with convex loss utility by assuming EUT (i.e., they ignore probability weighting) which leads to a bias towards convexity for losses.[19]

Loss Attitudes

Table 4.2 shows the distribution of loss aversion and the classification of participants according to the two definitions of Kahneman and Tversky (1979, KT79) and Köbberling and Wakker (2005, KW05). In the aggregate, we have mixed evidence for loss aversion. Both measures exceed on average the loss neutral level of one. However, the KW05 measure has a large standard deviation in relation to its mean. A more detailed look at the distribution of loss aversion shows that the KT79 measure indicates loss aversion for the entire interquartile range (IQR) whereas the KW05 measure reveals that 50% of the participants fall far short of loss neutrality. The KT79 measure is significantly different from one (SR test, $p = 0.0000$) but the KW05 measure is not (SR test, $p = 0.2640$). Hence, we can reject loss neutrality given the former measure but not if we consider the latter.

Table 4.2: Loss Attitudes.

Measure	Mean (Std. Dev.)	Median [IQR]
KT79	1.637	1.323
	(0.703)	[1.070–2.127]
KW05	5.662	0.465
	(24.774)	[0.187–1.515]

Notes: Reported are the summary statistics of loss-aversion measures. KT79 stands for the measure according to Kahneman and Tversky (1979), KW05 stands for the measure according to Köbberling and Wakker (2005), IQR stands for interquartile range. $N = 83$.

On the individual level, we can classify the vast majority of participants to be loss-averse according to KT79. However, given KW05's definition, only 31.33% of the sample are loss-averse. This difference in classification is statistically significant (Fisher's exact test, $p = 0.010$). Table 4.3 cross tabulates both classifications. In total, 22 subjects are classified consistently across the two measures. The KW05 measure classifies 55 participants as gain-seeking who are considered loss-averse according to KT79. None of the two measures classifies a participant to be loss-neutral.[20]

[19]This is because these studies use risky loss prospects with probabilities exceeding 1/3. However, under probability weighting, such larger probabilities are underweighted and, thus, the risky prospects become more attractive.

[20]Note that the classification of most subjects as loss-averse based on the definition of Kahneman and Tversky (1979) does not contradict the classification under risk aversion. The large number of loss-averse subjects indicates that losses generally have a larger slope for smaller monetary values and thus $-u(-x) > u(x)$ on average.

Table 4.3: Classification of Loss Attitudes.

	KT79		
KW05	Loss Averse	Gain Seeking	Total
Loss Averse	20	6	26
Gain Seeking	55	2	57
Total	75	8	83

Notes: Reported is the classification of participants based on their coefficient of loss aversion. KT79 stands for the measure according to Kahneman and Tversky (1979), KW05 stands for the measure according to Köbberling and Wakker (2005).

Allais-Type Preferences

We measure the Allais paradox via the common-ratio effect (CRE). On average, we measure a CRE of 3.13 (std. dev. 10.61, median 2.00). Hence, participants demand on average around three EUR more to be indifferent between both scaled-up lotteries (mean $c^D = 15.36$, std. dev. 8.51, median 13.00) than between both scaled-down lotteries (mean $c^R = 12.23$, std. dev. 8.01, median 11.00). Hence, they are more willing to choose the riskier alternative if both lotteries involve risk (i.e., no lottery is degenerate). The CRE is statistically different from zero (SR test, $p = 0.0000$). Based on their individual CRE, we are able to classify 57 subjects (69%) to have Allais-type preferences, 18 subjects (22%) to be consistent with EUT, and seven subjects (8%) to show reverse Allais-type preferences.[21]

Numeracy

Participants answer, on average, 4.43 (std. dev. 1.48, median 5) questions correctly. We can construct a Bayes sub-score which is based on questions BNT 2 and BNT 4 and tests the understanding of conditional probability. This sub-score can take three possible values: 0, 1, and 2. The median is zero and only six participants (7%) achieve a test score of 2. Thus, the majority of subjects had difficulties dealing with conditional probabilities which is crucial in deriving the optimal bidding strategy in the DA.

4.5 Experiment 2: Mechanisms and Decision Support System

In part two, we record individual bidding behavior in the two mechanisms. We first discuss how we control for external factors. We then describe the actual bidding environment in the lab. Subsequently, we present our implementation of the DSS. Appendix 4.D provides

[21] One subject could not be classified because $c^{i,R} < x$ in the scaled-down lotteries implying non-montone preferences in this comparison.

screenshots of the experimental implementation. Based on the data from Experiment 1, we predict the optimal bidding function in Experiment 2 and present our results regarding the accuracy of this prediction.

4.5.1 Mechanisms

As explained in Section 4.4, we utilize $N = 82$ independent observations. We vary the mechanism within-subject and counterbalance the order, i.e., half of the participants first play the FPSBA followed by the DA. The other half faced the reversed order.

Possible Confounds

Previous research argues that differences between the two mechanisms come from the heterogeneous organization of the two auctions. The FPSBA is faster as it only requires to place simultaneous bids and the winner can be announced immediately after all bids are collected. The DA, on the other hand, requires a certain time interval for the clock to reach the desired price level of an individual bidder. Hence, this bidder faces substantial waiting costs. Carare and Rothkopf (2005) analyze the effect of transaction costs that accrue from the necessity to return to the auction site to check whether the desired price level has been reached. Not surprisingly, facing these additional costs, a bidder is willing to stop the auction at a higher price so as not to need to return to the auction site anymore.

Cox, Smith, and Walker (1983) and Katok and Kwasnica (2007) analyze the following trade-off experimentally. Though bidders face transaction and/or opportunity costs from slow DAs, they also enjoy the "waiting game" as it implies a certain level of suspense. Cox, Smith, and Walker (1983) do not find that tripling payoffs, and therewith increasing the opportunity costs of playing the waiting game, significantly increases bids in a DA. Hence, they reject the hypothesis of "suspense utility". Katok and Kwasnica (2007) find that increasing the clock time, i.e., the time between consecutive price ticks, significantly increases bids in a DA. Slow clocks increase opportunity costs which have to be paid no matter if the bidder wins the auction or not. Katok and Kwasnica (2007) note that, in the laboratory, these opportunity costs correspond most likely to participants' value of leaving the laboratory earlier. Hence, a bidder is willing to accept a higher ask to reduce the time to complete the experiment and save opportunity costs.

We account for this possible confound in two ways. First, we hold opportunity costs constant across the two mechanisms. We follow Turocy, Watson, and Battalio (2007) and keep the time per mechanism constant. That is, we fix the absolute time per mechanism irrespective of how fast participants decide (FPSBA) or how early they stop (DA). One round of bidding in the FPSBA always lasts 60 seconds.[22] One round of bidding in the DA always lasts 220 seconds, i.e., 10 seconds per price tick. If a participant accepts a current ask, he wins the auction but the next round does not start before the 220 seconds are over.[23] Second, all subjects play both the FPSBA and the DA. This within-subject variation not only increases statistical power in analyzing the difference between the two mechanisms but also holds the overall time of Experiment 2 constant. Each participant plays 18 rounds of the FPSBA and 18 rounds of the DA.

Katok and Kwasnica (2007) show that the clock speed has great impact on the bids in a DA due to the implied differences in opportunity costs. Because we hold opportunity costs constant, this is not an argument in our experiment. Participants in the FPSBA have 60 seconds to arrive at a bid that balances the trade-off between the winning probability and the profit in case of winning. On the one hand, the trade-off between two consecutive price ticks in a DA is easier and participants should need less time. On the other hand, we provide some time for the reference point to form which is assumed to be given in Section 4.2.3. We therefore decide on a clock speed of 10 seconds. This is the same clock speed as in the middle treatment in Katok and Kwasnica (2007). However, in contrast to their experiment, each DA lasts for 220 seconds in our experiment.

In addition to control opportunity costs, we also hold action sets constant across the two mechanisms. In Cox, Smith, and Walker (1983), participants' bids are rounded to the next feasible bid in the DA. Participants can then either confirm or alter this rounded bid. In Katok and Kwasnica (2007), participants can bid integers in the FPSBA whereas price decrements in the DA were five tokens. In contrast, in our design, participants in the FPSBA face the same set of possible prices as in the DA. This is a direct transfer of the model environment in Section 4.2.1 to the laboratory and guarantees strict comparability between the two mechanisms.

Competitive Bidding

Each auction consists of one participant and one bidding robot as bidders. The bidding robot draws one price from B or P, respectively, according to a uniform distribution. This is the robot's bid in the FPSBA and its stopping price in the DA. We use a bidding robot as the competitor for three reasons. First, we do not want our results to be confounded by other-regarding preferences that are not considered in any of the models presented in Section 4.2.1. Second, we effectively reduce the strategic problem to a decision problem

[22] If participants do not enter a valid bid by the end of this time limit, they do not participate in the auction in that round.

[23] In both mechanisms, after the auction is over, participants see a screen showing the remaining time until the round is completed and whether or not they have won the auction.

by fixing the strategy of the competitor. This makes it easier for subjects to focus on their optimal strategy by breaking the dynamics of higher-order beliefs.[24] Third, we are able to precisely calculate the winning probability and the expected profit. The provision of this information depends on the DSS-treatment status.

First-Price Sealed-Bid Auction

In the FPSBA, the computer screen informs the participants about their valuation and features a testing area. In this area, participants can explore the consequences of a particular bid on their profit and, depending on their DSS-treatment status, on the winning probability and the expected profit (see Section 4.5.2). Participants are further informed about the remaining time of this round. Finally, they enter their actual bid and submit this bid by pressing a button. After submitting their bid, participants are immediately informed whether they have won the auction and about the remaining time the current auction lasts. When the round has timed out, a feedback screen informs the subjects about their valuations, the winning bid, whether or not they receive the item, and their profit for this round.

Dutch Auction

In the DA, the computer screen informs participants about their valuation and displays the current price, the time until the next price, and the next price. As in the FPSBA, participants are informed about their profit given both the current and the next price. Depending on their DSS-treatment status, participants are also informed about the probability to be offered the current price and the next price and the associated expected profits (see Section 4.5.2). Finally, participants can accept the current price by pressing a button. After either the participant or the computer bidder has accepted the current price, participants are immediately informed whether they have won the auction and about the remaining time the current auction lasts. When the round has timed out, participants receive the same feedback as in the FPSBA.

4.5.2 Decision Support System

The theoretical analysis on the role of preferences in Section 4.2 highlights the fact that deriving the optimal bid depends on the following aspects: (i) the profit from winning with the chosen bid, $v^i - b^i$, (ii) the probability to win with the chosen bid, $\Pr\{\text{win}|b^i\}$, and (iii) the expected utility derived from the combination of the former two. As pointed

[24]Note that the model in Section 4.2.3 assumes that loss aversion is common knowledge. However, one cannot ensure common knowledge in reality.

out in the previous section, the latter depends on the individual preferences whereas the former two are identical across all theories. Hence, we design a DSS that assists the bidder by providing (i) the profit from winning, (ii) the winning probability, and (iii) the expected profit which is the product of (i) and (ii).

Any deviation from bidding predictions can result from two sources: an omitted preference specification or problems in deriving the optimal bid. Our DSS allows us to disentangle the role of preferences from the impact of a lack of mathematical sophistication (complexity). This is because, in the experiment, we fix the bidding strategy of the competitor and hence reduce the problem to find mutual best responses to the problem to find a one-sided best response, i.e., an optimization problem. We can thus objectively state expected profits and winning probabilities that should help participants derive the bid that maximizes the expected utility based on their actual preference specification. In other words, we implement the DSS to analyze whether observed bids are due to the underlying preferences or the complexity of the auction.

Specifically, the DSS varies between participants regarding the information a bidder receives during an auction. There are three nested levels of DSS: No, Medium, and Full DSS. In the FPSBA, the information is given for the current test bid. In the DA, the information is given for both the current and the next price. We vary the information content of the DSS between participants. The information content in each condition is as follows:

- **No DSS.** In the FPSBA, subjects see the *profit if bid was successful* which is the profit their test bid would generate given that it won the auction. In the DA, subjects see the *profit at given price* which is the profit they would generate if they accepted the current price or if they now decided to accept the next price.

- **Medium DSS.** Subjects have the same information as in No DSS. In addition, in the FPSBA, they also see the *winning probability* of their test bid which is the probability of having a higher bid than the competitor plus the probability of having the same bid and being selected as winner by the tie-breaking rule. In the DA, subjects receive the *probability to be offered the given price* for both the current and the next price. The probability to receive the current price p_k is trivially given by 100%. However, the probability to be offered the next ask, H_k^i, is highly non-trivial to derive (see Section 4.A.2).

- **Full DSS.** Subjects have the same information as in Medium DSS. In addition, in the FPSBA, they also see the expected profit of their test bid. In the DA, subjects see the expected profit of the next price. In the FPSBA, the expected profit is the product of the winning probability and the profit if the bid was successful. In the

DA, the expected profit is the product of the probability to be offered the given price and the profit at the given price.

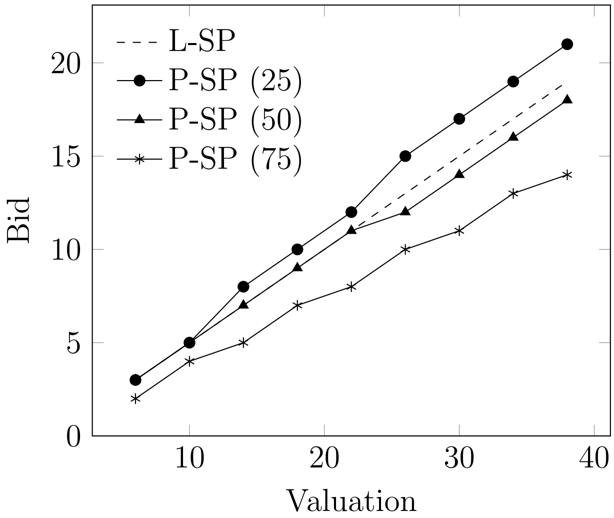
We are not aware of any other work that incorporates decision support in auctions. Armantier and Treich (2009) elicit both subjective probabilities and risk preferences in an attempt to find an explanation for overbidding in experimental first-price auctions. The authors report that participants underestimate their winning probability which indeed leads to overbidding. Furthermore, Armantier and Treich (2009) investigate the effect of a feedback system regarding winning probabilities. The feedback is implemented as follows. Participants are asked to predict their winning probability and they are given feedback regarding the precision of their prediction at the end of each round. As such, their feedback system is designed to induce learning whereas learning is not necessary in our setup as participants are given support before (FPSBA) or during (DA) the auction. Armantier and Treich (2009) show that overbidding is reduced if their feedback system is in place.

4.5.3 Predictions

Experiment 1 provides estimates of an individual's utility function and the respective input for each preference specification. To get an understanding how this data maps into bidding predictions, we display the estimated optimal bidding functions based on each preference specification.

Standard Preferences. If the EUT hypothesis holds, there is no difference between the behavior in the FPSBA and in the DA. We derive our predictions for standard preferences with power utility (P-SP) based on $u(x) = x^{\beta^G}$. For linear utility (L-SP), we set $\beta^G = 1$.

KR Preferences. Similar to standard preferences, we consider two alternative utility specifications. First, we analyze a *linear-utility* specification (L-KR) with linear consumption utility $u(x) = x$ and a two-piece linear specification for $n(x, r)$ in equation (4.2.7). Specifically, we set $n(x, r) = x - r$ if $x > r$ and $\lambda(x - r)$ if $x \leq r$. For λ, we use the coefficient based on the definition by Kahneman and Tversky (1979). Second, we assume a *power-utility* specification (P-KR) with consumption utility given by $u(x) = \alpha^G(x)^{\beta^G}$ and gain-loss utility given by $n(x, r) = \alpha^G(x - r)^{\beta^G}$ if $x > r$ and $\alpha^L(x - r)^{\beta^L}$ if $x \leq r$. This latter specification tests whether the curvature of the utility function in gains and losses yields a better fit than the standard assumption of linearity.

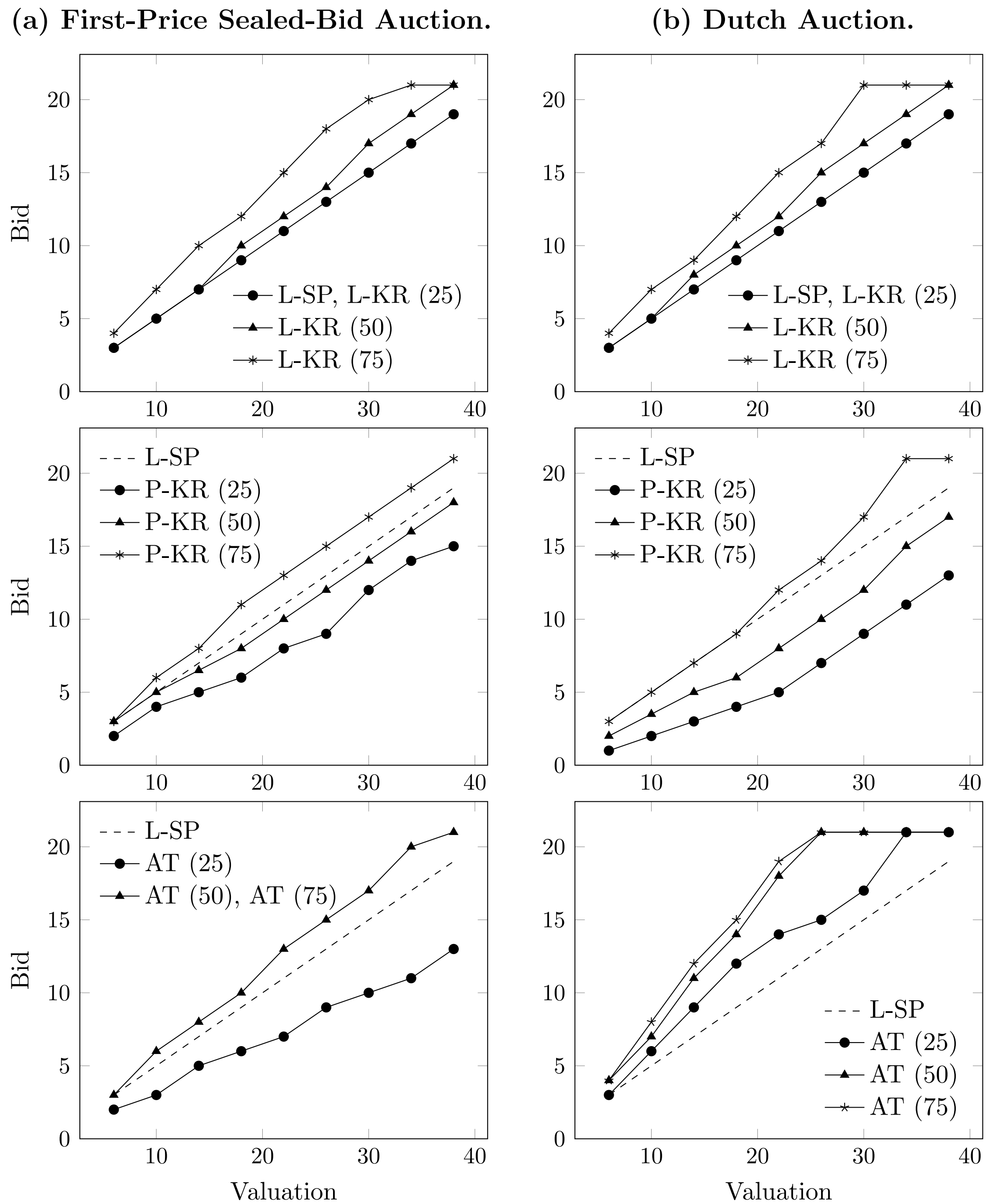


Notes: Depicted is the prediction based on standard preferences for both linear utility (L-SP) and power utility (P-SP). Standard preferences yield the same bidding strategy for both auction formats. Reported are predictions based on the 25, 50, and 75 quantiles of the curvature measure β_G.

Figure 4.2: Predictions Based on Standard Preferences.

Allais-Type Preferences. We assume that certain outcomes generate utility via $u^D(x) = x^{\beta^A}/\beta^{\text{AT}}$ and risky outcomes via $u^R(x) = \alpha^{\text{AT}} \cdot x^{\beta^A}/\beta^{\text{AT}}$. Hence, in the FPSBA, we base our predictions on $u^R(x)$. In the DA, we use $u^D(x)$ for utility derived from the current price, i.e., $x = v^i - p_k$. To evaluate the expected utility from waiting for the next price, we use $u^R(x)$ where $x = v^i - p_{k-1}$.[25]

[25]Note that for FPSBA Allais-type preferences coincide with standard preferences. Any differences result from the different elicitation methods.



Notes: Depicted are the predictions based on Linear KR preferences (L-KR, top panel), Power KR preferences (P-KR, middle panel), and Allais-type preferences (AT, bottom panel) for the FPSBA and the DA. The reference level is the prediction based on linear standard preferences (L-SP, dashed line). For Linear KR preferences, we report predictions based on the 25, 50, and 75 quantiles of the loss-aversion measure λ_{KT79}. Power KR and Allais-type preferences consist of multiple parameters. Hence, characteristics estimates cannot be ordered reasonably. For these two specifications, we report the 25, 50, and 75 quantiles of predicted bids.

Figure 4.3: Predictions Based on Non-Standard Preferences.

4.6 Results: Experiment 2

First, we compare bidding behavior across the three DSS treatments and assess the theoretical prediction that the two auction formats are strategically equivalent as predicted under standard preferences. Second, we present the accuracy of the predictions using the standard and non-standard preference specifications based on aggregate and individual parameters from Experiment 1.

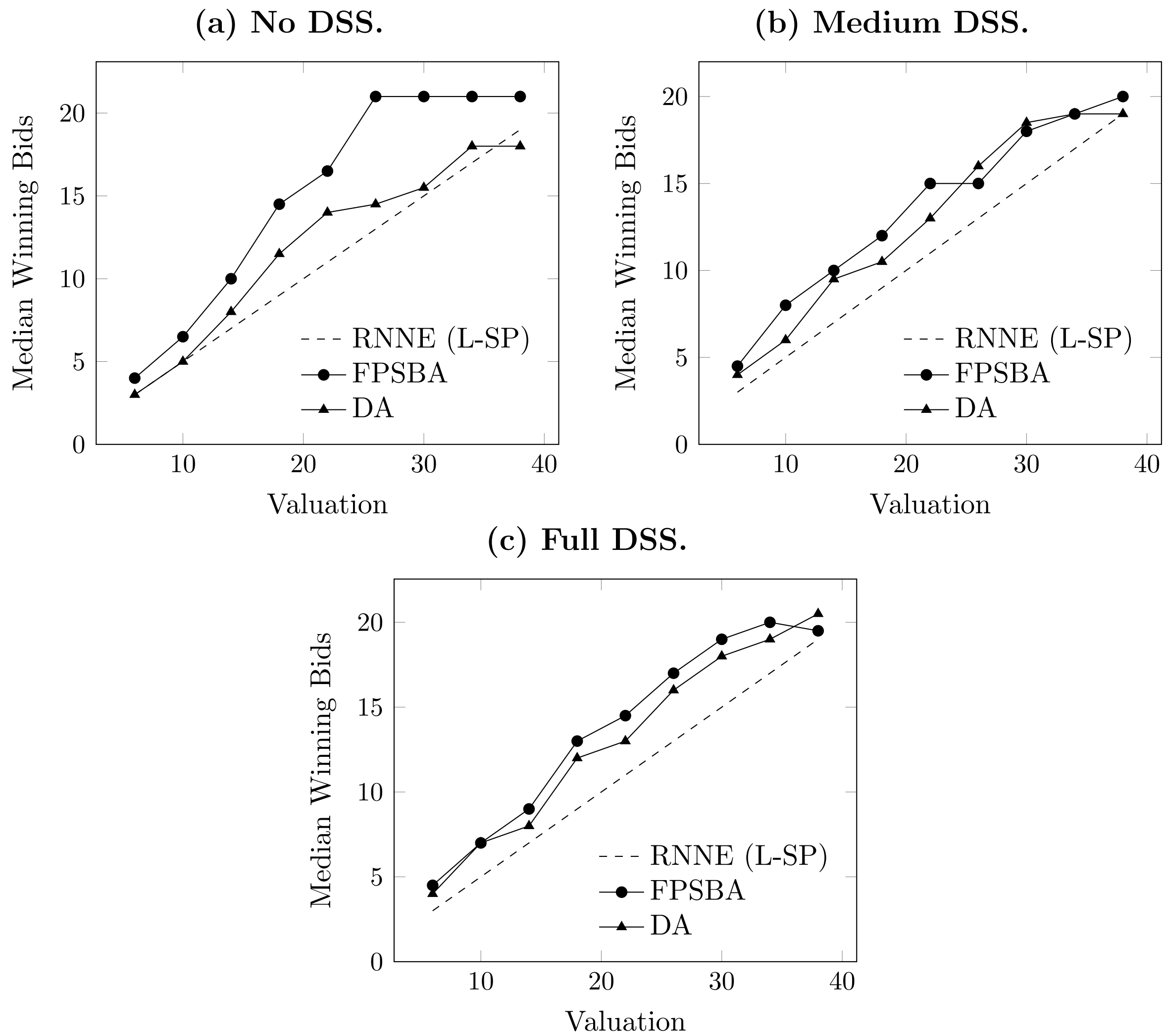
4.6.1 Strategic Equivalence

To compare the two auction formats directly, we can only consider winning bids because we only observe a bid in the DA if a participant stopped the auction and won. In line with the auction literature, we find overbidding in both formats defined relative to the risk-neutral Nash equilibrium (RNNE) benchmark given by Linear SP (L-SP). For the FPSBA, average overbidding is 2.78, which is significantly different from the benchmark (SR test, $p = 0.0000$). Specifically, overbidding in No DSS amounts to 3.45 (SR test, $p = 0.0008$), in Medium DSS to 2.45 (SR test, $p = 0.0010$), and in Full DSS to 2.34 (SR test, $p = 0.0042$). For the DA, average overbidding is 1.59 which is statistically different from the Linear SP benchmark (SR test $p = 0.0000$). In particular, overbidding is 1.14 in No DSS (SR test, $p = 0.0279$), 1.25 in Medium DSS (SR test, $p = 0.0844$), and 2.28 in Full DSS (SR test, $p = 0.0010$).

Figure 4.4 displays the median winning bids for each valuation by format over DSS treatment for the first order of auctions. A pairwise comparison of winning bids for each valuation shows that in treatment No DSS, the FPSBA generates higher prices than the DA except for the two lowest valuations based on Wilcoxon-Mann-Whitney U-tests (see Table 4.E.7 for details and p-values).[26] Treatment No DSS is comparable to standard experimental auction designs. Hence, the overbidding in the FPSBA in this treatment is also frequently observed in experimental comparisons between the FPSBA and the DA and is interpreted as evidence against the strategic equivalence of the two auction formats (e.g., Coppinger, Smith, and Titus 1980; Cox, Roberson, and Smith 1982; Cox, Smith, and Walker 1983).

However, we make two interesting observations in our data. First, the difference between the formats vanishes once we add information. In both treatments Medium DSS and Full DSS, there is no statistical difference between winning bids in the FPSBA and the DA. Recall that Medium DSS provides information about the probability to win (FPSBA) or the probability to receive the next price (DA). This manipulation is already

[26]Since reasonable bids are bounded by a subject's valuation and due to the discrete bid space, it is not surprising that bids are not significantly different for small valuations.



Notes: Depicted are medians of the winning bids for each valuation and format separated by decision support. The reference line is the risk-neutral Nash equilibrium (RNNE) given by Linear SP (L-SP). Participants in *No DSS* do not receive additional information. In treatment *Medium DSS*, participants receive information about the winning probability (FPSBA) or the probability to receive the next price (DA). In treatment *Full DSS*, participants receive the same information as in *Medium DSS* and, in addition, the expected profit associated with their bid.

Figure 4.4: Median Winning Bids Across Decision Support.

sufficient to statistically eliminate the difference between the two formats. The additional information in terms of expected values in Full DSS does not change this result.

Second, we observe that within participants, there is no difference between the two formats. In other words, it matters which format is conducted first but then subjects are consistent across formats. Table 4.E.8 reports a comparison of winning bids across the two orders. Based on the SR test, bids do not significantly differ for a given valuation. The other cited experiments that also vary the order of the two formats do not find a similar consistency in bidding. We think that the consistency in our data stems from the direct comparability of the two formats in our design by using the same price grid and holding opportunity costs constant (see Section 4.5.1). Hence, we cannot reject equivalence within a given participant. This finding is robust across all DSS conditions. We conclude that holding opportunity costs and action sets constant between the two formats makes differences insignificant for experienced bidders. However, it depends on which format they start with. Note that this between-participant non-equivalence also vanishes once information is provided. Thus, we reject strategic equivalence between participants under No DSS but not under Medium and Full DSS. This yields our first main result:

Result 4 *[Strategic Equivalence] We reject strategic equivalence between participants if no decision support is provided. However, we cannot reject it once information about the winning probability (in the FPSBA) or the probability to receive the next price (in the DA) is provided. Equivalence holds within-participants even without decision support.*

Due to the apparent order effect and the non-significant difference in bidding within-participants, the first order of the auction formats determines bidding behavior in the second order. In the following, we will thus base our results on the between-participant data of the first 18 periods (i.e., the first order for each format). In this data, every participant provides one independent observation.

4.6.2 Prediction Accuracy

This section presents the goodness of fit (GOF) of the bidding predictions based on the measurements from Experiment 1. We use three measures to assess GOF: (i) mean deviation (MD), (ii) mean absolute deviation (MAD), and (iii) mean squared deviation (MSD). The three GOF measures assess the deviation $D(v, \theta)$ between the observed winning bid $b(v)$ and the predicted bid $\hat{b}(v, \theta)$ for valuation v and preference specification $\theta = \{$L-SP, P-SP, L-KR, P-KR, AT$\}$, i.e., $D(v, \theta) = b(v) - \hat{b}(v, \theta)$. Whereas MD is a location measure, both MAD and MSD are dispersion measures.

Each GOF measure has unique characteristics necessary to obtain a comprehensive conclusion about the predictive power of each preference specification. MD shows the direction of the deviation. A positive MD indicates overbidding compared to the predicted bid (i.e., *underprediction*) whereas a negative MD indicates underbidding (i.e., *overprediction*). However, positive and negative deviations cancel each other out. This problem is overcome by the other two measures. MAD takes the average over the absolute deviation $|D(v, \theta)|$ between observed and predicted bid. Hence, deviations do not cancel and MAD is nonnegative. MSD takes the average over the squared deviation $D(v, \theta)^2$ between observed and predicted bids. MSD is also always nonnegative. Both measures cannot account for the direction of the prediction error by construction but are a measure for the magnitude of the deviation. MAD weights all deviations equally while MSD puts a smaller weight on small deviations and a larger weight on large deviations.

We analyze GOF both on the aggregate and the individual level. On the aggregate level, we use the mean and median characteristics of the population of bidders to predict individual bidding behavior. On the individual level, we use each bidder's individual behavior in Experiment 1 to derive predictions for his bidding behavior. Specifically, for each valuation v, $D^i(v, \theta)$ compares the individual observed bid $b^i(v)$ with the prediction $\hat{b}^i(v, \theta^i)$ that is based on the same bidder's measurement for the preference specification θ. Note again that we only consider winning bids in the FPSBA to be able to compare the results with those in the DA.

Aggregate Level

Table 4.E.9 reports all GOF measures based on the mean data for each preference specification and DSS treatment.[27] Table 4.4 presents the MAD for reference. The results for the Linear SP prediction are separated because they are independent of the measurements of Experiment 1 and always given by $\beta^{\text{L-SP}}(v) = v/2$. Thus, we set Linear SP as the benchmark and assess significance relative to this benchmark with the SR test.

Panel A of Table 4.E.9 shows the results for MD. We see that the predictions based on Linear SP and Linear KR preferences with linear utility (Linear KR) have the smallest mean deviation. For the FPSBA, all values are positive implying underprediction. In other words, participants show extensive overbidding given the prediction. In the DA, Linear KR and Allais-type preferences actually overpredict bidding behavior, i.e., observed bids are smaller than predicted. Panel A further shows that Linear KR generally has the smallest average prediction error which is further significantly different from the benchmark given by Linear SP in both auction formats.

Table 4.4 (Panel B of Table 4.E.9) reports the result for the first dispersion measure MAD. Similar to the MD results, Linear SP and Linear KR show the smallest prediction error for the median data. This holds for both FPSBA and DA. We see that all predictions besides Power SP show a similar dispersion because the MAD weights all deviations linearly. Panel C of Table 4.E.9 reports the results for the second dispersion measure based on the squared deviation. It is apparent that Power SP provide a much larger MSD than the other measures. This indicates that Power SP predictions yield some very large deviations which are hence amplified in the MSD measure. For the FPSBA, this poor fit occurs primarily under No DSS but remains around an order of magnitude three for the other two DSS treatments. For the DA, Power SP also performs the worst but the difference is not as striking as in the FPSBA. Both deviation measures select Linear KR preferences as the best prediction which is significantly different from Linear SP in the FPSBA but not statistically distinguishable in the DA.

[27] Table 4.E.10 reports the same measures based on the median data. The results are generally consistent with the mean data.

Table 4.4: Mean Absolute Deviation for Mean Data.

Format	First-Price Sealed-Bid Auction				Dutch Auction			
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	3.74	2.77	2.93	3.17	2.80	2.77	2.88	2.82
	(1.43)	(1.08)	(1.45)	(1.37)	(0.98)	(1.19)	(1.09)	(1.06)
Power SP	7.39***	6.24***	6.40***	6.71***	5.05***	5.43***	6.42***	5.66***
	(1.97)	(1.37)	(1.57)	(1.71)	(1.81)	(2.17)	(1.49)	(1.87)
Linear KR	2.14***	2.17*	2.28*	2.19***	2.91	2.69	1.77***	2.43
	(0.93)	(0.59)	(0.90)	(0.80)	(1.03)	(1.52)	(0.74)	(1.20)
Power KR	4.36***	3.23***	3.38***	3.69***	3.96***	4.32***	4.99***	4.44***
	(1.59)	(1.27)	(1.47)	(1.51)	(1.54)	(1.83)	(1.45)	(1.62)
Allais-Type	3.96***	2.85	2.98	3.30***	3.10	2.67	2.08**	2.60
	(1.46)	(1.19)	(1.38)	(1.42)	(1.24)	(1.79)	(0.64)	(1.31)

Notes: Reported is the mean of the goodness-of-fit (GOF) measure mean absolute deviation (MAD). The deviation is the difference between the individual observed winning bid and the predicted bid based on the mean measurement for Power SP and Linear KR preferences or the mean predicted bid for Power KR and Allais-type. Standard deviation in parentheses. Asterisks indicate a significant difference between the GOF measure and the benchmark of linear standard preferences (Linear SP) based on the Wilcoxon signed-rank test. * < 0.10, ** < 0.05, *** < 0.01.

