Four Essays on Economic Preferences

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Contents

List of Figures

List of Tables

Introduction

This thesis consists of four independent chapters. Each chapter contributes experimental evidence to our knowledge on economic preferences. Chapter one shows that a preference for truth-telling *per se* is even more prevalent than previous research suggests. Chapter 2 investigates the relationship between economic preferences and psychological personality measures and arrives at the conclusion that the degree of association between the two concepts is rather small and that they are complementary in explaining heterogeneity in life outcomes. Chapter 3 validates non-incentivized survey measures for key economic preferences, i.e. risk taking, time discounting and social preferences, by examining their predictive power for behavior in incentivized economic choice experiments. Chapter 4 shows that the variation in preferences across countries as documented in Falk, Becker, Dohmen, Enke, Huffman, and Sunde (2015) has deep cultural origins.

Chapter 1[1] attends to what is often called a non-standard preference: a preference for truth-telling *per se*.[2] We implement a truth-telling experiment, in which misreporting cannot be detected and participants have a strong monetary incentive to misreport, with a representative population sample which we call at home. We find that aggregate reporting behavior closely resembles the distribution that would result if everyone reported truthfully. This contrasts previous evidence from laboratory experiments which also documented substantial levels of truthful reporting as well, but consistently found considerable degrees of cheating. Since our partici-

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[1]This chapter is joint work with Johannes Abeler and Armin Falk. It has been published in the *Journal of Public Economics*, see Abeler, Becker, and Falk (2014).

[2]Such a preference is "non-standard" in the sense that a selfish and rational agent would not exhibit it.

pants made their reports via the phone while participants in laboratory experiments typically entered their reports into the computer, we conduct an additional laboratory experiment to rule out the possibility that the difference between behavior in our study and previous research is mainly driven by the difference in communication modes. Similarly, we can rule out that it is the difference in the subject pools - a representative sample versus the typical student participants in laboratory experiments - that explains the much higher level of truth-telling in our study: the behavior of the students in our representative sample does not differ from the behavior of the rest of the sample.

Chapter 2[3] examines the relationship between economic preferences and psychological personality measures. Using data from incentivized laboratory experiments and representative samples of the German population it shows that the association between the two concepts is rather low and that the two concepts are complementary in explaining heterogeneity in life outcomes.

Chapter 3[4] validates survey measures for the six key economic preferences - risk taking, time discounting, trust, altruism, positive and negative reciprocity - by assessing their (joint) explanatory power in explaining behavior in incentivized choice experiments. This results in a preference module consisting of two items per preference - one typically being a hypothetical version of the incentivized experiment and the other one being a subjective self-assessment. Next, we adjust the module by reducing complexity and excluding culturally loaded wording to allow implementability across heterogeneous participants, e.g. in terms of cultural or educational background, and across survey modes. We test this "streamlined" module in the field in 22 countries of diverse cultural backgrounds. The resulting feedback calls for only minor adjustments and overall confirms a good implementability of our preference module.

Chapter 4[5] explores whether differences in culture can explain part of the varia-

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[3]This chapter is joint work with Thomas Deckers, Thomas Dohmen, Armin Falk and Fabian Kosse. It has been published in the *Annual Review of Economics*, see Becker, Deckers, Dohmen, Falk, and Kosse (2012).

[4]This chapter is joint work with Armin Falk, Thomas Dohmen, David Huffman and Uwe Sunde, see Falk, Becker, Dohmen, Huffman, and Sunde (2016)

[5]This chapter is part of joint work with Armin Falk, Thomas Dohmen, Benjamin Enke, David Huffman and Uwe Sunde, which is currently invited for resubmission at the *Quarterly Journal of Economics*, see Falk, Becker, Dohmen, Enke, Huffman, and Sunde (2015).

tion in economic preferences we see across countries around the globe as documented in Falk, Becker, Dohmen, Enke, Huffman, and Sunde (2015) by using a specific feature of languages as a proxy for culture. Speakers of languages which require the speaker to grammatically mark the future when talking about future events are less patient and less prosocial than speakers of languages which lack such a grammatical requirement. Heterogeneity in preferences across countries and cultures seems to be partly driven by deep cultural differences.

Chapter 1

Representative Evidence on Lying Costs

1.1 Introduction

Situations with asymmetric information are ubiquitous. Most of economic theory assumes that people misreport their private information if this is to their material benefit; behavior is only determined by the trade-off between financial gains from misreporting and monetary fines when misreporting is detected.[1] In contrast, many recent models in various domains of Public Economics (and in Economics more generally) rely on the assumption that people can experience a psychological disutility which holds them back from misreporting, at least to some extent. These models invoke different underlying motives. Kartik, Tercieux, and Holden (2014), for instance, assume that people face an intrinsic lying cost and show that in this case the social planner can fully implement a much wider range of social choice rules compared to the standard Maskin (1977) case without lying costs (see, e.g., Matsushima (2008) and Dutta and Sen (2011) for similar assumptions). Many studies about incentive systems for doctors assume that doctors are altruistic towards their patients and thus do not always state the profit-maximizing diagnosis but rather treat patients honestly (e.g., Ellis and McGuire, 1986; Chalkley and Malcomson, 1998). The large literature on "tax morale" (e.g., Lewis, 1982; Cowell, 1990; Andreoni, Erard, and Feinstein, 1998; Slemrod, 2007; Torgler, 2007), demonstrates that many tax payers misreport their income only a little bit or not at all. This literature is usually ag-

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[1]See, e.g., Allingham and Sandmo (1972) on tax evasion, Falkinger (1991) on public good provision, Pitchik and Schotter (1987) on credence goods, along with the seminal Becker (1968) on crime.

nostic about the exact underlying motives but some studies cite efficiency concerns (e.g., Alm, McClelland, and Schulze, 1992), patriotism (Konrad and Qari, 2012), religiosity (Torgler, 2006), fairness (Bordignon, 1993), conditional cooperation (Traxler, 2010) or honesty (Erard and Feinstein, 1994).

To further improve these models and to provide an empirically-validated microfoundation, it is crucial to understand the relevance of the different potential motives. Additionally, understanding these motives could inform the design of more psychologically-realistic policies, e.g., in the area of tax enforcement, that have a higher potential of being successful. In this paper, we focus on intrinsic lying costs and investigate how widespread and how large lying costs are. The ideal data set to answer these questions would allow studying lying costs for a representative sample of the population and in an environment without the confounding effects of strategic interaction (including the levy of fines), reputational or efficiency concerns, or altruism. So far, the best evidence on lying costs comes from experiments conducted in tightly controlled laboratory situations. A robust result is that many subjects misreport their private information to their own advantage but that a substantial share of subjects refrains from reporting the payoff-maximizing type and that some are fully honest (e.g., Gneezy, 2005; Charness and Dufwenberg, 2006; Fischbacher and Föllmi-Heusi, 2013; de Haan, Offerman, and Sloof, 2015; Houser, Vetter, and Winter, 2012; Shalvi, Dana, Handgraaf, and De Dreu, 2011; Wibral, Dohmen, Klingmüller, Weber, and Falk, 2012; Serra-Garcia, van Damme, and Potters, 2013). These studies are a strong first indicator that lying costs influence behavior. However, lab experiments do not allow for inferences with respect to the prevalence of lying costs in the overall population since they have been conducted almost exclusively with student samples (DellaVigna, 2009; Falk and Heckman, 2009). Also, decision making took place in an austere laboratory environment which might trigger behavior representative only of certain non-lab situations. It could thus be that there are systematic differences between behavior of students in the laboratory and behavior of non-student subjects outside the lab.

To circumvent these limitations, we measure how people report their private information outside the laboratory by calling participants on the phone at their home. Participants were drawn randomly from the German population, yielding a repre-

sentative sample. An incentivized experiment was embedded in the interview. The experimental setup is related to the design of Fischbacher and Föllmi-Heusi (2013) and is extremely simple: participants were asked to toss a coin and report their type, i.e., either "heads" or "tails". Reporting tails yielded a payoff of 15 euros, which participants could choose to receive in cash or as an Amazon gift certificate, while reporting heads yielded a payoff of zero. Participants thus had a clear monetary incentive to report tails regardless of their true type. It was obvious that the true outcome was only known to the participants, as they tossed the coin privately at home. In this setup, we cannot draw reliable conclusions about the truthfulness of any individual report. But we can learn about aggregate behavior by comparing the distribution of reports to the true distribution of a fair coin (50 percent tails) and to the payoff-maximizing distribution (100 percent tails). This indirect observation therefore allows us to study the behavior of subjects in a situation in which private information is kept truly private and in which subjects do not face any risk of detection.[2] Moreover, the decision is non-strategic; altruism does not play a role as the money is not taken from any individual person; and reputational concerns are minimized since the interviewer is a stranger with whom no future interaction can be expected.

If all our participants were rational money maximizers, we would expect that all of them reported tails. If behavior on the phone was similar to previous, comparable laboratory experiments (e.g., Houser, Vetter, and Winter, 2012), we would expect about 75 percent of subjects reporting tails.

In contrast to these predictions, observed behavior does not statistically differ from everybody reporting honestly. If anything, participants report the payoff maximizing outcome less often than expected under truthful reporting. This latter effect, however, is small and disappears in a second treatment in which participants were

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[2]In other studies concerning how people report their private information (e.g., Gneezy, 2005; Charness and Dufwenberg, 2006) the experimenter knows or will later know the subject's true type (and the subject is aware of this) and can thus judge whether an individual was honest or not. In our experiment, only the participant knows his or her private information. Our setup is thus closer to situations in which information is truly private and only known by the individual, while Gneezy's and Charness & Dufwenberg's setup is more representative of situations in which the private information is known by more than one person, e.g., when filing a joint tax declaration. These papers are also interested in the interaction between sender and receiver, from which we abstract. (See, however, the recent paper by Deck, Servátka, and Tucker (2013) who do not find an additional effect of promises on cooperation in single-blind and double-blind conditions.)

asked to report the total number of tails in four consecutive coin tosses and received 5 euros times the number of reported tails. The resulting distribution of reports in the 4-coin treatment is indistinguishable from the distribution under complete truth-telling. Moreover, while previous studies (e.g., Dreber and Johannesson, 2008) have found correlations between individual characteristics, like gender, and truth-telling, we do not find any robust correlations between individual characteristics and reporting behavior. This is not surprising if almost all participants report truthfully. Reports are solely determined by chance, namely the coin toss, which cannot be related to any individual characteristic. Our results thus show that lying costs are pervasive and are influencing behavior regardless of gender, religious beliefs, education, or age.

We complement our telephone study with two additional control treatments in the laboratory to better understand what shapes lying costs, in particular the effect of the mode of communication. In both lab treatments subjects reported the outcomes of four consecutive coin tosses. Incentives were the same as in the 4-coin treatment in the telephone study: 5 euros times the numbers of tails reported. In the first lab treatment, subjects had to report the outcome directly to an interviewer via the phone, mirroring our telephone study. We observe the same patterns of behavior as in previous lab experiment: subjects lie much more than in the telephone study. In the second control treatment, subjects reported the outcomes by clicking a number between 0 and 4 on the computer screen as in most previous lab experiments. We find that subjects who enter their report by clicking report slightly higher numbers but this difference is not statistically significant. The difference to the telephone study persists: the average report in both lab treatments is higher than in the telephone study. This shows that the mode of communication does not systematically influence reporting behavior strongly and is not driving the widespread truth-telling in our telephone study. We also elicit beliefs about the behavior of other participants and find in all four treatments that participants believe others to lie more than they actually do. Older participants (correctly) believe that lying is less prevalent. In the lab, higher beliefs are correlated with higher own reports. We find no evidence that being a student has a significant impact on behavior, or that the perceived time pressure on the telephone or the limited experience of the survey participants with

the abstract design of economics experiments played a role.

Our paper adds to the nascent literature studying lying outside the lab. Previous studies focused on particular groups: Bucciol and Piovesan (2011) study a sample of children and find that many of them lie, unless they are reminded to be honest; Cohn, Maréchal, and Noll (2015) study prisoners and find that they become less honest when reminded of their criminal identity; and Utikal and Fischbacher (2013) ask a small sample of nuns to report the roll of a dice and find significant downward lying. Studies looking at unethical behavior in less abstract environments include Azar, Yosef, and Bar-Eli (2013) who find that the majority of customers in a restaurant do not return excessive change. Similarly, Bucciol, Landini, and Piovesan (2013) study free-riding in public transportation in Italy and find that 43 percent of passengers evade the fare. We add two features: we study a representative sample and we can investigate the underlying motives by conducting additional lab experiments using the same well-defined decision.

Taken together, our results strengthen the doubts that previous lab experiments have cast on the assumption of zero lying costs: we find evidence for even higher lying costs in the telephone study. This suggests that studying the theoretical implications of such costs (e.g., Kartik, Ottaviani, and Squintani, 2007; Kartik, Tercieux, and Holden, 2014; Doerrenberg, Duncan, Fuest, and Peichl, 2014) is a promising research avenue. At the same time, it is very likely that altruism, efficiency concerns, etc. are also important factors in the decision to pay taxes or how to treat patients, for example. Future research would need to investigate the relative importance of different motives that hold people back from misreporting and the interactions between motives. Our results also do not mean that lab experiments are uninformative about non-laboratory settings. However, the difference in behavior between our telephone study and our and previous lab experiments rather shows how malleable reporting behavior can be. This opens many new questions about how exactly reporting private information depends on the decision-making context. Intuitively, different norms might apply when making such a decision at home, representing a private and familiar environment. Similarly, people could be more attentive to their own moral rules, e.g., abstaining from lying when at home.[3] Irrespective of these differences be-

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[3]Previous research comparing behavior of student samples vs. non-students samples and be-

tween lab and field, our study establishes that lying costs are more important than previously assumed and are strongly influencing behavior across different decision environments.

In the next two sections, we present the design of the study and our hypotheses. Section 3.3 contains the results. We discuss policy implications in Section 1.5.

1.2   Design

The computer-assisted telephone interviews were operated by the Institute for Applied Social Sciences (infas), a private and well-known German research institute. They were conducted between November 2010 and February 2011.[4] The average interview lasted 20 minutes (standard deviation: 5.5 minutes). Telephone numbers were selected using a random digit dialing technique: numbers were generated randomly based on a data set of all potential telephone numbers in Germany. Only landline numbers were used in this study, as 92.7 percent of German households have a landline number (Destatis, 2012). The selection of the participant within each household was also random: only the member of the household whose birthday was the most recent among all household members was eligible to participate. We restricted participation to those aged between 18 and 70 years at the time of the interview.[5]

The survey was split into two parts. The first part of the questionnaire consisted of questions relating to the participants' socio-demographic background and their risk and trust preferences. Risk and trust preferences were measured by using subjective

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havior in the lab vs. outside the lab has in most cases shown little differences (with a few notable exceptions, e.g., Stoop, Noussair, and Van Soest, 2012). The strong difference in behavior between our field and lab studies suggests that truth-telling is more context dependent than other behaviors, like cooperation, altruistic behavior, or consumption choices (Abeler and Marklein, forthcoming). For an overview and critical discussion, see Falk and Heckman (2009), Camerer (in press), or Coppock and Green (2015).

[4]The interviews were conducted in the infas telephone studio. Infas ensures a high quality of interviews by supervising interviews randomly. Supervisors are present in the telephone studio at all times and interviews can be monitored without the interviewer noticing this.

[5]The majority of non-participation was due to no-one answering the phone or people hanging up immediately after hearing that a market research firm called. Of the 738 people who started the questionnaire of the 1-coin treatment at all and could condition their participation on the content of questionnaire or experiment, 658 participants (89.1 percent) completed the entire questionnaire and the experiment. Like in all telephone-based surveys, the resulting sample is therefore representative for the part of the population who was at home at the time of call and was willing to participate.

self-assessments, using the general risk question of the GSOEP ("How do you consider yourself? Are you in general a rather risk-loving person, or do you try to avoid risks? Use a scale from 1, meaning that you are not at all willing to take risks, to 7, meaning that you are absolutely willing to take risks." Dohmen, Falk, Huffman, Sunde, Schupp, and Wagner, 2011) and the World Value Survey trust question ("Generally speaking: Do you think one can trust other people, or that one should rather be careful when dealing with other people? Please indicate your answer on a scale from 1 to 7, with 1 meaning that one should be careful when dealing with other people, and 7 meaning that one can trust other people."). After this part, the experiment described below took place. After the experiment, participants were asked about their political preferences, their current living and financial situation, their religious beliefs, and their attitudes towards opportunistic behavior and everyday crime. At the very end of the interview, participants were asked to state their belief about other participants' behavior in the experiment.

Before the experiment started the participant was reminded that the resulting data would be anonymized, and that infas and the University of Bonn guaranteed the correct payment. The interviewer then asked the participant to take a coin and explained the rules of the experiment: the task was to toss the coin and report whether heads or tails came up.[6] If the participant reported heads, they received no payment. If the participant reported tails, they would receive 15 euros. Then, the participant was asked to toss the coin and report the outcome. We will call this treatment "1-Coin-Telephone." 658 people participated in this version of our experiment. A translation of the exact experimental instructions can be found in online appendix 1.6.1.

In a second treatment, 94 people were interviewed and participated in the following variation of the experiment. Participants were asked to take a coin, toss it four times, and report the number of times that tails came up. For each time participants reported tails they received 5 euros. Thus, they could earn 0, 5, 10, 15, or 20 euros. We will call this treatment "4-Coin-Telephone." Payment in both treatments could

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[6]A referee mentioned that some euro coins were reported to not be fair, in particular when spinning the coin. We don't think this is a concern in our study since we asked participants to flip or toss the coin. See Gelman and Nolan (2002) for an explanation on why it is extremely difficult to bias a coin when flipping it.

be received either in cash via regular mail or as an Amazon gift certificate code. The alphanumeric 14-digit gift certificate code was transmitted via email or directly on the phone at the end of the interview.

In order to further investigate what influences behavior in the telephone study, in particular the mode of reporting, we additionally conducted two versions of the 4-coin treatment in the laboratory. Subjects were students of the University of Bonn studying different majors except Economics. They were seated at a desk with a computer in separate room-high cubicles closed off by curtains. As the experiment took only a few minutes, it was run at the end of the sessions of a different experiment (similar to Fischbacher and Föllmi-Heusi, 2013). In the preceding experiment subjects made abstract consumption or labour supply choices which involved no private information and no interaction with other subjects. When the experiment started, subjects were asked to take a coin that was placed in their cubicle, toss it four times, and report how often tails came up. For each time they reported that tails came up they received 5 euros, i.e., up to 20 euros, just like in 4-Coin-Telephone. Their earnings were paid in cash directly after the experiment.[7]

The only difference between the two lab treatments was how the reporting was done. In the first treatment, subjects had to state their report directly to an interviewer via the phone, mirroring our telephone study. After tossing the coin in their cubicle, they were asked to go one-by-one to an adjacent room and pick up the telephone that we had placed there. An interviewer on the other side of the line (whom subjects never met directly) would then ask for their experimental ID and the number of times the coin showed tails. We made sure that other subjects could not hear the conversation. The starting times for the coin tossing was staggered, such that subjects did not have to wait between coin-tossing and reporting. 170 subjects participated in this treatment which we will call "4-Coin-Lab-Tel." This treatment serves to replicate our telephone study as closely as possible in the laboratory. In the second treatment, subjects reported their outcome by clicking a number 0 to 4 on the computer screen, similar to previous lab experiments. 180 subjects participated in the second treatment which we will call "4-Coin-Lab-Click". This treatment serves to

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[7]The instructions for the lab experiment can be found in online appendices 1.6.2 and 1.6.3. The experiment was conducted using ztree and ORSEE (Fischbacher, 2007; Greiner, 2004).

investigate whether the mode of communication, i.e., clicking on a computer screen versus reporting to a person via the telephone, influences reporting behavior.

1.3 Hypotheses

The standard economic prediction in our setup is straightforward: depending on the treatment, people will report tails one or four times, respectively. This is the payoff maximizing outcome as there are no exogenous costs linked to misreporting, no possibility of detection and no fines. The setup is extremely simple and participants should have no trouble identifying the payoff maximizing choice. Moreover, the setup is highly anonymous, discouraging any reputational concerns because of repeated interaction.

If, however, some participants incur a psychological cost or derive direct disutility from falsely reporting their private information per se we should expect both heads and tails to be reported in the experiment. There are a few recent theoretical papers that assume such a cost. For example, Kartik (2009) and Kartik, Ottaviani, and Squintani (2007) build on Crawford and Sobel's 1982 cheap-talk model and derive predictions for the case that some agents incur costs when misreporting their private information (see also, e.g., Saran, 2011; Kartik, Tercieux, and Holden, 2014). Assuming some degree of heterogeneity in the incurred costs when misreporting, it is then a question of the trade-off between psychological costs and monetary benefits of misreporting how many participants will report heads and how many report tails.

Participants in 1-Coin-Telephone have to make a clear, binary choice whether to lie or not; if lying costs are related to self-reputation or identity (e.g., Bénabou and Tirole, 2006; Akerlof and Kranton, 2000), lying in such a setting could impact self-reputation or identity more and thus make lying more costly. Participants in 4-Coin-Telephone can make a finer choice between being honest, exaggerating a little bit, or lying maximally; this could render small lies compatible with a positive self-reputation and thus enhance lying (Mazar, Amir, and Ariely, 2008). Such non-maximal lying has already been shown to be important by Fischbacher and Föllmi-Heusi (2013).

In the telephone study, participants tossed the coin at their home. It was thus

obvious that the interviewer could not secretly observe the true outcome of the coin toss.[8] If some participants in our lab experiments (erroneously) believed that the experimenter could observe the true outcome and believed (again erroneously) that misreporting would lead to some negative or unpleasant outcome, we would expect more truth-telling in the laboratory.[9]

Regarding potential differences in reporting behavior according to individual characteristics, we would expect that women are more honest than men (as already shown by Dreber and Johannesson, 2008; Houser, Vetter, and Winter, 2012). More religious participants would be expected to be more honest, since religious priming leads to less lying and more pro-social behavior (Mazar, Amir, and Ariely, 2008; Shariff and Norenzayan, 2007). Income could be positively correlated with honesty because of the lower marginal utility of the monetary rewards or negatively correlated because of reverse causality. A similarly ambiguous hypothesis can be derived for education or the social environment, e.g., the size of the community or family status. Along theories of endogenous social norms (e.g., Traxler, 2010; López-Pérez, 2010, 2012), we would expect that higher beliefs about the reporting of other participants are correlated with own high reporting.

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[8]We cannot rule out the possibility that, e.g., family members were in the same room with the participant. Behavior, however, does not differ between participants who live alone and those who do not.

[9]Actual anonymity is very high in the telephone study and clearly higher on the telephone than in the lab. Perceived anonymity can and will vary from actual anonymity, for example, participants might believe that someone calling their landline will also know their name or address (which was not the case). However, we don't see a clear reason why perceived anonymity should be higher in the lab than on the phone. The arguments above even suggest that perceived anonymity in the lab is lower than actual anonymity, increasing the telephone-lab difference in perceived anonymity. Either way, there is evidence that the degree of anonymity does not affect behavior much anyway. Fischbacher and Föllmi-Heusi (2013) conduct a double-blind version of their experiment in which both randomization and receiving payment are unobservable by the experimenter. Subjects roll a die in private, take the payment out of an envelope, and then put the envelope back into a box with other envelopes such that it is clear that payments and reports cannot be assigned to any individual. Behavior does not change compared to the baseline treatment, suggesting that (perception of) anonymity plays only a small role.

1.4 Results

1.4.1 Telephone Study

Result 1: *In 1-Coin-Telephone, the distribution of actual reports is very close to the truthful distribution; participants report the payoff-maximizing outcome slightly less often than expected if everyone reported truthfully. In 4-Coin-Telephone, the distribution of reports is indistinguishable from the truthful distribution.*

Figure 1.1 illustrates aggregate behavior (the dashed line corresponds to the expected distribution if every participant reported the true outcome of the coin toss). 55.6 percent of participants report heads as the outcome of the coin toss, yielding a payoff of zero, the remaining participants report tails yielding a payoff of 15 euros. The payoff-maximizing outcome is reported slightly less often than in 50 percent of the cases and although the difference is small in terms of effect size, it is significant (Binomial test, $p = 0.004$). Figure 1.2 shows aggregate behavior in 4-Coin-Telephone. Again, reporting behavior follows the expected distribution under complete honesty very closely (the dashed line corresponds to the truthful distribution). In fact, the distribution of reported outcomes is statistically indistinguishable from the truthful distribution (Kolmogorov-Smirnov test, $p = 0.61$; binomial tests of the expected against the observed frequency, all five $p > 0.13$). In particular, and unlike in 1-Coin-Telephone where "too many" people report the payoff-minimizing outcome, there is no significant over-reporting of zero in this treatment.[10] Looking at behavior in both treatments we can therefore summarize that the payoff-maximizing outcome is reported by much fewer participants than expected if no one incurred lying costs. It is also reported less often than suggested by previous lab experimental studies, which find some truth-telling but also many instances of the payoff-maximizing report. Instead, it is close to the distribution that would arise if every participant reported his or her type truthfully.[11]

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[10]While the sample size in 4-Coin-Telephone is substantially smaller than in 1-Coin-Telephone (94 vs. 658), the non-significance is not due to lack of power but rather due to the small effect size. If we (counterfactually) increase the sample size to 658 and assume the same shares of reports as in 4-Coin-Telephone, the distribution continues to be indistinguishable from the truthful distribution (Kolmogorov-Smirnov test, p=0.20).

[11]We can only speculate about why some people obviously falsely claim to be of the payoff-

Figure 1.1: Aggregate Behavior in 1-Coin-Telephone
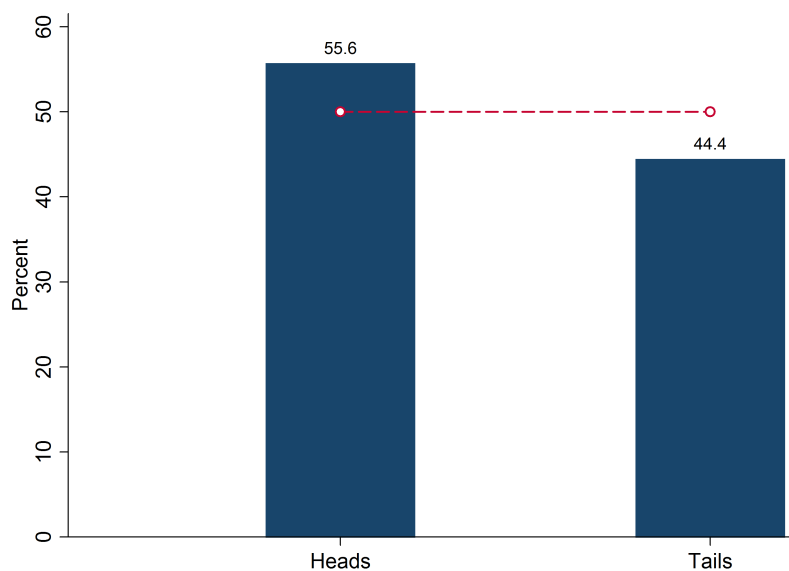
Reporting heads yielded no payoff; reporting tails yielded a payoff of 15 euros. The dashed line corresponds to the expected distribution if every participant reported the true outcome of their coin toss.

Figure 1.2: Aggregate Behavior in 4-Coin-Telephone
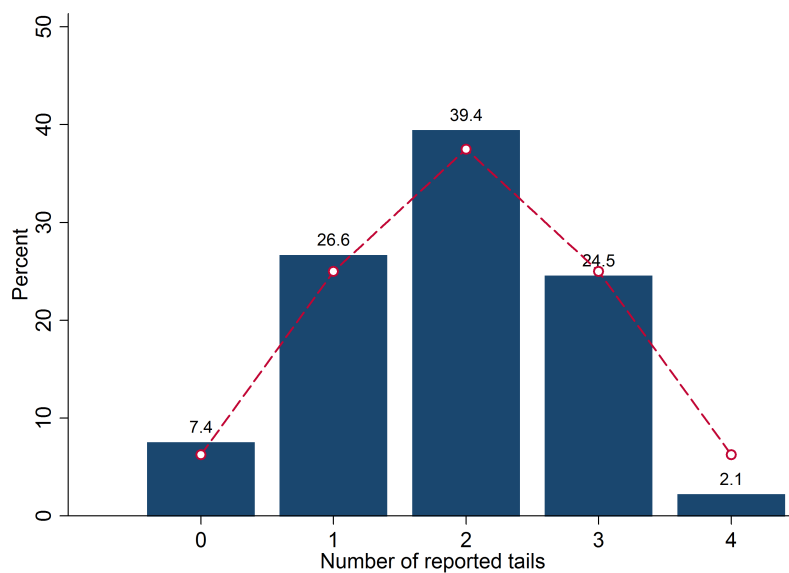
The payoff was 5 euros times the number of tails reported. The dashed line corresponds to the expected distribution if every participant reported the true outcomes of their coin tosses.

Previous studies have shown that truth-telling correlates with observable characteristics, e.g. gender or religiosity Dreber and Johannesson (2008); Houser, Vetter, and Winter (2012); Mazar, Amir, and Ariely (2008); Shariff and Norenzayan (2007). In contrast, if our conjecture that almost all participants report truthfully is correct, an individual's reported outcome will only be driven by their random coin toss; if this is the case, reporting cannot be correlated with any individual characteristic, as these are orthogonal to the chance move. Therefore, if we do not find such a correlation, our finding of (almost) complete honesty is supported. More specifically, we conduct regression analyses for the two experiments in order to examine whether there are systematic effects of individual characteristics on reporting behavior. First, we regress the report only on clearly exogenous variables such as age and gender, in a second step adding religious denomination. We then include income, the size of the city the individual lives in, and education dummies. Finally, we look at the effect of an individual's religiousness (interacted with denomination), their risk and trust preferences, and their belief about the reporting behavior of other participants.

Result 2: *There is no significant correlation between reporting behavior and any individual characteristic.*

First, we look for potential group differences in terms of reporting behavior in 1-Coin-Telephone by conducting Probit regressions of the reported outcome on the respective characteristics (see Table 1.2 in online appendix 1.6.4). No characteristic except for one's belief about others' behavior is significantly associated with reporting in the experiment: participants who think many other participants report tails dishonestly, are *less* likely to report tails themselves. This belief is, however, not significant if we include it as the only explanatory variable ($p = 0.15$). Note in par-

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minimizing type and why this only happens in 1-Coin-Telephone. The design of the experiment allows to rule out reputational concerns towards the interviewer as an important factor. Privacy concerns could drive this effect: reporting the type that gives zero payoff makes it unnecessary to hand over one's address. The reason why we do not observe such an effect in 4-Coin-Telephone might be that reporting zero to avoid handing over the address was less salient in this treatment. However, we ensured that privacy concerns were minimized in both treatments by giving participants the opportunity to receive the payment as a gift certificate code by email or directly via the phone. 17.2 percent of eligible participants chose this last payment option which made it unnecessary to hand over any additional contact details. Another possible explanation would be self-image concerns: refraining from easily and safely earning 15 euros could be a strong signal to oneself that one is not greedy and thereby flattering for one's self-image. This interpretation would be in line with how Utikal and Fischbacher (2013) interpret their finding that nuns lie to their monetary disadvantage. We will show more data below which strongly suggests that downward lying is not widespread in our study.

ticular that neither gender nor any religion-related variable is significantly correlated with reporting. Conducting the same regressions as in Table 1.2 using OLS leaves the results unchanged. Next, we check whether these results also hold in 4-Coin-Telephone. We run Ordered Logit regressions of the reported number of tails on the same explanatory variables as before. Table 1.3 in the online appendix illustrates the results from this estimation. Only the coefficient for trust is significant. This effect is, however, not robust to the inclusion of other explanatory variables. The effect is also not present in 1-Coin-Telephone. In contrast to 1-Coin-Telephone, the belief coefficient shows no significant association with reporting behavior in this treatment and the point estimate has the opposite sign. We will discuss the data on beliefs in more detail in Section 1.4.3.

Two further aspects of our analysis are worth noting. First, when running OLS regressions using the same predictor variables as above, we find that only one of the 10 specifications has an adjusted R^2 above 0 (at 0.0146), all other adjusted R^2 values are negative. Moreover, the resulting adjusted R^2 tend to decrease in the number of included variables. This again underlines our conclusion: the tested predictor variables do not increase explained variance in the dependent variable compared to pure chance. Second, we also test the correlations between reported number and answers to the survey questions that we did not include in the main specifications of Tables 1.2 and 1.3. These include a person's citizenship and country of birth, various personal characteristics, a person's current job or educational situation and their current or recent position in the professional hierarchy, a person's willingness to tell white lies in different situations, a person's family status and living situation (whether one lives with a partner and the number of people belonging to the household), the frequency of church attendance, a person's political preference, and the individual's tendency to behave in an opportunistic way as well as the belief about others' willingness to behave like that. Testing these variables as predictors in Probit and Ordered Logit regressions in the two different data sets, akin to Tables 1.2 and 1.3, we find no robust association between any of them and reporting behavior. In particular, this means that students and non-students do not behave differently in our sample. This holds when we consider current students or include former students as well (Kolmogorov-Smirnov tests, all $p > 0.409$). It is thus not a students

vs. non-students difference, e.g., a difference in education, age, cognitive skills, or socio-demographic background, which drives the difference between our results and previous lab experiments. Summing up, the overall picture is confirmed: no individual characteristic, whether exogenous or endogenous, is systematically associated with reporting behavior suggesting that almost all participants in our study tell the truth. It could still be that a subgroup of people, which we cannot identify with our background information, reports tails more often than actually true while another subgroup reports tails less often. This could result in the two effects offsetting each other, which would result in a similar picture of aggregate behavior. However, we consider this to not be likely as our analysis shows that this is not the case for any of the numerous subgroups that we can identify with our data. More importantly, such an effect would further need to recreate the distinct distributions of Figures 1.1 and 1.2 which is implausible.

1.4.2 Laboratory Experiment

To further investigate the motivations underlying behavior in the telephone study, we conducted two 4-coin treatments as laboratory experiments. We will first discuss the 4-Coin-Lab-Tel treatment which keeps the mode of communication as in the telephone study: subjects had to report their result over the phone directly to an experimenter.[12] Subsequently, we compare this treatment to 4-Coin-Lab-Click, in which subjects reported their number by clicking a button on a computer screen as in previous lab experiments. This second comparison will allow us to disentangle the influence of the mode of communication.[13]

> **Result 3:** *Subjects in 4-Coin-Lab-Tel report substantially higher numbers than subjects in 4-Coin-Telephone.*

The upper panel of Figure 1.3 shows aggregate behavior in 4-Coin-Lab-Tel: most subjects refrain from reporting the maximal outcome, forgoing on average 6.83 eu-

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[12]It was obvious to the subjects that the experimenter on the phone was not the same person as the experimenter in the lab, since the experimenter in the lab coordinated the procedure of calling subjects one-by-one into the separate room with the phone.

[13]We asked subjects to toss the coin four times instead of only once, to be able to replicate non-maximal overreporting, one of the main results of Fischbacher and Föllmi-Heusi (2013). See Houser, Vetter, and Winter (2012) and Bucciol and Piovesan (2011) for studies with a single coin toss; both also find significant overreporting.

ros, quite a considerable amount compared to the average hourly student wage in Germany of about 10 euros. At the same time, behavior is significantly different from the distribution expected under truthful reporting, the dashed line in the figure (Kolmogorov-Smirnov test, $p < 0.001$; binomial tests, all five $p < 0.009$). This replicates previous findings in the lab: many subjects lie but often not maximally. Reporting behavior also deviates strongly from what we have observed in the telephone study: reports are significantly higher in 4-Coin-Lab-Tel than in 4-Coin-Telephone. In Table 1.1, columns 1 and 2, we regress the reported number of tails on a dummy for being in the lab, a dummy for 4-Coin-Lab-Click and controls for age and gender. The lab dummy is highly significant.[14] We find the same result if we compare 4-Coin-Telephone and 4-Coin-Lab-Tel using a two-sample Kolmogorov-Smirnov test ($p < 0.001$). These results demonstrates that our 4-coin randomization mechanism does not drive the truthful behavior in 4-Coin-Telephone and that, by moving our telephone setup to the laboratory, we are able to strongly change behavior (as we showed within the telephone study, this is not driven by subjects being students per-se). How big is the additional effect if we also change the mode of communication?

> **Result 4:** *Subjects in 4-Coin-Lab-Click report slightly higher numbers than subjects in 4-Coin-Lab-Tel but this difference is not statistically significant. Only the report of 4 occurs significantly more often in 4-Coin-Lab-Click; the reports of 0, 1, 2, and 3 are not different across treatments. Reports in 4-Coin-Lab-Click are significantly higher than in 4-Coin-Telephone.*

The lower panel of Figure 1.3 shows aggregate behavior in 4-Coin-Lab-Click. The distribution of reports is very similar to the one in 4-Coin-Lab-Tel, the average report being only slightly higher (2.78 in Click vs. 2.64 in Tel). The overall distribution and the average report are not significantly different across the two treatments (two-sample Kolmogorov-Smirnov test, $p = 0.136$; Ordered Logit in Table 1.1, columns 1 and 2, both $p > 0.096$). The share of subjects reporting 0, 1, 2 or 3 are also not significantly different (tests of proportion, all $p > 0.101$). However, subjects in 4-Coin-Lab-Click report 4 significantly more often ($p = 0.007$).[15] At the same

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[14]We use Ordered Logit regressions in Table 1.1. All results, including the ones discussed below, also obtain when we use OLS instead.

[15]Two subjects in 4-Coin-Lab-Click told us that they "accidentally clicked the wrong button" and thus wanted to change their report; both subjects wanted to reduce their report, one subject

Figure 1.3: Aggregate Behavior in the 4-Coin-Lab-Tel (upper panel) and 4-Coin-Lab-Click treatments (lower panel)
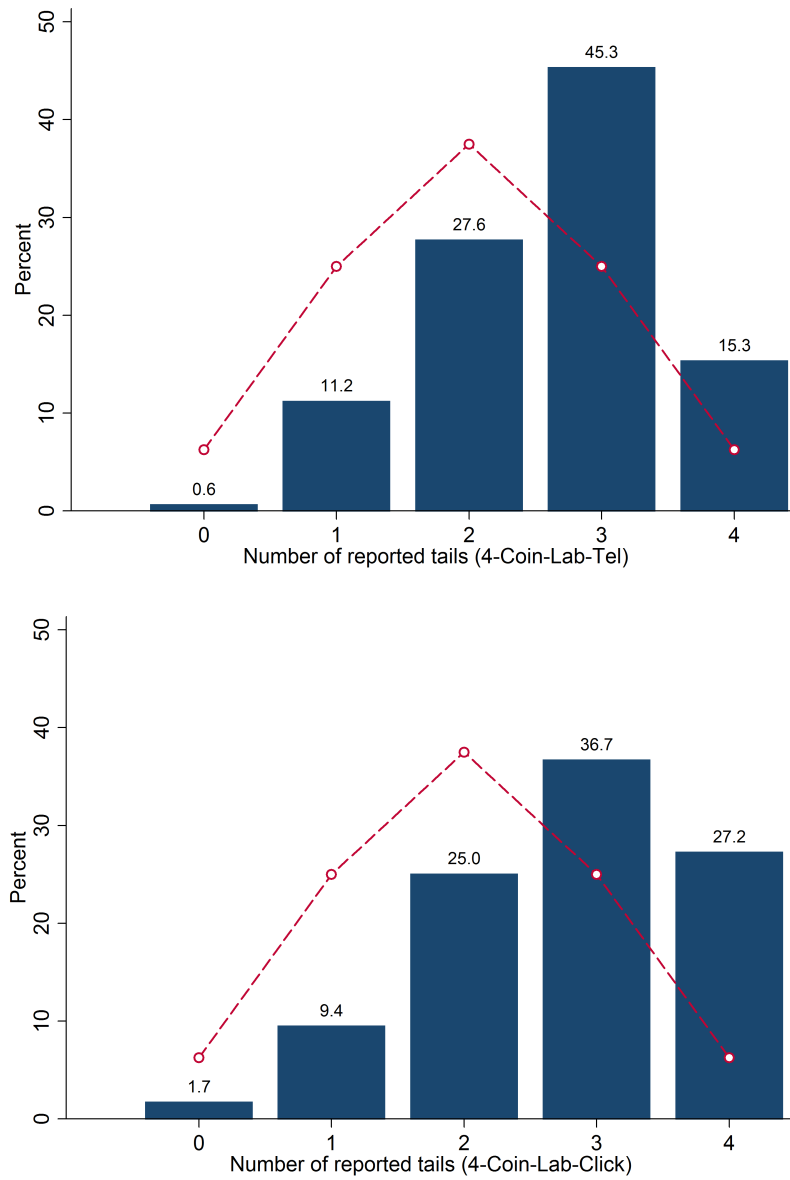
The payoff was 5 euros times the number of tails reported. The dashed line corresponds to the expected distribution if every participant reported the true outcomes of their coin tosses.

