Empirical Studies of Individual Behavior: Cheating, Corruption, and Insurance Choice

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Preface

When Vernon Smith and Daniel Kahneman, two pre-eminent figures of the "experimental revolution in economics", were awarded the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel* in 2002, the field of behavioral and experimental economics had finally received the highest recognition within the economics profession. The former of the two was honored "for having established laboratory experiments as a tool in empirical economic analysis" and the latter "for having integrated insights from psychological research into economic science, especially concerning human judgment and decision-making under uncertainty." Building upon the methodological developments of Vernon Smith and the psychological insights of Daniel Kahneman and the late Amos Tversky is at the heart of this doctoral dissertation in economics.

Conducting a laboratory experiment can have a variety of objectives. In one of the first monographs entirely devoted to experimental economics, Davis and Holt (1992) categorized them as (i) tests of behavioral hypotheses, (ii) theory stress tests, and (iii) searching for empirical regularities. The main advantage of experiments over real world data is that potentially confounding factors can be isolated and eliminated or controlled for. However, there are situations where experiments are not only a cleaner way of obtaining relevant data, but maybe even the *only* viable way to investigate a specific hypothesis. One example is research on behaviors which are generally considered morally objectionable or violate formal or informal rules. Observing them in the field is difficult for the very reason that people wouldn't behave in this way if they knew that they were being observed. The anonymity of the laboratory greatly reduces reputation or punishment concerns due to being caught doing something morally wrong.

The research questions which motivated the first three chapters fall into the category of behavioral regularities which would be difficult to observe directly in real-world environments. These chapters study violations of social norms, such as lying, cheating, and opportunism and corruption. In contrast, the main focus of the fourth chapter is on risk aversion, which is a morally neutral expression of individual preferences. People can be either more or less afraid of taking risks, but in most situations there is no incentive for concealing it. The reason why an experimental approach is useful in this context is that most people would not be able to give an objective and unbiased assessment of their own aversion to risk which would be interpersonally comparable. This is what we can obtain with experimental measures for risk attitudes.

In the first chapter, which is joint work with Daniel Houser and Joachim Winter, I investigate whether people lie and cheat more often when they have the impression that others do not treat them fairly. Understanding why people behave dishonestly is an important question given the widespread occurrence of cheating and fraud in various aspects of our daily lives. Related real world situations to our laboratory setting are for example employee theft, free-riding on public transport, tax evasion, and many more. However, even if such acts of cheating are discovered there is no information about the role of perceived unfairness. If we are interested in uncovering a causal link between the (un)fair behavior of others and an individual's propensity to cheat, economic experiments are a promising alternative.

As lying and cheating violate one of the arguably strongest social norms, namely the imperative of being honest, we were aware that traditional approaches would entail the drawback that subjects feel observed by the experimenter. This may not be a major reason for concern in most experiments, but a design which investigates why people break a social norm so central to our societies that more severe violations are even legally punished led us to look for a new approach.

To ensure that subjects could cheat without being observed – and thus without the threat of being held accountable – we implemented a design in which not even the experimenter can observe cheating directly. Subjects could flip a coin in private and report the result either truthfully or untruthfully. If they claimed to have flipped tails, whether true or not, they received an additional monetary reward. We implemented different perceptions of fairness across subjects by pairing them in a dictator game. The idea is that a receiver who was paired with a selfish dictator presumably experienced much stronger feelings of unfairness than another receiver who was paired with a dictator who split the endowment equally.

Our results show that the perception of how others adhere to the norm of being fair affects the decision to cheat. To exclude other possible explanations for our data (e.g. income targeting or a desire to reduce peer inequality among receivers) we conducted a “no intentions” treatment in which we implemented the same degree of inequality as in the dictator game, but assigned earnings randomly. As the cheating rate in this treatment without unfair dictator intentions is not related to earnings, we are confident that our main treatment really measures the effect of fairness on an individual’s inclination to cheat.

The second chapter presents a related study on lying behavior. This time the research question is whether people are more dishonest when they can hide behind a veil of ignorance about the consequences of their lies. To this aim, I conducted an experiment in which subjects interacted in pairs and one player (the sender) had to send a message to the other player (the receiver). The sender could either lie or tell the truth, and by successfully deceiving his counterpart he could increase his earnings from the experiment. This setup, introduced by Gneezy (2005) and Sutter (2009), allows us to evaluate an individual’s inclination to engage in deception. In this case, the link between a lie and a bad outcome for the other person is clear. In a second step, I conducted a responsibility treatment in which there were two senders for each receiver, but only one of their messages was transmitted to the receiver. As none of the senders would later know whether it was their own dishonest action or the lie of the other sender which led to a detrimental outcome for the receiver, the moral costs of lying might be lower. A sender might justify his lie vis-à-vis herself with the possibility that the other sender could still tell the truth. What we find, however, is that people do not lie more frequently when they remain ignorant about the consequences of their truth-telling behavior.

This result is in contrast to the *multiple dictator treatment* of Dana et al. (2007) who find that people are strikingly less generous in a dictator game when there is the possibility that another dictator’s choice may decide the amount to be shared. However, keeping in mind that lying is generally considered far more objectionable than not sharing equally, people might evaluate a violation of the honesty norm differently than a violation of the fairness/sharing norm. Instead of focusing on the consequences, as argued by Gneezy (2005), they might be more concerned about the act of lying per se.

The third chapter deals with opportunism and corruption, which is another issue where experiments are particularly useful because there is often no viable way of obtaining objective data for the question of interest. The enormous political relevance of this topic, even in developed countries with generally well-functioning legal systems and a high rule of law, is underscored for example by the fact that the Organization for Economic Co-Operation and Development (OECD) has initiated an *Anti-Bribery Convention* and runs a special *Working Group on Bribery*.

Opportunities for corruption can occur in many different scenarios. We focus on situations in which a decision-maker is supposed to act for the benefit of the public, but where there are various parties which pursue their own interests in the context of this decision. Such a situation can be found for example when politicians or public officials have to make decisions about procurement contracts, or when politicians are deciding about legislative changes which benefit one lobbying party at the expense of another (e.g. an industry-friendly vs. a consumer-friendly legislation). In such cases, a bidder in a procurement auction or a political lobby group would have a material interest in bribing decision-makers to ensure that their preferred outcome will be achieved. Vice versa, a corruptible decision-maker may be willing to exchange a favorable decision against a (usually monetary) reward.

These activities are obviously illegal and, if discovered, may entail a severe punishment for either side. Therefore, they are carried out in a clandestine way and hardly anyone would readily admit to be involved in a bribery relationship. The fundamental problem for an economic analysis is therefore the lack of credible data, but experiments can offer a way out. Early corruption experiments, e.g. Abbink et al. (2002), have shown that when situations with the possibility of bribing a decision-maker are replicated in the laboratory, subjects respond to the same incentives as the parties involved in corruption deals in the real world. This suggests that we can use the laboratory to investigate corrupt behaviors and to make causal statements about the motives for corruption.

Bribery occurs because it allows both the decision-maker and the lobbying party to reap benefits, usually to the detriment of the general public. However, it also has an important and obvious drawback for the partners-in-crime – it is illegal. In countries where the rule of law is very weak or an authoritarian regime protects its henchmen, this may not be much reason for concern. In most Western democracies, however, the punishment for being corrupt can be heavy, and even the shadow of a doubt can effectively terminate a political career. It would thus not be surprising if potential bribers and bribees were interesting in finding alternative channels for exchanging favorable decisions for money.

In the classical corruption scenario, the potential briber offers money in exchange for a political favor. However, if the order of moves is reversed, it is much easier to obscure the link between the favor and the reward. In other words, a decision-maker may do a favor pro-actively and anticipate that he will be rewarded for it in the future. After all, there are numerous ways of transferring money to former political decision-makers, such as making them members of the company's management, hiring them for mandates, inviting them for speeches, and many more. What all these cases have in common is that there are perfectly legitimate reasons why a firm would want to do this (e.g. benefitting from the knowledge, experience or the contacts of the decision-maker), which are difficult to disentangle from the motive of rewarding a previous favor. It would thus be almost impossible to prove that a monetary payment was made out of gratitude for an unethical act. However, this strategy also has a crucial drawback – the future reward is not contractible. Whether such an arrangement would actually work is therefore a-priori unclear.

The results show that such an implicit collusion can actually work. When subjects know that there is a possibility of being rewarded for previous decisions, they clearly favor the party that has more to gain and is thus more likely to reward. The favored party often reciprocates and makes a voluntary financial transfer to the decision-maker. In this way, the two partners in the gift vs. reward exchange can increase their earnings at the expense of the discriminated party.

For the final chapter we leave the confines of an experimental laboratory and demonstrate how experimental methods can serve as valuable tools for policy evaluation outside the lab. In the early years of experimental economics, there had been skepticism about the usefulness of experiments for evaluating policy issues for reasons such as the specificity of the subject pool and the particular environment in a laboratory. However, there is no reason for restricting ourselves to experiments with students, and for many policy questions it would be highly desirable to supplement information contained in surveys with experimental measures of individual behavior. Together with Florian Heiss, Daniel McFadden and Joachim Winter, I investigate the choice behavior of senior US citizens during the introduction phase of the new Medicare Part D program in 2006, using experimental measures of risk aversion and the prospect theory framework of Kahneman and Tversky.

Medicare Part D has often been referred to as the largest expansion in social security entitlement programs since the "Great Society" agenda of Lyndon B. Johnson in the mid-sixties. It offers highly subsidized coverage for prescription drug costs, especially directed to those suffering from chronic conditions. This reform was very costly and has thus been politically contested since the very beginning. It has also led to a vast body of research about the behavioral responses of the eligible population to the introduction of Part D. Most existing surveys, e.g. the Health and Retirement Study, extended their questionnaires to incorporate additional modules related to the policy reform. The importance of this legislative change even initiated an entirely new survey, the *Retirement Perspectives Survey*, which we used for our analysis.

Part D has the virtue of preserving the freedom to choose whether to opt in or remain without coverage. However, the default option was non-enrollment, which creates the concern that not all individuals may use their freedom to their own advantage. Especially very old, cognitively impaired or socially excluded individuals might fail to realize which benefits they would reap from this new program. Understanding who the people that did not enroll are, and which motives they had for not enrolling, remains an ongoing discussion and is vital for evaluating the workings of Part D.

At least in terms of enrollment numbers, Medicare Part D has generally been regarded as a major success. According to the *Centers of Medicare and Medicaid Services* there were almost 24 million beneficiaries who obtained Part D coverage during the first year until the beginning of 2007. This number further increased to more than 31 million until 2011. Most of these enrollees had no equivalent coverage for pharmaceutical drugs before Part D. However, more than 4 million Medicare beneficiaries lacked prescription drug coverage one year after the introduction of Part D, and this number has remained more or less constant since then. Richard Thaler and Cass Sunstein express their opinion about this issue in their recent book *Nudge*: "Many people are still not enrolled in this program, even though it is clear that they should be. Four million uncovered Americans is a large number, and studies suggest that this group is probably dominated by poorly educated people living just above the poverty line […]." However, this may not be the full story. While several empirical studies have investigated the determinants of (non-)enrollment into a Part D plan, none of them has controlled for a key ingredient of insurance choice: risk aversion. Standard insurance models predict that a risk averse individual will always opt for insurance when it is actuarially fair, and will generally have more insurance coverage than a risk seeking individual. Our paper attempts to close this gap and assess the importance of this possible link.

With experimental measures of risk aversion in situations involving gains and losses, respectively, we conclude that risk tolerance in the loss domain is highly significantly related to non-enrollment in Medicare Part D. Perhaps surprisingly, the role of risk aversion seems to be more important than various observable indicators for socio-economic status. This does not refute the claim that poor or uneducated seniors are overrepresented among those who remain outside of the Medicare Part D framework despite the substantial financial benefits. What it does suggest, however, is that the problem could be less severe than what has sometimes been argued. Focusing exclusively on the number of eligible yet uncovered persons when assessing the success of Part D will ignore the role of voluntary abstention due to a relatively high degree of risk tolerance. The final chapter shows that this motive plays an important role to understand non-enrollment.

1 Fairness and Cheating

1.1 Introduction

Interestingly, not all people who can improve their situation by lying will actually do so. Indeed, the rapidly growing literature on cheating (e.g. Gneezy, 2005; Hurkens and Kartig 2009; Sutter, 2009) reveals a robust finding that a substantial proportion of people prefer not to cheat. This preference for honesty endures even in situations when cheating cannot be detected by the experimenter (Fischbacher and Heusi, 2008). We focus on perceptions of fairness as a possible explanation for this behavior.

We ask whether individuals who feel they have been treated fairly in an interaction with others are less likely to subsequently cheat than those who believe they have been treated unfairly. This question is motivated by empirical evidence suggesting that fairness perceptions can affect honesty. Greenberg (1990), for example, showed that when employees were confronted with a pay cut, theft was significantly higher among the employees who were most likely to perceive this measure as unfair. While employee theft involves reciprocity, our goal is to investigate this link in a setting where reciprocity and other confounding motives can be controlled. This chapter discusses the laboratory experiments we designed to address this issue and the results we obtained.

In our experiment, cheating occurs when one misreports the outcome of a coin flip to receive a greater payoff. While we did not observe individually whether a particular subject cheated, we could observe the distribution of outcomes for sub-samples of subjects who were either treated fairly or unfairly. Statistical inference based on the self-reported outcome of a random event has been proposed by Warner (1965) as a way of eliminating answer bias in survey questions which are of sensitive nature and therefore likely to result in a high proportion of untruthful responses. Self-reports of random outcomes have been used by Batson et al. (1997) in a psychological experiment, and more recently by Fischbacher and Heusi (2008) as well as Bucciol and Piovesan (2011) in the economics context. While Fischbacher and Heusi studied the outcome of a die roll, Batson et al. and Bucciol and Piovesan also used a coin flip. Our study adds to this experimental device a first stage – the dictator game – to manipulate the objective experience of unequal treatment and perceptions of fairness. We further extend existing studies by proposing a simple, nonparametric estimator of the fraction of cheaters in the underlying population.

Outside the lab, we rarely encounter situations where people cheat openly. After all, being perceived as dishonest is generally considered objectionable (see, e.g., Hao and Houser, 2011). Moreover, depending on the situation, dishonest actions might lead one to be punished. Therefore, cheating mostly occurs when the offender considers the chance to remain undetected sufficiently large. By using the self-reported outcome of a random event we thus intended to create a more natural environment to study dishonest behavior than in experiments in which the researcher can observe cheating directly.

That experimenter blindness makes a difference has been shown, for example, by Hoffman et al. (1994) for the ultimatum game and by Hoffman et al. (1996) for the dictator game. These experiments varied the degree of anonymity and demonstrated that when subjects knew that the experimenter could not observe individually how much they allocated to another player, they behaved by far less pro-social and retained a much larger share for themselves. Given that this “double-blind” vs. “single-blind” effect is so pronounced in games which involve “fairness” we expect it to be important also in a situation which involves an arguably even stronger social norm, namely the norm of being honesty.

In our “fairness” treatment, we investigate whether the amount subjects receive in a dictator game affects the probability that they will cheat in a subsequent task. We consider both an objective measure of fairness (the amount a subject has received) and a subjective measure (the receiver’s assessment of the proposer’s transfer elicited after the dictator game). To ensure that “unfair” dictator decisions are salient, we ask receivers for their fairness assessment.[1]

In order to isolate fairness as an explanation, we implement a “no intentions” treatment where receiver earnings are determined by a random mechanism rather than an intentional dictator decision. Additionally, to keep perceptions of unfairness as low as possible, we do not ask subjects for their fairness opinion. This treatment allows us to assess the importance of other possible cheating motives, such as income-targeting or reducing peer inequality.

We found that in the “fairness” treatment, 74.5% of our subjects reported the better coin flip outcome, which is highly significantly different from the expected proportion of 50%. Assuming that subjects with the better outcome reported honestly, this suggests that 49% of those who flipped the inferior outcome cheated. What we observe is that cheating rates were drastically higher among receivers who earned nothing or who reported feeling like they were treated unfairly. In contrast, we do not observe a significant difference in cheating rates across earnings in the “no intentions” treatment, where earnings were determined randomly. Our results strongly support the idea that experiencing unfair treatment does indeed induce subjects to cheat.

The remainder of this chapter is organized as follows. Section 1.2 reviews related literature. Section 1.3 describes the experimental design of the “fairness” treatment, and section 1.4 presents the results. Section 1.5 reports design and results for the “no intentions” treatment. Section 1.6 concludes with a discussion of our findings.

[1] While drawing inferences about social preferences from proposer decisions in the dictator game has been criticized for neglecting the influence of contextual issues or the proposers’ action set, for example by List (2007) and Oechssler (2010), our focus is on the receivers’ behavior. The purpose of the dictator game in our experiment is to induce different perceptions of fairness among receivers.

1.2 Related Literature

In economics, cheating has been studied in the context of cheap-talk games (e.g. Sutter (2009); Charness and Dufwenberg (2006); Gneezy (2005)), where subjects send messages that can be used to deceive their counterparts. Experimental evidence on unobserved cheating has only recently begun to accumulate. In the experimental tournament of Freeman and Gelber (2010), subjects cheated by misreporting the number of correctly solved mazes. The misreporting varied according to monetary incentives. Bucchiol and Piovesan (2011) conducted an experiment in which children tossed a coin in private and reported the result, knowing that they would receive a reward only if they reported one of the two outcomes. 86% of the children reported the profitable outcome, suggesting a substantial proportion of them cheated. In a field study, Pruckner and Sausgruber (2011) observed that 2/3 of newspaper readers took advantage of an opportunity to take a newspaper without paying. Moreover, 90 percent of those who did pay actually paid less than the full price. This sort of “incomplete cheating” also occurred in an experiment conducted by Fischbacher and Heusi (2008), wherein subjects reported the result of a private die roll to determine their payoff. Fischbacher and Heusi found that subjects shaded the outcome of the roll favorably, but did not take the maximum earnings advantage offered by the lying opportunity. Note that a possible explanation for incomplete cheating is an individual’s desire to preserve a favorable self-concept (Mazar et al., 2008).

Researchers have also become increasingly interested in the cognitive and neuronal processes involved in (dis)honest decision-making. Using functional magnetic resonance imaging, Greene and Paxton (2009) showed that honest individuals do not exhibit significantly more activity in brain regions associated with response conflict and cognitive control when foregoing an opportunity for dishonest gain. They concluded that individuals who made honest decisions were not, for the most part, actively resisting the temptation to cheat, but were simply not tempted. Wang et al. (2010) used video-based eye tracking to infer the individual degree of level-k reasoning from a sender’s lookup pattern of payoffs for alternative choices presented on a screen, and showed that this information could be profitably exploited by receivers.

To the best of our knowledge, the only paper to examine how perceived unfairness undermines honesty is Greenberg (1990), which reported a field experiment in which a company subjected its workers to a temporary pay-cut due to the loss of a large contract. The experimental variation was the way in which the company communicated this measure across plants. Workers at one production site perceived the pay-cut to be unfair, while at the other site it was carefully explained so as not to evoke this feeling. Greenberg showed that the workers in the “unfair” condition responded with a substantial increase in employee theft that was not observed in the “fair” condition. This finding is consistent with Fehr et al. (1993) who showed in a laboratory experiment that the degree of co-operation of “workers” declined when they perceived the “employer’s” wage offers as unfair. It is also in line with the results of Schweizer and Gibson (2008) where subjects indicated in a questionnaire that they would be more likely to engage in unethical behaviors when they perceived their counterpart to violate fairness standards. While cheating in Greenberg’s experiment occurred at the expense of the party responsible for the “unfair” decision, we investigate whether the perception of unfair treatment erodes honesty even when there is no scope for reciprocity.

1.3 Design of the Experiment

The experiment was conducted in MELESSA, the experimental laboratory at the University of Munich. Participants were recruited from the lab’s subject database, which is maintained using software written by Greiner (2004). A total of 502 individuals participated in one of 21 sessions.[2] Most subjects were undergraduates from the University of Munich. Average earnings were €10.5, including a €4-show-up fee, for an average duration of around 35 minutes. The experiment was implemented in paper and pencil format and consisted of two stages: a decision situation (the dictator game) and the coin flip game. Subjects were not aware of the second stage until the dictator game was resolved. While the dictator game took around 25 to 30 minutes, the coin flip stage lasted no more than 5 minutes.

The dictator game was conducted using the strategy method. At first, subjects did not know whether they would be in the role of proposer or receiver. We asked them to decide how they would split an initial endowment of €8 between themselves and another person in the room if it turned out that they were in the role of the proposer. The intended transfer had to be marked on the decision sheet and could range from €0 to €8 in increments of €2. Subjects knew that after they made their decision, half of them would be randomly selected as proposers, and their choice would be implemented. The remaining half would be passive receivers, whose earnings would be determined solely by the decision of the matched proposer. A short quiz

[2] 20 sessions comprised of 24 subjects, one session of 22 subjects.

ensured that all subjects understood the rules of the dictator game, as well as the experimental procedure.

The randomization of roles and pairs was implemented by means of 24 identical envelopes. Each envelope contained a tag with a code consisting of a letter (from A through L) and a number (either 1 or 2). Thus, a tag might read A1 or A2, B1 or B2, C1 or C2, and so on. The number indicated the role in the game (1 for the proposer, 2 for the receiver) and the letter matched each participant with another person in the room. Those with an identical letter formed a pair, with Player 1 as proposer and Player 2 as receiver. After the randomization procedure, the sheets were collected and matched with the decision sheet of the respective counterpart, and the associated payoffs were recorded. Then subjects were informed of their earnings.