Result 5 *[Aggregate Accuracy] Based on mean and median data, Linear SP and Linear KR generally yield the best prediction accuracy across the three goodness-of-fit measures. Overall, Linear KR preferences yield a significant better fit in the FPSBA but are not statistically different from Linear SP in the DA.*

Individual Level

Table 4.E.11 reports the GOF based on the individual data for each preference specification and DSS treatment. We again test whether each GOF is significantly different from the GOF of the Linear SP benchmark based on the SR test. The individual analysis confirms the impression obtained from the aggregate data. Linear KR preferences generally have the best prediction accuracy closely followed by Linear SP. This difference is significant in the FPSBA but not in the DA.

Panel A of Table 4.E.11 shows the results for MD. Linear KR has the smallest mean prediction error across all DSS treatments and for both auction formats. For the FPSBA, Linear KR underpredicts under No and Medium DSS but overpredicts for Full DSS. For the DA, the pattern is different: overprediction under No DSS and underprediction for Medium and Full DSS. For both formats, Power SP and Power KR have a positive MD and thus show underprediction across all DSS treatments. Allais-type preferences underpredict for the FPSBA and generally overpredict for the DA. The good performance of Linear KR preferences is further significantly better than the benchmark.

Table 4.5: Mean Absolute Deviation for Individual Data.

Format	First-Price Sealed-Bid Auction				Dutch Auction			
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	3.74	2.77	2.93	3.17	2.80	2.77	2.88	2.82
	(1.43)	(1.08)	(1.45)	(1.37)	(0.98)	(1.19)	(1.09)	(1.06)
Power SP	4.44	3.71**	5.10*	4.38**	3.72	2.90	3.13	3.26
	(3.17)	(1.43)	(3.23)	(2.71)	(2.05)	(2.57)	(1.60)	(2.05)
Linear KR	2.67*	1.98*	3.12	2.57**	3.12	2.91	2.18*	2.72
	(1.09)	(0.82)	(1.39)	(1.18)	(1.08)	(1.69)	(1.20)	(1.36)
Power KR	4.18	4.39***	4.62*	4.38***	5.11***	4.32	5.47***	5.01***
	(3.53)	(1.50)	(2.53)	(2.62)	(2.70)	(4.12)	(3.11)	(3.27)
Allais-Type	4.80	3.25	3.40	3.86	4.06**	4.34*	4.61	4.34***
	(3.79)	(2.33)	(2.40)	(2.98)	(1.96)	(2.53)	(3.19)	(2.57)

Notes: Reported is the mean of the goodness-of-fit (GOF) measure mean absolute deviation (MAD). The deviation is the difference between the individual observed winning bid and the predicted bid based on the individual measurement. Standard deviation in parentheses. Asterisks indicate a significant difference between the GOF measure and the benchmark of linear standard preferences (Linear SP) based on the Wilcoxon signed-rank test. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

Table 4.5 (Panel B of Table 4.E.11) reports dispersion based on the MAD. Linear KR again has the smallest dispersion in prediction accuray and is significantly different from Linear SP for No and Medium DSS in the FPSBA and but only for Full DSS in the DA. Hence, on average, Linear KR is not significantly different from the benchmark in the DA. Power SP show, on average, significantly worse prediction accuracy than Linear SP in the FPSBA but the difference is not statistically different from zero in the DA. Similar to Power SP, Power KR preferences yield a worse fit than Linear SP and this difference is significant on average across both formats. Although Allais-type preferences show a worse accuracy than the benchmark, this difference is only significant in the DA. Using the MSD in Panel C of Table 4.E.11 shows a similar pattern as the MAD. Linear KR generally performs best while Power KR, Power SP, and Allais-type preferences are basically equally bad predictors with an MSD that is around three times as large as that of Linear KR preferences.

Result 6 *[Individual Accuracy] Based on the individual data, Linear SP and Linear KR generally yield the best prediction accuracy across the three goodness-of-fit measures. On average, both preference specifications underpredict bidding behavior, i.e., participants choose higher bids relative to their individual prediction. Overall, Linear KR preferences yield a significantly better fit in the FPSBA but are not statistically different from Linear SP in the DA.*

Prediction Accuracy: The Role of Numeracy

We analyze how the prediction accuracy relates to further individual characteristics. Our main independent variable is the numeracy score (*Numeracy*) elicited in Experiment 1 to account for mathematical illiteracy (see Section 4.3.3). We interact this score with the three DSS conditions to identify the impact of numeracy when decision support is available. As the GOF measure, we focus on the MAD in this section for two reasons. First, we use a dispersion measure instead of the MD because the latter is a net sum of negative and positive deviations. Although, in general MD and MAD are similar due to the general pattern of overbidding relative to the prediction, we prefer to base our analysis on the absolute deviation. Second, MAD shows the same pattern as MSD in general but we think that MAD is easier to interpret.

Regarding Numeracy, we cannot reject a normal distribution of answers according to the Shapiro-Wilk test ($p = 0.2762$) or the Shapiro-Francia test ($p = 0.5575$). Based on a Skewness-Kurtosis test, we find evidence for a slight negative skew of -0.5234. However, the joint test for normality in this case is also non-significant ($p = 0.1150$). The negative skewness confirms our choice of the combined test given our student population because 77.11% answer all SNT questions correctly. Hence, our incentivized implementation of the combined test yields very similar results as Cokely et al. (2012) and the distribution of scores over the full range makes it a good estimator for decision biases in our parametric regressions.

We estimate two OLS regression models separately for the two auction formats. Model 1 regresses MAD on the DSS treatments (with *No DSS* as the reference category), *Numeracy*, and the interaction between Numeracy and the DSS treatments. Model 2 is the same as the first model but additionally controls for the demographic variables *Age*, a dummy indicating male participants (*Male*), and a dummy indicating whether German is the native language of the participant (*German Native*) which is a coarse proxy of the understanding of the instructions and possible cultural differences. We report heteroscedasticity-robust standard errors. Appendix 4.E.5 reports all regression tables.

For Linear SP, the prediction accuracy generally does not depend on mathematical literacy. Numeracy is only significant in the DA but not once we control for demographics. In addition, we do not find a difference between the DSS treatments. Similarly, the prediction accuracy of Power SP does also neither depend on Numeracy nor on the provided decision support. For Linear KR preferences, we do not find a significant main effect of numeracy on the prediction accuracy. In the DA, numeracy interacts with Full DSS which itself has a large negative main effect. This means, that the prediction is more accurate in the Full DSS condition but within that condition, the prediction is slightly worse for those subjects who have better mathematical abilities. For Power KR preferences, Numeracy has a diametric effect depending on the auction format. In the FPSBA, Numeracy worsens prediction accuracy while it enhances it in the DA. We find no interaction between numeracy and decision support. Finally, for Allais-type preferences, there is no significant effect of Numeracy or DSS treatment on prediction accuracy.

Overall, Numeracy by itself only has a significant effect under Power KR preferences which is positive in the FPSBA but negative in the DA. Power KR preferences generally perform the worst based on the MAD calculated from individual data. This low prediction accuracy is thus further weakened in the FPSBA but slightly enhanced in the DA for participants with higher numeracy scores. The regression results further indicate that the prediction accuracy of the two best-performing preference specifications, Linear SP and Linear KR preferences, does neither depend on the DSS treatment nor on the mathematical ability in the static FPSBA. However, in the dynamic DA, more information (Full DSS) significantly increases the prediction accuracy for Linear KR preferences. This effect outweighs the slight adverse interaction effect of Full DSS and Numeracy.

Result 7 *[Numeracy] Generally, Numeracy has little impact on prediction accuracy particularly for the best predictors Linear KR and Linear SP. Hence, the prediction accuracy is absolute in the sense that we do not systematically predict participants with higher mathematical abilities differently well from those with low abilities.*

4.7 Internal Validity

We base our predictions on the elicited utility from Experiment 1. This section discusses the reliability of the elicitation procedure of Experiment 1. We argue that the heterogeneous fit of the preference specifications and in particular the poor fit under power utility does not result from our implementation of preference elicitation. We first present an auxiliary result about the degree of probability weighting in our sample. This data is a side product of the procedure of Abdellaoui, Bleichrodt, and Paraschiv (2007) and further confirms that our results are in line with the literature even though we use monetary instead of hypothetical incentives. Subsequently, we discuss the choice of using the non-parametric elicitation procedure by Abdellaoui, Bleichrodt, and Paraschiv (2007). We present possible problems arising from the chaining of decisions and argue that they do not influence our measurements.

Probability Weighting

Expected utility is linear in probabilities (Machina 2008a). However, Kahneman and Tversky (1979) find that subjects frequently deviate from linearity by overweighting small and underweighting large probabilities. The non-parametric elicitation procedure of Abdellaoui, Bleichrodt, and Paraschiv (2007) yields two probabilities that allow us to comment on the extent of probability weighting around 0.5. We derive the objective probability p^G for gains that feels like 0.5 for the individual. In other words, the weighting function on the gain domain, $w^G(\cdot)$, transforms p^G into 0.5, i.e., $w^G(p^G) = 0.5$. Further, we analogously elicit the probability p^L on the loss domain such that $w^L(p^L) = 0.5$. However, we note that, in contrast to the elicited utility, these measurements are point estimates and not the entire weighting function. On average, these probabilities are given by $p^G = 0.569$ and $p^L = 0.566$ and both are significantly different from 0.5 (SR test, $p = 0.0099$ for p^G and $p = 0.0239$ for p^L) but not distinguishable from each other.

Hence, close to the literature, we observe probability underweighting around 0.6. Abdellaoui, Bleichrodt, and Paraschiv (2007) observe median values of 0.59 for p^G and 0.6 for p^L. Kahneman and Tversky (1979) observe 0.65 for p^G and 0.57 for p^L. Our results are only statistically different from the latter value (SR test, $p = 0.0078$). Accounting for the precision of the bisection algorithm (± 0.015625), on the gain domain, a total of 49 participants (59.04%) show probability underweighting and 26 (31.33%) show probability overweighting. On the loss domain, 46 participants (55.42%) show probability underweighting and 27 (32.53%) show probability overweighting. We interpret these results as confirming our incentivized implementation.

Comments on the Elicitation Procedure

The procedure of Abdellaoui, Bleichrodt, and Paraschiv (2007) is non-parametric because it does not require assumptions about the functional form of utility. In contrast, parametric elicitations require a certain specification for identification. These specifications imply a link between preferences and their mathematical representation that may not be valid in reality (Van de Kuilen and Wakker 2011). Elicited values thus depend on the particular parameterization. One advantage of parametric measurements is that they are more efficient in the sense that far less pairwise comparisons are required (Abdellaoui et al. 2008). This is valuable if there is not much time in the experiment for the elicitation and explains the popularity of (semi-)parametric measurements (e.g., Abdellaoui et al. 2008; Abdellaoui, Driouchi, and L'Haridon 2011b; Abdellaoui et al. 2011a; Booij and Van de Kuilen 2009, and Karle, Kirchsteiger, and Peitz 2014). However, due to the drawbacks of parametric assumptions, in particularly in the measurement of loss aversion, we decided to elicit preferences non-parametrically.

Sequentially eliciting indifference points has two potential drawbacks: (i) incentive compatibility and (ii) error propagation. Regarding the first point, note that procedures based on the trade-off method are chained in the sense that subsequent lotteries are constructed based on the response in previous comparisons. This chained structure may potentially distort the incentive compatibility of the elicitation even if lotteries are played out with actual monetary consequences (Harrison 1986; Harrison and Rutström 2008). Participants may distort their choices to improve the lotteries that are presented to them subsequently (Van de Kuilen and Wakker 2011). Similar to Baillon (2008), we mitigate this problem by perturbing the order in which lotteries are presented to conceal the chaining of stimuli.[28] We have to maintain a certain order of elicitation in the chained procedure (e.g., step-1 elicitations have to be performed before step-2 elicitations and so on). However, we mix the elicitation of gains with the elicitation of losses in step 1. Further, we mix the elicitation of risk and loss aversion with the independent elicitation of Allais-type preferences.

We think that the above approach makes inferring the chained structure of the elicitation very hard for participants. In addition, Bleichrodt, Cillo, and Diecidue (2010) note that because indifference values are elicited indirectly, they are never presented to the subject which further impairs inferring the chained structure of the trade-off method. Van de Kuilen and Wakker (2011) argue that subjects would have to be aware of the presence of chaining and understand how former answers map into the construction of subsequent lotteries. This requires a very demanding introspection. Neither Bleichrodt, Cillo, and Diecidue (2010) nor Van de Kuilen and Wakker (2011) find evidence for strategic responses. We further follow Van de Kuilen and Wakker (2011) and check whether participants were aware of the chained structure by asking two strategy-check questions.[29] Two student assistants independently of each other classified the answers.[30] They found no indication that subjects noticed the chained structure of the elicitation.

As the second potential drawback, chained procedures are prone to error propagation because one answer feeds into the next question. However, based on simulation studies, Bleichrodt and Pinto (2000); Abdellaoui, Vossmann, and Weber (2005), and Van de Kuilen and Wakker (2011) show that error propagation is a negligible concern. In addition, in our experiment, subjects have to wait for at least ten seconds before they can move on to the next lottery pair. We think that this further reduces accidentally choosing a non-preferred lottery.

[28]Cerroni, Notaro, and Shaw (2012) find that hiding the chained structure indeed yields more valid risk estimates than clearly stating the chained structure and that real monetary incentives outperform hypothetical monetary incentives.

[29]Question 1: *"Was there any reason for you to choose left lotteries or right lotteries more often?"* [German: "Gab es einen Grund für Sie, linke Lotterien oder rechte Lotterien häufiger zu wählen?"]. Question 2: *"Can you briefly explain how you determined your choice?"* [German: "Können Sie kurz begründen, wie Sie Ihre Wahl getroffen haben?"]. Both questions are adapted from Van de Kuilen and Wakker (2011, p. 586). We slightly changed their wording to sound more idiomatic in German.

[30]Instructions: *"Can the answers be categorized in any way?"* [German: "Können die Antworten in irgendeiner Form kategorisiert werden?"].

4.8 Conclusion

We examine the predictive power of different preference specifications as well as the role of decision support in first-price sealed-bid and Dutch auctions. In a laboratory experiment, we first elicit each participant's utility function non-parametrically and use this data to derive various measures regarding risk and loss attitudes as well as Allais-type preferences. We then use these individual characteristics to predict bidding behavior in a follow-up experiment with the same participants. We vary the degree of decision support to account for the complexity in deriving the optimal bid.

We confirm the frequently observed non-equivalence of the first-price and Dutch auction under the absence of decision support (No DSS). In addition, we observe that any differences vanish once we provide information about the winning probability. Differences between the two auction formats based on preferences should be independent of the level of decision support. Our results thus indicate that the empirical breakdown of strategic equivalence is primarily caused by the complexity of the bidding decision rather than by bidders' preferences. In addition, Cox, Smith, and Walker (1983) argue that differences between the two mechanisms result from violations of Bayes' rule and indirectly test this conjecture by tripling individual payoffs which increases opportunity costs from miscalculations. In contrast, our design is a direct test of the impact of cognitive limitations and we find additional evidence for this conjecture.

In the experiment, the implemented DSS is perfect in the sense that we can precisely calculate the respective probabilities and expected values due to the fixed bidding strategy of the bidding robot. Obviously, this is not directly implementable in real auctions. However, the availability of historical bid data promotes the design of decision support systems similar to our implementation. Thus, our findings on the differences in auction formats indicates that the higher revenue in the FPSBA is less relevant in real auctions where bidders are likely to have such support.

Furthermore, we find no difference in bidding behavior within participants even without providing additional information. That is, subjects do not vary their bidding behavior if the auction format changes. This within-participant consistency is in contrast to the literature and we relate this finding to the strict comparability of the two formats in our experiment. Hence, whereas previous designs may be biased toward finding a difference, the bidding in our data indicates that a constant action set and fixed opportunity costs are necessary for consistency between the two formats.[31]

Regarding the prediction accuracy of elicited preferences, we find that both on the aggregate and the individual level, expectations-based reference-dependent preferences

[31] Opportunity costs include, e.g., monitoring costs (Carare and Rothkopf 2005) or costs from participating in the experiment (Katok and Kwasnica 2007).

with a linear utility function (Linear KR preferences) yield the overall best fit. Standard preferences with linear utility (Linear SP) perform second-best. These results are generally robust against controlling for demographics and numeracy.

Our results provide evidence for the skepticism of Kagel and Roth (1992) and Goeree, Holt, and Palfrey (2002) who doubt that risk aversion is a good explanation of overbidding in auctions. We find that risk attitudes (risk aversion and risk seeking) actually predict bidding behavior worst across all considered preference specifications. The degree of risk aversion that is necessary to explain the observed overbidding highly exceeds our estimates based on the lottery choices of our participants. We find no significant correlation between risk aversion and overbidding.

Goeree, Holt, and Palfrey (2002) further point out that probability weighting also explains overbidding but they cannot differentiate risk aversion and probability weighting. Our results indicate that probability weighting is not a good explanation either. This is because probability weighting changes the perceived expected value of a decision. However, participants with full decision support directly see the objective expected value which is independent from nonlinear probability weighting. As we find no difference between medium and full decision support, we conclude that the difference between the FPSBA and the DA is primarily due to difficulties in deriving probabilities (complexity) and not from weighting such probabilities nonlinearly.

Appendices

4.A Theoretical Appendix

4.A.1 First-Price Sealed-Bid Auction

In the following, we describe how to derive the bidding function for a first-price sealed-bid auction with standard preferences in detail (see also Chwe 1989; Cai, Wurman, and Gong 2010). The bidding functions concerning other preference specifications are derived in a similar fashion and we cover the differences here.

Linear Standard Preferences

Let there be two bidders and fix a bid grid $B = \{b_1, b_2, \ldots, b_j\}$ with $b_{k+1} = b_k + \delta$ and $b_1 = 0$. We prove that there exists a sequence$\{z_k\}_{k \in \{1,\ldots,n\}}$ with $z_1 < z_2 < ... < z_n$ such that

$$\beta(v) = \begin{cases} b_1 & \text{for } v_i \in [0, z_1] \\ b_k & \text{for } v_i \in (z_{k-1}, z_k] \text{ with } k \geq 2 \end{cases} \tag{4.A.1}$$

constitutes an equilibrium bidding strategy. We begin by writing down the expected profit for a fixed price b_k, given that the other bidder plays according to the bidding strategy β,

$$\begin{aligned} E[\pi^i|b_k] = &\ Pr\{v^j \leq z_{k-1}\}[v^i - b_k] + Pr\{z_{k-1} < v^j \leq z_k\}\frac{1}{2}[v^i - b_k] \\ = &\ \frac{1}{2}\Big[F(z_{k-1}) + F(z_k)\Big][v^i - b_k] \\ =: &\ P_\omega^k\ [v^i - b_k], \end{aligned}$$

where P_ω^k is the winning probability of a bid b_k. We note that for $z_1 < z_2 < \ldots < z_n$, it holds that $P_\omega^k < P_\omega^{k+1}$ for all k. The mapping $v \mapsto \beta(v)$ should establish an equilibrium, meaning that no other bid should be a better choice for the bidder. Since the winning probability and the utility function are both monotonic in b, it suffices to compare the expected profits from bidding b_{k-1} and b_{k+1} with the expected profit from submitting b_k. We have

$$E[\pi^i|b_{k+1}] = [v^i - b_{k+1}]P_\omega^{k+1} \tag{4.A.2}$$

$$E[\pi^i|b_{k-1}] = [v^i - b_{k-1}]P_\omega^{k-1}. \tag{4.A.3}$$

The comparisons yield the following inequalities:

$$E[\pi^i|b_k] \overset{!}{\geq} E[\pi^i|b_{k+1}] \text{ and} \tag{4.A.4}$$

$$E[\pi^i|b_k] \overset{!}{\geq} E[\pi^i|b_{k-1}]. \tag{4.A.5}$$

We note that $b_{k\pm1} = b_k \pm \delta$ and hence the first inequality can be rewritten as

$$\begin{aligned} v^i\Big[F(z_{k-1}) - F(z_{k+1})\Big] &\geq b_k\Big[F(z_{k-1}) - F(z_{k+1})\Big] - \delta\Big[F(z_{k+1}) + F(z_k)\Big] \\ v^i\Big[F(z_{k+1}) - F(z_{k-1})\Big] &\leq b_k\Big[F(z_{k+1}) - F(z_{k-1})\Big] + \delta\Big[F(z_{k+1}) + F(z_k)\Big] \\ v^i &\leq \underbrace{b_k + \delta\frac{F(z_{k+1}) + F(z_k)}{F(z_{k+1}) - F(z_{k-1})}}_{:=z_k}. \end{aligned}$$

Analogously, we rewrite the second inequality as

$$v^i \geq \underbrace{b_{k-1} + \delta\frac{P_\omega^k}{P_\omega^k - P_\omega^{k-1}}}_{:=z_{k-1}} = b_{k-1} + \delta\frac{F(z_k) + F(z_{k-1})}{F(z_k) - F(z_{k-2})}.$$

To compute z_1, we assume that no bidder's valuation exceeds z_1. Then, both bidders bid b_1 and P_ω^1 is equal to

$$P_\omega^1 = F(z_1). \tag{4.A.6}$$

It can be seen that it suffices to either compute the left bounds from which on the bidder would bid b_k or the right bounds until which she would bid b_k. This would only induce a slight modification in the bidding strategy from above.

To compute the sequence $\{z_k\}_{k\in\mathbb{N}}$ recursively, we still need an initial value. There are two natural choices. The first one is to set z_1 equal to zero; the second one, which coincides with our bidding strategy from above, is to set z_n equal to one. The valuations of the buyers are drawn from a continuous distribution, so it does not matter how we define the bidding function for specific valuations. Therefore, we set z_n to one and define that if buyer i's valuation lies between $v^i \in (z_{k-1}, z_k]$, he bids b_k. We have shown that β indeed constitutes a Bayes Nash Equilibrium since bidding according to this bidding strategy is a best response to a buyer playing the same strategy.

Power Standard Preferences

In the case of risk aversion or risk affinity, the utility function is given by a convex or a concave transformation on the utility $v - b$. Let us assume that the transformation is given by $f(x) := x^\beta$ with $\beta > 0$. The reasoning from above still holds in this case. For the first inequality, we have

$$[v^i - b_k]^\beta\ P_\omega^k \geq [v^i - b_k - \delta]^\beta\ P_\omega^{k+1} \tag{4.A.7}$$

$$\Leftrightarrow [v^i - b_k]\Big[(P_\omega^{k+1})^{1/\beta} - (P_\omega^k)^{1/\beta}\Big] \leq \delta\big[P_\omega^{k+1}\big]^{1/\beta} \tag{4.A.8}$$

$$\Leftrightarrow v^i \leq \underbrace{b_k + \delta\frac{\big(F(z_{k+1}) + F(z_k)\big)^{1/\beta}}{(F(z_k) + F(z_{k+1}))^{1/\beta} - (F(z_{k-1}) + F(z_k))^{1/\beta}}}_{:=z_k}. \tag{4.A.9}$$

For the first bound, we get

$$z_k := b_k + \delta \frac{\big(F(z_{k+1}) + F(z_k)\big)^{1/\beta}}{(F(z_k) + F(z_{k+1}))^{1/\beta} - (F(z_{k-1}) + F(z_k))^{1/\beta}}, \tag{4.A.10}$$

for the second bound, we get

$$z_{k-1} = b_{k-1} + \delta \frac{\big(P_\omega^k)\big)^{1/\beta}}{(P_\omega^k)^{1/\beta} - (P_\omega^{k-1})^{1/\beta}}. \tag{4.A.11}$$

It holds that

$$\frac{\big(P_\omega^{k+1}\big)^{1/\beta}}{(P_\omega^{k+1})^{1/\beta} - (P_\omega^k)^{1/\beta}} > \frac{P_\omega^{k+1}}{P_\omega^{k+1} - P_\omega^k} \tag{4.A.12}$$

for $\beta > 1$ and

$$\frac{\big(P_\omega^{k+1}\big)^{1/\beta}}{(P_\omega^{k+1})^{1/\beta} - (P_\omega^k)^{1/\beta}} < \frac{P_\omega^{k+1}}{P_\omega^{k+1} - P_\omega^k} \tag{4.A.13}$$

for $\beta < 1$. In (4.A.10), we can see that if the right-hand side becomes smaller, the left-hand side needs to become smaller as well. With this, we have shown that the equilibrium threshold values z_k are decreasing in the participant's risk aversion.

KR Preferences

In the case of KR preferences, the utility function is slightly more complex. It is given by

$$u^{\mathrm{KR}}(x, r) = u(x) + n(x, r) \tag{4.A.14}$$

with

$$n(x, r) = \begin{cases} \eta(u(x) - u(r)) & \text{if} \quad x > r \\ \eta\lambda(u(x) - u(r)), & \text{if} \quad x \leq r. \end{cases} \tag{4.A.15}$$

Let η be normalized to one and $\lambda > 0$. Then, the expected utility for a fixed bid b_k, given that the other buyer bids according to $\beta(v)$, is given by

$$E[\pi^i|b_k] = P_\omega^k[v^i - b_k] + P_\omega^k(1 - P_\omega^k)[v^i - b_k] - \lambda P_\omega^k(1 - P_\omega^k)[v^i - b_k] \tag{4.A.16}$$

$$= P_\omega^k\Big[1 + (1 - \lambda)\big(1 - P_\omega^k\big)\Big][v^i - b_k]. \tag{4.A.17}$$

This yields

$$z_k^{\mathrm{KR}} := b_k + \delta \frac{P_\omega^{k+1}\big[1 + (1-\lambda)\big(1 - P_\omega^{k+1}\big)\big]}{P_\omega^{k+1}\big[1 + (1-\lambda)\big(1 - P_\omega^{k+1}\big)\big] - P_\omega^k\big[1 + (1-\lambda)\big(1 - P_\omega^k\big)\big]}. \tag{4.A.18}$$

For $0 \leq \lambda \leq 2$, the effect of reference dependence is clear since for $0 \leq \lambda < 1$,

$$\frac{P_\omega^{k+1}\big[1 + (1-\lambda)\big(1 - P_\omega^{k+1}\big)\big]}{P_\omega^{k+1}\big[1 + (1-\lambda)\big(1 - P_\omega^{k+1}\big)\big] - P_\omega^k\big[1 + (1-\lambda)\big(1 - P_\omega^k\big)\big]} > \frac{P_\omega^{k+1}}{P_\omega^{k+1} - P_\omega^k} \tag{4.A.19}$$

whereas for $1 < \lambda \leq 2$, we have

$$\frac{P_\omega^{k+1}\big[1+(1-\lambda)\big(1-P_\omega^{k+1}\big)\big]}{P_\omega^{k+1}\big[1+(1-\lambda)\big(1-P_\omega^{k+1}\big)\big]-P_\omega^{k}\big[1+(1-\lambda)\big(1-P_\omega^{k}\big)\big]} < \frac{P_\omega^{k+1}}{P_\omega^{k+1}-P_\omega^{k}}. \tag{4.A.20}$$

This is, equilibrium threshold values z_k are decreasing in the participant's loss aversion if $\lambda \in [0,2]$. For $\lambda > 2$ and few price steps, it is not guaranteed that the sequence of z_k^{KR} is increasing which raises problems.

Allais-Type Preferences

As discussed in the main section of the paper, Allais-type preferences do not lead to a different optimization problem compared to standard preferences since there are no certain payoffs. The easiest way to see this is to boost the degenerate case instead of penalizing uncertain outcomes. Then, with

$$u^{\mathrm{AT}}(x) = \begin{cases} u^D(x) & \text{if } x \in X^D \\ u^R(x) & \text{if } x \in X^R, \end{cases} \tag{4.A.21}$$

one can write $u^R(x) = x$ and the same reasoning as under standard preferences applies.

4.A.2 Dutch Auction

We model the Dutch auction as in Bose and Daripa (2009). Let H_k^i be the probability that, with a given distribution $F(v)$, bidder i receives the item at price p_{k+1} given that she refuses the price p_k. The probability H_k^i consists of two parts: (i) the probability ϕ_k^i under F that bidder i obtains the item at the next price p_{k+1} given that it is still available at that price, i.e., that it has not been sold at price p_k; and (ii) the probability ρ_k^i under F that the item is actually available at price p_{k+1} given that bidder i refused price p_k. Consequently, $H_k^i = \phi_k^i \cdot \rho_k^i$.

Computation of ϕ_k^i

The probability to be asked first is one half. If bidder i accepts the offered price p_k, she receives the item with certainty. If bidder j is asked first, bidder i receives the item only if bidder j refuses the current price p_k. If the item is still available at p_k, it follows that $v^j < z_k$. Remember that z_k is the lowest valuation at which a price p_k is still accepted. Hence, bidder j will not accept the next price p_{k+1} (given that she has refused the current price p_k) with probability $\Pr\{v^j < z_{k+1} | v^j < z_k\} = F(z_{k+1})/F(z_k)$. Hence,

$$\phi_k^i = \frac{1}{2} + \frac{1}{2} \cdot \frac{F(z_{k+1})}{F(z_k)}. \tag{4.A.22}$$

Computation of ρ_k^i

Define the following events. First, denote by $\#_k^i \in \{1,2\}$ the position of bidder i in period k. Second, denote by A_k^i the event that the price p_k is offered to bidder i. We will now determine the probability of being offered the current price p_k:

$$
\begin{aligned}
&\Pr\{\#_k^i = 1|A_k^i\} && (4.A.23)\\
=&\frac{\Pr\{\#_k^i = 1\}\cdot\Pr\{A_k^i|\#_k^i = 1\}}{\Pr\{\#_k^i = 1\}\cdot\Pr\{A_k^i|\#_k^i = 1\} + \Pr\{\#_k^i = 2\}\cdot\Pr\{A_k^i|\#_k^i = 2\}} && (4.A.24)\\
=&\frac{\frac{1}{2}}{\frac{1}{2}+\frac{1}{2}\cdot\frac{F(z_k)}{F(z_{k-1})}} && (4.A.25)\\
=&\frac{F(z_{k-1})}{F(z_{k-1})+F(z_k)}. && (4.A.26)
\end{aligned}
$$

Note that $F(v_0^j) = 1$, i.e., the probability that bidder j buys before the starting price is zero. It holds that

$$
\begin{aligned}
\Pr\{\#_k^i = 2|A_k^i\} =&1 - \Pr\{\#_k^i = 1|A_k^i\} && (4.A.27)\\
=&\frac{F(z_k)}{F(z_{k-1})+F(z_k)}. && (4.A.28)
\end{aligned}
$$

The probability that the price p_{k+1} is reached consists of two terms. The first term reflects the case where bidder i is asked first at the current price p_k and refuses this price. This is given by $\Pr\{\#_k^i = 1|A_k^i\}$. Subsequently, bidder j refuses the price with probability $F(z_k)/F(z_{k-1})$. The second term reflects the situation where bidder i is asked after bidder j was asked first and refuses. Then, the next price is reached with certainty. Hence the probability ρ_k^i that the item is actually available at price p_{k+1} given that bidder i refuses price p_k is given by

$$
\begin{aligned}
\rho_k^i =&\Pr\{\#_k^i = 1|A_k^i\}\cdot\frac{F(z_k)}{F(z_{k-1})} + \Pr\{\#_k^i = 2|A_k^i\}\cdot 1 && (4.A.29)\\
=&\frac{2\cdot F(z_k)}{F(z_{k-1})+F(z_k)}. && (4.A.30)
\end{aligned}
$$

Computation of H_k^i

We have

$$
\begin{aligned}
H_k^i =&\rho_k^i\cdot\phi_k^i && (4.A.31)\\
=&\frac{F(z_k)+F(z_{k+1})}{F(z_k)+F(z_{k-1})}. && (4.A.32)
\end{aligned}
$$

In the case of the uniform distribution on $[0,1]$, this probability reads

$$
H_k^i = \frac{z_k + z_{k+1}}{z_k + z_{k-1}}. \qquad (4.A.33)
$$

Linear Standard Preferences

We begin by fixing the price grid such that p_1 is the starting price of the Dutch auction and $p_n = 0$. We determine the sequence z_k with $z_1 > z_2 > \ldots > z_n = 0$ which can then be used to construct the bidding sequence. The buyer has to decide whether to accept p_k now and end the auction or wait for the price p_{k+1} and accept then. We have a monotonic utility function and the buyer can always only decide between accepting and waiting in each stage. Therefore, it is sufficient to compare the payoff in the present period to the expected payoff in the next price step.

The payoff if the buyers accepts now is given by

$$\pi_k^i = v^i - p_k. \tag{4.A.34}$$

The expected payoff of waiting until the next period is given by

$$\begin{aligned} E[\pi_{k+1}^i] =& H_k^i \cdot (v^i - p_{k+1}) & (4.A.35)\\ =& \frac{F(v_k^j) + F(v_{k+1}^j)}{F(v_k^j) + F(v_{k-1}^j)} \cdot (v^i - p_{k+1}). & (4.A.36) \end{aligned}$$

Bidder i prefers to accept now over waiting if and only if

$$\begin{aligned} \pi_k^i \geq& E[\pi_{k+1}^i] & (4.A.37)\\ v^i - p_k \geq& H_k^i \cdot (v^i - p_{k+1}) & (4.A.38)\\ v^i \cdot (1 - H_k^i) \geq& p_k - p_{k+1} \cdot H_k^i, & (4.A.39) \end{aligned}$$

where $p_{k+1} = p_k - \delta$. Hence,

$$\begin{aligned} v^i \cdot (1 - H_k^i) \geq& p_k - (p_k - \delta) \cdot H_k^i & (4.A.40)\\ v^i \cdot (1 - H_k^i) \geq& p_k \cdot (1 - H_k^i) + \delta) \cdot H_k^i & (4.A.41)\\ v^i \geq& p_k + \delta \cdot \frac{H_k^i}{1 - H_k^i}. & (4.A.42) \end{aligned}$$

With $H_k^i / 1 - H_k^i = (F(z_k) + F(z_{k+1}))/(F(z_{k-1}) - F(z_{k+1}))$, we get

$$\begin{aligned} v^i \geq& \underbrace{p_k + \delta \cdot \frac{F(z_k) + F(z_{k+1})}{F(z_{k-1}) - F(z_{k+1})}}_{:=z_k} & (4.A.43)\\ v^i \geq& z_k. & (4.A.44) \end{aligned}$$

Bidder i prefers to wait for the next price over accepting now if and only if his valuation is strictly smaller than z_k. Every bidder with a valuation exceeding z_k accepts price p_k, receives the item, and ends the auction. To get the upper interval boundary, we determine the minimum valuation z_{k-1} above which the bidder accepts the previous, i.e., higher, price p_{k-1}. By the same reasoning as above, we have that the bidder accepts p_{k-1} over waiting for p_k if and only if

$$v^i \geq \underbrace{p_{k-1} + \delta \cdot \frac{F(z_{k-1}) + F(z_k)}{F(z_{k-2}) - F(z_k)}}_{:=z_{k-1}} \tag{4.A.45}$$

$$v^i \geq z_{k-1}. \tag{4.A.46}$$

Conclusively, bidder i accepts price p_k if and only if her valuation lies between z_{k-1} and z_k. The last thing necessary to recursively determine z_k is an initial value. As bidders with a valuation of zero bid zero, the initial value is given by $z_n = 0$.