Table 1.1: Comparison of 4-Coin Treatments

Dependent Variable:	Number of Reported Tails (0–4)				Belief about other participants	
	(1)	(2)	(3)	(4)	(5)	(6)
1 if Either Lab treatment	1.370***	1.079***	1.242***	1.260***	0.074**	-0.072
	(0.225)	(0.289)	(0.246)	(0.334)	(0.035)	(0.060)
1 if 4-Coin-Lab-Click	0.334*	0.307	0.230	0.204	0.098***	0.100***
	(0.201)	(0.203)	(0.204)	(0.206)	(0.031)	(0.030)
1 if Female		-0.345*		-0.371*		-0.017
		(0.183)		(0.190)		(0.028)
Age		-0.013		0.001		-0.006***
		(0.009)		(0.010)		(0.002)
Belief about other participants			2.114***	2.115***		
			(0.356)	(0.358)		
N.Obs.	444	443	425	424	425	424

Notes: Ordered Logit Estimates (columns 1–4) and Tobit estimates (columns 5–6). Robust standard errors are in parentheses. The sample includes all 4-coin treatments, i.e., 4-Coin-Telephone, 4-Coin-Lab-Tel, and 4-Coin-Lab-Click. "Belief about other participants" is the belief of this participant about the share of participants who report to have tossed more tails than they actually did and who report 4 tails (see text for details about the question). Significance at the 1, 5, and 10 percent level is denoted by ***, **, and *, respectively.

time, behavior in 4-Coin-Lab-Click is markedly different from 4-Coin-Telephone (two-sample Kolmogorov Smirnov test, $p < 0.001$, Ordered Logit in Table 1.1, columns 1 and 2, F-test, both $p < 0.001$). Overall, our data thus show that the mode of communication does not have a strong effect on behavior and cannot explain the difference between our telephone study and previous lab experiments. This result is further confirmed by Waubert De Puiseau and Glöckner (2012) who also find considerable truth-telling at home, though not as extreme as in our data, using an online panel in which participants answered questions at home by clicking on a computer screen. Houser, Vetter, and Winter (2012) conduct a 1-coin lab experiment and find similar levels of lying as in our lab experiments, replicating the other side of our results.

One could think that one reason why behavior in the telephone study differs is a perceived time pressure on the telephone which might make lying more difficult. However, we measure response times in the laboratory and do not find a correlation with the report (Ordered Logit, $p = 0.108$).[16] If anything, the report in the lab is higher for short decision times. This mirrors results of Shalvi, Eldar, and Bereby-Meyer (2012) who impose exogenous time pressure in a similar lab experiment and who find that subjects become less honest under time pressure. Taken together, these results suggest that behavior in the telephone study is not driven by perceived time pressure. We also find no correlation of the number of previous participations in other lab experiments with reporting behavior in the lab ($p = 0.829$), suggesting that the limited experience participants of the telephone study have with the abstract design of economics experiments does not play a role. It rather seems that different norms apply when making a reporting decision at home, representing a private and familiar environment, compared to the in lab where other, more selfish norms might be triggered.[17]

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from 4 to 2 and the other from 4 to 0. The data shown here includes their final report as they received this report as payoff. Results stay very similar when we consider their initial click.

[16]We restrict the sample to 4-Coin-Lab-Click as the response time is measured very noisily in 4-Coin-Lab-Tel where we cannot distinguish the actual decision time from the walking to the next room, reporting, and coming back to the cubicle.

[17]Our lab and field experiments differ in a couple of other respects which we cannot disentangle: subjects in the lab, for example, know that other subjects are in the same room, even though they are separated by walls and curtains, while at least some telephone participants will be alone; this might lead to different norms being triggered as suggested above. Furthermore, the telephone survey came as a surprise to participants while subjects in the lab experiment signed up in advance and

We showed above that women do not report differently from men in the telephone study. As one can see from Table 1.1, women do report lower numbers in the lab. This effect is only weakly significant in the sample of all three 4-coin treatments, i.e., also including 4-Coin-Telephone which dilutes the effect, and becomes significant if we restrict the sample to the two lab treatments ($p = 0.027$ and $p = 0.046$ in regressions akin to columns 2 and 4 of Table 1.1).

1.4.3 Beliefs about other Participants

Previous studies (e.g., López-Pérez, 2010; Diekmann, Przepiorka, and Rauhut, 2015) have investigated the relationship of reporting behavior and the beliefs about what other people report. Since our telephone and lab settings generate strong differences in reporting behavior, we next examine whether there is a similar difference in beliefs and whether this could help explain the differences in behavior.

In all four treatments, we elicited beliefs about the reporting behavior of the other participants. We will mainly focus on analyzing beliefs in the 4-coin treatments as the outcome variable is richer and we have additional treatments. In the 4-coin treatments, subjects were asked two questions regarding their beliefs about the behavior of other subjects in their treatment (the question referred to 1000 participants in 4-Coin-Telephone): "We are conducting this experiment also with 100 other participants. How many of these 100 participants do you think report tails more often than they actually tossed?" and "How many of these X overreporting participants do you think report that they tossed tails in each of the four coin tosses?"[18] We will use the answers as direct measure of the belief about the share of liars and about the share of maximal liars.

Result 5: *In all treatments, participants believe others to overreport more than they actually do.*

Figure 1.4 compares average beliefs with average actual behavior for each treatment. We take as variable of interest the share of participants who report the

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expected to participate and to earn money. Abeler (2013) explores the interaction of expectations and honesty and suggests that higher expectations could lead to less honesty, in line with our results.

[18]In 1-Coin-Telephone, we only asked one question: "How many of the participants report tails although they tossed heads?"

Figure 1.4: Share of maximal reports across treatments
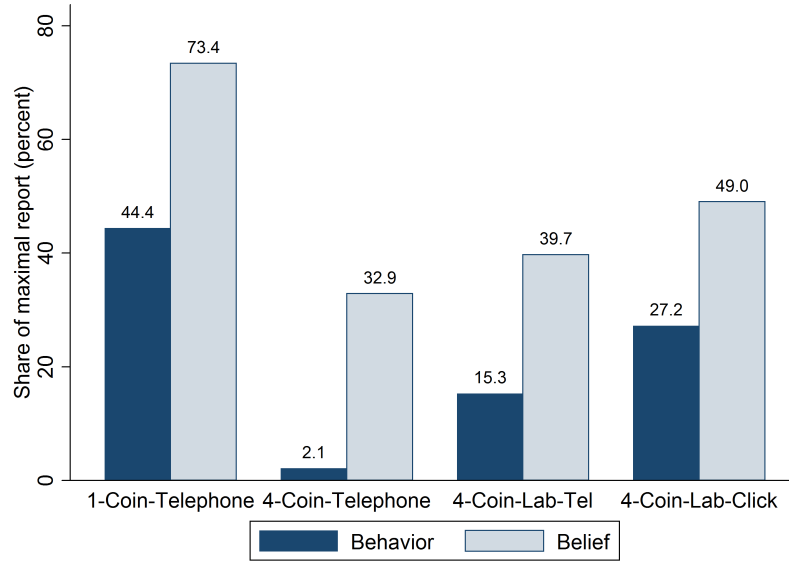
The maximal report is 4 in the 4-coin treatments and 1 in 1-Coin-Telephone. The dark bars depict actual behavior. The light-colored bars depict the average belief of participants in each treatment about the behavior of the other participants in their treatment.

payoff-maximizing outcome, i.e., 4 tails in the 4-coin treatments and tails in 1-Coin-Telephone. Since we expected participants to be unfamiliar with the true distribution of the sum of four coin tosses, we didn't ask directly for their belief about this share. We are able to calculate it, given the assumption of convex lying costs, from the two questions for the 4-coin treatments: it is the share of liars who report 4 (question 2) plus the share of honest 4's (the probability of a true 4 times (1 - answer1)). Since we do not observe whether any individual overreports we cannot directly compare the two answers to actual behavior.[19] We find in all four treatments that participants believe that others overreport more than they actually do. The differences are highly significant (t-tests, all $p < 0.001$). The same results obtain when we consider the average reported number as variable of interest.[20]

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[19] In Figure 1.4 we assume that subjects in 1-Coin-Telephone expect all tossed tails to be reported as tails.

[20] If some participants care about the distribution of behavior among all participants, i.e., a kind of group reputation, the wrong belief could be a potential reason for why we find that some people lie to their monetary disadvantage in 1-Coin-Telephone: their behavior could be motivated by a desire to compensate for others' behavior whom they (falsely) believe to be lying. In 4-Coin-Telephone, such a strategy is not fruitful as too many 0s would not help the group reputation.

What shapes these beliefs?

Result 6: *Older participants believe others to overreport less. Participants expect more overreporting when participants can enter their report by clicking on the screen.*

In Table 1.1, columns 5 and 6, we regress the answer to the second question on treatment dummies, a gender dummy and age (the table only considers the 4-coin treatments). We find that subjects in 4-Coin-Lab-Click believe others to overreport more than subjects in 4-Coin-Lab-Tel. Being in the laboratory seems to increase beliefs (column 5) but this effect goes away once we control for age (column 6). Participants in the telephone study are on average much older than the student sample in the lab and older participants expect others to overreport less. This means that the beliefs of older participants in the telephone study are closer to actual behavior than the beliefs of younger participants. The same effect of age is present in 1-Coin-Telephone ($p < 0.001$). Using the answer to the first question, or the belief about the average report or the belief about the share of participants reporting 4 does not change any of the results.

Result 7: *In the lab, participants who believe that others report high numbers also report higher numbers themselves.*

We discussed above that there is no robust correlation between reports and beliefs in the telephone study. In Table 1.1, columns 3 and 4, we study the correlation of reports and beliefs for the 4-coin treatments in lab and field. We regress the reported number of coin tosses on treatment dummies, controls for gender and age and on the answer to the second belief question. We find that participants who believe others to report high numbers also report higher numbers themselves. If we exclude 4-Coin-Telephone from the analysis, the coefficient on the belief variable becomes even bigger and stays significant. One could interpret this finding as yet another indication that almost all participants are honest in the telephone study because, if some were not, we should also find a correlation with beliefs in the telephone study (similar to the gender effect we do not find). Furthermore, since beliefs are on average higher in 4-Coin-Tel-Click, the difference between the two lab treatments—which is barely significant in column 1—becomes even smaller once we control for beliefs. These results are again robust to the exact belief measure we use.

The direction of causality between beliefs and behavior is obviously unclear in our setting. On the one hand, it could be that a high belief induces participants to also report higher numbers. This would be in line with a notion that moral norms are endogenous to the beliefs people hold about the behavior of their peers (see, e.g., Traxler, 2010; López-Pérez, 2010, 2012). Diekmann, Przepiorka, and Rauhut (2015) provide causal evidence that higher beliefs lead to higher reports. If this is the mechanism for the correlation between beliefs and behavior, it is even more surprising that participants, in particular in the telephone study, decided to refrain from exploiting the opportunity to receive a considerable amount of money when they believed that many others would do so. On the other hand, the causality might run in the opposite direction if participants ex-post justify their own high report with a stated belief that others also overreport.

1.5   Conclusion

Using a representative sample of the German population we conducted telephone interviews during which respondents participated in an incentivized experiment. Depending on the treatment, they could earn money by reporting tails as the outcome of one or four coin tosses. We find that almost all participants report their coin toss(es) honestly: the distributions of reports are extremely close to the true distribution of a fair coin toss or four coin tosses, respectively. Moreover, reports are not correlated with any individual characteristic, including gender which has been shown tp predict honesty in previous lab studies. We conduct additional laboratory experiments to study the motives underlying the behavior on the phone. While reports are generally higher in the lab than in the telephone study we find little evidence that the mode of communication (reporting directly to someone via the phone vs. clicking a number on a computer) influences behavior. Being a student has also no effect.

Our results underline doubts about the generalizability of economic models which assume that people always lie maximally when it is financially beneficial. Apparently, people do not only care for the trade-off between financial gains from misreporting and the monetary fines when misreporting is detected (cf. Becker, 1968). Our results instead support models like Erard and Feinstein (1994), Kartik, Tercieux, and Holden

(2014) or Doerrenberg, Duncan, Fuest, and Peichl (2014) which assume that many people do not lie or do not lie maximally; intrinsic lying costs could be a potential microfoundation for these and similar models. The effect of patriotism and religiosity on tax morale (Konrad and Qari, 2012; Torgler, 2006), for example, could also work through an increased lying cost.

The strong differences we find between telephone and lab environment suggest that lying costs are stronger in our setting outside the lab. It seems that different norms apply when reporting private information at home. Similarly, it might be that the familiar and intimate environment of one's own home reinforces one's personal identity and renders personal moral standards more salient. This is in line with recent evidence by Cohn, Maréchal, and Noll (2015) who conduct a similar experiment with prisoners. They find that priming prisoners with their criminal identity reduces honesty. Lab experiments, in turn, could be more representative of decisions for which people take on a particular role or identity in addition to their private identity.

At the same time, this study does not imply that everybody always reports their private information truthfully. The level of lying costs seems rather to be influenced by the context in which people are asked to report their type (see also Mazar and Ariely, 2006; Mazar, Amir, and Ariely, 2008). The difference in behavior on the phone and in the lab shows how malleable reporting behavior can be. Our results therefore point to important policy implications: institutions, e.g., tax authorities, could make use of the context dependence of reporting behavior when designing decision-making environments. As we find strong evidence for widespread lying costs, appropriate mechanisms might be much less complex than those resulting when assuming that agents have no qualms about lying. It might be possible to change reporting behavior in simple and low-cost ways in the spirit of libertarian paternalism (Thaler and Sunstein, 2003). Further research is necessary to uncover what the crucial aspects of the decision-making environment are that induce truth-telling.

1.6 Appendix to Chapter 1

1.6.1 Experimental Instructions (1-Coin- and 4-Coin-Telephone)

These are the instructions for 1-Coin-Telephone translated into English. The instructions for 4-Coin-Telephone were adapted according to the rules of 4-Coin-Telephone but otherwise identical.

The experiment's purpose is without any exception scientific, and all legal regulations of data protection are strictly respected and the anonymity of the data analysis is fully warranted. Your participation in the experiment is, of course, on a voluntary basis. Infas and the University of Bonn guarantee the correct and proper payment of the achieved amount of money.

In case the participant had doubts about any part of the experiment he or she was given the phone number of the supervisor of the study at Infas, so that he or she could make sure that everything was legitimate.

You can receive the payment in cash via regular mail (we will then need your address), or we will give you a gift certificate code which equals the respective amount via email or via phone. The gift certificate is valid for a purchase at Amazon. The address will only be used for mailing the payment of the money you earned and will be deleted from our database afterwards. You will need a coin to take part in the experiment (any coin). Please get the coin first. I will then explain the details of the experiment.

Now, I will explain the experiment. Your coin has one side showing a number, and another one, often showing some image. After receiving the instructions, I will ask you to toss the coin, and you will have to tell me afterwards which side of the coin came up. You receive 15 euros if "tails" comes up.[21] You receive 0 euros if "heads" comes up.

The interviewers were instructed to react with the following sentence to any comments or questions that could possibly influence the participants decision: "I can only give you the instructions. What side of the coin you report to me, is completely up to you." They were also reminded, to react neutrally towards any comment by the participant concerning the number they reported, and to refrain from making any

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[21]The German word for "tails" is "Zahl", literally "number".

comments or jokes.

Once again: You toss the coin and tell me which side came up. If "tails" came up, you receive 15 euros. Please don't start just yet. Do you understand the rules?

If participant said that he or she understood the rules, the interviewer continued with the experiment.

Please put aside your telephone receiver (but please do not hang up), and toss the coin. Afterwards please tell me which side of the coin came up. Please toss the coin now.

In case the participant hung up by accident he or she was called again immediately.

If necessary, the interviewer read the following question: Which side came up?

1.6.2 Experimental Instructions (4-Coin-Lab-Tel)

First screen

Please take now the coin to hand that you should find in your cubicle. If you can't find the coin, please notify the experimenter.

Your coin has one side, showing a number, and another one, often showing some image (tails and heads). After receiving the instructions, you will be asked to toss the coin, and you will have to report afterwards how often the coin came up with tails. For every time tails you will receive 5 euros.

You receive 0 euros if tails came never up; you receive 5 euros if tails came up once; 10 euros if it came up twice; 15 euros if it came up three times; and 20 euros if it came up four times. This payment will be in addition to the payments that you will receive for the previous experiment.

Once again: You toss the coin four times, count how often tails came up and then report how often tails came up. You'll receive 5 euros per times tails.

You will report the number of tails in the adjacent room via telephone: after you tossed the coin four times, notify the experimenter who will guide you to the adjacent room. There will be a telephone connecting you to another experimenter who will take your report. The payments due will be transmitted to us at the end of this experiment such that you will receive the payment for both of today's experiments at the end of the experiment.

Please don't start just yet.

If you have understood the rules, click "Next". Otherwise, please notify the experimenter.

Second screen

Please toss the coin four times now and remember how often tails came up.

Then notify the experimenter to make your report via telephone.

As soon as you are back from the telephone, click "Next".

1.6.3   Experimental Instructions (4-Coin-Lab-Click)

First screen

Please take now the coin to hand that you should find in your cubicle. If you can't find the coin, please notify the experimenter.

Your coin has one side, showing a number, and another one, often showing some image (tails and heads). After receiving the instructions, you will be asked to toss the coin, and you will have to report afterwards how often the coin came up with tails. For every time tails you will receive 5 euros.

You receive 0 euros if tails came never up; you receive 5 euros if tails came up once; 10 euros if it came up twice; 15 euros if it came up three times; and 20 euros if it came up four times. This payment will be in addition to the payments that you will receive for the previous experiment.

Once again: You toss the coin four times, count how often tails came up and then report how often tails came up. You'll receive 5 euros per times tails.

Please don't start just yet.

If you have understood the rules, click "Next". Otherwise, please notify the experimenter.

Second screen

Please toss the coin four times now. Then report how often tails came up.

How often did tails came up?

[Five buttons 0 to 4]

1.6.4 Additional Regression Tables

Table 1.2: Covariates in 1-Coin-Telephone

Dependent Variable: 1 if Reported Tails

	(1)	(2)	(3)	(4)	(5)
Age	0.002	0.002	0.002	0.002	0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
1 if Female	0.041	0.040	0.079*	0.065	0.066
	(0.039)	(0.039)	(0.047)	(0.048)	(0.049)
1 if Protestant		0.010	0.021	-0.038	-0.084
		(0.046)	(0.056)	(0.140)	(0.141)
1 if Catholic		0.006	-0.004	-0.004	-0.064
		(0.048)	(0.057)	(0.150)	(0.152)
Income			0.002	0.003	0.004
			(0.005)	(0.005)	(0.005)
1 if Professional Education			0.041	0.053	0.056
			(0.062)	(0.062)	(0.063)
1 if Academic Education			-0.016	-0.009	-0.015
			(0.055)	(0.056)	(0.057)
City Size			0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)
Religiousness				0.029	0.023
				(0.020)	(0.020)
Religiousness*Catholic				-0.010	0.004
				(0.035)	(0.036)
Religiousness*Protestant				0.005	0.013
				(0.034)	(0.035)
Risk Tolerance					-0.003
					(0.018)
Trust					-0.028
					(0.018)
Belief about other					-0.218**
Participants					(0.092)
N.Obs.	658	658	465	464	454

Notes: Probit Estimates. Marginal effects are shown, robust standard errors are in parentheses. "Risk tolerance" is the answer to the general risk question of the GSOEP ("How do you consider yourself? Are you in general a rather risk-loving person, or do you try to avoid risks? Use a scale from 1, meaning that you are not at all willing to take risks, to 7, meaning that you are absolutely willing to take risks.") and "trust" is the answer to the WVS trust question ("Generally speaking: Do you think one can trust other people, or that one should rather be careful when dealing with other people? Please indicate your answer on a scale from 1 to 7, with 1 meaning that one should be careful when dealing with other people, and 7 meaning that one can trust other people."). "Belief about other participants" is the belief of this participant about the share of other participants who reported "tails" while actually "heads" came up. Significance at the 1, 5, and 10 percent level is denoted by ***, **, and *, respectively.

Table 1.3: Covariates in 4-Coin-Telephone

Dependent Variable: Number of Reported Tails (0–4)

	(1)	(2)	(3)	(4)	(5)
Age	-0.015	-0.017	0.006	0.008	-0.013
	(0.012)	(0.012)	(0.020)	(0.023)	(0.027)
1 if Female	0.120	0.159	0.177	0.072	0.075
	(0.389)	(0.399)	(0.496)	(0.526)	(0.573)
1 if Protestant		-0.099	0.073	-1.052	-1.588
		(0.473)	(0.622)	(1.258)	(1.269)
1 if Catholic		0.292	0.703	-0.407	-1.425
		(0.452)	(0.565)	(1.373)	(1.520)
Income			0.039	0.042	0.025
			(0.176)	(0.184)	(0.211)
1 if Professional Education			-0.591	-0.721	-0.759
			(0.587)	(0.620)	(0.693)
1 if Academic Education			-0.640	-0.757	-0.764
			(0.697)	(0.693)	(0.729)
City Size			-0.002	-0.002	-0.002
			(0.002)	(0.002)	(0.002)
Religiousness				-0.271	-0.289
				(0.238)	(0.250)
Religiousness*Catholic				0.328	0.475
				(0.318)	(0.345)
Religiousness*Protestant				0.336	0.395
				(0.283)	(0.276)
Risk Tolerance					-0.324
					(0.251)
Trust					0.491**
					(0.215)
Belief about other					0.715
Participants					(1.311)
N.Obs.	94	94	62	62	60

Notes: Ordered Logit Estimates. Robust standard errors are in parentheses. "Risk tolerance" is the answer to the general risk question of the GSOEP ("How do you consider yourself? Are you in general a rather risk-loving person, or do you try to avoid risks? Use a scale from 1, meaning that you are not at all willing to take risks, to 7, meaning that you are absolutely willing to take risks.") and "trust" is the answer to the WVS trust question ("Generally speaking: Do you think one can trust other people, or that one should rather be careful when dealing with other people? Please indicate your answer on a scale from 1 to 7, with 1 meaning that one should be careful when dealing with other people, and 7 meaning that one can trust other people."). "Belief about other participants" is the belief of this participant about the share of participants who report to have tossed more tails than they actually did and who report 4 tails (see text for details about the question). Significance at the 1, 5, and 10 percent level is denoted by ***, **, and *, respectively.

Chapter 2

The Relationship Between Economic Preferences and Psychological Personality Measures

2.1 Introduction

Both economists and personality psychologists seek to identify determinants of heterogeneity in behavior. Economists typically depict decision problems in a framework of utility maximization. An individual's utility is shaped by preferences such as risk, time, and social preferences.[1] These preferences, in combination with expectations of future events, perceptions, beliefs, strategic consideration, prices and constraints shape behavior. Personality psychology, the branch of psychology studying personality and individual differences, offers several frameworks describing universal traits and individual differences. Personality traits – defined by Roberts (2009, p. 140) as "the relatively enduring patterns of thoughts, feelings, and behaviors that reflect the

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[1]In the standard expected utility framework, risk preference is captured by the curvature of the utility function, whereas the degree of risk aversion is represented in the concavity of the utility function (e.g. Gollier, 2001). Time preference describes how an individual trades off utility at different points in time (Samuelson, 1937; Frederick, Loewenstein, and O'Donoghue, 2002). Social preferences capture the idea that an individual's utility does not depend only on his own material payoff, but that it is also shaped by others' behavior and material payoff. Social preferences include altruism (e.g. Eckel and Grossmann, 1996) and negative and positive reciprocity (e.g. Falk and Fischbacher, 2006). Finally, trust describes an individual's belief about others' trustworthiness combined with a preference to take social risks (e.g. Fehr, 2009). Another important economic preference is the preference for work versus leisure. This preference is difficult to measure in experiments and is therefore not part of our analysis.

tendency to respond in certain ways under certain circumstances" – are important determinants of personality (Roberts, 2006) and affect outcomes. There has been a long tradition in personality psychology to measure personality traits. The Big Five or five-factor model is the most widely used taxonomy of personality traits. It originates from the lexical hypothesis of Allport and Odbert (1936), which postulates that individual differences are encoded in language (see Borghans et al. 2008). After years of research in this tradition, psychologists have arrived at a hierarchical organization of personality traits with five traits at the highest level. These Big Five traits, which are commonly labeled as openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism, capture personality traits at the broadest level of abstraction. Each Big Five trait condenses several distinct and more narrowly defined traits. It has been argued that the bulk of items that personality psychologists have used to measure personality can be mapped into the Big Five taxonomy (see, e.g., Costa and McCrae, 1992)).[2] Another important concept in psychology focusing on individual beliefs and perceptions is the locus of control framework by Rotter (1966). It represents the framework of the social learning theory of personality and refers to the extent people believe they have control over events.

An integration of the different measures and concepts used by economists and personality psychologists promises much potential for amalgamating evidence about the drivers of human behavior which accumulated disjointedly in the fields of economics and psychology (Borghans, Duckworth, Heckman, and ter Weel, 2008). Recently, scholars have begun to integrate personality into economic decision making (e.g., Borghans et al., 2008). Almlund, Duckworth, Heckman, and Kautz (2011) enrich theory by incorporating personality traits in a standard economic framework of production, choice, and information. Their model interprets measured personality as a "construct derived from an economic model of preferences, constraints, and information" (Almlund et al., 2011, p. 3). However, empirical knowledge is too limited to judge how personality traits relate to the concepts and parameters economists typically model to predict behavior.

To shed more light on the relationship between economic preferences and psy-

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[2]For a more detailed description of the research on the development of the Big Five, criticism of the approach and alternative measurement systems see Borghans, Duckworth, Heckman, and ter Weel (2008).

chological measures of personality we therefore study how key economic preferences, such as risk, time and social preferences, are linked to conventional measures of personality, such as the Big Five and locus of control. We analyze this relationship in a coherent framework using two main approaches. The first approach focuses on assessing the magnitude of the correlations between psychological and economic measurement systems in three unique data sets. The second approach departs from the fact that both preference measures and measures of personality traits predict a wide range of important life outcomes. If these two measurement systems are closely linked, they are expected to be substitutes in explaining heterogeneity in behavior. If, however, preferences and personality traits capture different aspects of behavior, the two measurement systems may have complementary predictive power for important life outcomes. We therefore evaluate the individual as well as the joint explanatory power of economic preferences and psychological measures of personality in explaining health, educational and labor market outcomes.

We use three complementary datasets. First, we look at data from laboratory experiments. Using a student subject pool we conducted choice experiments on key economic preferences, namely risk taking, time discounting, altruism, trust, and positive and negative reciprocity. We incentivized decision-making and obtained multiple behavioral measures for each preference. We assessed the Big Five domains using the 60-item NEO-FFI (NEO Five Factor Inventory) (Costa and McCrae, 1989) and a 15-item subset, the so-called BFI-S (Gerlitz and Schupp, 2005). We also measured the locus of control using 10 items adapted from Rotter (1966). Our second data set comprises very similar incentivized experimental measures with respect to risk taking and time discounting using a representative sample of almost 1000 participants from the German population. We are therefore able to obtain incentivized preference measures for a representative population. Personality was assessed using the BFI-S. The third data set stems from the German Socio-Economic Panel Study (SOEP), comprising preference and personality measures for a representative sample of more than 14.000 individuals. Preference measures were obtained using subjective self-assessment survey items rather than incentivized experiments, and personality was measured by using the BFI-S and the locus of control questionnaire. Using this data set we analyze associations between important life outcomes, such as labor market

36

success, subjective health status or life satisfaction, and individuals' preferences and personalities.

These three data sets allow for a comprehensive analysis. The first data set contains very detailed personality measures in combination with multiple experimental indicators for preferences. This student sample therefore provides a particularly accurate assessment of potential relations between economic preferences and personality. The second data set uses experimental measures for a limited set of preferences and a shorter version of the Big Five but a representative sample. A comparison of results of the two data sets therefore informs us about the generalizability of our findings from the student sample. The third data set additionally allows us to study an even larger sample and to explore the explanatory power of personality and preferences for important life outcomes.

We start by analyzing data on 489 university students. We relate all five factors that capture personality according to the Big Five taxonomy and the measure of Locus of Control to our experimental preference measures. We generally find only small correlations between personality traits and preferences. In particular, only 11 of the 36 correlations in our student sample exceed 0.1 in absolute value and only one correlation exceeds 0.2 in absolute value. These eleven correlation coefficients are all significant at conventional levels, and eight of them involve correlations between social preferences and personality traits.

Next, we gauge whether the correlation patterns generalize to representative samples. We first turn to the data set that contains very similar experimental measures of risk and time preferences and survey measures of the Big Five approximately 1000 individuals, who were sampled to be representative of the adult population living in Germany (see Dohmen, Falk, Huffman, and Sunde, 2010). The correlation structure between personality traits and risk and time preferences turns out to be similar to the one we find for students, with few exceptions.

Finally, we assess whether the empirical associations between preference parameters and personality traits are sensitive to the way in which preferences are measured. We compare correlations between personality traits and measures of preferences derived from the incentivized choice experiments in the student and the representative sample to correlations that are constructed based on the non-incentivized subjective

self-assessments in a representative sample of 14.000 individuals from the SOEP. Our result on the pattern of correlations between preference measures and personality measures is again largely confirmed.

We then turn to a different type of analysis in which we assess the power of preferences and personality in explaining life outcomes, including health, life satisfaction, earnings, unemployment and education. Our analysis reveals that both measurement systems have similar explanatory power when used separately as explanatory variables. The explained fraction of variance increases by approximately 60% when life outcomes are regressed on both measurement systems. We therefore conclude that each measurement system captures distinct sources of the heterogeneity in life outcomes. A coherent picture emerges from our analysis. Both approaches strongly suggest that standard measures of preferences and personality are complementary constructs.

So far no clear picture concerning the relations between measures of personality and economic preferences has emerged in the literature (see Almlund, Duckworth, Heckman, and Kautz, 2011). For example, the study by Daly, Delaney, and Harmon (2009) suggests a negative relationship between conscientiousness and the discount rate, but such a negative correlation is not corroborated by Dohmen, Falk, Huffman, and Sunde (2010), who relate experimental measures of willingness to take risk and impatience to survey measures of the Big Five in a representative sample of adults living in Germany, nor by Anderson, Burks, DeYoung, and Rustichini (2012), who relate a measure of delay acceptance to four of the Big Five domains in a sample of 1065 US trainee truckers.[3] In fact, Dohmen, Falk, Huffman, and Sunde (2010) find no significant relationship between personality traits and preference measures in a regression framework that includes controls for IQ, gender, age, height, education, and household income. Raw correlations between preference and personality measures, which are also reported in Almlund, Duckworth, Heckman, and Kautz (2011), are weak; time preference is significantly correlated only to agreeableness (at the 10 percent level).[4] This finding is confirmed by the significant correlation between delay acceptance and agreeableness in the truck-driver sample of Anderson, Burks,

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[3]The effect sizes of the correlations between preference and personality measures are all smaller than 0.1 in absolute value.

[4]We report this data in Table 2.3.

DeYoung, and Rustichini (2012).

Evidence on the link between risk preferences and the Big Five domains is equally mixed. Raw correlations between a lottery-choice measure of risk preference and personality traits in the data from Dohmen, Falk, Huffman, and Sunde (2010) indicate significant relationships between risk preferences and openness to experience (at the 1 percent level) and agreeableness (at the 5 percent level). Anderson, Burks, DeYoung, and Rustichini (2012) do not measure openness to experience. They do not find a significant correlation for risk preference and agreeableness, but report a weak correlation between risk preference and neuroticism (0.05 in absolute value), which is significant at the 10 percent level. This finding is in line with the significant positive association between risk aversion and neuroticism reported by Borghans, Golsteyn, Heckman, and Meijers (2009). Other researchers (e.g. Zuckerman, 1994) have related risk preferences to sensation seeking, a facet of extraversion in the Big Five taxonomy, and found mixed evidence. Whereas Bibby and Ferguson (2010) report a significant correlation between a measure of loss aversion and sensation seeking ($r = 0.27$), Eckel and Grossmann (2002) find no evidence of an association between risk preferences and sensation seeking.

Evidence on the link between social preferences and personality is somewhat stronger. Dohmen, Falk, Huffman, and Sunde (2008) relate survey measures of social preferences to measures of the Big Five using data from the SOEP and find significant associations between trust, as well as positive and negative reciprocity and personality traits. Trust is related positively to agreeableness and openness to experience, and negatively to conscientiousness and neuroticism; while positive reciprocity is positively associated with all five personality factors, negative reciprocity is related negatively to conscientiousness and extraversion, and positively to neuroticism. A link between extraversion and behavior in the dictator game, which can be interpreted as a measure of altruism, has been established by Ben-Ner and Kramer (2010).

This review is structured as follows. Section 2.2 describes our three data sets. In Section 2.3 we introduce our research strategy for investigating the link between personality and preferences. Section 2.4 presents evidence on the correlation between measures of personality and measures of preferences. In addition it contains

an assessment of the explanatory power of preferences and personality in explaining important life outcomes. Section 2.5 concludes.

2.2   Data and Measures

In this section, we provide a description of the three complementary data sets that we employ for our analysis. Before we present our experimental and survey measures in detail, a few comments on identification are warranted. Economists typically try to infer preferences from choices, the so-called revealed preference approach. For example, one might surmise that a person who does not wear a safety belt an who invests in risky stocks has a preference for taking risks. It is, however, easy to show that the same behavioral pattern is compatible with very different risk preferences if other factors affect the person's decisions. For example, differences in beliefs about how risky driving without a safety-belt or investing in stocks actually is may affect decisions equally strong than underlying risk preferences. The problem is that the decision context is uncontrolled and person specific, rendering precise statements about preference parameters very difficult.[5] This is why economists run experiments to infer preferences. In a typical choice experiment subjects make decisions in a well-controlled decision environment. In risk experiments, for example, stakes and probabilities are fixed and the action space is identical for every subject. Observing subjects' decisions in a controlled experimental environment therefore rules out many potentially confounding factors, allowing a more precise identification of preferences. Even in an experiment, however, the identification of preferences is limited (see Manski (2002) for a thorough discussion on the identification of experimental outcomes). The same observed action can reflect different risk attitudes, for example, if the experimental subjects dispose of different wealth levels and the curvature of the utility function is not invariant to wealth levels. Despite these limitations experiments deliver much more precise behavioral outcomes than non-experimental observations. In strategic situations, which are relevant for measuring trust and reci-

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[5]Conceptually identical problems apply to the identification of traits, such as ability, physical strength and personality characteristics from observed performance on tasks, when performance also depends on other unobserved factors such as time, energy and attention devoted to the task. An illuminating discussion of the identification problem is provided in section 3 of Almlund et al. (2011).

procity, we are able to elicit not just an action but a complete strategy. With field observations this is impossible. The relevance of eliciting a strategy is obvious: Suppose one observes a second mover who defects in a cooperation context, in response to a non-cooperative act of a first mover. This could reveal selfish preferences as well as reciprocal preferences. Disentangling the two requires knowledge about what the decision maker would have done, had the first mover cooperated. Eliciting a strategy instead of observing only actions does exactly this. Experimental observations have the additional advantage over survey responses that decisions have immediate monetary consequences. This is of obvious importance, for example, for identifying altruism. There is a big difference between simply stating altruistic preferences and revealing them in a costly manner.

2.2.1 Experimental Data

The first data set consists of decisions from laboratory experiments among university students. We ran a series of simple incentivized choice experiments to elicit preferences concerning risk taking, discounting, positive and negative reciprocity, and trust as well as altruism.[6] Table 3.2 presents an overview of the experiments and provides a short description of the elicitation methods and the obtained behavioral measures. Four important features about our experimental design are worth noting. First, subjects took part in two very similar experiments each for risk taking, discounting, trust and positive reciprocity. This allows us to average over both outcomes for each subject in order to minimize measurement error. Second, to reduce spillovers between different choices, we ran the experiments not in one single session but in two sessions, which were scheduled one week apart.[7] Third, to reduce possible income effects with respect to outcomes within a session, we gave feedback about experimental outcomes only at the end of an experimental session. Fourth, the vast majority of subjects in the experiments had never taken part in an experiment before. This eliminates possible confounds in behavior due to previous experiences in similar experiments. In total, 489 students from different majors from the University of Bonn partici-

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[6]For a detailed description of the experimental procedures see Falk, Becker, Dohmen, Huffman, and Sunde (2016).

[7]We reversed the order of the sessions for half of the subjects. Statistical tests reveal no significant order effects.

pated.[8] The experiments were run at the Laboratory for Experimental Economics at the University of Bonn (BonnEconLab). We used zTree (Fischbacher, 2007) as experimental software and recruited subjects using ORSEE (Greiner, 2004). Each session lasted about two hours, and average earnings were 64 Euros.

Table 2.1: Overview of the experimental measures in data set from laboratory experiments among university students

Preference	Experiment	Measure
Time	Two lists of choices between an amount of money "today" and an amount of money "in 12 months".	Average switching point over both lists of choices from the early to the delayed amount.
Risk	Two lists of choices between a lottery and varying safe options.	Average switching point over both lists of choices from the lottery to the safe option.
Positive Reciprocity	Second-mover behavior in two versions of the trust game (strategy method).	Average amount sent back in both trust games.
Negative Reciprocity	Investment into punishment after unilateral defection of the opponent in a prisoner's dilemma (strategy method).	Amount invested into punishment.
Trust	First mover behavior in two versions of the trust game.	Average amount sent as a first mover in both trust games.
Altruism	First mover behavior in a dictator game with a charitable organization as recipient.	Size of donation.