Not computerizing the experiment allowed us to provide receivers with their matched counterpart's decision sheet. As a result, receivers could read directly how much the proposer had transferred. They could also see the proposer's tag number, which was written on the sheet as well. This procedure guaranteed full anonymity while allowing receivers to verify that the matching procedure was performed correctly and not manipulated by the experimenter.

Receivers were also given a sheet on which they had to indicate, using a four-point scale, how fair they considered the behavior of the other person. They could rate the matched proposer as "fair," "rather fair," "rather unfair," or "unfair." In deliberately excluding a neutral category, we sought to avoid having subjects choose this category without further reflection. Proposers received a confirmation that their decision was actually payoff relevant, and an additional blank sheet to ensure that every subject received the same number of sheets, which made it impossible to figure out the roles of other subjects in the lab by counting the number of sheets.

After the dictator game was resolved, subjects were informed that they would get a chance to increase their earnings before the session closed. While the experimenter explained the rules of the coin flip game, an assistant provided each participant with a €1 coin. Subjects were told that they would have to flip the coin and that their payoff would be determined by the upper side of the coin. If the coin landed on heads, their payoff would be €1 (i.e., they could keep the coin); if the coin landed on tails, their payoff would be €3. Then subjects were instructed to flip the coin and to report the outcome by checking the appropriate box on the sheet.

Finally, the earnings from the coin flip and the dictator game were paid out and the experiment concluded.

We did not want to invite people to cheat. Indeed, we never mentioned the possibility. We also did not remind them to be honest. Nevertheless, the environment was quite simple, and it likely occurred to most subjects that cheating was a riskless way to earn two additional Euros.

It is important to emphasize that cheating was understood by the subjects to be riskless. Indeed, all seats in the laboratory were separated and view-protected; it was clear that cheating could not be detected. Furthermore, we explicitly instructed subjects to flip their coin in such a way that no one else could observe the outcome.

1.4 Results

1.4.1 Dictator Game and Reported Fairness Perceptions

We begin with the dictator game. Table 1.1 shows the distribution of those transfers that were actually implemented after random assignment of roles (the allocation decisions of assigned proposers) and the hypothetical transfers of assigned receivers (recall that we used the strategy method). A chi-2 test confirms that both distributions are not significantly different (p-value = 0.942). On average, €1.90 was transferred, meaning that proposers retained about 76% for themselves, which is within the usual range of outcomes in dictator games.[3]

Table 1.1: Hypothetical and Actual Transfers in the Dictator Game

	Proposers: actual transfers		Receivers: hypothetical transfers	
Amount	Frequency	Percentage	Frequency	Percentage
0	96	38.25	122	48.61
2	75	29.88	53	21.12
4	77	30.68	68	27.09
6	2	0.80	2	0.80
8	1	0.40	6	2.39
Total	251	100.00	251	100.00

[3] For an overview of dictator game results see e.g. chapter 2 of Camerer (2003).

We use the amount transferred as an indicator of how fair the receiver was treated. Evidence from a large number of ultimatum games presented in Camerer (2003) demonstrates that on average one-third of the receivers reject offers between 20% and 30% of the endowment, indicating that a substantial proportion of subjects perceives such a division as unfair. Camerer also shows that the rejection rate of offers between 0% and 10% is usually far above 50%, and in many experiments close or equal to 100%. We therefore interpret proposer decisions with a transfer below an equal split – €0 or €2 – as potentially unfair.

Table 1.2 shows the distribution of receivers' subjective fairness ratings. Almost 44% of receivers perceived that they had been treated "fairly." The response options "rather fair", "rather unfair" and "unfair" were all chosen by less than one fifth of our sample. In Table 1.3, we combine the responses "fair" and "rather fair" (62.55%), as well as "unfair" and "rather unfair" (37.45%), in a cross-tabulation with the amount received (also combined into three categories of about equal size). There is a statistically significant association ($\chi^2 = 97.01$, p-value < 0.001) between the two variables; subjects who received a larger amount were more likely to say that they had been treated (rather) fairly. In no case was an equal split rated as "unfair" or "rather unfair."

Table 1.2: Subjective Perception of the Fairness of Transfers Made (Receivers)

Perceived Fairness	Frequency	Percentage
Fair	110	43.82
Rather Fair	47	18.73
Rather Unfair	45	17.93
Unfair	49	19.52
Total	251	100.00

Interestingly, Table 1.3 also reveals that 28.13% of respondents regard a transfer of zero as (rather) fair. However, this claim was only made by respondents who would have acted similarly if they had been a proposer; 25 out of 27 would have transferred €0 and the remaining 2 persons would have transferred only €2. A possible interpretation is that their fairness rating reflects their desire to ex-post justify their own selfish behavior rather than their true opinion about the proposer's action.

Table 1.3: Transfer Received and Subjective Fairness Rating (Receivers)

Perceived Fairness	Amount received			Total
	0	2	≥ 4	
(Rather) Unfair	69 (71.88%)	25 (33.33%)	0 (0.00%)	94 (37.45%)
(Rather) Fair	27 (28.13%)	50 (66.67%)	80 (100.00%)	157 (62.55%)
Total	96	75	80	251 (100.00)

1.4.2 Reported Coin Flip Outcomes

Next, we turn to the outcome of the coin flip. 374 of the 502 subjects (74.5%) reported the high-payoff outcome (tails), which is significantly different from the expected outcome of a fair coin (p-value < 0.001). This finding shows that cheating occurred on a broad basis. The proportion of tails is higher among receivers (76.49%) than among dictators (72.51%). Among receivers, those who earned a positive amount in the dictator game were significantly less likely to report tails than those receivers who earned nothing (p-value = 0.088). However, the proportion of tails reported is a highly conservative estimate of cheating in the population since the majority (asymptotically 50%) obtained this result without resorting to cheating.

Therefore, we can go one step further and estimate the fraction of cheaters in the population from which our sample of subjects was drawn. The identification problem can be described by a simple mixture model with two types: cheaters and non-cheaters. With fair coins and a large sample, half of the subjects should have flipped tails, and the other half heads. The fifty percent of subjects who flipped tails (the high-payoff outcome) had no monetary incentive to lie; thus, it seems reasonable to assume that they reported the true outcome.[4] However, among the other half who flipped heads (the low-payoff outcome), some might have lied.

From the observed sample proportion of tails, we can estimate the implied proportion of cheaters in the population nonparametrically. The mixture model assumes (quite naturally) that the population from which the subjects were drawn consists of two types: cheaters and honest subjects. We can characterize the observed proportion of tails p in the population by

[4] We cannot, in principle, exclude the possibility that some subjects reported the bad outcome despite obtaining the good outcome, perhaps to make certain that nobody suspected them of cheating, or because they exhibited altruism towards the experimenter. However, we believe it is reasonable to assume, in the context of our laboratory experiment, that such behavior did not occur.

$$p = \gamma \cdot 1 + (1 - \gamma) \cdot 0.5 = 0.5(1 + \gamma).$$

A fraction γ of the members of the population cheats and reports tails with a probability of 1, while honest members of the population report tails only with probability 0.5. We will call γ the cheating rate. After solving for γ, we can use the sample analog of *p* (i.e., the observed fraction of tails) to obtain a consistent estimate of the population cheating rate γ as 2·(74.5% – 50%) = 49%. By the same logic, cheating rates can be computed for subsamples of our subjects. Figure 1.1 illustrates that the proportion of tails in a (sub)population maps directly into the implied fraction of cheaters via a linear function.

Figure 1.1: Relationship Between Proportion of Tails and Implied Fraction of Cheaters

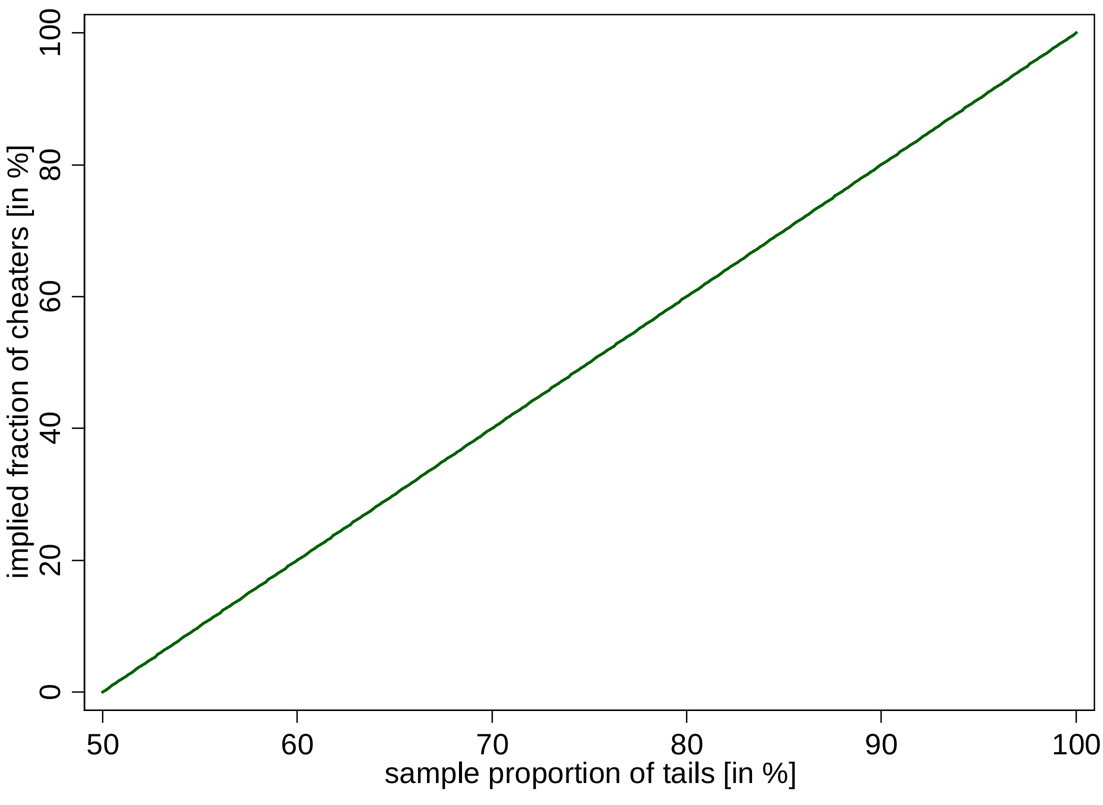

Table 1.4 contains information about the proportion of subjects who reported tails, and a comparison of cheating rates by experimental outcomes and demographic background variables. The proportion of tails is significantly different from 50% for all sub-groups.

Table 1.4: Comparison of Cheating Rates

variable	values	N	% tails	implied % of cheaters	p-value from test for equality of proportions
All subjects		502	74.50	49.00	
Role	Proposer	251	72.51	45.02	0.075*
	Receiver	251	76.49	52.98	
Earnings	≤ 4	80	68.75	37.50	≤4 vs. 6: 0.137
(Proposers)	6	75	74.67	49.34	6 vs. 8: 0.940
	8	96	73.96	47.92	≤4 vs. >4: 0.101
Earnings	0	96	82.29	64.58	0 vs. 2: 0.003***
(Receivers)	2	75	70.67	41.34	2 vs. ≥4: 0.280
	≥ 4	80	75.00	50.00	0 vs. >0: 0.004***
Hypothetical	0	122	78.69	57.38	0 vs. 2: 0.890
Transfer	2	53	79.25	58.50	2 vs. ≥4: 0.067*
(Receivers)	≥ 4	76	71.05	42.10	0 vs. >0: 0.176
Fairness rating	(rather) fair	157	73.25	46.50	0.008***
	(rather) unfair	94	81.91	63.82	
Gender	Male	215	80.47	60.94	< 0.001***
	Female	287	70.03	40.06	
Lab experience	0 or 1	279	69.53	39.06	< 0.001***
	>1	223	80.72	61.44	
Major field of	Economics	35	68.57	37.14	Econ vs. Maths:
study	Business	77	75.32	50.64	0.009***
	Law	34	76.47	52.94	Maths vs. other:
	Medicine	56	71.43	42.86	0.006***
	Education	62	77.42	54.84	all other pairwise
	Maths	62	82.26	64.52	comparisons
	Other	176	72.16	44.32	insignificant

Notes: “Maths” equal 1 if major subject is mathematics, physics, or engineering.
*** p<0.01, ** p<0.05, *p<0.10

The implied cheating rate is higher among receivers than among proposers (53% vs. 45%). Interestingly, this difference is almost entirely due to the high cheating rate of those receivers who earned nothing (64.6%). When comparing only proposers and receivers with a positive payoff (at least €2), the fraction of cheaters is almost identical – even though proposers earned roughly twice as much as receivers with non-zero earnings (€6.12 vs. €3.08). The fact that the cheating rates are similar, despite a huge earnings gap, seems to weigh against an "income-targeting" explanation for cheating. If subjects have an individual income target in mind when they come to the experiment, and if those who have not yet reached their income target have a higher propensity to cheat, groups with different income levels should also exhibit markedly different cheating rates.

The self-reported fairness rating helps to explain cheating among receivers. The cheating rate of those who rated their counterpart as "fair" or "rather fair" is 46.5%, compared to 64% among those who perceived to have been treated "unfairly" or "rather unfairly" (p-value=0.008).

Cheating seems to be correlated with distributional preferences in the sense that subjects who revealed stronger other-regarding preferences in the dictator game were more likely to be honest. In particular, proposers who shared half of their endowment with the receiver cheated significantly less than their more selfish peers (cheating rates are 37.5% vs. 48.5%). The same finding emerges when considering the hypothetical transfers of receivers. Those who would have implemented an equal split were significantly more honest than those who would have retained a larger share for themselves.

We also find that men cheated more often than women (p-value < 0.001), with cheating rates of 60% vs. 40%. This result is in line with Dreber and Johannesson (2008) who also observed that men are more dishonest when they can employ a lie to their own advantage.[5] Moreover, experienced subjects (defined as having participated in more than one previous experiment in the MELESSA lab) were significantly less honest than subjects who participated in no more than one previous experiment (61% vs. 39%). Cheating rates by subjects' major field of studies are similar and we do not find significant differences, with the exception that students in mathematical fields tend to be slightly more dishonest. However, sample sizes become relatively small when testing for differences across study fields.

[5] In contrast, evidence from psychology (De Paolo et al., 1996) suggests that women lie more often than men when the lie is intended to flatter or benefit the other person and is not linked to harmful consequences (a "white lie").

1.5 "No Intentions" Treatment

In this section, we present a treatment designed to disentangle motives for cheating. Unfair dictator behavior may not be the only reason why receivers with a low payoff perceive it as legitimate to seek compensation via cheating. Our design creates two types of inequality – hierarchical inequality (between a dictator-receiver pair) and peer inequality (among receivers). Hierarchical inequality is created by the intentional decision of the dictators. In contrast, peer inequality is the recognition by receivers that the decisions of different dictators are likely to induce inequality among receivers. While a receiver cannot observe the earnings distribution among his peers, he will likely believe that his earnings are higher than average if the dictator has implemented an equal split, but lower than average if the dictator has transferred nothing to him. Therefore, it might be argued that subjects not only cheated because they were treated unfairly by the dictator or fell short of their income target, but also because they believed that some (or most) of their peers were likely doing better.

1.5.1 Experimental Design

We investigate alternative motives for cheating by implementing a treatment that preserves earnings inequality across receivers but where unfair dictator intentions are absent. To do this while preserving the timing and procedures in the lab, we replaced the dictator game with a neutral task of the same length. This avoided distortion due to a correlation between the cheating propensity of subjects and the time spent in the lab. The task consisted of answering a questionnaire which was unrelated to issues about fairness or cheating.[6] As with the dictator game, the questionnaire was completed using paper-and-pencil. After completing the alternative task, subjects were informed of their earnings. However, they were also informed that others earned either more, less or the same as they did. By implementing this earning inequality via a random mechanism, we captured the dictator game's peer-inequality among receivers, while avoiding acts that might be viewed as particularly fair or unfair. Likewise, we avoided asking subjects to report their feelings regarding the fairness of the random earnings allocation. The reason is that doing this could have introduced an intentions confound by evoking perceptions of unfairness in relation to the experimenter. Finally, it is worthwhile to

[6] The questionnaire contained various hypothetical choice questions about housing demand and mobile phone use, as well as the same demographic background questions elicited in the dictator game.

note that an attractive feature of this "no-intentions" design is that it also provides evidence on an income-targeting explanation for differences in cheating rates.

We implemented these sessions in the following way. When subjects entered the lab, they followed the usual procedure of drawing a seat number. Although they did not know it, this draw also determined their earnings for the experiment. At their seat they found a sealed envelope which had been prepared before the experiment and randomly placed. Subjects were instructed not to open the envelope until told to do so. Each envelope contained a note stating how much a subject would earn for participating in the experiment. After all subjects finished the questionnaire, they were told that they would earn either 0, 2, or 4 Euros in addition to their show-up payment of 4 Euros. We chose the distribution of payoffs to reflect the distribution of the most frequent receiver earnings in each session of the "no intentions" treatment.[7] We then instructed subjects to open their envelope to find out how much they had earned and proceeded with the coin flip game just as in the "fairness" treatment. Total earnings consisted of the amount specified in the note plus the payoff associated with the coin flip.

1.5.2 Results

We ran 10 sessions of this condition with a total of 238 subjects. Results are summarized in Table 1.5.[8] The aggregate cheating rate is 42.86%, which is slightly, and insignificantly, lower than the overall cheating rate in the "fairness" treatment (49.00%). It is comparable to the cheating rate among proposers in the "fairness" treatment, which is 45.02%, but significantly (p-value = 0.025) lower than the cheating rate across receivers (52.98%). This is consistent with our initial hypothesis that cheating is higher among subjects who feel treated unfairly.

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[7] We did not match earnings above the equal split in the dictator game due to the low number of observations with 6 or 8 Euros in the dictator game.

[8] Due to the smaller sample size in the "no intentions" treatment, some comparisons of cheating rates, e.g., across study major, suffer from a low number of subjects per category.

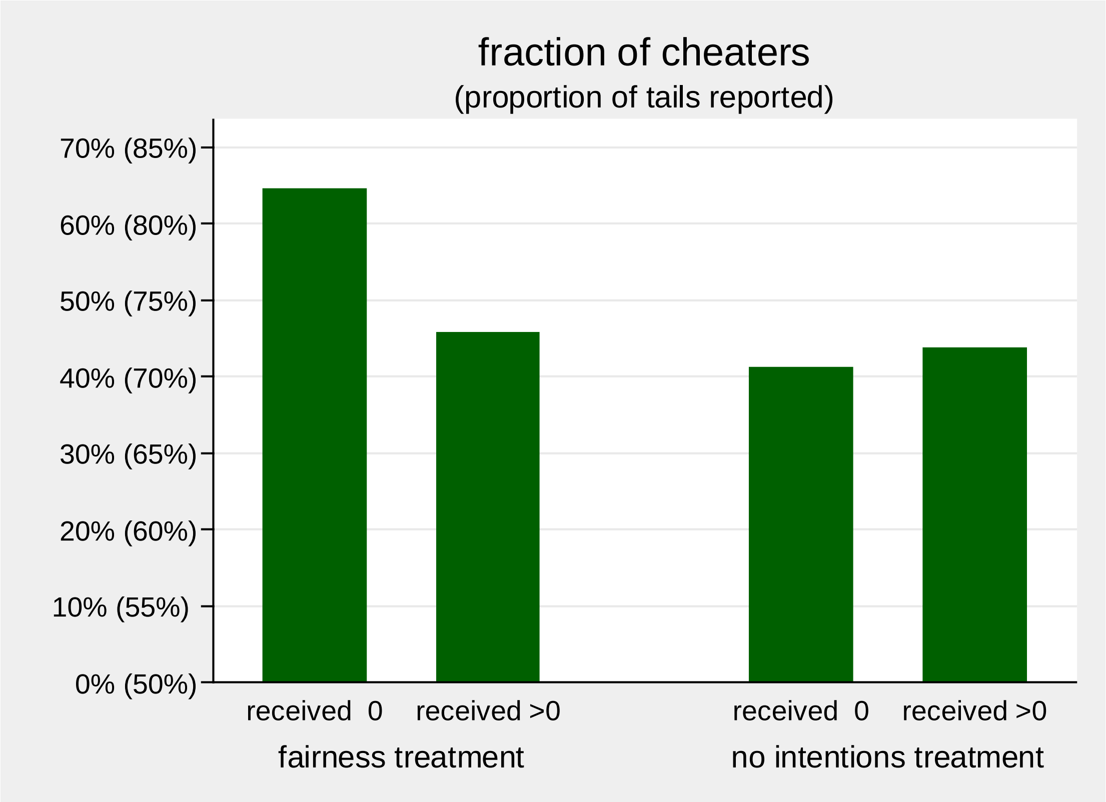
Table 1.5: Comparison of Cheating Rates in the "No Intentions" Treatment

variable	values	N	% tails	implied % of cheaters	p-value from test for equality of proportions
All subjects		238	71.43	42.86	
Earnings	0	92	70.65	41.30	0 vs. 2: 0.169
	2	71	76.06	52.12	2 vs. 4: 0.049 **
	4	75	68.00	36.00	0 vs. 4: 0.485
Gender	Male	102	74.51	49.02	0.096 *
	Female	136	69.12	38.24	
Lab experience	0 or 1	39	71.79	43.58	0.921
	>1	199	71.36	42.72	
Major field of study	Economics	26	80.77	61.54	
	Business	24	75.00	50.00	
	Law	18	77.78	55.56	
	Medicine	10	80.00	60.00	
	Education	33	63.64	27.28	
	Maths etc.	21	70.00	40.00	
	other	97	69.07	38.14	

Notes: "Maths etc." equal 1 if major subject is mathematics, physics, or engineering.
*** p<0.01, ** p<0.05, * p<0.10

Unlike in the "fairness" treatment, the implied cheating rates are not monotonically related to amounts earned. In particular, the implied cheating rate among those with €0 earnings (41.30%) is lower than among those who earn €2 (52.12%), while the implied cheating rate among €4 earners is 36.00%. Moreover, cheating rates are statistically identical (p-value = 0.699) between those who earned the minimum amount and those who earned more (see Figure 1.2). This result is evidence against both peer inequality and income-targeting as motives for cheating. Our data suggest, rather, that experiencing unfair treatment leaves one more likely to cheat on a subsequent decision.