Power Standard Preferences

As in the case of risk aversion and affinity in the FPSBA, the transformation does not yield any technical difficulties. The profit if the buyer accepts now is given by

$$\pi_k^i = (v^i - p_k)^\beta. \tag{4.A.47}$$

The expected payoff if the buyer accepts in the next round equals

$$E[\pi_{k+1}^i] = H_k^i \cdot (v^i - p_{k+1})^\beta. \tag{4.A.48}$$

The reasoning under standard preferences applies and this yields a z_k of

$$z_k := p_k + \delta \cdot \frac{(H_k^i)^{1/\beta}}{1 - (H_k^i)^{1/\beta}}. \tag{4.A.49}$$

For $\beta > 1$, it holds that

$$\frac{(H_k^i)^{1/\beta}}{1 - (H_k^i)^{1/\beta}} > \frac{H_k^i}{1 - H_k^i}; \tag{4.A.50}$$

for $\beta < 1$, we have

$$\frac{(H_k^i)^{1/\beta}}{1 - (H_k^i)^{1/\beta}} < \frac{H_k^i}{1 - H_k^i}. \tag{4.A.51}$$

In (4.A.49), we can see that if the right-hand side becomes smaller, the left-hand side needs to become smaller as well. With this we have shown that the equilibrium threshold values z_k are decreasing in the participant's risk aversion.

KR Preferences

In the case of reference dependence, the profit if the bidder accepts now is given by

$$\pi_k^i = v^i - p_k. \tag{4.A.52}$$

The expected profit from waiting until the next period is given by

$$E[\pi^i_{k+1}] = H^i_k \cdot (v^i - p_{k+1}) \tag{4.A.53}$$
$$+ H^i_k(1 - H^i_k) \cdot (v^i - p_{k+1}) - \lambda(1 - H^i_k)H^i_k \cdot (v^i - p_{k+1}) \tag{4.A.54}$$
$$= H^i_k \cdot (v^i - p_{k+1}) \tag{4.A.55}$$
$$+ (1 - \lambda)H^i_k(1 - H^i_k) \cdot (v^i - p_{k+1}). \tag{4.A.56}$$

Bidder i prefers to accept now over waiting if and only if

$$v^i - p_k \geq (v^i - p_{k+1}) \underbrace{\left[H^i_k + (1 - \lambda)H^i_k(1 - H^i_k)\right]}_{:=\Lambda^i_k(\lambda)}. \tag{4.A.57}$$

Rearranging yields

$$v^i\big(1 - \Lambda^i_k(\lambda)\big) \geq p_k - p_{k+1}\Lambda^i_k(\lambda) \tag{4.A.58}$$
$$\Leftrightarrow\ v^i \geq \frac{p_k - (p_k - \delta)\Lambda^i_k(\lambda)}{1 - \Lambda^i_k(\lambda)} \tag{4.A.59}$$
$$\Leftrightarrow\ v^i \geq p_k + \delta\frac{\Lambda^i_k(\lambda)}{1 - \Lambda^i_k(\lambda)}. \tag{4.A.60}$$

For $0 \leq \lambda < 1$, it holds that

$$\frac{\Lambda^i_k(\lambda)}{1 - \Lambda^i_k(\lambda)} > \frac{H^i_k}{1 - H^i_k}; \tag{4.A.61}$$

for $\lambda > 1$, we have

$$\frac{\Lambda^i_k(\lambda)}{1 - \Lambda^i_k(\lambda)} < \frac{H^i_k}{1 - H^i_k}. \tag{4.A.62}$$

This is, equilibrium threshold values z_k are decreasing in the participant's loss aversion.

Allais-Type Preferences

Allais-type preferences have an influence in the case of the DA because the current price can be accepted with certainty. This means that in the case of accepting the price p_k, the utility function u^D applies whereas in the case of declining, the utility function u^R is relevant. If we assume that for u^D and u^R, it holds that

$$u^R(x) = \alpha^{\mathrm{AT}} \cdot \frac{x^{\beta^{\mathrm{AT}}}}{\beta^{\mathrm{AT}}}, u^D(x) = \frac{x^{\beta^{\mathrm{AT}}}}{\beta^{\mathrm{AT}}} \tag{4.A.63}$$

where $\alpha^{\mathrm{AT}} \in \mathbb{R}_{\geq 0}$, we can derive the bidding function in the same way as with standard and KR preferences, respectively. The utility if the bidder accepts the current price is given by

$$u^D\big(v^i - p_k\big) = \alpha^{\mathrm{AT}} \cdot \frac{(v^i - p_k)^{\beta^{\mathrm{AT}}}}{\beta^{\mathrm{AT}}}. \tag{4.A.64}$$

The expected utility from waiting until the next period is given by

$$E[u^R(v^i - p_{k+1})|p_k] = \ H_k^i \cdot \frac{(v^i - p_{k+1})^{\beta^{\mathrm{AT}}}}{\beta^{\mathrm{AT}}}. \tag{4.A.65}$$

Bidder i prefers to accept now over waiting if and only if

$$v^i \geq p_k + \delta \cdot \frac{\left(H_k^i\right)^{1/\beta^{\mathrm{AT}}}}{\alpha^{1/\beta^{\mathrm{AT}}} - \left(H_k^i\right)^{1/\beta^{\mathrm{AT}}}}. \tag{4.A.66}$$

For $\alpha^{\mathrm{AT}} = 1$ expression (4.A.66) corresponds to (4.A.49). The higher α^{AT}, the smaller the equilibrium threshold values z_k.

4.B Numeracy Tests

We assess individual numeracy and probabilistic literacy by combining two numeracy tests. This combined test varies in difficulty and increases the variation in scores, i.e., the number of correct answers (Cokely et al. 2012). The German translation used in the experiment is provided in parentheses.

Schwartz et al. (1997) Numeracy Test.

Schwartz et al. (1997) assess numeracy with three open-ended questions. The score is the total number of correct answers. We slightly adapted the wording to fit into the neutral framing of our experiment.[32]

- **SNT 1:** Imagine that we flip a fair coin 1,000 times. What is your best guess about how many times the coin would come up heads in 1,000 flips?
  _____ times out of 1,000 flips.
  Answer: 500 .

  [*GERMAN TRANSLATION:* Stellen Sie sich vor, wir werfen eine faire Münze 1000 mal. Bei wie vielen der 1000 Würfe zeigt die Münze im Durchschnitt Kopf an?]

- **SNT 2:** In lottery A, the chance of winning a \$10 prize is 1%. What is your best guess about how many people would win a\$10 prize if 1,000 people each buy a single ticket to lottery A?
  _____ person(s) out of 1,000.
  Answer: 10.

  [*GERMAN TRANSLATION:* In Lotterie A beträgt die Chance, einen 10-EUR-Preis zu gewinnen, 1%. Wie viele Personen gewinnen im Durchschnitt einen 10-EUR-Preis, wenn 1000 Personen jeweils ein einzelnes Los für Lotterie A kaufen?]

[32] The original wording replaces "lottery A" for "BIG BUCKS LOTTERY" in question 2 and "lottery B" for "ACME PUBLISHING SWEEPSTAKES" in question 3 (Schwartz et al. 1997, p. 967).

- **SNT 3:** In lottery B, the chance of winning a car is 1 in 1,000. What percent of tickets to lottery B win a car?
  _____%.
  Answer: 0.1.

  [*GERMAN TRANSLATION:* In Lotterie B beträgt die Chance, ein Auto zu gewinnen, 1 in 1000. Wie viel Prozent der Lose für Lotterie B gewinnen ein Auto?]

Berlin Numeracy Test.

Cokely et al. (2012) develop the Berlin Numeracy Test to assess statistical numeracy and risk literacy with four items in educated and highly-educated populations. We use the paper-and-pencil format than incorporates the full set of questions.[33]

- **BNT 1:** Imagine we are throwing a five-sided die 50 times. On average, out of these 50 throws how many times would this five-sided die show an odd number (1, 3 or 5)?
  _____ out of 50 throws.
  Answer: 30.

  [*GERMAN TRANSLATION:* Stellen Sie sich vor, wir werfen einen fünfseitigen Würfel 50 mal. Bei wie vielen dieser 50 Würfe zeigt dieser fünfseitige Würfel im Durchschnitt eine ungerade Zahl (1, 3 oder 5)?]

- **BNT 2:** Out of 1,000 people in a small town 500 are members of a choir. Out of these 500 members in the choir 100 are men. Out of the 500 inhabitants that are not in the choir 300 are men. What is the probability that a randomly drawn man is a member of the choir? (please indicate the probability in percent).
  _____%.
  Answer: 25.

  [*GERMAN TRANSLATION:* Von 1000 Einwohnern einer Kleinstadt sind 500 Mitglied im Gesangsverein . Von diesen 500 Mitgliedern im Gesangsverein sind 100 Männer. Von den 500 Einwohnern, die nicht im Gesangsverein sind, sind 300 Männer. Wie groß ist die Wahrscheinlichkeit, dass ein zufällig ausgewählter Mann ein Mitglied des Gesangsvereins ist?]

- **BNT 3:** Imagine we are throwing a loaded die (6 sides). The probability that the die shows a 6 is twice as high as the probability of each of the other numbers. On average, out of these 70 throws, how many times would the die show the number 6?

[33] Cokely et al. (2012) also develop an adaptive version and a median-split version that only require a subset of questions and are intended to be used if there is little time to assess numeracy.

_____ out of 70 throws.

Answer: 20.

[*GERMAN TRANSLATION:* Stellen Sie sich vor, wir werfen einen gezinkten Würfel (6 Seiten). Die Wahrscheinlichkeit, dass der Würfel eine 6 zeigt, ist doppelt so hoch wie die Wahrscheinlichkeit jeder der anderen Zahlen. Bei wie vielen von 70 Würfen zeigt dieser Würfel im Durchschnitt eine 6?]

- **BNT 4:** In a forest 20% of mushrooms are red, 50% brown and 30% white. A red mushroom is poisonous with a probability of 20%. A mushroom that is not red is poisonous with a probability of 5%. What is the probability that a poisonous mushroom in the forest is red?

  _____%.

  Answer: 50.

  [*GERMAN TRANSLATION:* In einem Wald sind 20% der Pilze rot, 50% braun und 30% weiß. Ein roter Pilz ist mit einer Wahrscheinlichkeit von 20% giftig. Ein Pilz, der nicht rot ist, ist mit einer Wahrscheinlichkeit von 5% giftig. Wie groß ist die Wahrscheinlichkeit, dass ein giftiger Pilz aus diesem Wald rot ist?]

4.C Instructions

This section provides the instruction in German (original) and English (translated) separated by parts 1 and 2. Each part consists of part A and part B. Part B was always distributed after part A had been conducted. Experiment 1 was identical for each participant. Experiment 2 was counterbalanced, i.e., half of the participants received the first-price sealed-bid auction in part A followed by the Dutch auction in part B. The other half faced the reversed order. We present the instructions for the full-DSS treatment where subjects had full information. The instructions for the other treatments are the same and only exclude parts of the decision support which is reported in parentheses within the instructions.

INSTRUKTIONEN – Experiment 1

Allgemeine Instruktionen

Herzlich willkommen und vielen Dank für Ihre Teilnahme an diesem Experiment! Bitte kommunizieren Sie nicht mit den anderen Teilnehmern.

Wir bitten Sie, die Instruktionen aufmerksam zu lesen. Wenn Sie nach dem Lesen oder während des Experiments noch Fragen haben, heben Sie bitte Ihre Hand. Einer der Experimentleiter wird dann zu Ihnen kommen und Ihre Frage beantworten.

Das gesamte Doppelexperiment (Experiment 1 und Experiment 2) besteht aus vier unabhängigen Teilen, von denen zwei Teile (Teil 1.A und Teil 1.B) heute und die anderen zwei Teile (Teil 2.A und Teil 2.B) in einer Woche durchgeführt werden. Ihre Entscheidungen innerhalb eines Teils haben **keinen Einfluss** auf die anderen Teile.

Sie erhalten zunächst die Instruktionen für Teil 1.A. Nach Abschluss von Teil 1.A erhalten Sie die Instruktionen für Teil 1.B. Danach bitten wir Sie, einen Fragebogen auszufüllen. Ihre Auszahlung und Ihre Entscheidungen werden vertraulich behandelt.

Für Ihr Erscheinen zu beiden Experimenten erhalten Sie insgesamt 2,50 €. Zusätzlich erhalten Sie eine Anfangsausstattung von 20,00 €. Die Auszahlung findet im Anschluss an Teil 2.B statt. Sie erhalten Ihre Auszahlung nur, wenn Sie an beiden Experimenten teilgenommen haben.

<u>Hinweis:</u>
Sie können in diesem Doppelexperiment je nach Ihren Entscheidungen Gewinne und Verluste machen. Ihre Auszahlung wird nicht durch die Entscheidungen anderer Teilnehmer beeinflusst. Gewinne und Verluste in den einzelnen Teilen werden miteinander verrechnet.
Falls Sie über das gesamte Doppelexperiment Verluste ansammeln, sind Sie verpflichtet, diese nach Teil 2.B in bar zu begleichen.

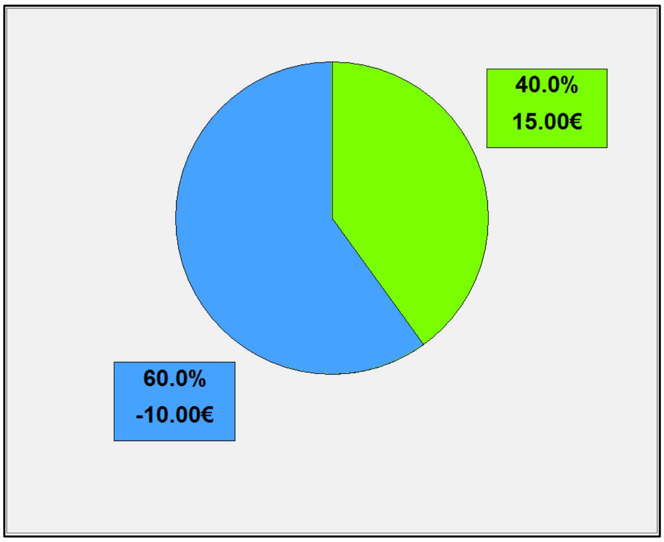
Teil 1.A

Lotterien

In Teil 1.A wählen Sie in jeder Runde zwischen zwei Lotterien. Eine Lotterie besteht aus Wahrscheinlichkeiten und dazugehörigen Geldbeträgen. Geldbeträge können positiv (Gewinne) oder negativ (Verluste) sein.

Dargestellt werden diese Lotterien als Tortendiagramme. Je größer die Fläche, desto größer die Wahrscheinlichkeit, dass der zugehörige Geldbetrag ausgewählt wird.

Beispiel: Eine Lotterie zahlt mit 40% Wahrscheinlichkeit 15 EUR aus und mit 60% Wahrscheinlichkeit -10 EUR. Somit ergibt sich also in 40% der Fälle ein Gewinn von 15 EUR und in 60% der Fälle ein Verlust von 10 EUR. Auf Ihrem Bildschirm sähe diese Lotterie wie folgt aus:



Entscheidung zwischen Lotterienpaaren

- Sie sehen auf Ihrem Bildschirm immer zwei Lotterien nebeneinander und entscheiden sich für eine von beiden. Dies tun Sie, indem Sie auf den Knopf unter der gewünschten Lotterie klicken.
- Insgesamt werden Ihnen 106 Lotterienpaare dargestellt. Die Knöpfe unter den Lotterien erscheinen erst nach 10 Sekunden.

Beachten Sie: Auf Ihrem Bildschirm werden Zahlen gerundet angezeigt. Dies kann dazu führen, dass sich die dargestellten Wahrscheinlichkeiten nicht immer zu 100% addieren. Der Computer rechnet jedoch mit den exakten Werten.

Auszahlung

Nachdem Sie Ihre Entscheidungen für alle Lotterienpaare getroffen haben, bestimmen Sie, ob Teil 1.A bei Ihrer Auszahlung berücksichtigt werden soll oder nicht.

a) Teil 1.A für Auszahlung verwenden

In diesem Fall wird Ihre Auszahlung wie folgt bestimmt:

1. Eine der von Ihnen gewählten Lotterien wird zufällig durch den Computer ausgewählt und durchgeführt. Jede Lotterie hat dabei die gleiche Wahrscheinlichkeit ausgewählt zu werden.

2. Das Ergebnis der Lotterie (Gewinn oder Verlust) wird mit Ihren Gewinnen und Verlusten aus den anderen Teilen (Experiment 1 und Experiment 2) verrechnet.

Das Ergebnis der Lotterie und Ihre Auszahlung erfahren Sie erst, nachdem Sie Teil 2.B abgeschlossen haben.

b) Teil 1.A nicht für Auszahlung verwenden

In diesem Fall wird Ihre Auszahlung wie folgt bestimmt:

1. Keine der von Ihnen gewählten Lotterien wird bei der Auszahlung berücksichtigt.

2. Es werden Ihnen 20,00 € von Ihrer Anfangsausstattung abgezogen.

Teil 1.B

Fragen

In Teil 1.B des Experiments werden Ihnen auf dem Bildschirm nacheinander sieben Fragen angezeigt. Sie haben zur Beantwortung einer Frage jeweils zwei Minuten Zeit. Nach Ablauf dieser Zeit, erscheint automatisch die nächste Frage.

Eine Frage gilt nur dann als richtig beantwortet, wenn Sie die korrekte Lösung in das entsprechende Feld eintippen und auf den Knopf „Weiter" drücken, bevor die Zeit abgelaufen ist.

Auszahlung

Für jede richtige Antwort erhalten Sie 0.50 €.

Beachten Sie: Ihr Ergebnis aus Teil 1.B erfahren Sie ebenfalls erst, nachdem Sie Experimentteil 2.B abgeschlossen haben.

Übersicht

Dieser Teil des Experiments besteht aus 18 Runden, die jeweils die gleiche Abfolge an Entscheidungen haben. Am Ende wird eine der 18 Runden zufällig durch den Computer ausgewählt und ausgezahlt. Alle Runden haben dabei die gleiche Wahrscheinlichkeit ausgewählt zu werden.

Erstpreisauktion

Sie nehmen an einer Erstpreisauktion teil, in der Sie ein Produkt erwerben können. Zu Beginn jeder Runde erfahren Sie, welchen Wert das Produkt für Sie hat. Dieser Wert wird aus der Menge

{ 6 €, 10 €, 14 €, 18 €, 22 €, 26 €, 30 €, 34 €, 38 € }

gezogen. Jeder Wert kommt genau zweimal vor. Die Reihenfolge ist jedoch zufällig bestimmt.

Sie befinden sich in einer Gruppe mit einem anderen Bieter. Der andere Bieter ist ein Bietroboter.

In der Auktion kann ein ganzzahliges Gebot zwischen 0 € und 21 € abgegeben werden. Der andere Bieter wählt sein Gebot zufällig zwischen 0 € und 21 €. Jedes Gebot ist dabei gleich wahrscheinlich.

Der Bieter, der das höchste Gebot abgegeben hat, gewinnt die Auktion und erhält das Produkt. Der Preis des Produkts entspricht diesem höchsten Gebot. Falls Sie und der andere Bieter das gleiche Gebot abgeben, erhalten Sie das Produkt mit 50% Wahrscheinlichkeit.

Falls Sie die Auktion gewinnen, ist Ihr Gewinn gegeben durch:

Gewinn = Wert – Gebot.

Falls Sie die Auktion nicht gewinnen, beträgt Ihr Gewinn 0.

Entscheidungshilfe

Bevor Sie Ihr echtes Gebot eingeben, können Sie verschiedene Gebote testen, wofür Ihnen ein Testbereich zur Verfügung steht.

Im Testbereich sehen Sie:

[Treatments: No DSS, Medium DSS, Full DSS]

- **Gewinn, falls Gebot erfolgreich**
  Der Gewinn, falls das aktuelle Testgebot erfolgreich wäre. Dieser wird wie folgt berechnet:
  Gewinn = Wert – Gebot.

[Treatments: Medium DSS, Full DSS]

- **Gewinnwahrscheinlichkeit**
  Die Wahrscheinlichkeit, dass Sie mit einem Gebot in Höhe des Testgebots die Auktion gewinnen.

[Treatments: Full DSS]

- **Erwarteter Gewinn**
  Durchschnittlicher Gewinn, den Sie mit dem Gebot erwarten können. Dieser wird wie folgt berechnet:
  Erwarteter Gewinn = (Gewinnwahrscheinlichkeit) x (Gewinn, falls Gebot erfolgreich).

Gebotsabgabe

- Um Ihr finales Gebot abzugeben, tippen Sie eine Zahl aus der erlaubten Menge der Gebote in das vorgesehene Feld ein. Anschließend klicken Sie auf „Gebot abgeben".
- Sie haben in jeder Runde 60 Sekunden Zeit, Ihr finales Gebot abzugeben. Sollten Sie kein Gebot in den 60 Sekunden abgeben haben, nehmen Sie in dieser Runde nicht an der Auktion teil.

Hinweis

Eine Runde dauert immer 60 Sekunden, unabhängig davon zu welchem Zeitpunkt Sie Ihr Gebot abgegeben haben. Nachdem Sie und der andere Bieter ein finales Gebot abgegeben haben, ist die Auktion zwar beendet, aber die Runde endet erst, wenn die 60 Sekunden abgelaufen sind.

Ergebnis

Übersicht

Dieser Teil des Experiments besteht aus 18 Runden, die jeweils die gleiche Abfolge an Entscheidungen haben. Am Ende wird eine der 18 Runden zufällig durch den Computer ausgewählt und ausgezahlt. Alle Runden haben dabei die gleiche Wahrscheinlichkeit ausgewählt zu werden.

Tickerauktion

Sie nehmen an einer Tickerauktion teil, in der Sie ein Produkt erwerben können. Zu Beginn jeder Runde erfahren Sie, welchen Wert das Produkt für Sie hat. Dieser Wert wird aus der Menge

{ 6 €, 10 €, 14 €, 18 €, 22 €, 26 €, 30 €, 34 €, 38 € }

gezogen. Jeder Wert kommt genau zweimal vor. Die Reihenfolge ist jedoch zufällig bestimmt.

Sie befinden sich in einer Gruppe mit einem anderen Bieter. Der andere Bieter ist ein Bietroboter.

In der Auktion startet der Preis bei 21 € und wird alle 10 Sekunden um 1 € gesenkt. Bei jedem neuen Preis wird zufällig einer der Bieter zuerst gefragt, ob er diesen Preis annehmen möchte. Nimmt der gefragte Bieter den Preis an, so endet damit die Auktion. Lehnt der gefragte Bieter ab, so wird der gleiche Preis dem verbleibenden Bieter angeboten. Beide Bieter haben die gleiche Wahrscheinlichkeit zuerst gefragt zu werden.

Der andere Bieter wählt zufällig einen Preis zwischen 0 € und 21 € aus, zu dem er annehmen würde. Jeder mögliche Preis hat dabei die gleiche Wahrscheinlichkeit ausgewählt zu werden.

Sie gewinnen die Auktion und erhalten das Produkt, falls Sie vor dem anderen Bieter einen Preis annehmen.

Falls Sie die Auktion gewinnen, ist Ihr Gewinn gegeben durch:

Gewinn = Wert – Preis.

Falls Sie die Auktion nicht gewinnen, beträgt Ihr Gewinn 0.

Entscheidungshilfe

Sie sehen auf dem Bildschirm den aktuellen Preis, den nächsten Preis sowie die Zeit bis zum nächsten Preis.

Zusätzlich sehen Sie:

[Treatments: No DSS, Medium DSS, Full DSS]

- **Gewinn bei gegebenem Preis**
  Der Gewinn, falls Sie den Preis annehmen würden. Dieser wird wie folgt berechnet:
  Gewinn bei gegebenem Preis = Wert – Preis.

[Treatments: Medium DSS, Full DSS]

- **Wahrscheinlichkeit, Preis angeboten zu bekommen**
  Die Wahrscheinlichkeit, dass Sie den jeweiligen Preis annehmen können.

[Treatments: Full DSS]

- **Erwarteter Gewinn**
  Durchschnittlicher Gewinn, den Sie erwarten können, wenn Sie sich jetzt entscheiden den jeweiligen Preis anzunehmen. Dieser wird wie folgt berechnet:
  Erwarteter Gewinn = (Wahrscheinlichkeit, Preis angeboten zu bekommen) x (Gewinn, bei gegebenem Preis).

Hinweis

Eine Runde dauert immer 220 Sekunden, unabhängig davon welchen Preis Sie annehmen. Nachdem Sie oder der andere Bieter einen Preis angenommen haben, ist die Auktion zwar beendet, aber die Runde endet erst, wenn die 220 Sekunden abgelaufen sind.

Ergebnis

Nach jeder Runde sehen Sie das Ergebnis der Runde. Hier erfahren Sie den Preis, ob Sie das Produkt erhalten haben und wie hoch Ihr Gewinn ist.

Instructions – Experiment 1

General Instructions

Welcome and thank you for participating in this experiment. Please do not communicate with other participants.

We kindly ask you to read the instructions carefully. If you have any questions after reading the instructions or during the experiment, please raise your hand. One of the instructors will then come to your place and answer your question.

The entire double experiment (experiment 1 and experiment 2) consists of four independent parts of which two (part 1.A and part 1.B) will be conducted today while the other two (part 2.A and part 2.B) will be conducted in one week from now. Your decisions in one part **do not influence** the other parts.

You will first receive the instructions for part 1.A. After part 1.A is completed, you will receive the instructions for part 1.B. Then, we ask you to fill out a questionnaire. Your payment and your decisions will be treated as confidential.

You will receive a show-up fee of 2.50 € for attending both experiments. In addition, you will receive an endowment of 20.00 €. The payment will take place at the end of part 2.B. You only receive your payment if you participate in both experiments.

> Note:
>
> In this double experiment, you can make gains and losses depending on your decisions. Your payment will not be influenced by the decisions of other participants. Gains and losses of the individual parts will be offset against each other.
>
> If you accumulate losses over the entire double experiment, you are obligated to pay these in cash at the end of part 2.B.

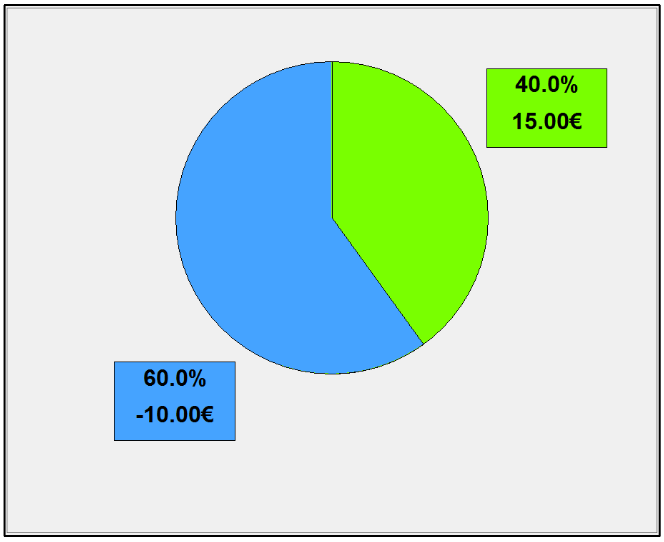
Part 1.A

Lotteries

In Part 1.A, you will choose between two lotteries in each round. A lottery consists of probabilities and according monetary values. Monetary values can be positive (gains) or negative (losses).

These lotteries will be shown as pie charts. The larger is the area, the larger the probability to receive the respective monetary value.

Example: A lottery pays 15 EUR with a 40% probability and -10 EUR with a 60% probability. Hence, in 40% of the time, there will be a gain of 15 EUR and in 60% of the time, there will be a loss of 10 EUR. On your screen, this lottery would look as follows:



Deciding between pairs of lotteries

- On your screen, you will always see two lotteries next to each other and choose one of these lotteries. You do so by clicking on the button below the lottery chosen.
- In total, there will be 106 lottery pairs. The buttons below the lotteries will appear after 10 seconds.

Note: On your screen, numbers will be rounded. This may cause probabilities to not always add up to 100%. The computer, however, does the calculations using the exact numbers.

Payoff

After you have made your decision for all lottery pairs, you decide whether part 1.A will be considered for your payoff or not.

a) Consider part 1.A for payoff

In this case, your payoff will be determined as follows:

1. One of the lotteries you chose will be randomly selected by the computer and played out. Every lottery has the same probability to be selected.

2. The result of the lottery (gain or loss) will be offset against the gains and losses from the other parts (experiment 1 and experiment 2).

You will only learn the result of the lottery and your payoff after you completed part 2.B.

b) Do not consider part 1.A for payoff

In this case, your payoff will be determined as follows:

1. None of your chosen lotteries will be considered for payoff.

2. 20.00 € will be deducted from your endowment.

Part 1.B

Questions

In Part 1.B of the experiment, you will subsequently see seven questions on your computer screen. You have two minutes to answer one question. After this time, the next question appears automatically.

A question is answered correctly if you type in the correct answer into the respective field and click on the button “next” before the time has elapsed.

Payoff

For each correct answer, you receive 0.50 €.

Note: You will learn your result from part 1.B only after you have completed part 2.B.

Overview

This part of the experiment consists of 18 rounds which have the same course of decisions. At the end, one of the 18 rounds will be randomly selected by the computer and paid out. All rounds have the same probability to be selected.

First-Price Auction

You will participate in a first-price auction in which you can acquire a product. At the beginning of each round, you will learn which value this product has for you. The value will be drawn from the set

{6 €, 10 €, 14 €, 18 €, 22 €, 26 €, 30 €, 34 €, 38 €}.

Each value occurs exactly twice. The order, however, is random.

You are in a group with one other bidder. This other bidder is a bidding robot.

In the auction, you can enter an integer bid between 0 € and 21 €. The other bidder will choose his bid randomly between 0 € and 21 €. Every bid is equally likely.

The bidder with the highest bid wins the auction and receives the product. The price of the product is given by this highest bid. If you and the other bidder submit the same bid, you have a 50% chance to receive the product.

If you win the auction, your profit is given by:

Profit = Value – Bid.

If you do not win the auction, your profit is 0.

Decision Support

Before you enter your actual bid, you can test different bids for which a testing area is provided for you.

In the testing area, you will see:

[Treatments: No DSS, Medium DSS, Full DSS]

- **Profit if bid was successful**
  The profit if the actual profit was successful. It is calculated as follows:
  Profit = Value – Bid.

[Treatments: Medium DSS, Full DSS]

- **Winning Probability**
  The probability that you win the auction with a bid equal to the test bid.

[Treatments: Full DSS]

- **Expected Profit**
  Average profit that you can expect with the bid. It is calculated as follows:
  Expected Profit = (Winning Probability) x (Profit if bid is successful).

Bid Submission

- To submit your final bid, type in a number out of the feasible set of bids into the respective field. Then, click on "submit bid".
- In each round, you have 60 seconds to submit your final bid. If you do not submit a bid within these 60 seconds, you will not participate in the auction in this round.

Note

One round always lasts for 60 seconds, independently of when you submit your bid. After you and the other bidder submitted a final bid, the auction end but the round will only end after the 60 seconds have elapsed.

Result

After each round, you will see the result of that round. Here you learn the price, whether or not you received the product, and how large your profit is.

Overview

This part of the experiment consists of 18 rounds which have the same course of decisions. At the end, one of the 18 rounds will be randomly selected by the computer and paid out. All rounds have the same probability to be selected.

Ticker Auction

You will participate in a ticker auction in which you can acquire a product. At the beginning of each round, you will learn which value this product has for you. The value will be drawn from the set

{6 €, 10 €, 14 €, 18 €, 22 €, 26 €, 30 €, 34 €, 38 €}.

Each value occurs exactly twice. The order, however, is random.

You are in a group with one other bidder. This other bidder is a bidding robot.

In the auction, the price starts at 21 € and will decrease by 1 € every 10 seconds. At every new price, one of the bidders is randomly asked whether or not he wants to accept the price. If the bidder accepts the price, the auction ends. If the bidder rejects the price, the same price is offered to the remaining bidder. Both bidders have the same probability to be asked first.

The other bidder will randomly choose a price a price between 0 € and 21 € which he would accept. Each feasible price has the same probability to be chosen.

You will win the auction and receive the product if you accept a price before the other bidder does.

If you win the auction, your profit is given by:

Profit = Value – Bid.

If you do not win the auction, your profit is 0.

Decision Support

On your screen, you see the current price, the next price, and the time until the next price is shown.

In addition, you will see:

[Treatments: No DSS, Medium DSS, Full DSS]

- **Profit at given price**
  The profit if you accepted the current price. It is calculated as follows:
  Profit at given price = Value – price.

[Treatments: Medium DSS, Full DSS]

- **Probability to be offered the given price**
  The probability that you can accept the respective price.

[Treatments: Full DSS]

- **Expected Profit**
  Average profit that you can expect if you decide now to accept the respective price. It is calculated as follows:
  Expected Profit = (Probability to be offered this price) x (Profit at given price).

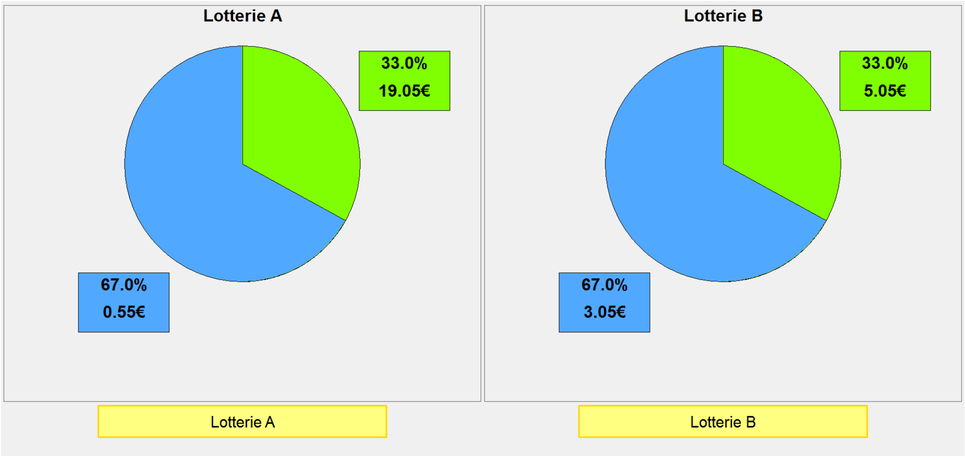
Note

One round always lasts 220 seconds, independently of which price you accept. After you or the other bidder accepted a price, the auction ends but the round will only end after the 220 seconds have elapsed.