Preference Measures

Risk Preferences   To elicit risk attitudes we adapted the design from Dohmen, Falk, Huffman, and Sunde (2010). Subjects were shown a list of binary alternatives, a lottery and a (varying) safe option. The lottery was the same for each decision: If they chose the lottery participants could receive either 1000 points or zero points

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[8]Out of these 489 students, 80 took part in a pretest of the study. Most of these 80 subjects had taken part in an experiment before. The pretest did not include the experiments on altruism and negative reciprocity.

with 50 percent probability each. The safe option increased from row to row, starting from a value of (close to) zero, and increasing up to a value of (close to) the maximum payoff of the lottery. To reduce measurement error subjects participated in two risk experiments. The choice list of the second experiment was simply a perturbed version of the first one. Perturbations were constructed such that a randomly drawn integer value between -5 and +5 was added to the safe option in every choice, corresponding to perturbations of maximally 5% of the step size of the increase in the safe option. The complete list of choices was shown to subjects on the first screen. Each choice situation was then presented on a separate screen, where subjects entered their respective choice. Subjects were informed that one choice in each list would be selected randomly and paid. Subjects with monotonic preferences should choose the lottery for lower safe options and switch to the safe option when the latter reaches or exceeds the level of their certainty equivalent. Thus switching points inform us about individual risk attitudes. The earlier a subject switches to the save option the less she is willing to take risks. For our analysis we constructed a risk preference measure using the average of the two switching points from the two experiments.[9]

Time Preferences   To measure individuals' time preferences we implemented a procedure very similar to the one for risk attitudes. In the discounting experiments, subjects were given two lists of choices between an earlier amount of money ("today"), which was the same in all choices, and an increasing delayed amount of money ("in 12 months"). In the first row, the early amount was equal to the delayed amount. Delayed amounts increased from row to row by 2.5%. As for risk preferences subjects participated in a very similar second discounting experiment with small perturbations of delayed amounts between +0.5 and -0.5 percentage points. One choice in each of the two lists was randomly selected for payment. Payments resulting from the two experiments were sent to subjects via regular mail. If a subject chose the early amount, the payment was sent out on the day of the experimental session. If a subject chose the delayed amount, the payment was sent out with a delay of 12

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[9]If subjects switched between the lottery and the safe option more than once, we took the average switching row as an estimate of their certainty equivalent. This happened in 16 % of the cases in the first experiment on risk taking, and in 11 % of the cases in the second experiment.

months.[10] The switching point from early to delayed payment informs us about a subject's time preference. Subjects who switch later discount the future amount by more (i.e., are less patient) than subjects who switch earlier.[11] Our measure of individual discounting is the average switching row in both lists. To ease interpretation of the correlations reported below, we recode the measure, such that higher values imply earlier switching rows, i.e., a higher level of patience.

Trust We elicited trust from first-mover behavior in the so-called trust game (Berg, Dickhaut, and McCabe, 1995). We conducted two versions of the trust game. In one version, the amount sent by the first mover was doubled by the experimenter, whereas in the second version the amount was tripled. Every subject was in the role of the first and of the second mover twice.[12] Both trust games were incentivized, i.e., every (relevant) decision was paid. In the role of a first mover, subjects could choose to send any amount in $\{0, 50, 100, \ldots, 500\}$ points to the second mover. All interactions in the trust game as well as in all other social preference experiments were one-shot and anonymous (perfect stranger matching protocol). The average amount sent as a first mover in both trust games constitutes our experimental measure for trust: Subjects who send higher amounts of money are those who display higher levels of trust.

Positive Reciprocity To elicit positive reciprocal inclinations we measure subjects' second-mover behavior in the trust game (see above). We implemented the strategy method (Selten, 1967). This means that for every possible amount sent by the first mover, subjects were asked to indicate how much they wanted to send back. The actual decision of the first mover determined which of these decisions became payoff relevant. The average amount sent back as a second mover in both trust games was taken as individuals' willingness to reciprocate, such that higher values imply a higher willingness to reciprocate.

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[10]Keeping the payoff mode identical over both time horizons rules out credibility concerns.

[11]For subjects, who switched more than once, we took the average switching row as an estimate of their discount rate. This happened in 5 % of the cases in the first experiment on time discounting, and in 7 % of the cases in the second experiment.

[12]Overall, we therefore ran four trust games.

Negative Reciprocity   To measure subjects' willingness to engage in costly punishment of unfair behavior, we conducted a prisoner's dilemma with a subsequent punishment stage.[13] In the punishment stage, subjects could choose to invest points in order to deduct points from their opponent. Punishment was costly. Again, we implemented the strategy method. Before taking their decisions in the first stage of the experiment (i.e., in the prisoner's dilemma) subjects were asked to indicate how many points they wanted to deduct from the other player in case he cooperated or defected, for both own cooperation and own defection. Then they played a simultaneous prisoner's dilemma. The outcome of the first stage determined which choice of the second stage became payoff relevant. The chosen investment into punishment after unilateral defection of the other player served as a measure of an individual's willingness to reciprocate negatively.

Altruism   To measure altruistic behavior we had subjects take part in a modified dictator game in which the recipient was a charitable organization (adapted from Eckel and Grossmann, 1996). Subjects were endowed with 300 points and had to decide how much of this endowment to donate to a charitable organization.[14] This decision serves as our experimental measure of subjects' altruistic inclination.

Personality Measures

Big Five   As part of the study, subjects were given a paper-and-pencil survey, which they were asked to fill out at home and return to us via mail.[15] Of the 489 subjects, 319 completed the survey and sent it back to us. The survey included the NEO-FFI version of the Big Five (Costa and McCrae, 1989). During the experimental sessions, all 489 subjects also answered a shorter version of the NEO-FFI: the BFI-S, a subset consisting of 15 items. The BFI-S has been developed by Gerlitz and Schupp (2005) and was also part of the 2005 and 2009 waves of the SOEP. Correlations between the long version and the short version of the Big Five differ between the five personality dimensions. The lowest correlation is $r = 0.48$ for openness, and the highest is

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[13]The design of the experiment was adapted from Falk, Fehr, and Fischbacher (2005)

[14]Subjects could choose a charitable organization from a list, or name one themselves.

[15]We also handed out stamped envelopes with the address of our research institute, in order to minimize additional costs for returning the survey to us.

$r = 0.71$ for conscientiousness (all p-values < 0.001). We constructed our Big Five measure in that we use data from the long version whenever available, while for the remaining subjects we refer to the short version. That way, we have measures of the Big Five domains for all 489 subjects.

Locus of Control   The paper-and-pencil survey included 10 items that allows us to construct a measure of the locus of control for the 319 individuals who filled in the survey. These 10 items have been adapted from Rotter (1966) and they have also been implemented in the 2005 wave of the SOEP. The personality construct of locus of control assesses how much people believe they have control over their life outcomes, or how much their lives are determined by forces that are outside of their control, such as luck or faith. We constructed the measure such that higher values represent a more internal locus of control, i.e., the belief that the person can influence their life outcomes. Lower values represent a more external locus of control.

2.2.2   Representative Experimental Data

The second data set we employ consists of experimental data for a representative sample of the German population.[16] This data set is used to assess whether the findings from the sample of university students can be corroborated in a representative sample. Subjects' risk and time preferences were elicited, and we again have information on participants' personality. The data used here stem from a study conducted in 2005 and contains information on 1012 individuals. For a detailed description of the study and its procedures see Dohmen, Falk, Huffman, and Sunde (2010).

Preference Measures   The experiments on risk and time preferences were similar to the ones we used in the laboratory experiments. In both experiments subjects had to make multiple decisions in a list of choices. To elicit their risk preferences we had subjects choose between a lottery, which remained the same in all choices, and safe options, which increased in their value. As in the experiments discussed above, the switching point informs us about the individual's willingness to take risks. Similarly,

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[16]The same data set is used in Dohmen, Falk, Huffman, and Sunde (2010).

to elicit individuals' time preferences we had all participants make a number of intertemporal choices. They had to decide between an amount "today" and a larger amount "12 months" later. The early amount remained the same in all choices. The first delayed amount presented to subjects was devised to imply a 2.5% return on the early amount assuming semi-annual compounding. In the subsequent choices the delayed payment was gradually increased and was calculated such that the implied rate of return rose in steps of 2.5 percentage points. Again, the switching points from the early to the delayed option inform us about the subjects' time preferences.

Personality Measures   The five personality domains were assessed using the BFI-S (see Section 2.1.2 for a more detailed description).

2.2.3   Representative Panel Data

The third data set we use stems from the SOEP, a large panel data set that is representative of the adult population living in Germany (see Schupp and Wagner (2002) and Wagner, Frick, and Schupp (2007) for a detailed description of the SOEP). We use information from eight waves collected in the years between 2003 and 2009. In each of these waves more than 20,000 individuals were interviewed. The SOEP combines extensive sociodemographic information with various measures of attitudes, preferences and psychological traits. In particular, the SOEP includes survey items relating to all personality and preference measures that we discuss in the previous sections.

Personality and economic preference measures were elicited several times between 2003 and 2009. To construct a measure for each individual, we use the maximum available number of observations of a given measure. If several measures of personality and preferences are available, we take the average of the standardized measures of all years in which this measure was elicited. The resulting average is then standardized as well. In case a particular measure was elicited only in one wave (e.g., as it is the case for patience) we just take the standardized measure from that respective year. We restrict the sample to individuals for whom we have information about each personality and preference measure. This results in a sample size of 14,243 individuals.

Preference Measures   As a measure for time preference we use answers to the following survey question: "How would you describe yourself: Are you generally an impatient person, or someone who always shows great patience?".[17]   Participants gave an answer on an 11-point scale where zero means "very impatient" and 10 means "very patient". This survey question was implemented in the SOEP only in 2008. The risk preference question is worded in the same manner: "How do you see yourself: Are you generally a person who is fully prepared to take risks, or do you try to avoid taking risks?" Answers were given on an 11-point scale, where zero means "unwilling to take risks" and 10 means "fully prepared to take risks". This question was included in the 2004, 2006, 2008 and 2009 waves. The general risk question has been studied in various papers and has been validated using incentivized experiments in representative samples as well as through behavioral evidence in Dohmen, Falk, Huffman, Sunde, Schupp, and Wagner (2011). In 2005 the SOEP contained six items to measure reciprocal inclinations, three items each on positive and negative reciprocity. Examples for positive and negative reciprocity are as follows: "If someone does me a favor, I am prepared to return it" and "If I suffer a serious wrong, I will take revenge as soon as possible, no matter what the costs". Participants expressed how well these six statements apply to them on a seven-point Likert scale. For a detailed description see Dohmen, Falk, Huffman, and Sunde (2009). Standard trust questions were included in the 2003 and 2008 waves, using three sub-statements about whether "one can trust people", whether "in these times one can't rely on anybody else" and whether "when dealing with strangers it is better to be cautious". Answers were given on a five-point scale ranging from "totally agree" to "totally disagree". Finally, our survey measure for altruism is the answer to the question of how important it is for the participant "to be there for others". Answers were given on a four-point scale. The altruism question was asked in the 2004 and 2008 waves.

Personality Measures   The 2005 and 2009 waves of the SOEP contained the BFI-S questionnaire, developed by Gerlitz and Schupp (2005). The locus of control was elicited in 2005 using Rotter's (1966) locus of control scale. Both inventories were also used in our laboratory experimental data (see Section 2.1.2 for more details on

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[17]The behavioral validity of this question with respect to incentivized experiments is documented in Vischer, Dohmen, Falk, Huffman, Schupp, Sunde, and Wagner (2013).

the BFI-S and the locus of control scale).

2.3 Research Strategy

To answer the question of whether measures of personality and economic preferences are closely linked we first study the raw correlations between these measures. High correlations would indicate some degree of substitutability. Low correlations, conversely, would suggest that the two measurement systems are complementary concepts in explaining heterogeneity in behavior. Whether a correlation should be interpreted as "high" or "low" is of course always debatable. We therefore first look at statistical significance levels. Statistical significance, however, can also be found for correlations that are low in terms of effect size (Cohen, 1992). Following conventions in the social sciences we interpret effect sizes, i.e., correlations r, as rather "low" if r is between 0.1 and 0.3, as "medium" if r is between 0.3 and 0.5 and as "large" if r is larger than 0.5. Because the analysis of correlations is restricted to linear relations, we also check for potential non-linear associations by conducting non-parametric regressions. In particular, we look at kernel-weighted local linear polynomial regressions.

We then check to see whether measures of personality and preferences are substitutes or complements in terms of their explanatory power for life outcomes. In particular, we conduct linear regressions and assess the explanatory power of the two concepts by reporting levels of adjusted R^2. In these regressions, measures of personality and preferences are included individually as well as jointly. If the two measurement systems are substitutes, adjusted R^2 in the combined regressions should not be distinctly higher than in regressions that include only one of the two concepts. The opposite should hold for complements. Additionally, we investigate model selection criteria in these regressions. We check for robustness using binary and ordered choice models as well as more comprehensive specifications including square terms and cross-products of all regressors.

2.4 Results

In this section we discuss our main findings. To ease comparison between data sets and measures, we standardized all experimental as well as all personality measures for the data analysis.

2.4.1 Correlation Structure

Experimental Data

Table 2.2 displays the 36 raw correlations of the personality and economic preference measures obtained from the laboratory experiments. A first inspection of Table 2.2 reveals that only 11 of these 36 correlations are statistically significant at the 5% or 1% level.[18] All correlation coefficients are smaller than 0.3 in absolute value. Hence there is no correlation with a "medium" effect size or larger. Moreover, of the 36 correlations only 11 exceed 0.1 in absolute value and only 1 slightly exceeds 0.2.[19]

Table 2.2 also shows that among all personality factors agreeableness exhibits the highest and statistically most significant correlations with measures of economic preferences. It is significantly correlated with measures for positive and negative reciprocity, trust and altruism (all p-values < 0.01) as well as with time preference (p-value < 0.05). Correlations with social preferences range between 0.1 and 0.3 in absolute value, indicating a small effect size according to the classification of Cohen (1988). The high frequency of significant correlations of agreeableness with social preferences is not surprising as the former is defined as "the tendency to act in a cooperative, unselfish manner,..." (see Table 2.5).

The finding of only moderate correlations between preference and personality measures does not necessarily indicate that these constructs are weakly connected; it indicates only that there are weak linear relations. For example, a perfect U-shaped relation between a personality factor and a preference would result in an insignificant linear correlation. To explore the possibility of non-linear relationships we therefore

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[18]Five additional correlations are weakly significant, i.e., significant at the 10% significance level.

[19]Results qualitatively stay the same when investigating Spearman correlations instead of Pearson correlations (see Table 2.6 in the appendix). Moreover, when looking at a potential linear mapping, i.e., linear regressions of either the Big Five on preferences or vice versa, R^2 is always below 10%.

Table 2.2: Pearson correlation structure experimental data set

	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism	LoC
Time	0.0370	0.0057	−0.0084	0.1026**	−0.0518	0.0847
Risk	−0.0379	−0.0611	0.0762*	0.0202	−0.1201***	0.0434
Pos. Reciprocity	0.1724***	0.0140	0.0211	0.2042***	0.0361	0.0152
Neg. Reciprocity	−0.0885*	−0.0393	0.0943*	−0.1451***	−0.0136	−0.1418**
Trust	0.1232***	−0.1300***	0.0004	0.1665***	−0.0134	−0.0140
Altruism	0.1242**	−0.0979*	0.0249	0.1911***	0.0847*	0.0480

*, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Correlations between economic preferences and the Big Five were calculated using 394 - 477 observations. Correlations between economic preferences and locus of control were calculated using between 254 - 315 observations. All measures are standardized.

estimate kernel-weighted local linear polynomial regressions.[20] In each regression, we restrict the sample to a range of four standard deviations around the mean of each variable to circumvent an analysis biased by outliers. Therefore, the results are calculated using 70% to 97% of all observations. The predicted regressions are displayed in Figure 2.2. Although sometimes there are small deviations from linearity at the boundaries, the overall picture strongly suggests a linear relation in the vast majority of combinations.

Summarizing our analysis of the laboratory experimental data, we find that associations between preference and personality measures are linear and that the degree of association is rather low, suggesting a complementary relationship. We next turn to the question of whether the correlation patterns observed in student samples can be replicated in a sample that is representative of the adult population.

Representative Experimental Data

Table 2.3: Pearson correlation structure representative experimental data

	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism
Time	−0.0080	−0.0682	−0.0655	−0.0830*	−0.0602
Risk	0.1356***	−0.0720	0.0757	−0.0941**	−0.0290

*, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. All measures are standardized.

Table 2.3 shows the correlations between the outcomes from the risk and time experiments and the personality traits. As above, the measure for time is reversed so that higher values indicate higher patience. In terms of significance the pattern is similar to the one in the laboratory study. Only one correlation is significant at the 1%-level, one is significant at the 5%-level and one is significant at the 10%-level. In terms of effect size, only the coefficient of the association between openness and risk preferences exceeds the 0.1 benchmark to be classified as a small correlation (Cohen, 1988).[21] Interestingly, the sign is positive, in contrast to our laboratory data. The

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[20]We use the Epanechikov kernel and bandwidth is selected via the plugin estimator of the asymptotically optimal constant bandwidth.

[21]Results qualitatively stay the same when investigating Spearman correlations instead of Pearson correlations (see Table 2.7 in the appendix).

other two significant coefficients are even smaller. The analysis of representative data therefore confirms that the level of association between preference personality measures is rather small. However, we can draw this conclusion only with respect to time and risk preferences, as we do not have experimental data on trust and social preferences. We next analyze whether these findings also hold when looking at all preference measures in a large representative sample.

Representative Panel Data

In this section, we study whether our findings from the experiments generalize to a large representative sample using survey rather than experimental instruments for measuring economic preferences. Table 2.4 shows the raw correlations between personality measures and economic preferences using 14,243 observations from the SOEP. Given the large number of observations it is not surprising to find a large number of significant correlation coefficients (p-values < 0.05 for all correlation coefficients). In terms of effect size, however, only two correlations are of "medium" size, i.e., larger than 0.3. Of the reported 36 correlations, 18 can be classified as "small", whereas 16 correlations are even below 0.1. This confirms the overall picture that emerged from the analysis of the two experimental data sets.[22] A closer comparison of the SOEP survey measures with our experimental measures further reveals large similarities. As reported above, 11 correlations are significant at the 5% level in the experimental data. Ten of these correlations have the same sign and are significant at the 1% level using survey data. Moreover, as it is the case in the laboratory data set, the personality trait agreeableness exhibits the highest correlations with economic preferences, in particular social preferences. Although there are small differences in the results compared with the experimental data set (i.e., seven of the 36 correlation coefficients show a different sign), the general pattern emerging from the SOEP measures is consistent with our previous findings. Of the seven correlation coefficients only two are (weakly) significant in the experimental data set. Nevertheless, the inconsistency of signs brings into question the conjecture

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[22]Results qualitatively stay the same when investigating Spearman correlations instead of Pearson correlations (see Table 2.8 in the appendix). Moreover, when looking at a potential linear mapping, i.e., linear regressions of either the Big Five on preferences or vice versa, R^2 is always around 15% with the exception of agreeableness, where R^2 reaches 28%.

Table 2.4: Pearson correlation structure between personality measures and economic preferences from SOEP observations

	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism	LoC
Time	0.0183**	0.1122***	−0.0415***	0.3122***	−0.0584***	0.0681***
Risk	0.2793***	−0.0400***	0.2601***	−0.1454***	−0.0996***	0.1521***
Pos. Reciprocity	0.1814***	0.2520***	0.1473***	0.1842***	0.0872***	0.0954***
Neg. Reciprocity	−0.0522***	−0.1558***	−0.0264***	−0.3756***	0.0612***	−0.2154***
Trust	0.1272***	−0.0680***	0.0575***	0.0945***	−0.1919***	0.2094***
Altruism	0.1756***	0.1495***	0.1670***	0.2557***	0.0908***	0.0874***

*, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Correlations are calculated using 14,243 observations. All measures are standardized.

that correlations are universally identical (i.e., identical irrespective of age or other person characteristics). We return to this aspect in the final section.

We conclude this section with an analysis of potential non-linearities between our SOEP preference and personality measures. As for the laboratory experimental data, we perform kernel-weighted local linear polynomial regressions restricting the sample in each regression to four standard deviations above and below the mean. The resulting subsamples represent 92% to 97% of the observations of the main sample. The predicted functions presented in Figure 2.3 show no particular non-linearities, except for some splines at the left ends of the considered range. Thus, analogous to the experimental data set, it is not the case that systematic non-linearities bias correlation coefficients.

Explanatory Power for Life Outcomes

Figure 2.1: Adjusted R^2 for Life Outcomes
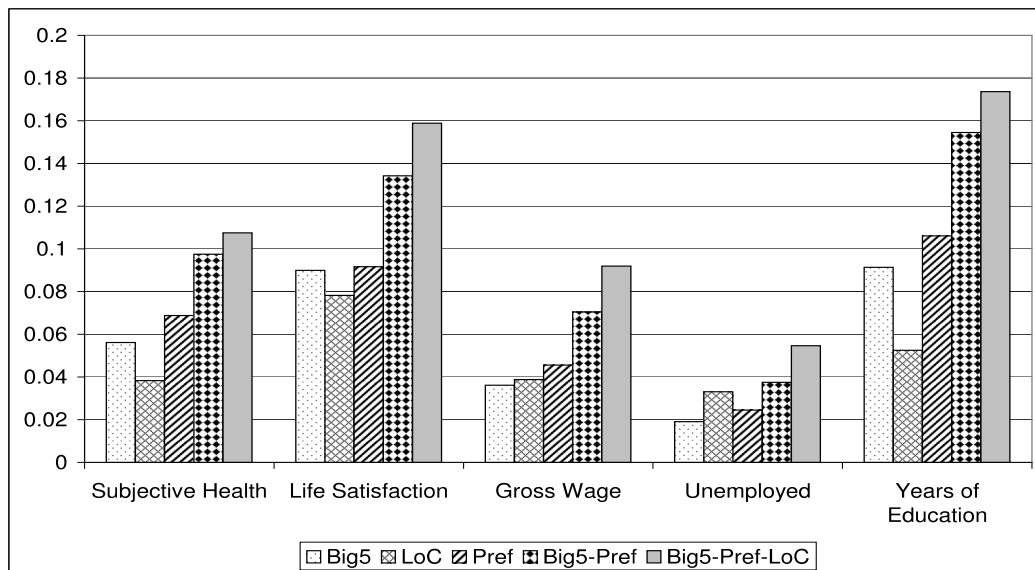
Adjusted R^2's for linear regressions for life outcomes. The number of observations available varies for the different life outcomes: subjective health (14,218), life satisfaction (14,214), gross wage (7,199), unemployed (9,095), and years of education (13,768). Gross wage measures the gross hourly wage.

All reported correlation structures indicate that personality and preference mea-

sures are far from perfectly substitutable. To determine whether they actually complement each other, we now analyze their explanatory power with respect to important life outcomes. To that end we again use data from the SOEP. In particular, we consider the following outcomes: subjective health, life satisfaction, gross wage, being unemployed and years of education. For each outcome we estimate linear regression models in which outcomes are regressed on the set of economic preferences, the Big Five and the locus of control, separately as well as jointly.[23] The idea is to assess the explanatory power of each concept in isolation and in combination. This enables us to check the extent to which explanatory power increases when combining the concepts and thus allows us to reach conclusions regarding the degree of their complementarity. The criterion used to compare differences in explanatory power is adjusted R^2.

All life outcomes we use come from the 2009 wave of the SOEP. Subjective health was measured on a five-point-scale, from "very good" to "bad". We reverse the answer scale such that higher values indicate a better subjective health status. Life satisfaction was elicited using the question "How satisfied are you with your life, all things considered?", which was answered on an 11-point-scale (with higher values indicating higher life satisfaction). Our measure for gross hourly wage is the gross monthly wage divided by monthly working hours.[24] Unemployment status is a binary variable equal to one if the person was unemployed at the time of the survey and zero otherwise. The variable years of education is created by adding up years of schooling and additional occupational training (including university).[25]

Figure 2.1 shows adjusted R^2's for the different life outcomes. R^2 values for the three concepts – Big Five, Locus of Control and economic preferences – in isolation range from 1% to 10% and vary both between concepts and outcomes. Thus, they contribute to explaining heterogeneity in important life outcomes.[26] More important in light of our research question, however, is that the explanatory power is

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[23]The corresponding regressions are shown in Table 2.9 in the appendix.

[24]Monthly working hours are calculated as the average weekly working hours multiplied by four.

[25]For each school degree and occupational training (including university) official standard graduation times in years are used for the calculation.

[26]In the explanation of life outcomes such as gross wages, unemployment and years of education the preference for work versus leisure would probably play a key role. However, no question related to this preference was included in the survey.

considerably larger when combining the Big Five, the locus of control and economic preferences compared to using each concept individually. Moreover, explanatory power is always maximized when all three concepts are included in the regression, hereafter referred to as the full model. In this case, resulting adjusted R^2 values reach levels of about 6% to 18%. This clearly indicates the existence of important complementarities among the different concepts.[27]

Because the question here is one of model selection, we also employ model selection criteria (in particular the Akaike and Bayesian information criterion) to check whether the full model is also chosen by model selection criteria. As can be seen in Table 2.10 in the appendix this is the case for all life outcomes considered, corroborating our previous results. We perform the same analysis using binary and ordered choice models when appropriate. Again, the full model is chosen by the model selection criteria in all cases. As another robustness check we consider more flexible models: Along with including each predictor linearly in our regressions we also include square terms and all possible cross-products (see Table 2.11 in the appendix). Again the full model obtains the highest adjusted R^2 measures when using ordinary-least-squares estimation and is also chosen by the information criteria in nearly all cases.[28] Results are again robust for employing binary and ordered choice models when appropriate. Moreover, in all models considered the joint hypothesis that all coefficients are equal to zero is always rejected at the 1% level (Tables 2.10 and 2.11 in the appendix). In summary, sizeable complementarities among the different concepts are corroborated in all robustness checks.

2.5 Discussion

In this review we examine the relation between economic preferences and personality using three different data sets. We find no indication for a strong linear or a non-linear association between the two. Thus we conclude that the two concepts cannot

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[27]For an overview over the raw correlations between each preference and personality trait and life outcomes see Figure 2.4 and 2.5 in Section 2.6.

[28]Only the Bayesian information criterion chooses a model just including the locus of control when it comes to explaining gross wage and unemployment. However, this is not surprising given the number of regressors included and the tendency of Bayesian information criterion to choose parsimonious models.

substitute for each other. In fact, with regard to explaining heterogeneity in life outcomes, we find that the two concepts play complementary roles. Our findings imply that researchers in economics and psychology can benefit greatly from the respective disciplines when looking for potential sources of heterogeneity in life outcomes.

The finding of a rather low association between economic preferences and psychological measures of personality is perhaps not surprising. First, both concepts are constructed in very different ways. Whereas preferences are rooted in utility theory, derived in terms of specific functional forms of utility functions, the Big Five personality indicators originate in language analysis. Second, the Big Five measure rather broad aspects of personality. In particular, each dimension of the Big Five is by itself already an aggregation of different attitudes or subfacets. Thus, although our results show low associations between personality and economic preferences, we cannot exclude the possibility that there is a stronger degree of association between economic preferences and subfacets of the five personality traits. The trait extraversion, for example, comprises different attitudes, such as being "relatively outgoing, gregarious, sociable, and openly expressive" (see Table 2.5), measured by 12 different questions in the NEO-FFI or three different questions in the BFI-S. In other words, each personality measure is not only comprises multiple items, but more importantly captures distinct aspects of a character trait. Economic preferences, conversely, are defined more narrowly. For example, the concept of time preferences refers to the individual's willingness to abstain from something in the present in order to benefit from that decision in the future. Although this concept is applicable to different domains (e.g., to health outcomes or financial decision making) the underlying concept remains the same and is measured by standard incentivized experiments or survey items as employed in this study. In this sense, our preference measures might resemble the subordinate aspects of the five personality factors.

Third, the finding of strong complementarities between economic preferences and personality measures may simply reflect conceptual differences in the way economic and psychological models are constructed. The economic model explains heterogeneity in behavior in terms of three distinct components: preferences, beliefs and constraints, such as abilities. In contrast, psychological measures such as the Big Five include notions of preferences as well as beliefs and constraints. In other words,

in our analysis we correlate economic preferences at least partly with beliefs and constraints, which by construction should not necessarily be correlated. A good example is conscientiousness. Being able and willing to work hard and being organized comprises aspects of both, preferences and personal abilities. Likewise, emotional instability, which is part of the neuroticism facet, is related to personal inability rather than a preference. Even more extreme is the case of the locus of control, which is clearly a belief rather than a preference. This does not rule out the possibility that the two concepts are related, for example, because an external locus of control is conducive to the development of impatient behavior: if it does not pay off to invest because life circumstances are predominantly determined by circumstances beyond my control, the willingness to forgo current consumption and wait in order to earn a return in the future makes little sense. Yet, beliefs and preferences are two distinct concepts.

The main focus of this review is the rather weak association and complementary nature of economic and psychological measures of personality. We do not discuss the specific signs of the correlations or ways to integrate personality into the economic model. Important work in this direction has been done by Almlund, Duckworth, Heckman, and Kautz, 2011. Many signs of the correlations reported above are consistent across the three data sets, in particular those that are significant. For example, in all three data sets risk attitudes and extraversion are positively correlated, and risk and neuroticism are negatively correlated. There are important exceptions, however. In the student sample, for example, risk attitudes and openness are negatively correlated, whereas they are positively and significantly negatively correlated in the two representative data sets. These and other inconsistencies raise important questions. One possible reason for finding different signs is the use of different elicitation methods for economic preferences (experiments and survey responses). Another possibility is that the reported correlations vary over the life-cycle. If traits develop with different speed and at different points in life correlations should vary with age. This could explain differences between a relatively young student sample and the representative samples. Not much is known about how economic preferences develop over the life-cycle but at least for risk attitudes there seems to be a robust and large negative age effect on willingness to take risks (Dohmen, Falk, Huffman, Sunde,

Schupp, and Wagner, 2011). Another possibility is that preferences and personality are generically differentially correlated between specific groups of the population (e.g., varying by gender, age, height or education). From an evolutionary perspective the co-evolution of traits may serve different purposes depending on specific life circumstances. It may be "optimal" for one subgroup of the population to develop a positive correlation among particular traits, whereas for another subgroup it is adaptive to form a negative correlation. More work needs to be done to uncover potential group-specific correlations between personality and preferences.

The approach taken above is agnostic in the sense that we simply correlate existing and important measurement systems as they are. We think this is an important exercise but it can only be a first step. What is needed is the development of a comprehensive framework that combines insights from the approaches taken by economists and psychologists to capture sources of heterogeneity in behavior. It is surprising that the Big Five apparently misses important preferences such as attitudes towards risk and time. Similarly, the economic model is incomplete not only with respect to important preferences, but also with respect to heterogeneity in abilities and beliefs. In the standard economic framework, beliefs are assumed to be endogenous to the strategic situation and formed in a rational way. Perhaps, with the exception of interpersonal trust, beliefs are typically assumed to follow common prior assumptions and rational updating. The role of the locus of control in explaining fundamental life outcomes on top of preferences, however, reveals the importance of enduring and individual specific belief systems. Other examples include optimism, pessimism, religious beliefs and ideological beliefs. The stability of belief heterogeneity is not well understood. It probably originates in different priors inherited from parents, self-selection into peer groups and institutions with reinforcing belief characteristics and boundedly rational belief formation, such as selected perception, non-Bayesian updating and ego utility (Köszegi, 2006). Regardless of the precise channels that support enduring heterogeneous beliefs, economics would largely benefit from measuring and including them in explanations of economic outcomes. In addition, economists have started to model the fact that preferences and beliefs are intimately related and not separable as traditionally assumed. In fact, people often want to believe certain things, for example, in terms of being liked by others or be-

ing better than others (overconfidence). Finally, another important extension of the economic model would be the measurement of person-specific abilities. Whereas IQ has become a standard individual-specific characteristic to be included in outcome regressions, little work has acknowledged the importance of other competencies captured by Big Five traits, for example, the role of conscientiousness for educational or labor market outcomes.

2.6 Appendix to Chapter 2

Table 2.5: Definitions of the Big Five Domains

Big Five Domain	APA Dictionary Definition
Openness	Individual differences in the tendency to be open to new aesthetic, cultural, and intellectual experiences.
Conscientiousness	The tendency to be organized, responsible, and hardworking; located at one end of a dimension of individual differences: conscientiousness vs. lack of direction.
Extraversion	An orientation of one's interests and energies toward the outer world of people and things rather than the inner world of subjective experience; includes the quality of being more outgoing, gregarious, sociable, and openly expressive.
Agreeableness	The tendency to act in a cooperative, unselfish manner; located at one end of a dimension of individual differences: agreeableness vs. disagreeableness.
Neuroticism	A chronic level of emotional instability and proneness to psychological distress.

This table is in parts reproduced from Borghans, Duckworth, Heckman, and ter Weel (2008).

Table 2.7: Spearman correlation structure representative experimental data

	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism
Time	-0.0199	-0.0737	-0.0764^*	-0.0829^*	-0.0598
Risk	0.1315^*	-0.0744	0.0661	-0.0854^*	-0.0261

*, **, and *** indicate significance at the 10%, 5%, and 1% level. All measures are standardized.

Table 2.6: Spearman correlation structure experimental data set

	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism	LoC
Time	0.0388	0.0162	−0.0114	0.1077**	−0.0684	0.1063*
Risk	0.0027	−0.0486	0.0786*	0.0206	−0.0995**	0.0485
Pos. Reciprocity	0.1606***	0.0078	0.0177	0.2029***	0.0152	0.0441
Neg. Reciprocity	−0.0967*	−0.0221	0.0462	−0.083*	−0.0165	−0.1376**
Trust	0.1354***	−0.1198***	0.002	0.1696***	−0.002	−0.0648
Altruism	0.0969*	−0.0804	0.0034	0.2000***	0.0879*	0.0418

*, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Correlations between economic preferences and the Big Five were calculated using 394 - 477 observations. Correlations between economic preferences and Locus of Control were calculated using 254 - 315 observations. All measures are standardized.

Table 2.8: Spearman Correlation Structure SOEP

	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism	LoC
Time	0.0233	0.1192	−0.0342	0.3099	−0.0643	0.0709
Risk	0.2632	−0.0500	0.2452	−0.1496	−0.1049	0.1426
Pos. Reciprocity	0.1835	0.2622	0.1547	0.1947	0.0808	0.1041
Neg. Reciprocity	−0.0616	−0.1767	−0.0426	−0.3853	0.0572	−0.2257
Trust	0.1224	−0.0693	0.0523	0.0788	−0.1889	0.2012
Altruism	0.1693	0.1501	0.1602	0.2416	0.0860	0.0843

All correlations are significant at the 1% level and are calculated using 14,243 observations. All measures are standardized.

Table 2.9: Outcome Regressions: Representative Experimental Data

Life Outcomes	(1) Subj. Health	(2) Life Satisf.	(3) Gross Wage	(4) Unemployed	(5) Years of Educ.
Openness	0.043***	0.123***	0.989***	-0.018***	0.667***
	(0.009)	(0.017)	(0.162)	(0.004)	(0.027)
Conscientiousn.	0.038***	0.106***	0.565***	-0.014***	-0.182***
	(0.009)	(0.017)	(0.161)	(0.004)	(0.026)
Extraversion	0.026***	0.134***	-1.201***	0.006*	-0.309***
	(0.009)	(0.017)	(0.154)	(0.004)	(0.026)
Agreeableness	0.033***	0.139***	-1.288***	0.023***	-0.146***
	(0.010)	(0.018)	(0.165)	(0.004)	(0.028)
Neuroticism	-0.140***	-0.186***	-1.009***	0.018***	-0.272***
	(0.009)	(0.016)	(0.158)	(0.004)	(0.026)
LoC	0.105***	0.307***	1.899***	-0.043***	0.421***
	(0.008)	(0.015)	(0.145)	(0.003)	(0.024)
Patience	0.024***	0.129***	-0.343**	0.001	-0.151***
	(0.008)	(0.015)	(0.136)	(0.003)	(0.023)
Risk	0.131***	0.076***	0.415**	0.003	0.210***
	(0.009)	(0.017)	(0.166)	(0.004)	(0.027)
Pos. Recip.	-0.035***	0.006	0.388***	-0.002	0.005
	(0.008)	(0.015)	(0.140)	(0.003)	(0.023)
Neg. Recip.	0.064***	0.039**	-0.329**	0.006*	-0.137***
	(0.008)	(0.015)	(0.147)	(0.003)	(0.024)
Trust	0.122***	0.308***	1.763***	-0.035***	0.587***
	(0.009)	(0.015)	(0.145)	(0.003)	(0.024)
Altruism	0.070***	0.072***	-0.780***	0.005	0.084***
	(0.009)	(0.016)	(0.152)	(0.003)	(0.025)
Constant	3.300***	6.852***	16.100***	0.099***	12.346***
	(0.007)	(0.014)	(0.131)	(0.003)	(0.021)
Observations	14,218	14,214	7,199	9,095	13,768
Adj. R-squared	0.108	0.159	0.0919	0.0547	0.174

*, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. All measures are standardized.