Figure 1.2: Cheating Rates (Proportion Tails Reported) by Earnings in Both Treatments



The same conclusion emerges from a regression framework. Table 1.6 reports average marginal effects for a probit regression where the dependent variable is 1 if a subject reports tails. Thus, positive marginal effects correspond to an increased likelihood of cheating.[9] Column 1 contains results for receivers in the "fairness treatment" and shows that the probability of reporting tails is significantly higher for those subjects who received zero in the dictator game vs. those who received a positive amount. Male subjects and those with more lab experience were also significantly more likely to report tails, while none of the dummies for major subject was significant.

[9] Again, it should be kept in mind that the proportion of subjects reporting tails is a highly conservative estimate for actual cheating.

Table 1.6: Probit Regression for Reporting Tails (i.e. the Better Outcome)

	(1)	(2)	(3)	(4)
	"fairness treatment" only receivers	"no intentions" treatment	"no intentions" treatment; only subjects with lab experience < 7	"no intentions" treatment; weighted regression
Received zero	0.0901 *	-0.0087	0.0184	-0.0826
	(0.0535)	(0.0604)	(0.0682)	(0.0615)
Male	0.1371 ***	0.0558	0.1027	0.1060 *
	(0.0528)	(0.0632)	(0.0691)	(0.0591)
Lab experience > 1	0.1120 **	-0.0061	-0.0155	0.0049
	(0.0523)	(0.0781)	(0.0792)	(0.0580)
Major Econ	-0.1343	0.1061	0.1272	0.1939 ***
	(0.1211)	(0.0881)	(0.0907)	(0.0683)
Major Business	0.0155	0.0421	0.0340	-0.0313
	(0.0762)	(0.0985)	(0.1090)	(0.0889)
Major Law	-0.0128	0.0788	0.1517	-0.0654
	(0.1123)	(0.1059)	(0.1063)	(0.1854)
Major Medicine	-0.0691	0.1175	0.1145	0.1377
	(0.0998)	(0.1219)	(0.1262)	(0.1293)
Major Education	0.0663	-0.0527	-0.0094	-0.1386
	(0.0743)	(0.0933)	(0.0984)	(0.0936)
Major Maths	-0.0279	-0.0146	-0.1751	-0.2380 **
	(0.0975)	(0.0981)	(0.1327)	(0.1112)
Log-likelihood	-130.03	-140.30	-106.53	-126.76
# observations	251	238	184	236

Notes: Average marginal effects reported. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.10

Column 2 of Table 1.6 reports the results of the same specification for the "no intentions" treatment. If income-targeting or peer inequality are to explain the variation in cheating rates across earnings, the dummy for earning the minimum amount should continue to be a significant predictor of reporting tails. However, column 2 shows that this is not the case; the marginal effect is insignificant and very close to zero. Being male is again positively associated with reporting tails, but not significantly so, and the strong experience effect from the fairness treatment vanishes completely.

Since the proportion of inexperienced subjects was much lower in the "no intentions" treatment, we wanted to ensure that differences in sample composition were not driving our main result. Therefore, as a robustness check, we repeated the analysis using only subjects who had not previously participated in more than seven experiments (the number corresponding to the most experienced subject in the fairness treatment (Column 3)). We further estimated a weighted regression where we weighted lab experience such that it corresponded to the distribution in the fairness treatment (Column 4). Neither of these robustness checks changed our result that randomly assigned earnings are uncorrelated with the decision to report tails in the "no intentions" treatment.

1.6 Conclusion

Substantial recent progress has been made in understanding the importance of lying and cheating for economic decision making (see, e.g., Gneezy (2005), Fischbacher and Heusi (2008), Mazar et al. (2008), Sutter (2009), Wang et al. (2010)). In this chapter we reported data from an experiment designed to investigate how unfair treatment in a dictator game affects individuals' propensities to cheat in a subsequent task. We used two approaches to measure fairness in the dictator game: 1) the amount transferred; and 2) the receiver's subjective fairness perception of the amount received. Our analysis shows that both measures significantly predict cheating. We also reported a 'no-intentions" treatment that rules out natural alternative explanations for our data, including income-targeting or a desire to reduce inequality in relation to peers.

Our data argue that the perception of being treated unfairly by another person significantly increases an individual's propensity to cheat. One way to interpret our findings is that individuals might be more likely to violate a social norm (the no-stealing norm) when they perceive that others do not adhere to a different, unrelated norm (the fairness norm). This interpretation is consistent with Keizer et al. (2009) who documented such a "cross-norm inhibition effect" in a series of field experiments. They showed that an envelope hanging out of a mailbox with a €5 note attached was stolen twice as often when the area around the mailbox was covered with graffiti than when the area was clean. Remarkably, general disorder seems to induce a violation of the no-stealing norm, which is not only widely accepted but even legally protected. This is an extension of the well-known Broken Window Theory of Wilson and Kelling (1982), which suggests that when individuals observe frequent violations of a social norm, the probability that they themselves conform to this norm declines.

The causal link between fairness and cheating suggested in this chapter is important for understanding decisions such as whether to evade taxes. A tax system may be perceived as unjust if tax rates are excessively high or if it contains loopholes which allow certain segments of the population to substantially reduce their tax burden. An experimental study by Spicer and Becker (1980) showed that in a situation that was specifically framed in a taxation context, subjects indeed tried to evade taxes more often when they were exposed to higher than average tax rates. A related experiment by Heinemann and Kocher (2010) shows that a change in the tax regime (in their case from a proportionate to a progressive tax, or vice versa) leads to an increase in tax evasion of reform losers whose taxes have increased. In contrast, Kleven et al. (2011) conclude from a large field experiment that marginal tax rates have only a modest effect on tax evasion. However, a high individual tax rate is only one aspect of how fair a tax system is might be considered. Our study suggests that the connection between fairness and tax evasion might have important implications for public policy and merits further research. Also, in an experiment framed in the related context of emissions trading, Cason and Raymond (2011) find that an equitable allocation of emission permits leads to more honest reporting behavior.

Methodologically, our experimental design creates an environment that allows us to study how interaction with others affects an individual's propensity to cheat. The limitation, however, is that individual cheating cannot be observed. Thus, inferences must be based on aggregate statistics that characterize differences between observed and predicted distributions of self-reported outcomes of a random event. This approach requires large sample sizes to find significant effects. Nevertheless, we did find significant effects, namely that subjects who perceive they have been treated unfairly by their respective dictators cheat more often in a subsequent coin flip game.

Appendix: Instructions

The Experiment

In this experiment there will be two roles, which will be referred to as **Person 1** and **Person 2**, respectively. You will be randomly assigned to one of these roles. In the course of the experiment you will be randomly and anonymously matched with another participant, who will be in the opposite role. As mentioned above, you will not receive any information about the other participant's identity.

Procedure

At the beginning, Person 1 receives an initial endowment of $8. Then Person 1 has to divide the $8 between himself and Person 2.

The **payoff to Person 1** is €8 minus the amount sent to Person 2.

The **payoff to Person 2** is the amount that Person 1 has sent.

When deciding about how much to send, Person 1 can choose **one** of the following options:

- Send €0 to Person 2 and keep €8; or
- Send €2 Euros to Person 2 and keep €6; or
- Send €4 to Person 2 and keep €4; or
- Send €6 to Person 2 and keep €2; or
- Send €8 to Person 2 and keep €0.

We will ask you to indicate your preferred option **before you know if you actually are Person 1**.

Whether you will be Person 1 or Person 2 will be determined randomly. Therefore you will have to pick an envelope later. Inside there will be a tag with a combination of a letter and a number (either 1 or 2). So at tag might read A1 or A2, B1 or B2, C1 or C2, and so on. The number indicates whether you will be Person 1 or Person 2. The letter matches you with another participant in the opposite role. For example, the person who gets A1 is matched with the person who gets A2.

So, if you have a “1” you will be Person 1. Your earnings will correspond to the option which you chose. If you have a “2” you will be Person 2. Then your earnings will be the amount sent by your matched Person 1.

Quiz

This short quiz ensures that you understand the features of the experiment. You cannot earn anything in the test, but it might help you to make a good decision afterwards.

Question 1:

Imagine you have chosen to send €4 to Person 2 **in case that it turns out that you are Person 1**, and to keep €4 for yourself.

Then you pick an envelope which contains the tag **X2**. This means that your role is Person 2.

Your matched counterpart (i.e. the person with the tag **X1**) has indicated that he would keep €6 and send only €2.

a) How much do you earn in this case? ___

b) How much does your matched counterpart in the role of Player 1 earn? ___

Question 2:

Now imagine that you have chosen to keep €8 and send nothing to Person 2. Then you pick an envelope with the tag **X1**. This means that your role is Person 1.

a) How much do you earn in this case? ___

b) How much does your matched counterpart in the role of Player 2 earn? ___

Your Choice

So far you do not know if you are Person 1 or Person 2. Now you have to decide what you will do **if it turns out you will be Player 1.**

Please decide now how much you want to give to Person 2 by ticking the appropriate box.

IMPORTANT: Think carefully about your decision! Once you ticked the box, you cannot change it any more.

How much of the €8 would you send to Person 2?

€0 ☐

€2 ☐

€4 ☐

€6 ☐

€8 ☐

We will continue after all subjects have made their decisions. Please wait quietly.

Questionnaire

Please answer the following questions about yourself. Your anonymity remains guaranteed.

1) Are you Male or Female
   - o Male
   - o Female

2) What is your major?

   ____

3) What is your age?

   ____

4) In how many MELESSA experiments did you participate so far? If you are not sure please give an approximate number.

   ______

Only dictators:

You have been randomly assigned to the role of Person 1.

Your payoff is €8 minus the amount that you decided to send to Person 2.

Please wait for a moment until you receive further instructions.

Only receivers:

You have been randomly assigned to the role of Person 2.

You received a sheet with the choice of the person who has been matched with you. On this sheet you can see how much Person 1 has sent to you. This is your payoff from this experiment, which we will pay out to you later.

Now we would like to know your opinion about the fairness of Person 1. How would you rate the behavior of Person 1?

- Fair ☐
- Rather Fair ☐
- Rather Unfair ☐
- Unfair ☐

Please wait for a moment until you receive further instructions.

All subjects:

Now you have the chance to increase your earnings.
You will get a €1 coin from us. We will ask you to flip the coin and to report the result below. Depending on how lucky you are you can earn the following:

If you flip **heads** on the upper side, you have earned €1. In this case you keep the €1 coin.
If you flip **tails** on the upper side, you have earned €3. In this case you keep the €1 coin and you will receive a further €2 at the end.

Now please flip the coin.
Flip the coin at your seat and make sure that the coin doesn't fall to the ground and that none of the other participants can see the result of the coin flip!

Please mark what was on the upper side of the coin:

- **Heads**
- **Tails**

Thank you for your participation!
For the payment we will now call you person by person. Please take all sheets with you.

2 Lying and Responsibility

2.1 Introduction

An average person tells between a handful and several dozens of lies per days, depending on which psychological study we are willing to believe and whether a bold exaggeration already qualifies as a lie. For example, in an experiment by Feldman et al. (2002) over 60% of subjects admitted to have lied at least once during a 10 minute conversation, and the number of lies told ranged from 1 to 12. While the investigation of dishonest behavior has for long been a rather neglected field in economics, it has recently moved to the focus of several theoretical as well as experimental studies, e.g. Fischbacher and Heusi (2008), Kartig (2009), Lundquist et al. (2009), Sánchez-Pagés and Vorsatz (2007), or Serra-Garcia et al. (2011).

Dishonest behavior can be observed in a variety of situations with primary interest to economists. Examples are incorrectly filled tax report, claiming qualifications in a job interview which one does not actually possess, hiding negative aspects when selling a product or service, and many more. Of course, a lie need not be believed by the other party. The tax administration's central purpose is to make sure that a sufficiently large proportion of lies are detected. A job applicant's bold assertions about the number of fluently spoken languages or an impressive educational background are likely to be questioned and tested in an interview. And when making a non-trivial purchase, many customers look for independent information to confirm the promises made by the sales agent.

Lying is far from being a rarely occurring event although it violates an important social norm and hardly anyone would readily admit to be a dishonest and untrustworthy person. Akerlof (1983) and Hao and Houser (2011) argue that individuals have a preference if not for *being* honest then at least for *appearing* to be honest, and Mazar et al. (2008) show that this is true not only vis-à-vis others but also vis-à-vis oneself. In their experiment people cheated significantly more often in situations which provided them with a justification for their behavior, thus allowing them to maintain a favorable self-concept.

Given that lying to the disadvantage of others is morally costly, an interesting question is whether alleviating or diluting the responsibility for the negative outcome of another person reduces the threshold for telling a lie, thereby making lying more frequent. Studying this research question in the laboratory allows us to have full control over the probability with which lies are transmitted. For the dictator game, Dana et al. (2007) show that subjects indeed behave significantly less pro-social when responsibility is diluted. In this context, lying is of particular interest as it is arguably a much stronger violation of social norms than refusing to share in the dictator game.

The control treatment of our experiment uses the sender-receiver game introduced by Gneezy (2005) and subsequently modified by e.g. Dreber and Johannesson (2008), Hurkens and Kartig (2009) and Sutter (2009), where a sender can lie to a receiver in order to increase her own payoff at the expense of the other person. We augment the sender-receiver design with a responsibility treatment in which we introduce uncertainty about whether the message from the sender will actually be forwarded to a receiver or not. We fix the probability that lies are transmitted to exactly 50% and investigate whether reducing the possibility of harming others with a lie affects a sender's inclination to be dishonest.

Our research question is also related to the experimental literature on decision-making with responsibility for others. In a trust game setting, Song (2008) finds that subjects both trust less and reciprocate less when they act as group-representatives with responsibility for the other group members' outcomes than when their decisions only have consequences for themselves. In contrast, Charness and Jackson (2009) show that responsibility for others induces subjects to play a less risky strategy in the Prisoner's Dilemma, which might be interpreted as more pro-social behavior. With binary lotteries involving gains or losses Pahlke et al. (2010) show that responsibility for others increases both risk aversion in the gain domain and risk seeking in the loss domain.

This chapter is structured in the following way: Section 2.2 explains the design of the experiment, section 2.3 reports the results for senders and receivers, and section 2.4 concludes with a brief discussion.

2.2 Experimental Design

This experiment employs a sender-receiver communication game with cheap talk messages and consists of two treatments: (i) a *control treatment* which replicates the original design of Gneezy (2005) with sender-receiver pairs and (ii) a *responsibility treatment* where two senders simultaneously send a message, but only one of the messages is actually forwarded to the receiver.

2.2.1 A Cheap Talk Sender-Receiver Game

In the *control treatment*, the sender has private information about payoffs and sends a cheap-talk message to the receiver, who takes an action which determines the payoffs for both players. Specifically, the sender can choose between the following two messages:

> *Message A: "Option A will earn you more money than option B."*
>
> *Message B: "Option B will earn you more money than option A."*

While the sender knows the payoffs associated with both options, the receiver has no information except for the sender's message. Thus, the sender can either lie or tell the truth, and the receiver may or may not believe the message. Note that in a cheap-talk equilibrium a costless message should not convey relevant information (Crawfold and Sobel, 1982). After receiving an envelope with the sender's message, the receiver decides whether to implement the "recommended" option or whether to choose the alternative option instead. The earnings in our experiment are (€8, €6) and (€6, €8), i.e. the payoff of both players are perfectly negatively correlated and with each option one person earns 6 Euros while the other person earns 8 Euros. In this way, the monetary incentive to lie is slightly larger than in the low-incentive treatment of Gneezy (2005) or Sutter (2009), where the difference was only $1 or €1, but clearly smaller than in their high-incentive treatment, where the difference was $10 or €10. Which of the two options would be more favorable for the sender was randomly determined before each session.

Gneezy's main result is that people are averse to lying, and that this aversion (i) decreases with the potential benefit from lying and (ii) increases with the harm a lie inflicts on others. However, as Sutter (2009) points out, only looking at the proportion of senders who forward a deceptive message does not capture all aspects of deception. By eliciting the senders' expectations about the receivers' behavior, he shows that some senders tell the truth because they don't expect the receivers to believe them. While these "sophisticated truth-tellers" appear honest at first sight, they clearly act with the intention to deceive their counterpart. We thus follow Sutter (2009) and elicit the sender's expectations about (i) whether the receiver will follow the recommended option (Q1) and (ii) what is the proportion of receivers who follow the sender's message (Q2). In this way are able to investigate the effect of responsibility alleviation on dishonesty using both the more narrow definition of Gneezy, as well as Sutter's more comprehensive measure. Both questions were incentivized and each correct answer was rewarded with one additional Euro. We classify subjects according to Table 2.1, where we interpret the behavior of "liars" and "sophisticated truth-tellers" as deception.

Table 2.1: Classification of Senders

	Q1: Sender expects that receiver believes message	
Sender's message	YES	NO
LIE	*Liar*	*benevolent liar*
TRUTH	*benevolent truth-teller*	*sophisticated truth-teller*

The experiment took place in MELESSA at the University of Munich.[10] It was conducted in paper-and-pencil format to keep the procedure as transparent as possible, which was especially important for the responsibility treatment. We ran 6 sessions of the control treatment with 16 subjects each – 8 senders and 8 receivers – which lasted approximately 20

[10] Subjects were recruited via ORSEE (Greiner, 2004).

minutes. Roles were randomly allocated and subjects were seated view-protected and separated by roles. The senders picked an envelope out of a set of otherwise identical envelopes which were all marked with a different letter and contained the decision sheet on which they indicated which message they wanted to send. The receivers also had been randomly assigned to a letter and received the corresponding envelope with the message. In this way, anonymity between receivers and senders as well as within roles was guaranteed during the entire experiment. Upon receipt of their envelope the receivers selected their preferred option, the experimenter collected the decision-sheets and recorded the payoffs. The experiment concluded with a brief socio-demographic questionnaire which for the senders also included the incentivized questions Q1 and Q2.

2.2.2 Responsibility Treatment

In the control treatment each message from a sender had one designated recipient. In contrast, the *responsibility treatment* was designed such that the probability that a sender's message reaches a receiver is exactly 50%. For this purpose there are two senders for each receiver. Again the senders decide which message to send and put the decision-sheet in their envelope. However, with two senders for each receiver there are also two envelopes with the same letter. As the senders are seated in two rows, the envelopes are collected separately for each row and a randomly chosen receiver picks one set of envelopes which is distributed to the receivers. The other set is withdrawn from the experiment. In this way, neither side knows whose envelopes have actually been chosen. Then again each receiver opts for either option A or B, which determines her own payoff and the payoffs of both senders with the same letter. All subjects had been informed in detail about this procedure at the beginning of the experiment, to make sure that they understood that a sender's message is forwarded with exactly 50% probability.

The asymmetric number of senders and receivers introduces the complication that a receiver with a very strong preference for maximizing total efficiency might prefer the option with lower earnings for herself, because this means that two senders receive the higher payment. To eliminate efficiency concerns we matched each receiver with a passive receiver who made no choice but earned the same as his active counterpart. In this way, there were 2 persons on each side of the sender-receiver game, and the total payoff was identical for each possible outcome. The passive receivers were guided into a separate room at the beginning of the session, answered a questionnaire unrelated to the experiment, and were paid according to the decision of their matched active receiver. We conducted 3 sessions of this treatment, with 46 senders and correspondingly 23 active receivers and 23 passive receivers.[11] For our treatment comparison we thus end up with approximately the same sample size as Gneezy (2005) who had 50 subjects in each treatment.

2.3 Results

In the control treatment 44% of the senders (21 out of 48) forwarded the untruthful message. As the benefit of successfully deceiving one's counterpart was 2 Euros, this proportion of liars is plausible in the light of other studies which used treatments with high and low deception benefits. For example, lying rates in treatments with symmetric payoffs and a gain from lying of 1 Euro/Dollar were 36% (Gneezy) and 44% (Sutter). In treatments with a substantially larger gain from lying (10 Euros/Dollars) the proportion of liars was higher than in our treatment, namely 52% (Gneezy) and 59% (Sutter). Thus, our control treatment produced a plausible deception level.

Table 2.2 provides an overview of the results. Our main hypothesis was that the alleviation of responsibility leads to an increase in dishonest behavior. However, this does not seem to be the case. The proportion of liars is 44% in the control treatment and 48% in the responsibility treatment (see row [5]). While this difference is in the expected direction, it is clearly not significant (p-value from a two-sample test of proportions = 0.692). The same conclusion emerges when instead of lying per se we consider acts with dishonest intentions, i.e. senders who either qualify as liars or as sophisticated truth-tellers. The fraction of dishonest senders is 48% in the control treatment vs. 52% in the responsibility treatment (see row [6]). Again, this result goes in the expected direction but the difference is small and again insignificant (p-value = 0.690).

Our data allow us to test whether we find a similar gender effect as Dreber and Johannesson (2008), i.e. a higher propensity to lie among male subjects. As there is no treatment effect, we pool the data and have 36 male and 58 female senders. While we find a difference in the same direction (men: 53%; women: 41%), it is smaller and insignificant (p-value = 0.256).[12]

[11] We have two senders less in this treatment due to non-show-up in one session.
[12] We also estimate a linear regressions and a probit model and come to the same conclusion.