Result

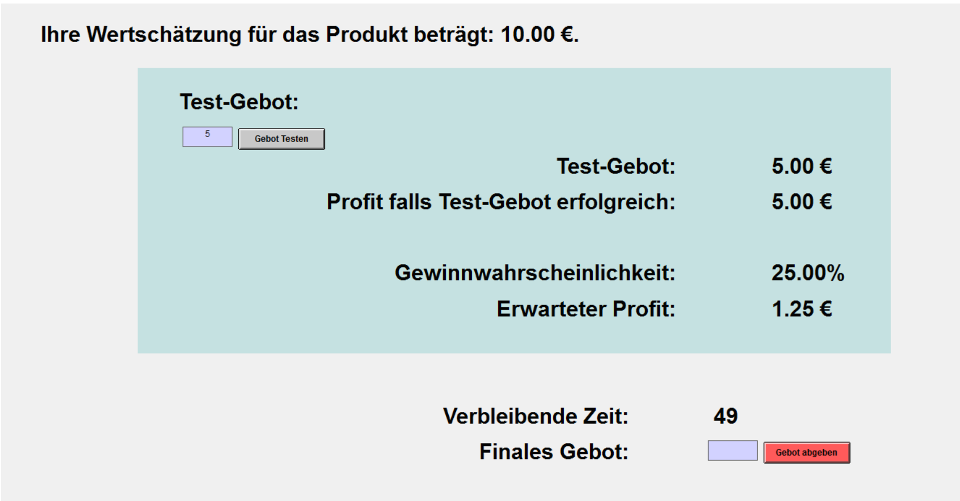
After each round, you will see the result of that round. Here you learn the price, whether or not you received the product, and how large your profit is.

4.D Screens in the Lab Experiment



Notes: Depicted is the computer interface used in Experiment 1 for the pairwise comparison of lotteries. A lottery can be chosen by pressing either the button *Lotterie A* (*Lottery A*) or the button *Lotterie B* (*Lottery B*). Buttons appear after ten seconds.

Figure 4.D.5: Pairwise Comparison of Lotteries.



Notes: Depicted is the computer interface used in the first-price sealed-bid auction. The individual valuation is depicted at the very top. Participants have a test button *Test-Gebot* (*Test bid*) that allows to enter a bid. Depending on the decision support, the following information is calculated from the test bid: *Profit falls Test-Gebot erfolgreich* (*Profit if bid was successful*) (No, Medium, and Full DSS), *Gewinnwahrscheinlichkeit* (*Winning probability*) (Medium and Full DSS), and *Erwarteter Profit* (*Expected profit*) (Full DSS). A timer displays the remaining time to submit a real bid that can be entered in the text field in the lower right corner and submitted by pressing the button *Gebot abgeben* (*Submit bid*).

Figure 4.D.6: Computer Interface: FPSBA.

Notes: Depicted is the computer interface used in the Dutch auction. The individual valuation is depicted at the very top. The screen shows the current price, the time until the next price, and the next price. Depending on the decision support, the following information is calculated automatically: *Gewinn bei gegebenem Preis* (*Profit at given price*) (No, Medium, and Full DSS), *Wahrscheinlichkeit, Preis angeboten zu bekommen* (*Probability to be offered the given price*) (Medium and Full DSS), and *Erwarteter Gewinn* (*Expected profit*) (Full DSS). The current price can be accepted by pressing the button *Preis annehmen* (*Accept price*).

Figure 4.D.7: Computer Interface: DA.

4.E Empirical Appendix

4.E.1 Summary Statistics: Experiment 1

Age is an integer variable reporting participants' age. *Male* and *German Native* are dummy variables indicating male participants and whether the native language is German. AUC_G is the area under the curve on the gain domain and AUC_L is the area under the curve on the loss domain. λ is the parameter of loss aversion: *KT79* indicates the definition of Kahneman and Tversky (1979) and *KW05* indicates the definition of Köbberling and Wakker (2005). *Allais Type* is a dummy indicating whether participants are consistent with Allais-type preferences based on the common-ratio effect (CRE). *Numeracy* is an integer variable reporting participants' numeracy scores.

Table 4.E.6: Summary Statistics by Treatment.

	No DSS	Medium DSS	Full DSS	Total
Age	25.48	24.15	22.74	24.16
	(4.595)	(3.461)	(2.596)	(3.798)
Male	0.483	0.481	0.407	0.458
	(0.509)	(0.509)	(0.501)	(0.501)
German Native	0.828	0.889	0.926	0.880
	(0.384)	(0.320)	(0.267)	(0.328)
AUC_G	0.463	0.514	0.488	0.488
	(0.138)	(0.119)	(0.127)	(0.129)
AUC_L	0.369	0.419	0.381	0.389
	(0.166)	(0.182)	(0.200)	(0.182)
λ_{KT79}	1.627	1.575	1.710	1.637
	(0.725)	(0.675)	(0.727)	(0.703)
λ_{KW05}	7.885	0.945	7.991	5.662
	(35.49)	(1.382)	(23.31)	(24.77)
Allais-Type	0.621	0.741	0.481	0.614
	(0.677)	(0.526)	(0.700)	(0.641)
Numeracy	4.517	4.222	4.556	4.434
	(1.765)	(1.396)	(1.219)	(1.475)

Notes: Reported is the mean of each variable with standard deviation in parentheses separated by treatment. $N = 83$. Participants in *No DSS* do not receive additional information. In treatment *Medium DSS*, participants receive information about the winning probability (FPSBA) or the probability to receive the next price (DA). In treatment *Full DSS*, participants receive the same information as in *Medium DSS* and, in addition, the expected profit associated with their bid.

4.E.2 Individual Utility

This section reports the parametric fit of utility for each participant. $N = 83$. Dots represent the non-parametric elicitation of utility (*Elicited Utility*) and the line is the parametric fit via non-linear least-squares estimation (*Parametric Fit*). Figure 4.E.8 shows the individual utility functions for those participants who started with the FPSBA. Figure 4.E.9 shows the individual utility functions for those participants who started with the DA.

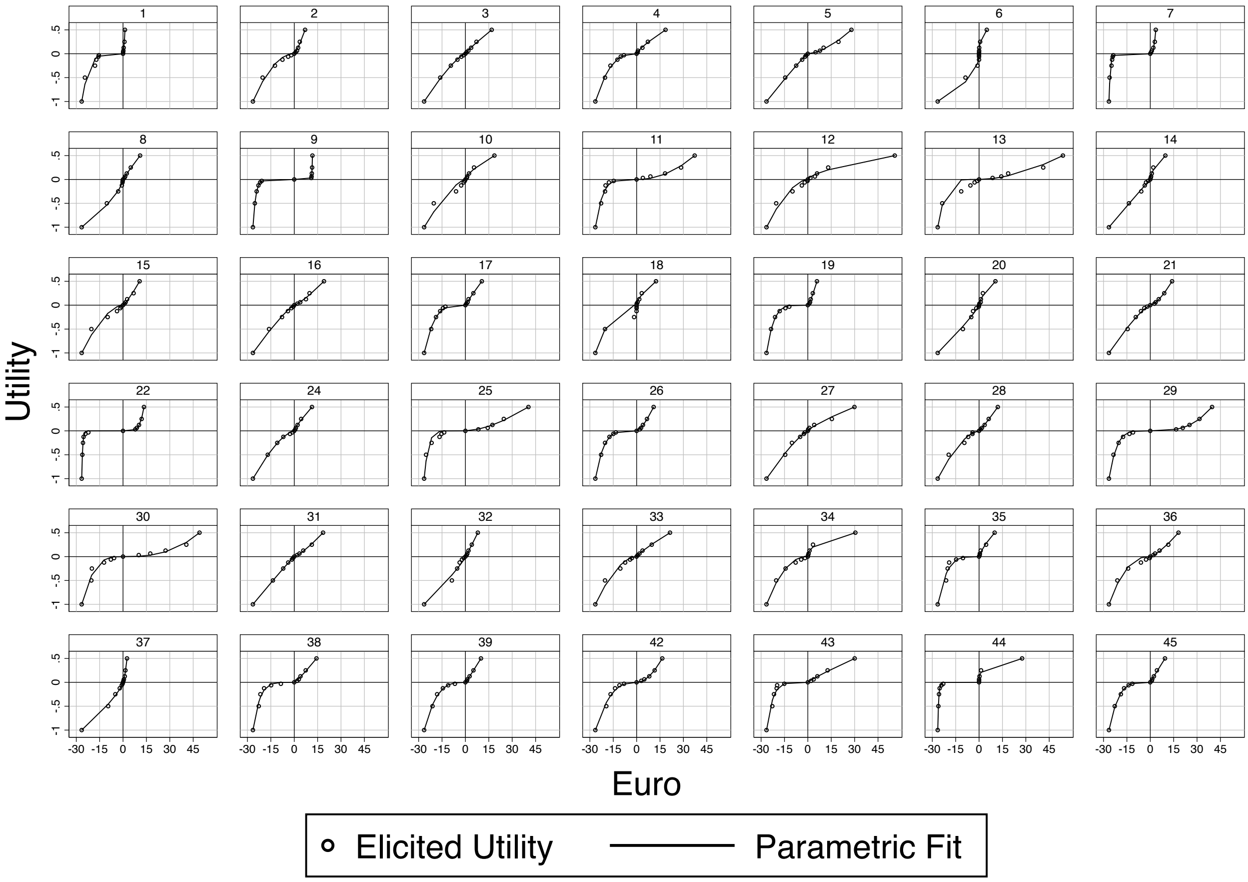


Figure 4.E.8: Individual Utility Functions (FPSBA).

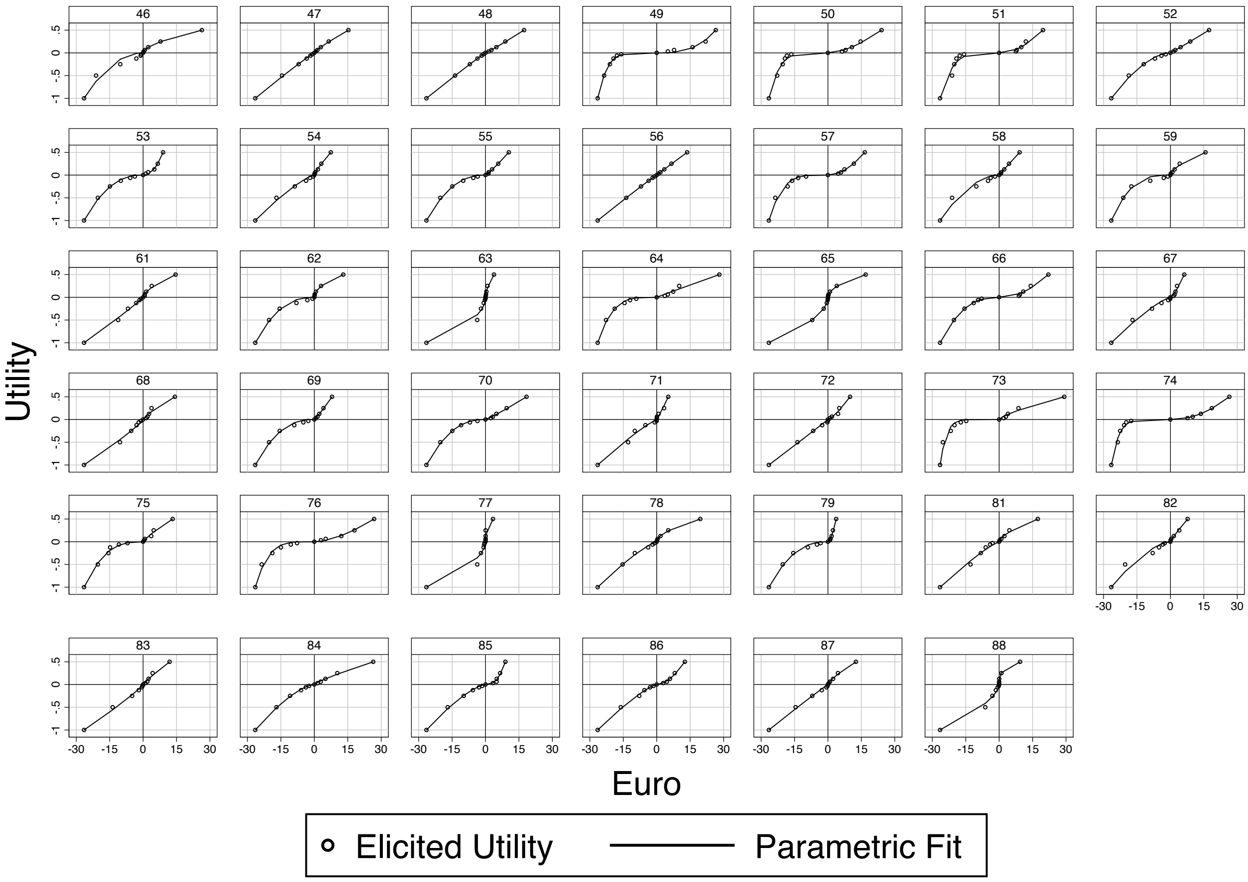


Figure 4.E.9: Individual Utility Functions (DA).

4.E.3 Bidding Behavior

Table 4.E.7: Average Winning Bids for Periods 1 to 18.

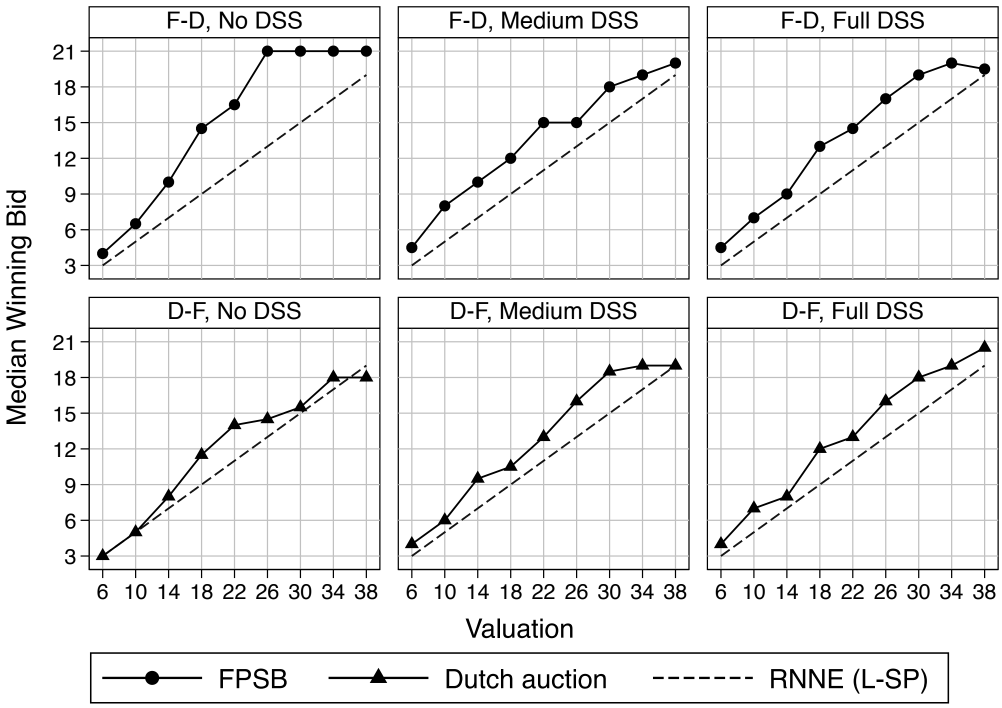
	No DSS			Medium DSS			Full DSS			KW test	
Valuation	FPSBA	DA	p-value	FPSBA	DA	p-value	FPSBA	DA	p-value	p-value FPSBA	p-value DA
6	4.25	7.42	0.8710	4.25	4.00	0.5541	4.25	3.67	0.4450	0.9905	0.9191
10	6.67	6.00	0.3417	7.31	5.93	0.1234	7.38	6.57	0.6310	0.5114	0.6148
14	10.20	8.35	0.0397	10.38	10.50	0.9575	8.67	8.55	0.7575	0.1396	0.1450
18	14.39	11.04	0.0042	12.57	10.91	0.1105	11.25	11.80	0.1498	0.2347	0.7103
22	15.29	12.05	0.0740	14.54	13.83	0.5108	14.67	13.42	0.2008	0.7910	0.6215
26	18.88	14.50	0.0022	15.18	15.71	0.6428	17.09	17.15	0.8142	0.0150	0.0878
30	19.71	16.14	0.0019	17.96	15.73	0.3084	18.00	18.20	0.786	0.0189	0.1328
34	20.20	17.65	0.0062	18.68	17.04	0.1268	18.83	19.17	0.6750	0.0219	0.1285
38	20.20	17.86	0.0190	19.35	18.42	0.4404	18.50	19.77	0.1287	0.0265	0.1372
Average	15.87	13.33	-	14.57	13.86	-	14.93	15.01	-	-	-

Notes: Reported are the average winning bids for periods 1 to 18 and the probability that bids in the different formats are drawn from the same distribution based on the Wilcoxon-Mann-Whitney U-test. The Kruskal-Wallis (KW) test reports whether there is any significant difference across decision support systems for a given auction format.

Table 4.E.8: Average Winning Bids for All Periods.

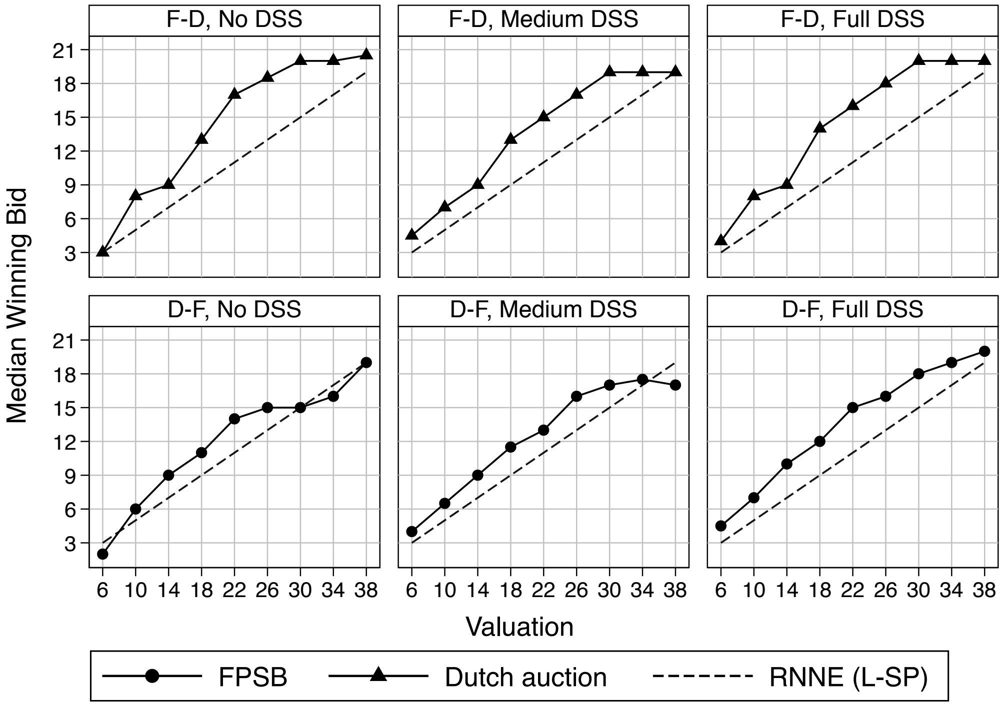
	No DSS			Medium DSS			Full DSS		
Valuation	FPSBA	DA	p-value	FPSBA	DA	p-value	FPSBA	DA	p-value
6	3.65	5.15	0.8913	4.10	6.25	0.5637	4.30	3.83	0.1025
10	6.24	6.38	0.5127	6.82	6.23	0.1111	6.86	7.00	0.4242
14	9.61	8.47	0.3387	10.05	9.66	0.3784	9.09	8.84	0.6934
18	12.53	12.06	0.4408	12.50	11.57	0.1619	12.53	12.10	0.7530
22	14.33	14.02	0.7584	13.93	14.43	0.6532	14.70	14.34	0.4190
26	16.63	16.38	0.3626	15.68	16.27	0.1397	16.62	17.56	0.2342
30	17.65	17.88	0.6796	17.88	17.25	0.5559	17.76	18.48	0.1198
34	18.61	18.79	1.0000	17.98	17.98	0.6732	18.56	19.20	0.0861
38	19.41	18.97	0.2599	18.79	18.79	0.1950	18.67	19.50	0.2918

Notes: Reported are the average winning bids for periods 1 to 36 and the probability that bids in the different formats are drawn from the same distribution based on Wilcoxon signed-rank test.



Notes: Depicted are the medians of the winning bids for each valuation, format, treatment, and order for periods 1 to 18. F-D is the order where participants start with the FPSBA. D-F is the order where participants start with the DA.

Figure 4.E.10: Median Winning Bids for Periods 1 to 18.



Notes: Depicted are the medians of the winning bids for each valuation, format, treatment, and order for periods 19 to 36. F-D is the order where participants start with the FPSBA. D-F is the order where participants start with the DA.

Figure 4.E.11: Median Winning Bids for Periods 19 to 36.

4.E.4 Prediction Accuracy: All GOF Measures

Table 4.E.9: Prediction Accuracy for Mean Data.

Format	**First-Price Sealed-Bid Auction**				**Dutch Auction**			
Panel A. Mean Deviation (MD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	3.45	2.42	2.46	2.81	0.98	1.27	2.39	1.58
	(1.97)	(1.39)	(1.64)	(1.72)	(1.78)	(2.54)	(1.56)	2.02
Power SP	7.39***	6.24***	6.40***	6.71***	4.85***	5.21***	6.36***	5.51***
	(1.97)	(1.37)	(1.57)	(1.71)	(1.81)	(2.48)	(1.56)	(2.02)
Linear KR	1.19***	0.17***	0.24***	0.56***	-1.37***	-1.22***	-0.09***	-0.86***
	(1.95)	(1.45)	(1.73)	(1.75)	(1.78)	(2.59)	(1.56)	(2.02)
Power KR	4.19***	3.10***	3.18***	3.52***	3.45***	3.78***	4.91***	4.08***
	(1.95)	(1.38)	(1.61)	(1.71)	(1.79)	(2.50)	(1.55)	(2.01)
Allais-Type	3.72***	2.61***	2.69***	3.04***	-1.84***	-1.74***	-0.53***	-1.33***
	(1.94)	(1.39)	(1.61)	(1.71)	(1.74)	(2.60)	(1.57)	(2.02)
Panel B. Mean Absolute Deviation (MAD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	3.74	2.77	2.93	3.17	2.80	2.77	2.88	2.82
	(1.43)	(1.08)	(1.45)	(1.37)	(0.98)	(1.19)	(1.09)	(1.06)
Power SP	7.39***	6.24***	6.40***	6.71***	5.05***	5.43***	6.42***	5.66***
	(1.97)	(1.37)	(1.57)	(1.71)	(1.81)	(2.17)	(1.49)	(1.87)
Linear KR	2.14***	2.17*	2.28*	2.19***	2.91	2.69	1.77***	2.43
	(0.93)	(0.59)	(0.90)	(0.80)	(1.03)	(1.52)	(0.74)	(1.20)
Power KR	4.36***	3.23***	3.38***	3.69***	3.96***	4.32***	4.99***	4.44***
	(1.59)	(1.27)	(1.47)	(1.51)	(1.54)	(1.83)	(1.45)	(1.62)
Allais-Type	3.96***	2.85	2.98	3.30***	3.10	2.67	2.08**	2.60
	(1.46)	(1.19)	(1.38)	(1.42)	(1.24)	(1.79)	(0.64)	(1.31)
Panel C. Mean Squared Deviation (MSD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	19.65	11.34	13.23	14.93	13.24	12.68	12.61	12.85
	(12.36)	(6.98)	(10.26)	(10.58)	(8.82)	(9.56)	(8.78)	(8.80)
Power SP	63.51***	44.14***	47.44***	52.20***	34.92***	37.69***	48.47***	40.69***
	(26.34)	(16.94)	(21.49)	(23.25)	(19.81)	(25.02)	(20.30)	(21.93)
Linear KR	8.90***	7.59*	8.53**	8.35***	15.94	14.00	6.87**	12.05
	(6.51)	(3.28)	(5.54)	(5.20)	(11.20)	(13.75)	(5.85)	(11.01)
Power KR	24.60***	14.37***	16.19***	18.65***	23.47***	25.21**	31.20***	26.81***
	(14.30)	(8.60)	(11.85)	(12.48)	(14.60)	(18.66)	(15.88)	(16.28)
Allais-Type	20.50**	11.57	13.04	15.27*	16.42	13.17	7.04*	12.04
	(12.13)	(7.30)	(10.06)	(10.64)	(11.70)	(15.83)	(5.53)	(11.86)

Notes: Reported is the mean of the goodness-of-fit (GOF) measures: mean deviation (MD), mean absolute deviation (MAD), and mean squared deviation (MSD). The deviation is the difference between the individual observed winning bid and the predicted bid based on the mean measurement for Power SP and Linear KR preferences or the mean predicted bid for Power KR and Allais-type. Standard deviation in parentheses. *SP* indicates standard and *KR* indicates Köszegi-Rabin preferences. Asterisks indicate a significant difference between the GOF measure and the benchmark of linear standard preferences (Linear SP) based on the Wilcoxon signed-rank test. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

Table 4.E.10: Prediction Accuracy for Median Data.

Format	First-Price Sealed-Bid Auction				Dutch Auction			
Panel A. Mean Deviation (MD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	3.45	2.42	2.46	2.81	0.98	1.27	2.39	1.58
	(1.97)	(1.39)	(1.64)	(1.72)	(1.78)	(2.54)	(1.56)	(2.02)
Power SP	4.21***	3.12***	3.20***	3.54***	1.68***	2.03***	3.16***	2.32***
	(1.98)	(1.37)	(1.62)	1.72)	(1.80)	(2.54)	(1.56)	(2.03)
Linear KR	2.12***	1.14***	1.14***	1.50***	-0.53***	-0.25***	0.81***	0.04***
	(1.96)	(1.41)	(1.69)	(1.73)	(1.77)	(2.56)	(1.57)	(2.01)
Power KR	4.03***	3.01***	3.06***	3.40***	3.53***	3.83***	4.96***	4.14***
	(1.96)	(1.36)	(1.62)	(1.71)	(1.80)	(2.53)	(1.56)	(2.02)
Allais-Type	1.56***	0.58***	0.62***	0.95***	-3.67***	-3.64***	-2.46***	-3.22***
	(2.00)	(1.42)	(1.67)	(1.75)	(1.71)	(2.61)	(1.58)	(2.01)
Panel B. Mean Absolute Deviation (MAD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	3.74	2.77	2.93	3.17	2.80	2.77	2.88	2.82
	(1.43)	(1.08)	(1.45)	(1.37)	(0.98)	(1.19)	(1.09)	(1.06)
Power SP	4.36***	3.24***	3.45***	3.71***	2.98	3.09*	3.46***	3.19***
	(1.64)	(1.26)	(1.47)	(1.52)	(1.12)	(1.40)	(1.27)	(1.25)
Linear KR	2.73***	2.26**	2.51*	2.51***	2.72	2.57	1.98***	2.41**
	(1.10)	(0.68)	(1.01)	0.95)	(0.82)	(1.28)	(0.72)	(0.98)
Power KR	4.19***	3.21***	3.36***	3.61***	4.11***	4.43***	5.06***	4.55***
	(1.60)	(1.14)	(1.47)	(1.46)	(1.48)	(1.78)	(1.46)	(1.58)
Allais-Type	2.39***	2.04**	2.25*	2.23***	4.11**	3.79	2.71	3.50*
	(1.01)	(0.53)	(0.92)	(0.84)	(1.82)	(2.46)	(1.47)	(1.97)
Panel C. Mean Squared Deviation (MSD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	19.65	11.34	13.23	14.93	13.24	12.68	12.61	12.85
	(12.36)	(6.98)	(10.26)	(10.58)	(8.82)	(9.56)	(8.78)	(8.80)
Power SP	25.97***	15.06***	17.32***	19.71***	14.68	15.22*	17.28***	15.79***
	(15.29)	(8.75)	(12.53)	(13.19)	(9.50)	(11.82)	(10.91)	(10.53)
Linear KR	12.73***	8.03**	9.80**	10.27***	13.94	12.00	7.58***	11.05*
	(8.58)	(4.26)	(7.15)	(7.06)	(9.74)	(10.49)	(5.86)	(8.99)
Power KR	24.25***	14.85***	16.86***	18.88***	25.31***	27.14***	32.63***	28.52***
	(15.08)	(8.55)	(12.39)	(12.78)	(15.01)	(19.33)	(16.24)	(16.70)
Allais-Type	10.26***	6.93**	8.36**	8.57***	28.56***	24.64	13.05	21.74**
	(7.02)	(3.08)	(8.36)	(8.57)	(17.51)	(25.94)	(9.97)	(19.15)

Notes: Reported is the mean of the goodness-of-fit (GOF) measures: mean deviation (MD), mean absolute deviation (MAD), and mean squared deviation (MSD). The deviation is the difference between the individual observed winning bid and the predicted bid based on the median measurement for Power SP and Linear KR preferences or the median predicted bid for Power KR and Allais-type. Standard deviation in parentheses. *SP* indicates standard and *KR* indicates Köszegi-Rabin preferences. Asterisks indicate a significant difference between the GOF measure and the benchmark of linear standard preferences (Linear SP) based on the Wilcoxon signed-rank test. * < 0.10, ** < 0.05, *** < 0.01.

Table 4.E.11: Prediction Accuracy for Individual Data.

Format	**First-Price Sealed-Bid Auction**				**Dutch Auction**			
Panel A. Mean Deviation (MD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	3.45	2.42	2.46	2.81	0.98	1.27	2.39	1.58
	(1.97)	(1.39)	(1.64)	(1.72)	(1.78)	(2.54)	(1.56)	(2.02)
Power SP	4.03	2.96	4.76	3.88*	2.39	1.19	2.48	2.07
	(3.64)	(2.64)	(3.64)	(3.33)	(3.02)	(3.46)	(2.37)	(2.92)
Linear KR	1.21***	0.49***	-0.43***	0.48***	-0.33***	0.07**	0.92***	0.25***
	(2.63)	(1.55)	(3.22)	(2.55)	(2.62)	(3.20)	(2.05)	(2.61)
Power KR	3.28	3.86**	3.42	3.52	4.17**	2.78	4.97**	4.06***
	(4.75)	(2.52)	(3.87)	(3.77)	(3.66)	(5.09)	(3.80)	(4.16)
Allais-Type	3.78	1.06	2.72	2.54	-3.54***	-0.68**	0.31	-1.29***
	(4.85)	(3.76)	(3.10)	(4.10)	(2.05)	(5.06)	(5.62)	(4.72)
Panel B. Mean Absolute Deviation (MAD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	3.74	2.77	2.93	3.17	2.80	2.77	2.88	2.82
	(1.43)	(1.08)	(1.45)	(1.37)	(0.98)	(1.19)	(1.09)	(1.06)
Power SP	4.44	3.71**	5.10*	4.38**	3.72	2.90	3.13	3.26
	(3.17)	(1.43)	(3.23)	(2.71)	(2.05)	(2.57)	(1.60)	(2.05)
Linear KR	2.67*	1.98*	3.12	2.57**	3.12	2.91	2.18*	2.72
	(1.09)	(0.82)	(1.39)	(1.18)	(1.08)	(1.69)	(1.20)	(1.36)
Power KR	4.18	4.39***	4.62*	4.38***	5.11***	4.32	5.47***	5.01***
	(3.53)	(1.50)	(2.53)	(2.62)	(2.70)	(4.12)	(3.11)	(3.27)
Allais-Type	4.80	3.25	3.40	3.86	4.06**	4.34*	4.61	4.34***
	(3.79)	(2.33)	(2.40)	(2.98)	(1.96)	(2.53)	(3.19)	(2.57)
Panel C. Mean Squared Deviation (MSD)								
Preference Specification	No DSS	Medium DSS	Full DSS	All	No DSS	Medium DSS	Full DSS	All
Linear SP	19.65	11.34	13.23	14.93	13.24	12.68	12.61	12.85
	(12.36)	(6.98)	(10.26)	(10.58)	(8.82)	(9.56)	(8.78)	(8.80)
Power SP	33.16	19.18**	39.45*	30.23***	23.69	17.39	16.04	19.05
	(37.43)	(10.89)	(41.27)	(32.66)	(22.87)	(30.28)	(13.66)	(22.37)
Linear KR	12.08	6.92	14.19	10.94*	17.19	15.35	9.39	13.80
	(7.80)	(5.14)	(10.49)	(8.34)	(12.18)	(18.27)	(8.97)	(13.42)
Power KR	33.62	25.45***	32.25*	30.43***	41.43***	38.57	45.86***	42.21***
	(45.59)	(15.43)	(33.19)	(33.50)	(36.40)	(8.92)	(33.38)	(29.60)
Allais-Type	41.19*	18.92	19.78	27.32*	29.14**	28.93*	39.15*	32.74***
	(42.16)	(21.56)	(22.32)	(32.00)	(19.26)	(29.72)	(45.70)	(33.45)

Notes: Reported is the mean of the goodness-of-fit (GOF) measures: mean deviation (MD), mean absolute deviation (MAD), and mean squared deviation (MSD). The deviation is the difference between the individual observed winning bid and the predicted bid based on the individual measurement. Standard deviation in parentheses. *SP* indicates standard and *KR* indicates Köszegi-Rabin preferences. Asterisks indicate a significant difference between the GOF measure and the benchmark of linear standard preferences (Linear SP) based on the Wilcoxon signed-rank test. * < 0.10, ** < 0.05, *** < 0.01.

4.E.5 Parametric Regressions

Table 4.E.12: Regression of MAD Under Linear Standard Preferences.

Auction Format	First-Price Sealed-Bid		Dutch	
Model	(FI)	(FII)	(DI)	(DII)
Medium DSS	-0.877	-1.804	1.655	1.789
	(1.560)	(1.952)	(1.317)	(1.473)
Full DSS	1.949	0.670	-1.378	-1.563
	(2.137)	(2.627)	(1.277)	(1.083)
Numeracy	0.322	0.206	-0.204*	-0.103
	(0.251)	(0.345)	(0.111)	(0.108)
Medium DSS X Numeracy	0.011	0.253	-0.344	-0.382
	(0.322)	(0.397)	(0.253)	(0.282)
Full DSS X Numeracy	-0.649	-0.349	0.305	0.331
	(0.462)	(0.546)	(0.255)	(0.230)
Age		0.017		0.082*
		(0.056)		(0.044)
Male		-0.842*		-0.379
		(0.435)		(0.393)
German Native		-0.383		0.517
		(0.744)		(0.539)
Constant	2.346*	3.017	3.759***	1.106
	(1.292)	(3.084)	(0.579)	(1.438)
Observations	41	41	41	41
R2	0.19	0.28	0.18	0.27

Notes: Reported are parametric OLS regression estimates. Dependent variable is the mean absolute deviation (MAD) for linear standard preferences. Robust standard errors in parentheses. * < 0.10, ** < 0.05, *** < 0.01.