Figure 2.2: Kernel-weighted local linear polynomial regressions using experimental data
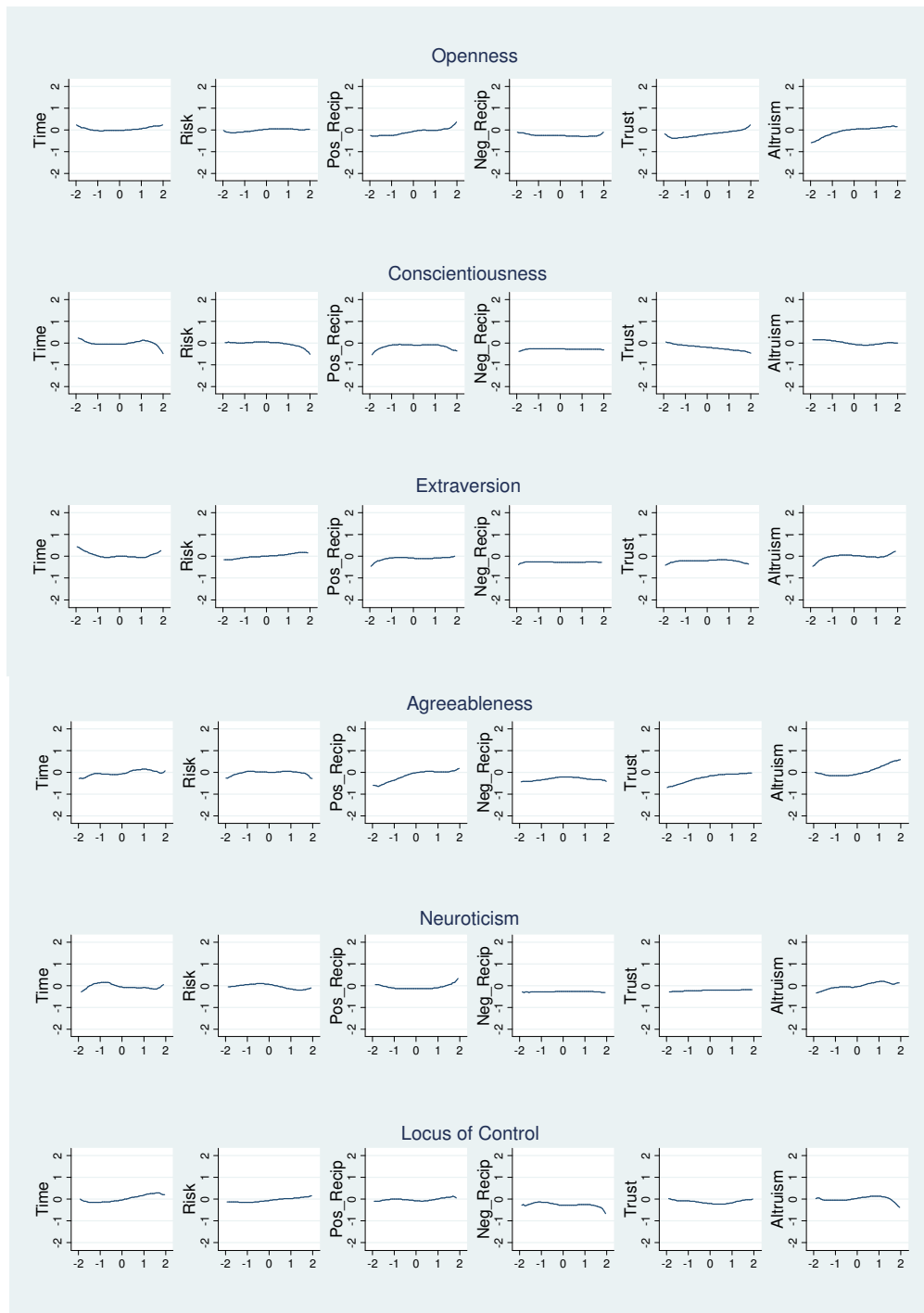
Figure 2.3: Kernel-weighted local linear polynomial regressions using SOEP data
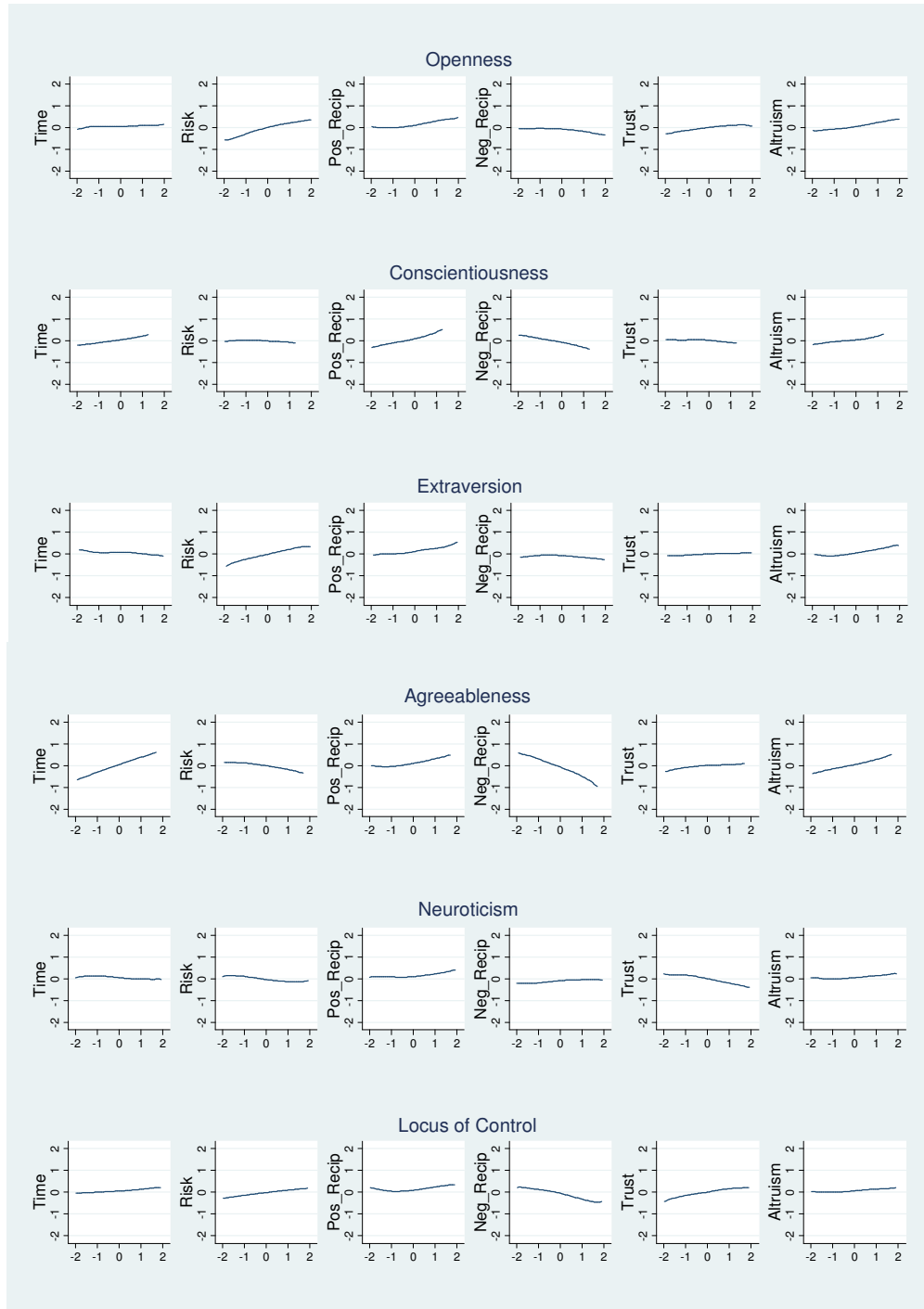
Figure 2.4: Correlation Coefficients Between Preference Measures and Life
Outcomes Using SOEP Data
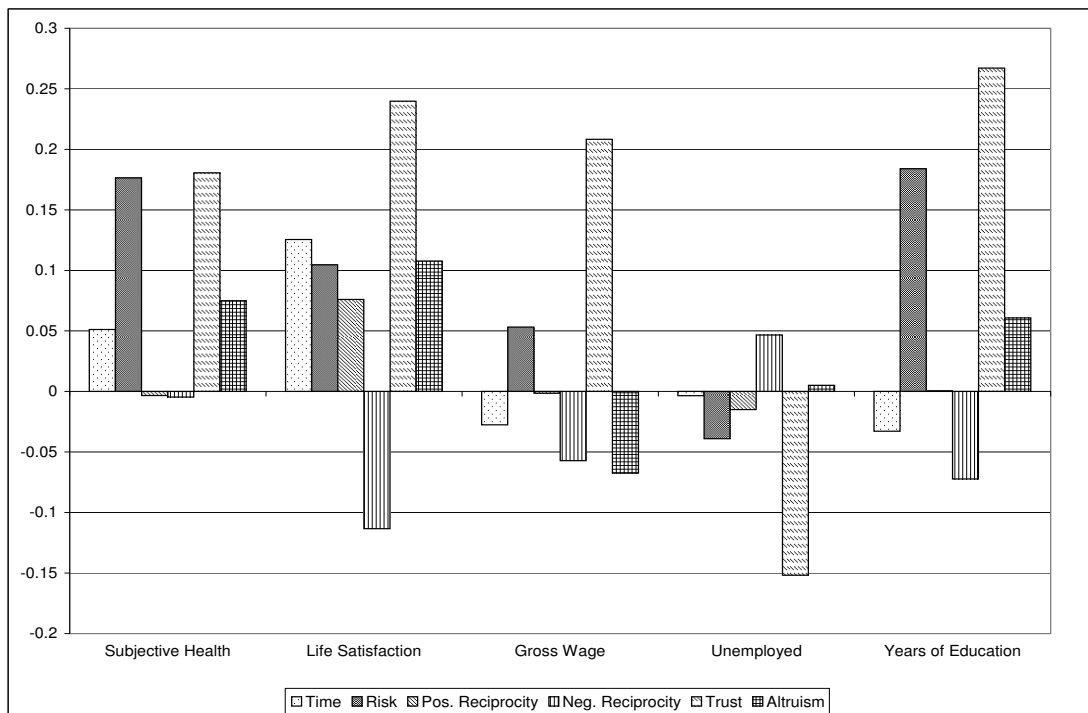
Pearson correlation coefficients between preference measures and life outcomes using SOEP data. Trust always shows the strongest association with life outcomes. More trust and a higher willingness to take risk are always related to better life outcomes, e.g. better health and greater life satisfaction, whereas negative reciprocity is associated with less life satisfaction and lower wages. The number of observations available varies for the different life outcomes: subjective health (14,218), life satisfaction (14,214), gross wage (7,199), unemployed (9,095), years of education (13,768). Gross wage measures the gross hourly wage.

Figure 2.5: Correlation Coefficients Between Personality Measures and Life Outcomes Using SOEP Data
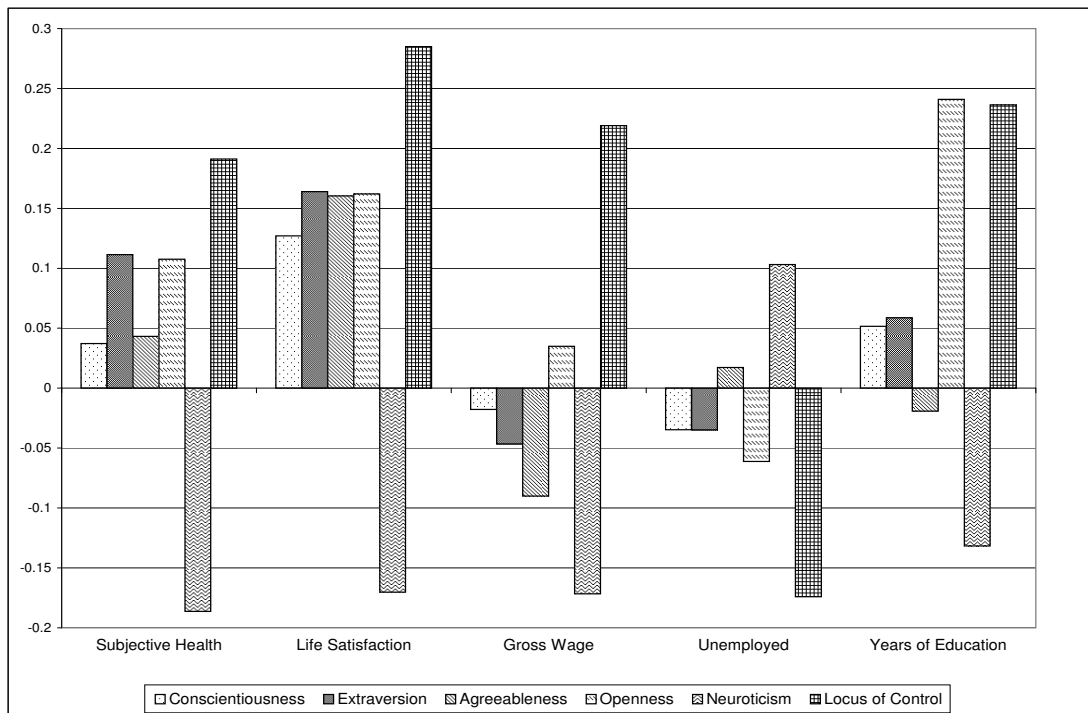
Pearson correlation coefficients between personality measures and life outcomes using SOEP data. The locus of control and neuroticism show the strongest associations with life outcomes. A more internal locus of control is always related to better outcomes (e.g. better health or more life satisfaction), whereas a higher degree of neuroticism is associated with lower wages or a higher probability of being unemployed. The number of observations available varies for the different life outcomes: subjective health (14,218), life satisfaction (14,214), gross wage (7,199), unemployed (9,095), years of education (13,768). Gross wage measures the gross hourly wage.

Table 2.10: Linear representation of outcome regressions

| | Subjective Health (OLS) | | | | | Subjective Health (o. probit) | | | | |
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	0.0561	0.0383	0.0688	0.0975	0.1075	0.0220	0.0145	0.0268	0.0388	0.0429
F-Test/LR-Test	170.04	567.35	176.01	140.59	143.72	834.99	550.62	1016.47	1471.22	1627.11
AIC	37833	38094	37641	37201	37043	37139	37415	36960	36515	36361
BIC	37878	38109	37694	37292	37142	37207	37453	37035	36628	36482
	Life Satisfaction (OLS)					Life Satisfaction (o. probit)				
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	0.0899	0.0782	0.0917	0.1342	0.1588	0.0261	0.0219	0.0256	0.0390	0.0467
F-Test/LR-Test	281.88	1206.91	240.08	201.27	224.67	1406.38	1178.16	1376.73	2098.73	2513.61
AIC	55038	55216	55012	54335	53926	52448	52668	52480	51768	51355
BIC	55083	55231	55065	54426	54024	52561	52751	52601	51926	51521
	Gross Wage(OLS)									
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	0.0361	0.0388	0.0456	0.0704	0.0919	-	-	-	-	-
F-Test/LR-Test	54.97	291.20	58.31	50.57	61.71	-	-	-	-	-
AIC	55088	55088	55042	54857	54690	-	-	-	-	-
BIC	55102	55102	55090	54940	54779	-	-	-	-	-
	Unemployed (OLS)					Unemployed (probit)				
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	0.0191	0.0331	0.0245	0.0375	0.0547	0.0322	0.0527	0.0412	0.0648	0.0926
F-Test/LR-Test	36.34	312.13	39.05	33.22	44.82	180.12	294.52	230.37	361.89	517.42
AIC	3067	2932	3017	2900	2738	5420	5298	5372	5250	5097
BIC	3110	2946	3067	2986	2830	5463	5312	5422	5336	5189
	Years of Education (OLS)					Years of Education (o. probit)				
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	0.0914	0.0525	0.1061	0.1545	0.1736	0.0209	0.0126	0.0241	0.0359	0.0415
F-Test/LR-Test	277.93	763.89	273.29	229.74	242.03	1355.80	817.10	1563.14	2329.14	2688.38
AIC	65506	66078	65282	64520	64206	63490	64021	63285	62529	62171
BIC	65551	66093	65335	64610	64304	63641	64141	63443	62724	62375

For the ordinary-least-squares (OLS) models we calculate R^2, whereas for the ordinal models we calculate pseudo R^2. The joint significance of all coefficients is tested using the F-test (OLS) and the LR-test (ordinal models). All F- and LR-tests are significant at the 1% level. With regard to the Akaike information criterion (AIC) and Bayesian information criterion (BIC), the smallest value for each outcome regression is underlined. Note that the full model (including the Big 5, locus of control and preferences) is always chosen by both information criteria. The number of observations available varies for the different life outcomes: subjective health (14,218), life satisfaction (14,214), gross wage (7,199), unemployed (9,095 obs.), and years of education (13,768). Gross wage measures the gross hourly wage.

Table 2.11: Outcome Regressions: Flexible Specification

	Subjective Health (OLS)					Subjective Health (o. probit)				
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	.0632	.0388	.0714	.1054	.1165	.0251	.0146	.0282	.0435	.0483
F-Test/LR-Test	48.99	288.17	41.48	22.75	21.83	952.98	555.19	1068.56	1651.38	1834.03
AIC	37740	38088	37623	37142	<u>36977</u>	37051	37413	36949	36467	<u>36310</u>
BIC	37899	38110	37834	37732	<u>37665</u>	37232	37458	37184	37079	<u>37021</u>
	Life Satisfaction (OLS)					Life Satisfaction (o. probit)				
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	.0948	.0783	.0948	.1397	.1659	.0278	.0219	.0273	.0422	.0505
F-Test/LR-Test	75.47	605.45	56.12	30.967	32.41	1493.78	1178.45	1470.26	2273.51	2715.76
AIC	54976	55214	54984	54311	<u>53884</u>	52391	52670	52428	51725	<u>51309</u>
BIC	55135	55237	55196	54901	<u>54572</u>	52617	52761	52708	52383	<u>52065</u>
	Gross Wage(OLS)									
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	-	-	-	-	-
adj. R^2/pseudo R^2	.0382	.0387	.0527	.0797	.1039	-	-	-	-	-
F-Test/LR-Test	15.30	145.74	15.84	9.092	10.27	-	-	-	-	-
AIC	55111	55090	55009	54851	<u>54672</u>	-	-	-	-	-
BIC	55256	<u>55111</u>	55202	55388	55298	-	-	-	-	-
	Unemployed (OLS)					Unemployed (probit)				
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	.0212	.0385	.0291	.0463	.0705	.0357	.0539	.0498	.0852	.1166
F-Test/LR-Test	10.87	183.13	11.11	6.73	8.66	199.54	301.02	278.38	475.96	651.83
AIC	3062	2882	2995	2882	<u>2662</u>	5431	5294	5366	5268	<u>5118</u>
BIC	3211	<u>2903</u>	3194	3437	3309	5580	<u>5314</u>	5565	5823	5766
	Years of Education (OLS)					Years of Education (o. probit)				
	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC	Big5	LoC	Pref	Big5-Pref	Big5-Pref-LoC
adj. R^2/pseudo R^2	.1043	.0525	.1200	.1771	.1982	.0243	.0126	.0281	.0433	.0497
F-Test/LR-Test	81.13	382.50	70.55	39.48	38.81	1575.60	817.25	1819.82	2808.59	3223.85
AIC	65324	66079	65087	64213	<u>63869</u>	63300	64023	63070	62181	<u>61792</u>
BIC	65482	66102	65297	64800	<u>64554</u>	63564	64151	63386	62874	<u>62583</u>

The outcome variables are regressed on the indicated personality and preference measures. The difference with regard to the linear specification is that the model includes squares of all variables as well as all cross-products. Cross-products are also calculated between concepts in case more than one concept is included, e.g., in the Big 5-preferences case, we also include the cross-term neuroticism*risk. For the ordinary-least-squares (OLS) models we calculate R^2, whereas for the ordinal models we calculate pseudo-R^2. The joint significance of all coefficients is tested using the F-test (OLS models) and the LR-test (ordinal models). All F- and LR-tests are significant at the 1% level. With regard to the Akaike information criterion (AIC) and Bayesian information criterion (BIC), the smallest value for each outcome regression is underlined. Note that the full model (including the Big 5, locus of control and preferences) is chosen by both information criteria in nearly all cases; only for gross wage and unemployment does the BIC indicate that the model with only LoC and LoC^2 included should be used. The number of observations available varies for the different life outcomes: subjective health (14,218), life satisfaction (14,214), gross wage (7,199), unemployed (9,095), and years of education (13,768). Gross wage measures the gross hourly wage.

Chapter 3

The Preference Survey Module: A Validated Instrument for Measuring Risk, Time, and Social Preferences

3.1 Introduction

Individuals' preferences - next to individuals' beliefs and their constraints - largely determine their choices and are thus fundaments of virtually all types of outcomes, e.g. educational attainment, labor market success, health status or life satisfaction.[1] Experimental economics offers a clean and reliable way to measure of these preferences: by conducting incentivized experiments. However, in non-laboratory settings, e.g. in representative samples or multinational studies, it is practically impossible due to budget, time or administrative constraints to obtain incentivized experimental measures. Nevertheless, it is highly desirable to have reliable preference measures for these settings. Empirical economics - like many other empirical sciences - often relies on survey measures to assess individual preferences. Since survey measures

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[1]Empirically, risk aversion, time preferences, altruism, trust, positive reciprocity and negative reciprocity have all been shown to predict wide range of choices at the individual level – ranging, e.g., from financial decision-making, to educational choices, labor market behavior, charitable giving, and social norm enforcement and health outcomes (see, e.g., Dohmen et al., 2009 ; Dohmen et al., 2011; Fehr et al., 2002; Kirby et al., 1999; Komlos et al., 2004; Rose-Ackerman, 1996; Smith et al., 2005; Tanaka et al., 2010) – and are associated with important life outcomes at the individual level (e.g., Becker et al., 2012) as well as with economic outcomes at the organization level (e.g., LaPorta et al., 1997) and the aggregate level (e.g., Beugelsdijk et al., 2004; Knack and Keefer, 1997; Zak and Knack, 2001).

lack incentives, it is unclear whether and how precisely they actually measure the underlying trait or whether they are confounded otherwise, e.g. by respondents' associations or inattention. Moreover, the variety of survey measures is wide-ranging, from short subjective self-assessments to lengthy hypothetical scenarios, and it is an open question which of these survey measures are superior to others.

This paper develops the first comprehensive, experimentally-validated survey module for measuring risk aversion, time discounting, trust, altruism, positive and negative reciprocity. In contrast to previous studies, which have typically focused on developing a survey measure of a single preference, our module covers preferences in six dimensions and provides validated measures using a consistent framework.[2] Our main criterion in the item selection process was explanatory power with respect to behavior in the incentivized experiments. In our item selection process, we systematically test a large battery of candidate questions, including many questions that have been used in previous studies of preferences, as well as new questions. This is an important feature because the resulting preference module not only consists of survey questions that predict behavior, it is composed of the best predictors out of a large set of candidate measures.

We propose a module that involves two survey items for the elicitation of each preference, trading off parsimony and explanatory power. This module provides a way to measure preferences without the cost of financial incentives, while preserving a good level of explanatory power. The preference module is symmetric, in that most preferences are measured with one quantitative and one qualitative item. Typically, the quantitative item is a hypothetical version of the experiment itself.[3] The second survey item that is selected for our module is a qualitative question, asking about a general orientation in the relevant preference dimension. The module thus offers an attractive balance between measures that allow inferring (cardinal) preference

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[2]Fehr et al. (2003), for example, examine six different attitudinal trust questions in terms of their ability to predict behavior in a trust game as introduced by Berg et al. (1995), and find that self-rated trusting behavior and willingness to trust strangers are most strongly associated with behavior in the incentivized experiment. Dohmen et al. (2011) show that self-rated willingness to take risk "in general" is significantly correlated with decisions in an incentivized lottery choice experiment. Vischer et al. (2013) relate answers to a survey question asking respondents to rate their general level of impatience to behavior in an experiment involving inter-temporal trade-offs.

[3]Naturally, this items usually also turns out to be the single best predictor of behavior in the experiment as well

parameters, and subjective measures that capture other contexts besides choices involving monetary tradeoffs.

It turns out some of the survey items that are selected by our methodology have been shown in previous studies to be behaviorally valid in various populations. For example, the same qualitative measure of risk preference that is selected for our module has been shown to predict behavior in an incentivized risk experiment with a representative sample of German adults (Dohmen et al., 2011). Notably, the correlation between the survey measure and experiment observed in their representative sample is virtually the same as that found in our validation exercise. Other research shows that the same survey question about risk preference predicts behavior in incentivized experiments in 30 different countries (Vieider et al., 2013). Thus, there are strong indications that the types of measures selected for our module have good predictive power in representative and cross-cultural samples.

In our construction of the survey module we took into account several considerations. We strove to reduce measurement error in the experimental preference measures by having subjects participate in more than one experiment for a given preference and averaging over both choice-based preference measures. We designed the validation to limit spurious interdependencies in decision-making in the experiments and response behavior in the survey items by never asking survey questions relating to a particular preference in the same session in which the experiments to elicit the respective preference were conducted. Instead, experiments and surveys for a given preference were conducted one week apart. We restricted the subject pool to university students who had never participated in an experiment before, in order to rule out possible biases in behavior due to experiences in previous experiments. Out item selection procedure was extensive: we considered all possible combinations of a given number of survey items and chose the combination that best explained behavior in the experimental preference elicitation task.

We also offer a second, streamlined version of the preference module for applications in which time efficiency, and simplicity, are of paramount importance. A prime example is the case of an international telephone survey measuring preferences; with the telephone format and such a large-scale data collection effort, time constraints are likely to be severe. In the streamlined module some of the hypothetical experi-

74

ments were exchanged with more time efficient quantitative measures, which perform almost as well as the more extended versions in terms of association with the incentivized experimental measures. Other quantitative items that are relatively complex or difficult to explain were dropped altogether from the set of possible items. After selecting the items with best explanatory power from the modified battery of questions, we tested the performance of the resulting module in an international pilot study.[4] The streamlined module was implementable within tight time constraints, and detailed feedback elicited from respondents was encouraging in terms of confirming a common understanding of the preference module across a very diverse set of cultures. The feedback lead to a few minor wording changes that are incorporated in this version of the module.

Both versions of our preference module are of great use for scientists interested in measuring economic preferences. Our methodology leverages the strengths of both experimental and survey approaches to measuring preferences. It encompasses measures for six key economic preferences as provided conceptually by economic theory. It also offers an opportunity to standardize preference measures in non-experimental settings in order to increase comparability across studies and thereby accelerate scientific progress.

The remainder of the paper is organized as follows. Section 3.2 describes the design of the validation study and the procedures to elicit preferences in experiments and surveys. Section 3.3 explains the process and the criteria for the selection of items. It also presents the preference module with two items for measuring each of the six preferences, which performs best in out-of-sample prediction. Section 3.4 discusses additional important properties of the preference module, such as explanatory power and its suitability for non-student subject pools. Section 3.5 proposes the streamlined version of the preference module and discusses its applicability in representative and cross-cultural samples. Section 3.6 concludes.

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[4]The pilot study was run in 22 countries in Southeast Asia, Central Asia, East Africa, Eastern Europe and the Middle East.

3.2 Design of the Validation Study

3.2.1 Procedural Details

409 subjects participated in our study. Subjects were students from the University of Bonn, who were recruited using ORSEE Greiner (2004). They were required to have never taken part in an experiment before in order to minimize potential confounds due to earlier experiences in similar experiments. Subjects signed up for two laboratory sessions.[5] These were scheduled one week apart and run at the Laboratory for Experimental Economics at the University of Bonn in winter 2010/2011. Both sessions consisted of incentivized experiments and non-incentivized surveys, programmed in zTree Fischbacher (2007). Each session lasted about two hours. Payoffs earned in the incentivized experiments were paid out to the subjects at the end of each session.[6] Average earnings over both sessions amounted to 64 Euros (corresponding to approximately 83 US-dollars at the time of the experiment), including a fixed fee of 10 Euros for showing up to the second session.

In order to minimize spillovers between the experimental and the survey measures, e.g., because individuals might try to avoid cognitive dissonance (Festinger, 1957) and strive for giving consistent responses (Falk and Zimmermann, 2015, forthcoming), we never ran survey and experiment for the same preference in the same session. More specifically, we conducted all experiments relating to social preferences and all surveys relating to time discounting and risk taking in one session. The other session then contained the experiments relating to time discounting and risk taking as well as the surveys on social preferences. In addition, we reversed the order of experimental and survey elicitation of preferences for about half of our subjects. This design feature takes care of potential order effects, i.e., differences in behavior or responses due to differences in the way preferences were measured first. Table 3.1 gives an overview of the general study design.

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[5]We also conducted a pre-test with 80 students. This pre-test was intended to provide information on the duration and feasibility of the experiment. Experimental measures for negative reciprocity and altruism were not elicited in this pre-test and the constraints on the participants regarding previous participation were not applied.

[6]The payments resulting from the choice experiments on time discounting were delivered to the subjects in cash via regular mail, either at the same day of the session or 12 months later, depending on the payoff relevant choice.

Table 3.1: Overview: Study Design

	Week 1	**Week 2**
Group 1 (n=198)	Experiments on risk taking and time discounting; Surveys on social preferences	Experiments on social preferences Surveys on risk taking and time discounting
Group 2 (n=211)	Experiments on social preferences; Surveys on risk taking and time discounting	Experiments on risk taking and time discounting Surveys on social preferences

3.2.2   Preference Elicitation in Choice Experiments

We conducted standard economic experiments on risk taking, time discounting, altruism, trust, positive and negative reciprocity in order to obtain behaviorally valid preference measures. The experiments that were used in each of the preference dimensions are summarized in Table 3.2. A detailed description of the experiments is relegated to Appendix 3.7.1. Monetary stakes were presented to subjects in points, where 100 points equaled 80 Cents. Subjects received feedback about the outcome of the experiments only at the end of the sessions in order to limit the impact of possible income effects on subsequent choices within a session. All experiments involving social or strategic interaction were one-shot to isolate social preferences from repeated game motives. We also implemented a perfect stranger random matching protocol implying that subjects never interacted more than once with the same person. Subjects were informed about this at the beginning of each session as well as before each experiment involving social interaction.

For risk taking, time discounting, trust, and positive reciprocity we conducted two experiments each. These experiments had the same structure, but payoffs in the second experiment differed in small nuances, such that subjects were never asked to make tradeoffs between alternatives that involved the exact same amounts. For instance, the first lottery choice experiment involved 21 choices between a safe payment option, which increased in steps of 50 points from 0 points in the first choice to 1000 points in the last choice, and a lottery that yields 1000 points with probability 0.5 and 0 points otherwise. We perturbed the safe payments in the second experiment by adding or subtracting up to five points to each safe payment alter-

native. The number of points added or subtracted was determined by a randomly drawn integer value between -5 and +5. In the discounting experiments, in which subjects made choices between an immediate payment and a larger payment with a 12-months delay, we perturbed the delayed payment in the second experiment in the same manner.

The experimental measure of risk aversion was constructed by averaging over the switching rows in the two lottery choices experiments.[7] This averaging reduces measurement error compared to using a single experimental measure. Analogously, we constructed our experimental measure of time preference by averaging the switching rows in the discounting experiments.[8] Trust and positive reciprocity were elicited as first and second mover behavior, respectively, in two versions of the Investment Game (Berg et al., 1995). Each subject was in the role of the first and the second mover twice, such that overall each subject participated in four Investment Games. In one version, the amount sent by the first mover was tripled, in the other one it was doubled. For the second mover behavior, we implemented the strategy method (Selten, 1967). As our measure of trust, we took again the averages from the two decisions made as a first mover. For positive reciprocity, we first averaged all decisions from the strategy method in the two versions of the Investment Game. The average of these two amounts constitutes our preference measure of positive reciprocity. For altruism, we conducted a dictator game with a charitable organization as recipient. The chosen donation then constitutes our preference measure of altruism. For negative reciprocity, we conducted two different experiments. A subjects' minimum acceptable offer in an ultimatum game (Güth et al., 1982) serves as one assessment of negative reciprocity. We obtain a second assessment from a subject's investment into punishment after unilateral defection of their opponent in a prisoner's dilemma (Falk et al., 2005). In order to obtain our preference measure of negative reciprocity,

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[7]As is common for this type of elicitation methods, some subjects exhibit multiple switching points. We observe that about 86 individuals switch more than once from preferring the lottery to the safe payment in either of the two lottery choices experiments, 36 of them have multiple switch points in both experiments. For subjects who make that kind of inconsistent choices, we calculate the average switching row in each choice table and construct the experimental measure of risk aversion as the mean of the two averages.

[8]In the discounting experiments, we observe that around 7 percent of subjects switch more than once from preferring the early payment to the late payment. For these subjects we construct the experimental measure by taking the mean of the average switching row in the two experiments involving intertemporal choices.

we standardized both variables to account for the different response scales and then took the average.

Table 3.2: Overview: Experimental Measures

Preference	Experiment	Measure
Risk Taking	Two multiple price lists in which subjects choose between a lottery and varying safe options.	Average of rows in both price lists in which a subject switches from preferring the lottery to the safe option.
Time Discounting	Two multiple price lists in which subjects choose between a payment "today" and a larger payment "in 12 months".	Average of rows in both price lists in which a subject switches form preferring the early to the delayed payment.
Trust	First mover behavior in two trust games.	Average amount sent as a first mover in both trust games.
Altruism	First mover behavior in a dictator game with a charitable organization as recipient.	Size of donation.
Positive Reciprocity	Second mover behavior in two trust games (strategy method).	Average amount sent back in both trust games.
Negative Reciprocity	Investment into punishment after unilateral defection of the opponent in a prisoner's dilemma (strategy method) and minimum acceptable offer in an ultimatum game.	Average score: amount invested into punishment and minimum acceptable offer in an ultimatum game.

3.2.3 Preference Elicitation in Surveys

In the survey, we asked both quantitative and qualitative questions to measure a given preference. On average, we asked about 32 survey questions in each of the six preference dimensions. Qualitative questions included subjective assessments of the respective preference. Many survey items were taken or adapted from existing surveys, like the German Socio-Economic Panel Study (SOEP) or the National Longitudinal Study of Youth (NLSY), or from previous research (e.g., Weber et al., 2002; Perugini et al., 2003). Additionally, we designed and included a number of new items. Each battery of survey questions on a particular preference began with a qualitative measure, asking respondents to self-assess their preference "in general"

on an 11-point scale.[9] Next, respondents were asked to state how they believe others judge them with respect to that preference and to compare their preference to the attitude of others. Then, respondents had to assess their attitude in qualitative terms with respect to different domains, e.g., financial decision-making. This sequence of items was then followed by other qualitative survey items and quantitative items.

Quantitative items typically included a hypothetical version of the incentivized choice experiment. Since the multiple price lists used in the lottery choice experiment and in the inter-temporal choice experiment involve 30 choices and are rather time-consuming, we also included an alternative elicitation procedure in which subjects only had to make five sequential choices. In the five-question measure of risk preference all subjects first decided between the lottery versus a safe payment that slightly exceeds the expected value of the lottery. In the second decision (and all subsequent decisions) the lottery remained the same. If the participant chose the safe option in the first question, the safe option in the subsequent decision was smaller. If the participant chose the lottery, the safe payment increased. In the same manner, the safe option was increased or decreased in the third decision when the lottery or the safe payment were preferred in the second decision, respectively. This procedure was repeated five times. Figure 3.2 in the Appendix illustrates the method underlying this condensed quantitative measure, which is commonly referred to as an "unfolding brackets" method.[10] For the case of time discounting, an analogous unfolding brackets elicitation was used in which the early option was identical in every choice while the delayed option varied. The detailed procedures are described in Appendix 3.7.6 and 3.7.7. Finally, we asked all subjects to rate the reliability of their answers in the survey part.

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[9]An example of this type of question is the general risk question that was validated in Dohmen et al. (2011).

[10]In psychology this approach is often referred to as the "staircase method" Cornsweet (1962).

3.3 The Preference Module

3.3.1 Item Selection Procedure

Our aim was to develop a survey preference module that contains the set of items for each preference that best capture revealed preferences in incentivized laboratory experiments, in the sense of an optimal tradeoff between explanatory power and parsimony. While previous studies have typically focused on identifying survey measures that are significantly correlated with experimental preference measures, our approach is to identify the combination of survey items from an extensive battery of alternative survey items that best predicts choices in incentivized experimental preference elicitation tasks.

In order to identify the best linear combination of items for measuring a particular preference, we regressed each experimental preference measure on different combinations of the respective survey items. In the spirit of best subset selection, we considered all possible combinations of survey items as regressors.[11] We then took into account statistical model selection criteria, based on explanatory power and prediction error, in order to identify the preferred combination of survey items for each preference.[12]

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[11]Alternative selection procedures commonly applied in, e.g., personality psychology are stepwise selection procedures, including forward selection and backward elimination procedures. In forward selection approaches the analysis starts with the null model and chooses the predictor variable which explains the highest share of variance in the dependent variable. Given this predictor, the next variable is selected applying the same criterion. This process is repeated until no additional predictor variables can be found that meet a certain criterion, e.g., an F-statistic above a certain threshold (compare, e.g., Kadane and Lazar, 2004). In backward selection approaches the analysis starts with the model that includes all potential predictor variables and then, one by one, eliminates variables from the model that perform worst according to a predetermined criterion. Again, this procedure is repeated until only predictor variables are left in the model that fulfill a certain criterion. Clearly, the resulting model in both forward selection and backward elimination procedures strongly depends on the order of selecting (eliminating) items. Consequently, they do not necessarily result in the same model (see also Graybill, 1976). Stepwise regression approaches combine backward elimination and forward selection procedures and mitigate the problem of order dependence. However, all three approaches share the feature that not all possible models are evaluated. A further potential alternative would be to use the so-called Lasso-technique as introduced by Tibshirani (1996). Lasso is particular useful when best subset selection is not feasible, e.g. when there are more potential explanatory variables than observations, which is not the case in our setting in which we consider linear models only that are additively separable in explanatory variables. In fact, in this case Lasso selects largely the same modules. We deliberately did not consider non-linear and fully interacted prediction models for reasons of simplicity, to facilitate applicability and interpretation of the preference module, and to enhance comparability of results across studies.

[12]Another important ex ante criterion was cost efficiency, i.e., considering the tradeoff between

We implemented the selection procedure using a stepwise approach. In the first step, we ran OLS regressions and identified for every number of regressors the best model in terms of explanatory power, using an $\bar{R}^2$ criterion.[13] In the second step, we considered all models selected in the first step, i.e., one model for any number of regressors, and used information criteria to narrow down the number of candidate models.[14] Since these information criteria differ with respect to the extent to which the inclusion of additional regressors is penalized, the different information criteria will not necessarily all favor the same model. In our case, the two-item and three-item models were among the set of candidate modules for each preference, in the sense that they were reasonably close according to the different information criteria.[15] Since we value brevity of the preference module, we favor the Bayesian information criterion (BIC), which contains a larger penalty for additional regressors than the Akaike information criterion (AIC). According to the BIC, the two-item model is superior for altruism, negative reciprocity and trust, while the three-item model is selected for risk taking, time discounting and positive reciprocity.[16]

In the final step, we considered the predictive power of our candidate modules, in order to identify the preferred preference module. Whenever possible, we considered out-of-sample predictive power, making use of a truly independent sample of 80 subjects for whom we had collected data on the same experimental and survey measures on risk taking, time discounting, positive reciprocity and trust. For each of

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predictive power and conciseness of the module. It turned out, however, that the statistical criteria were not in conflict with the cost criterion as favored combinations are parsimonious in terms of the number of items.

[13]In the following we will only report results from OLS regressions. However, all results reported here are robust to estimating Ordered Probit models and selecting items using the criteria of maximum log-likelihood or Pseudo-$\bar{R}^2$.

[14]Naturally, R^2 will increase with the number of regressors, but adding regressors may result in overfitting. Different criteria such as adjusted $\bar{R}^2$, the Akaike information criterion (AIC), or the Bayesian information criterion (BIC) contain a penalty term for the number of items.

[15]In particular, in our case, the one-item module is never selected, irrespective of whether we consider AIC or BIC. Many previous studies have relied on only one item, which suggests that many of the results in previous literature understate the strengths of correlations between different preference dimensions or the strength of estimated relationships between preferences and outcome variables, due to attenuation bias that results from measurement error. Moreover, studies using survey measures of preferences are often not based on survey preference measures that exhibit the highest correlation with the experimental preference measure (cf. Online Appendix C). The pairwise correlations of single items with the experimental preference measure are also informative with respect to comparability of results across existing studies that are based on single but different measures.

[16]The AIC, on the other hand, favors the two-item module only in the case of positive reciprocity.

these preferences we determined the predicted values of the respective experimental preference measure according to the candidate models that differ with respect of the number of items.[17] For each preference, we then assessed the predictive power of the different candidate models by comparing their mean squared prediction error (MSPE). For all four preference dimensions, the MSPE is minimized for the model with two items.

Since data on altruism and negative reciprocity is lacking in our independent sample, we evaluated the predictive power of the models with different numbers of items for these preference dimensions based on cross-validation using the original sample.[18] In line with our out-of-sample prediction results for the other four preferences, the cross validation errors are smallest when using the two-item models for negative reciprocity and altruism.[19] As a result, we prefer two-item models for each preference dimension.

3.3.2 Survey Items Contained in the Preference Module

Table 3.3 displays the items that were selected for the preference module with two survey questions for each preference dimension. Appendix 3.7.3 presents the wording of the survey items in the preference module, translated from German to English; the original wording of the items in German is provided in section D in the online appendix.

A notable feature of the preference module is its symmetry: For most preference dimensions, it contains a measure based on a hypothetical choice experiment and a qualitative item.[20] These two types of measures are complementary in the sense that the quantitative measure is akin to the standard revealed preference approach whereas the qualitative item is a subjective self-assessment. Previous research has shown that subjective assessments with abstract framings can lead to strong all-

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[17]Predicted values were calculated as the product of the vector of observed answers to the specific preference module and the vector of estimated coefficients from the regression of the experimental preference measure on the respective preference module in the main sample on which the selection procedure was based.

[18]Our cross-validation procedure entails that the sample is randomly split into k partitions. One partition is used as a validation sample, whereas the remaining $k - 1$ samples are used as the "training" sample.

[19]Our results obtain using $k = 5$ or using $k = 10$ partitions.

[20]The only exception is positive reciprocity.

around predictors of life choices across many different life contexts. For example, a general assessment of willingness to take risks can predict a range of behaviors ranging from holding risky assets, to being self-employed, to smoking (Dohmen et al., 2011). Quantitative survey measures that involve explicit monetary stakes are no exception, as they are somewhat tied to the context of financial decision making by construction; they may be better predictors of financial decisions in life than qualitative measures of a general disposition, but less predictive of choice in other domains. The preference module has an attractive balance between both approaches.

Table 3.3 also documents the correlations between the module items and the respective behavioral measures. The last column of Table 3.3 provides estimated OLS coefficients obtained from a multivariate regression of the standardized experimental preference measure on standardized measures of the two survey items for the respective preference dimension. In applications, these coefficients can be used to calculate weights, and then construct measures for each preference as the weighted sum of the two items that capture the respective preference.