Table 2.2: Experimental Results (Frequencies)

	Control N=48	Responsibility N=46
Sender lies	0.44	0.48
Q1: sender expects the receiver to implement message	0.63	0.78
Q2: expected proportion of receivers who implement message	0.66	0.66
Receiver beliefs the sender's message	0.79	0.87
Classification of senders		
[1] Liar	0.27	0.39
[2] Benevolent liar	0.17	0.09
[3] Benevolent truth-teller	0.35	0.39
[4] Sophisticated truth-teller	0.21	0.13
[5] Liars [1] + [2]	0.44	0.48
[6] Dishonest subjects [1] + [4]	0.48	0.52

Although theoretically a costless message from the sender should not be informative, the vast majority of receivers in previous experiments actually believed the sender's message.[13] This was not different in our sessions where 79% in the control treatment and 87% in the responsibility treatment followed the sender's message. Not only is this difference insignificant, but it is also surprising that receivers were even more trusting in the responsibility treatment. If receivers had shared our prior, they would have anticipated a decrease in honesty due to the alleviation of responsibility and consequently find their counterparts rather *less* trustworthy.

2.4 Conclusion

In our experiment, senders had the possibility to lie to a receiver and hide behind a veil of ignorance as it was unclear whether their own dishonest behavior inflicted harm on a receiver. Perhaps surprisingly, we find no significant difference in the fraction of dishonest senders compared to a situation in which there is no uncertainty about whether a sender's lie is

[13] For comparison, the fraction of receivers who believed the sender‘s message was 78% in Gneezy (2005), 76% in Dreber and Johannesson (2008), 66% in Hurkens and Kartig (2009), and between 67% and 75% in the three treatments of Sutter (2009).

transmitted. This contrasts with the result of Dana et al. (2007) that anti-social behavior increases in a dictator game when the responsibility for the outcome is obscured. However, lying violates a much stronger social norm than not sharing in a dictator game. As shown by Gneezy (2005), the initial threshold for telling a lie is significantly higher than the threshold for choosing an unfair distribution in a dictator game. Furthermore, if people are averse to lying per se instead of the consequences of their lies, obscuring the causality between one's dishonesty and someone else's negative outcome does not make lying more acceptable. This is in line with the interpretation of Hurkens and Kartig (2009) that a person either never lies, or lies as soon as she finds the outcome obtained by lying preferable. The latter does not vary with the degree of responsibility, which might explain why there is no responsibility alleviation effect in our experiment. Thus, our findings lend additional support to the hypothesis that the evaluation of lying is action-based rather than outcome-based.

Appendix: Instructions

A1: Instructions for Senders

There are two roles in this experiment, which we will refer to as Person 1 and Person 2. By drawing your seat number you have been randomly assigned to one role. You are in the role of *Person 1*.

Information and Payoffs

How much Person 1 and Person 2 earn in this experiment is determined with the decision of *Person 2*. There are two possible payoff options, Option A and Option B, which lead to different earnings for Person 1 and Person 2.

Person 1 is informed about all payoffs for each option and sends a message to Person 2. Person 2 has no information about possible payoffs and decides which option will be paid.

Information only for you (=Person 1):

1. The options are the following:

   Option A: **6€** for you (=Person 1) and **8€** for Person 2

   Option B: **8€** for you (=Person 1) and **6€** for Person 2

2. In earlier experiments of this type 78% of subjects in the role of Person 2 have followed the recommendation by Person 1.

Your Choice:

The only information Person 2 receives comes from you. You will have to send a message to Person 2 and recommend her/him one option. For this purpose you will receive an envelope with a decision sheet on which you can mark either:

“Option **A** will earn you more money than Option **B**” or

“Option **B** will earn you more money than Option **A**”.

Then you can put back the decision sheet in the envelope. The envelope is randomly assigned to you and marked with a letter from A-H. Each Person 2 also draws a letter from A-H and receives the corresponding envelope.

[Only Responsibility Treatment]

In this experiment there are ***exactly twice as many people in the role of Person 1 than Person 2****. Therefore, there are two envelopes with the same letter.*

After you made your choice, one of the two envelopes with the same letter is randomly chosen to be forwarded to Person 2. The other envelopes are withdrawn from the experiment. Thus, the probability that Person 2 receives your envelope is exactly 50%. Neither you nor any other person in the room will be informed about whether your envelope has been forwarded or withdrawn.

Payoffs:

The payoffs are determined with the decision of Person 2. Always two persons earn the same:

- *Both participants in the role of Person 1 with the same letter earn the same.*
- *The decision of Person 2 also determines the payoff for one of the participants who take part in a different experiment in the other room. This participant earns the same as Person 2.*

Person 2 receives the message and chooses one option. Afterwards the experiment is over and you receive your earnings. Your anonymity remains guaranteed. In particular, Person 2 will neither be informed about how much you have earned nor about the possible payoffs in the other option.

[Incentivized questions in the questionnaire]

Q1: Do you expect the receiver to follow your message? □ Yes □ No

Q2: How many out of the 8 participants in the role of Person 2 do you think follow the message of Person 1?

A2: Instructions for Receivers

There are two roles in this experiment, which we will refer to as Person 1 and Person 2. By drawing your seat number you have been randomly assigned to one role. You are in the role of *Person 2*.

Information and Payoffs

How much Person 1 and Person 2 earn in this experiment is determined with the decision of *Person 2*. There are two possible payoff options, Option A and Option B, which lead to different earnings for Person 1 and Person 2.

Person 1 is informed about the corresponding payoffs and sends a message to Person 2.

Person 2 has no information about possible payoffs and will decide which option will be paid.

The message from Person 1 is either

"Option **A** will earn you more money than Option **B**" or

"Option **B** will earn you more money than Option **A**".

Person 1 will put the message in an envelope which is marked with a letter from A-H. Each Person 2 also draws a letter from A-H and receives the corresponding envelope.

[Only Responsibility Treatment]

In this experiment there are exactly ***twice as many people in the role of Person 1 than Person 2****. Therefore, there are two envelopes with the same letter.*

After Person 1 made her choice, one of the two envelopes with the same letter is randomly chosen to be forwarded to you (=Person 2). The other envelopes are withdrawn from the experiment. Thus, the probability that an envelope from Person 1 is forwarded is exactly 50%.

Neither you nor any other person in the room will be informed about whose envelope you received.

Payoffs:

After receiving the message you have to make a decision. Always two persons earn the same:

- *Both participants in the role of Person 1 with the same letter earn the same.*
- *Your decision also determines the payoff for one of the participants who take part in a different experiment in the other room. This participant earns the same as you (=Person 2).*

After receiving the message from Person 1 you have to make a decision. You will not be informed how much Person 1 earned, or how much you and Person 1 would have earned with the other option. Afterwards the experiment is over and you will receive your earnings. Your anonymity remains guaranteed.

3 Delegation and Rewards

3.1 Introduction

Corruption is a pervasive feature across all political systems. Politicians or public officials have to make decisions that potentially favor one party at the expense of another. For example, a politician may have to vote either for a consumer-friendly or an industry-friendly legislation, and a public official may have to decide which of two competing firms is successful in the bid for a public contract. Bribing a decision-maker can be an effective way not to end up as the losing party, but has one obvious disadvantage – it is illegal. Whether this is sufficiently deterrent for the involved parties depends on the risk of being caught and the resulting penalties for bribery, and varies over countries and political systems. In Western democracies with strong rule of law, at least, bribery carries the constant threats of revelation by free media, prosecution by independent courts, and a negative backlash from voters and consumers for both the briber and the bribee. Instead, many industrialized countries have institutionalized lobbying as a legal and regulated form of gaining influence in exchange for e.g. campaign contributions. However, contributions are usually capped at rather low levels compared to the rents that are at stake for the lobbying parties.[14]

Our research question is whether there are more subtle but similarly effective ways of gaining an unfair advantage. In particular, we wonder whether a mutually beneficial relationship can also be maintained by an implicit agreement to exchange favors at two distinct points in time. This question is motivated by the fact that there are few outright corruption cases of high-ranking public officials in Western democracies, while after their political career they frequently enter business relationships with parties who might have benefitted from their previous decisions. Given that each party has an existential interest to conceal corruption (or activities closely bordering on corruption), appropriate field data are not available. Thus, our objective is to explore this research question experimentally.

In Western democracies there is a notable discrepancy between the monetary rewards of pursuing a political career during the years in office on the one hand, and the financial possibilities that can be exploited when a politician leaves office. A potential briber who refrains from bribing and instead establishes a relationship based on mutual gift-giving has various opportunities to reward a decision-maker after his political or administrative career, e.g. via honorariums for speeches or mandates, or by directly offering a position in the upper management level. A politician may anticipate this and proactively help the party which is more likely to reward him in the future. Of course, there are also legitimate reasons why a firm may seek the experience of a person who had an important role in the public service, such as personal contacts and expert knowledge. However, the line between both motives is thin and often blurred. Conducting an experiment offers us the possibility to create an environment where we can eliminate all plausibly legitimate reasons for such a business relationship and focus entirely on whether such a long-term co-operation can be established as a result of the decision-maker being “helpful” in the preceding step.

In our experiment, we first create a situation in which a decision-maker has to allocate points between two other participants, while his own payoff is unaffected. This reflects that - in the absence of illegal payments - a politician’s income is fixed and not related to the decisions he takes. In the second stage, we introduce the possibility that other players reward the decision-maker for his choice. Knowing this, the decision-maker gets the option to delegate his decision right, such that one self-interested player can impose her preferred allocation. A

[14] See Harstad and Svensson (2011) for a model which can explain why bribery is relatively more common in poor countries, whereas lobbying is relatively more common in richer countries.

decision-maker may expect that doing another party a favor by delegating his decision right increases his reward. However, this is not contractible and entirely depends on the reciprocal inclinations of the party to whom the decision was delegated. It is thus uncertain whether such an arrangement of mutual favor trading can be similarly effective as corruption.[15]

We find that even the pure anticipation of a future reward from a lobbying party suffices to bias a decision-maker in favor of this party, even though it creates negative externalities to others. The favored party frequently reciprocates and voluntarily compensates the decision-maker for his partisan choice. In this way, they both end up with a higher payoff, but aggregate welfare is lower than without a rewards channel. Thus, we find that the outcome mirrors one that could have been achieved via a conventional bribery relationship.

The chapter proceeds as follows: in section 3.2 we provide a brief and selective survey of economic research on corruption, with a focus on experiments. Section 3.3 explains the experimental design and section 3.4 makes behavioral predictions. Section 3.5 presents the results and section 3.6 concludes.

3.2 Literature on Corruption and Gift-Giving

By its very nature as an illegal activity, obtaining objective data on corruption at the individual level is virtually impossible when both the briber and the receiver of the bribe are reaping benefits from it. Furthermore, the observed occurrence of bribing is not a particularly informative indicator for corruption as it confounds the occurrence of bribing with the authorities' determination to crack down on corruption. A different scenario is when a person or firm is required to pay bribes to an official in order to receive a treatment it is actually entitled to. A prerequisite for this situation is that the rule of law is sufficiently weak for an official or politician to demand a bribe without being charged, and therefore we are more likely to encounter cases in developing countries. Svensson (2003) uses data on involuntary bribe payments reported by Ugandan firms and concludes that a firm's "ability to pay" and "refusal power" explain a large part of variation in bribes. A different line of research uses information on perceived corruption from business risk surveys and investigates its

[15] Note that the difference between the two is not only that the order of moves is reversed (the briber first pays the reward, then the politician takes the decision) but also that the course of action is more consequentially in the case of bribery. Once a politician accepts a bribe, he already commits an illegal act. In contrast, a politician who implements the desired choice of a self-interested party can always claim that he found this option preferable himself.

determinants in a cross-country comparison. Treisman (2000), for example, comes to the interesting conclusion that while a long exposure to democracy predicts lower perceived corruption for the countries in his sample, the current state of democracy does not.

In the situations described above, data for bribes are available because if they are paid involuntarily the entity which is forced to bribe may not have a need to conceal it. Our focus, however, is on situations in which a bribe is paid voluntarily with the objective of gaining an unfair advantage. In this case, both sides commit an unethical and punishable act, and we should not expect that any of the two will admit this when asked in the context of a survey. Laboratory experiments offer the possibility of developing a better understanding of the underlying mechanisms when the relevant actors have no incentives for disclosure.

The experiment designed by Abbink et al. (2002) represents essential features of corruption, such as a reciprocal relationship between bribers and public officials, negative welfare effects, and penalties in case of detection. They show how scope for reciprocity can establish a bribery relationship and that negative externalities had no moderating effect, while introducing a penalty threat did prove effective in reducing corruption. Using the same design, Abbink and Henning-Schmidt (2006) investigate whether the framing of the game in corruption terms as opposed to neutrally framed instructions had an effect on the behavior of subjects, but found no treatment difference. In contrast, Barr and Serra (2009) find that subjects were significantly less likely to offer bribes when a corruption frame was applied. Interestingly, Lambsdorff and Frank (2010) show that when bribers can choose whether to call it a “bribe” or a “gift”, around 20% of the subjects preferred the term “bribe” despite its morally negative connotation. Potters and Van Winden (2000) compare the behavior of students with professional lobbyists and find that the latter behave more in line with game theoretic predictions and earn more money. Büchner et al. (2008) conduct an experiment to study bidding behavior in public procurement auctions and show that bribes are actively and frequently used although they were framed in negative terms. Finally, Abbink (2004) reports that a change in the matching protocol from partners to strangers significantly reduces corruption activity, suggesting that a staff rotation policy might be a partial remedy.

Another related paper is the gift-giving experiment of Malmendier and Schmidt (2011). Their study is motivated by the excessive gift-giving practice of lobbyists in sectors such as the pharmaceutical industry, which is unconditional but clearly driven by the expectation of influencing the target group (i.e. medical doctors and other health care professionals). In their experimental setup a decision-maker acts on behalf of a principal and has to buy a product from one of two producers. One of the producers is randomly selected and receives the option to pass on a small gift to the decision-maker. Malmendier and Schmidt show that decision-makers are significantly more likely to choose the product of the very producer who made the gift, even when the other product has a higher expected payoff and even though most decision-makers fully understand that the sole purpose of the gift was to influence them. This is an interesting result because it shows that a producer doesn't have to go the illegal route of paying a bribe in order to gain an unfair advantage. Instead, it is possible to obtain a similar outcome by giving a seemingly unsuspicious gift. In our experiment we use a different design but follow a similar idea. By changing the order of moves we go one logical step further to see whether even the pure anticipation of a gift may result in favorable decisions for the potential gift giver and lead to outcomes that otherwise might have been achieved via bribing.

Finally, the analysis in this chapter is related to the experimental literature concerned with the strategic motives and benefits of delegating tasks or decisions, beginning with Fershtman and Gneezy (2001). This line of research has shown that by delegating a decision to an agent, a principal can also shift the responsibility for the outcome to the agent (see e.g. Bartling and Fischbacher, 2011, or Coffman, 2011). Hamman et al. (2010) show that a principal who is reluctant to take a self-serving action may use an agent to achieve an outcome which is less pro-social than if he had taken the decision himself. However, delegation can also help to increase efficiency. Hamman et al. (2011) find that the contributions in a public goods game are higher when the decision is delegated to an elected agent, and Charness et al. (forthcoming) show that workers who can choose their wages themselves have significantly higher effort levels. In the latter case, delegation might pay off because both sides can increase their earnings if workers reciprocate. This rationale for delegation is what we are interested in also in our context. Delegating a decision right to a lobbying party may be beneficial for both if the lobbying party is reciprocally inclined.

3.3 Experimental Design

We use a novel design to investigate whether the possibility of being rewarded in the future induces an otherwise neutral decision-maker to favor a more powerful lobbying party at the disadvantage of another. In the main part of our experiment, subjects interact in groups of 3, with each group member in a different role. Player 1 is a decision-maker who is not directly affected by the decisions he takes, and can be seen as representing a public official or politician. Player 2 and Player 3 are directly affected by the decision of Player 1, but their interests are diametrically opposed. This reflects e.g. the conflicting interests between producers and consumers in the face of legislative changes. In the experimental design we incorporate that some pressure groups are more influential, have easier access to politicians via lobbyists and thus an advantage in exerting direct influence compared to other groups. In our design, we model this by giving the decision-maker the opportunity to implement the outcome preferred by Player 2, but not the preferred outcome of Player 3.

The experiment consists of two distinct parts and is briefly summarized in Table 3.1. In the first ("preference elicitation") stage we elicit subjects' preferences for earnings distributions conditional on their role in the game. With the instructions for the first part subjects are informed that there will be a second stage afterwards in which their payoffs are to be decided based on decisions from the first stage, but they do not receive more information about it. In the second ("gift-giving") stage, the decision-maker has to decide whether he ex-post delegates his decision right to the more powerful lobbying party, i.e. Player 2. Both Player 2 and 3 have to decide whether, conditional on the outcome, they reward the decision-maker. The experimental instructions can be found in Appendix C.

Table 3.1: Summary of the Experimental Design

1. Stage:	- random assignment of roles (Player 1, Player 2, and Player 3); no information about the differences between roles - subjects are informed that there will be a second stage and that payoffs will be based on decisions from the first stage - each subject makes 12 decisions about the distribution of earnings among players
2. Stage:	New information: Player 1 is the decision-maker For each decision situation - Player 1 decides if he wants to delegate the decision right such that the choice of Player 2 is adopted - Players 2 and 3 can reward Player 1 by transferring points; transfer decisions are made conditional on the implemented option (strategy method) - incentivized expectation questions
End of the experiment	Questionnaire on demographics, BIG5 traits and risk aversion

3.3.1 First Stage

In the beginning of the preference elicitation stage subjects are randomly assigned to one of the three roles in which they remain for the entire experiment. However, they are not matched into groups until the second stage, and instructions for the first stage are identical for all players and across treatments.

The purpose of the first stage is to elicit distributional preferences of subjects, in particular whether they are motivated by the equity and efficiency consequences of their choices. For this reason, subjects make a series of binary decisions between two payoff distributions. Option A, the equitable distribution, is the same in each round and corresponds to (100, 100, 100), which means that each player receives 100 points if this option is the one to be implemented. With Option B, the unequal distribution (100, p, q) Player 1 again earns 100 points, but the potential payoffs for Player 2 and Player 3 change in each round in a way that either $p < 100 < q$ or $p > 100 > q$. In each round p and q vary over the following dimensions:

(i) *Advantage Player 2*: whether Player 2 earns more than Player 3, or vice versa

(ii) *Efficiency*: whether the sum of all payoffs from Option B is larger, equal to, or smaller than 300 points (the total payoff from Option A)

(iii) *Degree of inequality*: whether the absolute gap between p and q is only 40 points (e.g. 120 vs. 80), or 120 points (e.g. 40 vs. 160)

Each subject makes 12 decisions. The respective payoffs under Option B are listed in Appendix A. In each session the order was randomized.

At this time, subjects know the role in which they remain also for the second part. In addition, they had been informed at the beginning that it will be determined only in the second part whose decisions will be payoff-relevant in which round. However, they receive no further information about the decision mechanisms, so they have no strategic incentive not to reveal their preferred distributions. In particular, subjects do not yet know that Player 1 is the decision-maker and that Player 2 is in a more advantageous position than Player 3.

A key feature of this setup is that Player 1 has no monetary incentive to bias his decision in favor of either Option A or B. This reflects the fact that an administrative or political decision-maker receives a fixed salary but no direct financial compensation for his decisions.

This is in contrast to Players 2 and 3, who have an obvious interest to influence the outcome, but no decision rights.

3.3.2 Second Stage

In the beginning of the second stage, subjects receive a new set of instructions. They learn that they will see the previous decision situations again, and that Player 1 is the key player. In each round Player 1 decides whether he wants his initial choice between options A and B to be relevant for determining the payoffs. Alternatively, he can replace his choice with the initial choice of Player 2 (but not Player 3) from the first part. The choice has to be made without knowing the actual decisions of the others.[16] This setup introduces asymmetry between the self-interested players as it provides the decision-maker with an opportunity to do Player 2 a favor at the expense of Player 3, but not vice versa.[17]

Players 2 and 3 can reward the decision-maker by sending a transfer. As all subjects have identical instructions, this is common knowledge from the beginning of the gift-giving stage on. In each round the player(s) with the highest earnings can make a transfer to Player 1, which is elicited via the strategy method, i.e. conditional on the implemented option. If Option B is implemented, one of the two self-interested players earns strictly more and only he makes a transfer decision. Following Abbink et al. (2002) and Malmendier and Schmidt (2011), the transfer is multiplied by 3 to reflect that a bribe or gift generally has a larger marginal utility for the receiver. If instead Option A is chosen, earnings are identical and both Player 2 and Player 3 make a transfer decision. Then the average transfer is multiplied by 3. In this way, the expected transfer of a decision-maker is identical across options, assuming Players 2 and 3 would always send the same amount.[18]

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[16] By taking the previously made decision instead of asking Player 2 again, we want to ensure that Player 1 cannot justify favoring Player 2 by assuming that Player 2 would behave more pro-social if he knows that his decision determines the group outcome.

[17] We refer to Players 2 and 3 as “self-interested” players because they have something at stake already in the first stage, while Player 1 has by then no own financial interests by construction. However, this changes in the second stage when Player 1’s may very well be also guided by pure self-interest.