Table 4.E.13: Regression of MAD Under Power Standard Preferences.

Auction Format	First-Price Sealed-Bid		Dutch	
Model	(FI)	(FII)	(DI)	(DII)
Medium DSS	-1.306	-2.879	2.224	-0.420
	(3.250)	(2.967)	(2.966)	(2.459)
Full DSS	-2.292	-5.750	-1.808	-2.531
	(3.764)	(3.969)	(2.885)	(2.519)
Numeracy	0.415	0.134	-0.441	-0.340
	(0.677)	(0.576)	(0.306)	(0.328)
Medium DSS X Numeracy	0.191	0.523	-0.617	-0.179
	(0.724)	(0.706)	(0.496)	(0.382)
Full DSS X Numeracy	0.725	1.416	0.264	0.289
	(0.911)	(0.955)	(0.553)	(0.486)
Age		-0.096		0.004
		(0.152)		(0.089)
Male		-0.879		-1.555**
		(0.871)		(0.653)
German Native		-1.339		1.979**
		(1.141)		(0.887)
Constant	2.641	7.830	5.796***	4.670*
	(3.085)	(5.306)	(1.670)	(2.585)
Observations	41	41	41	41
R2	0.17	0.22	0.22	0.34

Notes: Reported are parametric OLS regression estimates. Dependent variable is the mean absolute deviation (MAD) for power standard preferences. Robust standard errors in parentheses. * < 0.10, ** < 0.05, *** < 0.01.

Table 4.E.14: Regression of MAD Under Linear KR Preferences.

Auction Format	First-Price Sealed-Bid		Dutch	
Model	(FI)	(FII)	(DI)	(DII)
Medium DSS	-1.639	-1.619	1.955	0.096
	(1.185)	(1.388)	(1.922)	(1.546)
Full DSS	-0.925	-1.339	-3.673**	-3.865***
	(1.544)	(1.743)	(1.539)	(1.387)
Numeracy	-0.173	-0.154	-0.172	-0.253
	(0.228)	(0.233)	(0.155)	(0.171)
Medium DSS X Numeracy	0.223	0.208	-0.446	-0.084
	(0.294)	(0.351)	(0.329)	(0.285)
Full DSS X Numeracy	0.321	0.441	0.566*	0.574*
	(0.342)	(0.361)	(0.317)	(0.300)
Age		0.027		-0.098*
		(0.071)		(0.053)
Male		-0.009		-0.566
		(0.374)		(0.429)
German Native		-1.009**		-0.140
		(0.413)		(0.930)
Constant	3.424***	3.506	3.939***	7.214***
	(0.983)	(2.468)	(0.945)	(2.221)
Observations	41	41	41	41
R2	0.18	0.28	0.24	0.36

Notes: Reported are parametric OLS regression estimates. Dependent variable is the mean absolute deviation (MAD) for Linear KR preferences. Robust standard errors in parentheses. * < 0.10, ** < 0.05, *** < 0.01.

Table 4.E.15: Regression of MAD Under Power KR Preferences.

Auction Format	First-Price Sealed-Bid		Dutch	
Model	(FI)	(FII)	(DI)	(DII)
Medium DSS	1.937	1.293	3.904	-0.053
	(2.684)	(2.627)	(4.476)	(4.140)
Full DSS	2.567	1.951	-0.895	-2.158
	(3.789)	(3.592)	(4.617)	(3.873)
Numeracy	1.226*	1.170**	-0.919***	-0.683*
	(0.613)	(0.570)	(0.310)	(0.358)
Medium DSS X Numeracy	-0.313	-0.125	-0.949	-0.262
	(0.650)	(0.661)	(0.771)	(0.708)
Full DSS X Numeracy	-0.461	-0.282	0.286	0.387
	(0.951)	(0.902)	(0.912)	(0.816)
Age		0.049		0.096
		(0.183)		(0.140)
Male		-0.736		-2.880***
		(0.705)		(0.964)
German Native		-0.240		2.564*
		(0.706)		(1.400)
Constant	-1.136	-1.729	9.447***	5.804
	(2.573)	(5.124)	(1.744)	(4.001)
Observations	41	41	41	41
R2	0.22	0.24	0.29	0.43

Notes: Reported are parametric OLS regression estimates. Dependent variable is the mean absolute deviation (MAD) for Power KR preferences. Robust standard errors in parentheses. * < 0.10, ** < 0.05, *** < 0.01.

Table 4.E.16: Regression of MAD Under Allais-Type Preferences.

Auction Format	First-Price Sealed-Bid		Dutch	
Model	(FI)	(FII)	(DI)	(DII)
Medium DSS	1.633	2.481	0.489	-1.749
	(4.029)	(4.326)	(3.976)	(4.674)
Full DSS	-4.266	-4.315	-2.706	-2.943
	(3.737)	(4.669)	(3.455)	(3.079)
Numeracy	0.222	0.344	0.036	-0.061
	(0.775)	(0.766)	(0.236)	(0.194)
Medium DSS X Numeracy	-0.787	-1.046	-0.045	0.508
	(0.879)	(0.925)	(0.754)	(0.890)
Full DSS X Numeracy	0.696	0.727	0.667	0.793
	(0.823)	(1.001)	(0.772)	(0.726)
Age		0.014		-0.053
		(0.204)		(0.123)
Male		0.841		-1.179
		(1.018)		(0.949)
German Native		-1.939*		-2.605***
		(1.113)		(0.699)
Constant	3.839	4.357	3.894***	8.464**
	(3.566)	(7.288)	(1.292)	(3.607)
Observations	41	41	41	41
R2	0.13	0.20	0.03	0.17

Notes: Reported are parametric OLS regression estimates. Dependent variable is the mean absolute deviation (MAD) for Allais-type preferences. Robust standard errors in parentheses. * < 0.10, ** < 0.05, *** < 0.01.

Chapter 5

THE EFFECT OF PAYOFF EQUALITY ON EQUILIBRIUM SELECTION

5.1 Introduction

Coordination requires that people match each other's action to achieve a mutually beneficial outcome. Coordination games generically feature multiple equilibria. This impairs comparative-static analyses and the derivation of policy advice and makes coordination games an important topic in both theoretical and experimental economics (Van Huyck, Battalio, and Beil 1990; Cooper et al. 1990; 1992; Van Huyck, Gillette, and Battalio 1992).[1] We study coordination in a minimum-effort game (MEG, Van Huyck, Battalio, and Beil 1990). In the MEG, actions are strategic complements in the sense that mutually higher payoffs require greater matched action profiles, often termed 'efforts'. In addition, equilibria differ in the degree of risk as higher benefits are associated with larger costs from miscoordination. Players thus always aim at matching the minimum effort in their group while at the same time preferring that the minimum is high. This results in a trade-off between efficiency and risk. The Pareto-dominant equilibrium involves the highest risk while individual payoffs in the least-efficient equilibrium are independent of the actions of others and thus "secure". Despite an obvious social optimum, empirical investigations of the MEG often show that groups are not able to realize the Pareto-optimal outcome (Van Huyck, Battalio, and Beil 1990; Devetag and Ortmann 2007).

The MEG mimics numerous coordination scenarios, e.g., global public goods (Hirshleifer 1983; Sandler 1998; Nordhaus 2006), computer security (Riedl, Rohde, and Strobel 2012), and team production such as assembly lines or co-authorships in academia (Brandts and Cooper 2006). In the financial literature, bank runs (Diamond and Dybvig 1983), speculative attacks against exchange-rate pegs (Obstfeld 1996), and debt crises (Cole and Kehoe 2000) are typical coordination problems. While the standard MEG is symmetric, real-world scenarios typically involve highly asymmetric benefits. For example, global vaccination campaigns such as the eradication of smallpox are minimum-effort games because the success depends on the country that implements the weakest campaign. Barrett (2006, p. 181) notes that "the real smallpox game was characterized by substantial asymmetries. By the time the eradication program began, the rich countries had already eliminated smallpox within their borders. Eradication would succeed

[1] The literature on coordination games is extensive, both theoretically and experimentally. We refer the reader to the surveys by Ochs (1995),Camerer (2003), and Devetag and Ortmann (2007).

only if the remaining endemic countries eliminated the disease." However, rich countries still occasionally imported smallpox from poor countries due to international travel and trade. Hence, while the "vaccination" equilibrium was clearly in common interest, poor countries had larger benefits from coordination on this equilibrium than rich countries.

This paper introduces asymmetric payoffs into the MEG and investigates how the resulting payoff inequality affects individuals' ability to coordinate. We manipulate payoff inequality by introducing two types: a low-cost type and a high-cost type. The low-cost type has low costs of effort provision and thus always benefits more from coordination on higher effort levels than the high-cost type. Payoffs between the two types are unequal in all but one equilibrium. We say that an equilibrium is *equality-dominant* if it is the only one featuring equal payoffs. In the experiment, either the Pareto-dominant equilibrium is equality-dominant or the secure equilibrium is equality-dominant. As a check for robustness, we further vary the strategic uncertainty of the game by manipulating off-equilibrium payoffs of the low-cost type. Under low strategic uncertainty, the low-cost type is only hardly affected by miscoordination while strongly benefiting from successful coordination. Under high strategic uncertainty, the low-cost type still benefits more from coordination but now also loses much from miscoordination.

An extensive literature shows that preferences for avoiding inequality are important across a wide range of settings and significantly impact the outcomes in many kinds of social interaction (Fehr and Schmidt 1999; Bolton and Ockenfels 2000; Fehr and Schmidt 2006). We incorporate inequality aversion into the theory of potential games (Monderer and Shapley 1996) and derive the according social-preference potential to investigate whether equality dominance might serve as an equilibrium selection device. We predict that subjects always choose the equality-dominant equilibrium irrespective of whether it is Pareto-dominant or secure. In an experimental investigation of this game, we show that social preferences indeed strongly affect behavior in the MEG, with groups converging towards the secure equilibrium when it is equality-dominant and strategic uncertainty is high. An increase in strategic uncertainty however, does not deter successful coordination if the Pareto-dominant equilibrium is equality-dominant.

We contribute to the literature in several ways. We are the first to incorporate payoff inequality into an MEG and vary which equilibrium is equality-dominant. Furthermore, we advance the literature on the role of the theory of potential games for predicting behavior in the MEG. While our experimental investigation generally finds support for our theoretical prediction which is in line with previous research, the data also show that off-equilibrium payoffs in general and strategic uncertainty in particular matter for decision making in the MEG. Increasing strategic uncertainty by asymmetric changes in off-equilibrium payoffs cannot be analyzed by the theory of potential games but matter strongly for actual behavior. Last, we complement the findings in Chen and Chen (2011) who show the effect of group identity on equilibrium selection, in revealing that also equality in equilibrium helps with regard to equilibrium selection, emphasizing the role of social preferences for coordination success.

The remainder of the paper is organized as follows. Section 5.2 presents the standard minimum-effort game. We introduce payoff inequality and derive our prediction in Section 5.3. We then present our experimental design (Section 5.4). In Section 5.5, we present our results. Section 5.6 concludes.

5.2 The Standard Minimum-Effort Game

The standard symmetric minimum-effort game MEG $= (N, E, \pi_i)$ consists of $i \in N = \{1, \ldots, n\}$ players that choose an effort level $e_i \in E_i = \{e^0, \ldots, e^k\}$ with $1 = e^0 < e^1 < \ldots < e^k$, $\Delta e = e^1 - e^0 = \ldots = e^k - e^{k-1} = 1$, and $E = E_1 \times \ldots \times E_n$. We denote an action profile by $\mathbf{e} = (e_1, \ldots, e_n) \in E$ and an equilibrium by $\mathbf{e}^*$. A player's payoff function is given by:

$$\pi_i(\mathbf{e}) = b \min\{\mathbf{e}\} - ce_i + a, \tag{5.2.1}$$

for every $i = 1, \ldots, n$, where $b > c$ is the benefit from coordination, $c > 0$ is the effort cost, and $a \geq 0$ is a constant. The generic feature of the MEG is that all common effort levels are Nash equilibria because a player does not gain from unilateral deviation. A unilateral increase in effort does not raise the minimum but only increases costs. A unilateral decrease in effort reduces the minimum but the savings in costs is less than the forfeited benefit from higher coordination because $b > c$. Hence, the number of Nash equilibria of the MEG corresponds to the number of effort levels $|E_i|$.[2]

In addition, all equilibria are Pareto-rankable, i.e., larger equilibrium efforts generate higher payoffs. Two equilibria of the game often receive special attention: the Pareto-dominant and the secure equilibrium. In the Pareto-dominant equilibrium, all players choose their maximum effort and, hence, maximize efficiency and individual payoffs. In the secure equilibrium, all players choose the minimum effort thereby minimizing efficiency in equilibrium while also minimizing their individual risk from miscoordination because the respective effort is the maximin strategy.

We analyze equilibrium selection using the theory of potential games. Potential games are games that admit a potential function and only Nash equilibria are local maximizers of the potential function (Monderer and Shapley 1996). Hence, the theory of potential games is a refinement that can be applied to the MEG (Monderer and Shapley 1996; Anderson, Goeree, and Holt 2001; Goeree and Holt 2005; Chen and Chen 2011). The potential function accounts for all players' deviation incentives and in particular

[2] As is standard in the coordination literature, we only consider pure-strategy Nash equilibria. See also Footnote 4.

coincides with risk dominance in symmetric 2x2 games (Goeree and Holt 2005).[3] In the MEG, the theory of potential games can be used as a refinement because it determines a threshold equilibrium benefit from coordination, b^{MEG}, that can select a unique equilibrium for a given value of the actual benefit b (Monderer and Shapley 1996). If the actual benefit is below the equilibrium threshold, the secure equilibrium is selected. If the actual benefit is above the equilibrium threshold, the Pareto-dominant equilibrium is selected. If the actual benefit equals the equilibrium threshold, the potential does not select an equilibrium. The predictions of this deterministic potential game are thus very clear cut.

Anderson, Goeree, and Holt (2001), Goeree and Holt (2005), and Chen and Chen (2011) augment the deterministic theory of potential games by allowing for decision errors in the sense of quantal responses (McKelvey and Palfrey 1995). Introducing noise into the decision process weakens the clear-cut statements obtained from the potential and turns them more into a prediction about convergence. Both Goeree and Holt (2005) and Chen and Chen (2011) find experimentally that coordination outcomes in a continuous MEG closely resemble the prediction of such stochastic potential games after some time of learning.

The Potential Minimum-Effort Game

The symmetric MEG belongs to the class of games that admit a potential function (Monderer and Shapley 1996; Ui 2001). A potential function P maps the set of action profiles into the real numbers such that the difference from unilateral deviations is proportional to the difference in the deviator's payoff (Ui 2001). $P : E \to \mathbb{R}$ is called a weighted potential for game MEG if

$$\pi_i(e_i, e_{-i}) - \pi_i(e_i', e_{-i}) = w_i\big(P(e_i, e_{-i}) - P(e_i', e_{-i})\big) \tag{5.2.2}$$

for every $i \in N$ and all $e_i, e_i', e_{-i} \in E$. The parameter $w_i > 0$ is the weighting factor. MEG is called a w-potential game if it admits a weighted potential function. The potential is unique up to an additive constant. If $w_i = 1$ for every i, P is an exact potential and MEG an (exact) potential game. The potential is basically a global payoff function that captures the incentives of unilateral deviations for every player.

[3]Equilibrium selection based on strategic uncertainty is at the core of the concept of *risk dominance* (Harsanyi and Selten 1988). Risk dominance is an equilibrium-selection criterion that incorporates the deviation incentives of each player. The risk-dominant equilibrium has the highest loss from unilateral deviation. The according risk-dominant strategy is also a best reply to a strategy that plays each action with equal probability. In symmetric 2x2 coordination games, such as the stag-hunt game, the secure equilibrium is usually also risk-dominant. Although this concept is theoretically appealing and experimentally successful (e.g., Cabrales, Garcia-Fontes, and Motta 2000), it is not clear how to generalize it to games with larger action spaces.

Potential games have several interesting features. First, every local maximizer of the potential function is a Nash equilibrium but not vice versa. Hence, the potential refines the set of equilibria (Monderer and Shapley 1996).[4] In case that P has a unique maximizer, the potential can therefore also be used as a selection criterion. Second, the argmax set of P often has the largest basin of attraction and several learning algorithms converge to this set. Monderer and Shapley (1996) show that every finite weighted potential game has the fictitious-play property (Brown 1951). Fictitious play is a belief-based learning process that selects a pure strategy given the current history of opponent's choices.[5] In addition, better-response dynamics (Young 1993) as well as log-linear learning (Marden and Shamma 2012) typically converge to the argmax set of P. Further, Ui (2001) shows that the unique maximizer of P is robust to imperfect information (Kajii and Morris 1997).[6]

Monderer and Shapley (1996) show that the MEG is a potential game with the following exact potential

$$P^{\mathrm{MEG}}(\mathbf{e}) = b\min\{\mathbf{e}\} - c\sum_{i\in N} e_i. \tag{5.2.3}$$

The potential (5.2.3) allows to derive a threshold b^{MEG} for the benefit parameter b such that the secure equilibrium is the unique maximizer of $P^{\mathrm{MEG}}(\mathbf{e})$ if $b < b^{\mathrm{MEG}}$ and the Pareto-dominant equilibrium is the unique maximizer if $b > b^{\mathrm{MEG}}$. The threshold is derived by noting that any candidate action profile to maximize the potential has to be an equilibrium action profile $\mathbf{e}^*$ where every player chooses the same effort, i.e., $e_i = e$ for every i.[7] Hence, the equilibrium potential of game MEG is given by $P^{\mathrm{MEG}}(\mathbf{e}^*) = be - nce$ with threshold benefit $b^{\mathrm{MEG}} = nc$. If $b < b^{\mathrm{MEG}}$, then $P^{\mathrm{MEG}}(\mathbf{e})$ is maximized at $\underline{\mathbf{e}}^*$ where $e_i = e^0$ for every i. If $b > b^{\mathrm{MEG}}$, then $\overline{\mathbf{e}}^*$, where $e_i = e^k$ for every i, maximizes $P^{\mathrm{MEG}}(\mathbf{e})$. If $b = b^{\mathrm{MEG}}$, every equilibrium maximizes $P^{\mathrm{MEG}}(\mathbf{e})$ and the potential does not refine the set of equilibria.

The empirical practicability of the theory of potential games in predicting behavior in the MEG has first been discussed in Monderer and Shapley (1996). Goeree and Holt (2005) and Chen and Chen (2011) test the theory directly with experimental data showing that predicted behavior actually often coincides with empirical behavior.

[4] Further, if P is concave and continuously differentiable, then every mixed-strategy equilibrium profile is pure and maximizes P (see Neyman 1997 and footnote 4 in Monderer and Shapley 1996).

[5] Fictitious play modifies the player's beliefs given the history of his opponents' choices. The player then rationally chooses an action based on these beliefs.

[6] An equilibrium of the complete-information game G is robust if every incomplete-information game with payoffs given by G with high probability has a Bayesian-Nash equilibrium such that the equilibrium of G is played with high probability (Ui 2001).

[7] This observation holds for all potentials in this paper and we thus focus on the equilibrium potential and the equilibrium benefit for clarity of exposition. Nevertheless, keep in mind that a potential has to fulfill condition (5.2.2).

5.3 The Asymmetric Minimum-Effort Game

We modify the standard symmetric MEG by introducing heterogeneous costs for the players. We first examine standard preferences, i.e., players only consider their own payoffs. Subsequently, we analyze the role of payoff (in)equality for equilibrium selection using a Fehr and Schmidt (1999) preference specification.

5.3.1 Asymmetric MEG with Standard Preferences

There are two types identified by $\theta \in \{l, h\}$ and c_θ is the cost parameter of type θ. Higher types ($\theta = h$) have higher costs than lower types ($\theta = l$), i.e., $c_h > c_l$. We will construct the payoff function such that all but one equilibrium feature unequal payoffs. Second, we vary which of the two extreme equilibria features equal payoffs. The treatment indicator $\tau \in \{\underline{\tau}, \overline{\tau}\}$ identifies the equilibrium with equal payoffs. If $\tau = \underline{\tau}$, then the secure equilibrium $\underline{\mathbf{e}}^*$ has equal payoffs and all other equilibria have unequal payoffs. If $\tau = \overline{\tau}$, then the Pareto-dominant equilibrium $\overline{\mathbf{e}}^*$ has equal payoffs and all other equilibria have unequal payoffs. We say that an equilibrium is *equality-dominant* if it has equal payoffs.

The resulting asymmetric minimum-effort game (aMEG) has the same set of players and the same action set as the standard MEG. However, the individual payoff function with heterogeneous costs now reads:

$$\pi_i^{\text{het}}(\mathbf{e}; \theta, \tau) = b \min\{\mathbf{e}\} - \begin{cases} c_l e_i + a + c_l + d(l, \tau) & \text{if } \theta = l \\ c_h e_i + a + c_h + d(h, \tau) & \text{if } \theta = h, \end{cases} \tag{5.3.1}$$

where $b > c_h > c_l > 0$. Hence, the set and Pareto-ranking of Nash equilibria is the same as in the standard MEG of Section 5.2. We abbreviate (5.3.1) by $\pi_{i,\theta,\tau}$. The fixum $d(\theta, \tau) \in \{\underline{d}(\theta), \overline{d}(\theta)\}$ depends on type θ for a given value of the treatment indicator τ. We will discuss the value of $d(\theta, \tau)$ for both treatments in the following.

Secure Equilibrium is Equality-Dominant. In this case, $\tau = \underline{\tau}$ and $d(\theta, \underline{\tau})$ is given by:

$$\underline{d}(\theta) = 0, \quad \theta = l, h. \tag{5.3.2}$$

Hence, there is no adjustment of payoffs for $\tau = \underline{\tau}$ and the constants a, c_l and c_h scale payoffs such that $\underline{\mathbf{e}}^* = (e_1^0, \ldots, e_n^0)$ is equality-dominant. In any other equilibrium $\mathbf{e}^*$, payoff inequality between the low and high-cost type is given by $(\mathbf{e}^* - 1)(c_h - c_l)$. Hence, the slope of inequality is constant and greater equilibria generate greater payoff inequality.

Pareto-Dominant Equilibrium is Equality-Dominant. In this case, $\tau = \overline{\tau}$ and $d(\theta, \overline{\tau})$ is given by:

$$\overline{d}(\theta) = \begin{cases} -d & \text{if } \theta = l \\ +d & \text{if } \theta = h. \end{cases} \tag{5.3.3}$$

In treatment $\overline{\tau}$, the additional fixum $d > 0$ scales payoffs such that the Pareto-dominant equilibrium has equal payoffs, i.e., $\overline{\mathbf{e}}^* = (e_1^k, \ldots, e_n^k)$ is equality-dominant. We achieve this by setting

$$d = \frac{(|E_i| - 1)(c_h - c_l)}{2} \tag{5.3.4}$$

such that d equals the average maximum inequality between the two types. Hence, d mirrors the previous wedge between payoffs. However, in this case, smaller equilibria generate greater payoff inequality. The slope of inequality is given by the cost difference $c_h - c_l$ and is the same as if the secure equilibrium was payoff-dominant. It is important to note that due to the definition of $d(\theta, \tau)$, both games MEG and aMEG have the same efficiency measured by the sum of players' payoffs in each action profile.

Asymmetric Potential MEG. The game aMEG $= (N, E, \pi_{i,\theta,\tau})$ is a potential game admitting the following exact potential function:

$$P^{\text{aMEG}}(\mathbf{e}) = b \min\{\mathbf{e}\} - c_l \sum_{i \in N_l} e_i - c_h \sum_{i \in N_h} e_i \tag{5.3.5}$$

where N_h is the set of low-cost types and N_h is the set of high-cost types. There are n_l low-cost types and n_h high-cost types and $n = n_h + n_l$. The equilibrium threshold benefit is thus $b^{\text{aMEG}} = n_l c_l + n_h c_h$. The interpretation of the threshold is the same as before. If $b < b^{\text{aMEG}}$ then $\{\underline{\mathbf{e}}^*\} = \arg\max_{\mathbf{e} \in E} P^{\text{aMEG}}(\mathbf{e})$ and the secure equilibrium maximizes the potential. If $b > b^{\text{aMEG}}$ then $\{\overline{\mathbf{e}}^*\} = \arg\max_{\mathbf{e} \in E} P^{\text{aMEG}}(\mathbf{e})$ and the Pareto-dominant equilibrium maximizes the potential. However, if $b = b^{\text{aMEG}}$ then the potential is the same in all equilibria. We will now introduce social preferences into game aMEG and analyze the effect of payoff inequality on the equilibrium threshold benefit and hence on equilibrium selection.

5.3.2 Asymmetric MEG with Social Preferences

Chen and Chen (2011) emphasize the importance of social preferences in coordination. In particular, they analyze the effect of group identity on behavior in the MEG and find that subjects choose higher effort levels if playing against an in-group member rather than an out-group member. Their results show that a common group identity can lead to more efficiency in the MEG.[8]

[8]Other explanations are also in line with such a pattern. For example, an increase in perceived similarity among members of the in-group might align higher-order beliefs in a way that it is easier for

We assume that subjects have social preferences and are averse to payoff inequality. Fehr and Schmidt (1999) propose that people dislike absolute payoff differences. They distinguish advantageous inequality ("guilt") where the individual earns more than her peers and disadvantageous inequality ("envy") where the individual earns less than her peers. Hence, in an aMEG, individual utility under Fehr-Schmidt (FS) preferences is given by:

$$\begin{aligned} u_i(\mathbf{e};\theta,\tau,\alpha,\beta) = \pi_{i,\theta,\tau} &- \alpha_i \frac{1}{n-1}\sum_{j\neq i}\max\{\pi_{j,\theta,\tau}-\pi_{i,\theta,\tau},0\} \\ &- \beta_i \frac{1}{n-1}\sum_{j\neq i}\max\{\pi_{i,\theta,\tau}-\pi_{j,\theta,\tau},0\}. \end{aligned} \tag{5.3.6}$$

The factor $\beta_i \in [0,1)$ weights guilt against the own monetary payoff and $\alpha_i \geq 0$ weights envy for every i. Fehr and Schmidt (1999) further assume that people are loss-averse in social comparison, i.e., envy weights heavier than guilt and hence $\alpha_i \geq \beta_i$. Note that payoffs only differ between types but are equal within types. We abbreviate (5.3.6) by u_i and denote the aMEG with social preferences by aMEG-SP $= (N, E, u_i)$.

Because of the construction of $d(\theta,\tau)$, the absolute payoff difference between types in any equilibrium $\mathbf{e}^*$ is given by:

$$\Delta\pi_i^*(\tau) = \begin{cases} \pi^*_{i,l,\underline{\tau}} - \pi^*_{i,h,\underline{\tau}} = (c_h - c_l)(e-1) & \text{if } \tau = \underline{\tau} \\ \pi^*_{i,h,\overline{\tau}} - \pi^*_{i,l,\overline{\tau}} = (c_h - c_l)(|E_i| - e) & \text{if } \tau = \overline{\tau}, \end{cases} \tag{5.3.7}$$

for every i. If $\tau = \underline{\tau}$, then low-cost types make greater equilibrium profits than high-cost types unless the equilibrium is the equality-dominant equilibrium $\underline{\mathbf{e}}^*$ in which case payoffs are the same. If $\tau = \overline{\tau}$, then high-cost types make greater equilibrium profits than low-cost types in all but the equality-dominant equilibrium $\overline{\mathbf{e}}^*$. Hence, individual equilibrium utility under FS preferences is given by:

$$u_i(\mathbf{e}^*;\theta,\tau,\alpha_i,\beta_i) = \begin{cases} \pi^*_{i,l,\tau} - \beta_i \frac{n_h}{n-1}\Delta\pi^*(\underline{\tau}) - \alpha_i \frac{n_h}{n-1}\Delta\pi^*(\overline{\tau}) & \text{if } \theta = l \\ \pi^*_{i,h,\tau} - \alpha_i \frac{n_l}{n-1}\Delta\pi^*(\underline{\tau}) - \beta_i \frac{n_l}{n-1}\Delta\pi^*(\overline{\tau}) & \text{if } \theta = h. \end{cases} \tag{5.3.8}$$

Using (5.3.7) and (5.3.8), we can construct the equilibrium potential for game aMEG-SP and derive the equilibrium threshold $b^{\text{aMEG}}(\tau)$ that we state in Lemma 1.

Lemma 1 *[Equilibrium Threshold Benefit of aMEG-SP] In the minimum-effort game with heterogeneous costs and social preferences (aMEG-SP), the equilibrium threshold benefit is given by*

$$b^{aMEG\text{-}SP}(\tau) = n_l c_l + n_h c_h \begin{cases} +\frac{n_h}{n-1}(c_h - c_l)\sum_{i\in N_l}\beta_i + \frac{n_l}{n-1}(c_h-c_l)\sum_{i\in N_h}\alpha_i & \textit{if } \tau = \underline{\tau} \\ -\frac{n_h}{n-1}(c_h - c_l)\sum_{i\in N_l}\alpha_i - \frac{n_l}{n-1}(c_h-c_l)\sum_{i\in N_h}\beta_i & \textit{if } \tau = \overline{\tau}. \end{cases}$$

individuals to match others' actions. Another explanation might be a higher degree of trust towards in-group members (see, for example, Mussweiler and Ockenfels (2013) for evidence on similarity-effects in social interaction). Manzini, Sadrieh, and Vriend (2009) further show that simple social cues signaling trust can help groups to overcome coordination failures.

The derivation is relegated to Appendix 5.A.

The equilibrium threshold allows us to derive predictions about equilibrium selection. In the experiment, we set the benefit from coordination to the value of b^{aMEG}, i.e., to the equilibrium value without social preferences. In that case, the potential does not refine the set of equilibria for standard preferences. However, if players are inequality averse, they choose the minimum effort if the secure equilibrium is equality-dominant and the maximum effort if the Pareto-dominant equilibrium is equality-dominant.

Proposition 7 *[Equilibrium Selection Under Social Preferences] Consider $b = b^{aMEG}$. Under social preferences, if the secure equilibrium is equality-dominant, subjects choose the minimum effort level. If the Pareto-dominant equilibrium is equality-dominant, subjects choose the maximum effort level.*

Proof. Consider $b = b^{\text{aMEG}}$ and note that $b^{\text{aMEG}} = b^{\text{aMEG}}(\underline{\tau}) = b^{\text{aMEG}}(\overline{\tau})$. Players are inequality averse, i.e., $\alpha_i, \beta_i > 0$ for every i. If $\tau = \underline{\tau}$, then $b^{\text{aMEG}}(\underline{\tau}) < b^{\text{aMEG-SP}}(\underline{\tau})$. The equilibrium potential $P^{\text{aMEG-SP}}(\underline{\tau})$ is maximized in the minimum action profile $\underline{\mathbf{e}}^*$ and all players choose the minimum effort. If $\tau = \overline{\tau}$, then $b^{\text{aMEG}}(\overline{\tau}) > b^{\text{aMEG-SP}}(\overline{\tau})$. The equilibrium potential $P^{\text{aMEG-SP}}(\overline{\tau})$ is maximized in the maximum action profile $\overline{\mathbf{e}}^*$ and all players choose the maximum effort. □

In the experiment, we focus on $n = 2$ players in which case the equilibrium threshold benefit without social preferences reduces to $b^{\text{aMEG}} = c_l + c_h$. The equilibrium threshold benefit with social preferences is then given by $b^{\text{aMEG-SP}}(\underline{\tau}) = c_l + c_h + (\alpha_j + \beta_i)(c_h - c_l) > b^{\text{aMEG}}$ if the secure equilibrium is equality-dominant and by $b^{\text{aMEG-SP}}(\overline{\tau}) = c_l + c_h - (\alpha_j + \beta_i)(c_h - c_l) < b^{\text{aMEG}}$ if the Pareto-dominant equilibrium is equality-dominant.

5.3.3 Robustness: Increasing Strategic Uncertainty

The prediction in Proposition 7 is very clear cut because any positive degree of social preferences selects a unique equilibrium. Section 5.4.1 presents our parameterization. We choose parameters to maximize equilibrium inequality for a given value of the benefit b. This means that c_l is much smaller than c_h under the condition that $b^{\text{aMEG-}} = c_l + c_h$. While the benefit from coordination on higher effort levels is very large for the low-cost type, their loss from miscoordination is small and thus they do not risk much to strive for high effort levels. Hence, the low-cost types can try to enforce the Pareto-dominant equilibrium by sticking to the maximum effort despite some experiences that this does not always pay off.

For the high-cost type, this situation might have the spirit of a decision rather than a strategic problem because he decides whether to "follow" the maximum effort or not. In other words, strategic uncertainty is greatly reduced if the low-cost types face low payoff risks. This is a consequence of our decision to maximize payoff inequality in equilibrium. To make the risk for the low-cost type more meaningful, we have to change the off-equilibrium payoffs for the case that the low-cost type chooses higher efforts. Such a change increases the strategic uncertainty in the game because the high-cost type can no longer be sure that the low-cost type sticks to high effort levels. Hence, we test the robustness of our results with two additional treatments that increase strategic uncertainty in the aMEG.

We manipulate the low-cost type's off-equilibrium payoffs by subtracting a term that linearly increases in the difference between his own effort level, e_i, and the effort level of his partner, e_j. Specifically, the adjusted payoff is given by

$$\tilde{\pi}_{i,\theta,\tau} = \pi_{i,\theta,\tau} - \zeta_\theta \cdot \max\{e_i - e_j, 0\} \tag{5.3.9}$$

where $\zeta_l > \zeta_h \geq 0$ is the adjustment factor of the low-cost and high-cost type, respectively. Appendix 5.A.2 shows the according normal form of the adjusted asymmetric MEG which we denote by aMEG-adj $= (N, E, \tilde{\pi}_{i,\theta,\tau})$.[9]

5.4 Experiment

We first present the parameters used in the experiment and define the treatments that we conduct. Subsequently, we discuss the procedural and organizational details of the experiment.