3.4 Properties of the Preference Module

3.4.1 Correlation between Survey Preference Measures and Experimental Preference Measures

As a first indication of the quality of the preference module, we present the correlations between the experimental preference measure and its predicted value based on the two survey items. The correlations are 0.4079 for risk taking, 0.5861 for time discounting, 0.6748 for trust, 0.4235 for altruism, 0.5771 for positive reciprocity, and 0.3729 for negative reciprocity. One might be inclined to evaluate these correlations against a benchmark of 1. This benchmark would only be appropriate, however, if the experimental preference measures and the survey based preference measures were measured without error and perfectly aligned with the respective underlying preference. The assumption that there is no measurement error is unlikely to be correct in the case of preference measures. For example, measuring preference parameters that are inherently continuous on a discrete grid, the typical approach in

Table 3.3: The Preference Module

Preference		Item Description	Correlation	OLS Coeff.
Risk	risk_quant	Multiple price list (31 hypothetical choices between a lottery and a safe option)	0.4095***	0.2758***
Taking	risk_qual	How do you see yourself: Are you a person who is generally willing to take risks, or do you try to avoid taking risks?	0.3524***	0.2034***
Time	time_quant	List of 25 hypothetical choices between an early payment "today" and a delayed payment "in 12 months"	0.5826***	0.4849***
Discounting	time_qual	In comparison to others, are you a person who is generally willing to give up something today in order to benefit from that in the future?	-0.4039***	-0.1712***
Trust	trust_quant	Hypothetical investment game: first mover behavior	0.6201***	0.6289***
	trust_qual	Self-assessment: As long as I am not convinced otherwise, I assume that people have only the best intentions.	0.2829***	0.1331***
Altruism	altr_quant	You won 1,000 Euro in a lottery. Considering your current situation, how much would you donate to charity?	0.3913***	0.1845***
	altr_qual	How do you assess your willingness to share with others without expecting anything in return when it comes to charity?	0.3845***	0.3210***
Positive	posrecip_quant1	Hypothetical investment game: second mover behavior	0.5560***	0.4857***
Reciprocity	posrecip_quant2	Hypothetical scenario: Which bottle of wine do you give as a thank-you gift?	0.3530***	0.1640***
Negative	negrecip_quant	Minimum acceptable offer in hypothetical ultimatum game	0.3416***	0.3284***
Reciprocity	negrecip_qual	How do you see yourself: Are you a person who is generally willing to punish unfair behavior even if this is costly?	0.1609***	0.1479***

See Appendix 3.7.3 for the exact wordings of the survey questions. The column "Correlation" displays Spearman correlations between the survey item and the respective experimental measure. The final column displays OLS coefficients in a regression of the standardized experimental measure on the standardized module items. For details see the regression tables in section B in the online appendix. ***, **, and * denote significance at the 10-, 5-, and 1-percent level, respectively.

choice experiments, gives rise to measurement error (see Einav et al., 2012).

With measurement error, the correlation between the experimental preference measure and a candidate item from our battery of survey questions would be smaller than one, even if the survey item measured the underlying preference equally well as the experimental measure. It seems therefore more adequate to consider a benchmark that recognizes the consequences of measurement error. An obvious benchmark is the correlation θ between two measurements that arise from the repetition of the exact same experiment because the best predictor of behavior in an experiment is arguably a prior choice in the same experiment. In what follows, we use a test-retest sample to measure θ. This test-retest correlation then becomes our benchmark for the highest possible correlation one might achieve between survey measures and experiment, should the two be perfectly aligned. We compare the actual explanatory power of the survey measures to this revised benchmark.

In order to assess the size of measurement error in the experimental preference measures, we conducted additional experiments with 44 subjects, who participated in preference elicitation experiments twice. The experimental sessions were scheduled one week apart (there was no perturbation of experimental parameters across sessions). The data on two identical experimental measures elicited one week apart allow us to compute the test-retest correlations (i.e., θ) between two experimental measures of the same underlying preference.

We estimate the test-retest correlation (more precisely, the square of the correlation) by regressing the preference measure revealed in the experiments in the first session on the respective preference measure obtained in the second session and calculating the R^2 for this regression. The share of variance that can be explained by the second experimental measure is substantially lower than 1, indicating the presence of measurement error in the experimental measures. The correlations are 0.3469 for risk taking, 0.6715 for discounting, 0.5986 for trust, and 0.4203, 0.4336, 0.4446 for altruism, positive reciprocity and negative reciprocity respectively.[21] The R^2-values of these regressions serve as a sensible benchmark against which to evaluate the explanatory power of our preference module, since these values measure the explanatory power for behavior in the experiments of an identical repeated measure

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[21] A more detailed regression table is relegated to section B in the online appendix.

of the experiment itself. Compared to this benchmark, our survey module achieves high explanatory power.

3.4.2 Out-of-Sample Prediction

After having established the superiority of the two item survey module in out-of-sample prediction relative to longer modules in the previous section, we now discuss the out-of-sample performance of the two item survey module in absolute terms. For the subjects in our pretest panel we used their survey responses to predict their choices in the four experimental preference elicitation tasks (measuring risk and time preferences, trust and positive reciprocity), and regressed the actual choices on the predicted choices. If our preference module reliably captures the preferences of individuals in this sample, one would expect the intercept of the regression of actual on predicted choices to be zero and the coefficient of the predicted value to be 1. In fact, we cannot reject the hypothesis that the constant is zero and the slope coefficient equals one for all preferences, except for trust, at the 10 percent significance level. For trust, we find that the slope coefficient is not statistically different from one if we suppress the constant in the regression. It is also reassuring that predicted and actual choices are strongly and statistically significantly correlated. The correlations are 0.2919 for risk preferences, 0.5868 for time discounting, 0.2629 for trust, and 0.4424 for positive reciprocity.

3.4.3 Validity in Non-Student Samples

Conceptually, the module will be behaviorally relevant for non-students as long as the correlations between survey items and experiments are similar to those in our student sample. While the distributions of preferences may differ for students and non-students, there is no particular reason to think that the correlation structure should differ. Even if it does, it seems likely that the same types of survey items would still be selected as best predictors for non-students as for students; in the student sample the top two predictors are typically superior to other measures by a substantial margin. Moreover, the quantitative survey items in the module closely resemble the experimental measures that are widely used to elicit preferences in non-

student samples. Hence, there is no compelling reason why the correlations between these hypothetical and incentivized measures should exist only among students.

Empirically, there is evidence that survey measures are significantly correlated with experimental preference measures in representative, non-student samples. For example, Fehr et al. (2003) used a representative sample of adults, and documented a significant correlation between subjects' behavior in an incentivized investment game, and survey measures on trust of the type contained in our preference module. Likewise it has been shown that answers to the qualitative survey question to elicit risk attitudes, contained in our preference module, are significantly correlated with incentivized lottery choices in a large representative subject pool (Dohmen et al., 2011). In fact, they report a correlation coefficient between the survey measure and behavior in the lottery choice experiment in their representative sample that is almost identical to the one in our validation sample consisting of students.[22] It is also notable that the correlation is not significantly different for students versus non-students in their representative sample. Similarly, Ziegelmeyer and Ziegelmeyer (2012) predict risk-taking behavior in an alternative lottery choice experiment (Holt and Laury, 2002) using the same survey item that is part of our module. In addition, the qualitative survey risk measure contained in our preference module has previously been administered in the German Socio-Economic Panel Study, and other large representative surveys in the US, Asia and Australia as well as in other European countries. Various studies have documented that answers to this question are related to risky behaviors in many contexts of life, for example, occupational choice and self-employment, geographical mobility, ownership of risky assets, as well as smoking (see, e.g., Barasinska et al., 2012; Bauernschuster et al. 2014; Bonin et al., 2007; Caliendo et al., 2009; Dohmen et al., 2011; Fouarge et al., 2014; Jaeger et al., 2010). These findings illustrate that the types of survey items selected in our preference module provide behaviorally valid preference measures in non-student samples.

There is also evidence that items from our preference survey module are valid across cultures. For example, recent empirical work by Vieider et al. (2015) uses

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[22]The correlations are 0.25 in the representative sample of Dohmen et al. (2011), and 0.24 in our validation sample if we focus on the same survey measure for predicting behavior in a single risk experiment (as shown above, the correlation is even higher for the validation sample if we use choices from both risk aversion experiments).

the same qualitative measure of risk attitudes that is included in our module and documents that it correlates with incentivized lottery choice experiments conducted in 30 different countries. In addition, Hardeweg et al. (2013) replicate the validation exercise of Dohmen et al. (2011) and confirm the significant relationship between this risk question and incentivized lottery choices for a representative sample of 900 inhabitants of rural Northern Thailand. Ding et al. (2010) corroborate these results for a sample of 121 Beijing University students. Taken together, this evidence suggests that the survey module can provide a useful tool for preference elicitation also in an international context.

3.5 A Streamlined Version of the Preference Module

Our survey module offers a reliable, easily implementable and low cost alternative to conducting incentivized experiments. Nevertheless, there are applications for which our module will not be ideal, as some of the quantitative items either require instructions that are as complex as corresponding experiments (e.g., the hypothetical investment game) or entail a considerable number of decisions (e.g., choice tables for eliciting risk and time preferences). Particularly if time constraints are severe or if respondents have limited cognitive capacity, an even simpler and shorter module seems useful, even if it comes at some costs in terms of lower explanatory power. A streamlined module is also particularly useful for measuring preferences in large scale, representative, and cross cultural surveys, as these may use telephone, have severe time constraints, and cover a subject pool that is heterogeneous in terms of education ad cognitive capacity. With this in mind, when developing the streamlined version we also paid particular attention to ensuring that the preference survey measures can be implemented across different cultural backgrounds, and are understandable and measure the same preference across different cultures.

Streamlining the module involved several steps. First, we discarded the most lengthy or complicated items, i.e., the (hypothetical) investment game, ultimatum game, and the time-consuming lists of (hypothetical) choices between safe payments

and a lottery and between early and delayed payments. As we discuss in detail below, in the majority of cases, our set of remaining survey items included simpler analogues of the discarded items. As a result, this restriction ultimately only led to a minimal reduction in explanatory power (R^2). Second, we made some minor wording changes where this seemed appropriate, for cultural neutrality. Third, using the modified sets of candidate measures, we then implemented the same selection procedure we used for the original module version. We only deviated from this procedure in a couple of cases, when other relevant features - such as implementability or comparability of preference measures between the two module versions - would otherwise have been jeopardized. Importantly, this never resulted in a substantial decrease of adjusted R^2, our main selection criterion. In our final step we tested the resulting streamlined version of our preference module in collaboration with Gallup Europe in the field in 22 countries representing different cultural backgrounds. Using respondent's feedback from this pilot study we made some additional minor changes to the wording. In what follows, we describe the development of the adjusted module for each preference and describe the composition and nature of the multinational pilot study in more detail.

For the sub-modules for risk taking and time discounting, we discarded the choice lists from the list of candidate items, and ran the selection procedure described in section 3.3 on the restricted set of items. For risk preferences, the "staircase" procedure for a hypothetical lottery choice was selected. This quantitative measure is very comparable to the choice list measure, as it contains the same lottery. Yet, it is much more time-efficient to use "staircase" procedures, as they only require five interdependent choices (lottery vs. safe payments and early vs. delayed payments, respectively).[23] Both preference measures are highly correlated with experimental measure of risk preference (see section C in the online appendix). The other item selected for risk was the same qualitative measure selected in the original module. The resulting reduction in explanatory power of the streamlined version compared to the original version in terms of R^2 is only 0.02. In the case of time discounting, the item selection procedure also selected the "staircase" measure for intertemporal choice (see Appendix 3.7.7), which mirrors the hypothetical choice list for the

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[23]The staircase procedures are presented in detail in Appendix 3.7.6

same intertemporal trade-off as in the original version of the module, but it yielded a slightly different subjective self-assessment than the one in the original module version. Instead of the item asking for a self-assessment of one's willingness to abstain from something today in order to benefit from that in the future *in comparison to others*, the item selected asks for the same self-assessment *in general*. Since this change was only minor relative to the original module we modified the sub-module accordingly. The resulting reductions in adjusted R^2 compared to the original module version are again rather modest (reduction in R^2 by 0.04).

For positive reciprocity, we discarded the hypothetical choices as a second mover in the investment games before running the selection procedure. Corresponding to the original sub-module, the procedure selected the quantitative item measuring one's willingness to reciprocate by asking for which wine bottle (a cheaper or a more expensive one) one would give to a stranger in order to reciprocate kindness in a hypothetical scenario. Since giving a bottle of wine is a very common and popular gesture in Western industrialized societies but very uncommon or even inappropriate in other cultures, e.g. Muslim societies, we replaced "bottles of wine" with the more neutral term "thank-you-gift". As a second item, the selection procedure picked a simple subjective self-assessment: "When someone does me a favor I am willing to return it". The resulting modified sub-module for positive reciprocity comes with a reduction in adjusted R^2 to 0.19 in our experimental subject pool.

In the case of negative reciprocity we discarded the hypothetical experiment. The item selection procedure resulted in selecting two qualitative self-assessments, the first of them being the "general willingness to punish"-item that was also included in our original module version. In this case, there was a more substantial reduction in adjusted R^2 relative to our original module (0.0367 vs. 0.1342). Since the second item strongly resembled the first item ("general willingness to punish"), we decided to instead include an item asking for one's willingness to take revenge, thereby adding a more emotional and less neutral item to the sub-module. This change resulted in a negligible reduction of adjusted R^2 (0.0320 vs. 0.0367).

Due to severe time constraints in the pilot study we could only include one item for trust. We therefore discarded the hypothetical experiment which would have been discarded anyways due to its length and complexity and - since we did not have

an adequate and implementable alternative for the hypothetical experiment - kept the qualitative self-assessment of the original module version.[24] While running the selection procedure for a one-item sub-module without the hypothetical experiment would have resulted in a different item, we decided to stick to the original module version as closely as possible in order to ensure a maximum degree of comparability of preference measures between studies using the original module version and those using the adapted streamlined version. In the case of altruism, we did not make any changes to the module items since the hypothetical experiment was neither lengthy nor complicated and the qualitative self-assessment seemed unproblematic for the purpose of a multinational study.

In the next step, we tested the streamlined module in an in-depth pilot study in 22 countries. In collaboration with Gallup Europe, we surveyed respondents from 10 countries in central Asia (Armenia, Azerbaijan, Belarus, Georgia, Kazakhstan, Kyrgyzstan, Russia, Tajikistan, Turkmenistan, Uzbekistan), 2 countries in South-East Asia (Bangladesh and Cambodia), 5 countries in Southern and Eastern Europe (Croatia, Hungary, Poland, Romania, Turkey), 4 countries in the Middle East and North Africa (Algeria, Jordan, Lebanon, and Saudi-Arabia), and 1 country in Eastern Africa (Kenya).[25] In each country, the sample size was 10 to 15 people. Overall, more than 220 interviews were conducted. In most countries, the sample was mixed in terms of gender, age, educational background, and area of residence (urban vs. rural). For all items involving hypothetical monetary amounts we adjusted the stake sizes for each country in terms of their real value such that they represent the same share of a country's median income in local currency as the share of the amount in Euro of the German median income, where our validation study had been conducted. Monetary amounts used in the validation study with the German sample were rounded numbers to facilitate easy calculations (e.g., the expected return of a lottery with equal chances

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[24]This leads to a substantial reduction in adjusted R^2 from 0.44 to 0.07 due to the omission of the hypothetical experiment.

[25]Gallup Europe ensured that the items of the preference module were translated into the major languages of each target country, using state-of-the-art techniques. The translation process involved three steps. As a first step, a translator suggested an English, Spanish or French version of a German item, depending on the region. A second translator, being proficient in both the target language and in English, French, or Spanish, then translated the item into the target language. Finally, a third translator would review the item in the target language and translate it back into the original language. If differences between the original item and the back-translated item occurred, the process was adjusted and repeated until all translators agreed on a final version.

of winning and losing) and to allow for easy comparisons (e.g., 100 Euro today versus 107.50 in 12 months). To proceed in a similar way in all countries, monetary amounts were always adjusted to the next "round and easy" number after adjusting the amounts in terms of their real values.[26]

In order to detect potential difficulties in the understanding of module items and differences in the respondents' interpretation, respondents were explicitly asked to give extensive feedback with respect to the appropriateness and understandability of the module. In particular, we asked respondents to rephrase the meaning of the items in their own words and to state any difficulties in understanding the items.[27] If they encountered difficulties in understanding or interpreting items, respondents were asked to make suggestions on how to modify the wording of the item in order to attain the desired meaning.

Overall, the understanding and implementability of our module was very good. Nevertheless, respondents' feedback induced some additional changes to some items. In terms of wording changes, the use of the term "lottery" in hypothetical risky choices was troubling to some Muslim participants and some refused to answer the item completely since gambling is a taboo (*haram*) in Islam. As a consequence, we dropped the term "lottery" and replaced it with the more neutral but equally accurate term "random draw". Second, the term "charity" caused confusion in Eastern Europe and Central Asia, so it was replaced it with "good cause". Third, some respondents had difficulties answering the question asking about one's willingness to punish unfair behavior without knowing who was treated unfairly. We therefore decided to split the question into two separate items, one item asking for one's willingness to punish unfair behavior towards others, and another asking for one's willingness to punish unfair behavior towards oneself. Fourth, some participants, especially in countries with current or relatively recent phases of volatile and high inflation rates, stated that their answer in questions involving intertemporal trade-offs would depend on the rate of inflation, or said that they would always take the immediate payment due to uncertainty with respect to future inflation. Therefore,

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[26]While this necessarily resulted in some (minor) variations in the real stake size between countries, it minimized cross-country differences in the understanding the quantitative items due to difficulties in assessing the involved monetary amounts.

[27]For example, respondent were explicitly asked to explain a "50-percent chance" in their own words and give their own interpretation of "safe payment".

we added the following phrase to each question involving hypothetical choices between immediate and future monetary amounts: "Please assume there is no inflation, i.e., future prices are the same as today's prices." The final version of the streamlined preference module is presented in Table 3.4. Finally, the survey questions were brought into a format that is consistent with the Gallup World Poll questionnaire style. For example, the first question of the module, which happened to be the qualitative survey question on risk taking, was commenced by the request "Please tell me". The complete module version including exact wordings is relegated to Section 3.7.8 in the appendix.

3.6   Conclusion

This paper presents an experimentally validated survey module to measure six key economic preferences – risk aversion, discounting, trust, altruism, positive and negative reciprocity – in a reliable, parsimonious and cost-effective way. The paper offers two versions of the module. One provides the maximum explanatory power, subject to having a parsimonious number of survey items (two items) per preference. We strongly recommend this tool for eliciting preferences in small to medium-scale studies among (fairly) educated respondents, such as lab experiments and field experiments. This version of the module is also well-suited for surveys that use detailed questionnaires or that are based on written or computer-assisted personalized interviews (CAPI). The other version of the module is a more streamlined one that prioritizes time efficiency, and simplicity, at the expense of a modest reduction in explanatory power. This streamlined version of the module is particularly useful in the context of large-scale international surveys among respondents sampled from the entire age and education spectrum and covering individuals from diverse socio-economic backgrounds.

The streamlined version of the module is also well-suited for all kinds of survey modes, including telephone surveys. Indeed, it has now been successfully implemented within framework of the Gallup World Poll 2012, a major international

Table 3.4: The Streamlined Module

Preference	Module Items
Risk Taking	1. *Staircase measure (five interdependent choices between a lottery and a safe option)* 2. Please tell me, in general, how willing or unwilling you are to take risks.
Time Discounting	1. *Staircase measure (five interdependent choices between an early and a delayed amount of money)* 2. How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?
Trust	1. I assume that people have only the best intentions.
Altruism	1. *Hypothetical donation.* 2. How willing are you to give to good causes without expecting anything in return?
Positive Reciprocity	1. *Hypothetical choice: size of a "thank-you"-gift.* 2. When someone does me a favor I am willing to return it.
Negative Reciprocity	1. If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so. 2. How willing are you to punish someone who treats **you** unfairly, even if there may be costs for you? 3. How willing are you to punish someone who treats **others** unfairly, even if there may be costs for you?

survey. This has yielded the Global Preference Survey (GPS) data, which has preference measures for more than 80,000 drawn as representative samples in each of 76 countries worldwide (see Falk et al., 2015, for a detailed description of this data set). Importantly, these data reveal the behavioral validity of our preference module (Falk et al., 2015). For example, more risk tolerant individuals are more likely to become self-employed and are more likely to smoke. Likewise, patient individuals are more likely to save and have higher educational attainment around the world. Finally, social preferences are strongly correlated with a broad range of prosocial behaviors and outcomes such as donating, volunteering time, assisting strangers, helping friends and relatives, or family structure.

Both versions of the preference module share several desirable features. First, the module items are experimentally validated. The ability of the items to explain behavior in incentivized choice experiments helps ensure that they are meaningful for predicting choices under real incentives, mitigating one of the major concerns about hypothetical questions. The selected items are not just significant predictors of behavior, but are jointly the best predictors out of a large set of alternative measures. The validation is based on a consistent research design across preferences, and applies state-of-the-art experimental techniques and transparent, quantitative criteria for module selection. Second, the modules consist of a balanced mix of qualitative self-assessments and questions involving quantitative hypothetical trade-offs. This gives the module an attractive balance between different approaches to assessing preferences. Third, the module has a wide range of possible applications. The two versions can be implemented in various survey modes, including modes with tight time constraints. Some module items have already been validated in representative samples, and in different countries. We additionally conducted an international pilot in order to verify comprehension and implementability of the module across very different cultures. Thus, the two versions of the module can be applied to a range of different subject pools, from lab experiments, to large representative samples, or to samples that are culturally very heterogeneous. Fourth, by providing an attractive and low cost approach to measuring preferences the module has the potential for widespread adoption, with potentially significant positive externalities in terms of easier comparison of results across studies.

3.7 Appendix to Chapter 3

3.7.1 Design of Experimental Preference Elicitation Tasks

Risk Taking   We used a multiple price list format to elicit how subjects trade off risky payments and sure payments. Subjects made choices in two tables. In each of the 21 rows of a given table they had to choose between a safe payment and a lottery that yielded 1000 points with probability 0.5 and 0 points otherwise. The lottery was always the same in all rows of both price lists, while the safe payment varied. We call these tables "price lists" as is commonly done in the literature. In one price list, we increased the safe payment in steps of 50 points from 0 points in the first choice to 1000 points in the last choice. In the other price lists we perturbed these safe payments by adding or subtracting up to five points to each safe payment alternative. The number of points added or subtracted was determined by a randomly drawn integer value between -5 and +5. These integer values were randomly drawn once and for all before the experiment was programmed. As a result, all subjects faced the same lists of choices. After subjects had made their choices, one of the choices was randomly selected for payment. Subjects were informed about this procedure in advance. The row in which a subject switched from preferring the lottery to preferring the safe payment informs us about the subjects' risk preferences. Earlier switching points indicate a lower certainty equivalent than later switching points.

Time Discounting   In order to obtain a measure of the subjects' willingness to trade off monetary payoffs at two different points in time we adapted a the design from Dohmen et al. (2010), and asked subjects to make choices in two price lists. In both price lists, subjects had to trade off a payment of 400 points "today" and a higher payment that would be received 12 months in the future. In one price list, we increased the delayed amount such that the implied annual return from waiting would rise in steps of 2.5 percentage points from 0 percent in the first row to 60 percent in the 25th row, assuming semiannual compounding. In the second price list we perturbed the actual delayed payments by adding or subtracting an amount of up

to 0.6 points. Again, one choice made in the two price lists was randomly selected by the computer for payment. Subjects were informed about this procedure in advance.

We also notified subjects ex ante about the payment mode. In particular, they were told that any payment resulting from this experiment would be delivered to them via regular mail. If they chose the payment "today" the respective amount would be sent on the same day. If they chose the payment "in 12 months", it would be sent to them exactly 12 months after the experiment. By keeping the payoff mode identical over all time horizons we can rule out concerns about differential credibility of payments dependent on timing, or simply a taste for a certain payoff mode, as drivers of decision making. These features were made very salient to subjects: To enhance credibility an envelope was placed in each cubicle and subjects had to write on the envelope the address to which they wanted the payment delivered. In order to allow us to identify the relevant payment they also had to note their identification number on the envelope. No participant expressed any concern with respect to this procedure.

The row in which a subject switched from preferring the earlier payment to the larger delayed payment (or, equivalently, the implied annual rate of return in the switching row) provides a measure of impatience.

Trust We conducted two versions of the Investment Game as introduced by Berg et al. (1995). We refer to this as the Trust Game. In one version of this game the amount sent by the first to the second mover was doubled by the experimenter, in the second version the amount was tripled. In every version of this experiment both subjects were endowed with 500 points. The choice set of the first mover was restricted to amounts in $\{0, 50, 100, ..., 500\}$, because we applied the contingent response method for the second mover. Each subject acted in the role of the first and second mover in each version, such that overall each subject took part in four Investment Games. All outcomes of the four decisions of the Investment Games were payoff relevant. The average amount sent as a first mover in the two versions serves as our measure of the subjects' willingness to trust strangers.

Altruism   Subjects were endowed with 300 points and had to decide how many of these points to assign to a charitable organization. We gave them a list of well-established and well-known charitable organizations with various purposes but they could also name a different charitable organization to which they wanted the money to be donated. The list of charitable organizations included: Brot für die Welt, Kindernothilfe, German Red Cross, Welthungerhilfe, Bund für Umwelt und Naturschutz Deutschland, Greenpeace, Terre des Hommes, and Aktion Mensch. At the end of the laboratory session we gave the subjects an address of a website on which they could look up all donations made to the charitable organizations. Subjects were informed again about the possibility to check their donation after all sessions had been conducted and the money had been transferred to the charitable organizations. This was done in order to ensure credibility and transparency of the procedure. The amount an individual transferred to charity serves as a measure of their altruistic inclination.

Positive Reciprocity   We elicited positive reciprocity from second mover behavior in the Trust Games described above. The use of the contingent response method for second mover behavior allowed us to measure how much a subject wanted to send back for each possible amount sent to them by the first mover. The payoff relevant choice was the one corresponding to the actual choice made by the first mover. Average second mover behavior in the Investment Games then constitutes our behavioral measure of the individual's willingness to reciprocate positively. Subjects were informed about their opponents' decisions and the resulting payoffs at the end of the laboratory session.

Negative Reciprocity   We conducted two different types of experimental game in order to elicit subjects' willingness to reciprocate negatively. First, subjects took part in two Ultimatum Games as introduced by Güth et al., 1982. Subjects were randomly assigned the role of the proposer in one game and the role of the responder in the other game. Proposers had to decide how many of 500 points they wanted to offer to the responder. Responders, in turn, had to indicate their minimum acceptable offer and this was taken as a first measure of the individuals' level of negatively reciprocal

inclination. A higher minimum acceptable offer increases the rejection probability, and is hence a measure of the higher willingness to forego a monetary payoff in order to reduce the payoff of the proposer.

We also conducted a Prisoner's Dilemma with a subsequent punishment stage (see e.g., Falk et al., 2005 or Fehr and Gächter, 2000). The Prisoner's Dilemma was framed as a project in which both players could decide to participate or not. If both players decided to participate they both received 480 points. If both players decided not to participate, both received 300 points. If one player decided not to participate while the other decided to do so, the former received 540 points while the latter received 240 points. Figure 3.1 illustrates the payoff structure of this part of the experiment. First, subjects had to decide how many points to invest into punishing their opponent contingent on every possible first stage outcome. Punishment was costly.[28] Then they were asked to decide whether they wanted to participate in the project or not. All decisions were taken simultaneously.

As a measure of the individuals' willingness to reciprocate negatively we consider behavior in both experiments, i.e., minimum acceptable offer in the Ultimatum Game and the amount invested into punishment given unilateral defection of the other player. We standardized both measures to account for the different response scales and took the average. This constitutes the score for the level of negative reciprocity.

Player 2

		In	Out
Player 1	In	480, 480	240, 540
	Out	540, 240	300, 300

Figure 3.1: Payoff Matrix: Prisoner's Dilemma

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[28]We implemented two different punishment technologies: in 7 sessions the technology was such that each point invested into punishment resulted in one point being deducted from the opponent. In the other sessions each point invested into punishment lead to three points being deducted from the other player.

3.7.2 All Survey Items

This section presents all survey items on preferences that subjects answered.[29] Unless stated otherwise, all items were answered on an eleven-point scale from 0 to 10. For example, all items asking for one's willingness to behave in a certain way were answered on a scale from 0 meaning "not willing to do so" to 10 meaning "very willing to do so". Likewise, items asking for how well a statement describes the participant as a person were answered on a scale from 0 "does not describe me at all" to 10 "describes me very well". Items which were not answered according to this pattern are, for example, hypothetical experiments. In these cases, the potential answers are presented at the end of the respective item.

Risk Taking

1. Staircase Measure (see Appendix 3.7.6)

2. List of 31 hypothetical choices between a lottery (300 Euro with a 50 percent chance, 0 Euro with a 50-percent chance), which is the same in all choices, and varying safe options (starting at 0 Euro and increasing to 300 Euro in increments of 10 Euro). Answer options: lottery or safe payment.

3. Sind Sie im Allgemeinen ein risikobereiter Mensch, oder versuchen Sie, Risiken zu vermeiden? [*Generally speaking, are you a person who is willing to take risks or do you try to avoid risks?*]

4. Sind Sie im Vergleich zu anderen ein risikobereiter Mensch, oder versuchen Sie im Vergleich zu anderen, Risiken zu vermeiden? [*In comparison to others, are you a person who is willing to take risks or do you try to avoid risks?*]

5. Schätzen andere Sie im Allgemeinen als einen risikobereiten Menschen ein, oder schätzen andere Sie als jemanden ein, der versucht, Risiken zu vermeiden? [*Do other people assess you as a person who is willing to take risks or as a person who tries to avoid risks?*]

6. Wie schätzen Sie Ihre Risikobereitschaft in Bezug auf folgende Bereiche ein? [*How do you assess your willingness to take risks in the following contexts?*]

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[29]Subjects were required to answer each question, i.e. they did not have an option to skip items.

(a) Wenn es um Geldanlagen geht? [*When it comes to financial investments?*]

(b) Wenn es um wichtige Entscheidungen im Leben geht? [*When it comes to important decisions in life?*]

(c) Wenn es um die berufliche Karriere geht? [*When it comes to your professional career?*]

(d) Wenn es um Freizeit und Sport geht? [*When it comes to leisure and sports?*]

(e) Wenn es um Verhalten im Straßenverkehr geht? [*When it comes to behavior in road traffic?*]

(f) Wenn es um den Umgang mit anderen Menschen geht? [*When it comes to dealing with other people?*]

7. Wie wahrscheinlich ist es, dass... [*How likely is it, that...*][30]

(a) Sie zugeben, dass Ihr Geschmack sich von dem Ihrer Freunde unterscheidet? [*you admit that your tastes are different from those of your friends?*]

(b) Sie in der Wildnis zelten, fernab der Zivilisation oder eines Campingplatzes? [*you go camping in the wild, far away from civilization or campgrounds?*]

(c) Sie Ihr Tageseinkommen beim Pferderennen für Wetten einsetzen? [*you bet a day's income at the horse races?*]

(d) Sie illegale Drogen für Ihren eigenen Konsum kaufen? [*you buy an illegal drug for your own use?*]

(e) Sie in einer Klausur versuchen zu täuschen? [*you try to cheat on an exam?*]

(f) Sie einen Tornado oder Hurricane im Auto verfolgen, um spektakuläre Fotos zu schießen? [*you chase a tornado by car to take spectacular photos?*]

(g) Sie 10% Ihres Jahreseinkommens in einen Anlagefonds mit moderaten Wachstumsraten investieren? [*you invest 10% of your annual income into an investment funds with moderate growth rates?*]

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[30]Most of these items are adapted from Weber et al. (2002).

(h) Sie fünf oder mehr als fünf alkoholische Getränke an einem einzigen Abend verzehren? [*you drink five or more alcoholic drinks on one evening?*]

(i) Sie einen wesentlichen Betrag bei der Steuererklärung falsch angeben? [*you cheat subtantially on your income tax?*]

(j) Sie sich mit Ihrem Vater in Bezug auf ein wichtiges Thema nicht einig sind? [*you disagree with your father on a major issue?*]

(k) Sie Ihr Tageseinkommen bei einem Pokerspiel mit hohen Einsätzen verwenden? [*you take a day's income to play poker with high stakes?*]

(l) Sie eine Affäre mit einem verheirateten Mann oder Frau haben? [*you have an affair with a married man or woman?*]

(m) Sie die Unterschrift einer anderen Person fälschen? [*you forge somebody's signature?*]

(n) Sie die Arbeit einer anderen Person als Ihre eigene darstellen? [*you present somebody else's work as your own?*]

(o) Sie in ein Land der Dritten Welt reisen, ohne vorher festgelegte und arrangierte Reiseroute und Übernachtungsmöglichkeiten? [*you go on vacation in a third-world country without a pre-arranged travel route and without booking accomodations ahead?*]

(p) Sie sich mit einem Freund/einer Freundin über etwas streiten, bei dem sich seine/ihre Meinung stark von Ihrer unterscheidet? [*you argue with a friend who has a very different opinion on an issue?*]

(q) Sie eine Skipiste nehmen, die Ihre Fähigkeiten übersteigt, oder gesperrt ist? [*you go down a ski run that is too hard or closed?*]

(r) Sie 5% Ihres Jahreseinkommens in eine sehr spekulative Aktie anlegen? [*you invest 5% of your annual income in a very speculative stock?*]

(s) Sie Ihren Chef um eine Gehaltserhöhung bitten? [*you ask your boss for a raise?*]

(t) Sie illegal Software kopieren? [*you illegally copy a piece of software?*]

(u) Sie Wildwasser-Rafting bei reißenden Wasserströmungen im Frühling betreiben? [*you go whitewater rafting at high water in the spring?*]

(v) Sie Ihr Tageseinkommen bei einer Sportwette einsetzen (zum Beispiel Fußball, Basketball, Baseball)? [*you invest your day's income on sports bets (e.g. Soccer, Basketball, Baseball)?*]

(w) Sie einem Freund oder einer Freundin erzählen, dass sein oder ihr Partner mit Ihnen geflirtet hat? [*you tell a friend that his/her partner flirted with you?*]

(x) Sie 5% Ihres Jahreseinkommens in einer konservativen Aktie anlegen? [*you invest 5% of your annual income in a conservative stock?*]

(y) Sie einen kleinen Gegenstand in einem Geschäft klauen (z.B. einen Stift oder einen Lippenstift)? [*you shoplift a small item (e.g., a pen or a lipstick?*]

(z) Sie provokative oder unkonventionelle Kleidung bei Gelegenheiten tragen? [*you wear unconventional or provocative clothes?*]

8. Wie wahrscheinlich ist es, dass... [*How likely is it, that...*][31]

   (a) Sie ungeschützten Sex haben? [*you engage in unprotected sex?*]

   (b) Sie von Ihrem Kabelanschluss, den Sie bezahlen, noch einen weiteren Anschluss abzweigen? [*you steal an additional TV cable connection?*]

   (c) Sie sich nicht anschnallen, wenn Sie im Auto vorne sitzen? [*you don't wear a seatbelt when in the front seat?*]

   (d) Sie 10% Ihres Jahreseinkommens in Staatsanleihen investieren? [*you invest 10% of your annual income in government bonds (treasury bills)?*]

   (e) Sie dann und wann eine gefährliche Sportart ausüben (z.B. Bergsteigen oder Sky Diving)? [*you periodically engage in a dangerous sport (e.g. mountain climbing or sky diving)?*]

   (f) Sie keinen Helm tragen wenn Sie ein Motorrad fahren? [*you ride a motorcycle without wearing a helmet?*]

   (g) Sie das Einkommen einer Woche im Casino verspielen? [*you gamble away a week's income at a casino.*]

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[31]Most of these items are adapted from Weber et al. (2002).

(h) Sie einen Job annehmen, der Ihnen Spaß macht, anstelle eines Jobs, der angesehener ist, Ihnen aber weniger Spaß macht? [*you take a job that you like instead of a job that is very reputable but that you like less?*]

(i) Sie einen unbeliebten Standpunkt, von dem Sie überzeugt sind, bei einer Gelegenheit vertreten? [*you openly express an opinion or viewpoint that is unpopular but of which you are convinced?*]

(j) Sie sich der Sonne aussetzen, ohne Sonnenschutz benutzt zu haben? [*you don't wear sunscreen when you expose yourself to the sun?*]

(k) Sie zumindest einmal im Leben Bungee Jumping ausprobieren? [*you try bungee jumping at least once in your life?*]

(l) Sie ein eigenes kleines Flugzeug fliegen, wenn Sie könnten? [*you fly a small plane if you could?*]

(m) Sie nachts alleine in einer eher unsicheren Gegend der Stadt herumlaufen? [*you walk alone through a rather unsafe part of the city at night?*]

(n) Sie regelmäßig Essen mit hohem Cholesterin-Gehalt essen? [*you regularly eat high-cholesterol food?*]

9. Wie sehr treffen folgenden Aussagen auf Sie zu? [*How well do the following statements describe you as a person?*]

   (a) Ich handle oft nach dem Motto: Vorsicht ist besser als Nachsicht. [*I often behave according to the motto: It is better to be safe than sorry.*]

   (b) Ich vermeide riskante Dinge. [*I avoid risky things.*]

   (c) Ich mag es, Risiken einzugehen. [*I like taking risks.*]

10. Stellen Sie sich vor, dass Sie in einem Preisausschreiben 100.000 Euro gewinnen. Unmittelbar nach Erhalt des Gewinns bekommen Sie ein Angebot für folgende Lotterie: Es gibt eine Chance, das Geld zu verdoppeln. Es gibt aber auch ein gleich hohes Risiko, die Hälfte des eingesetzten Geldes zu verlieren. Sie können mit Ihren 100.000 Euro ganz oder teilweise an der Lotterie teilnehmen. Wir würden von Ihnen gerne wissen: Welchen Teil des Gewinns aus dem Preisausschreiben würden Sie für die einerseits riskante, andererseits

gewinnversprechende Lotterie einsetzen? [*Imagine you win 100.000 Euro in a lottery. Immediately after receiving the money you get an offer to participate in the following lottery: There is a chance to double the money. But there is an equally high chance to lose half of the money invested in the lottery. You can participate in the lottery using the whole amount you won or only a part of it. We would like to know: How much of the money you won in the lottery would you invest in the risky yet profitable lottery?*]

11. Stellen Sie sich vor Sie haben in einem Preisausschreiben gewonnen. Sie können zwischen zwei Auszahlungsalternativen wählen. Entweder erhalten Sie ein Los oder eine sichere Auszahlung. Wenn Sie sich für das Los entscheiden erhalten Sie mit 50% Wahrscheinlichkeit 1.000 Euro und mit 50% Wahrscheinlichkeit nichts. Überlegen Sie bitte: Wie hoch müsste die sichere Auszahlung mindestens sein, damit Sie die sichere Auszahlung gegenüber dem Los bevorzugen? [*Imagine you won a prize in a lottery. You can choose between two payment options. Either you get a raffle ticket or you get a safe payment. If you decide to take the raffle ticket you receive 1,000 Euro with a probability of 50% and you receive nothing with a probability of 50%. Please consider: How much money would the safe payment need to be in order for you to prefer it over the raffle ticket?*]

12. Stellen Sie sich folgende Situation vor: Sie sind die einzige Person im Haushalt mit einem monatlichen Einkommen, und Sie haben einen guten Job, durch den Ihr aktuelles Familieneinkommen für den Rest Ihres Lebens gesichert ist. Nun wird Ihnen die Möglichkeit angeboten einen neuen und ebenso guten Job anzunehmen. Bei dem neuen Job ist die Bezahlung variabel, so dass sich mit einer Wahrscheinlichkeit von 50% Ihr Haushaltseinkommen verdoppeln wird, und mit gleicher Wahrscheinlichkeit Sie eine Einkommenseinbuße von 30% haben. Wären Sie bereit diesen neuen Job anzunehmen? [*Imagine the following situation: you are the only member of your household that has a monthly income, and you have a good job which would guarantee your family income for the rest of your life. Now you have the option to take a new and equally good job. The payment at this new job is variable, so that your house-*

106

hold income will double with a probability of 50% and will decrease by 30% with the same probability. Would you be willing to take the new job?]