[18] Instead of using the average transfer of Player 2 and 3, we could have allowed that both players can make a transfer under B. However, a strictly disadvantaged player would probably not have rewarded Player 1 anyway – especially as there is no strategic benefit in doing so. Thus, this would have created an asymmetry between two possible senders under Option B vs. one possible plus one extremely unlikely sender under Option A. In this case, a risk averse decision-maker might have found Option A more appealing as it increases the likelihood of receiving a positive transfer. We thus decided to use the raw transfer of the higher earning individual under B and the average of both transfers under A.

Transfers can range between 0 and 25 points. The upper limit is imposed to ensure that extreme outliers do not bias the econometric results. Sending an amount equal to the maximum of 25 corresponds to a substantial transfer of between 15 and 25 percent of Player 2's or Player 3's total earnings in a particular round. Therefore, it is unlikely that capping contributions at 25 imposes a strong restriction even on extremely reciprocally inclined subjects.

After each choice, subjects answer incentivized expectation questions about the other players' behavior. For each possible outcome, subjects in the role of Player 1 have to state their beliefs as to whether the other players have made a transfer. Player 2 and Player 3 have to state their beliefs about which option Player 1 has initially chosen and whether he has decided to stick to his initial decision or instead accepted the choice of Player 2. Each correct prediction is rewarded with 10 additional points. At the end of the experiment, subjects complete a short questionnaire with socio-demographic questions, a self-reported risk aversion question, and the compact BIG-5 module with 15 questions developed for the German Socioeconomic Panel.

Due to the tripled transfers to Player 1 this experiment bears resemblance to the popular trust game of Berg et al. (1995), but note that there are two crucial differences. First, our game introduces negative externalities. Thus, the equivalent of *trustor* and *trustee* can increase their respective payoffs only at the expense of a third party. Second, the *trustor* does not make an investment in the classical sense, i.e. there is no monetary payment to the *trustee*. However, what she does "invest" is her preferred allocation and thus her notion of fairness.

3.3.3 Treatments

A bribing relationship can be either a one-shot interaction or a repeated situation. Abbink (2004) has shown that this makes a difference in a classical corruption setting, and finds that the level of corruption is significantly higher with partner matching compared to stranger matching. We were thus interested in how repeated interaction affects the degree to which the weak position of Player 3 is exploited and whether it fosters the bond between the two "partners-in-crime" in the gift-exchange. Therefore, we conducted the experiment under two treatment conditions – *Partner* vs. *Stranger*. The first stage was identical for both treatments because subjects had no information about how groups would be formed. In the second stage, the *Partner* treatment assigned subjects to the same group in each round, while they were always matched with different persons in the *Stranger* treatment.

Theoretically, there is no reason why the matching protocol should make a difference for purely selfish individuals. Unlike in repeated public goods games, for example, there is no possibility to build up reputation because we do not provide information about other players' actions during the experiment. We do this in order to keep the number of independent observations identical across treatments. Also, in real-life situation it can sometimes be difficult or even impossible for the gift-giver to observe whether a public official has already carried out the favorable act on behalf of the gift-giver, e.g. when votes are cast anonymously.

What the treatment variation changes, however, is the degree of responsibility for the outcome of others, because a decision-maker can hide behind the veil of ignorance in the *Stranger* treatment. Even if he makes a very harmful decision at the expense of Player 3 he may convince himself that other decision-makers treat Player 3 better than he did. In the *Partner* setup, however, an opportunistic decision-maker would always disadvantage the same person, thus being fully responsible for the poor outcome of Player 3.

3.3.4 Implementation

We conducted in total 6 sessions (3 in *Partner*, 3 in *Stranger*) at MELESSA, the experimental lab at the University of Munich, and all participants were university students from various disciplines.[19] The experiment was computerized with the software z-tree (Fischbacher, 2007). Most sessions were conducted with 21 subjects, but due to non-show-up one *Partner* session was conducted with 18 and one *Stranger* session with 15 subjects. Subjects earned points which were converted at an exchange rate of 1 Euro for 150 points. Average earnings were 13 Euros, and each session lasted approximately 50 minutes.

[19] Recruitment of subjects was done with the software Orsee (Greiner, 2004).

3.4 Behavioral Predictions

In the first stage each subject makes several choices between the equitable distribution A and the unequal distribution B. By design, there is always one of the self-interested players who is better off with the unequal option, and one who is worse off. In contrast, Player 1 is in a neutral position and earns the same in both cases. Therefore, his decisions should reflect purely distributional preferences, in particular how the trade-off between equity and efficiency is evaluated. If Player 1 is sufficiently inequality averse, he will always opt for Option A. However, in cases when B yields a higher overall payoff a decision-maker with preferences for efficiency may consider deviating from the equality-preserving choice.

Conjecture 1: *Player 2 and Player 3 choose B if their own payoff is higher than with A. Player 1 only deviates from the equitable distribution if the unequal option results in efficiency gains.*

At the beginning of the second stage, Player 1 learns that he can rule his distributional decision from the first stage irrelevant and instead adopt the preferred option of Player 2. For a purely benevolent decision-maker there is no reason why this possibility would be appealing, since adopting the partisan choice of Player 2 is unlikely to result in a better outcome in terms of efficiency and equity. In the absence of rewards, an entirely selfish decision-maker should be indifferent between sticking to his own and Player 2’s previous choice. When there is scope for rewards, however, the decision-maker may strategically favor Player 2 in the hope that his action will be reciprocated.

Conjecture 2: *With the possibility of being rewarded, decision-makers will strategically delegate the decision right to increase their own payoff if Player 2 is the one who benefits from giving up the equity-preserving option.*

Whether this kind of strategic behavior pays off for Player 1 depends on the reciprocal inclinations of the Player 2 subjects. As there is no feedback during the experiment, there is also no way to build up reputation, not even in the *Partner* treatment. Thus, the transfer decisions have no strategic component and an entirely self-interested player would always choose to send zero points. Positive transfers reflect purely reciprocal intentions.

Conjecture 3: *As it is individually rational to send nothing, we expect a large proportion of zero transfers. However, most individuals are reciprocally inclined at least to some degree.*

We thus expect that the transfers of Players 2 and 3 increase in their own earnings from the implemented option.

Finally, our experimental design allows us to test whether the introduction of a rewards channel changes the aggregate outcome for a group. If the presumably benevolent decision of Player 1 is replaced by a decision which was not made in the best interest of all, the society as a whole might be worse off.

Conjecture 4: *Introducing the possibility that a decision-maker can favor a self-interested party in exchange for a reward leads to worse aggregate outcomes.*

In the next section we will evaluate each of these conjectures in turn.

3.5 Results

3.5.1 First Stage

With 117 subjects making 12 decisions about their preferred allocations we obtain a sample of 1404 single decisions. Due to the symmetric setting, we have 702 single decisions in cases when Option B is better for Player 2 and Player 3, respectively. As mentioned in the previous section, we expect that Players 2 and 3 generally opt for B when they earn more from it, while Player 1 seeks a compromise between equity and efficiency. Table 3.2 provides a summary of choices in the first stage. More detailed information about the choice frequencies for each decision situation can be found in Appendix A.

Table 3.2: Choices in the Preference Elicitation Stage

	Frequency of choosing B (in percent)		
	total	advantage Player 2	advantage Player 3
Player 1	13.67	12.39	14.96
Player 2	46.37	89.74	2.99
Player 3	46.15	2.99	89.31
#choices	1404	702	702

The comparison across roles reveals that subjects in the role of Player 1 were generally reluctant to implement inequality. They opted for B only with 13.67 percent even though in one third of the situations it would actually have been efficiency enhancing. However, the desire to maintain equity seems to have been sufficiently strong to choose A in most cases. This result demonstrates neatly that subjects with no personal stakes in a distribution decision hardly ever sacrifice equity between the other involved parties. In contrast, the self-interested players clearly opted for B if it was to their advantage – 89.74% of Players 2 and 89.31% of Players 3 did so – and clearly avoided B if it was to their disadvantage, with a probability of being chosen of less than 3% for both of them.

These results show that Players 2 and 3 have very similar distributional preferences. In addition, we also see that Player 1 did not favor one person at the expense of the other, as the probability of choosing B does not depend on who gains more from it.

In the next step we estimate probit models in which the dependent variable is 1 if a subject chooses B, i.e. deviates from the equitable distribution, with standard errors clustered by individual to account for error dependence across periods. The first column of Table 3.3 uses only the choices of subjects in the role of Player 1, the second column only those of Players 2 and 3. The regressions include two dummies indicating whether the choice of B over A results in an overall efficiency gain/loss; an inequality dummy for a large payoff gap between Player 2 and Player 3 (a difference of 120 vs. 40 points); a "treatment" dummy (but recall that there is no treatment difference in the first stage); and a control for period effects. In addition, the first regression includes a dummy for whether it is Player 2 or Player 3 who has an advantage from B. The second regression instead includes the amount to be earned with option B (which is omitted in column (1) as there is no variation for Player 1).

Table 3.3: Probit Model for Chosen Option; Dependent Variable = 1 if Option B

	(1)	(2)
	Player 1	Players 2&3
constant	-1.229***	-5.182***
	(0.352)	(0.858)
advantage Player 2 (0/1)	0.129	-
	(0.085)	
Option B earnings	-	0.051***
		(0.009)
efficiency 20 plus (0/1)	0.797***	-0.078
	(0.240)	(0.176)
efficiency 20 minus (0/1)	0.140	-0.028
	(0.097)	(0.110)
gap large (0/1)	-0.087	-1.392***
	(0.148)	(0.515)
stranger treatment (0/1)	-0.037	-0.112
	(0.317)	(0.172)
period	-0.019	0.006
	(0.021)	(0.017)
N	468	936
log-likelihood	-173.8	-239.9

Notes: Standard errors clustered by individuals in parentheses * p<0.10, ** p<0.05, *** p<0.01

The results clearly demonstrate that Player 1's main motive for implementing an unequal allocation by choosing B over A is to increase total efficiency. The degree of inequality, i.e. if the gap is 120 or only 40 points, does not play a role. Also whether it is Player 2 or Player 3 who benefits from Option B does not influence the decision of Player 1, which again confirms that in this symmetric setting the decision-maker acts as a neutral and benevolent authority. Turning to the decisions of Players 2 and 3, the potential earnings under Option B are a highly significant predictor for preferring the unequal option (with p-value < 0.001). However, this selfish motive is moderated by the significantly negative influence of the size of the gap between Players 2 and 3, which reflects a concern for others. Efficiency gains or losses do not seem to be important. The "treatment" dummy is insignificant in both regressions, which indicates that subjects are comparable in terms of social preferences across treatments. Finally, as Player 1 subjects make decisions in which they have nothing to gain at this stage, one might be concerned that they get less attentive over time and make their decisions less thoughtfully as the experiment progresses, but we find no time effects which would substantiate this concern.

Result 1: *Players 2 and 3 generally choose the option which maximizes their payoff, although a large degree of inequality moderates selfish behavior. Player 1 wants to preserve equity and does not favor one at the expense of the other; he only deviates from the equity-preserving option if the alternative allocation results in efficiency gains.*

3.5.2 Second Stage: Delegation Decisions of Player 1

In this section we investigate how decision-makers react to the new information that they can do Player 2 a favor by ex-post delegating the decision right about the payable option. Conditional on having chosen A in the first stage, decision-makers decide to render their own decision irrelevant with a probability of 38.61% and instead let payoffs be determined by the first stage decision of Player 2. In the less likely case that a decision-maker has opted for B in the preference elicitation stage, the percentage is only slightly higher, at 43.75%. Based on the observed frequencies, a Pearson's chi-squared test does not reject the hypothesis that the delegation decision is independent of Player 1's initial choice in Stage 1 (p-value = 0.434). Of course, this comparison of percentages is not informative about the underlying incentives in each decision situation, which is why we turn to regression analysis in the next step.

Table 3.4 contains results for 3 different probit regressions. The dependent variable is equal to 1 if Player 1 delegated his decision right. In column (1) we regress it on the same variables used for the estimations reported in Table 3.3. In column (2) we add Player 1's beliefs about possible transfers from Players 2 and 3, elicited after each round. Finally, column (3) adds a set of demographic and behavioral covariates.

Table 3.4: Probit Regression; Dependent Variable = 1 if Player 1 Delegates

	(1)	(2)	(3)
constant	-0.990***	-1.486***	-2.899***
	(0.199)	(0.229)	(0.829)
advantage Player 2 (0/1)	0.775***	0.809***	0.834***
	(0.184)	(0.216)	(0.224)
efficiency 20 plus (0/1)	0.240*	0.191	0.215
	(0.128)	(0.135)	(0.139)
efficiency 20 minus (0/1)	-0.195	-0.116	-0.123
	(0.150)	(0.152)	(0.158)
gap large (0/1)	0.220	0.071	0.062
	(0.134)	(0.138)	(0.146)
stranger treatment (0/1)	0.357*	0.262	-0.027
	(0.184)	(0.174)	(0.309)
period	-0.000	0.015	0.017
	(0.015)	(0.018)	(0.015)
belief receiving transfer from 2 or 3 if Option B (0/1)		0.669***	0.716***
		(0.189)	(0.159)
belief receiving transfer from 2 if Option A (0/1)		0.066	0.101
		(0.236)	(0.208)
belief receiving transfer from 3 if Option A (0/1)		0.052	0.106
		(0.235)	(0.219)
demographics, risk aversion & BIG5	NO	NO	YES
N	468	468	468
Log-likelihood	-284.2	-271.8	-258.3

Notes: Standard errors clustered by individuals in parentheses * p<0.10, ** p<0.05, *** p<0.01; column (3) includes controls for age, gender, lab experience, study major, self-reported risk aversion and BIG5 traits.

In comparison to the first stage, the most striking result is that despite the symmetric setup of payments, the dummy variable which indicates an advantage for Player 2 from Option B is now highly significant. This is due to the non-symmetric setup of the delegation decision. Regardless of who gains more from inequality, the decision-maker can only guarantee that the option preferred by Player 2 is implemented, but not the option preferred by Player 3. Helping to impose an unequal distribution might be explained by the anticipation of potential rewards.

We investigate this hypothesis more thoroughly by adding the decision-makers' beliefs about receiving transfers from Players 2 and 3. We find that the expectation of receiving a reward from the Player who gained more from B is positively and significantly associated with renouncing one's initial choice. Whether the decision-maker expects to receive a reward from either player if option A is implemented does not seem to be important. By adding beliefs, the initially significant effect of potential efficiency gains from choosing B becomes insignificant, and the spurious correlation with the treatment dummy vanishes. Whether subjects interact with partners or strangers has no effect on their decision to implement the preferred option of Player 2. Finally, adding further demographic and behavioral covariates does not reveal statistically significant associations, with the exceptions that more risk tolerant subjects are more likely to delegate and more conscientious persons are less likely to do so (coefficients not reported).

Result 2: *In anticipation of potential rewards, decision-makers act opportunistically and are significantly more likely to delegate their decision-right if Player 2 gains from inequality.*

3.5.3 Second Stage: Transfer Decisions

As transfers were elicited via the strategy method, subjects in the role of Player 2 and Player 3 had to make decisions conditional on Option A or B being the relevant outcome. Subjects could transfer any integer number of points between 0 and 25 to the decision-maker. A purely self-interested subject would always choose a transfer of 0, because her actions are not observable for other players. Note that this holds even in the *Partner* treatment. A positive transfer therefore reflects reciprocal intentions without any strategic considerations.

Figures 3.1 & 3.2 show that indeed the most frequent choice was to send nothing – in around 70 percent of cases when the final outcome was Option A and with around 40 percent in case it was Option B. However, there is also a nontrivial number of relatively high transfers, with multiples of 5 as focal points.

Figure 3.1: Transfer Conditional on Player 1 Choosing Option A

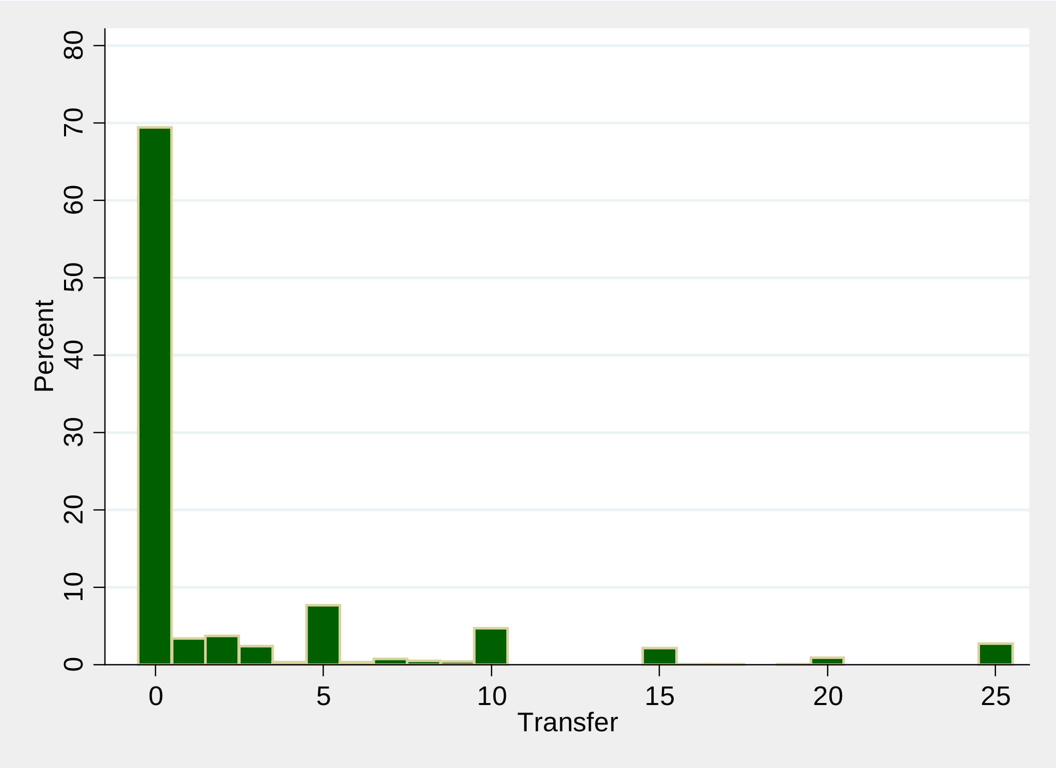


Figure 3.2: Transfer Conditional on Player 1 Choosing Option B

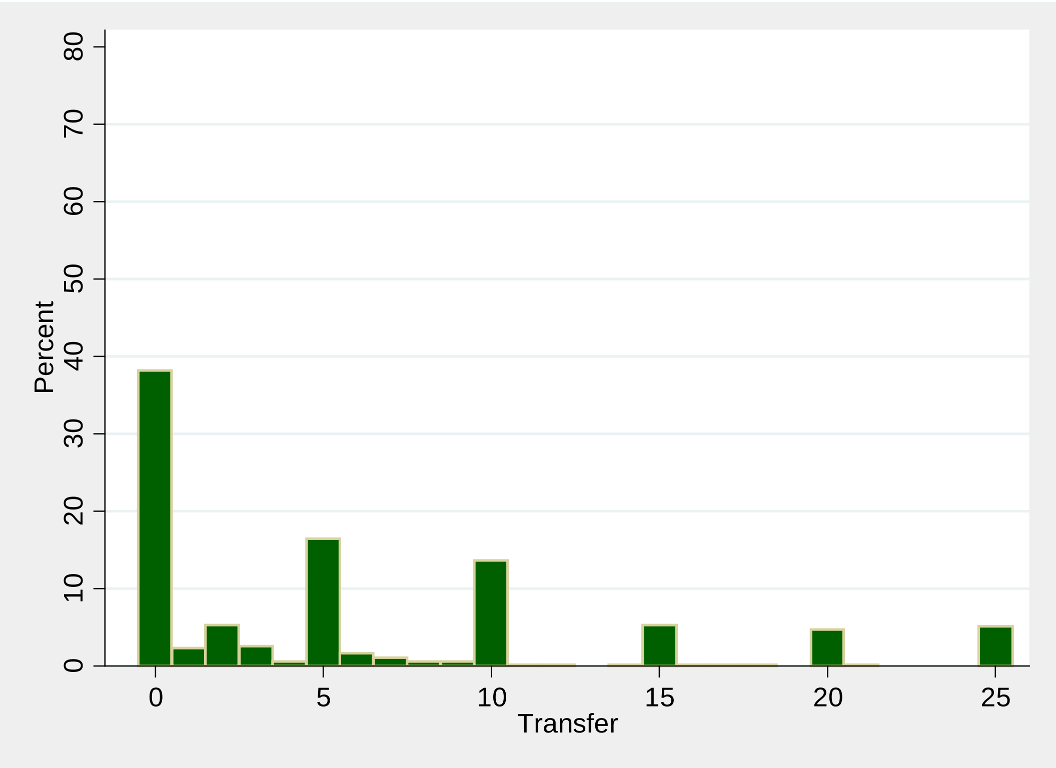


Table 3.5 displays mean transfers by option and role. In case Option A was implemented, Player 2 transferred 2.81 points on average, which is slightly more than the 2.11 points of

Player 3 but the difference is statistically indistinguishable. A different picture emerges with transfers conditional on Option B being implemented. Again the average transfer of Player 2 is larger, but this time the difference is more pronounced (6.96 vs. 4.97 points) and statistically significant (p-value = 0.003). Thus, from the perspective of the decision-maker, helping to implement Option A earned him on average 7.39 points (the average group transfer of 2.46 multiplied by 3), while he earned 5.97*3 = 17.90 points when Option B was implemented.