[9]Note that the incorporation of such asymmetric off-equilibrium payoffs does not allow to construct a potential function fulfilling condition (5.2.2) for all $e_i, e_i', e_{-i} \in E$. This is because changes in the potential from unilateral deviations do no longer coincide with changes in the deviator's payoff if $e_i \neq e_j$. Thus, we cannot analyze the role of increased strategic uncertainty using the theory of potential games. However, we can determine the best responses of each player given the strategy of his partner under FS preferences. The low-cost type always benefits more from higher matched action profiles, hence, his strategy is to always match his partners effort level. If the secure equilibrium is equality-dominant, the high-cost type chooses the minimum effort in our high-uncertainty setting as long as $\tilde{\pi}_{i,h,\underline{\tau}}(1,1) > \tilde{\pi}_{i,h,\underline{\tau}}(2,1) > \tilde{\pi}_{i,h,\underline{\tau}}(2,2)$ for the action profile $\mathbf{e} = (e_l, e_h)$. This condition can be rewritten as $\alpha_h > \underline{\alpha}(\beta_h) > 0$ where

$$\underline{\alpha}(\beta_h) := \frac{b - c_h}{c_h - c_l} + \beta_h \frac{c_l + \zeta_l}{c_h - c_l} \tag{5.3.10}$$

denotes a minimum level of disadvantageous inequality for the high-cost type (With a slight abuse of notation, let β_h denote the advantageous-inequality parameter of the high-cost type.). For an average value of advantageous inequality given by $\beta = 0.3$, we obtain a minimum level of disadvantageous inequality of $\underline{\alpha}(0.3) = 0.43$ which is much below the average level found in the literature. For example, Fehr and Schmidt (1999) report an average α of 0.85 and an average β of 0.32. Goeree and Holt (2000) report an average α of 0.84 and an average β of 0.39. Blanco, Engelmann, and Normann (2011) report an average α of 0.91 and an average β of 0.38. If the Pareto-dominant equilibrium (7,7) is equality-dominant, best responses under FS preferences coincide with best responses under standard preferences and do not refine the set of equilibria as long as $\alpha_i, \beta_i \geq 0$. As Engelmann (2012) shows, negative values for α and β are equivalent to a preference for efficiency (e.g., Charness and Rabin 2002). In other words, agents might be willing to increase inequality in order to maximize efficiency.

5.4.1 Parameters and treatments

In our experiment, subjects play the asymmetric MEG in groups of $n = 2$ as in Goeree and Holt (2005) and Chen and Chen (2011). In contrast to these former studies, we stick to the standard discrete representation of the MEG (Van Huyck, Battalio, and Beil 1990). The discrete MEG with two players allows us to write the game in normal form which has two advantages. First, subjects see the entire distribution of payoffs for any action profile and do not have to calculate their own and their partner's payoffs. Second, the normal-form representation is easier to understand compared to stating the payoffs in their functional form.

Subjects simultaneously choose an effort level from the set $E_i = \{1, 2, \ldots, 7\}$. The benefit factor is $b = 12$ and the costs are $c_l = 1$ for the low-cost type and $c_h = 11$ for the high-cost type. The constant is $a = 55$. This implies that the additional fixum takes the values $d = 0$ if the secure equilibrium is equality-dominant and $d = (7-1)(11-1)/2 = 30$ if the Pareto-dominant equilibrium is equality-dominant. In the robustness treatments, low-cost types have an adjustment factor of $\zeta_l = 10$ and high-cost types have an adjustment factor of $\zeta_h = 0$.

We thus manipulate two factors: (i) equality dominance and (ii) strategic uncertainty. Equality dominance has two levels indicating whether the secure equilibrium $(1, 1)$ is equality-dominant or whether the Pareto-dominant equilibrium $(7, 7)$ is equality-dominant. Strategic uncertainty also has two levels indicating low uncertainty (LU) if $\zeta_l = \zeta_h = 0$ or high uncertainty (HU) if $\zeta_l = 10$ and $\zeta_h = 0$. We fully cross both factors. This results in four treatments: LU11, LU77, HU11, and HU77. As successful coordination should be most prevalent in treatment LU77, we refer to this treatment as our baseline and interpret all results relative to this benchmark throughout the analysis.

5.4.2 One-Shot and Repeated Interaction

Our theory considers a static game with simultaneous decisions. However, subjects in coordination experiments typically play the stage game repeatedly to coordinate on one equilibrium over time. Theories involving potential games in MEGs are often used in this sense of convergence (e.g. Goeree and Holt 2005; Chen and Chen 2011). We also assume that groups converge while learning. However, as our theory suggests, we expect to find differences even in a one-shot interaction.

Therefore, we apply a two-part procedure during our experiment. Subjects were aware that the experiment consists of two parts but did not know what each part was about. In the first part, subjects played game aMEG for only one round. They were told that they would receive information about this part only after the second part was completed. In the second part, subjects played the stage game for $T = 30$ periods. Each period $t = 1, \ldots, T$ consists of two screens: (i) a decision screen and (ii) a feedback screen (see Appendix 5.B for screenshots). We adopt the design of Chen and Chen (2011) while also showing the normal form of the game on the decision screen. After making their decisions for round t, each subject received the following information: her own effort, the effort of her partner, her own payoff, and the payoff of her partner. Furthermore, as in Chen and Chen (2011), a subject saw her history of play for every period $k = 1, \ldots, t-1$: her effort in k, her period-k partner's effort, her own payoff in period k, and her period-k partner's payoff.

5.4.3 Organization

We conducted the experiment in the Cologne Laboratory for Economic Research (CLER), University of Cologne, Germany.[10] The experiment was computerized via *z-Tree* (Fischbacher 2007). We used the recruiting system *ORSEE* (Greiner 2004) to invite a random sample of the laboratory's subject pool via email. A total of 256 subjects participated in the experiment. Subjects were randomly assigned to one of the treatment groups and were only allowed to participate once.

We ran eight sessions with 32 subjects each. Half the subjects were low-cost types (denoted as "role X" in the instructions) and the other half were high-cost types (role Y). We applied a strangers matching procedure as follows: Subjects were grouped in *cohorts* of $n = 8$ individuals with four subjects in role X and four subjects in role Y. Each round, within a cohort, we randomly matched one X subject with one Y subject.

The experiment took place in January and February 2015. Subjects received a hard copy of the instructions (see Appendix 5.C) which were read out loud. Then, subjects were allowed to take as much time as needed to familiarize themselves with the experiment. Questions were answered in private. During the experiment all payoffs were stated in Experimental Currency Units (ECU). The exchange rate was 30 ECU to 1 Euro for the one-shot part and 400 ECU to 1 EUR for the repeated part. Average payment was 12.25 EUR (about 13.99 USD) for about 60 minutes of experimentation.

5.5 Results

For our analysis, we are mainly interested in effort choices, coordination success, and efficiency. We present the results of both the one-shot interaction (part one of the experiment) and the repeated interaction (part two) in each of these dimensions. We first discuss the effort choices. Here, we also show whether and how the cost types influence behavior. Afterwards, we investigate how differences in choices affect coordination success

[10] `www.cler.uni-koeln.de`.

and efficiency.[11] In part one of the experiment, each of the $N = 256$ subjects generates one independent observation. In part two, each subject plays the stage game for $T = 30$ periods generating 7680 observations clustered in 32 cohorts in a panel-structure dataset, generating 8 independent observations per treatment.

5.5.1 Effort Provision

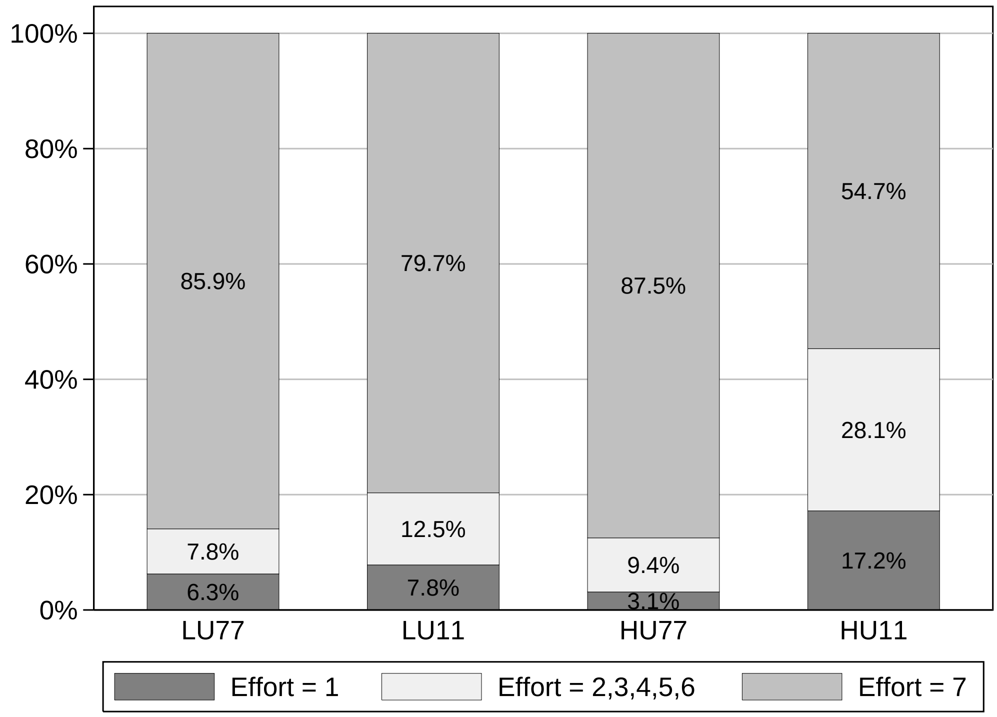
Figure 5.1 shows the distribution of one-shot efforts across treatments. In our baseline treatment LU77, the equilibrium $(7,7)$ is both equality- and Pareto-dominant. Proposition 7 states that subjects choose the maximum effort of seven. In total, 85.9% of the subjects indeed choose the maximum effort level while only 6.3% choose the minimum. In treatment LU11, the equilibrium $(1,1)$ is equality-dominant but Pareto-inferior to all other equilibria. Proposition 7 now states that subjects choose the minimum effort. However, in total, 79.7% of the subjects choose the maximum effort and 7.8% choose the minimum effort. Effort choices do not differ between treatments, with the average efforts in both treatments are close to the maximum (6.47 vs. 6.28; Wilcoxon-Mann-Whitney U-test, $p = 0.3665$). This suggests that, at least in the initial one-shot interaction, decision makers do not choose in line with our theoretical prediction in case of LU11.[12] With regard to the robustness treatments, in HU77, a total of 87.5% choose the maximum effort and 3.1% choose the minimum while in HU11 54.7% choose the maximum and 17.2% choose the minimum. Choices are significantly different (6.58 vs. 5.19, U-test, $p = 0.0000$).[13] Furthermore, pooling the data across treatments, we observe that high-cost types choose lower effort levels than low-cost types (U-test, $p = 0.0058$) and that subjects tend to react to increased strategic uncertainty by decreasing their effort choices (U-test, $p = 0.0243$). These results suggest that subjects generally coordinate quite successfully while we still find evidence for differences between treatments conditional on the equality-dominant equilibrium. In addition, both cost types and the level of uncertainty seem to be relevant when it comes to coordination success which we analyze in more detail in the following.

Collapsing all data of part two on the cohort level yields eight independent observations per treatment. We find that the trend observed in the one-shot interaction is similar but more pronounced for the collapsed data. LU77 again has the highest average effort which is significantly different from LU11 (U-test, $p = 0.0208$) while effort levels

[11] Summary statistics on demographic and attitudinal variables regarding risk and social preferences of our subject sample can be found in Appendix C. This data has been elicited through a post-experimental questionnaire.

[12] Even though these results do not reflect our theoretical prediction, they are in line with high initial effort levels in former research on the discrete MEG (e.g. Van Huyck, Battalio, and Beil 1990; Manzini, Sadrieh, and Vriend 2009).

[13] The distribution of efforts in HU11 also differs from LU77 (U-test, $p = 0.0001$) and HU77 (U-test, $p = 0.0000$).

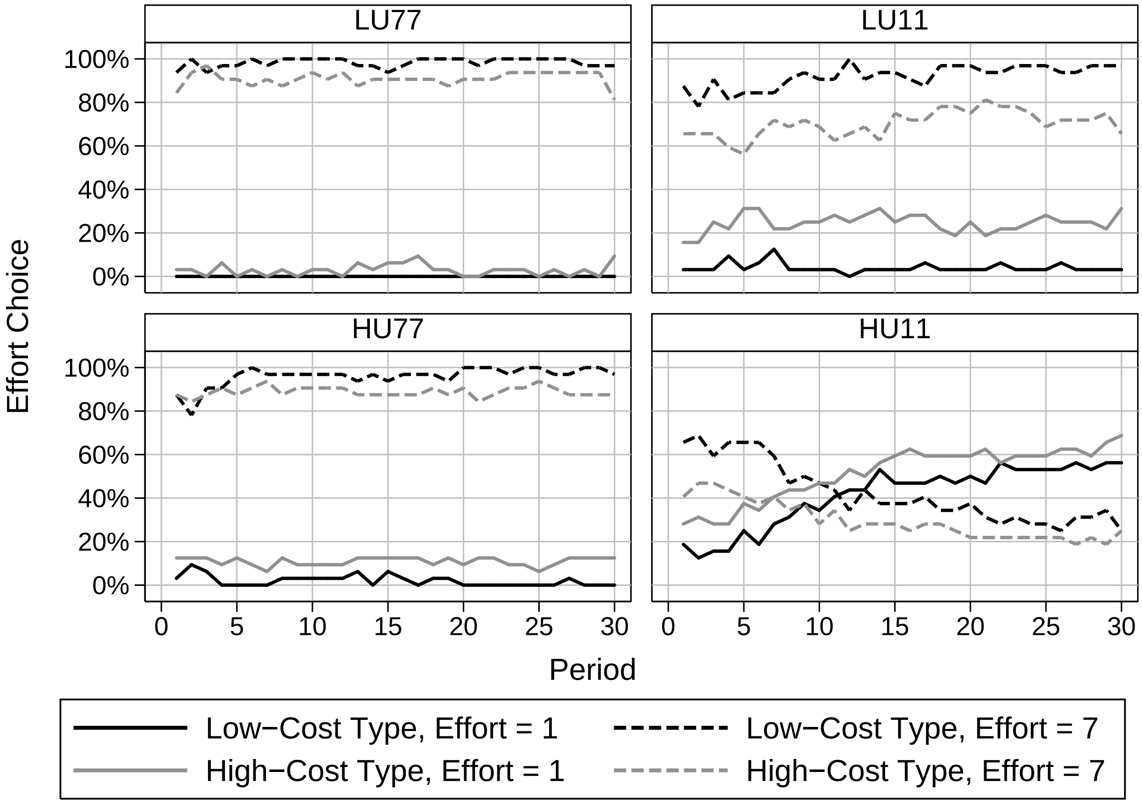


Notes: Reported is the distribution of one-shot effort separated by treatment. $N = 256$. *77* indicates that the Pareto-Dominant equilibrium is equality-dominant and *11* indicates that the secure equilibrium is equality-dominant. *LU* refers to low strategic uncertainty and *HU* refers to high strategic uncertainty.

Figure 5.1: Distribution of One-Shot Efforts across Treatments.

in LU77 are not statistically distinguishable from HU77 (U-test, $p = 0.6737$). Moreover, effort choices differ substantially and significantly between LU11 and HU11 (U-test, $p = 0.0209$), showing that under high uncertainty subject refrain from choosing high effort levels when $(1, 1)$ is equality-dominant.

Over the course of the 30 periods, most of the subjects either choose the maximum or the minimum effort level which are, with regard to our theory, the two levels we are primarily interested in. Figure 5.2 shows the frequency of these effort levels separated by cost type for each treatment. The top panel shows the two main treatments. We see that both types most often choose the maximum effort in LU77 whereas high-cost types choose somewhat lower effort levels in LU11. The frequency of maximum effort choices across all periods drops from 98.3% in LU77 to 92.0% in LU11 for low-cost types and from 90.9% to 70.2% for high-cost types. The large drop for high-cost types indicates the role of inequality aversion. Even though low-cost types tend to generally stay with the maximum effort, some high-cost types refuse to choose the maximum which hence often results in non-equilibrium outcomes. However, because low-cost types do not lose much from miscoordination in LU11, they continue to choose high effort levels anticipating that many high-cost types choose the maximum.



Notes: Reported is the frequency of effort choice for the maximum (7) and the minimum (1) effort level for low and high-cost types over periods separated by treatments. $N = 256$. *77* indicates that the Pareto-dominant equilibrium (7,7) is equality-dominant and *11* indicates that the secure equilibrium (1,1) is equality-dominant. *LU* refers to low strategic uncertainty and *HU* refers to high strategic uncertainty.

Figure 5.2: Frequency of Effort Choices across Treatments.

The bottom panel shows the robustness treatments with high strategic uncertainty. We observe that both types generally choose the maximum effort in HU77. The left graph confirms our result that strategic uncertainty does not have much of an effect if (7,7) is equality-dominant. Effort levels remain high throughout the experiment. However, the right graph shows how increased strategic uncertainty interacts with equality dominance. In treatment HU11, low-cost types are confronted with severe losses from miscoordination. Hence, in contrast to LU11, it is expensive to stick to the maximum effort if high-cost types refuse to choose the maximum as well. This explains the convergence to lower effort levels in HU11 depicted in Figure 5.D.10 in Appendix 5.D. In fact, we observe that modal effort-choices in HU11 reverse around period ten. Starting with around 60% of the low-cost types choosing the maximum effort in period one, this fraction drops to around

30% for the last ten periods. The share of high-cost types choosing the maximum effort also drops by 50% from about 40% in period one to around 20% in the last ten periods.

We further run parametric regressions to analyze the effect of equality dominance and strategic uncertainty on effort choices while controlling for demographics and attitudinal variables regarding risk and social preferences that we define in Appendix 5.D.1. Dummy variable *ED11* takes value one if (1,1) is equality-dominant and zero if (7,7) is equality-dominant. *High SU* takes value one if strategic uncertainty is high and zero if strategic uncertainty is low. We use these two dummies and their interaction as our main explanatory variables. Their factorial crossing generates all four treatment conditions. It follows that the baseline treatment in the regression is given by LU77. *Period* is an integer variable indicating the period in which a decision was made. The dummy *Type* takes value one for high-cost types and zero for low-cost types.

Table 5.1 shows the results of random-effects regressions of effort choices on the treatment factors and various controls. The first two models utilize the complete data while the last two models are based on data from the last five periods to observe behavior after convergence. Models (I) and (III) analyze the main explanatory variables and only control for period. Models (II) and (IV) control for players' types, demographics and attitudinal variables. Models (II Type) and (IV Type) examine the effects of the two cost types.

The regression results confirm our previous results and show that the effects are robust over time and against the inclusion of controls. We find that the impact of equality dominance is significant and that strategic uncertainty has no significant effect by itself. The coefficients have the hypothesized signs. If (1,1) is equality-dominant, subjects choose lower effort levels than if (7,7) is equality-dominant. The interaction effect between equality dominance and strategic uncertainty is quantitatively large and significant. In line with our previous results, strategic uncertainty only influences subjects' effort choices if (1,1) is equality-dominant. Regarding the control variables, we see that none of the demographic or attitudinal variables has a consistent or significant effect on effort choices.

Result 8 *[Effort Choice] If (7,7) is equality-dominant, subjects choose significantly larger effort levels than if (1,1) is equality-dominant. Strategic uncertainty has no effect by itself but strongly reduces effort levels if (1,1) is equality-dominant. The effects are robust over time and against the inclusion of demographics as well as risk and social preferences.*

Models II and IV suggest that *Type* has a negative effect on effort choices, i.e., high-cost types choose lower effort levels, confirming the impression of Figure 5.2. We disentangle the impact of the cost type in two further models. In Models (II Type) and (IV Type), we interact the *Type*-dummy with the treatment indicators. We find that the main effect of equality dominance vanishes while its interaction with strategic uncertainty increases in absolute terms and remains significant. The main effect of *Type* is now less

Table 5.1: Repeated-Interaction Effort: Random-Effects Regressions.

	All Periods			Last Five Periods		
Model	(I)	(II)	(II Type)	(III)	(IV)	(IV Type)
Estimation	GLS	GLS	GLS	GLS	GLS	GLS
ED11	-0.818***	-0.657**	-0.096	-0.759**	-0.562*	0.012
	(0.278)	(0.281)	(0.161)	(0.324)	(0.317)	(0.216)
High SU	-0.254	-0.141	-0.071	-0.241	-0.087	0.047
	(0.234)	(0.242)	(0.128)	(0.200)	(0.219)	(0.118)
ED11 X High SU	-2.048***	-2.228***	-2.674***	-2.866***	-3.106***	-3.741***
	(0.768)	(0.705)	(0.678)	(0.851)	(0.760)	(0.800)
Period	-0.016*	-0.016*	-0.016*	-0.022	-0.022	-0.022
	(0.009)	(0.009)	(0.009)	(0.023)	(0.023)	(0.023)
Type		-0.682***	-0.285**		-0.659***	-0.280**
		(0.146)	(0.140)		(0.166)	(0.123)
Age		0.003	0.004		0.001	0.003
		(0.021)	(0.021)		(0.024)	(0.023)
Male		-0.151	-0.158		-0.371	-0.369
		(0.166)	(0.161)		(0.231)	(0.232)
German Native		-0.337	-0.354		-0.429*	-0.458*
		(0.222)	(0.223)		(0.253)	(0.259)
Risk-Averse		-0.416	-0.459		-0.246	-0.286
		(0.282)	(0.291)		(0.492)	(0.506)
Risk-Seeking		0.298	0.259		0.365	0.334
		(0.414)	(0.431)		(0.493)	(0.514)
Social Preference		-0.055	-0.062		-0.046	-0.052
		(0.039)	(0.039)		(0.048)	(0.047)
Type X ED11			-1.107***			-1.129**
			(0.386)			(0.449)
Type X High SU			-0.131			-0.258
			(0.287)			(0.324)
Type X ED11 X High SU			0.893*			1.267**
			(0.513)			(0.614)
Constant	7.085***	8.088***	7.943***	7.459***	8.464***	8.307***
	(0.159)	(0.715)	(0.712)	(0.644)	(0.996)	(0.998)
Observations	7680	7680	7680	1280	1280	1280
Subjects	256	256	256	256	256	256
Cohorts	32	32	32	32	32	32
R2	0.29	0.33	0.34	0.41	0.45	0.46

Notes: Reported are random-effects GLS regressions with random effect on subject. Dependent variable is the chosen effort. Models (I), (II), and (II Type) are based on data from all periods. Models (III), (IV) and (IV Type) are based on data from the last five periods. *ED11* indicates that (1,1) is equality-dominant. *High SU* indicates high strategic uncertainty. Standard errors clustered on the cohort level in parentheses. * < 0.10, ** < 0.05, *** < 0.01.

in absolute terms but remains significant. The interaction terms including *Type* show that if (1,1) is equality-dominant, high-cost types choose significantly lower effort levels.

The interaction between *Type* and strategic uncertainty is not significant, suggesting that the two cost types do not react differently to strategic uncertainty. Finally, the three-way interaction confirms that the low-cost types under high strategic uncertainty abstain from choosing high effort levels if (1,1) is equality-dominant because this is getting too expensive. This behavior results in a convergence towards worse equilibria as evident in Figure 5.2.

Result 9 *[The Role of Types on Effort Choice] The effect of equality dominance varies by cost type. High-cost types choose significantly lower effort levels in particular in cases where payoffs in (1,1) are equal. Low-cost types' willingness to choose high effort levels only vanishes when confronted with higher losses due to increased strategic uncertainty.*

5.5.2 Coordination and Efficiency

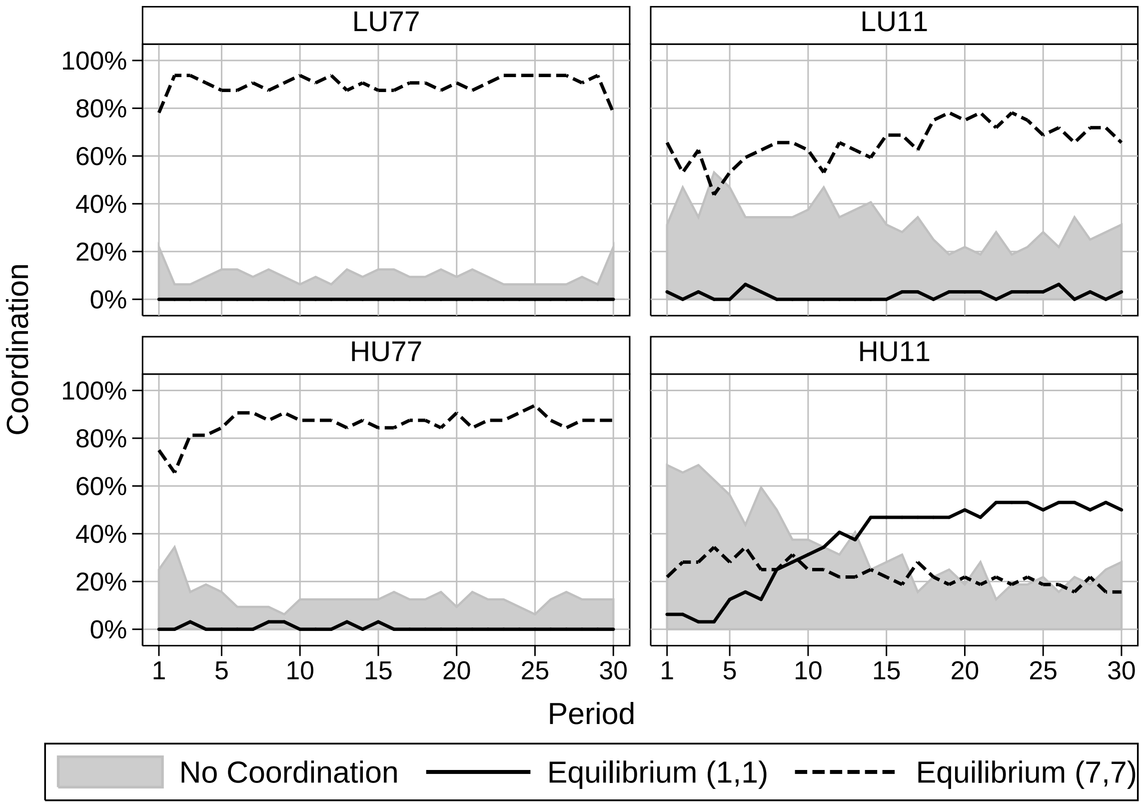
The differences in choices should also cause differences in outcomes between treatments. Van Huyck, Battalio, and Beil (1990) argue that there are two types of coordination failure in the MEG. First, players may fail to coordinate at all. That is, they may fail to anticipate their partner's effort and thus choose $e_i \neq e_j$. Hence, miscoordination results in disequilibrium. Second, players may fail to coordinate on the Pareto-efficient equilibrium and thus waste efficiency. That is, they choose a common action profile, $e_i = e_j$, but this action profile is not optimal in efficiency terms, i.e., $(e_i, e_j) \neq (7, 7)$. This section reports how well subjects manage to overcome both these forms of coordination failures.

Coordination is achieved if both players choose the same effort level, defined by a variable that can take the values zero (no coordination) or one (coordination). Furthermore, following Chen and Chen (2011), we call *Efficiency* the difference between the sum of actual payoffs and minimum payoffs normalized by the range of possible payoffs.

$$\begin{aligned}\text{Efficiency} &:= \frac{\text{Actual Payoffs} - \text{Minimum Payoffs}}{\text{Maximum Payoffs} - \text{Minimum Payoffs}} \\ &= \frac{(\pi_i + \pi_j) - \min_{\mathbf{e}}\{\pi_i + \pi_j\}}{\max_{\mathbf{e}}\{\pi_i + \pi_j\} - \min_{\mathbf{e}}\{\pi_i + \pi_j\}}. \end{aligned} \tag{5.5.1}$$

The lowest efficiency is realized if one player chooses the maximum effort while the other player chooses the minimum effort. Highest efficiency is realized if both players choose the maximum effort. We will report efficiency in percentage values by multiplying equation (5.5.1) by 100%.

Regarding the one-shot decisions, treatment LU77 achieves 71.9% coordination which is only marginally larger than and not statistically distinguishable from the 68.8% in treatment LU11, based on Fisher's exact test. Increasing strategic uncertainty in HU77 does not have an effect on successful coordination (75.0%) compared to LU77. However, coordination rates collapse when (1,1) is equality-dominant and strategic uncertainty is high. Treatment HU11 only achieves 25.0% coordination. This 64% drop from LU11 is both economically large and statistically significant (Fisher's exact test, $p = 0.000$). In terms of efficiency, treatment LU77 also achieves the highest level of 89.8% which is not different from the 86.2% in LU11 (U-test, $p = 0.6032$). As with coordination, efficiency is not affected by increased strategic uncertainty in HU77 where subjects realize 85.9%. However, efficiency deteriorates if (1,1) is equality-dominant and strategic uncertainty is high. Average efficiency drops by around 41% to 51.1% in HU11 (U-test, $p = 0.0000$).



Notes: Reported is the frequency of coordination over periods separated by treatments. $N = 256$. *Coordination* is achieved if both players simultaneously choose the same effort level. The gray area represents no successful coordination, i.e., both players choose different effort levels. The solid line represents coordination on the secure equilibrium (1,1) and the dashed line represents coordination on the Pareto-dominant equilibrium (7,7). *77* indicates that the Pareto-dominant equilibrium (7,7) is equality-dominant and *11* indicates that the secure equilibrium (1,1) is equality-dominant. *LU* refers to low strategic uncertainty and *HU* refers to high strategic uncertainty.

Figure 5.3: Frequency of Coordination across Treatments.

Figure 5.3 shows the frequency of coordination on either equilibrium (1,1) or equilibrium (7,7) and the frequency of no successful coordination for the repeated data over the course of the experiment. We generally observe a high level of coordination in LU77. In particular, if they coordinate successfully, groups in LU77 coordinate on the Pareto-efficient equilibrium (7,7). In LU11 subjects also mainly coordinate on the Pareto-dominant equilibrium but are less successful as groups often miscoordinate. Collapsing the data on the cohort level, we find that coordination rates drop significantly in treatment LU11. Both the frequency of coordination on (7,7) drops from 90.0% in LU77 to 66.0% in LU11 (U-test, $p = 0.0306$) and the frequency of miscoordination rises from 10.0% to 32.0% (U-test, $p = 0.0305$). In addition, the frequency of coordination on the secure equilibrium (1,1) rises from 0.0% to 1.8% which is statistically significant (U-test, $p = 0.0645$) but economically small. In terms of efficiency, subjects in treatment LU77 realize nearly perfect results with an average of 96.9% across all periods. This is significantly more than the 81.8% in treatment LU11 (U-test, $p = 0.0156$).

The high uncertainty treatments show that the prevalent interaction between equality dominance and strategic uncertainty also affects coordination rates. Increasing strategic uncertainty has no significant effect on coordination if (7,7) is equality-dominant; neither for the frequency to coordinate on either (7,7) or (1,1) nor on the miscoordination rate. However, if (1,1) is equality-dominant, coordination on (7,7) drops to 23.1% if strategic uncertainty is high (U-test, $p = 0.0208$) while there is no statistical difference in the frequency of miscoordination which averages 32.1% in LU11 and 34.4% in HU11. Coordination on (1,1) gets more common, increasing from an average of 1.8% in LU11 to 36.8% in HU11 (U-test, $p = 0.0548$). Subjects in treatment HU77 realize an average efficiency of 87.3% which is not statistically different from LU77. Compared to treatment LU11, treatment HU11 features a large drop in efficiency which falls by around 33% to 54.6% (U-test, $p = 0.0157$).[14]

Result 10 *[Coordination and Efficiency] Coordination rates and efficiency are significantly higher if (7,7) is equality-dominant than if (1,1) is equality-dominant. This holds both for overall coordination rates and for coordination on the Pareto-dominant equilibrium (7,7). Strategic uncertainty has no effect if (7,7) is equality-dominant but coordination rates and efficiency are lower if (1,1) is equality-dominant and strongly deteriorate when strategic uncertainty is also high.*

5.6 Conclusion

We analyze the effect of payoff inequality in a minimum-effort game with Pareto-rankable equilibria. Making use of a social-preference model applied to the potential of the coordination game, we predict that subjects choose the maximum effort and coordinate on the efficient Pareto-dominant equilibrium if it is also equality-dominant. If the secure equilib-

[14]We complement the analyses on coordination outcomes and efficiency using parametric regressions in Appendix 5.D.2, confirming these findings.

rium is equality-dominant, we predict that subjects choose the minimum effort. We test this prediction in a laboratory experiment with both one-shot and repeated interactions.

We find that inequality aversion plays an important role in equilibrium selection. When the Pareto-dominant equilibrium is also equality-dominant, subjects coordinate on the maximum effort rapidly. This high coordination outcome is robust against increasing strategic uncertainty. This is remarkable because subjects could actually lose money from miscoordination in the respective treatment, suggesting that they are very certain about their opponent's behavior if the Pareto-dominant is the only equilibrium with equal payoffs. If the secure equilibrium is equality-dominant, coordination success worsens. In this case, we find a large interaction effect of strategic uncertainty and equality dominance resulting in coordination on the least-efficient equilibrium in many cases. Our experiment suggests that an equal-payoff Pareto-dominant equilibrium can result in substantial efficiency gains.

Our results have implications for the economic design of institutions such as banking regulation. In financial coordination settings, agents often have heterogeneous risks depending on their investment and how diversified their portfolio is. Bank runs are a prime example for this heterogeneity and deposit insurance is the most-prevalent policy intervention. In case of a run, small depositors would thus lose a substantial fraction of their wealth from miscoordination relative to large diversified depositors. Deposit insurance is one mean to protect small depositors from the adverse effects of bank runs. There are two effects of deposit insurances. First, the insurance reduces inequality in the run equilibrium because only deposits exceeding the insurance level are at risk. Second, common knowledge of such a mechanism should prevent larger depositors from withdrawing because they know that small depositors will not withdraw whatsoever. Hence, under deposit insurance, small investors still have a larger benefit from higher coordination but now their risk is greatly reduced which in turn reduces the strategic uncertainty faced by large depositors.

However, before the financial crises starting in 2007, bank deposits were only secured up to around 20,000 EUR in the EU and up to 100,000 USD in the US (CESifo 2011). Such an insurance creates inequality for large depositors who at the same time benefit little from the interest payment. In 2008, the fourth-largest US bank in terms of assets was Wachovia (Alvarez 2010). This bank experienced a so-called "silent run" of large investors who withdrew around five billion USD to bring their account balance below the level covered by the insurance.[15] As a consequence, regulators urged the sale of Wachovia which was eventually bought by Wells Fargo. In the aftermath of this and other severe banking panics, the European Union raised the deposit insurance to 100,000 EUR and the

[15] See Rothacker (2008) and Stevenson and Slater (2008).

US increased it to 250,000 USD.[16] In addition, in 2008 and 2010, respectively, Germany and Ireland declared all deposits to be safe to prevent massive capital flights.