Time Discounting

1. Staircase

2. List of 25 hypothetical choices between 100 Euro today or an equal or larger payment in 12 months. The larger payment starts at 100 Euro and increases up to 185 Euro.[32]

3. Sind Sie jemand, der im Allgemeinen bereit ist, heute auf etwas zu verzichten, um in der Zukunft davon zu profitieren, oder sind Sie dazu nicht bereit? [*Are you a person who is generally willing to give up something today in order to benefit from that in the future, or are you not willing to do so?*]

4. Sind Sie im Vergleich zu anderen im Allgemeinen bereit, heute auf etwas zu verzichten, um in der Zukunft davon zu profitieren, oder sind Sie im Vergleich zu anderen dazu nicht bereit? [*In comparison to others, are you a person who is generally willing to give up something today in order to benefit from that in the future or are you not willing to do so?*]

5. Schätzen andere Sie im Allgemeinen als jemanden ein, der bereit ist, heute auf etwas zu verzichten, um in der Zukunft davon zu profitieren, oder als jemanden, der dazu nicht bereit ist? [*Do other people generally assess you as a person who is willing to give up something today in order to benefit from that in the future or as someone who is not willing to do so?*]

6. Wie schätzen Sie Ihre Bereitschaft, auf etwas zu verzichten, um in Zukunft davon zu profitieren, in Bezug auf die folgenden Bereiche ein? [*How would you assess your willingness to give up something today in order to benefit from that in the future in the following contexts:*]

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[32]The larger payments are 100.0/103.0/106.1/109.2/112.4/115.6/118.8/122.1/125.4/128.8/ 132.3/135.7/139.2/142.8/146.4/150.1/153.8/157.5/161.3/165.1/169.0/172.9/176.9/180.9/185 Euro.

(a) Wenn es um finanzielle Entscheidungen geht. [*When it comes to financial decisions.*]

(b) Wenn es um wichtige Entscheidungen im Leben geht. [*When it comes to important decisions in life.*]

(c) Wenn es um die berufliche Karriere geht. [*When it comes to your professional career.*]

(d) Wenn es um größere Anschaffungen geht. [*When it comes to bigger purchases.*]

(e) Wenn es um eine größere Reise geht. [*When it comes to a longer journey/trip.*]

7. In welchen Maße treffen folgende Aussagen auf Sie zu? [*How well do the following statements describe you as a person?*]

(a) Ich stelle oft fest, dass ich Entscheidungen treffe, von denen ich weiß, dass ich sie künftig bereuen werde. [*I often realize that I make decisions knowing that I will regret them in the future.*]

(b) Ich denke oft über die Zukunft nach. [*I often think about the future.*]

(c) Mir fällt es oft schwer, auf ungesundes, aber leckeres Essen zu verzichten. [*I find it hard to resist unhealthy but delicious food.*]

(d) Ich bin jemand, dem es ziemlich egal ist, was morgen passiert, und der nur im Hier und Jetzt lebt. [*I am a person who does not care about tomorrow and who only lives for the moment.*]

(e) Ich bin eine Person, die häufig getroffene Entscheidungen bereut. [*I am a person who often regrets my own decisions.*]

(f) Ich bin eine Person, die oft vorschnell handelt. [*I am a person who often acts hastily/prematurely.*]

(g) Ich spare für meine Rente. [*I save for my retirement.*]

(h) Mir fällt es nicht allzu schwer, Versuchungen zu widerstehen. [*I do not find it hard to resist temptations.*]

(i) Ich gebe zu viel Geld aus. [*I spend too much money.*]

(j) Ich esse zu viel. [*I eat too much.*]

(k) Ich mache zu wenig Sport. [*I work out too little.*]

(l) Ich wünschte, ich hätte mehr Selbstdisziplin. [*I wish I was more self-disciplined.*]

(m) Ich bin meistens ausreichend auf Klausuren vorbereitet. [*Usually I am sufficiently prepared for exams.*]

(n) Ich handle oft, ohne alle Alternativen in Betracht gezogen zu haben. [*I often act without considering all alternatives.*]

(o) In Gesprächen neige ich dazu, Leute zu unterbrechen. [*I tend to interrupt people in conversations.*]

(p) Wenn ich mir ein Ziel gesetzt habe, erreiche ich dieses in der Regel auch. [*Once I set a goal for myself I usually achieve it.*]

(q) Mir fällt es schwer, schlechte Angewohnheiten abzulegen. [*I find it hard to give up bad habits.*]

(r) Ich bin immer pünktlich. [*I am always on time.*]

(s) Ich mag es überhaupt nicht, an der Ampel darauf zu warten, dass sie grün wird. [*I completely dislike waiting for a red light to turn green.*]

(t) Wenn ich auf etwas warten muss, empfinde ich das als unangenehm. [*I find waiting uncomfortable.*]

(u) Dinge, die Spaß machen, halten mich oft davon ab, andere wichtigere Dinge zu erledigen. [*Things that are fun often keep me from taking care of more important things.*]

(v) Ich neige dazu, Dinge auf später zu verschieben, auch wenn es besser wäre, diese sofort zu erledigen. [*I tend to postpone things even though it would be better to take care of them right away.*]

8. In welchem Maße treffen folgende Aussagen auf Sie zu? [*How well do the following statements apply to you:*]

(a) Ich kann mir gut vorstellen, wie mein nächster Job aussieht. [*I have a good idea of what my next job will look like.*]

(b) Mein derzeitiges Leben ist völlig anders, als ich es mir vor drei Jahren vorgestellt habe. [*My life at the moment is completely different from what I imagined it would be like three years ago.*]

(c) Ich habe ein klares Bild von dem, was ich im kommenden Jahr erwarten kann. [*I have a precise idea/clear picture of what I can expect in the upcoming year.*]

(d) Letztes Jahr ist ziemlich anders verlaufen, als ich vorher erwartet hatte. [*Last year went very differently from what I previously expected.*]

(e) Wenn ich eine wichtige Entscheidung treffen muss, bilde ich mir eine sehr genaue Vorstellung über die Konsequenzen dieser Entscheidung. [*When I have to make an important decision, I try to paint a clear picture/get a precise idea of the consequences of that decision.*]

(f) Wenn ich eine wichtige Entscheidung getroffen habe, stimmt das Ergebnis gewöhnlich mit dem überein, was ich mir vorgestellt hatte. [*When I make an important decision, the outcome usually corresponds with what I have imagined it to be.*]

9. Stellen Sie sich vor, Sie hätten eine 10-tägige Urlaubsreise im Wert von 2.000 Euro für 2 Personen zu einem spannenden Reiseziel gewonnen. Aufgrund von großer Nachfrage bei der Buchung werden Sie gefragt, ob Sie bereit wären, drei Jahre auf den Urlaub zu warten. [*Imagine you had won a 10-day trip for two people worth 2,000 Euro to an exciting destination. Due to high demand you are asked whether you would be willing to wait three years before making the trip.*]

(a) Im Gegenzug würde man Ihnen zusätzliche Reisetage schenken. Bitte überlegen Sie: Wie viele zusätzliche Reisetage müsste man Ihnen anbieten, damit Sie bereit wären, die Reise erst in drei Jahren zu unternehmen? [*In return for waiting you would be given an extension of the trip. Please consider: how many extra days would one have to offer you for you to be willing to postpone the trip for three years?*]

(b) Wenn es ebenfalls möglich wäre, die Urlaubsreise gegen einen Geldbetrag

zu tauschen: wie viel Geld müsste man Ihnen anbieten, so dass Sie bereit wären, auf die Urlaubsreise zu verzichten? [*If it was possible to exchange the trip for money: how much money would one need to offer you for you to be willing to forgo the trip?*]

10. Die folgenden Aussagen kennzeichnen verschiedene Einstellungen zum Leben und zur Zukunft. [*The following statements characterize different attitudes towards life and the future.*]

    (a) Ich bemühe mich, immer eine Geldreserve für unerwartete Ausgaben zu haben. [*I try hard to always have some extra money for unexpected expenditures.*]

    (b) Ich verzichte heute auf etwas, damit ich mir morgen mehr leisten kann. [*I give up something today so that I can afford more tomorrow.*]

    (c) Ich will lieber heute meinen Spaß haben, und denke dabei nicht an morgen. [*I would rather have some fun today and not think about tomorrow.*]

    (d) Meine monatlichen Ausgaben sind oft höher, als ich es mir leisten kann. [*My monthly expenses often exceed what I can afford.*]

    (e) Ich bin jemand, der sich an die eigenen guten Vorsätze oft nicht hält. [*I am a person who often does not keep my own good resolutions.*]

11. Wie viel Geld sparen Sie pro Monat? Versuchen Sie bitte, Ihren monatlichen Sparbetrag so genau wie möglich anzugeben. [*How much money do you save per month? Please try to specify the amount you save per month as exactly as possible.*]

12. Wenn Sie plötzlich in eine unvorhergesehene Situation geraten würden, und Sie innerhalb von zwei Wochen etwa 1.000 Euro bezahlen müssten, könnten Sie das schaffen? [*I you suddenly got into an unforeseen situation, and you had to pay about 1,000 Euro within two weeks: could you manage that?*]

Altruism

1. Sind Sie jemand, der im Allgemeinen bereit ist, mit anderen zu teilen, ohne dafür eine Gegenleistung zu erwarten, oder sind Sie dazu nicht bereit? [*Are*

you a person who is generally willing to share with others without expecting something in return, or are you not willing to do so?]

2. Sind Sie im Vergleich zu anderen jemand, der im Allgemeinen bereit ist, mit anderen zu teilen, ohne dafür eine Gegenleistung zu erwarten, oder sind Sie im Vergleich zu anderen dazu nicht bereit? [*In comparison to others, are you a person who is generally willing to share with others without expecting something in return, or are you not willing to do so (in comparison to others)?*]

3. Schätzen andere Sie als jemanden ein, der im Allgemeinen bereit ist, mit anderen zu teilen, ohne dafür einen Gegenleistung zu erwarten, oder als jemanden, der dazu nicht bereit ist? [*Do other people assess you as a person who is generally willing to share with others without expecting something in return or as a person who is not willing to do so?*]

4. Wie schätzen Sie Ihre Bereitschaft mit anderen zu teilen, ohne dafür einen Gegenleistung zu erwarten, in Bezug auf die folgenden Bereiche ein? [*How do you assess your willingness to share with others without expecting anything in return in the following contexts:*]

   (a) Gegenüber Menschen in Ihrer Stadt. [*With people in your hometown.*]

   (b) Gegenüber Menschen in Ihrem Freundeskreis. [*With people in your circle of friends.*]

   (c) Im beruflichen Umfeld. [*With people from your professional environment.*]

   (d) Gegenüber Fremden. [*With strangers.*]

   (e) Gegenüber Menschen in Ihrer Nachbarschaft. [*With people in your neighborhood.*]

   (f) Gegenüber Menschen in Notlagen. [*With people in distress or emergency situations.*]

   (g) Wenn es um gemeinnützige Zwecke geht. [*When it comes to charity.*]

5. Stellen Sie sich folgende Situation vor: Sie haben in einem Preisausschreiben 1.000 Euro gewonnen. Wie viel würden Sie in Ihrer momentanen Situation für

einen gemeinnützigen Zweck spenden? [*Imagine the following situation: you won 1,000 Euro in a lottery. Considering your current situation, how much would you donate to charity?*]

6. Wie sehr treffen folgende Aussagen auf Sie zu? [*How well do the following statements describe you as a person?*]

   (a) Im Beruf bin ich nur dann bereit etwas für einen Kollegen zu tun, wenn ich davon ausgehe, dass dieser dasselbe für mich tun würde. [*At work I am only willing to do something for a colleague if I expect that he would do the same for me.*]

   (b) Ich bin bereit, Zeit und Geld für einen mir sinnvoll erscheinenden gemeinnützigen Zweck aufzuwenden, auch wenn mir das nicht direkt selber nützt. [*I am willing to donate time and money to charity, even if I don't profit from that directly.*]

   (c) Ich bin bereit anderen zu helfen, auch wenn ich davon ausgehe, dass ich diesen Menschen nie wieder begegnen werde. [*I am willing to help others even if I expect that I will never meet them again.*]

   (d) Wenn ich Zeit und Geld für etwas aufwende, erwarte ich, in Zukunft selbst davon zu profitieren. [*When I spend time and money on something I expect to profit from that in the future.*]

   (e) Wenn ich Geld spende, erwarte ich, dass dies zur Kenntnis genommen wird, und ich Bestätigung erhalte. [*When I donate money I expect that this is recognized and acknowledged.*]

   (f) Ich kann nicht nachvollziehen, warum manche Menschen ihre Lebenszeit dafür verwenden, für einen Zweck zu kämpfen, der ihnen nicht unmittelbar nützt. [*I do not understand why some people spend their lifetime fighting for a cause which they do not benefit from directly.*]

   (g) Ich bin jemand, der sein letztes Hemd gibt, um anderen zu helfen. [*I am a person who would give their shirt off their back to help others.*]

   (h) Im Vergleich zu anderen bin ich eher selbstlos. [*In comparison to others I am a rather selfless person.*]

113

(i) Ich bin nur bereit Menschen zu helfen, wenn ich davon ausgehe, dass diese dasselbe für mich tun würden. [*I am only willing to help others if I expect that they would do the same for me.*]

(j) Andere Menschen betrachten mich als eine uneigennützige Person. [*Other people regard me as an unselfish person.*]

7. Geben Sie bitte möglichst genau an, wie viele Stunden Sie pro Monat aufwenden, um sich für gemeinnützige Zwecke einzusetzen, wie etwa Umweltschutz, Jugendarbeit, usw. [*Please specify as precisely as possible how many hours per month you volunteer for good causes, e.g. protecting the environment.*]

8. Wie viele Menschen wissen von Ihrem gemeinnützigen Engagement? [*How many people know that you commit time to charitable purposes?*]

Trust

1. Sind Sie im Allgemeinen jemand, der bereit ist, anderen Menschen zu vertrauen, oder sind Sie nicht bereit, anderen zu vertrauen? [*Generally speaking, are you a person who is willing to trust other people, or are you not willing to trust other people?*]

2. Sind Sie im Vergleich zu anderen im Allgemeinen bereit, anderen Menschen zu vertrauen, oder sind Sie im Vergleich zu anderen nicht bereit, anderen zu vertrauen? [*In comparison to others are you a person who is generally willing to trust other people, or a you not willing to trust others (in comparison to others)?*]

3. Schätzen andere Sie im Allgemeinen als jemanden ein, der bereit ist, anderen zu vertrauen, oder als jemanden, der nicht bereit ist, anderen zu vertrauen? [*Do other people assess you as a person who is generally willing to trust others or as a person who is not willing to trust others?*]

4. Wie schätzen Sie Ihre Bereitschaft, anderen zu vertrauen, in Bezug auf die folgenden Bereiche ein? [*How do you assess your willingness to trust others in the following contexts?*]

(a) Gegenüber Menschen in Ihrer Stadt. [*When it comes to people in your hometown.*]

(b) Gegenüber Menschen in Ihrem Freundeskreis. [*When it comes to people in your circle of friends.*]

(c) Im beruflichen Umfeld. [*When it comes to your professional environment.*]

(d) Gegenüber Fremden. [*When it comes to strangers.*]

(e) Gegenüber Menschen in Ihrer Nachbarschaft. [*When it comes to people in your neighborhood.*]

5. Sie sind im Urlaub in einem fremden Land, und eine Person, die Sie im Hotel treffen, die Sie aber nicht kennen, bittet Sie um einen Gefallen: Sie benötigt schnell Bargeld, um den Arztbesuch ihres Partners zu bezahlen, und versichert Ihnen, das Geld am kommenden Tag zurück zu geben. Wie viel wären Sie bereit, dieser Person zu leihen? [*You are on vacation in a foreign country. A person, whom you meet in your hotel but whom you do not know, asks you for a favor. He or she urgently needs cash in order to pay for their partner's doctor visit, and promises to pay you back the following day. How much money would you be willing to lend to that person?*]

6. Wie oft kommt es vor, dass... [*How often does it happen that...*]

(a) Sie einen Anhalter mitnehmen? [*you take a hitchhiker with you?*]

(b) Sie Ihre persönlichen Wertgegenstände an einem öffentlichen Ort unbeobachtet lassen? [*you leave your personal belongings unattended in a public place?*]

(c) Sie Ihre Wohnungstür nicht abschließen? [*do not lock your apartment door?*]

7. Wie sehr treffen folgende Aussagen auf Sie zu? [*How well do the following statements describe you as a person?*]

(a) Im Vergleich zu anderen Menschen fasse ich schnell Vertrauen in fremde Personen. [*In comparison to others I quickly (build up) trust with strangers.*]

(b) Andere Menschen halten mich für zu vertrauensselig. [*Other people regard me as too credulous and trusting.*]

(c) Mir fällt es nicht schwer, persönliche Dinge mit Menschen zu besprechen, die ich noch nicht lange kenne. [*I find it difficult to talk about personal issues with people I haven't known for a long time yet.*]

(d) Solange man mich nicht vom Gegenteil überzeugt, gehe ich stets davon aus, dass andere Menschen nur das Beste im Sinn haben. [*As long as I am not convinced otherwise, I assume that people have only the best intentions.*]

8. Was glauben Sie, wie sehr treffen die folgenden Aussagen im Allgemeinen zu? [*What do you think: how well do the following statements apply?*]

   (a) Im Allgemeinen kann man den Menschen vertrauen. [*In general, one can trust other people.*]

   (b) Heutzutage kann man sich auf niemanden mehr verlassen. [*Nowadays one cannot rely on anyone anymore.*]

   (c) Im Umgang mit Fremden ist es besser, vorsichtig zu sein, bevor man sich auf sie verlässt. [*When dealing with strangers it is better to be careful before one relies on them.*]

9. Glauben Sie... [*Do you think...*]

   (a) dass die meisten Menschen Sie ausnutzen würden, wenn sie die Gelegenheit hätten, oder... [*that most people would take advantage of you when they have the chance, or...*]

   (b) dass sich die meisten Menschen fair Ihnen gegenüber verhalten würden? [*that most people would be fair to you?*]

10. Würden Sie eher sagen... [*Would you rather say...*]

    (a) dass Menschen meistens versuchen hilfsbereit zu sein, oder... [*that most people try to be helpful/cooperative, or...*]

    (b) dass die Menschen meistens nur in ihrem eigenen Interesse handeln? [*that most people only act in their own best interest?*]

Positive Reciprocity and Negative Reciprocity

1. Sind Sie jemand, der sich im Allgemeinen besonders anstrengt einen Gefallen oder eine Hilfe zu erwidern, auch wenn das für Sie mit Kosten verbunden ist, oder sind Sie dazu nicht bereit? [*Are you a person who is generally willing to go out of their way to return a favor or a help even if it is costly, or are you not willing to do so?*]

2. Sind Sie im Vergleich zu anderen jemand, der sich besonders anstrengt einen Gefallen oder eine Hilfe zu erwidern, auch wenn das für ihn mit Kosten verbunden ist, oder sind Sie im Vergleich zu anderen dazu nicht bereit? [*In comparion to others, are you a person who goes out of their way to return a favor or a help even if it is costly, or are you not wiling to do so (in comparison to others)?*]

3. Schätzen andere Sie im Allgemeinen als jemanden ein, der sich besonders anstrengt einen Gefallen oder eine Hilfe zu erwidern, auch wenn das für ihn mit Kosten verbunden ist, oder als jemanden, der dazu nicht bereit ist? [*Do other people assess you as a person who goes out of their way to return a favor or a help even if it is costly or as a person who is not willing to do so?*]

4. Wie schätzen Sie Ihre Bereitschaft, einen Gefallen oder eine Hilfe zu erwidern, in Bezug auf die folgenden Bereiche ein? [*How do you assess your willingness to return a favor or a help in the following contexts?*]

   (a) Gegenüber Menschen in Ihrer Stadt. [*When it comes to people in your hometown.*]

   (b) Gegenüber Menschen in Ihrem Freundeskreis. [*When it comes to your circle of friends.*]

   (c) In Ihrem beruflichen Umfeld. [*When it comes to your professional environment.*]

   (d) Gegenüber Fremden. [*When it comes to strangers.*]

   (e) Gegenüber Menschen in Ihrer Nachbarschaft. [*When it comes to people in your neighborhood.*]

5. Sind Sie jemand, der in Allgemeinen bereit ist, unfaires Verhalten zu bestrafen, auch wenn das für Sie mit Kosten verbunden ist, oder sind Sie dazu nicht bereit? [*Are you a person who is generally willing to punish unfair behavior even if it is costly?*]

6. Sind Sie im Vergleich zu anderen jemand, der im Allgemeinen bereit ist, unfaires Verhalten zu bestrafen, auch wenn das für Sie mit Kosten verbunden ist, oder sind Sie im Vergleich mit anderen dazu nicht bereit? [*In comparison to others, are you a person who is generally willing to punish unfair behavior even if it is costly, or are you not willing to do so (in comparison to others)?*]

7. Schätzen andere Sie als jemanden ein, der im Allgemeinen bereit ist, unfaires Verhalten zu bestrafen, auch wenn das für ihn mit Kosten verbunden ist, oder als jemanden, der im Allgemeinen nicht dazu bereit ist? [*Do other people assess you as a person who is generally willing to punish unfair behavior even if it is costly, or as a person, who is generally not willing to do so?*]

8. Wie würden Sie Ihre Bereitschaft, unfaires Verhalten zu bestrafen, auch wenn das für Sie mit Kosten verbunden ist, in Bezug auf die folgenden Bereiche einschätzen? [*How would you assess your willingness to punish unfair behavior even if it is costly in the following contexts?*]

    (a) Gegenüber Menschen in Ihrer Stadt. [*When it comes to people in your hometown.*]

    (b) Gegenüber Menschen in Ihrem Freundeskreis. [*When it comes to your circle of friends.*]

    (c) Im beruflichen Umfeld. [*When it comes to your professional environment.*]

    (d) Gegenüber Fremden. [*When it comes to strangers.*]

    (e) Gegenüber Menschen in Ihrer Nachbarschaft. [*When it comes to people in your neighborhood.*]

9. Sind Sie jemand, der im Allgemeinen bereit ist, faires Verhalten zu belohnen und unfaires Verhalten zu bestrafen, auch wenn das für Sie mit Kosten verbunden ist, oder sind Sie dazu nicht bereit? [*Are you a person who is generally*

willing to reward fair behavior and punish unfair behavior even if it is costly, or are you not willing to do so?]

10. Sind Sie im Vergleich zu anderen jemand, der im Allgemeinen bereit ist, faires Verhalten zu belohnen und unfaires Verhalten zu bestrafen, auch wenn das für Sie mit Kosten verbunden ist, oder sind Sie im Vergleich zu anderen dazu nicht bereit? [*In comparison to others, are you a person who is generally willing to reward fair behavior and punish unfair behavior, even if it is costly, or are you not willing to do so (in comparison to others)?*]

11. Schätzen andere Sie als jemanden ein, der im Allgemeinen bereit ist, faires Verhalten zu belohnen und unfaires Verhalten zu bestrafen, auch wenn das für ihn mit Kosten verbunden ist, oder als jemanden, der dazu nicht bereit ist? [*Do other people assess you as a person who is generally willing to reward fair behavior and punish unfair behavior even if it is costly, or as a person who is not willing to do so?*]

12. Stellen Sie sich folgende Situation vor: Zusammen mit einer anderen Person, die Sie nicht kennen, haben Sie 100 Euro bei einem Preisausschreiben gewonnen. Die Regeln besagen nun folgendes: Einer von Ihnen soll einen Vorschlag darüber machen, wie die 100 Euro aufgeteilt werden. Der andere erfährt den Vorschlag, und hat dann zwei Möglichkeiten. Er kann die Aufteilung annehmen oder ablehnen. Wenn er den Vorschlag annimmt, wird das Geld so aufgeteilt, wie die andere Person es vorgeschlagen hat. Wird die Aufteilung abgelehnt, gehen beide leer aus.

Angenommen, die andere Person schlägt folgende Aufteilung vor: 50 Euro für Sie und 50 Euro für sich. Nehmen Sie diese Aufteilung an? Dann erhalten Sie 50 Euro und die andere Person 50 Euro. Wenn Sie ablehnen erhalten beide null Euro.

Insgesamt wurden 5 Fragen mit gleichem Wortlaut aber unterschiedlichen Aufteilungen beantwortet. die Aufteilungen sahen Beträge von 50, 40, 30, 20 und 10 Euro für den Entscheider vor.

[*Imagine the following situation: together with a person whom you do not know*

you won 100 Euro in a lottery. The rules stipulate the following: One of you has to make a proposal about how to divide the 100 Euro between you two. The other one gets to know the proposal and has to decide between two options. He or she can accept the proposal or reject it. If he or she accepts the proposal, the money is divided according to the proposal. If he or she rejects the proposal, both receive nothing.

Suppose that the other person offered the following split: 50 Euro for you and 50 Euro for himself/herself. Do you accept this split? If you do, you will receive 50 Euro and the other person will receive 50 Euro. If you reject, both of you receive 0 Euro.

Note that individuals answered a total of 5 questions that use the same wording but vary the amount that was offered by the other person. These amounts were 50, 40, 30, 20, and 10.]

(a) Angenommen, die andere Person macht einen Vorschlag über die Aufteilung. Sie wiederum sollen entscheiden, ob Sie den Vorschlag annehmen oder ablehnen. Welchen Betrag muss die andere Person Ihnen mindestens anbieten, damit Sie bereit sind, den Vorschlag über die Aufteilung anzunehmen? [*Assume that the other person makes the proposal about how to divide the money. You on the other hand have to decide whether to accept or reject the proposal. What is the minimum amount the other person has to offer you for you to be willing to accept the proposal?*]

(b) Angenommen, Sie sollen den Vorschlag über die Aufteilung machen. Welchen Betrag würden Sie der anderen Person anbieten? [*Assume that you have to make the proposal about how to divide the money. Which amount would you offer to the other person?*]

13. Stellen Sie sich folgende Situation vor: Sie sind beim Einkaufen unterwegs in einer fremden Stadt, und merken, dass Sie sich verlaufen haben. Sie fragen eine fremde Person nach dem Weg. Die Person bietet Ihnen an, Sie mit dem Auto zu Ihrem Ziel zu fahren. Die Fahrt dauert etwa 20 Minuten, und kostet

die fremde Person alles in allem etwa 20 Euro. Die fremde Person will aber kein Geld dafür. Sie haben 6 Flaschen Wein dabei. Die billigste Flasche kostet 5 Euro, die teuerste kostet 30 Euro. Sie entscheiden, der fremden Person eine Flasche Wein als Dankeschön zu geben. Welche Flasche schenken Sie? [*Imagine the following situation: you are shopping in an unfamiliar city and realize you lost your way. You ask a stranger for directions. The stranger offers to take you with their car to your destination. The ride takes about 20 minutes and costs the stranger about 20 Euro in total. The stranger does not want money for it. You carry six bottles of wine with you. The cheapest bottle costs 5 Euro, the most expensive one 30 Euro. You decide to give one of the bottles to the stranger as a thank-you gift. Which bottle do you give?* (Options: The bottle for 5/10/15/20/25/30 Euro)]

14. Angenommen, Sie sind im Ausland und müssen ärztlich behandelt werden. Es ist in diesem Land üblich, dass der Arzt nur gegen Barzahlung behandelt. Die Behandlung kostet umgerechnet 100 Euro. Sie haben aber kein Bargeld bei sich. Eine fremde Person im Wartezimmer beobachtet dies, und schenkt Ihnen umgerechnet 100 Euro. Sie nehmen das Geschenk gerne an. Sie fragen nach der Adresse der Person. Als Sie zwei Wochen später wieder zu Hause sind, überlegen Sie, dass Sie sich bei der Person bedanken und ein Geschenk nach Hause schicken möchten. Wie viel investieren Sie in ein Geschenk, das Sie dann verschicken? [*Assume that you are abroad and need medical treatment. In the country you are in it is common that the doctor treats patients only for cash. The treatment costs about 100 Euro. You don't have any cash with you. A stranger in the waiting room observes the situation and gives 100 Euro as a gift to you. You are happy to take the gift. You ask the stranger for their address. When returning home two weeks later you decide that you want to thank the stranger and send them a present. How much do you spend on a present that you then send to the stranger?*]

15. Überlegen Sie bitte, was Sie in folgender Situation tun würden: Sie sind mit einer fremden Person in einen Verkehrsunfall verwickelt. Sie trifft keinerlei Schuld, aber die andere Person behauptet, Sie seien über Rot gefahren, obwohl

die Person selbst über Rot gefahren ist. Obwohl die Behauptung der Person falsch ist, glaubt man ihr und Sie müssen eine Strafe in Höhe von 300 Euro bezahlen. Es hab einen Augenzeugen, der gesehen hat, was passiert ist. Wenn der Augenzeuge aussagt, müssen Sie die Strafe von 300 Euro nicht zahlen, sondern der fremde Fahrer. Zusätzlich muss der fremde Fahrer eine Strafe wegen Falschaussage in Höhe von 1.000 Euro bezahlen. Nehmen Sie an, dass ein Detektiv den Augenzeugen auf jeden Fall findet, und dass der Augenzeuge aussagt, wenn er gefunden wird. Wie viel Geld sind Sie höchstens bereit, für den Detektiv auszugeben? [*Please consider what you would do in the following situation: you and a stranger are involved in a car accident. You are not to blame for the accident, but the stranger claims that you ran a red light even though it was the stranger himself who ran the red light. Even though the stranger's claim is false, the claim is believed to be correct and you have to pay a fine of 300 Euro. There was an eyewitness who saw what really happened. If the eyewitness testifies, you don't have to pay the fine but the stranger has to instead. In addition the stranger will then have to pay a fine for making a false testimony. Assume that there is detective who will definitely find the eyewitness, and that the eyewitness will testify if the detective finds him. What is the maximum amount of money that you are willing to spend on hiring the detective?*]

16. Überlegen Sie bitte, was Sie in folgender Situation tun würden: Sie und eine andere Person, die Sie nicht kennen, treffen beide eine Entscheidung über die Verwendung von Geld und erzielen zusammen ein Ergebnis. Die Regeln gehen so: Jeder Teilnehmer erhält ein Konto mit 20 Euro. Am Anfang haben Sie und die andere Person also jeweils 20 Euro auf dem Konto. Zuerst entscheidet die andere Person. Sie kann Ihnen Geld auf Ihr Konto überweisen. Sie kann Ihnen einen beliebigen Eurobetrag überweisen, also 0 Euro, 1 Euro, 2 Euro, usw. bis 20 Euro. Jeder Euro, den die andere Person an Sie überweist, wird von den Leitern der Studie verdreifacht und Ihrem Konto gutgeschrieben. Nach dem ersten Schritt sind also auf dem Konto der anderen Person 20 Euro minus der Überweisung an Sie. Auf Ihrem Konto sind 20 Euro plus dem Dreifachen

der Überweisung an Sie. Jetzt entscheiden Sie: Sie haben die Möglichkeit, der anderen Person Geld zurück zu überweisen. Sie können jeden beliebigen Eurobetrag zurück überweisen, also 0 Euro, 1 Euro, 2 Euro, usw. bis 80 Euro, je nachdem, wie viel Geld Sie insgesamt auf Ihrem Konto gutgeschrieben haben, nachdem Sie die Überweisung der anderen Person erhalten haben. Damit ist die Studie beendet. Die endgültigen Kontostände sind erreicht. Auf dem Konto der anderen Person sind jetzt 20 Euro minus der Überweisung an Sie plus Ihrer Rücküberweisung. Auf Ihrem Konto sind jetzt 20 Euro plus das Dreifache der Überweisung der anderen Person an Sie minus Ihrer Rücküberweisung. Wir möchten nun von Ihnen wissen, welche Rücküberweisung Sie wählen würden, wenn die andere Person Ihnen einen bestimmten Betrag überweist. [*Please consider what you would do in the following situation: you and a person whom you do not know both have to make a decision about the employment of money and together you achieve an outcome. The rules are the following: both of you get an account with 20 Euro. Thus, at first, both you and the other person have 20 Euro each on their account. The other person has to decide first. She can transfer money to your account. She can transfer any round amount, i.e. 0 Euro, 1 Euro, 2 Euro, etc. up to 20 Euro. Each Euro that the other person decides to transfer to you is tripled by the people conducting the study and then credited to your account. Thus, after the first step the other person has 20 Euro minus the amount she transferred to you on her account. You on the other hand have 20 Euro plus three times the amount that was transferred to you on your account. Now you have to make a decision. You can transfer money back to the other person. You can transfer any amount to the other person, i.e. 0 Euro, 1 Euro, 2 Euro, etc. up to 80 Euro depending on how much money is on your account after receiving the transfer from the other person. After this decision the study is over, and the amount on the two accounts are final. The other person has 20 Euro minus the amount she transferred to you plus the amount you transferred back on her account. You have 20 Euro plus three times the amount the other person transferred to you minus the amount you transferred to the other person on your account. For a given transfer of the other person we would now like to know how much money you would decide to transfer back.*]

123

(a) Angenommen, die andere Person überweist Ihnen 5 Euro. Sie haben dann nach dem ersten Schritt 20+3*5 Euro = 35 Euro, die andere Person hat 20-5 Euro = 15 Euro. Wie hoch ist Ihre Rücküberweisung? [*Assume that the other person transfers 5 Euro to your account. After the first step you have 20+3*5 Euro = 35 Euro, the other person has 20-5 Euro = 15 Euro. Which amount do you transfer back?*]

(b) Angenommen, die andere Person überweist Ihnen 10 Euro. Sie haben dann nach dem ersten Schritt 20+3*10 Euro = 50 Euro, die andere Person hat 20-10 Euro = 10 Euro. Wie hoch ist Ihre Rücküberweisung? [*Assume that the other person transfers 10 Euro to your account. After the first step you have 20+3*10 Euro = 50 Euro, the other person has 20-10 Euro = 10 Euro. Which amount do you transfer back?*]

(c) Angenommen, die andere Person überweist Ihnen 15 Euro. Sie haben dann nach dem ersten Schritt 20+3*15 Euro = 65 Euro, die andere Person hat 20-15 Euro = 5 Euro. Wie hoch ist Ihre Rücküberweisung? [*Assume that the other person transfers 15 Euro to your account. After the first step you have 20+3*15 Euro = 65 Euro, the other person has 20-15 Euro = 5 Euro. Which amount do you transfer back?*]

(d) Angenommen, die andere Person überweist Ihnen 20 Euro. Sie haben dann nach dem ersten Schritt 20+3*20 Euro = 80 Euro, die andere Person hat 20-20 Euro = 0 Euro. Wie hoch ist Ihre Rücküberweisung? [*Assume that the other person transfers 20 Euro to your account. After the first step you have 20+3*20 Euro = 80 Euro, the other person has 20-20 Euro = 0 Euro. Which amount do you transfer back?*]

(e) Zum Schluss noch eine andere Frage. Angenommen Sie wären in der Rolle der anderen Person, d.h. Sie müssten entscheiden, welchen Betrag Sie überweisen würden. Welchen Betrag würden Sie überweisen? [*Finally, a different question: assume you were in the position of the other person and had to decide which amount to transfer. Which amount would you transfer?*]

17. In welchem Maße treffen folgende Aussagen auf Sie zu? [*How well do the*

following statements describe you as a person?]

(a) Wenn mir jemand einen Gefallen tut, bin ich bereit, diesen zu erwidern. [*When someone does me a favor I am willing to return it.*]

(b) Wenn mir schweres Unrecht zuteil wird, werde ich mich bei nächster Gelegenheit um jeden Preis dafür rächen. [*If I suffer a serious wrong I will take revenge at the first opportunity.*]

(c) Wenn mich jemand in eine schwierige Lage bringt, werde ich das Gleiche mit ihm machen. [*When someone puts me into a difficult situation I will do the same to them.*]

(d) Ich strenge mich besonders an, um jemandem zu helfen, der mir früher schon einmal geholfen hat. [*I go out of my way to help someone who has helped me before.*]

(e) Wenn mich jemand beleidigt, werde ich mich auch ihm gegenüber beleidigend verhalten. [*If someone insults me I will also behave in an insulting way towards him.*]

(f) Ich bin bereit Kosten auf mich zu nehmen, um jemandem zu helfen, der mir früher schon mal geholfen hat. [*I am willing to incur costs to help someone who has helped me before.*]

(g) Wenn mir jemand mit Absicht Schaden zufügt, werde ich versuchen, es dieser Person mit gleicher Münze heimzuzahlen. [*If someone harms me on purpose I will try to give that person a taste of his own medicine.*]

(h) Ich bin jemand, der sich nicht für dumm verkaufen lässt. [*I am not a person who is taken for a fool.*]

(i) Ich mag das Gefühl nicht, jemandem etwas zu schulden. [*I do not like the feeling of owing something to someone.*]

(j) Wenn sich jemand im Sport unfair mir gegenüber verhält, werde ich mich bei nächster Gelegenheit auch unfair verhalten. [*If someone behaves unfairly towards me in sports, I will also behave unfairly towards them.*]

(k) Ich bin jemand, der sich nicht auf der Nase herumtanzen lässt. [*I am not a person who lets others push me around.*]

(l) Wenn mir ein Kollege am Arbeitsplatz einen Gefallen tut, achte ich besonders darauf, diesen bei nächster Gelegenheit zu erwidern, auch wenn ich dafür kostbare Zeit aufwenden muss. [*If a colleague does me a favor at work, I make sure to return the favor at the next occasion, even if I have to invest precious time to do so.*]

(m) Wenn mich jemand schlecht behandelt, lasse ich das nicht einfach so stehen. [*When someone treats me in a bad way, I don't just let it go.*]

(n) Ich kann es überhaupt nicht leiden, der Dumme zu sein. [*I absolutely dislike being the fool.*]

(o) Mir ist es wichtig, von anderen respektiert zu werden. [*It is important to me to be respected by others.*]

(p) Man muss manchmal eine gewisse Härte an den Tag legen, sonst wird man immer über den Tisch gezogen. [*You sometimes have to play tough in order not to be taken advantage of.*]

18. Stellen Sie sich folgende Situation vor: Sie sind beim Einkaufen unterwegs in einer fremden Stadt, und merken, dass Sie sich verlaufen haben. Sie fragen eine fremde Person nach dem Weg. Die Person bietet Ihnen an, Sie mit dem Auto zu Ihrem Ziel zu fahren. Die Fahrt dauert etwa 20 Minuten, und kostet die fremde Person alles in allem etwa 20 Euro. Die fremde Person will aber kein Geld dafür. Sie haben 6 Flaschen Wein dabei. Eine Flasche Wein kostet 5 Euro. Sie entscheiden, der fremden Person eine Flasche Wein als Dankeschön zu geben. Wie viele Flaschen Wein schenken Sie der fremden Person? [*Imagine the following situation: you are shopping in an unfamiliar city and realize you lost your way. You ask a stranger for directions. The stranger offers to take you with their car to your destination. The ride takes about 20 minutes and costs the stranger about 20 Euro in total. The stranger does not want money for it. You have six bottles of wine with you. One bottle costs 5 Euro. You decide to give a bottle to the stranger as a thank-you gift. How many bottles do you give?* (Options: One/two/three/four/five/six bottles.)]