Table 3.5: Transfers Sent by Players 2 and 3 Conditional on Implemented Option

Amount sent		Mean	Std. Dev.	N	Percentage of zero transfers
if Option A implemented	Player 2	2.81	5.53	468	64.10 %
	Player 3	2.12	5.25	468	74.79 %
	Group average	2.47	5.40	936	69.45 %
if Option B implemented	Player 2	6.96	7.37	234	29.49 %
	Player 3	4.97	6.87	234	47.01 %
	Group average	5.97	7.19	468	38.25 %

As the individually rational strategy for a selfish subject would be to never transfer anything, we can further look at subjects individually and classify them in the spirit of the "conditional co-operator" vs. "free-rider" distinction of Fischbacher et al. (2001).[20] In total 23.1 percent of Players 2 and 3 (i.e. 18 out of 78) are completely "selfish" across all 12 decision situations. Table 3.6 further displays the fraction of subjects that never send any transfer, conditioned on the implemented option. Two interesting aspects become evident from this table: (i) Player 3 is more likely to be completely unwilling to send a transfer and (ii) regardless of their role, subjects are less likely to never reward conditioned on Option B as opposed to Option A.

[20] The equivalent of conditional co-operation in this context is to send a positive transfer in case the implemented option is to one's own advantage, while a free-rider is a subject who never sends a transfer regardless of the outcome.

Table 3.6: Fraction of Subjects that Never Transfer, Conditional on Chosen Option

Person 2		Person 3	
If Option is A	If Option is B	If Option is A	If Option is B
28.21 %	17.95 %	48.72 %	33.3 %

We next turn to the determinants of transfer choices. As mentioned before, transfers were censored at 25 in order to avoid extreme outliers. As we did not allow transfers to be negative, they were also naturally censored from below at 0. We thus use a two-limit Tobit model to account for the double censoring, but a one-limit Tobit model with censoring at zero or conventional OLS regressions yield very similar results.[21]

The pattern of transfers suggests that reciprocity (positive as well as negative) is the key to subjects' choices. When making the transfer decision conditional on Option A being chosen, the earnings for the counterfactual outcome B has a significantly negative effect. In contrast, when Option B is implemented, the rewards that Players 2 and 3 send to the decision-maker are increasing in their earnings. Perhaps surprisingly, beliefs about the decision-maker's choices in both stages hardly have an effect, with the notable exception that Player 2 subjects send a significantly larger reward if they believe that the decision-maker delegated the decision right to implement their own preferred option. So Player 2 seems to recognize the favor and is willing to reciprocate. Again there are no time effects, and treatment differences are mostly insignificant but it is interesting to observe that Player 2 tends to send more in the Partner treatment, while Player 3 sends less.

[21] The results are available upon request.

Table 3.7: Two-limit Tobit-Regressions for Transfers Decisions of Players 2 and 3

	Transfer if outcome is			
	Option A		Option B	
	Player 2	Player 3	Player 2	Player 3
Constant	1.087 (13.44)	41.81 (27.5)	-6.767 (10.84)	8.143 (20.57)
Option B earnings	-0.138*** (0.036)	-0.133*** (0.037)	0.107*** (0.036)	0.147*** (0.052)
gap large (0/1)	2.066** (0.951)	0.167 (1.39)	2.583 (1.943)	0.294 (2.145)
period	0.120 (0.183)	-0.178 (0.162)	0.002 (0.134)	-0.111 (0.183)
stranger treatment (0/1)	2.603 (3.779)	-4.618 (4.27)	1.202 (3.24)	-7.839** (3.775)
Belief Option B was Player 1's initial choice (0/1)	3.31* (1.939)	0.696 (2.669)	1.92 (2.118)	2.263 (2.502)
Belief Player 1 delegated decision (0/1)	2.665 (1.86)	-1.45 (2.436)	3.951** (1.726)	0.572 (2.852)
N	468	468	234	234

Notes: Standard errors clustered by individuals in parentheses * p<0.10, ** p<0.05, *** p<0.01; Tobit regressions include controls for age, gender, lab experience, and study major.

Result 3: *While a nontrivial fraction of subjects (23.1 percent) never sends any transfer to the decision-maker, the majority of subjects is conditionally cooperative and sends rewards which increase in their own earnings.*

Setting an upper limit does not impose a restriction on most subjects' choices (there are only 3.56 percent of 25 point transfers) but we observe a large fraction of zero transfers. In order to take into account that the excess zeroes could be generated by a different process than the transfer choices, we estimate a two-part model with a probit regression in the first stage, and OLS in the second stage. The results of the two-part estimation are consistent with the two-limit Tobit model and can be found in Appendix B.

3.5.4 Earnings Comparison

In this section we investigate whether a strategy based on mutual gift-giving can serve as a worthwhile alternative for the two partners-in-crime of a bribery relationship. To this end, we make several comparisons of the average per-round earnings for each player and the total sum of per-round earnings, displayed in Table 3.8. In column (1) we see the players' payoffs for the hypothetical situation that the experiment had ended after the first stage. In other words, this is an earnings comparison based on the decision-maker's social preferences in the absence of strategic considerations. By construction, the decision-maker earns exactly 100 points each round. The other two players earn significantly more but their earnings are statistically indistinguishable across roles (p-value = 0.979), which reflects that most decision-makers want to maximize social welfare and opt for the efficiency enhancing option, but have no intention to discriminate against any of the players.

Table 3.8: Comparison of Average Earnings per Round by Experimental Roles

Player	(1) Hypothetical earnings given Stage 1 choice of Player 1	(2) Earnings without incentive questions and transfers	(3) Earnings without incentive questions
1	100	100	111.96
2	117.91	109.72	105.44
3	117.86	92.20	90.81
Total Earnings	335.77	301.92	308.21
p-value for test of H_0: identical earnings for 2 and 3	0.979	<0.001	<0.001

However, after the gift-giving opportunity is revealed, an entirely different picture emerges. Column (2) contains the actual average "raw" earnings (i.e. net of transfer payments and points for correct guesses in the incentivized questions) after stage 2. In the absence of transfers, the decision-makers' earnings remain at 100 points per round. In contrast, Players 2 and 3 now earn less than before, and a large and statistically highly significant (p-value < 0.001) gap has opened up between them. This demonstrates that with potential rewards the decision-maker now clearly favors Player 2 at the expense of Player 3, even though it drastically reduces total welfare. From column (3) it becomes evident that this strategy actually pays off for the subjects in the role of Player 1, because when transfers are taken into account they can increase their earnings from 100 points to almost 112 points. This leads us to our final result which concludes this section.

Result 4: *Compared to a situation in which the decision-maker acts in the best interest of all players, the introduction of a favor vs. reward exchange increases the payoff for Player 2 relative to Player 3, but reduces aggregate welfare because of negative externalities for the disadvantaged party.*

3.6 Conclusion

Our experiment demonstrates that even when there is no feasible coordination mechanism between a potential briber and a bribee, the anticipation of an uncertain future reward can lead to biased decisions of a supposedly neutral decision-maker. The role of expectations is sufficiently strong to produce an outcome similar to what might have been expected by conventional corruption. This shows that even a non-contractible exchange of gifts can serve as a viable bribery substitute for lobbying parties who prefer to refrain from illegal acts. However, this favor trading leads to negative externalities for the less influential side and reduces aggregate welfare.

From a policy perspective, the results suggest to broaden the focus of anti-corruption policies to include measures which increase the uncertainty that a favor can ever be reciprocated. Especially the imposition of a waiting period between leaving a political office and taking up a private job and the prolongation of existing waiting periods should be considered. What these results also suggest, however, is that even when lobbying is regulated with clearly defined rules it might be ineffective if lobbyists and decision-makers circumvent the limitations by pursuing the strategy we examined in this experiment.

Appendix A: Options and Choices

Table 3.9: Overview of Options and Choice Frequencies

	Payoffs with **Option B** for Player			Efficiency relative to Option A	Absolute gap between 2 and 3	% of Players choosing **Option B**		
Round	1	2	3			1	2	3
1	100	120	80	0	40	10.26	94.87	2.56
2	100	30	150	–20	120	12.82	0	84.62
3	100	130	90	+20	40	28.21	97.44	10.26
4	100	110	70	–20	40	7.69	76.92	0
5	100	160	40	0	120	2.56	87.18	0
6	100	170	50	+20	120	15.38	94.87	5.13
7	100	70	110	–20	40	5.13	2.56	71.79
8	100	90	130	+20	40	28.21	12.82	97.44
9	100	80	120	0	40	7.69	0	97.44
10	100	50	170	+20	120	28.21	2.56	94.87
11	100	40	160	0	120	7.69	0	89.74
12	100	150	30	–20	120	10.26	87.18	0
Mean	100	100	100	0	80	13.67	46.37	46.15

Note: in each session the order was randomly determined.

Appendix B: Additional Regression Results

Table 3.10: Two Part Model for Transfer Decisions

Part 1: Probit (transfer>0)

	Transfer if outcome is			
	Option A		Option B	
	Player 2 (1)	Player 3 (2)	Player 2 (3)	Player 3 (4)
constant	-0.325 (1.423)	2.508 (2.253)	-2.493 (1.748)	0.702 (2.831)
Option B earnings	-0.012*** (0.002)	-0.008*** (0.002)	0.0055 (0.007)	0.007 (0.008)
gap large (0/1)	0.122 (0.095)	-0.104 (0.107)	0.0369 (0.293)	0.042 (0.291)
period	-0.002 (0.019)	-0.019 (0.0148)	0.006 (0.026)	-0.009 (0.023)
stranger treatment (0/1)	0.196 (0.408)	-0.357 (0.351)	0.5053 (0.493)	-0.937** (0.428)
Belief Option B was Player 1's initial choice (0/1)	0.170 (0.408)	-0.0263 (0.180)	0.267 (0.310)	0.320 (0.334)
Belief Player 1 delegated decision (0/1)	0.131 (0.186)	-0.166 (0.199)	0.119 (0.274)	-0.073 (0.337)
N	468	468	234	234
Log-likelihood	-253.6	-116.3	-233.2	-139.5

Part 2: OLS conditional on transfer > 0

	Transfer if outcome is			
	Option A		Option B	
	Player 2 (5)	Player 3 (6)	Player 2 (7)	Player 3 (8)
Constant	13.57 (8.432)	73.42*** (13.67)	0.509 (8.94)	15.94 (12.64)
Option B earnings	-0.041** (0.016)	-0.050** (0.019)	0.142*** (0.036)	0.181*** (0.058)
gap large (0/1)	2.200** (0.900)	1.760 (1.059)	1.224 (1.701)	-1.450 (2.218)
period	0.152 (0.167)	-0.004 (0.200)	0.014 (0.117)	0.138 (0.173)
stranger treatment (0/1)	1.087 (2.515)	3.474* (1.817)	-0.408 (2.077)	0.485 (2.203)
Belief Option B was Player 1's initial choice (0/1)	3.484** (1.522)	1.733 (1.959)	0.463 (1.063)	-0.276 (1.686)
Belief Player 1 delegated decision (0/1)	1.885* (1.009)	0.036 (1.151)	3.037*** (0.996)	0.952 (1.747)
N	168	165	118	124
R-squared	0.286	0.426	0.527	0.360

Notes: Standard errors clustered by individuals in parentheses * p<0.10, ** p<0.05, *** p<0.01;
Regressions in both parts include controls for age, gender, lab experience, and study major.

Summary: Conditional on Option A being the outcome, there is a negative and highly significant relationship between the probability of sending a positive transfer and the foregone earnings under the alternative option (columns 1 and 2). In other words, Players 2 and 3 refuse to reward the decision-maker for Option A if they would have preferred the counterfactual. We have seen before that subjects generally transfer more under B, but the binary transfer choice does not depend significantly on the points earned (column 3 and 4). In Part 2, we again observe the same "punishment" pattern as under A (columns 5 and 6). Conditional on B, the amount earned is now a highly significant predictor for the transferred points (columns 7 and 8), so the more an outcome is beneficial for Player 2, the higher is the expected reward for the decision-maker. As in the one-step estimation, none of the other covariates has a consistently significant influence on any of the two choice components.

Appendix C: Instructions

Welcome to the experiment and thank you for your participation! Please read the following instructions carefully. They are identical for all participants, so you will receive the same information as the other participants. The decisions that you and others make in this experiment will determine your earnings, which will be paid to you in cash at the end of the experiment. In addition, you will receive 4 Euros for showing up in time.

During the experiment you are not allowed to communicate with others, use mobile devices, or run other programs on your PC. If you fail to comply with these rules, we have to exclude you from the experiment and all the payoffs. If you have a question, please raise your hand. We will then come to your seat and answer your question in private. If the question is relevant for all participants, we will repeat and answer the question for all participants.

During this experiment we will refer not to Euros, but to points. At the end of the experiment your total points over all rounds will be converted to Euros at an exchange rate of

150 points = 1 Euro

The experiment consists of two parts. In the first part you will have to make a number of decisions. Which of them are relevant for your payoff will be determined in the second stage. You will be informed about the rules for the second part after the first part is completed.

Instructions for Part 1:

In this experiment there will be three roles, which we will refer to as Player 1, Player 2, and Player 3. You will be randomly allocated to one of these roles at the beginning of the experiment, and remain in the same role until the end.

The decision situation:

In this experiment you will have to make a series of decisions. Each decision consists of a choice between 2 possible options: Option A and Option B. The two options denote different possible payoffs for each player involved.

Example 1:

	OPTION A	**OPTION B**
Player 1	100	100
Player 2	100	120
Player 3	100	80

With Option A each person earns 100 points. With option B, Player 1 earns 100 points, Player 2 earns 120 points, and Player 3 earns 80 points.

Before you start, the computer will randomly determine your role. Then you will make a series of decisions in which Option A will always result in 100 points for all players. With Option B the payoffs will vary in each round. Depending on your role, you may prefer either Option A or Option B. Whose decision will be relevant for your earnings will only be determined in the second part of the experiment.

Test:

	OPTION A	**OPTION B**
Player 1	100	100
Player 2	*???*	40
Player 3	100	160

Suppose you are **Player 3**: How many points would you get with **Option B**? _____

Suppose you are **Player 1**: How many points would you get with **Option A**? _____

Suppose you are **Player 2**: How many points would you get with **Option B**? _____

Suppose you are **Player 2**: How many points would you get with **Option A**? _____

Instructions for Part 2:

In this part of the experiment it will be determined which option will be paid in which round. You can earn additional points by correctly answering some questions about which decisions you expect others to have taken. The sum of your points over all rounds constitutes your earnings.

On your screen you will now see the same decision situations as in the first part, in identical order. As before, you will see the decision situation on the left hand side. On the right hand side you can make your decisions for this part and answer the questions to increase your payoff. You will not receive any information about the decisions of other participants, neither during nor after the experiment.

[Only Stranger:] For each round, groups consisting of one Player 1, one Player 2, and one Player 3 will be randomly formed. In each round, the groups will be formed anew.

[Only Partner:] In the first round of this part, a group consisting of one Player 1, one Player 2, and one Player 3 will be randomly formed. You will remain in this group until the end of the experiment.

The Decision of Player 1:

The person which decides about the option to be implemented is Player 1. As you have seen in the first part, Player 1 earns 100 points regardless of which option is chosen.

Now Player 1 has two possible choices:

1. He/she can decide that his/her initial choice from Part 1 remains valid
2. He/she can decide that instead the choice of Player 2 from Part 1 will be valid (without knowing, which option has actually been by Player 2).

Example 2:

	OPTION A	**OPTION B**
Player 1	100	100
Player 2	100	120
Player 3	100	80

Suppose Player 1 has chosen Option A in Part 1, while Player 2 has chosen Option B.

Player 1 will now receive the following information on the screen:

YOUR DECISION WAS A

Do you instead prefer the choice of Player 2 to be valid?

☐YES

☐NO

If Player 1 opts for NO, his/her initial choice remains valid. Here this would be option A. In this case, each player in the group earns 100 points.

If Player 1 opts for YES, the initial choice of Player 2 will become valid. Here this would be option B. In this case, Player 1 earns 100 points, Player 2 120 points, and Player 3 80 points.

The Decision of Player 2 and Player 3:

After each round, the person with the highest earnings in this round (or both Player 2 and Player 3, in case they earn the same) can transfer part of their earnings to Player 1.

Player 2 and Player 3 will have to make this decision without actually knowing, which option will be valid in a particular round. In other words, both decide how much they want to transfer to Player 1 if Option A will be relevant for payoff, AND how much they want to transfer to Player 1 if Option B will be relevant for payoff.

If Option A is relevant for Payoff:

In this case, Player 2 and Player 3 earn 100 points each. Both earn the same, so both can make a transfer between 0 and 25 points to Person 1. The transfers of Player 2 and Player 3 are multiplied by 1.5 (i.e. the average transfer is multiplied by 3), and transferred to Player 1.

If Option B is relevant for Payoff:

Case 1: Player 2 earns more with Option B than Player 3 (as in example 1)

In this case, Player 2 can transfer between 0 and 25 points to Player 1. The transfer will be multiplied by 3 and transferred to Player 1. Player 3 has no decision to make.

Case 2: Player 3 earns more with Option B than Player 2 (as in the following example)

Example 3:

	OPTION A	**OPTION B**
Player 1	100	100
Player 2	100	80
Player 3	100	120

In this case, Player 3 can transfer between 0 and 25 points to Player 1. The transfer will be multiplied by 3 and transferred to Player 1. Player 2 has no decision to make.

After each round, you will be asked to guess what the other players in your group decided, and you can earn additional points for each correct guess. If you are Player 1 you will be asked to guess which transfers Player 2 or Player 3 made. As Player 2 and Player 3 you will be asked to guess which option Player 1 has initially chosen and whether he/she has decided to stick to his initial choice. Think carefully before you answer – each correct guess will earn you 10 additional points.

Summary:

Player 1 decides for each decision situation whether his/her initially chosen option will be relevant for payoff, or whether instead the option chosen by Player 2 is relevant.

Player 2 and Player 3 decide for each decision situation how many points (0-25) they want to transfer to Player 1, both in case that Player 1 has chosen Option A and in case Player 1 has chosen Option B.

[Only Stranger:] In each round you will be randomly allocated to a new group of three.

[Only Partner:] In each round you will interact with the same group members.

After the last round, all your points will be added up and converted. Then you will have to complete a short questionnaire and you will receive your earnings.

Consider again some of the examples from above:

Example 1 (cont'd):

	OPTION A	OPTION B
Player 1	100	100
Player 2	100	120
Player 3	100	80

Option A will be the valid option

- If Player 1 has initially chosen Option A and decides in Part 2 that it remains valid
- If Player 1 decides that instead the choice of Player 2 is decisive, and Player 2 has initially chosen Option A.

In this case, both Player 2 and Player 3 can make a transfer between 0 and 25 points to Player 1, e.g. transfer of Player 2: 5 points and transfer of Player 3: 15 points

Both transfers are multiplied by 1.5 and the earnings from this round are:

- Player 1: 100 + 1,5*5 + 1,5*15 = 130
- Player 2: 100 – 5 = 95
- Player 3: 100 – 15 = 85

Option B will be the valid option

- If Player 1 has initially chosen Option B, and decides in Part 2 that it remains valid
- If Player 1 decides that instead the choice of Player 2 is decisive, and Player 2 has initially chosen Option B.

In this case, Player 2 earns more than Player 3 and can make a transfer between 0 and 25 points to Player 1, e.g. transfer of Player 2: 25 points

The transfer of Player 2 is multiplied by 3 and the earnings from this round are:

- Player 1: 100 + 3*25 = 175
- Player 2: 120 – 25 = 95
- Player 3: 80

Example 3 (cont'd):

	OPTION A	OPTION B
Player 1	100	100
Player 2	100	80
Player 3	100	120

Suppose Option A is valid. In this case, both Player 2 and Player 3 can transfer between 0 and 25 points to Player 1, e.g. transfer of Player 2: 0 points and transfer of Player 3: 20 points

Both transfers are multiplied by 1.5 and the earnings from this round are:

- Player 1: 100 + 1,5*0 + 1,5*20 = 130
- Player 2: 100 – 0 = 100
- Player 3: 100 – 20 = 80

Now suppose that instead Option B is valid. In this case, Player 3 earns more than Player 2 and can make a transfer between 0 and 25 points to Player 1, e.g. transfer of Player 3: 6 points The transfer of Player 3 is multiplied by 3 and the earnings from this round are:

- Player 1: 100 + 3*6 = 118
- Player 2: 80
- Player 3: 120 – 6 = 114

4 Risk Attitudes and Medicare Part D Enrollment Decisions

4.1 Introduction

Risk preferences are crucial for understanding individual choices whenever agents make economic decisions under uncertainty. Consequently, the demand for insurance coverage as a means of reducing or eliminating uncertainty depends on the individual attitude to risk. Standard economic theory predicts that risk averse individuals will always opt for insurance when it is actuarially fair, and will generally have more insurance coverage than risk seeking agents. This chapter investigates the link between risk aversion and medical insurance coverage in the context of Medicare Part D, which was introduced in the United States in the beginning of 2006. It offers insurance coverage against "catastrophic" costs for prescription drugs and is targeted to Americans aged 65 and older.

Since the main objective of Medicare Part D is to reduce the number of persons without health insurance coverage, the program is generously subsidized so that the vast majority of the eligible population unequivocally benefits from it. However, for individuals with currently low expenses for pharmaceutical drugs it may be optimal not to enroll if they expect their drug bill to remain low for the near future. Here is where risk aversion comes into play. Future expenses for pharmaceutical drugs are uncertain and current expenses or health status are only imperfect indicators for the future (e.g., Winter et al., 2006; Heiss et al., 2010). This is especially true for people around retirement age, whose health status is likely to decline over time. One might therefore expect that highly risk averse persons choose to enroll even when they are still in relatively good health. On the other hand, individuals who perceive the risk of catastrophic drug costs to be tolerably small may decide to defer enrollment in Medicare Part D to a future date and remain uncovered for the moment. A series of papers investigate enrollment decisions and plan choice in Medicare Part D, but as Ketchman et al. (2011) note, little is known about the role of risk attitudes.