Our results indicate the chances and risks of such a policy intervention. On the one hand, the general argument for an increase in the insurance level is the reduction in strategic uncertainty. Our results reinforce this logic. Higher deposit insurance reduces the losses from miscoordination and thus strategic uncertainty among depositors. On the other hand, we also point out the risk of such a change because higher insurance coverage leads to less inequality in the run equilibrium. As outcomes are generally worse across all dimensions if the secure equilibrium has equal payoffs, such a policy change may actually weaken the systemic stability of the banking sector. However, our results regarding the interaction effect suggest that the net effect is positive and thus the no-run equilibrium is fortified by an increase in deposit insurance.

[16] See EUC (2010) and FDIC (2010).

5.A Theoretical Appendix

5.A.1 Proof of Lemma 1

In this section, we construct the equilibrium potential for game aMEG-SP. Note that we only have to consider equilibria because we search for the arg max set of the potential which is by construction of game aMEG only maximized if $e_1 = e_2 = \ldots = e_n$ (Goeree and Holt 2005). Subsequently, we derive the equilibrium threshold stated in Lemma 1. We will construct the equilibrium potential and hence the threshold values for both values of τ separately.

Case 1: Secure Equilibrium is Equality-Dominant

In this case, $\tau = \underline{\tau}$, and the equilibrium utility is given by

$$u_i(\mathbf{e}^*; \theta, \underline{\tau}, \alpha_i, \beta_i) = be - \begin{cases} c_l e + a + c_l - \beta_i \frac{1}{n-1} n_h (c_h - c_l)(e-1) & \text{if } \theta = l \\ c_h e + a + c_h - \alpha_i \frac{1}{n-1} n_l (c_h - c_l)(e-1) & \text{if } \theta = h. \end{cases}$$

yielding the equilibrium Potential

$$\begin{aligned} P^{\text{aMEG-SP}}(\underline{\tau}) = be - n_l c_l e - n_h c_h e - \sum_{i \in N_l} \beta_i \frac{1}{n-1} n_h (c_h - c_l)(e-1) \\ - \sum_{i \in N_h} \alpha_i \frac{1}{n-1} n_l (c_h - c_l)(e-1) \end{aligned}$$

and thus the equilibrium threshold benefit is given by

$$b^{\text{aMEG-SP}}(\underline{\tau}) = n_l c_l + n_h c_h + \sum_{i \in N_l} \beta_i \frac{n_h}{n-1}(c_h - c_l) + \sum_{i \in N_h} \alpha_i \frac{n_l}{n-1}(c_h - c_l). \tag{5.A.1}$$

Case 2: Pareto-Dominant Equilibrium is Equality-Dominant

In this case, $\tau = \overline{\tau}$, and the equilibrium utility is given by

$$u_i(\mathbf{e}^*; \theta, \overline{\tau}, \alpha_i, \beta_i) = be - \begin{cases} c_l e + a + c_l - d - \alpha_i \frac{1}{n-1} n_h (c_h - c_l)(|E_i| - e) & \text{if } \theta = l \\ c_h e + a + c_h + d - \beta_i \frac{1}{n-1} n_l (c_h - c_l)(|E_i| - e) & \text{if } \theta = h. \end{cases}$$

yielding the equilibrium Potential

$$\begin{aligned} P^{\text{aMEG-SP}}(\overline{\tau}) = be - n_l c_l e - n_h c_h e - \sum_{i \in N_l} \alpha_i \frac{1}{n-1} n_h (c_h - c_l)(|E_i| - e) \\ - \sum_{i \in N_h} \beta_i \frac{1}{n-1} n_l (c_h - c_l)(|E_i| - e) \end{aligned}$$

and thus the equilibrium threshold benefit is given by

$$b^{\text{aMEG-SP}}(\overline{\tau}) = n_l c_l + n_h c_h - \sum_{i \in N_l} \alpha_i \frac{n_h}{n-1}(c_h - c_l) - \sum_{i \in N_h} \beta_i \frac{n_l}{n-1}(c_h - c_l). \tag{5.A.2}$$

The combination of (5.A.1) and (5.A.2) yields the expression stated in Lemma 1. □

5.A.2 Normal Form Representations

Number of X	Role	Number of Y: 7	6	5	4	3	2	1
7	X	103	91	79	67	55	43	31
	Y	103	102	101	100	99	98	97
6	X	92	92	80	68	56	44	32
	Y	91	102	101	100	99	98	97
5	X	81	81	81	69	57	45	33
	Y	79	90	101	100	99	98	97
4	X	70	70	70	70	58	46	34
	Y	67	78	89	100	99	98	97
3	X	59	59	59	59	59	47	35
	Y	55	66	77	88	99	98	97
2	X	48	48	48	48	48	48	36
	Y	43	54	65	76	87	98	97
1	X	37	37	37	37	37	37	37
	Y	31	42	53	64	75	86	97

Notes: Reported is the normal-form representation of treatment LU77. In this treatment, the Pareto-dominant equilibrium (7,7) is equality-dominant and strategic uncertainty is low.

Figure 5.A.4: Normal Form of LU77.

Number of X	Role	Number of Y: 7	6	5	4	3	2	1
7	X	133	121	109	97	85	73	61
	Y	73	72	71	70	69	68	67
6	X	122	122	110	98	86	74	62
	Y	61	72	71	70	69	68	67
5	X	111	111	111	99	87	75	63
	Y	49	60	71	70	69	68	67
4	X	100	100	100	100	88	76	64
	Y	37	48	59	70	69	68	67
3	X	89	89	89	89	89	77	65
	Y	25	36	47	58	69	68	67
2	X	78	78	78	78	78	78	66
	Y	13	24	35	46	57	68	67
1	X	67	67	67	67	67	67	67
	Y	1	12	23	34	45	56	67

Notes: Reported is the normal-form representation of treatment LU11. In this treatment, the secure equilibrium (1,1) is equality-dominant and strategic uncertainty is low.

Figure 5.A.5: Normal Form of LU11.

		Number of Y						
Number of X	**Role**	**7**	**6**	**5**	**4**	**3**	**2**	**1**
7	X	103	81	59	37	15	-7	-29
	Y	103	102	101	100	99	98	97
6	X	92	92	70	48	26	4	-18
	Y	91	102	101	100	99	98	97
5	X	81	81	81	59	37	15	-7
	Y	79	90	101	100	99	98	97
4	X	70	70	70	70	48	26	4
	Y	67	78	89	100	99	98	97
3	X	59	59	59	59	59	37	15
	Y	55	66	77	88	99	98	97
2	X	48	48	48	48	48	48	26
	Y	43	54	65	76	87	98	97
1	X	37	37	37	37	37	37	37
	Y	31	42	53	64	75	86	97

Notes: Reported is the normal-form representation of treatment HU77. In this treatment, the Pareto-dominant equilibrium (7,7) is equality-dominant and strategic uncertainty is high.

Figure 5.A.6: Normal Form of HU77.

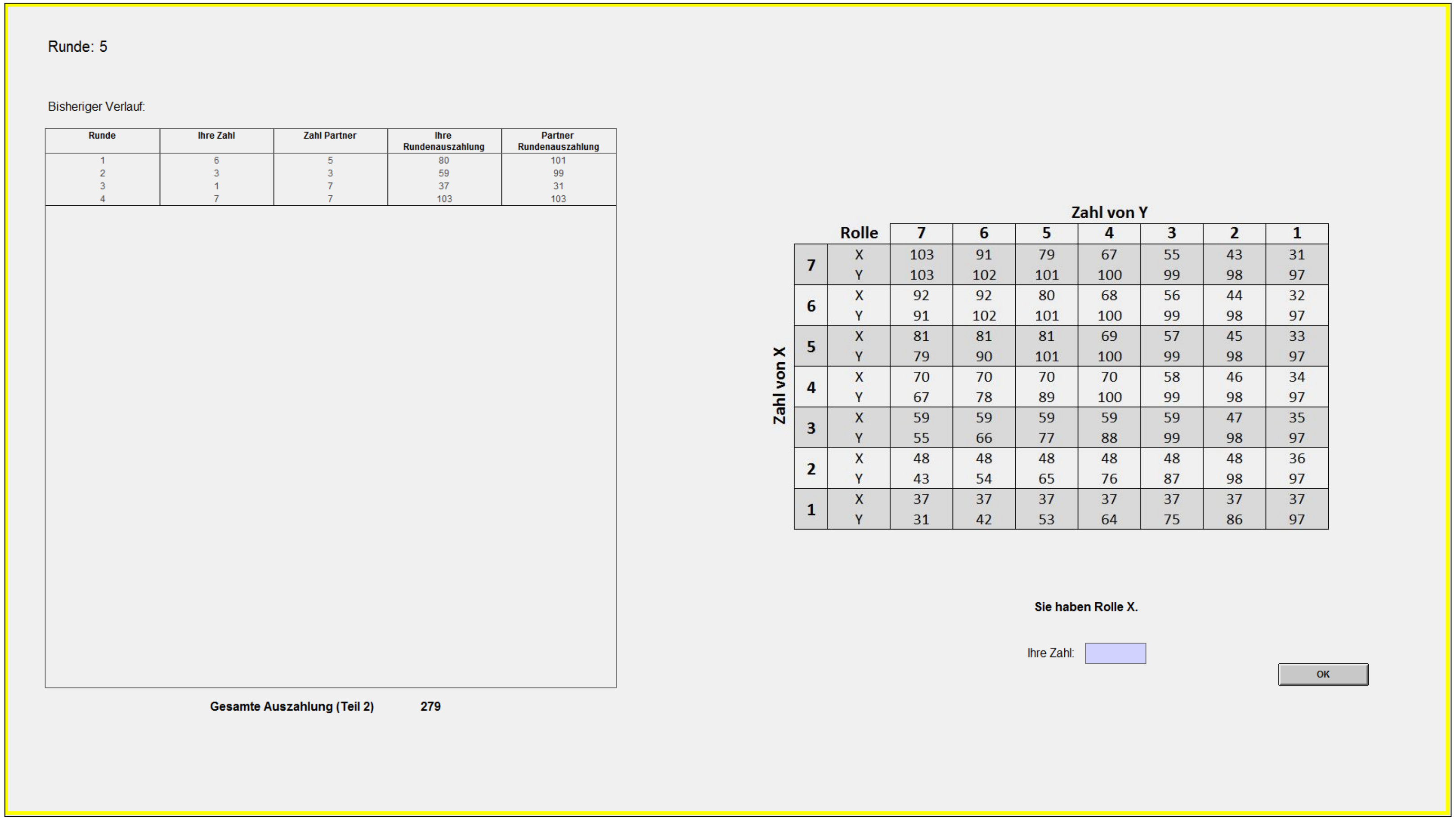
		Number of Y						
Number of X	**Role**	**7**	**6**	**5**	**4**	**3**	**2**	**1**
7	X	133	111	89	67	45	23	1
	Y	73	72	71	70	69	68	67
6	X	122	122	100	78	56	34	12
	Y	61	72	71	70	69	68	67
5	X	111	111	111	89	67	45	23
	Y	49	60	71	70	69	68	67
4	X	100	100	100	100	78	56	34
	Y	37	48	59	70	69	68	67
3	X	89	89	89	89	89	67	45
	Y	25	36	47	58	69	68	67
2	X	78	78	78	78	78	78	56
	Y	13	24	35	46	57	68	67
1	X	67	67	67	67	67	67	67
	Y	1	12	23	34	45	56	67

Notes: Reported is the normal-form representation of treatment HU11. In this treatment, the secure equilibrium (1,1) is equality-dominant and strategic uncertainty is high.

Figure 5.A.7: Normal Form of HU11.

5.B Screens in the Lab Experiment

This section shows the computer screens in the repeated game. The one-shot game has a similar interface but without the history of play. In the one-shot game, subjects received information about their own and their partner's choice only at the very end of the experiment (after the repeated game was completed). In the repeated game, subjects received information at the end of each period. For more information see Section 5.4.2.



Runde: 5

Bisheriger Verlauf:

Runde	Ihre Zahl	Zahl Partner	Ihre Rundenauszahlung	Partner Rundenauszahlung
1	6	5	80	101
2	3	3	59	99
3	1	7	37	31
4	7	7	103	103

Gesamte Auszahlung (Teil 2) 279

Zahl von Y

Zahl von X	Rolle	7	6	5	4	3	2	1
7	X	103	91	79	67	55	43	31
	Y	103	102	101	100	99	98	97
6	X	92	92	80	68	56	44	32
	Y	91	102	101	100	99	98	97
5	X	81	81	81	69	57	45	33
	Y	79	90	101	100	99	98	97
4	X	70	70	70	70	58	46	34
	Y	67	78	89	100	99	98	97
3	X	59	59	59	59	59	47	35
	Y	55	66	77	88	99	98	97
2	X	48	48	48	48	48	48	36
	Y	43	54	65	76	87	98	97
1	X	37	37	37	37	37	37	37
	Y	31	42	53	64	75	86	97

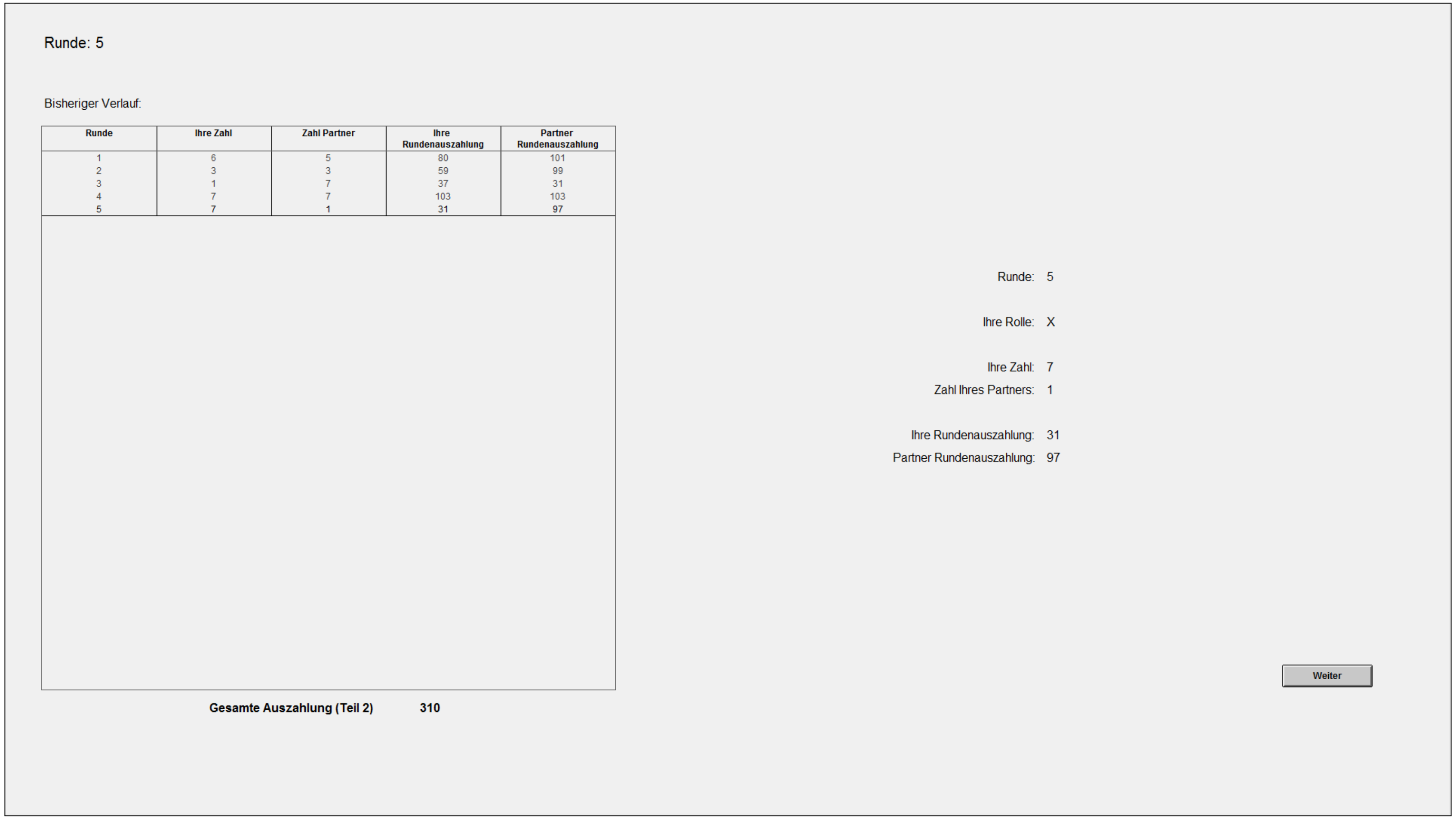
Sie haben Rolle X.

Ihre Zahl:

OK

Notes: Displayed is the input interface in the repeated game. On the left side, subjects see a history of play up to but not including the current period. Reported is the period number, subject's own number, the number that the partner in that period chose, the subject's payoff, and the payoff of the partner in that period. In addition, the subject sees her cumulative payoff. On the right side, the screen displayed the normal form of the game and the subject's role. The period choice was input to a text field and was confirmed by clicking the "OK" button. Screen size and resolution: 23", Full HD 1920x1080.

Figure 5.B.8: Input Screen in the Repeated Game.



Notes: Displayed is the input interface in the repeated game. On the left side, subjects see a history of play up to and including the current period. Reported is the period number, subject's own number, the number that the partner in that period chose, the subject's payoff, and the payoff of the partner in that period. In addition, the subject sees her cumulative payoff. On the right side, the screen displays the curent period's information: the period number, the subject's role, the subject's own number, the number that the partner in the current period chose, the subject's payoff, and the payoff of the partner in the current period. Screen size and resolution: 23", Full HD 1920x1080.

Figure 5.B.9: Input Screen in the Repeated Game.

5.C Instructions

The following pages report the instructions in both German (original) and English (translated). Subjects first received the general instructions along with the instructions of part 1 and the according payoff table. Only the payoff table varied by treatment. The respective payoff tables were given on a separate sheet (referred to as "Figure 1") and are identical to the normal form representations (see Section 5.A.2). After part 1 was finished, subjects received the instructions for part two. All instructions were read out loud.

Allgemeine Instruktionen

Herzlich willkommen und vielen Dank für Ihre Teilnahme an diesem Experiment. Bitte kommunizieren Sie ab sofort und bis zum Ende des Experiments nicht mehr mit den anderen Teilnehmern.

Wir bitten Sie, die Instruktionen aufmerksam zu lesen. Die Instruktionen sind für alle Teilnehmer in diesem Raum identisch. Wenn Sie nach dem Lesen oder während des Experiments noch Fragen haben, heben Sie bitte Ihre Hand. Einer der Experimentleiter wird dann zu Ihnen kommen und Ihre Frage persönlich beantworten. Ihre Auszahlung und Ihre Entscheidungen werden vertraulich behandelt.

Sie erhalten für Ihr Erscheinen eine Teilnahmepauschale in Höhe von 2,50 EUR. Zusätzlich können Sie in diesem Experiment Geld verdienen. Wie viel Sie verdienen, hängt sowohl von Ihren Entscheidungen als auch den Entscheidungen anderer Teilnehmer ab. Ihre Auszahlungen werden im Laufe des Experiments in virtuellen Geldeinheiten, den Experimental Currency Units (ECU), angegeben**.** Ihre Auszahlung wird nach dem Ende des Experimentes in Euro (EUR) umgerechnet und in bar an Sie ausgezahlt.

Das Experiment besteht aus zwei Teilen (Teil 1 und Teil 2). Sie erhalten zunächst die Instruktionen für Teil 1. Nach Abschluss von Teil 1 erhalten Sie die Instruktionen für Teil 2. Danach bitten wir Sie einen Fragebogen auszufüllen. Ihre Auszahlung, Ihre Entscheidungen und Ihre Antworten im Fragebogen werden vertraulich behandelt.

Instruktionen Teil 1

Übersicht

- Teil 1 besteht aus einer einzigen Entscheidung.
- Es gibt zwei Rollen: X und Y. Die Hälfte der Teilnehmer hat Rolle X und die andere Hälfte hat Rolle Y. Es wird zu Beginn des Experiments zufällig bestimmt, ob Sie Rolle X oder Rolle Y haben.
- Sie bilden eine Gruppe mit einem Partner, der die jeweils andere Rolle hat: Wenn Sie Rolle X haben, hat ihr Partner Rolle Y. Wenn Sie Rolle Y haben, hat ihr Partner Rolle X.
- 30 ECU entsprechen 1 EUR.

Ablauf

- Sowohl Sie als auch Ihr Partner wählen eine Zahl aus der Menge {1, 2, 3, 4, 5, 6, 7}.
- Die Höhe der Auszahlungen hängt ab von:
    - Ihrer gewählten Zahl.
    - Der Zahl Ihres Partners.
    - Ihrer Rolle (X oder Y).
- Tabelle 1 zeigt die möglichen Auszahlungen. Die Entscheidung der Person in Rolle X ist hier in den Zeilen abgetragen, während die Entscheidung der Person in Rolle Y in den Spalten abgetragen ist. Die Entscheidungen beider Personen ergeben das Feld in der Tabelle, in dem Sie die Auszahlungen ablesen können. Die Auszahlung von X steht jeweils im oberen Teil eines Feldes und die Auszahlung von Y steht im unteren Teil.

Informationen

Das Ergebnis des ersten Teils erfahren Sie nach dem Ende des zweiten Teils des Experiments. Sie erhalten dabei folgende Informationen: Ihre gewählte Zahl, die Zahl Ihres Partners, Ihre Auszahlung und die Auszahlung Ihres Partners.

Instruktionen Teil 2

Übersicht

- Teil 2 besteht aus 30 Runden, in denen Sie jeweils eine Entscheidung treffen.
- Jede Runde hat den gleichen Ablauf wie Teil 1 und auch die Entscheidungssituation ist die gleiche, die in Tabelle 1 dargestellt ist.
- Sie haben die gleiche Rolle wie in Teil 1 (X oder Y). Diese Rolle behalten Sie für alle 30 Runden.
- Sie bilden in jeder Runde eine Gruppe mit einem Partner, der die jeweils andere Rolle hat: Wenn Sie Rolle X haben, hat ihr Partner immer Rolle Y. Wenn Sie Rolle Y haben, hat ihr Partner immer Rolle X.
- In jeder Runde wird Ihnen zufällig ein Partner aus diesem Raum neu zugeordnet mit dem Sie in der jeweiligen Runde eine Gruppe bilden.
- 400 ECU entsprechen in diesem Teil 1 EUR. Ihre Auszahlungen aus allen Runden werden am Ende für Ihre Endauszahlung zusammengerechnet.

Informationen

- Nach jeder Runde erhalten Sie folgende Informationen: Ihre gewählte Zahl, die Zahl Ihres Partners, Ihre Rundenauszahlung und die Rundenauszahlung Ihres Partners.
- Zusätzlich zeigen wir Ihnen Ihre vorherigen Entscheidungen, die Entscheidungen Ihrer vorherigen Partner und Ihre gesamte bisherige Auszahlung in Teil 2 an.

Endauszahlung

Am Ende des Experiments zahlen wir Ihnen Teil 1 und Teil 2 in bar aus.

Tabelle 1: Auszahlungen

Zahl von X	Rolle	Zahl von Y: 7	6	5	4	3	2	1
7	X	103	91	79	67	55	43	31
	Y	103	102	101	100	99	98	97
6	X	92	92	80	68	56	44	32
	Y	91	102	101	100	99	98	97
5	X	81	81	81	69	57	45	33
	Y	79	90	101	100	99	98	97
4	X	70	70	70	70	58	46	34
	Y	67	78	89	100	99	98	97
3	X	59	59	59	59	59	47	35
	Y	55	66	77	88	99	98	97
2	X	48	48	48	48	48	48	36
	Y	43	54	65	76	87	98	97
1	X	37	37	37	37	37	37	37
	Y	31	42	53	64	75	86	97

General Instructions

Welcome and thank you for participating in this experiment. From now on until the end of the experiment, please refrain from communicating with other participants.

We kindly ask you to read the instructions thoroughly. The instructions are identical for all participants in this room. If you have any questions after reading the instructions or during the experiment, please raise your hand. One of the instructors will then come to you and answer your question in person. Your payment and your decisions will be treated confidentially.

You will receive a show-up fee of 2,50€. Additionally, you can earn money in this experiment. How much you earn depends on your decisions as well as on the decisions of other participants. During the experiment your payoff will be calculated in a virtual currency: Experimental Currency Units (ECU). After the experiment, your payoff will be converted into Euros (EUR) and given to you in cash.

The experiment consists of two parts (part 1 and part 2). First, you will receive the instructions for part 1. After the completion of part 1, you will receive the instruction for part 2. Next, we ask you to complete a questionnaire. Your payoff, your decisions and the answers in the questionnaire will be treated confidentially.

Instructions Part 1

Overview

- Part 1 consists of a single decision.
- There are two roles: X and Y. Half of the participants take on role X and the other half takes on role Y. It will be randomly determined at the beginning of the experiment, whether you take on role X or role Y.
- You will form a group with one partner, who takes on the respective other role: If you take on role X, your partner takes on role Y. If you take on role Y, your partner takes on role X.
- 30 ECU correspond to 1 EUR.

Procedure

- You and your partner both choose a number out of the set {1, 2, 3, 4, 5, 6, 7}.
- You payoff is determined by:
    - Your chosen number.
    - The number of your partner.
    - Your role (X or Y)
- Table 1 shows the possible payoffs. The decision of the participant with role X is displayed in the rows and that of the participant with role Y in the columns. The decisions of both participants result in the cell of the table where you can find the payoffs. The payoff of X is displayed in the upper part of the cell, whereas the payoff of Y is displayed in the lower part.

Information

You will learn the results of the first part of the experiment at the end of the second part. Then, you will receive the following information: Your chosen number, the number of your partner, your payoff, and your partner's payoff.

Instructions Part 2

Overview

- Part 2 consists of 30 rounds. In each round you will make a decision.
- Each round follows the same course as in part 1 and the decision situation is the same as displayed in Table 1.
- You take on the same role as in part 1 (X or Y). You will keep this role for all 30 rounds.
- You will form a group with one partner who takes on the respective other role: If you take on role X, your partner always takes on role Y. If you take on role Y, your partner always takes on role X.
- In each round, you will be randomly assigned a new partner from this room with whom you will form a group.
- 400 ECU correspond to 1 EUR in this part. Your payoff from all rounds will be converted and summed up at the end.

Information

- At the end of each round, you will receive the following information: Your chosen number, the number of your partner, your payoff in this round and your partner's payoff in this round.
- In addition, we will display your previous decision, your previous-partners' decisions, and your total payoff of part 2 up to that point.

Final Payoff

At the end of the experiment, you will receive your payment for part 1 and part 2 in cash.

5.D Empirical Appendix

5.D.1 Summary Statistics

Table 5.D.2 reports summary statistics for the two main treatments, LU77 and LU11, as well as for the two robustness treatments, HU77 and HU11. Panel A reports demographics. *Age* reports the age of the participant. *Male* is a dummy variable taking the value one for male participants and zero for female participants. *German Native* is a dummy variable indicating whether a participant's mother tongue is German or not. If a participant indicated multiple mother languages including German, she is also considered German native.

Because coordination games feature strategic risk by construction and, in addition, payoffs in our game are generally unequal, we ask one question on risk preferences and one question on social preferences regarding income inequality. Panel B reports these attitudes. *Risk-Averse* is a dummy indicating whether the participant was classified as risk-averse, *Risk-Neutral* is a dummy indicating whether the participants was classified as risk-neutral, and *Risk-Seeking* is a dummy indicating whether the participant was classified as risk-seeking.[17] *Social Preference* takes values from one to ten where one indicates that income should be made more equal and ten indicates that income differences need to be larger as incentives for individual effort.[18]

[17]Risk preferences were obtained by asking participants to choose between a certain payoff of $50 and a gamble that pays with equal probability either $100 or $0. Because the gamble is a mean-preserving spread of the certain payoff, we classify subjects as being *risk-averse* if they indicate to prefer the certain outcome, as *risk-neutral* if they indicate to be indifferent, and as *risk-seeking* if they indicate to prefer the gamble.

[18]The social preference question is taken from the World Values Survey (WVS), Wave 6 (See `www.worldvaluessurvey.org`): "How would you place your views on this scale? 1 means you agree completely with the statement on the left; 10 means you agree completely with the statement on the right; and if your views fall somewhere in between, you can choose any number in between. [1] Incomes should be made more equal, ..., [10] We need larger income differences as incentives for individual effort." In the questionnaire, we used the translation provided in the German version of the WVS.

Table 5.D.2: Summary Statistics by Treatment: Demographics and Attitudinal Variables.

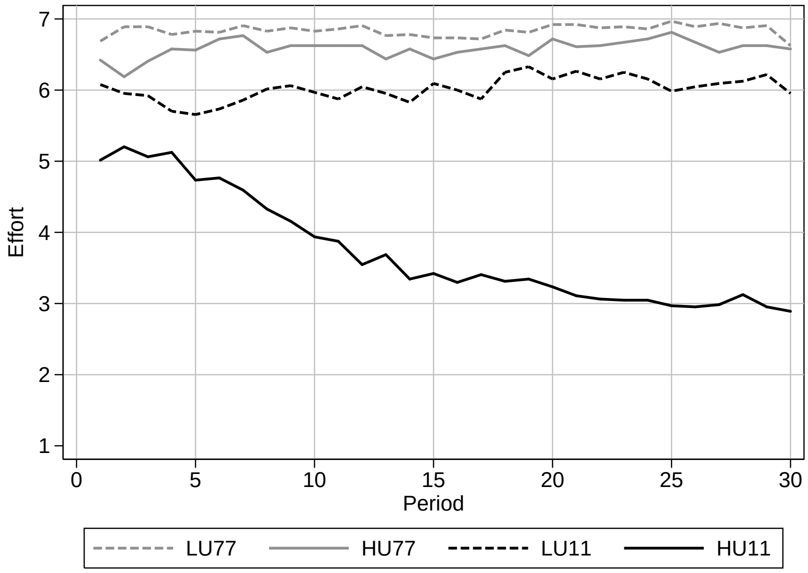
Treatments	LU77	LU11	HU77	HU11	Total
Panel A. Demographics					
Age	23.86	23.48	24.45	24.08	23.97
	(5.65)	(3.90)	(3.17)	(7.56)	(5.33)
Male	35.9%	50.0%	51.6%	45.3%	45.7%
German Native	53.1%	76.6%	70.3%	67.2%	66.8%
Panel B. Attitudes					
Risk-Averse	75.0%	85.9%	84.4%	76.6%	80.5%
Risk-Neutral	12.5%	6.25%	6.25%	1.56%	6.64%
Risk-Seeking	12.5%	7.81%	9.38%	21.9%	12.9%
Social Preference	4.25	4.25	3.98	5.25	4.43
	(2.64)	(2.14)	(2.24)	(2.31)	(2.38)

Notes: Reported is the mean or the share of demographic and attitudinal variables by treatment. Standard deviations in parentheses. $N = 256$. All information were surveyed through a post-experimental questionnaire.

5.D.2 Further Analyses

Effort choices over time

Figure 5.D.10 shows the mean effort level over all periods for each treatment. As one can see, the interaction between strategic uncertainty and equality dominance increases over time. While average effort levels remain fairly constant in LU77 and HU77, the gap between LU11 and HU11 widens.



Notes: Reported are the average effort levels over periods separated by treatment. $N = 256$. *77* indicates that the Pareto-dominant equilibrium (7,7) is equality-dominant and *11* indicates that the secure equilibrium (1,1) is equality-dominant. *LU* refers to low strategic uncertainty and *HU* refers to high strategic uncertainty.

Figure 5.D.10: Average Effort across Treatments.

Coordination outcomes

We further analyze coordination outcomes by logistic regressions of the dummy *Coordination* that takes the value one if $e_i = e_j$ and zero if $e_i \neq e_j$. Each period, two matched subjects generate one observation. Hence, we have 3840 observations when we utilize all 30 periods and 640 observations when we analyze the last five periods. Table 5.D.3 reports the results. Coordination is significantly hampered if (1,1) is equality-dominant. In line with the results regarding effort choice, we do not find any direct effect of increased strategic uncertainty on the ability to coordinate. In contrast to the analysis of effort, the interaction between strategic uncertainty and equality dominance is also not significant. The period coefficient indicates that subjects learn to better coordinate over time.

Table 5.D.3: Repeated-Interaction Coordination: Logistic Regressions.

	All Periods		**Last Five Periods**	
Model	(I)	(II)	(III)	(IV)
Estimation	Logistic	Logistic	Logistic	Logistic
ED11	-2.003***	-1.476**	-1.639**	-1.264**
	(0.416)	(0.644)	(0.649)	(0.552)
High SU	-0.314	-0.348	-0.020	-0.308
	(0.433)	(0.782)	(0.704)	(0.700)
ED11 X High SU	0.017	0.242	0.618	0.645
	(0.579)	(0.904)	(0.929)	(0.966)
Period	0.052***	0.042***	-0.176*	-0.120**
	(0.005)	(0.013)	(0.091)	(0.056)
Constant	2.304***	1.602***	8.058***	5.561***
	(0.324)	(0.546)	(2.626)	(1.657)
Observations	3840	3840	640	640
Subject Pairs	128	128	128	128
Cohorts	32	32	32	32
Random Effects	Yes	No	Yes	No
Clustered S.E.	No	Yes	No	Yes
Pseudo R2	0.04	0.09	0.03	0.04

Notes: Logistic regressions. Dependent variable is coordination in perecentages. Coordination is achieved if both players choose the same effort level. Data is collapsed for every two subjects that are partners in a given period. Models (I) and (II) are based on data from all periods. Models (IV) and (V) are based on data from the last five periods. Standard errors in parentheses. If applicable, standard errors are clustered on the cohort level. Random effects are on subject pairs. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

Efficiency

Table 5.D.4 presents the results of random-effects regression of efficiency on the treatment indicators confirming the non-parametric results. If (1,1) is equality-dominant, efficiency significantly drops by about 15% points while strategic uncertainty has no significant main effect. The interaction between equality dominance and strategic uncertainty is also significant and increases in magnitude for the last five periods. The effect of Period is significant but economically small.

Table 5.D.4: Repeated-Interaction Efficiency: Random-Effects Regressions.

	All Periods	**Last Five Periods**
Model	(I)	(II)
Estimation	GLS	GLS
ED11	-15.155***	-13.392**
	(5.013)	(5.637)
High SU	-9.580	-9.706
	(6.741)	(6.354)
ED11 X High SU	-17.600*	-20.439**
	(10.555)	(10.025)
Period	0.137*	-0.956**
	(0.080)	(0.422)
Constant	94.783***	123.800***
	(2.030)	(11.854)
Observations	3840	640
Subjects Pairs	128	128
Cohorts	32	32
R2	0.25	0.29

Notes: Reported are random-effects GLS regressions with random effect on subject. Dependent variable is efficiency in percentages. Efficiency is defined as the difference between the sum of actual payoffs and the minimum sum of payoffs divided by the difference between the maximum sum of payoffs and the minimum sum of payoffs. Model (I) is based on data from all periods. Model (II) is based on data from the last five periods. *ED11* indicates that (1,1) is equality-dominant. *High SU* indicates high strategic uncertainty. Standard errors clustered on the cohort level in parentheses. $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$.