19. Stellen Sie sich folgendes Szenario vor: In einer Gemeinde mit hoher Arbeit-

slosigkeit gibt es ein Unternehmen, das trotz Rezession noch Gewinne macht. Der Vorstand des Unternehmens kündigt an, ab dem kommenden Quartal alle Löhne und Gehälter um 5% zu kürzen. Wie fair finden Sie diese Entscheidung? [*Imagine the following scenario: A business in a city with a high level of unemployment makes profits despite a recession. The enterprise's chairman announces a decision to cut all wages and salaries by 5%. How fair do you think is this decision?*]

20. Stellen Sie sich folgendes Szenario vor: Es ist das Wochenende eines alljährlichen Volksfestes, das wie immer gut besucht ist. Die Temperaturen sind dieses Jahr unerwartet hoch, so dass die Besucher des Festes viel mehr an Getränken konsumieren wollen, als in den Vorjahren. Daraufhin erhöhen die Besitzer der Festzelte die Preise der Getränke. Wie fair finden Sie diese Entscheidung? [*Imagine the following scenario: It is the weekend of the annual fair, which is well-attended as usual. It is warmer than expected, so that the people at the fair drink much more than in the preceding years. As a result, the hosts decide to raise the prices of the drinks. How fair do you think is this decision?*]

21. Stellen Sie sich folgendes Szenario vor: Für ein Seminar an der Universität sind Sie und zwei andere Studenten aufgefordert, in einer Dreiergruppe eine Präsentation vorzubereiten. In Ihrer Gruppe haben Sie und ein anderer Student ihren Teil der Präsentation bereits fertig gestellt. Am Abend vor der Präsentation hat der dritte Student seinen Teil der Präsentation noch immer nicht bearbeitet, so dass Sie und der andere Student, der seinen Teil schon fertiggestellt hat, beschließen, die Nacht durchzuarbeiten, um die Präsentation zu vervollständigen. Am nächsten Tag stellt der dritte Student den Teil der Präsentation im Seminar als seine eigene Arbeit dar. Drücken Sie die Intensität Ihrer Empfindung gegenüber diesem Studenten aus. [*Imagine the following scenario: you and two other students have to prepare a presentation as a team for a seminar at the university. You and one of the other two students have already prepared your respective parts of the presentation. On the evening before the presentation you realize that the third student still has not started to work on their part of the presentation. Consequently, you and the other student decide*

to work all night in order to prepare the third part of the presentation. On the day of the presentation, the third student presents your work as his work. Please express the intensity of your feelings towards that student.]

(a) Wie verärgert sind Sie auf einer Skala von 0 bis 10? [*How upset are you on a scale from 0 to 10?*]

(b) Wie wütend sind Sie auf einer Skala von 0 bis 10? [*How angry are you on a scale from 0 to 10?*]

22. Stellen Sie sich folgendes Szenario vor: In einem Unternehmen, in dem Sie arbeiten, steht der Jahresabschluss an, so dass alle Mitarbeiter länger im Büro sein müssen, um die Arbeit, die ihr Vorgesetzter von ihnen erwartet, schaffen zu können. Einer der Mitarbeiter verlässt das Büro dennoch täglich pünktlich zur gewohnten Zeit, so dass Sie und Ihre Kollegen seinen Teil der Arbeit zusätzlich übernehmen müssen. Drücken Sie die Intensität Ihrer Empfindung gegenüber diesem Mitarbeiter aus. [*Imagine the following scenario: The preparation of the annual accounts is coming up for the business you are employed by. Hence, all employees have to work overtime in order to manage and finish the workload that the boss expects from them. Nevertheless, one of your co-workers leaves the office every day at the usual time, so that you and the other colleagues additionally have to take on his workload as well. Please express the intensity of your feelings towards that co-worker.]*

(a) Wie verärgert sind Sie auf einer Skala von 0 bis 10? [*How upset are you on a scale from 0 to 10?*]

(b) Wie wütend sind Sie auf einer Skala von 0 bis 10? [*How angry are you on a scale from 0 to 10?*]

3.7.3 The Preference Module

1. Risk Taking

(a) List of 31 hypothetical choices between a lottery (300 Euro with a 50-percent chance and 0 Euro with a 50-percent chance) and varying safe

options (starting at 0 Euro and increasing to 300 Euro in increments of 10 Euro)

(b) How do you see yourself: are you a person who is generally willing to take risks, or do you try to avoid taking risks? *Please use a scale from 0 to 10, where a 0 means you are "completely unwilling to take risks" and a 10 means you are "very willing to take risks". You can also use the values in-between to indicate where you fall on the scale.*

2. Time Discounting

(a) List of 25 hypothetical choices between an early payment "today" (100 Euro) and a varying delayed payment "in 12 months" (100.0/103.0/106.1/ 109.2/112.4/115.6/118.8/122.1/125.4/128.8/132.3/135.7/139.2/ 142.8/ 146.4/150.1/153.8/157.5 161.3/165.1/169.0/172.9/176.9/180.9/185 Euro).

(b) In comparison to others, are you a person who is generally willing to give up something today in order to benefit from that in the future or are you not willing to do so? *Please use a scale from 0 to 10, where a 0 means you are "completely unwilling to give up something today" and a 10 means you are "very willing to give up something today". You can also use the values in-between to indicate where you fall on the scale.*

3. Trust

(a) Please consider the following situation: You and another person, whom you do not know, both participate in a study where you can decide on how to assign a certain amount of money and thereby determine the outcome. The rules are as follows. Both participants get an account with 20 Euros. At the beginning, both participants thus own 20 Euros. The other person decides first. She can transfer money to your account. She can transfer any amount: 0, 1, 2 Euro, etc. up to 20 Euro. Each Euro that she transfers to you is tripled by the conductors of the study and booked to your account. After this first stage the other person therefore has 20 Euro minus the amount she transferred to you in her account. You have 20 Euro plus the tripled amount of the transfer of the other person on your

account. Now you get to decide: you have the opportunity to transfer money back to the other person. You can transfer any amount up to 80 Euro, depending on how much you have in your account. This will be the end of the study and the account balances will be final. The other person has in her account 20 Euros minus the amount she transferred to you plus the amount you transferred back. You have 20 Euro plus the tripled amount of what the other person transferred to you minus the amount you transferred back to her. We would like to know how much you would choose to transfer back to the other person, for a given transfer of her to you.

Suppose you were assigned the role of the other person. Which amount would you choose to transfer?

(b) How well does the following statement describe you as a person? As long as I am not convinced otherwise, I assume that people have only the best intentions. *Please use a scale from 0 to 10, where 0 means "does not describe me at all" and a 10 means "describes me perfectly". You can also use the values in-between to indicate where you fall on the scale.*

4. Altruism

(a) Imagine the following situation: you won 1,000 Euro in a lottery. Considering your current situation, how much would you donate to charity? *(Values between 0 and 1000 are allowed)*

(b) How do you assess your willingness to share with others without expecting anything in return when it comes to charity? *Please use a scale from 0 to 10, where 0 means you are "completely unwilling to share" and a 10 means you are "very willing to share". You can also use the values in-between to indicate where you fall on the scale.*

5. Positive Reciprocity

(a) Please consider the following situation: You and another person, whom you do not know, both participate in a study where you can decide on how to assign a certain amount of money and thereby determine the outcome.

The rules are as follows. Both participants get an account with 20 Euros. At the beginning, both participants thus own 20 Euros. The other person decides first. She can transfer money to your account. She can transfer any amount: 0, 1, 2 Euro, etc. up to 20 Euro. Each Euro that she transfers to you is tripled by the conductors of the study and booked to your account. After this first stage the other person therefore has 20 Euro minus the amount she transferred to you in her account. You have 20 Euro plus the tripled amount of the transfer of the other person on your account. Now you get to decide: you have the opportunity to transfer money back to the other person. You can transfer any amount up to 80 Euro, depending on how much you have in your account. This will be the end of the study and the account balances will be final. The other person has in her account 20 Euros minus the amount she transferred to you plus the amount you transferred back. You have 20 Euro plus the tripled amount of what the other person transferred to you minus the amount you transferred back to her. We would like to know how much you would choose to transfer back to the other person, for a given transfer of her to you.

Suppose the other person transfers 5/10/15/20 Euro to your account. After the first stage you then own 20+3*5/10/15/20=35/50/65/80 Euro, the other person owns 20-5/10/15/20=15/10/5/0 Euro. What amount do you choose to transfer back?

(b) Imagine the following situation: you are shopping in an unfamiliar city and realize you lost your way. You ask a stranger for directions. The stranger offers to take you with their car to your destination. The ride takes about 20 minutes and costs the stranger about 20 Euro in total. The stranger does not want money for it. You carry six bottles of wine with you. The cheapest bottle costs 5 Euro, the most expensive one 30 Euro. You decide to give one of the bottles to the stranger as a thank-you gift. Which bottle do you give?

Respondents can choose from the following options: The bottle for 5, 10,

15, 20, 25, or 30 Euro)

6. Negative Reciprocity

   (a) Imagine the following situation: together with a person whom you do not know you won 100 Euro in a lottery. The rules stipulate the following: One of you has to make a proposal about how to divide the 100 Euro between you two. The other one gets to know the proposal and has to decide between two options. He or she can accept the proposal or reject it. If he or she accepts the proposal, the money is divided according to the proposal. If he or she rejects the proposal, both receive nothing.

   Suppose that the other person offered the following split: 50 Euro for you and 50 Euro for himself/herself. Do you accept this split? If you do, you will receive 50 Euro and the other person will receive 50 Euro. If you reject, both of you receive 0 Euro.

   Note that individuals answered a total of 5 questions that use the same wording but vary the amount that was offered by the other person. These amounts were 50, 40, 30, 20, and 10.

   (b) How do you see yourself: Are you a person who is generally willing to punish unfair behavior even if this is costly? *Please use a scale from 0 to 10, where 0 means you are "not willing at all to incur costs to punish unfair behavior" and a 10 means you are "very willing to incur costs to punish unfair behavior". You can also use the values in-between to indicate where you fall on the scale.*

Table 3.5: The Preference Module

	(1) Risk	(2) Time	(3) Trust	(4) Altruism	(5) Pos. Reciprocity	(6) Neg. Reciprocity
risk_quant	0.276*** (0.051)					
risk_qual	0.203*** (0.051)					
time_quant		0.485*** (0.048)				
time_qual		-0.171*** (0.048)				
trust_quant			0.629*** (0.039)			
trust_qual			0.133*** (0.038)			
altr_quant				0.185*** (0.053)		
altr_qual				0.321*** (0.050)		
posrecip_quant1					0.486*** (0.046)	
posrecip_quant2					0.164*** (0.047)	
negrecip_quant						0.328*** (0.049)
negrecip_qual						0.148*** (0.050)
Constant	-0.00125 (0.047)	-0.00821 (0.042)	0.00425 (0.038)	0.0118 (0.046)	0.0289 (0.043)	0.0112 (0.049)
Observations	382	382	382	382	360	360
Adjusted R^2	0.162	0.340	0.452	0.175	0.329	0.134
F	37.81	99.15	158.4	41.41	89.15	28.83

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses. OLS Regressions of the standardized behavioral measure (obtained from incentivized experiments) on the two standardized items that were selected for the preference module.

Table 3.6: Test-Retest Correlation

	Risk (1)	Time (2)	Trust (3)	Altruism (4)	Pos. Reciprocity (5)	Neg. Reciprocity (6)
Risk (Retest)	0.508*** (0.107)					
Time (Retest)		0.780*** (0.084)				
Trust (Retest)			0.730*** (0.092)			
Altruism (Retest)				0.586*** (0.106)		
Pos. Reciprocity (Retest)					0.608*** (0.107)	
Neg. Reciprocity (Retest)						0.636*** (0.110)
Constant	5.800*** (1.107)	2.229* (1.306)	83.25*** (24.841)	50.10** (24.570)	148.3*** (24.723)	1.29e-08 (0.087)
Observations	44	44	44	44	44	44
Adjusted R^2	0.331	0.664	0.589	0.406	0.420	0.431
F	22.31	85.85	62.64	30.45	32.15	33.62

Standard errors in parentheses OLS Regressions: Test-Retest Correlations. The dependent variables are the preference measures obtained from behavior in the experiments in the first week. These are regressed on the preference measures obtained from behavior in the respective experiments in the second week. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.7.4 Regression Tables

3.7.5 Highest Correlations between Experimental and Survey Measures

Risk Taking

Table 3.7: Highest Correlations: Risk Taking

Item	Item Description	Correlation
2	List of hypothetical choices: lottery vs. varying safe options	0.4095
3	General willingness to take risks	0.3524
1	Staircase measure: 5 interdependent choices between a lottery and varying safe options	0.3356
7 (k)	Likelihood of spending a day's income on poker with high stakes	0.3115
11	Estimation of certainty equivalent (safe amount necessary to give up lottery)	0.3070
7 (c)	Likelihood of spending a day's income on betting at horse races	0.3043
6 (a)	Willingness to take risks: financial investments	0.2937

The detailed wording of each item can be found in Appendix 3.7.2. The first column displays the item number as given in Appendix 3.7.2. The third column displays the Spearman correlation coefficient between the survey item and the experimental measure. All correlations are significant at the 1-percent level.

Time Discounting

Table 3.8: Highest Correlations: Time Discounting

Item	Item Description	Correlation
2	List of hypothetical choices: early vs. delayed amounts of money	0.5826
1	Staircase measure: 5 interdependent choices between an early and delayed amount of money	0.5547
3	General willingness to abstain from something today	-0.4091
4	General willingness to abstain from something today: in comparison to others	-0.4039
6 (a)	General willingness to abstain from something today: financial decisions	-0.3802
5	General willingness to abstain from something today: how others assess you	-0.2712
9 (a)	Hypothetical scenario of winning a vacation in a lottery: how many extra days of vacation would you ask for to be willing to delay the vacation	0.2606
10 (b)	I abstain from something today so that I can afford more tomorrow.	-0.2454

The detailed wording of each item can be found in Appendix 3.7.2. The first column displays the item number as given in Appendix 3.7.2. The third column displays the Spearman correlation coefficient between the survey item and the experimental measure. All correlations are significant at the 1-percent level.

Trust

Table 3.9: Highest Correlations: Trust

Item	Item Description	Correlation
16 (e)	First mover decision in a hypothetical trust game	0.6201
4 (d)	General willingness to trust: strangers	0.3477
5	Hypothethical scenario: willingness to lend money to a stranger	0.2848
7 (d)	As long as I am not convinced otherwise I assume that people have the best intentions.	0.2829
4 (a)	General willingness to trust: towards people in your city.	0.2778
8	In general one can trust other people.	0.2756
1	General willingness to trust	0.2672
2	General willingness to trust: in comparison to others.	0.2592

The detailed wording of each item - except for item 16 (e) - can be found in Appendix 3.7.2. The first column displays the item number as given in Appendix 3.7.2. Item 16 (e) can be found in Appendix 3.7.2. The third column displays the Spearman correlation coefficient between the survey item and the experimental measure. All correlations are significant at the 1-percent level.

Altruism

Table 3.10: Highest Correlations: Altruism

Item	Item Description	Correlation
5	Hypothetical donation	0.3913
4 (g)	General willingness to share: charitable purposes	0.3845
6 (b)	I am willing to spend time and money on a charitable purpose, even if I don't profit from that directly.	0.3171
6 (c)	I am willing to help others even if I presume that I will never meet them again.	0.2658
6 (f)	I do not comprehend why some people spend their lifetime fighting for a cause which they do not benefit from directly.	-0.2612
2	General willingness to share: in comparison to others.	0.2268
4 (f)	General willingness to share: towards people in need	0.2186

The detailed wording of each item can be found in Appendix 3.7.2. The first column displays the item number as given in Appendix 3.7.2. The third column displays the Spearman correlation coefficient between the survey item and the experimental measure. All correlations are significant at the 1-percent level.

Positive Reciprocity

Table 3.11: Highest Correlations: Positive Reciprocity

Item	Item Description	Correlation
16 (a)-(d)	Second mover decision in a hypothetical trust game.	0.5560
13	Hypothetical scenario: willingness to pay for a thank-you-gift	0.3530
17 (a)	When someone does me a favor, I am willing to return it.	0.2970
17 (d)	I go out of my way to help someone who has helped me before.	0.2175
18 (a)	Hypothetical scenario: willingness to pay for a thank-you-gift	0.2137
4 (d)	General willingness to return a favor: towards strangers.	0.2082
14	Hypothetical scenario: willingness to pay for a thank-you-gift.	0.2032

The detailed wording of each item can be found in Appendix 3.7.2. The first column displays the item number as given in Appendix 3.7.2. The third column displays the Spearman correlation coefficient between the survey item and the experimental measure. All correlations are significant at the 1-percent level.

Negative Reciprocity

Table 3.12: Highest Correlations: Negative Reciprocity

Item	Item Description	Correlation
12 (a)	Minimum acceptable offer in a hypothetical ultimatum game.	0.3416
5	General willingness to punish unfair behavior	0.1609
17 (p)	You sometimes have to play tough in order not to be taken advantage of.	0.1487
8 (b)	General willingness to punish: people among your circle of friends.	0.1436
6	General willingness to punish: in comparison to others.	0.1422
7	General willingness to punish: how others assess you	0.1349
17 (j)	If someone behaves unfairly towards me in sports, I will also behave unfairly towards them.	0.1343

The detailed wording of each item can be found in Appendix 3.7.2. The first column displays the item number as given in Appendix 3.7.2. The third column displays the Spearman correlation coefficient between the survey item and the experimental measure. All correlations are significant at the 1-percent level.

3.7.6 Staircase Risk

The staircase procedure for eliciting risk preferences consists of a sequence of lottery choices. Everybody starts with the same first question. The choice for the lottery or the safe payment option then determines the next question in the sequence. This procedure is repeated four times. Subjects were instructed as follows:

Please imagine the following situation: You can choose between a sure payment and a lottery. The lottery gives you a 50 percent chance of receiving 300 Euro. With an equally high chance you receive nothing. Now imagine you had to choose between the lottery and a sure payment. We will present to you five different situations. The lottery is the same in all situations. The sure payment is different in every situation.

1. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 160 Euro as a sure payment?

    (a) lottery → *go to question 17*

    (b) sure payment → *go to question 2*

2. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 80 Euro as a sure payment?

(a) lottery → *go to question 10*

(b) sure payment → *go to question 3*

3. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 40 Euro as a sure payment?

   (a) lottery → *go to question 4*

   (b) sure payment → *go to question 7*

4. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 60 Euro as a sure payment?

   (a) lottery → *go to question 5*

   (b) sure payment → *go to question 6*

5. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 70 Euro as a sure payment?

   (a) lottery

   (b) sure payment

6. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 50 Euro as a sure payment?

   (a) lottery

   (b) sure payment

7. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 20 Euro as a sure payment?

   (a) lottery → *go to question 8*

(b) sure payment → *go to question 9*

8. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 30 Euro as a sure payment?

   (a) lottery

   (b) sure payment

9. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 10 Euro as a sure payment?

   (a) lottery

   (b) sure payment

10. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 120 Euro as a sure payment?

    (a) lottery → *go to question 14*

    (b) sure payment → *go to question 11*

11. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 100 Euro as a sure payment?

    (a) lottery → *go to question 13*

    (b) sure payment → *go to question 12*

12. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 90 Euro as a sure payment?

    (a) lottery

    (b) sure payment

13. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 110 Euro as a sure payment?

    (a) lottery

    (b) sure payment

14. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 140 Euro as a sure payment?

    (a) lottery → *go to question 15*

    (b) sure payment → *go to question 16*

15. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 150 Euro as a sure payment?

    (a) lottery

    (b) sure payment

16. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 130 Euro as a sure payment?

    (a) lottery

    (b) sure payment

17. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 240 Euro as a sure payment?

    (a) lottery → *go to question 25*

    (b) sure payment → *go to question 18*

18. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 200 Euro as a sure payment?

    (a) lottery → *go to question 22*

    (b) sure payment → *go to question 19*

19. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 180 Euro as a sure payment?

    (a) lottery → *go to question 20*

    (b) sure payment → *go to question 21*

20. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 190 Euro as a sure payment?

    (a) lottery

    (b) sure payment

21. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 170 Euro as a sure payment?

    (a) lottery

    (b) sure payment

22. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 220 Euro as a sure payment?

    (a) lottery → *go to question 23*

    (b) sure payment → *go to question 24*

23. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 230 Euro as a sure payment?

   (a) lottery

   (b) sure payment

24. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 210 Euro as a sure payment?

   (a) lottery

   (b) sure payment

25. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 280 Euro as a sure payment?

   (a) lottery → *go to question 29*

   (b) sure payment → *go to question 26*

26. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 260 Euro as a sure payment?

   (a) lottery → *go to question 27*

   (b) sure payment → *go to question 28*

27. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 270 Euro as a sure payment?

   (a) lottery

   (b) sure payment

28. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 250 Euro as a sure payment?

    (a) lottery

    (b) sure payment

29. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 300 Euro as a sure payment?

    (a) lottery → *go to question 31*

    (b) sure payment → *go to question 30*

30. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 290 Euro as a sure payment?

    (a) lottery

    (b) sure payment

31. What would you prefer: a 50 percent chance of winning 300 Euro when at the same time there is 50 percent chance of winning nothing, or would you rather have the amount of 310 Euro as a sure payment?

    (a) lottery

    (b) sure payment

The staircase procedure is illustrated in Figure 3.2.

3.7.7 Staircase Time

Start with the first question. Depending on whether the participant chooses the earlier or the delayed option, go to the respective next question. This procedure is repeated four times.

Suppose you were given the choice between the following: receiving a payment today or a payment in 12 months. We will now present to you five situations. The payment today is the same in each of these situations. The payment in 12 months is different in every situation. For each of these situations we would like to know which you would choose.

1. Would you rather receive 100 Euro today or 153.8 Euro in 12 months?

   (a) today → *go to question 17*

   (b) in 12 months → *go to question 2*

2. Would you rather receive 100 Euro today or 125.4 Euro in 12 months?

   (a) today → *go to question 10*

   (b) in 12 months → *go to question 3*

3. Would you rather receive 100 Euro today or 112.4 Euro in 12 months?

   (a) today → *go to question 7*

   (b) in 12 months → *go to question 4*

4. Would you rather receive 100 Euro today or 106.1 Euro in 12 months?

   (a) today → *go to question 6*

   (b) in 12 months → *go to question 5*

5. Would you rather receive 100 Euro today or 103.0 Euro in 12 months?

   (a) today

   (b) in 12 months

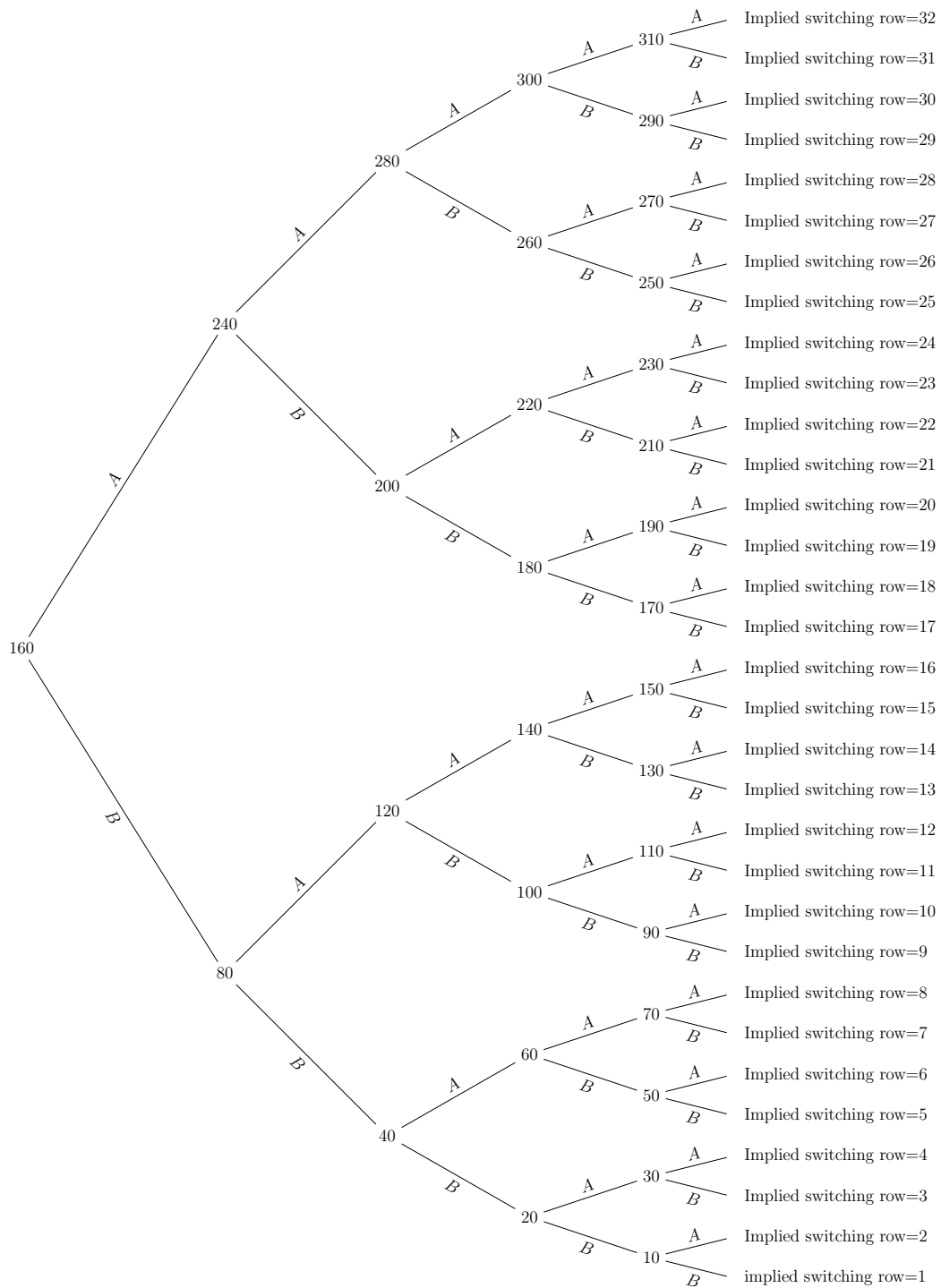6. Would you rather receive 100 Euro today or 109.2 Euro in 12 months?

Figure 3.2: Decision Tree for the Staircase Task for Risk Taking.

Notes. Numbers represent sure payments. "A" denotes the choice of the sure payment, "B" denotes the choice of the lottery. The staircase procedure worked as follows. First, each respondent was asked whether they would prefer to receive 160 euros for sure or whether they preferred a 50:50 chance of receiving 300 euros or nothing. In case the respondent opted for the safe choice ("B"), the safe amount of money being offered in the second question decreased to 80 euros. If, on the other hand, the respondent opted for the gamble ("A"), the safe amount was increased to 240 euros. Working further through the tree follows the same logic.

145

(a) today

(b) in 12 months

7. Would you rather receive 100 Euro today or 118.8 Euro in 12 months?

   (a) today → *go to question 8*

   (b) in 12 months → *go to question 9*

8. Would you rather receive 100 Euro today or 122.1 Euro in 12 months?

   (a) today

   (b) in 12 months

9. Would you rather receive 100 Euro today or 115.6 Euro in 12 months?

   (a) today

   (b) in 12 months

10. Would you rather receive 100 Euro today or 139.2 Euro in 12 months?

   (a) today → *go to question 14*

   (b) in 12 months → *go to question 11*

11. Would you rather receive 100 Euro today or 132.3 Euro in 12 months?

   (a) today → *go to question 13*

   (b) in 12 months → *go to question 12*

12. Would you rather receive 100 Euro today or 128.8 Euro in 12 months?

   (a) today

   (b) in 12 months

13. Would you rather receive 100 Euro today or 135.7 Euro in 12 months?

   (a) today

   (b) in 12 months

14. Would you rather receive 100 Euro today or 146.4 Euro in 12 months?

(a) today → *go to question 16*

(b) in 12 months → *go to question 15*

15. Would you rather receive 100 Euro today or 142.8 Euro in 12 months?

    (a) today

    (b) in 12 months

16. Would you rather receive 100 Euro today or 150.1 Euro in 12 months?

    (a) today

    (b) in 12 months

17. Would you rather receive 100 Euro today or 185.0 Euro in 12 months?

    (a) today → *go to question 18*

    (b) in 12 months → *go to question 25*

18. Would you rather receive 100 Euro today or 201.6 Euro in 12 months?

    (a) today → *go to question 22*

    (b) in 12 months → *go to question 19*

19. Would you rather receive 100 Euro today or 193.2 Euro in 12 months?

    (a) today → *go to question 20*

    (b) in 12 months → *go to question 21*

20. Would you rather receive 100 Euro today or 197.4 Euro in 12 months?

    (a) today

    (b) in 12 months

21. Would you rather receive 100 Euro today or 189.1 Euro in 12 months?

    (a) today

    (b) in 12 months

22. Would you rather receive 100 Euro today or 210.3 Euro in 12 months?

(a) today → *go to question 23*

(b) in 12 months → *go to question 24*

23. Would you rather receive 100 Euro today or 214.6 Euro in 12 months?

(a) today

(b) in 12 months

24. Would you rather receive 100 Euro today or 205.9 Euro in 12 months?

(a) today

(b) in 12 months

25. Would you rather receive 100 Euro today or 169.0 Euro in 12 months?

(a) today → *go to question 29*

(b) in 12 months → *go to question 26*

26. Would you rather receive 100 Euro today or 161.3 Euro in 12 months?

(a) today → *go to question 28*

(b) in 12 months → *go to question 27*

27. Would you rather receive 100 Euro today or 157.5 Euro in 12 months?

(a) today

(b) in 12 months

28. Would you rather receive 100 Euro today or 165.1 Euro in 12 months?

(a) today

(b) in 12 months

29. Would you rather receive 100 Euro today or 176.9 Euro in 12 months?

(a) today → *go to question 31*

(b) in 12 months → *go to question 30*

30. Would you rather receive 100 Euro today or 172.9 Euro in 12 months?

(a) today

(b) in 12 months

31. Would you rather receive 100 Euro today or 180.9 Euro in 12 months?

(a) today

(b) in 12 months

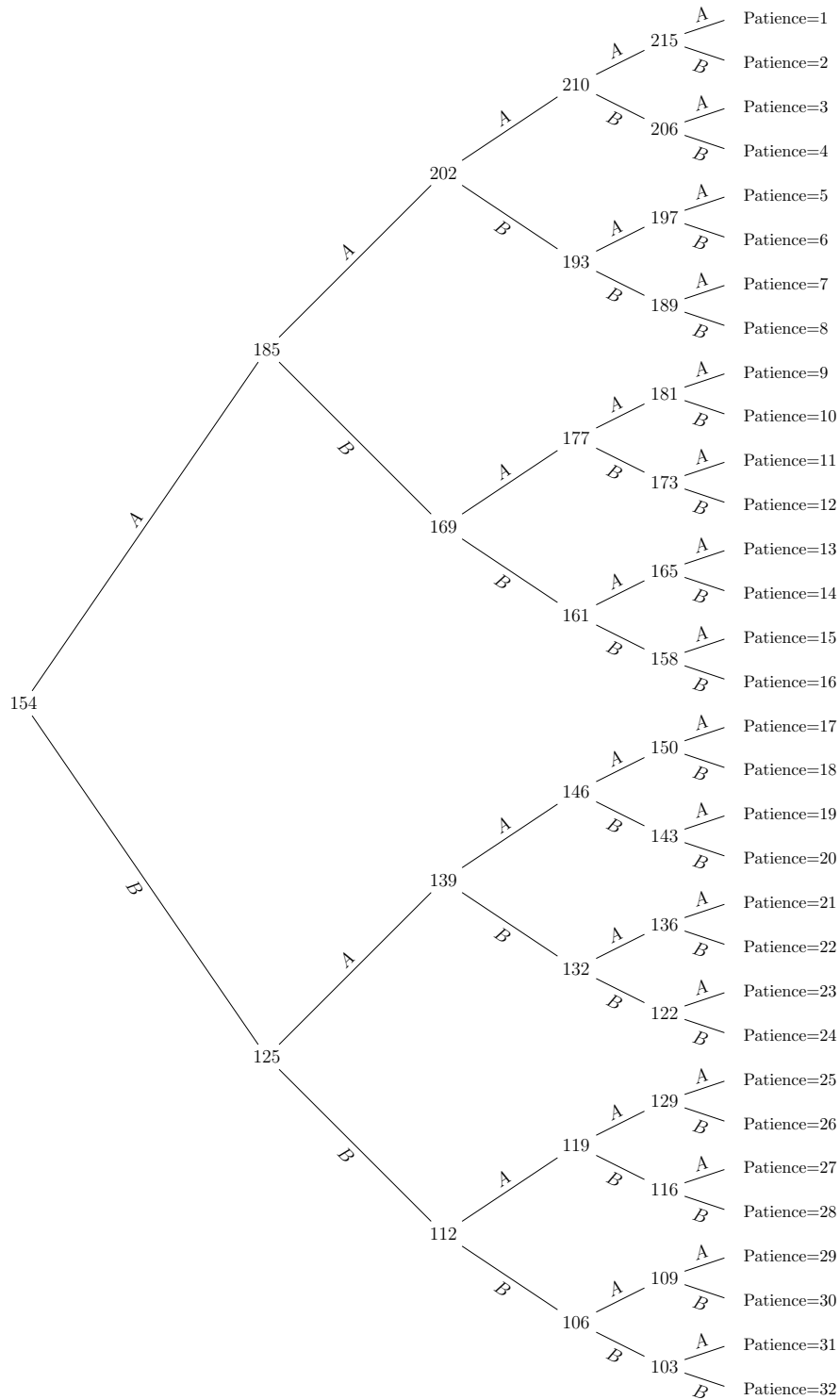The staircase procedure is illustrated in Figure 3.3.

Figure 3.3: Decision Tree for the Staircase Task for Time Discounting

Notes. Numbers represent payments in 12 months. "A" denotes the choice of "100 euros today", "B" denotes the choice of "x euros in 12 months". The staircase procedure worked as follows. First, each respondent was asked whether they would prefer to receive 100 euros today or 154 euros in 12 months from now (leftmost decision node). In case the respondent opted for the payment today ("A"), in the second question the payment in 12 months was adjusted upwards to 185 euros. If, on the other hand, the respondent chose the payment in 12 months, the corresponding payment was adjusted down to 125 euros. Working further through the tree follows the same logic.

150

3.7.8 Refined Version of the Preference Module Used to Collect Global Preference Data in Gallup World Poll 2012

1. Please tell me, in general, how willing or unwilling you are to take risks.

   Please use a scale from 0 to 10, where 0 means you are "completely unwilling to take risks" and a 10 means you are "very willing to take risks". You can also use any numbers between 0 and 10 to indicate where you fall on the scale, like 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

completely unwilling to take risks								very willing to take risks		
0	1	2	3	4	5	6	7	8	9	10
☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐

2. We now ask for your willingness to act in a certain way in four different areas.

   Please again indicate your answer on a scale from 0 to 10, where 0 means you are "completely unwilling to do so" and a 10 means you are "very willing to do so". You can also use any numbers between 0 and 10 to indicate where you fall on the scale, like 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

	completely unwilling to do so								very willing to do so		
How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?	0 ☐	1 ☐	2 ☐	3 ☐	4 ☐	5 ☐	6 ☐	7 ☐	8 ☐	9 ☐	10 ☐
How willing are you to punish someone who treats **you** unfairly, even if there may be costs for you?	0 ☐	1 ☐	2 ☐	3 ☐	4 ☐	5 ☐	6 ☐	7 ☐	8 ☐	9 ☐	10 ☐
How willing are you to punish someone who treats **others** unfairly, even if there may be costs for you?	0 ☐	1 ☐	2 ☐	3 ☐	4 ☐	5 ☐	6 ☐	7 ☐	8 ☐	9 ☐	10 ☐
How willing are you to give to good causes without expecting anything in return?	0 ☐	1 ☐	2 ☐	3 ☐	4 ☐	5 ☐	6 ☐	7 ☐	8 ☐	9 ☐	10 ☐

3. How well do the following statements describe you as a person?

Please indicate your answer on a scale from 0 to 10. A 0 means "does not describe me at all" and a 10 means "describes me perfectly". You can also use any numbers between 0 and 10 to indicate where you fall on the scale, like 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

	does not describe me at all									describes me perfectly	
When someone does me a favor I am willing to return it.	0 ☐	1 ☐	2 ☐	3 ☐	4 ☐	5 ☐	6 ☐	7 ☐	8 ☐	9 ☐	10 ☐
If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so.	0 ☐	1 ☐	2 ☐	3 ☐	4 ☐	5 ☐	6 ☐	7 ☐	8 ☐	9 ☐	10 ☐
I assume that people have only the best intentions.	0 ☐	1 ☐	2 ☐	3 ☐	4 ☐	5 ☐	6 ☐	7 ☐	8 ☐	9 ☐	10 ☐

4. Please imagine the following situation: You can choose between a sure payment of a particular amount of money, or a draw, where you would have an equal chance of getting 300 Euro or getting nothing. We will present to you five different situations.

4.1 What would you prefer: a draw with a 50 percent chance of receiving 300 Euro, and the same 50 percent chance of receiving nothing, or the amount of 160 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.17*

☐ = Sure payment → *Go to question 4.2*

4.2 Would you prefer the 50/50 chance or the amount of 80 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.10*

☐ = Sure payment → *Go to question 4.3*

4.3 Would you prefer the 50/50 chance or the amount of 40 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.4*

☐ = Sure payment → *Go to question 4.7*

4.4 Would you prefer the 50/50 chance or the amount of 60 Euro as a sure payment?

□ = 50/50 chance → *Go to question 4.5*

□ = Sure payment → *Go to question 4.6*

4.5 Would you prefer the 50/50 chance or the amount of 70 Euro as a sure payment?

□ = 50/50 chance → *Go to question 5*

□ = Sure payment → *Go to question 5*

4.6 Would you prefer the 50/50 chance or the amount of 50 Euro as a sure payment?

□ = 50/50 chance → *Go to question 5*

□ = Sure payment → *Go to question 5*

4.7 Would you prefer the 50/50 chance or the amount of 20 Euro as a sure payment?

□ = 50/50 chance → *Go to question 4.8*

□ = Sure payment → *Go to question 4.9*

4.8 Would you prefer the 50/50 chance or the amount of 30 Euro as a sure payment?

□ = 50/50 chance → *Go to question 5*

□ = Sure payment → *Go to question 5*

4.9 Would you prefer the 50/50 chance or the amount of 10 Euro as a sure payment?

□ = 50/50 chance → *Go to question 5*

□ = Sure payment → *Go to question 5*

4.10 Would you prefer the 50/50 chance or the amount of 120 Euro as a sure payment?

□ = 50/50 chance → *Go to question 4.14*

□ = Sure payment → *Go to question 4.11*

4.11 Would you prefer the 50/50 chance or the amount of 100 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.13*

☐ = Sure payment → *Go to question 4.12*

4.12 Would you prefer the 50/50 chance or the amount of 90 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 5*

☐ = Sure payment → *Go to question 5*

4.13 Would you prefer the 50/50 chance or the amount of 110 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 5*

☐ = Sure payment → *Go to question 5*

4.14 Would you prefer the 50/50 chance or the amount of 140 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.15*

☐ = Sure payment → *Go to question 4.16*

4.15 Would you prefer the 50/50 chance or the amount of 150 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 5*

☐ = Sure payment → *Go to question 5*

4.16 Would you prefer the 50/50 chance or the amount of 130 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 5*

☐ = Sure payment → *Go to question 5*

4.17 Would you prefer the 50/50 chance or the amount of 240 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.25*

☐ = Sure payment → *Go to question 4.18*

4.18 Would you prefer the 50/50 chance or the amount of 200 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.22*

$\square$ = Sure payment → *Go to question* 4.19

4.19 Would you prefer the 50/50 chance or the amount of 180 Euro as a sure payment?

$\square$ = 50/50 chance → *Go to question* 4.20

$\square$ = Sure payment → *Go to question* 4.21

4.20 Would you prefer the 50/50 chance or the amount of 190 Euro as a sure payment?

$\square$ = 50/50 chance → *Go to question* 5

$\square$ = Sure payment → *Go to question* 5

4.21 Would you prefer the 50/50 chance or the amount of 170 Euro as a sure payment?

$\square$ = 50/50 chance → *Go to question* 5

$\square$ = Sure payment → *Go to question* 5

4.22 Would you prefer the 50/50 chance or the amount of 220 Euro as a sure payment?

$\square$ = 50/50 chance → *Go to question* 4.23

$\square$ = Sure payment → *Go to question* 4.24

4.23 Would you prefer the 50/50 chance or the amount of 230 Euro as a sure payment?

$\square$ = 50/50 chance → *Go to question* 5

$\square$ = Sure payment → *Go to question* 5

4.24 Would you prefer the 50/50 chance or the amount of 210 Euro as a sure payment?

$\square$ = 50/50 chance → *Go to question* 5

$\square$ = Sure payment → *Go to question* 5

4.25 Would you prefer the 50/50 chance or the amount of 280 Euro as a sure payment?

$\square$ = 50/50 chance → *Go to question* 4.29

$\square$ = Sure payment → *Go to question* 4.26

4.26 Would you prefer the 50/50 chance or the amount of 260 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.27*

☐ = Sure payment → *Go to question 4.28*

4.27 Would you prefer the 50/50 chance or the amount of 270 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 5*

☐ = Sure payment → *Go to question 5*

4.28 Would you prefer the 50/50 chance or the amount of 250 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 5*

☐ = Sure payment → *Go to question 5*

4.29 Would you prefer the 50/50 chance or the amount of 300 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 4.31*

☐ = Sure payment → *Go to question 4.30*

4.30 Would you prefer the 50/50 chance or the amount of 290 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 5*

☐ = Sure payment → *Go to question 5*

4.31 Would you prefer the 50/50 chance or the amount of 310 Euro as a sure payment?

☐ = 50/50 chance → *Go to question 5*

☐ = Sure payment → *Go to question 5*

5. Please think about what you would do in the following situation.

You are in an area you are not familiar with, and you realize that you lost your way. You ask a stranger for directions. The stranger offers to take you to your destination. Helping you costs the stranger about 20 Euro in total. However, the stranger says he or she does not want any money from you. You have 6

presents with you. The cheapest present costs 5 Euro, the most expensive one costs 30 Euro. Do you give one of the presents to the stranger as a "thank-you"-gift? If so, which present do you give to the stranger?