Medicare Part D provides an excellent opportunity to study if this self-selection process is actually at work. In 2006, the Part D standard plan, as formulated by the Centers for Medicare and Medicaid Services, had an annual premium of $444 and a deductible of $250. For prescription drug costs of up to $2,250 it paid 75% of the pharmacy bill. Between $2,250 and $5,100 there was a “donut hole” without additional benefits, but the standard plan covered 95% of the costs above $5,100.[22] With these features, the break-even point for enrollment in 2006 was prescription drug costs of $842. Winter et al. (2006) calculate that only 27% of the Medicare-eligible population fell below this level. However, because of a late enrollment penalty (a 1% increase in premium for each month of delay), enrollment was dynamically efficient even for many with a slightly lower bill. Non-enrollment was only optimal for currently healthy individuals who expected expenses for prescription drugs to remain low in the near future. Many Americans in the target population did not have to make an active decision because they had prescription drug coverage via their employer, which was automatically converted to Part D coverage. Only those who did not fall into this category had to make a choice of whether to enroll or not. We will refer to these persons as “active deciders”. A more detailed overview of the institutional features of Part D is provided for example in Winter et al. (2006), Duggan et al. (2008), Duggan and Scott Morton (2010), or Neuman and Cubanski (2009).

For our purposes, a particularly attractive feature of the design of Part D is the late enrollment penalty, which ensures that hardly anybody should find it optimal not to enroll in the inaugural period only to obtain Part D coverage shortly afterwards. Therefore, almost everybody who decided not to enroll during the first six months must have done it with the expectation of remaining uncovered at least until the next official enrollment period. Furthermore, since

[22] The details of the standard benefit plan are adjusted annually. For example, for 2012 the deductible has been set to $320 and the “donut hole” started at $2,930.

enrollment in a Part D plan was strictly optimal for drug costs exceeding $842 per year it should largely be independent of income or wealth – quantities for which it is usually difficult to obtain reliable information in surveys.

In our analysis we focus on the role of risk aversion in the decision of whether to sign up for Medicare Part D. The determinants of enrollment in Part D have been examined by Heiss et al. (2010) and Levy and Weir (2010). The former study used the Retirement Perspectives Survey (RPS), while the latter employed the Health and Retirement Study (HRS) which is both larger and contains more background variables than the RPS, but is less detailed with respect to information related to Part D. Both studies find that current prescription drug use and poor or very poor self-assessed health are important determinants for enrollment. Levy and Weir also show that enrollment is positively related to a proxy for IQ. Apart from that, the take-up decision seems to be unrelated to most other background variables, in particular variables reflecting educational attainment, income and wealth.[23]

In the analysis of insurance decisions, the role of individual heterogeneity in preferences, for example risk aversion, has so far often been neglected. As Amy Finkelstein commented: "An important direction for future work – both for Medicare Part D and for other social insurance programs more generally – is distinguishing between 'true' preference heterogeneity and failures of rationality" (Finkelstein, 2010). If differences in risk preferences can partly explain enrollment choices, this also has important implications for the benefits of allowing individuals to choose within social insurance. As there is also individual heterogeneity in risk perceptions, these need to be separated from risk aversion (Spinnewijn, 2009). In the survey from which our data are taken, risk preferences were elicited by means of hypothetical lotteries with an objectively quantifiable risk in order to eliminate differences in perceptions.

The introduction of Part D was accompanied by the launch of the Retirement Perspectives Survey which surveyed a large sample of older Americans immediately before the initial enrollment period started on November 15, 2005.[24] Survey participants were re-interviewed shortly after the initial enrollment period ended on May 15, 2006. The RPS contains two questions which are intended to reflect an individual's attitude towards risk. One is a standard

[23] A series of recent papers studies individuals' plan choice conditional on enrollment (Abaluck and Gruber, 2011a,b; Ketcham et al., 2011; Kling et al., 2011; Heiss et al., 2012). Depending on the region where they live, individuals who have decided to enroll can choose among around 40 different plans that differ across various dimensions, such as premium, copay and coinsurance regulations, and coverage in Part D's infamous "donut hole". These papers generally show that plan choices are far from optimal but the assessment of how large this problem is varies across these studies.

[24] https://www.cms.gov/MedicarePresDrugEligEnrol/Downloads/PDPEnrollmentGuidanceUpdateFINAL2010.pdf

"textbook" lottery with a 50:50 chance of winning a monetary price, which elicits the respondents' willingness-to-accept (WTA) for such a lottery. The other question elicits the willingness-to-pay (WTP) for a hypothetical insurance against a 5% shortfall risk. These questions account for the different behavior of people in the gain and loss domain (Kahneman and Tversky, 1979). Our main substantive finding is that those respondents who are risk tolerant according to these measures were significantly less likely to enroll in Part D.

An important survey design question is whether a general measure of risk aversion has higher predictive power than a context-specific measure. Our results confirm previous research which finds that context-specific measures are superior when employed in the context at hand. A special feature of the RPS which we also exploit is that both risk preference measures were asked in two ways: first in a bracketed format and afterwards as an open-ended question. Since the sample was randomly split by half into different bracketing conditions for both questions, this two-step procedure allows us to explore whether differential priming leads to the emergence of different reference points. When considering risk aversion as a potential predictor for enrollment into Medicare Part D we find that even minor differences in the priming of respondents would lead to different conclusions.

This chapter is organized in the following way: in Section 4.2 we discuss related literature about the association between risk attitudes and behaviors in the health domain. Section 4.3 presents the relevant section of the questionnaire. In Section 4.4 we explain our risk measures and present an overview of descriptive results and framing effects. In Section 4.5 we apply the risk preference questions to Medicare Part D enrollment decisions, and Section 4.6 concludes.

4.2 Measures of Risk Preferences and Health Behaviors

While the evolution of a person's health status over time is highly uncertain, individual behavior can have a considerable impact upon its expected path. Economists have been investigating if an individual's aversion to risk is actually reflected in a tendency to engage in risk reducing activities (e.g. preventive medical checks) or to avoid risk enhancing behaviors (e.g. smoking, heavy drinking, or consuming unhealthy food). Anderson and Mellor (2008), who conducted a large-scale experiment to examine the association between risk attitudes and risky health behaviors, show that smoking, heavy drinking, and obesity are significantly less prevalent among risk averse persons. They elicit risk attitudes using the procedure suggested by Holt and Laury (2002).[25] Picone et al. (2004) use data from the HRS and find no significant correlation between risk aversion and the demand for preventive medical tests. Their measure of risk aversion is the willingness to engage in hypothetical gambles about lifetime labor income.

Dohmen et al. (2011) investigate how a self-assessed measure for risk attitudes predicts risk taking in different domains. They use data from the German Socioeconomic Panel (SOEP) where respondents rated their willingness to take risks in general and in specific domains, such as financial investments and health, on a scale from 1 to 10. The willingness to take risks in general, and even more so the willingness to take health risks, is positively and significantly correlated with smoking, while self-reported risk taking in other domains has less predictive power. Heiss et al. (2011) included self-assessed risk attitude measures based on these SOEP questions in a predictive regression model for Part D enrollment (using a different wave of the dataset we analyze in this chapter) and found no significant effect.

Related to the question of whether risk attitude is a general character trait or varies across domains is the study of van der Pol and Ruggeri (2008) who investigate whether an individual's risk attitude in hypothetical gambles for life years differs from the risk attitude in quality of life gambles. They find that individuals tend to be risk averse with respect to the gamble involving risk of immediate death, but risk seeking with respect to other health gambles.

The relationship between risk aversion and health insurance coverage was examined by Barsky et al. (1997) and Giuso and Paiella (2006). Barsky et al. show that the probability of being covered by health insurance or life insurance increases with the respondents' aversion to risk as measured by the hypothetical income gambles in the HRS. In contrast, Guiso and Paiella (2006) obtain the finding that risk averse individuals are significantly *less* likely to have health insurance coverage. However, their measure of risk aversion is a question about the willingness-to-pay for a hypothetical risky asset from the Italian Survey of Household Income and Wealth, which has several problems. The framing in an investment context probably explains the excessive proportion of risk averse choices (96%). Only 3.6% of answers indicated risk neutrality, and only 0.55% reflected risk tolerance, which is unreasonably low even in financial contexts. Furthermore, while this risk preference measure can explain portfolio composition it probably does not accurately reflect risk aversion in non-financial

[25] For the use in sample surveys, this procedure might be less well suited since it requires more time than what is usually available.

contexts. Finally, many respondents perceived the question as too difficult and refused to answer. None of these problems arises with the two risk preference measures in the Retirement Perspectives Survey, which will be described in the next section.

4.3 The Retirement Perspectives Survey

We use data from the Retirement Perspectives Survey which was specifically designed to study the introduction of Medicare Part D in 2006. The RPS was conducted in four waves in 2005, 2006, 2007 and 2009 by Knowledge Networks, a commercial survey firm which administers a large panel of households. Panel members are representative for the U.S. population in terms of demographic and socioeconomic characteristics. They are periodically surveyed via a web TV hardware device and receive financial rewards for their participation. The objective of the RPS was to collect information about the responsiveness of older Americans to the introduction of the new Medicare Part D program during the first months of 2006, their plan choices and their experiences with the program. Heiss et al. (2010) provide a detailed description of the RPS. The analysis below uses data from the first two waves with 2598 observations. Respondents were between 50 and 97 years of age at the beginning of the survey. A summary of demographic characteristics can be found in Appendix B, Table 4.4. For analyzing the enrollment decision, we only consider those 443 respondents who had to make an active choice. Summary statistics for this subsample are displayed in Table 4.4 in Appendix B.

In addition to questions about Medicare Part D, health conditions and socio-demographic status, the RPS also contains a section on risk preferences. It includes two hypothetical lotteries which differ not only in probabilities and expected payoffs, but also in the context. In addition, one lottery is framed in terms of a potential gain, while the other is about a potential loss. This accounts for the markedly different behavior in situation involving gains and losses (Kahneman and Tversky, 1979). In order to investigate potential bracketing effects, the sample is split in half and both groups received different brackets for each lottery. The relevant section of the questionnaire can be found in Appendix A.

In each of the two risk preference questions the respondents had to indicate which certain amount would leave them indifferent with the risky lottery. The main virtue of question A1, the first measure of risk preference, is its simplicity. Participants were asked the following question:

> *"Suppose your supermarket announces that in a drawing from its customers you are the winner of the grand prize. This prize gives you a fifty-fifty chance of getting $10,000 or getting nothing. Alternately, you may choose an amount for certain rather than taking a chance of getting nothing.*
>
> *Please indicate for each of these amounts whether you would take it for certain (rather than taking a fifty-fifty chance of getting $10,000 or nothing): "*

In question A1 a decreasing dollar sequence was presented and respondents had to decide for each amount if they preferred to accept this amount instead of playing the lottery. In addition, respondents were randomly assigned to one of two conditions. The wide brackets treatment presented a broader range of alternative dollar amounts ($6,500, $5,000, $3,500, $2,000), while the alternatives in the other treatment covered a narrower range (with amounts $6,000, $5,500, $5,000, and $4,500). Clearly, a person who accepts $6,500 dollars instead of playing the lottery but prefers the lottery for lower amounts is less risk averse than another person who finds a certain amount of $5,000 still better than taking a risk and playing the lottery.

In order to have a comparable measure across treatments and to fully account for individual heterogeneity, participants then had to answer the same question in an open-ended format (question A2), i.e. they had to state the minimum amount which would just be sufficient to forego the lottery. The answer to this open-ended question will serve as our first risk preference measure. The expected value of $5,000 corresponds to risk neutrality, while a WTA below (above) $5,000 indicates risk aversion (tolerance). The payoffs and probabilities in this hypothetical lottery are easy to understand, and even people without a sophisticated mathematical background should intuitively grasp that the lottery pays $5,000 in expectation. Another feature of question A2 is that it is basically context-free.

In contrast, the second risk measure B1 is explicitly framed in an insurance context, although not health-related but as an insurance against the cancellation of a holiday trip:

> *"Suppose you are planning a vacation that costs $2,000 up front. There is a five percent chance that something will come up, and you will be unable to go. Your travel agent offers you insurance that will refund your $2,000 if you can't go.*
>
> *Please indicate below whether you would buy this insurance at various costs rather than taking a chance: "*

Respondents were again assigned to two bracketing treatments. The first offered a wider range ($40, $70, $100, $130) while the amounts in the second sequence ($85, $100, $115, $130) were narrowly centered around the expected value of $100. Then the same hypothetical situation was again presented as an open-ended question (B2), which provides the second measure for risk preference. Each respondent had to report her maximum willingness-to-pay for this insurance, and higher values represent an increasing aversion against risk.

From a survey design perspective, a potentially useful feature of this two-step procedure is that it helps respondents to understand the questions by providing anchors. This probably explains why the nonresponse rate is extremely low (around 2.2% for A2 and 2.0% for B2).[26] Other elicitation procedures, such as those used in Barsky et al. (1997) or Holt and Laury (2002), indicate a range for a respondent's coefficient of risk aversion. While Kimball et al. (2008) demonstrate that one can construct a cardinal proxy from these ordinal risk aversion measures, explicitly taking measurement error into account, their procedure requires repeated observations and assumptions such as risk aversion being constant over time. Instead, the RPS questions directly provide a point estimate for risk attitudes. The answers to these questions are obviously subject to noise, but as measurement error would most likely lead to attenuated coefficients in our setting, the results presented in the last section can be interpreted as conservative estimates of the link between risk aversion and Part D enrollment.

While several studies, for example Dohmen et al. (2011), suggest that a context-specific measure for risk attitude has more predictive power for actual behavior than general measures, some respondents might find the amounts and probabilities involved in B2 more difficult to process than those in A2. It is therefore difficult to form an a-priori conjecture about the relative predictive power of both questions. It turns out, however, that B2 contains more information than A2.

[26] In comparison, the question employed by Guiso and Paiella (2006) was answered by only 3458 out of 8135 respondents (or 42.5%). The remaining persons either reported "don't know", or refused to answer or pay a positive price for a hypothetical risky asset.

4.4 Construction and Properties of the Risk Attitude Measures

4.4.1 A Framework Consistent with Prospect Theory

Prospect theory (Kahneman and Tversky, 1979) is the descriptively most successful alternative to expected utility theory. One of the key predictions of prospect theory is that individuals behave risk averse with respect to potential gains, but risk seeking in situations which involve a potential loss. As we will show in the next section, the risk preference questions in the RPS reveal a picture which is consistent with this theory. Respondents are generally risk averse or risk neutral in the supermarket lottery, but mainly risk seeking in the question about the hypothetical travel insurance.

We use the following CRRA two-part power function proposed by Tversky and Kahneman (1992) to calculate risk aversion coefficients separately for the gain and loss domain:

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

where x represents the potential gain or loss, and α and β are the corresponding risk aversion coefficients for lotteries involving gains and losses, respectively. By λ we denote the coefficient of loss aversion, which specifies by how much more individuals suffer from a loss than they would benefit from an equally large gain.[27]

We calculate the coefficient α for A2 and β for B2. We do this by setting the expected utility of the lottery equal to the utility from the respondents' reported certainty equivalent (the answers in A2 and B2). By doing this, λ cancels out and we can solve numerically for α or β. Calculating risk aversion coefficients instead of using the raw answers offers the advantage of reducing the effect of outliers but entails two minor complications. First, the power utility specification results in very high α for a WTA above $9,000 and a very high β for a WTP above $1,000. To avoid extreme outliers we therefore top-code and use the same coefficient as for $9,000 also for the very few even higher answers (1 person with $9,500 and 13 with $9,999). In the same way, we use the β corresponding to $1,000 also for three outliers (1 person each with $1,500, $1,800 and $1,900), and drop respondents whose stated WTP is above $2,000, i.e. higher than the value of the trip itself. The second complication is that α and

[27] Tversky and Kahneman (1992) estimate λ to be around 2.25.

β are not defined for 0. We therefore use the same coefficient as for the lowest positive amount ($1 in both cases) also for $0 answers. We performed several robustness checks with alternative cutoff points and obtained very similar results.

To ensure that our functional form assumption is not too restrictive, we perform a functional form test with our specifications estimated in section 4.5. We add restricted cubic splines of the original answers (Stata module "rc_spline" by Dupont and Plummer) but find no evidence that a different functional specification would be preferable.

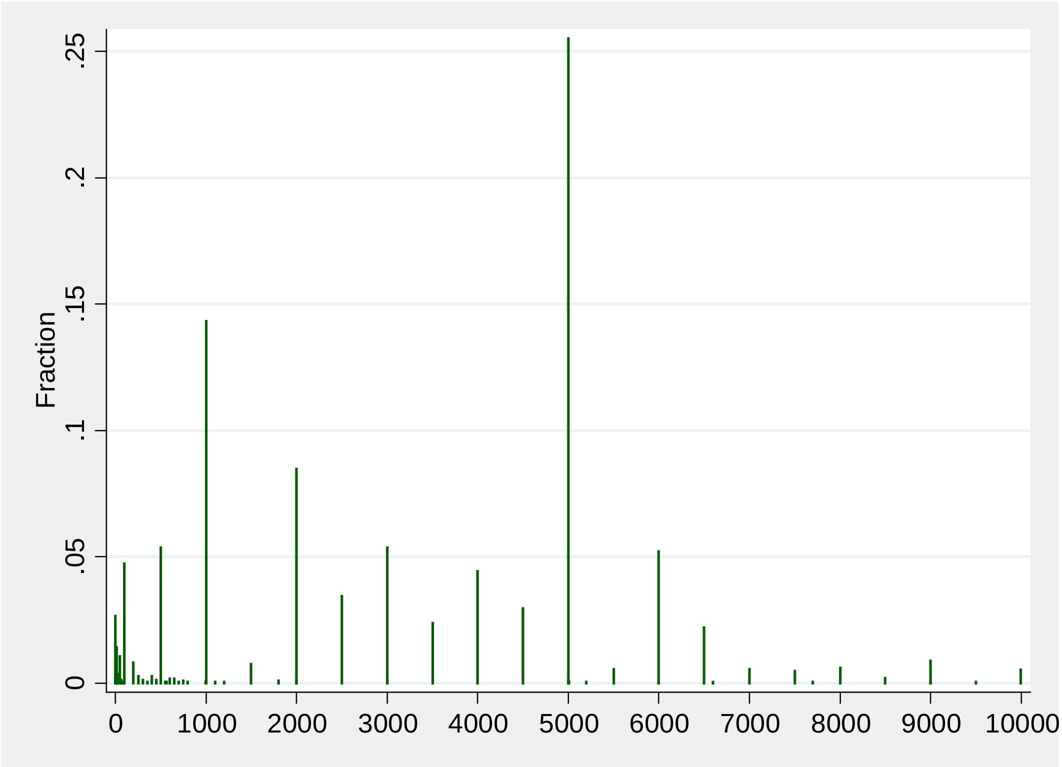
4.4.2 Descriptive Statistics

We will restrict our analysis of the Part D take-up decision in section 5 to active deciders. In this section, however, we present the descriptive results for the risk aversion questions and the framing effects for the entire sample of the RPS. From Appendix B, which contains summary statistics for both samples, we see that respondents in the active deciders sample are slightly older on average, more likely to be female, and seem to be in better health (e.g. as measured by self-rated health, the reported number of prescription drugs regularly taken, and the calculated monthly costs for these drugs). However, with respect to most variables there are only minor differences between both samples.

Figure 4.1 shows the distribution of answers to question A2. A few aspects are worth pointing out. Firstly, the majority of respondents make a risk averse choice. Around one quarter of answers indicate risk neutrality and only 11.8% in our sample are risk seeking and state a WTA of more than $5,000. Secondly, the expected value of the lottery is by far the most frequent answer. Furthermore, multiples of 1000 and 500 are focal points which attract the bulk of answers, especially when they were included in one of the bracketing treatments.