BIBLIOGRAPHY

Abdellaoui, M., 2000. Parameter-free elicitation of utility and probability weighting functions. Management Science 46 (11), 1497–1512.

Abdellaoui, M., Baillon, A., Placido, L., Wakker, P. P., 2011a. The rich domain of uncertainty: Source functions and their experimental implementation. American Economic Review 101 (2), 695–723.

Abdellaoui, M., Bleichrodt, H., L'Haridon, O., 2008. A tractable method to measure utility and loss aversion under prospect theory. Journal of Risk and Uncertainty 36 (3), 245–266.

Abdellaoui, M., Bleichrodt, H., Paraschiv, C., 2007. Loss aversion under prospect theory: A parameter-free measurement. Management Science 53 (10), 1659–1674.

Abdellaoui, M., Driouchi, A., L'Haridon, O., 2011b. Risk aversion elicitation: reconciling tractability and bias minimization. Theory and Decision 71 (1), 63–80.

Abdellaoui, M., Vossmann, F., Weber, M., 2005. Choice-based elicitation and decomposition of decision weights for gains and losses under uncertainty. Management Science 51 (9), 1384–1399.

Abel, A. B., 1990. Asset prices under habit formation and catching up with the joneses. American Economic Review 80 (2), 38–42.

Acland, D., Levy, M., 2013. Naivete, projection bias, and habit formation in gym attendance. Working Paper.

Ahn, D., Choi, S., Gale, D., Kariv, S., 2011. Estimating ambiguity aversion in a portfolio choice experiment. Working Paper.

Allais, M., 1953. Le comportement de l'homme rationnel devant le risque: Critique des postulats et axiomes de l'ecole americaine. Econometrica 21 (4), 503–546.

Allingham, M. G., Sandmo, A., 1972. Income tax evasion: A theoretical analysis. Journal of Public Economics 1 (3-4), 323–338.

Alm, J., 2012. Measuring, explaining, and controlling tax evasion: Lessons from theory, experiments, and field studies. International Tax and Public Finance 19 (1), 54–77.

Alm, J., Jackson, B. R., McKee, M., 2009. Getting the word out: Enforcement information dissemination and compliance behavior. Journal of Public Economics 93 (3-4), 392–402.

Alm, J., Sennoga, E. B., 2010. Mobility, competition, and the distributional effects of tax evasion. National Tax Journal 63 (4), 1055–84.

Alm, J., Torgler, B., 2006. Culture differences and tax morale in the United States and in Europe. Journal of Economic Psychology 27 (2), 224–246.

Alvarez, S. G., 2010. The acquisition of Wachovia corporation by wells fargo & company. before the financial crisis inquiry commission, Washington, D.C.Http://www.federalreserve.gov/newsevents/testimony/alvarez20100901a.htm (Accessed: March 20, 2015).

Anderhub, V., Güth, W., Müller, W., Strobel, M., 2000. An experimental analysis of intertemporal allocation behavior. Experimental Economics 3, 137–152.

Anderson, S. P., Goeree, J. K., Holt, C. A., 2001. Minimum-effort coordination games: Stochastic potential and logit equilibrium. Games and Economic Behavior 34 (2), 177–199.

Andreoni, J., Sprenger, C., 2010. Certain and uncertain utility: The Allais paradox and five decision theory phenomena. Working Paper.

Anscombe, F. J., Aumann, R. J., 1963. A definition of subjective probability. Annals of mathematical statistics, 199–205.

Armantier, O., Treich, N., 2009. Subjective probabilities in games: An application to the overbidding puzzle. International Economic Review 50 (4), 1079–1102.

Arrow, K. J., 1965. Aspects of the Theory of Risk Bearing. Yrjo Johansson Saatio, Helsinki.

Augenblick, N., Niederle, M., Sprenger, C., 2013. Working over time: Dynamic inconsistency in real effort tasks. Quarterly Journal of Economics (forthcoming).

Baillon, A., 2008. Eliciting subjective probabilities through exchangeable events: An advantage and a limitation. Decision Analysis 5 (2), 76–87.

Ballinger, P. T., Palumbo, M. G., Wilcox, N. T., 2003. Precautionary saving and social learning across generations: An experiment. Economic Journal 113, 920–947.

Barberis, N., 2013. Thirty years of prospect theory in economics: A review and assessment. Journal of Economic Perspectives 27 (1), 173–196.

Barrett, S., 2006. The smallpox eradication game. Public Choice 130 (1/2), 179–207.

Beattie, J., Loomes, G., 1997. The impact of incentives upon risky choice experiments. Journal of Risk and Uncertainty 14 (2), 155–168.

Belica, M., Ehrhart, K.-M., 2013. Reference-dependent Bayesian games. Working Paper.

Benartzi, S., Thaler, R. H., 1995. Myopic loss aversion and the equity premium puzzle. Quarterly Journal of Economics 110 (1), 73–92.

Benitez-Silva, H., 2002. Labor supply flexibility and portfolio choice: An empirical analysis. In: Zame, D. K. L. W., Case, A., Fernandez, R., Rust, J., Wolpin, K. (Eds.), Proceedings of the 2002 North American Summer Meetings of the Econometric Society: Labor Economics and Applied Econometrics. North Holland.

Blanco, M., Engelmann, D., Normann, H. T., 2011. A within-subject analysis of other-regarding preferences. Games and Economic Behavior 72 (2), 321–338.

Blavatskyy, P., 2006. Error propagation in the elicitation of utility and probability weighting functions. Theory and Decision 60 (2-3), 315–334.

Bleichrodt, H., Cillo, A., Diecidue, E., 2010. A quantitative measurement of regret theory. Management Science 56 (1), 161–175.

Bleichrodt, H., Pinto, J. L., 2000. A parameter-free elicitation of the probability weighting function in medical decision analysis. Management Science 46 (11), 1485–1496.

Bodie, Z., Merton, R. C., Samuelson, W. F., 1992. Labor supply flexibility and portfolio choice in a life cycle model. Journal of Economic Dynamics and Control 16 (3-4), 427–449.

Bolton, G. E., Ockenfels, A., 2000. ERC: A theory of equity, reciprocity, and competition. American Economic Review 90 (1), 166–193.

Booij, A. S., Van de Kuilen, G., 2009. A parameter-free analysis of the utility of money for the general population under prospect theory. Journal of Economic Psychology 30 (4), 651–666.

Bose, S., Daripa, A., 2009. A dynamic mechanism and surplus extraction under ambiguity. Journal of Economic Theory 144 (5), 2084–2114.

Bossaerts, P., Ghirardato, P., Guarnaschelli, S., Zame, W. R., 2010. Ambiguity in asset markets: Theory and experiment. Review of Financial Studies 23 (4), 1325–1359.

Bossaerts, P., Plott, C., Zame, W. R., 2007. Prices and portfolio choices in financial markets: Theory, econometrics, experiments. Econometrica 75 (4), 993–1038.

Brandt, M. W., 2010. Portfolio choice problems. In: Ait-Sahalia, Y., Hansen, L. P. (Eds.), Handbook of Financial Econometrics: Tools and Techniques. Vol. 1 of Handbooks in Finance. North-Holland, Ch. 5, pp. 269 – 336.

Brandts, J., Cooper, D. J., 2006. A change would do you good.... An experimental study on how to overcome coordination failure in organizations. American Economic Review 96 (3), 669–693.

Bricker, J., Kennickell, A., Moore, K., Sabelhaus, J., 2012. Changes in U.S. family finances from 2007 to 2010: Evidence from the survey of consumer finances. Federal Reserve Bulletin 98 (2), 1–80.

Brown, A. L., Chua, Z. E., Camerer, C. F., 2009. Learning and visceral temptation in dynamic saving experiments. Quarterly Journal of Economics 124 (1), 197–231.

Brown, G. W., 1951. Iterative solution of games by fictitious play. Activity analysis of production and allocation 13 (1), 374–376.

Brunnermeier, M., Nagel, S., 2008. Do wealth fluctuations generate time-varying risk aversion? Micro-evidence on individuals' asset allocation. American Economic Review 98 (3), 713–736.

Cabrales, A., Garcia-Fontes, W., Motta, M., 2000. Risk dominance selects the leader: An experimental analysis. International Journal of Industrial Organization 18 (1), 137–162.

Cai, G. G., Wurman, P. R., Gong, X., 2010. A note on discrete bid first-price auction with general value distribution. International Game Theory Review 12 (1), 75–81.

Camerer, C., 2003. Behavioral game theory: Experiments in strategic interaction. Princeton University Press, Princeton, New Jersey.

Camerer, C. F., 1989. An experimental test of several generalized utility theories. Journal of Risk and Uncertainty 2 (1), 61–104.

Campbell, J. Y., Cochrane, J. H., 1999. By force of habit: A consumption-based explanation of aggregate stock market behavior. Journal of Political Economy 107 (2), 205–251.

Campbell, J. Y., Deaton, A., 1989. Why is consumption so smooth. Review of Economic Studies 56 (3), 357–374.

Carare, O., Rothkopf, M., 2005. Slow Dutch auctions. Management Science 51 (3), 365–373.

Carbone, E., Duffy, J., 2014. Lifecycle consumption plans, social learning and external habits: Experimental evidence. Journal of Economic Behavior & Organization 106, 413–427.

Carbone, E., Hey, J. D., 2004. The effect of unemployment on consumption: An experimental analysis. Economic Journal 114 (497), 660–683.

Carbone, E., Infante, G., 2014. Comparing behavior under risk and under ambiguity in a lifecycle experiment. Theory and Decision 77 (3), 313–322.

Card, D., Dahl, G. B., 2011. Family violence and football: The effect of unexpected emotional cues on violent behavior. Quarterly Journal of Economics 126 (1), 103–143.

Cassady, R., 1967. Auctions and Auctioneering. University of California Press, Oakland, California.

Cerroni, S., Notaro, S., Shaw, W. D., 2012. Do monetary incentives and chained questions affect the validity of risk estimates elicited via the exchangeability method? An experimental investigation. Working Paper.

CESifo, 2011. Deposit insurance coverage. CESifo DICE Report 9 (4), 69–70.

Cettolin, E., Riedl, A., 2011. Fairness and uncertainty. Working Paper, Maastricht University.

Charness, G., Gneezy, U., 2009. Incentives to exercise. Econometrica 77 (3), 909–931.

Charness, G., Gneezy, U., 2010. Portfolio choice and risk attitudes: An experiment. Economic Inquiry 48 (1), 133–146.

Charness, G., Gneezy, U., 2012. Strong evidence for gender differences in risk taking. Journal of Economic Behavior & Organization 83 (1), 50–58.

Charness, G., Gneezy, U., Kuhn, M. A., 2012. Experimental methods: Between-subject and within-subject design. Journal of Economic Behavior & Organization 81 (1), 1–8.

Charness, G., Kuhn, P., 2011. Lab labor: What can labor economists learn from the lab? In: Ashenfelter, O., Card, D. (Eds.), Handbook of Labor Economics Vol 4A. North Holland, Amsterdam, pp. 229–330.

Charness, G., Rabin, M., 2002. Understanding social preferences with simple tests. Quarterly Journal of Economics 117 (3), 817–869.

Chen, R., Chen, Y., 2011. The potential of social identity for equilibrium selection. American Economic Review 101 (6), 2562–2589.

Chetty, R., 2009. Is the taxable income elasticity sufficient to calculate deadweight loss? The implications of evasion and avoidance. American Economic Journal: Economic Policy 1 (2), 31–52.

Chetty, R., Szeidl, A., 2014. Consumption commitments and habit formation. Working Paper.

Chwe, M. S.-Y., 1989. The discrete bid first auction. Economics Letters 31 (4), 303–306.

Cokely, E. T., Galesic, M., Schulz, E., Ghazal, S., Garcia-Retamero, R., 2012. Measuring risk literacy: The Berlin numeracy test. Judgement and Decision Making 7 (1), 25–47.

Cole, H. L., Kehoe, T. J., 2000. Self-fulfilling debt crises. Review of Economic Studies 67 (1), 91–116.

Constantinides, G. M., 1990. Habit formation: A resolution of the equity premium puzzle. Journal of Political Economy 98 (3), 519–543.

Cooper, R., DeJong, D. V., Forsythe, R., Ross, T. W., 1992. Communication in coordination games. Quarterly Journal of Economics 107 (2), 739–771.

Cooper, R. W., DeJong, D. V., Forsythe, R., Ross, T. W., 1990. Selection criteria in coordination games: Some experimental results. American Economic Review 80 (1), 218–233.

Coppinger, V. M., Smith, V. L., Titus, J. A., 1980. Incentives and behavior in English, Dutch and sealed bid auctions. Economic Inquiry 18 (1), 1–22.

Cox, J. C., Roberson, B., Smith, V. L., 1982. Theory and behavior of single object auctions. In: Smith, V. L. (Ed.), Research in Experimental Economics. Vol. 2. JAI Press Inc., Greenwich, Connecticut, pp. 1–43.

Cox, J. C., Smith, V. L., Walker, J. M., 1983. A test that discriminates between two models of the Dutch-first auction non-isomorphism. Journal of Economic Behavior & Organization 4 (2–3), 205–219.

Cramer, J. S., Hartog, J., Jonker, N., Van Praag, C. M., 2002. Low risk aversion encourages the choice for entrepreneurship: An empirical test of a truism. Journal of Economic Behavior & Organization 48 (1), 29–36.

Crawford, V. P., Meng, J., 2011. New York City cab drivers' labor supply revisited: Reference-dependent preferences with rational-expectations targets for hours and income. American Economic Review 101 (5), 1912–32.

Cubitt, R. P., Starmer, C., Sugden, R., 1998. Dynamic choice and the common ratio effect: An experimental investigation. Economic Journal 108 (450), 1362–1380.

de Jong, F., Zhou, Y., 2013. Portfolio and consumption choice with habit formation under inflation. Netspar Discussion Papers 08/2013-023.

Dean, M., Ortoleva, P., 2014. Is it all connected? A testing ground for unified theories of behavioral economics phenomena. Working Paper.

DellaVigna, S., 2009. Psychology and economics: Evidence from the field. Journal of Economic Literature 47 (2), 315–372.

Detemple, J. B., Zapatero, F., 1991. Asset prices in an exchange economy with habit formation. Econometrica 59 (6), 1633–1657.

Devetag, G., Ortmann, A., 2007. When and why? A critical survey on coordination failure in the laboratory. Experimental Economics 10 (3), 331–344.

Diamond, D. W., Dybvig, P. H., 1983. Bank runs, deposit insurance, and liquidity. Journal of Political Economy 91 (3), 401–419.

Djawadi, B. M., Fahr, R., 2012. The impact of tax knowledge and budget spending influence on tax compliance. Working Paper, University of Paderborn.

Doerrenberg, P., Duncan, D., 2014. Experimental evidence on the relationship between tax evasion opportunities and labor supply. European Economic Review 68, 48–70.

Doerrenberg, P., Duncan, D., Zeppenfeld, C., 2015. Circumstantial risk: Impact of future tax evasion and labor supply opportunities on risk exposure. Journal of Economic Behavior & Organization 109, 85–100.

Doran, K., 2010. Wages, daily income goals and daily labor supply. Working Paper, University of Notre Dame.

Ehrhart, K.-M., Ott, M., 2014. Reference-dependent bidding in dynamic auctions. Working Paper.

Engelmann, D., 2012. How not to extend models of inequality aversion. Journal of Economic Behavior & Organization 81 (2), 599–605.

Ericson, K. M. M., Fuster, A., 2011. Expectations as endowments: Evidence on reference-dependent preferences from exchange and valuation experiments. Quarterly Journal of Economics 126 (4), 1879–1907.

Etchart-Vincent, N., l'Haridon, O., 2011. Monetary incentives in the loss domain and behavior toward risk: An experimental comparison of three reward schemes including real losses. Journal of Risk and Uncertainty 42 (1), 61–83.

EUC, 2010. Commission proposes package to boost consumer protection and confidence in financial services. European Commission, Press Release, July 12, 2010.

FDIC, 2010. Basic FDIC insurance coverage permanently increased to $250,000 per depositor. Federal Deposit Insurance Corporation, Press Release, July 21, 2010Https://www.fdic.gov/news/news/press/2010/pr10161.html (Accessed: March 23, 2015).

Fehr, E., Schmidt, K. M., 1999. A theory of fairness, competition, and cooperation. Quarterly Journal of Economics 114 (3), 817–868.

Fehr, E., Schmidt, K. M., 2006. Chapter 8 the economics of fairness, reciprocity and altruism - experimental evidence and new theories. In: Kolm, S.-C., Ythier, J. M. (Eds.), Foundations. Vol. 1 of Handbook of the Economics of Giving, Altruism and Reciprocity. Elsevier, pp. 615–691.

Fehr, E., Zych, P. K., 1998. Do addicts behave rationally? Scandinavian Journal of Economics 100 (3), pp. 643–662.

Feldhaus, C., Rockenbach, B., Zeppenfeld, C., 2015. The effect of payoff equality on equilibrium selection. Working Paper.

Fernandez-Villaverde, J., Krueger, D., 2007. Consumption over the life cycle: Facts from consumer expenditure survey data. Review of Economics and Statistics 89 (3), 552–565.

Fischbacher, U., 2007. z-tree: Zurich toolbox for ready-made economic experiments. Experimental Economics 10 (2), 171–178.

Franke, G., Schlesinger, H., Stapleton, R. C., 2011. Risk taking with additive and multiplicative background risks. Journal of Economic Theory 146 (4), 1547–1568.

Frederick, S., Loewenstein, G., O'Donoghue, T., 2002. Time discounting and time preference: A critical review. Journal of Economic Literature 40 (2), 351–401.

Fugger, N., Gillen, P., Rasch, A., Zeppenfeld, C., 2015. Preferences and decision support in competitive bidding. Working Paper.

Février, P., Visser, M., 2004. A study of consumer behavior using laboratory data. Experimental Economics 7 (1), 93–114.

Gakidis, H., 1997. Stocks for the old? Earnings uncertainty and life-cycle portfolio choice. manuscript, MIT.

Gill, D., Prowse, V., 2012. A structural analysis of disappointment aversion in a real effort competition. American Economic Review 102 (1), 469–503.

Gill, D., Prowse, V., 2013a. A novel computerized real effort task based on sliders. Oxford Department of Economics Discussion Paper 435.

Gill, D., Prowse, V. L., 2013b. Gender differences and dynamics in competition: The role of luck. Quantitative Economics 5 (2).

Gneezy, U., Kapteyn, A., Potters, J., 2003. Evaluation periods and asset prices in a market experiment. Journal of Finance 58 (2), 821–838.

Gneezy, U., Potters, J., 1997. An experiment on risk taking and evaluation periods. Quarterly Journal of Economics 112 (2), 631–645.

Goeree, J. K., Holt, C. A., 2000. Asymmetric inequality aversion and noisy behavior in alternating-offer bargaining games. European Economic Review 44 (4-6), 1079–1089.

Goeree, J. K., Holt, C. A., 2005. An experimental study of costly coordination. Games and Economic Behavior 51, 349–364.

Goeree, J. K., Holt, C. A., Palfrey, T. R., 2002. Quantal response equilibrium and overbidding in private-value auctions. Journal of Economic Theory 104 (1), 247–272.

Gollier, C., 2001. The Economics of Risk and Time. MIT Press, Cambridge, Massachusetts.

Gollier, C., Zeckhauser, R., 2002. Horizon length and portfolio risk. Journal of Risk and Uncertainty 24 (3), 195–212.

Gorodnichenko, Y., Martinez-Vazquez, J., Sabirianova Peter, K., 2009. Myth and reality of flat tax reform: Micro estimates of tax evasion response and welfare effects in Russia. Journal of Political Economy 117 (3), 504–554.

Greiner, B., 2004. An online recruitment system for economic experiments. In: Kremer, K., Macho, V. (Eds.), Forschung und wissenschaftliches Rechnen. GWDG Bericht 63. Ges. für Wiss. Datenverarbeitung, Göttingen, pp. 79–93.

Grimm, V., Schmidt, U., 2000. Equilibrium bidding without the independence axiom: A graphical analysis. Theory and Decision 49 (4), 361–374.

Guler, K., Fine, L., Chen, K., Karp, A., Liu, T., Tang, H., Safai, F., Wu, R., Zhang, A., 2002. Automated decision support system for designing auctions. US Patent App. 09/858,251.

Guler, K., Liu, T., Tang, H., 2003. Joint estimation of bidders' risk attitudes and private information. US Patent App. 09/904,311.

Guler, K., Liu, T., Tang, H. K., 2009. Method and system for automated bid advice for auctions. US Patent No 7536338.

Halla, M., 2012. Tax morale and compliance behavior: First evidence on a causal link. The B.E. Journal of Economic Analysis & Policy 12 (1), Article 13.

Hammermann, A., Mohnen, A., Nieken, P., 2012. Whom to choose as a team mate? A lab experiment about in-group favouritism. Working Paper, IZA Discussion Papers 6286.

Harrison, G. W., 1986. An experimental test for risk aversion. Economics Letters 21 (1), 7–11.

Harrison, G. W., Rutström, E. E., 2008. Risk aversion in the laboratory. In: Cox, J. C., Harrison, G. W. (Eds.), Risk Aversion in Experiments. Vol. 12 of Research in Experimental Economics. Emerald Group, pp. 41–196.

Harsanyi, J. C., Selten, R., 1988. A General Theory of Equilibrium Selection in Games. Vol. 1. MIT Press, Cambridge, Massachusetts.

Hartog, J., Ferrer-i Carbonell, A., Jonker, N., 2002. Linking measured risk aversion to individual characteristics. Kyklos 55 (1), 3–26.

Heaton, J., Lucas, D., 1997. Market frictions, savings behavior, and portfolio choice. Macroeconomic Dynamics 1, 76–101.

Heaton, J., Lucas, D., 2000. Portfolio choice in the presence of background risk. The Economic Journal 110 (460), 1–26.

Hirshleifer, J., 1983. From weakest-link to best-shot: The voluntary provision of public goods. Public Choice 41 (3), 371–386.

Hodgson, G. M., 2006. What are institutions? Journal of Economic Issues 40 (1), 1–25.

Houser, D., Schunk, D., Winter, J., Xiao, E., 2010. Temptation and commitment in the laboratory. University of Zurich - IEW Working Paper 488.

Inglehart, R., n.d. Values change the world. http://worldvaluessurvey.org (Accessed: April 2010, March 23, 2015).

Jappelli, T., Pistaferri, L., 2010. The consumption response to income changes. Annual Review of Economics 2, 479–506.

Kagel, J. H., Roth, A. E., 1992. Theory and misbehavior in first-price auctions: Comment. American Economic Review 82 (5), 1379–1391.

Kahneman, D., Tversky, A., 1979. Prospect theory: An analysis of decision under risk. Econometrica 47 (2), 263–291.

Kaiser, J., 2007. An exact and Monte Carlo proposal to the Fisher-Pitman permutation tests for paired replicates and for independent samples. Stata Journal 7 (3), 402–412.

Kajii, A., Morris, S., 1997. The robustness of equilibria to incomplete information. Econometrica 65 (6), 1283–1309.

Karle, H., Kirchsteiger, G., Peitz, M., 2014. Loss aversion and consumption choice: Theory and experimental evidence. American Economic Journal: Microeconomics (forthcoming).

Katok, E., Kwasnica, A. M., 2007. Time is money: The effect of clock speed on seller's revenue in Dutch auctions. Experimental Economics 11 (4), 344–357.

Kim, E. H., Morse, A., Zingales, L., 2006. What has mattered to economics since 1970. Journal of Economic Perspectives 20 (4), 189–202.

Kleven, H. J., Knudsen, M. B., Kreiner, C. T., Pedersen, S., Saez, E., 2011. Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark. Econometrica 79 (3), 651–692.

Klos, A., 2004. The investment horizon and dynamic asset allocation – some experimental evidence. Economics Letters 85 (2), 167–170.

Klos, A., Weber, E. U., Weber, M., 2005. Investment decisions and time horizon: Risk perception and risk behavior in repeated gambles. Management Science 51 (12), 1777–1790.

Klos, A., Weber, M., 2006. Portfolio choice in the presence of non-tradable income: An experimental analysis. German Economic Review 7 (4), 427–448.

Knight, F. H., 1921. Risk, Uncertainty and Profit. Houghton Mifflin, Boston.

Köbberling, V., Wakker, P. P., 2005. An index of loss aversion. Journal of Economic Theory 122 (1), 119–131.

Köszegi, B., Rabin, M., 2006. A model of reference-dependent preferences. Quarterly Journal of Economics 121 (4), 1133–1165.

Köszegi, B., Rabin, M., 2007. Reference-dependent risk attitudes. American Economic Review 97 (4), 1047–1073.

Köszegi, B., Rabin, M., 2009. Reference-dependent consumption plans. American Economic Review 99 (3), 909–936.

Kotlikoff, L. J., Samuelson, W., Johnson, S., 1988. Consumption, computation mistakes, and fiscal policy. American Economic Review 78 (2), 408–412.

Kroll, Y., Levy, H., 1992. Further tests of the separation theorem and the capital asset pricing model. American Economic Review 82 (3), 664–70.

Kroll, Y., Levy, H., Rapoport, A., 1988a. Experimental tests of the mean-variance model for portfolio selection. Organizational Behavior and Human Decision Processes 42 (3), 388–410.

Kroll, Y., Levy, H., Rapoport, A., 1988b. Experimental tests of the separation theorem and the capital asset pricing model. American Economic Review 78 (3), pp. 500–519.

Lange, A., Ratan, A., 2010. Multi-dimensional reference-dependent preferences in sealed-bid auctions – how (most) laboratory experiments differ from the field. Games and Economic Behavior 68 (2), 634–645.

Liu, X., Yang, F., Cai, Z., 2014. Does relative risk aversion vary with wealth? Evidence from households' portfolio choice data. Working Paper.

Loewenstein, G., O'Donoghue, T., Rabin, M., 2003. Projection bias in predicting future utility. Quarterly Journal of Economics 118 (4), 1209–1248.

Lucking-Reiley, D., 1999. Using field experiments to test equivalence between auction formats: Magic on the internet. American Economic Review 89 (5), 1063–1080.

Luhan, W., Roos, M., Scharler, J., 2011. An experiment on consumption responses to future prices and interest rates. Working Paper, Ruhr Economic Paper 248.

Machina, M., 2008a. Non-expected utility theory. In: Durlauf, S. N., Blume, L. E. (Eds.), The New Palgrave Dictionary of Economics, 2nd Edition. Palgrave Macmillan, New York City, New York, pp. 74–84.

Machina, M., 2008b. Risk. In: Durlauf, S. N., Blume, L. E. (Eds.), The New Palgrave Dictionary of Economics, 2nd Edition. Palgrave Macmillan, New York City, New York, pp. 74–84.

Manski, C. F., 2000. Economic analysis of social interactions. Journal of Economic Perspectives 14 (3), 115–136.

Manzini, P., Sadrieh, A., Vriend, N. J., 2009. On smiles, winks and handshakes as coordination devices. Economic Journal 119 (537), 826–854.

Marden, J. R., Shamma, J. S., 2012. Revisiting log-linear learning: Asynchrony, completeness and payoff-based implementation. Games and Economic Behavior 75 (2), 788–808.

Mas, A., 2006. Pay, reference points, and police performance. Quarterly Journal of Economics 121 (3), 783–821.

Mattei, A., 2000. Full-scale real tests of consumer behavior using experimental data. Journal of Economic Behavior & Organization 43 (4), 487–497.

McKelvey, R. D., Palfrey, T. R., 1995. Quantal response equilibria for normal form games. Games and Economic Behavior 10 (1), 6–38.

Merton, R. C., 1969. Lifetime portfolio selection under uncertainty: The continuous-time case. Review of Economics and Statistics 51 (3), 247–257.

Mitchell, O. S., Utkus, S. P., 2006. How behavioral finance can inform retirement plan design. Journal of Applied Corporate Finance 18 (1), 82–94.

Monderer, D., Shapley, L. S., 1996. Potential games. Games and Economic Behavior 14 (1), 124–143.

Mossin, J., 1968. Optimal multiperiod portfolio policies. The Journal of Business 41 (2), 215–229.

Munk, C., 2008. Portfolio and consumption choice with stochastic investment opportunities and habit formation in preferences. Journal of Economic Dynamics & Control 32 (11), 3560–3589.

Mussweiler, T., Ockenfels, A., 2013. Similarity increases altruistic punishment in humans. Proceedings of the National Academy of Sciences 110 (48), 19318–19323.

Nakajima, D., 2011. First-price auctions, Dutch auctions, and buy-it-now prices with Allais paradox bidders. Theoretical Economics 6 (3), 473–498.

Neyman, A., 1997. Correlated equilibrium and potential games. International Journal of Game Theory 26 (2), 223–227.

Nordhaus, W. D., 2006. Paul Samuelson and global public goods: A commemorative essay for Paul Samuelson. In: Szenberg, M., Ramrattan, L., Gottesman, A. A. (Eds.), Samuelsonian Economics and the Twenty-First Century. Oxford University Press, Oxford, Ch. 5, pp. 88–98.

Obstfeld, M., 1996. Models of currency crises with self-fulfilling features. European Economic Review 40 (3-5), 1037–1047.

Ochs, J., 1995. Coordination problems. In: Kagel, J. K., Roth, A. E. (Eds.), Handbook of Experimental Economics. Princeton, NJ, Princeton University Press, Princeton, New Jersey, pp. 195–252.

Odean, T., 1998. Are investors reluctant to realize their losses? Journal of Finance 53 (5), 1775–1798.

Pagel, M., 2012. Expectations-based reference-dependent preferences and asset pricing. Journal of the European Economic Association (forthcoming).

Pagel, M., 2014. Expectations-based reference-dependent life-cycle consumption. Working Paper.

Pagel, M., Zeppenfeld, C., 2015. Dynamic on-the-spot consumption and portfolio choice in the lab. Working Paper.

Pope, D. G., Schweitzer, M. E., 2011. Is tiger woods loss averse? Persistent bias in the face of experience, competition, and high stakes. American Economic Review 101 (1), 129–57.

Pratt, J. W., 1964. Risk aversion in the small and in the large. Econometrica 32 (1/2), 122–136.

Riedl, A., Rohde, I. M. T., Strobel, M., 2012. Efficient coordination in weakest-link games. Working Paper.

Riener, G., Wiederhold, S., 2011. On social identity, subjective expectations, and the costs of control. Working Paper, Jena Economic Research Papers 2011-035.

Rothacker, R., 2008. $5 billion withdrawn in one day in silent run. Charlotte Observer (10/11/2008), http://www.charlotteobserver.com/news/article9016391.html (Accessed: March 20, 2015).

Samuelson, P. A., 1937. A note on measurement of utility. Review of Economic Studies 4 (2), 155–161.

Samuelson, P. A., 1969. Lifetime portfolio selection by dynamic stochastic programming. Review of Economics and Statistics 51 (3), pp. 239–246.

Sandler, T., 1998. Global and regional public goods: A prognosis for collective action. Fiscal Studies 19 (3), 221–247.

Savage, L. J., 1954. The Foundations of Statistics. John Wiley & Sons, Ltd., Hoboken, New Jersey.

Scheinkman, J. A., 2008. Social interactions. In: Durlauf, S. N., Blume, L. E. (Eds.), The New Palgrave Dictionary of Economics, 2nd Edition. Palgrave Macmillan, New York City, New York, pp. 74–84.

Schmidt, U., Seidl, C., 2014. Reconsidering the common ratio effect: The roles of compound idependence, reduction, and coalescing. IfW Kiel Working Paper 1930.

Schwartz, L. M., Woloshin, S., Black, W. C., Welch, G., 1997. The role of numeracy in understanding the benefit of screening mammography. Annals of Internal Medicine 127 (11), 966–972.

Siegel, S., Castellan, N. J., 1988. Nonparametric Statistics for The Behavioral Sciences, 2nd Edition. McGraw-Hill, New York City, New York.

Sippel, R., 1997. An experiment on the pure theory of consumer's behaviour. Economic Journal 107 (444), 1431–1444.

Slemrod, J., 1994. On the high-income laffer curve. In: Slemrod, J. (Ed.), Tax Progressivity and Income Inequality. Cambridge University Press, Cambridge, New York and Melbourne, pp. 177–210.

Slemrod, J., 2003. Trust in public finance. In: Cnossen, S., Sinn, H.-W. (Eds.), Public Finance and Public Policy in the New Century. The MIT Press, Cambridge, USA, pp. 49–88.

Smith, V. L., 1976. Experimental economics: Induced value theory. American Economic Review 66 (2), 274–279.

Sprenger, C., 2010. An endowment effect for risk: Experimental tests of stochastic reference points. Working Paper.

Stevenson, R., Slater, S., 2008. Bank savers run at the click of a mouse. Reuters (10/07/2008), http://www.reuters.com/article/2008/10/07/us-financial-silentrun-idUSTRE49600Z20081007 (Accessed: March 20, 2015).

Thaler, R. H., Tversky, A., Kahneman, D., Schwartz, A., 1997. The effect of myopia and loss aversion on risk taking: An experimental test. The Quarterly Journal of Economics 112 (2), 647–661.

Turocy, T. L., Watson, E., Battalio, R. C., 2007. Framing the first-price auction. Experimental Economics 10 (1), 37–51.

Ui, T., 2001. Robust equilibria of potential games. Econometrica 69 (5), 1373–1380.

Van de Kuilen, G., Wakker, P. P., 2011. The midweight method to measure attitudes toward risk and ambiguity. Management Science 57 (3), 582–598.

Van Huyck, J. B., Battalio, R. C., Beil, R. O., 1990. Tacit coordination games, strategic uncertainty, and coordination failure. American Economic Review 80 (1), 234–248.

Van Huyck, J. B., Gillette, A. B., Battalio, R. C., 1992. Credible assignments in coordination games. Games and Economic Behavior 4 (4), 606–626.

Vickrey, W., 1961. Counterspeculation, auctions, and competitive sealed tenders. Journal of Finance 16 (1), 8–37.

Wakker, P., Deneffe, D., 1996. Eliciting von Neumann-Morgenstern utilities when probabilities are distorted or unknown. Management Science 42 (8), 1131–1150.

Weber, B. J., 2007. The effects of losses and event splitting on the Allais paradox. Judgment and Decision Making 2 (2), 115–125.

Weber, R. J., 1982. The Allais paradox, Dutch auctions, and alpha-utility theory. Northwestern University Discussion Paper 536.

Wrede, M., 1995. Tax evasion and risk taking: Is tax evasion desirable? Public Finance / Finances Publique 50 (2), 303–316.

Young, H. P., 1993. The evolution of convenstions. Econometrica 61 (1), 57–84.

Zhang, B., Guler, K., 2013. Determination of a bid value associated with a selected bidder. US Patent 8,548,882.