☐ no present

☐ the present worth 5 Euro

☐ the present worth 10 Euro

☐ the present worth 15 Euro

☐ the present worth 20 Euro

☐ the present worth 25 Euro

☐ the present worth 30 Euro

6. Imagine the following situation: Today you unexpectedly received 1,000 Euro. How much of this amount would you donate to a good cause? *(Values between 0 and 1,000 are allowed)*

7. Suppose you were given the choice between receiving a payment today or a payment in 12 months. We will now present to you 5 situations. The payment today is the same in each of these situations. The payment in 12 months is different in every situation. For each of these situations we would like to know which you would choose. Please assume there is no inflation, i.e. future prices are the same as today's prices.

7.1 Please consider the following: would you rather receive 100 Euro today or 154 Euro in 12 months?

☐ = Today → *Go to question 7.17*

☐ = In 12 months → *Go to question 7.2*

7.2 Would you rather receive 100 Euro today or 125 Euro in 12 months?

☐ = Today → *Go to question 7.10*

☐ = In 12 months → *Go to question 7.3*

7.3 Would you rather receive 100 Euro today or 112 Euro in 12 months?

☐ = Today → *Go to question 7.7*

☐ = In 12 months → *Go to question 7.4*

7.4 Would you rather receive 100 Euro today or 106 Euro in 12 months?

☐ = Today → *Go to question 7.6*

☐ = In 12 months → *Go to question 7.5*

7.5 Would you rather receive 100 Euro today or 103 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.6 Would you rather receive 100 Euro today or 109 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.7 Would you rather receive 100 Euro today or 119 Euro in 12 months?

☐ = Today → *Go to question 7.8*

☐ = In 12 months → *Go to question 7.9*

7.8 Would you rather receive 100 Euro today or 122 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.9 Would you rather receive 100 Euro today or 116 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.10 Would you rather receive 100 Euro today or 139 Euro in 12 months?

☐ = Today → *Go to question 7.14*

☐ = In 12 months → *Go to question 7.11*

7.11 Would you rather receive 100 Euro today or 132 Euro in 12 months?

☐ = Today → *Go to question 7.13*

☐ = In 12 months → *Go to question 7.12*

7.12 Would you rather receive 100 Euro today or 129 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.13 Would you rather receive 100 Euro today or 136 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.14 Would you rather receive 100 Euro today or 146 Euro in 12 months?

☐ = Today → *Go to question* 7.16

☐ = In 12 months → *Go to question* 7.15

7.15 Would you rather receive 100 Euro today or 143 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.16 Would you rather receive 100 Euro today or 150 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.17 Would you rather receive 100 Euro today or 185 Euro in 12 months?

☐ = Today → *Go to question* 7.18

☐ = In 12 months → *Go to question* 7.25

7.18 Would you rather receive 100 Euro today or 202 Euro in 12 months?

☐ = Today → *Go to question* 7.22

☐ = In 12 months → *Go to question* 7.19

7.19 Would you rather receive 100 Euro today or 193 Euro in 12 months?

☐ = Today → *Go to question* 7.20

☐ = In 12 months → *Go to question* 7.21

7.20 Would you rather receive 100 Euro today or 197 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.21 Would you rather receive 100 Euro today or 189 Euro in 12 months?

☐ = Today [*Final question*]

☐ = In 12 months [*Final question*]

7.22 Would you rather receive 100 Euro today or 210 Euro in 12 months?

$\square$ = Today → *Go to question* 7.23

$\square$ = In 12 months → *Go to question* 7.24

7.23 Would you rather receive 100 Euro today or 215 Euro in 12 months?

$\square$ = [*Final question*]

$\square$ = [*Final question*]

7.24 Would you rather receive 100 Euro today or 206 Euro in 12 months?

$\square$ = Today [*Final question*]

$\square$ = In 12 months [*Final question*]

7.25 Would you rather receive 100 Euro today or 169 Euro in 12 months?

$\square$ = Today → *Go to question* 7.29

$\square$ = In 12 months → *Go to question* 7.26

7.26 Would you rather receive 100 Euro today or 161 Euro in 12 months?

$\square$ = Today → *Go to question* 7.28

$\square$ = In 12 months → *Go to question* 7.27

7.27 Would you rather receive 100 Euro today or 158 Euro in 12 months?

$\square$ = Today [*Final question*]

$\square$ = In 12 months [*Final question*]

7.28 Would you rather receive 100 Euro today or 165 Euro in 12 months?

$\square$ = Today [*Final question*]

$\square$ = In 12 months [*Final question*]

7.29 Would you rather receive 100 Euro today or 177 Euro in 12 months?

$\square$ = Today → *Go to question* 7.31

$\square$ = In 12 months → *Go to question* 7.30

7.30 Would you rather receive 100 Euro today or 173 Euro in 12 months?

$\square$ = Today [*Final question*]

$\square$ = In 12 months [*Final question*]

7.31 Would you rather receive 100 Euro today or 181 Euro in 12 months?

$\square$ = Today [*Final question*]

$\square$ = In 12 months [*Final question*]

3.7.9   The Preference Module: Original Wording

1. Risk Taking

   (a) Wie schätzen Sie sich persönlich ein? Sind Sie im Allgemeinen ein risikobereiter Mensch oder versuchen Sie, Risiken zu vermeiden? Bitte klicken Sie ein Kästchen auf der Skala an, wobei der Wert 0 bedeutet "gar nicht risikobereit", und der Wert 10 bedeutet "sehr risikobereit". Mit den Werten dazwischen können Sie Ihre Einschätzung abstufen.

   (b) *Liste mit 31 hypothetischen Entscheidungen:* Stellen Sie sich bitte folgende Situation vor: Sie haben die Wahl zwischen einer sicheren Auszahlung und einer Lotterie. Bei der Lotterie erhalten Sie mit 50 Prozent Chance 300 Euro, und mit 50 Prozent Chance erhalten Sie nichts. Bitte stellen Sie sich nun vor, Sie müssten sich zwischen der Lotterie (die immer gleich bleibt), und einer sicheren Auszahlung (die sich von Situation zu Situation unterscheidet), entscheiden. Auf dem folgenden Bildschirm werden Ihnen verschiedene Entscheidungssituationen angezeigt. Anschliessend bitten wir Sie, für jede dieser hypothetischen Situationen einzeln Ihre Entscheidung zwischen der Lotterie und der sicheren Auszahlung anzugeben.
   Bitte überlegen Sie: Was hätten Sie lieber: eine 50-prozentige Chance 300 Euro zu gewinnen bei gleichzeitiger 50-prozentiger Chance nichts zu gewinnen, oder einen Geldbetrag von ___ [33] Euro als sichere Auszahlung?

2. Time Discounting

   (a) Sind Sie im Vergleich zu anderen im Allgemeinen bereit, heute auf etwas zu verzichten, um in der Zukunft davon zu profitieren, oder sind Sie im Vergleich zu anderen dazu nicht bereit? Bitte klicken Sie ein Kästchen auf der Skala an, wobei der Wert 0 bedeutet "gar nicht bereit", und der Wert 10 bedeutet "sehr bereit". Mit den Werten dazwischen können Sie Ihre Einschätzung abstufen.

   (b) *Liste mit 25 hypothetischen Entscheidungen:* In diesem Teil des Experiments bitten wir Sie, sich Folgendes vorzustellen: Nehmen Sie an, Sie

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[33]Compare Section 3.7.2

hätten folgende Wahl: eine Auszahlung heute oder eine Auszahlung in 12 Monaten. Im Folgenden werden Ihnen verschiedene Situationen präsentiert. In jeder Situation ist die heutige Auszahlung dieselbe, die Auszahlung in 12 Monaten ist jedoch in jeder Situation anders. Wir möchten für jede dieser Situationen wissen, wie Sie sich entscheiden würden.

Bitte überlegen Sie: Würden Sie lieber 100 Euro heute bekommen oder ___ [34] Euro in 12 Monaten?

3. Trust

   (a) *(after reading the instructions for the Trust Game, see paragraph on Positive Reciprocity)* Angenommen, Sie sollen den Vorschlag über die Aufteilung machen. Welchen Betrag würden Sie der anderen Person anbieten?

   (b) Wie sehr trifft die folgende Aussage auf Sie zu? Solange man mich nicht vom Gegenteil überzeugt, gehe ich stets davon aus, dass andere Menschen nur das Beste im Sinn haben. Bitte klicken Sie ein Kästchen auf der Skala an, wobei der Wert 0 bedeutet "trifft gar nicht zu", und der Wert 10 bedeutet "trifft voll zu". Mit den Werten dazwischen können Sie Ihre Einschätzung abstufen.

4. Altruism

   (a) Wie schätzen Sie Ihre Bereitschaft mit anderen zu teilen, ohne dafür eine Gegenleistung zu erwarten, in Bezug auf den folgenden Bereich ein: wenn es um gemeinnützige Zwecke geht? Bitte klicken Sie ein Kästchen auf der Skala an, wobei der Wert 0 bedeutet "gar nicht bereit zu teilen ohne eine Gegenleistung zu erwarten", und der Wert 10 bedeutet "sehr bereit zu teilen ohne eine Gegenleistung zu erwarten". Mit den Werten dazwischen können Sie ihre Einschätzung abstufen.

   (b) Stellen Sie sich folgende Situation vor: Sie haben in einem Preisausschreiben 1.000 Euro gewonnen. Wie viel würden Sie in Ihrer momentanen

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[34]Compare Section 3.7.2

Situation für einen gemeinnützigen Zweck spenden? *(Values between 0 and 1000 are allowed)*

5. Positive Reciprocity

   (a) Überlegen Sie bitte, was Sie in folgender Situation tun würden: Sie und eine andere Person, die Sie nicht kennen, treffen beide eine Entscheidung über die Verwendung von Geld und erzielen zusammen ein Ergebnis. Die Regeln gehen so: Jeder Teilnehmer erhält ein Konto mit 20 Euro. Am Anfang haben Sie und die andere Person also jeweils 20 Euro auf dem Konto. Zuerst entscheidet die andere Person. Sie kann Ihnen Geld auf Ihr Konto überweisen. Sie kann Ihnen einen beliebigen Eurobetrag überweisen, also 0 Euro, 1 Euro, 2 Euro usw. bis 20 Euro. Jeder Euro, den die andere Person an Sie überweist, wird von den Leitern der Studie verdreifacht und Ihrem Konto gutgeschrieben. Nach dem ersten Schritt sind also auf dem Konto der anderen Person 20 Euro minus der Überweisung an Sie. Auf Ihrem Konto sind 20 Euro plus dem Dreifachen der Überweisung an Sie. Jetzt entscheiden Sie: Sie haben die Möglichkeit, der anderen Person Geld zurück zu überweisen. Sie können jeden beliebigen Eurobetrag zurück überweisen, also 0, 1, 2, 3, usw. bis 80 Euro, je nachdem, wie viel Geld Sie insgesamt auf Ihrem Konto gutgeschrieben haben, nachdem Sie die Überweisung der anderen Person erhalten haben. Damit ist die Studie beendet. Die endgültigen Kontostände sind erreicht. Auf dem Konto der anderen Person sind jetzt 20 Euro minus der Überweisung an Sie plus Ihrer Rücküberweisung. Auf Ihrem Konto sind jetzt 20 Euro plus das Dreifache der Überweisung an Sie minus Ihrer Rücküberweisung. Wir möchten nun von Ihnen wissen, welche Rücküberweisung Sie wählen würden, wenn die andere Person Ihnen einen bestimmten Betrag überweist.

   Angenommen, die andere Person überweist Ihnen 5(10/15/20) Euro. Sie haben dann nach dem ersten Schritt 20+3*5(10/15/20)=35(50/65/80) Euro, die andere Person hat 20-5(10/15/20)=15(10/5/0) Euro. Wie hoch ist Ihre Rücküberweisung?

   (b) Stellen Sie sich folgende Situation vor: Sie sind beim Einkaufen unterwegs

163

in einer fremden Stadt, und merken, dass Sie sich verlaufen haben. Sie fragen eine fremde Person nach dem Weg. Die Person bietet Ihnen an, Sie mit dem Auto zu Ihrem Ziel zu fahren. Die Fahrt dauert etwa 20 Minuten, und kostet die fremde Person alles in allem etwa 20 Euro. Die fremde Person will aber kein Geld dafür. Sie haben 6 Flaschen Wein dabei. Die billigste Flasche kostet 5 Euro, die teuerste kostet 30 Euro. Sie entscheiden, der fremden Person eine Flasche Wein als Dankeschön zu geben. Welche Flasche schenken Sie? [Die Flasche für 5/10/15/20/25/30 Euro]

6. Negative Reciprocity

   (a) Sind Sie jemand, der im Allgemeinen bereit ist, unfaires Verhalten zu bestrafen, auch wenn das für Sie mit Kosten verbunden ist? Bitte klicken Sie ein Kästchen auf der Skala an, wobei der Wert 0 bedeutet "gar nicht bereit Kosten auf sich zu nehmen um zu bestrafen", und der Wert 10 bedeutet "sehr bereit Kosten auf sich zu nehmen um zu bestrafen". Mit den Werten dazwischen können Sie ihre Einschätzung abstufen.

   (b) Stellen Sie sich folgende Situation vor: Zusammen mit einer anderen Person, die Sie nicht persönlich kennen, haben Sie 100 Euro bei einem Preisausschreiben gewonnen. Die Regeln besagen nun Folgendes. Einer von Ihnen soll einen Vorschlag darüber machen, wie die 100 Euro aufgeteilt werden. Der andere erfährt den Vorschlag, und hat dann zwei Möglichkeiten. Er kann die Aufteilung annehmen oder ablehnen. Wenn er den Vorschlag annimmt, wird das Geld so aufgeteilt, wie die andere Person es vorgeschlagen hat. Wird die Aufteilung abgelehnt, gehen beide leer aus. Angenommen, die andere Person macht einen Vorschlag über die Aufteilung. Sie wiederum sollen entscheiden, ob Sie den Vorschlag annehmen oder ablehnen. Welchen Betrag muss die andere Person Ihnen mindestens anbieten, damit Sie bereit sind, den Vorschlag über die Aufteilung anzunehmen?

Cultural Origins of Cross-Country Variation in Economic Preferences

4.1 Introduction

Empirical research on preference formation during the last decades has uncovered a wide variety of sources of heterogeneity in preferences across individuals - among them age, gender, height, parental socioeconomic status, early life circumstances, hormones, political regimes and cognitive ability.[1] While several studies have documented cross-country heterogeneity in economic behavior and preferences, the task of determining a cultural component has remained difficult as these studies had to rely on non-representative population samples or a small number of countries or both.[2] The novel dataset presented in Falk, Becker, Dohmen, Enke, Huffman, and Sunde (2015) is ideally suited to shed light on this question. It contains economic preference measures for representative population samples from 76 countries worldwide, with a total of more than 80,000 observations. We show that the variation in preferences across countries and across individuals is systematic in the cultural backgrounds by

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[1]Compare, for example, Alesina and Fuchs-Schündeln (2007), Burks, Carpenter, Goette, and Rustichini (2009), Croson and Gneezy (2009), Dohmen, Falk, Huffman, Sunde, Schupp, and Wagner (2011), Falk and Kosse (2012) and Kosfeld, Heinrichs, Zak, Fischbacher, and Fehr (2005).

[2]Compare, for example, Henrich (2000) and Roth, Prasnikar, Okuno-Fujiwara, and Zamir (1991) for differences in bargaining behavior in two and four countries, respectively, Henrich, Boyd, Bowles, Camerer, Fehr, Gintis, and McElreath (2001) for ultimatum game behavior across 15 small-scale societies, Vieider, Lefebre, Bouchouicha, Chmura, Hakimov, Krawczyk, and Martinsson (2015) for a comparison of risk attitudes across 30 countries, and Wang, Rieger, and Hens (2016) for evidence on time preferences from 45 countries.

using a specific feature of languages as a proxy for culture.

While it has remained unclear whether culture is a driver of differences in preferences, research has increasingly focused on its role across a wide range of economically important domains.[3] For example, Alesina and Giuliano (forthcoming) examine the relationship between culture and institutions. Alesina, Giuliano, and Nunn (2013) investigate the origins of gender roles and document a correlation between contemporary female labor force participation and female participation in agriculture in the pre-industrial era, originating in different agricultural technologies back then. Gorodnichenko and Roland (2011a) and Gorodnichenko and Roland (2011b) look at individualism, one of the cultural dimensions as introduced by Hofstede (2001), and show that countries which score higher on the individualism dimension have higher levels of innovation, are more productive and have higher long-run growth than more collectivist countries. Guiso, Sapienza, and Zingales (2006) use religion as a proxy for culture and find cultural differences in preferences for redistribution, which in turn affect actual levels of redistribution at the U.S. state level. Fernández and Fogli (2009) document the impact of cultural beliefs regarding the appropriate role of women in society on female labor force participation and fertility decisions.

Several studies have used language to proxy for culture. Fearon (2003) employs structural distance between languages as a proxy for cultural distance between groups in a given country to construct an index of cultural fractionalization. Desmet, Ortuño-Ortín, and Weber (2009) proxy cultural diversity with linguistic diversity to show its effect on redistribution at the country level. Licht, Goldschmidt, and Schwartz (2007) identify a causal effect of a culture's emphasis on autonomy versus embeddedness on countries' rule of law, corruption, and democratic accountability by using the grammatical feature of pronoun drop (whether a language allows to drop the pronouns "I" or "you") as an instrument. Kashima and Kashima (1998) document an association between pronoun drop and cultural dimensions such as Individualism and Power Distance (Hofstede, 2001). Spolaore and Wacziarg (2015) link cultural distance to genetic distance and document a strong association with linguistic distance, corroborating the validity of language as a proxy for culture.

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[3]A notable exemption is Galor and Özak (2014), who show that differences in agricultural conditions on the pre-industrial era explain differences in time preferences today.

In a recent paper, Chen poses what he calls a "linguistic-savings hypothesis" (Chen, 2013). He argues that languages which require its speakers to make a grammatical distinction between the present and the future also induce less future-oriented behavior. In line with his hypothesis, he shows that individuals who speak languages which require a future-time reference ("strong FTR languages") save less, retire with less wealth, smoke more, are less likely to practice safer sex, and are more likely to be obese than individuals who speak languages that allow to use the present tense when talking about future events ("weak FTR languages").[4]

In this chapter we also abstract from concrete concepts of culture, such as the cultural dimensions by Hofstede, and use the FTR-criterion as employed by Chen (2013) to proxy differences in individuals' cultural backgrounds. Replicating Chen's findings for direct preference measures, we show that speakers of weak FTR languages are more patient. Moreover, we hypothesize that weak FTR languages should foster future orientation in other preference domains as well, such as prosociality or negative reciprocity. Indeed, we find that at the country level the fraction of people speaking a weak FTR language is positively associated with patience, positive reciprocity and trust. We do not find an association with altruism or negative reciprocity. For patience, positive reciprocity and trust we find the same results within countries as well. In addition, we also find an association between the FTR criterion and altruism at the individual level. Our results indicate that part of the cross-country variation in preferences across the globe reflects differences in cultural heritage.

The rest of this chapter is structured as follows. Section 4.2 describes the data from the Global Preference Survey as well as the language data. Section 4.3 presents our hypotheses and Section 4.4 the results. Section 4.5 concludes.

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[4]Sutter, Angerer, Glätzle-Rützler, and Lergetporer (2015) elicit children's time preferences in a bilingual city in Northern Italy and find that children who speak Italian (strong FTR) are less patient than children who speak German (weak FTR).

4.2 Data

4.2.1 The Global Preference Survey

The Global Preference Survey (GPS) is a new globally representative survey designed to measure six key economic preferences: risk taking, time discounting, altruism, trust, positive and negative reciprocity. The GPS data were collected within the framework of the Gallup World Poll, which surveys representative population samples in a large number of countries about social and economic issues on an annual basis. In 2012, we added the GPS to the World Poll's questionnaire in 76 countries.

In each country, we obtained preference measures for representative population samples. The median sample size was 1,000 participants per country. Respondents were selected through probability sampling. Ex-post representativeness of the data was achieved by using the weights provided by Gallup.[5] In total, we collected preference measures for more than 80,000 participants worldwide.

The countries were selected to maximize geographical representativeness. Our sample of 76 countries is not restricted to Western industrialized nations, but covers all continents, various cultures, and different levels of development. Our sample includes 15 countries from the Americas, 25 from Europe, 22 from Asia and the Pacific, as well as 14 from Africa, 11 of which are Sub-Saharan. This set of countries covers about 90% of the world population and of global income.

Our preference measures were selected using a rigorous ex-ante experimental validation and selection procedure as described in Chapter 3 of this thesis. The procedure is described in detail in Sections 3.3 and 3.5. The survey items are found in Section 3.7.8.

The survey items were translated into the different interview languages back and forth in an iterative process using Gallup's regular translation scheme.[6] All monetary values were calibrated in relation to the median household income for each country,

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[5]The weights are constructed to render the observations representative in terms of age, gender, income, education, and geographic location.

[6]The translation process is very careful. It starts with one translator, who is proficient in two languages, e.g. English and Swahili, who will complete the first step of the translation process, e.g. from English into Swahili. Two translators, who are proficient in the same languages, will then do a back-translation (from Swahili to English). A fourth translator will then compare the resulting versions. This process is iterated until all translators agree on the same version.

using the relationship of monetary values used in the validation study to the German median household income as a benchmark.[7]

4.2.2 Language Data

We make use of the language classification and the respective data presented in Chen (2013). Chen applies the classification in EUROTYP[8], Dahl (2000), and Thieroff (2000), and extends their classification to further languages using corresponding cross-linguistic studies (compare the detailed description and sources provided in the Appendix of Chen (2013)).

In particular, Chen's classification divides languages into two sets according to a structural feature called future-time reference (FTR): those, who require its speakers to grammatically mark future events (strong FTR languages) and those who allow its speakers to use the present tense when talking about the future (weak FTR languages). As an illustration: strong FTR languages, like English, require its speakers to explicitly distinguish between the present and the future by making use of constructions such as "Tomorrow I *will* submit my thesis", whereas in weak FTR languages, such as German, it is possible to use the present tense when talking about the future ("Morgen reiche ich meine Dissertation ein" which would literally correspond to "Tomorrow I submit my thesis").

Applying Chen's classification to our dataset we arrive at a set of 55 coded languages. In addition, we were able to code four additional languages ourselves using the methodology outlined in Chen (2013).[9] In sum, this gives us 59 classified lan-

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[7]Since monetary amounts used in the validation study with the German student sample were round numbers to facilitate easy calculations (e.g., the expected return of a lottery with equal chances of winning and losing) and to allow for easy comparisons (e.g., 100 Euro today versus 107.50 in 12 months), we also rounded monetary amounts in all other countries to the next "round" number. While this necessarily resulted in some (minor) variations in the real stake size between countries, it minimized cross-country differences in the understanding of the quantitative items due to otherwise arising difficulties in assessing the involved monetary amounts.

[8]See https://www.eva.mpg.de/lingua/tools-at-lingboard/questionnaire/eurotypdescription.php: EUROTYP was a large-scale project of the European Science foundation aimed at examining the range of typological variation found in the languages of Europe with tense and aspect being one grammatical area under examination.

[9]These languages are Fulfulde (weak FTR), Khmer (strong FTR), Moroccan Arabic (weak FTR), and Dari (strong FTR). In addition, we changed one of Chen's classification after corresponding with him. He classified Persian (Farsi) as strong FTR, while it is in fact weak FTR. None of our results (and neither his) depend on how we code Persian.

guages for a total of 75,224 respondents.[10]

4.3   Hypotheses

As Chen (2013) posits, a grammatical separation of the future from the present makes the speaker perceive the future as more distant compared to when there is no such grammatical separation. This might lead to a devaluation of the future and thus to less future-orientation in decision-making.[11]

Analogously with Chen (2013) we therefore expect speakers of strong FTR languages to exhibit less future-orientation in our preference measures for time discounting. This would show that Chen's results also hold true for a very direct measure of how people trade off current and future rewards.

One can argue in a similar way for positive and negative reciprocity. The tendency to reciprocate can incorporate repeated interaction motives (e.g. in relational contracts settings). As such, negative reciprocity can be described as the willingness to punish unfairness or norm violation so as to enforce fair behavior or norm adherence (in the future). Similarly, positive reciprocity can be described as the tendency to reward kind, cooperative or fair actions so as to foster such a behavior (in the future).[12] We therefore expect speakers of weak FTR to be more future oriented in the reciprocity domains, i.e. to have stronger positive as well as stronger negative reciprocal inclinations. An immediate consequence from our hypothesis on the association between the FTR criterion and positive reciprocity is that we should expect the same association for trust, since the tendency to reciprocate positively naturally fosters trust.

We do not expect a correlation between the FTR criterion and risk attitudes or altruism.

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[10]We could not code 23 languages, which are mostly spoken by small minorities (5,113 respondents in total).

[11]More precisely, Chen derives his linguistic-savings hypothesis from two different channels: on the one hand, speaking about the future using the present tense might bias beliefs such that the future feels less distant. On the other hand and leading to the same conclusion, not explicitly marking the future might introduce uncertainty about the timing of future rewards, which would similarly facilitate future-orientation if some probability is put on the future occurring sooner.

[12]See Dohmen, Falk, Huffman, and Sunde (2009) for a discussion and empirical evidence.

4.4 Results

4.4.1 Cross-Country Analysis

For each country, we compute the country-level fraction of people whose language corresponds to weak as opposed to strong FTR. Then, we regress average preferences in a given country on this fraction. To take into account that we can classify only a subset of respondents in some countries (making the fraction speaking weak FTR languages a less precise estimate of the true population counterpart), our regressions weigh all observations by the fraction of people whose language can be classified. Thus, countries in which we can classify a larger fraction of respondents receive higher weight, as should be the case from a measurement error perspective.[13]

Table 4.1 presents the results. For each preference, we report two specifications, one without covariates and one with control variables commonly employed in cross-country regressions, i.e., continent fixed effects, (log) per capita income, distance to the equator, longitude, the fraction of the population that is at risk of contracting malaria, and average precipitation. Results show that, across countries, weak FTR is significantly correlated with average patience (columns (1)-(2)). As columns (5)-(6) and (11)-(12) show, similar patterns obtain for positive reciprocity and trust. In contrast, altruism, risk taking, and negative reciprocity are not significantly correlated with the fraction speaking weak FTR languages.

4.4.2 Within-Country Analysis

In a second step of the analysis, we exploit within-country variation in preferences and FTR. Such analyses are arguably better suited to identify cultural origins of preferences because they can account for unobserved heterogeneity at the country level.

In many countries in our sample, we observe some variation in interview languages. However, variation in language does not necessarily mean variation in FTR. In fact, only in Estonia, Nigeria, and Switzerland (2,925 respondents in total) do

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[13]Appendix 4.6 confirms that virtually identical results are obtained when running unweighted OLS regressions, suggesting that measurement error in the fraction speaking weak FTR languages is weak.

Table 4.1: Preferences and FTR: Cross-country results

	Patience		Risk taking		Pos. reciprocity		Neg. reciprocity		Altruism		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Fraction of population speaking weak FTR	0.37***	0.24***	-0.14*	-0.0080	0.13*	0.16**	-0.024	-0.088	0.043	0.099	0.17**	0.18**
	(0.13)	(0.09)	(0.07)	(0.07)	(0.08)	(0.08)	(0.07)	(0.09)	(0.09)	(0.10)	(0.08)	(0.08)
Log [GDP p/c PPP]		0.15***		0.032		-0.072*		0.058		-0.078*		-0.00072
		(0.04)		(0.03)		(0.04)		(0.04)		(0.04)		(0.03)
Distance to equator		0.010*		0.0017		-0.0069		-0.0081		-0.0022		-0.0072
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)		(0.00)
Longitude		-0.0019		0.0023		0.0022		0.00091		0.0025		-0.000039
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
% at risk of malaria		0.25		-0.14		-0.27		-0.13		-0.71**		-0.16
		(0.21)		(0.23)		(0.29)		(0.17)		(0.27)		(0.19)
Average precipitation		-0.00024		-0.00092		0.00065		-0.0011		0.0031**		-0.0011
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
Constant	-0.055	-1.42***	0.034	-0.041	-0.047	1.39***	0.020	-0.13	-0.049	1.27**	-0.047	0.56
	(0.04)	(0.51)	(0.04)	(0.36)	(0.05)	(0.43)	(0.04)	(0.50)	(0.05)	(0.50)	(0.04)	(0.38)
Continent FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	75	74	75	74	75	74	75	74	75	74	75	74
R^2	0.146	0.636	0.031	0.442	0.022	0.253	0.001	0.271	0.002	0.334	0.053	0.408

WLS estimates, robust standard errors in parentheses. All observations are weighted by the fraction of respondents whose language can be classified as weak or strong FTR. The regressions exclude Haiti for which no respondent could be classified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

interview languages vary across respondents such that we observe within-country variation in FTR. Thus, we proceed by regressing individual-level preferences on a dummy for whether a respondent speaks a weak or a strong FTR language, conditional on country fixed effects and age, age squared, gender, and our cognitive skills proxy. Columns (1), (3), (5), (7), (9), and (11) of Table 4.2 present the results. Consistent with the cross-country evidence, we find that individuals speaking weak FTR languages are more patient, more positively reciprocal, and more trusting. In addition, these people are also significantly more altruistic.[14] We do not find significant relationships between FTR and risk taking or negative reciprocity. For each preference, a second column adds further controls, i.e., regional (state or province) fixed effects, religion fixed effects, household income, health, and subjective institutional quality. Despite this comprehensive set of covariates, and only exploiting within-region variation in FTR and preferences, we obtain almost identical results.

In sum, the results at the subnational level closely mirror those obtained in cross-country analyses, the one exception being altruism.[15] Thus, across levels of aggregation, weak FTR is predictive of higher patience and higher levels of the prosocial traits positive reciprocity, altruism, and trust.

4.5 Discussion and Conclusion

Using a specific linguistic feature as a proxy for cultural heritage, we examine whether the variation in economic preferences across countries and across individuals has a cultural component. In line with our hypotheses we find an association between the FTR criterion of languages and time discounting, positive reciprocity, and trust. Speakers of weak FTR languages are more future oriented than speakers of strong FTR languages in that they are more patient and more positively reciprocal. They also exhibit higher levels of trust.

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[14]When we restrict the sample to those countries with within-country variation in FTR and regress the respective preference only on the FTR indicator as well as country fixed effects, the resulting coefficient is always positive and statistically significant at the 10% level for patience and at the 1% level for positive reciprocity, trust, and altruism. In Appendix 4.6, we report the coefficient on FTR separately for each country in which we observe within-country variation.

[15]Note that the correspondence between within- and across-country results is in no way mechanical, i.e., it need not necessarily be the case that individual- and country-level correlations are aligned.

Table 4.2: Preferences and FTR: Individual-level results

	Patience		Risk taking		Dependent variable: Pos. reciprocity		Neg. reciprocity		Altruism		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1 if weak FTR	0.095**	0.053***	0.067	0.0079	0.18***	0.11***	-0.073*	-0.0037	0.24***	0.19**	0.33***	0.32**
	(0.05)	(0.02)	(0.04)	(0.07)	(0.04)	(0.04)	(0.04)	(0.09)	(0.04)	(0.09)	(0.04)	(0.13)
Age	0.76***	0.80***	-0.098	0.49**	1.07***	0.92***	-0.39***	-0.25	0.032	0.074	0.42***	0.045
	(0.10)	(0.18)	(0.10)	(0.21)	(0.10)	(0.17)	(0.10)	(0.20)	(0.10)	(0.16)	(0.10)	(0.16)
Age squared	-1.51***	-1.48***	-1.21***	-1.80***	-1.22***	-1.09***	-0.44***	-0.63***	-0.017	-0.21	-0.0044	0.27
	(0.11)	(0.21)	(0.11)	(0.21)	(0.11)	(0.18)	(0.11)	(0.20)	(0.11)	(0.17)	(0.11)	(0.16)
1 if female	-0.061***	-0.040***	-0.18***	-0.17***	0.045***	0.054***	-0.13***	-0.13***	0.100***	0.093***	0.069***	0.053***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Subj. math skills	0.028***	0.023***	0.045***	0.040***	0.039***	0.039***	0.039***	0.036***	0.044***	0.040***	0.056***	0.056***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Log [Household income p/c]		0.037***		0.052***		0.049***		0.012		0.046***		-0.0083
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Subj. health index		0.10***		0.14***		0.062***		-0.028		0.084***		0.053***
		(0.02)		(0.03)		(0.02)		(0.03)		(0.02)		(0.02)
Subj. law and order index		0.065***		0.054**		0.0041		-0.075***		0.022		0.19***
		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)
Constant	-0.49***	-0.88***	0.15***	-0.79***	-0.13**	-0.51***	0.60***	0.16	-0.17***	-0.40***	-0.46***	-0.74***
	(0.06)	(0.08)	(0.06)	(0.10)	(0.06)	(0.09)	(0.07)	(0.13)	(0.06)	(0.13)	(0.06)	(0.16)
Country FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Region FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Religion FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	73460	52628	73414	52610	73811	52862	72501	52003	73580	52675	72811	52159
R^2	0.166	0.215	0.172	0.254	0.127	0.230	0.117	0.200	0.137	0.199	0.113	0.167

OLS estimates, robust standard errors (clustered at country level) in parentheses. For the purposes of this table, age is divided by 100. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Unlike what we hypothesized, we do not find a correlation between the FTR criterion and negative reciprocity. We can only speculate about why this is the case. One potential explanation is that negative reciprocity does not only capture a strategic (future-oriented) aspect of punishment with the goal of, e.g., norm enforcement, but also a more emotional and non-strategic aspect of "getting even" or taking revenge when treated unfairly. This rather impulsive or affective motivation does not necessarily bear rational considerations about potential future payoffs. This would render finding an association with the FTR criterion less likely.

For altruism, our results at the country level are in line with our hypothesis as we do not find a relation between the countries' average level of altruism and their fraction of weak FTR languages speaking inhabitants. However, at the individual level we do find an association: speakers of weak FTR languages are more altruistic than speakers of strong FTR languages. This might not so much result from a direct association between altruism and the FTR criterion, but rather mirror the strong association between positive reciprocity and trust on the one hand and altruism on the other, reflecting pro-sociality in its different facets.

In line with the work by Chen (2013), our results lend themselves to two interesting interpretations. First, speaking a weak FTR language may actually *cause* patience and cooperation-enhancing pro-sociality.[16] Second, the historical evolution of linguistic features and the formation preferences may both be a product of some other very deep cultural trait. Regardless of the precise interpretation adopted, our results highlight that the contemporary preference variation may have very deep historical roots,[17] and that the GPS data are well-suited to identify such effects.

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[16]The idea that language might influence our thought, the way we perceive the world and ultimately our behavior and decision making has been brought up a quite some time ago and has become well-known as the Sapir-Whorf-hypothesis (Whorf, 1956). Among the evidence arguing in favor of such a hypothesis is, for example, Fausey and Boroditsky (2011) who find cross-linguistic differences in eye-witness memory.

[17]As discussed by Chen (2013), variation in future-time reference is at least several hundred years old.

4.6 Appendix to Chapter 4

4.6.1 Individual-Level Regressions Separately by Country

Table 4.3: Preferences and FTR: Within-country results

Country	Weak FTR	Strong FTR	Patience	Pos. reciprocity	Trust	Altruism
Estonia	Estonian	Russian	0.05	0.13*	0.38***	0.45***
Nigeria	Yoruba	English, Hausa, Igbo	-0.08	0.54***	0.63***	-0.11
Switzerland	German	French, Italian	0.17**	0.14**	0.28***	0.30***

OLS estimates, robust standard errors. The regressions report the coefficient on FTR in univariate regressions for each country in which we observe within-country variation in FTR. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.6.2 Country-Level Regressions: Robustness

While the main text reported WLS estimates, Table 4.4 reports OLS estimates.

Table 4.4: Preferences and FTR: Cross-country results

	Patience		Risk taking		Pos. reciprocity		Neg. reciprocity		Altruism		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Fraction of population speaking weak FTR	0.36*** (0.13)	0.23** (0.09)	-0.11 (0.07)	0.015 (0.07)	0.15* (0.07)	0.17** (0.08)	-0.018 (0.07)	-0.082 (0.09)	0.061 (0.09)	0.11 (0.09)	0.19** (0.08)	0.19** (0.07)
Log [GDP p/c PPP]		0.16*** (0.04)		0.032 (0.03)		-0.073* (0.04)		0.052 (0.04)		-0.077* (0.04)		-0.0055 (0.03)
Distance to equator		0.010* (0.01)		0.0015 (0.00)		-0.0078 (0.01)		-0.0054 (0.01)		-0.0033 (0.01)		-0.0057 (0.00)
Longitude		-0.0018 (0.00)		0.0024 (0.00)		0.0021 (0.00)		0.00043 (0.00)		0.0028 (0.00)		0.000065 (0.00)
% at risk of malaria		0.25 (0.19)		-0.15 (0.24)		-0.33 (0.28)		-0.089 (0.17)		-0.72*** (0.26)		-0.092 (0.19)
Average precipitation		-0.00013 (0.00)		-0.00081 (0.00)		0.00055 (0.00)		-0.0010 (0.00)		0.0031*** (0.00)		-0.0011 (0.00)
Constant	-0.067 (0.04)	-1.42** (0.56)	0.034 (0.04)	-0.51 (0.45)	-0.053 (0.05)	0.56 (0.67)	0.014 (0.04)	-0.019 (0.50)	-0.043 (0.05)	0.25 (0.61)	-0.058 (0.04)	0.39 (0.48)
Continent FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	75	74	75	74	75	74	75	74	75	74	75	74
R^2	0.141	0.641	0.021	0.381	0.029	0.280	0.001	0.246	0.005	0.356	0.072	0.420

OLS estimates, robust standard errors in parentheses. The regressions exclude Haiti for which no respondent could be classified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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