In the open-ended question, all amounts between $1 and $9,999 are not unreasonable a-priori. While indeed nobody stated a higher value, there is a nontrivial number of $0 answers (77 respondents, or 3.1%). Since a rational decision maker would not prefer a certain amount of $0 to a 50:50 chance of either winning a positive amount or getting nothing, it might be tempting to interpret $0 answers as reflecting a WTA very close to zero and correspondingly a very high degree of risk aversion. However, these answers could also be motivated by an aversion against such gambles in general. The problem of correctly interpreting these ambiguous answers is inherent in this type of lotteries and has also been encountered by Hartog et al. (2002), for example. However, we can employ our two-step procedure to shed more light on this issue. The vast majority of those with a $0 answer rejected *all* possible amounts in the preceding question, thereby indicating extreme risk aversion in the open-ended question, but a very high degree of risk tolerance in the bracketed question. This inconsistent pattern makes these answers suspicious and presumably expresses a general aversion against engaging in gambles of this type, rather than accurately reflecting an individual's attitude in risky financial decisions. While we do not drop these answers for our analysis of the determinants of Part D enrollment in Section 5, our main findings do not change when we exclude these suspicious answers.[28]

Figure 4.1: Distribution of Answers to Question A2

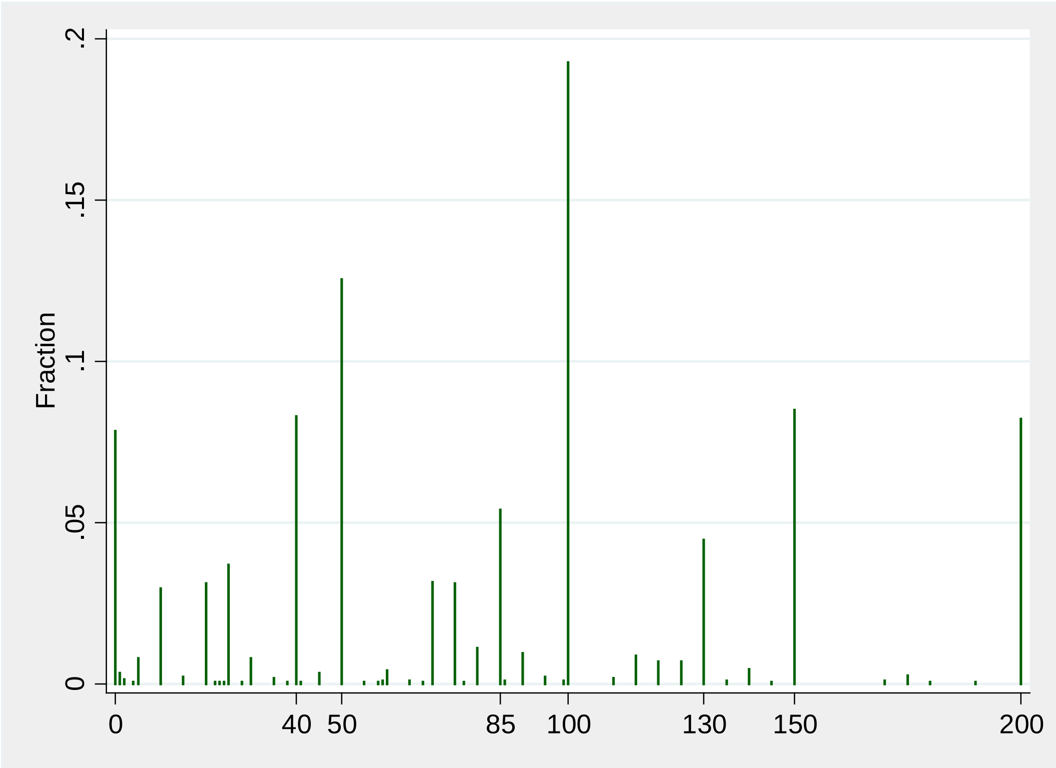


In B2 again the risk neutral choice, in this case $100, was the most frequent answer. Figure 4.2 shows the distribution which is truncated at 200 for expositional clarity. While many respondents rounded to multiples of 50, also the amounts previously seen in the bracketed questions ($40, $70, $85, $130) were frequently chosen. The majority of answers (54%) fall into the risk seeking region, which is consistent with prospect theory. However, the high

[28] Results are available upon request.

proportion of risk tolerant subjects is an interesting result in the light of previous studies which found that framing a question in an insurance context enhances risk *averse* choices (see for example Hershey et al. 1982, or Wakker et al., 2007).

Figure 4.2: Distribution of Answers to Question B2 (Truncated at 200)



Those respondents who reported a WTP of $0 in B2 were overwhelmingly consistent in the sense that they had also opted for a risk seeking choice in B1. Unlike in A2, there is therefore no reason to find $0 answers generally suspicious, and they can be interpreted as WTP very close to zero (presumably everybody would buy this insurance for 1 cent). However, there are some problematic answers on the other end of the scale which correspond to an implausibly high degree of risk aversion (for some people the reported WTP for the travel insurance was even higher than the hypothetical value of the trip itself). Again a comparison of the closed and open-ended questions can help to assess the reliability of answers. Most respondents who stated an amount between $500 and $2000 (1.57% in our sample) were by far less risk averse in the preceding bracketing question, which suggests that these outliers reflect a misunderstanding of the question rather than extremely high risk aversion.

4.4.3 Framing Effects

In order to assess whether the two bracketing conditions in A1 affect response behavior it is instructive to compare the proportion of rejections at the only amount included in both treatments, which is the lottery's expected value of $5,000. Table 4.1 shows that 14.96% of the respondents in the "wide" brackets group rejected a certain payment of $5,000 in favor of playing the lottery, compared to 18.14% in the "narrow" brackets group. While this difference is small, the answers to the following open-ended question A2 show that the priming was not without effect. The median answer in the wide brackets treatment was $2500, compared to $3500 in the narrow brackets treatment (the mean was $2920 and $3202, respectively). A Kolmogorov-Smirnov test clearly rejects the hypothesis that both samples are drawn from the same distribution (p-value < 0.001).

Preceding A2 by the bracketing treatments introduces different reference points which affect the answers to the open-ended question. Figure 4.3 shows that while both framings exhibit large spikes at $5,000 and $1,000, many respondents chose one of the numbers previously seen in A1. 252 persons (20.1%) in the wide brackets treatment stated either $6,500, $3,500 or $2,000 in A2, compared to only 79 persons (or 6.1%) in the narrow brackets treatment. Conversely, the reference points which only respondents of the narrow brackets treatment were exposed to were $6,000, $5,500, and $4,500. 192 respondents (14.9%) in this group stated one of these amounts in A2, but only 29 persons (or 2.3%) did so in the wide brackets treatment. The cumulative distribution functions in Figure 4.4 demonstrate that there are some bracketing-induced differences, especially at $2,000 and $6,000, but also at $6,500. However, bracketing effects are confined to the region between $2,000 and $6,500. The fraction and distribution of answers below or above this range are almost identical.

Table 4.1: Treatment Comparison of WTP and WTA Questions

A1: WTA for supermarket lottery

Please indicate for each of these amounts whether you would take it for certain (rather than taking a fifty-fifty chance of getting $10,000 or nothing):

	wide brackets		narrow brackets	
$	NO	YES	NO	YES
6500	10.27%	89.73%		
6000			10.86%	89.14%
5500			16.29%	83.71%
5000	14.96%	85.04%	18.14%	81.86%
4500			28.81%	71.19%
3500	36.76%	63.24%		
2000	48.44%	51.56%		

B1: WTP for travel insurance

Please indicate below whether you would buy this insurance at various costs rather than taking a chance:

	wide brackets		narrow brackets	
$	NO	YES	NO	YES
40	16.19%	83.81%		
70	41.90%	58.10%		
85			33.14%	66.86%
100	60.73%	39.27%	50.17%	49.83%
115			66.52%	33.48%
130	72.34%	27.66%	69.84%	30.16%

Figure 4.3: Answers to Question A2 by Brackets Treatments

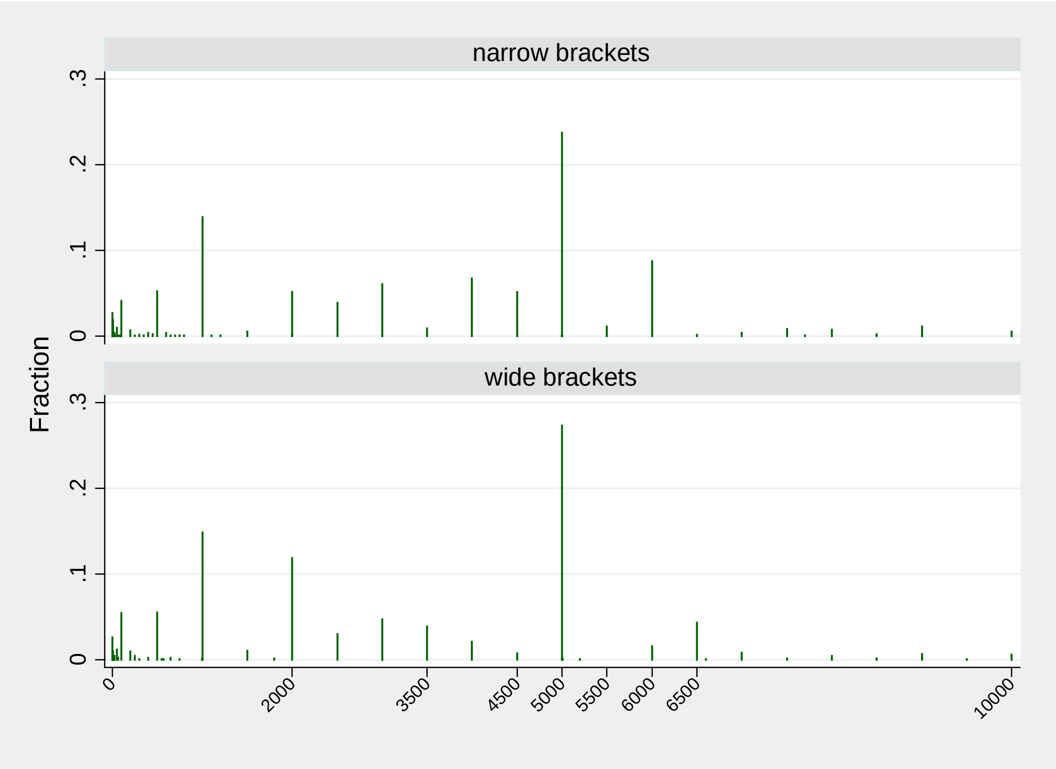
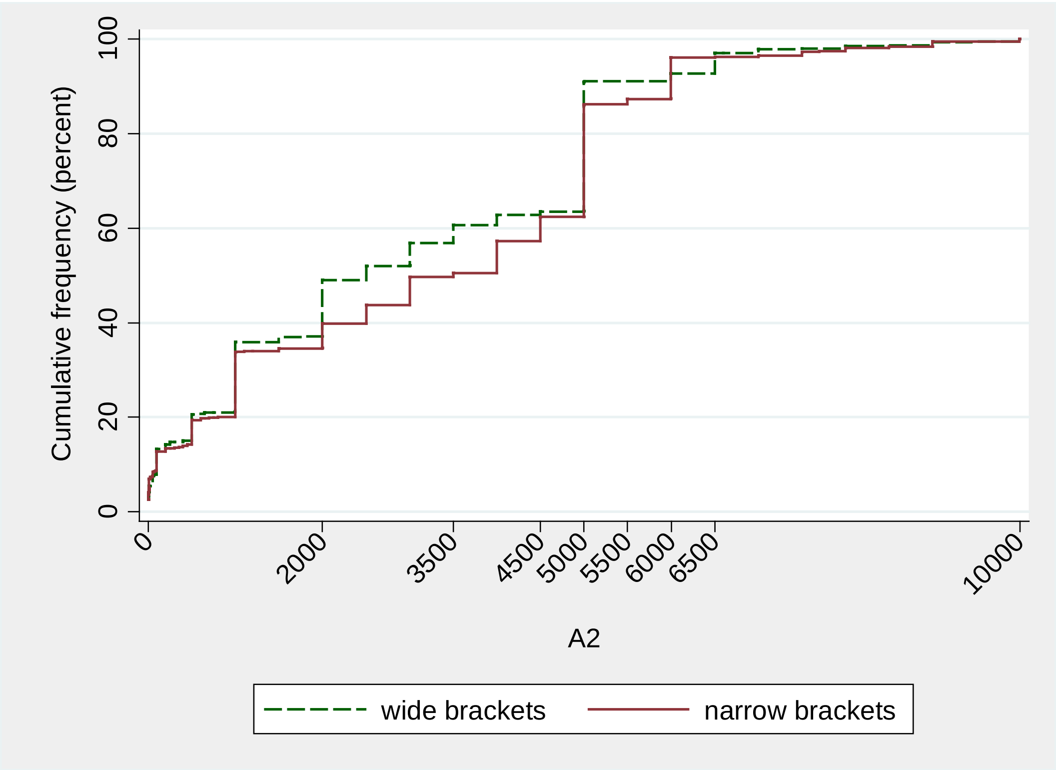


Figure 4.4: Cumulative Distribution Function for Question A2



Two-sample Kolmogorov-Smirnov test for equality of distribution functions: p-value < 0.001

With respect to B2, even casual inspection shows that the treatment effect is more pronounced (see Figure 4.5). The fact that they saw the \$85 option in B1 induces 132 persons (or 10.54%) of those in the narrow brackets condition to report exactly this number in B2. Another 21 (or 1.68%) adopted the previously seen \$115. In the wide treatment, both numbers were stated not even once. Conversely, 270 persons in the wide treatment named either \$40 or \$70 in B2, which amounts to more than 21%. In the other framing, only 0.8% chose these numbers. Both bracketing treatments included the expected value of \$100 as a cut-off point, but while 49.83% would have bought the hypothetical travel insurance for this price in the narrow brackets treatment, only 39.27% would have done so in the wide condition (see Table 4.1). The inclusion of cheap alternatives (\$40 and \$70) clearly changes the respondents' valuation of the insurance. Interestingly, there is almost no difference with respect to the fraction of answers above and below \$130, which is the second value common to both treatments. However, the cumulative distribution function (Figure 4.6) reveals that bracketing-induced distortions are not confined to the area between 40 and 130, but rather affect the entire distribution. Those persons who were assigned to the narrow brackets treatment, with \$85 as the lowest suggested value, are more likely to have a WTP below \$40. The framing effect is strongest in between 40 and 100, but there remains a difference even above 130. The treatment effect is reflected in the median answers to B2. While the respondents assigned to the narrow brackets state a median willingness-to-pay of \$100, those in the wide brackets treatment are significantly more risk-loving, with a median of \$70. Again, a Kolmogorov-Smirnov test rejects that both samples are drawn from the same distribution (p-value < 0.001).

Figure 4.5: Answers to Question B2 by Brackets Treatments (Truncated at 200)

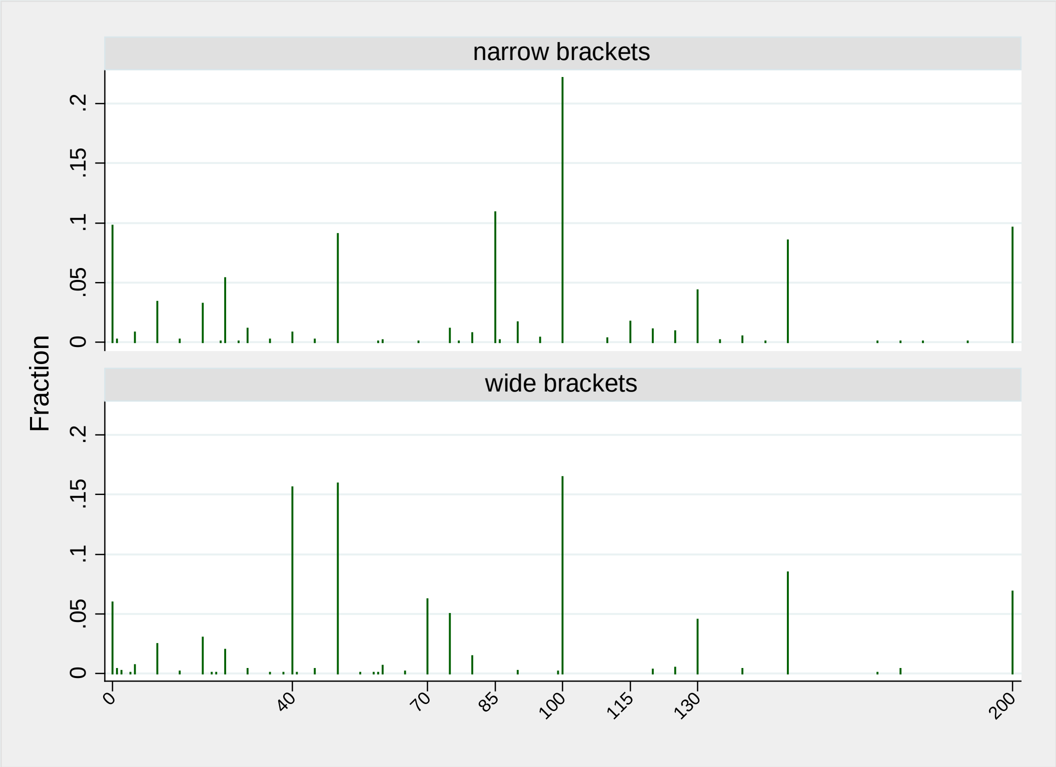
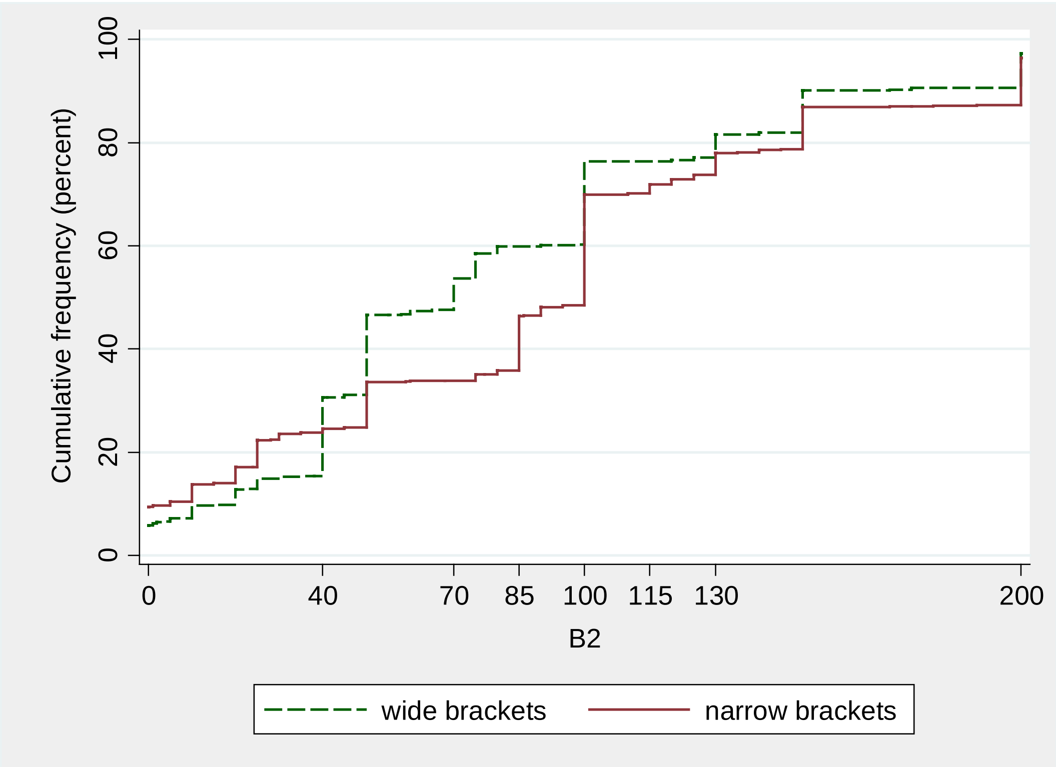


Figure 4.6: Cumulative Distribution Function for Question B2 (Truncated at 200)



Two-sample Kolmogorov-Smirnov test for equality of distribution functions: p-value < 0.001

4.4.4 Determinants of the Measures for Risk Attitudes in the RPS

Our objective in this section is twofold. First, we want to examine whether there remains a significant treatment effect when we control for covariates which have been found to be associated with risk attitudes in other studies based on different datasets. Second, we are interested in whether there is a correlation between some socio-demographic covariates and the answers to the lottery questions.

As shown in Table 4.2, the bracketing-induced treatment effects are also evident when we regress the risk preference measures (both the raw answers and the calculated risk aversion coefficients) on socio-demographic variables. In all regressions the treatment dummy is significant.

Two further conclusions emerge from Table 4.2: First, converting the raw answers into the risk aversion coefficients α and β does not substantially alter the results. Second, the association between the risk preference measures and respondent characteristics is low, especially in the case of A2. This contrast with Dohmen et al. (2011) and Hartog et al. (2002), for example, who find that women are significantly more risk averse, and that risk aversion increases with age and a measure of subjective health status, while a better education level is associated with higher risk tolerance. Both papers also report some evidence for a positive relationship between income and risk tolerance. Halek and Eisenhauer (2001) as well as Donkers et al. (2001) find similar effects for gender and education.[29] Generally we find little association between reported certainty equivalents and demographic characteristics. Columns (3) and (4) show that B2 is significantly lower for persons with education below high school level and significantly higher for nonwhite respondents, and that poor or very poor self-reported health is associated with slightly more risk averse choices. There are no significant differences across household income levels, but the income measure in the RPS is self-reported and neither informative about the source of income (e.g. labor income vs. pension income), nor about the relative contribution of the household members to the total household income or about how much is actually disposable. In contrast to the previously mentioned studies we do not find a gender effect. However, Schubert et al. (1999) also employ insurance-context lotteries to measure risk aversion in the loss domain and do not find a significant gender difference.

[29] Surveys of evidence for gender differences in risk taking can be found in Eckel and Grossman (2008), and Croson and Gneezy (2009).

Overall, the fact that A2 is not significantly associated with any of the respondents' characteristics might be indicative that it is a noisier measure than B2. On the other hand, both risk aversion measures might measure different aspects of risk aversion, which cannot be explained in the same fashion. Interestingly, within respondents α and β are virtually unrelated, with a correlation coefficient of -0.016 (p-value = 0.421).

Table 4.2: Predictors of the Risk Aversion Measures (OLS Regressions)

	(1) **A2**	**(2)** **α**	**(3)** **B2**	**(4)** **β**
constant	3179.2122*** (132.2073)	0.7468*** (0.05322)	103.8621*** (6.6897)	0.9592*** (0.0236)
A1 treatment dummy	-281.6155*** (90.8509)	-0.0732** (0.0345)		
B1 treatment dummy			-11.7355** (4.6909)	-0.0300* (0.0161)
age > 75	85.3777 (102.9833)	0.0318 (0.0402)	-2.4164 (5.2935)	-0.0081 (0.0193)
education below highschool level	-101.076 (95.4555)	-0.0220 (0.0356)	-12.9473*** (4.9274)	-0.0421*** (0.0157)
male	-41.6113 (91.2655)	0.0013 (0.0345)	-2.3736 (4.7179)	-0.0012 (0.0165)
household income<$30,000	-34.7375 (110.6319)	0.0207 (0.0401)	-4.8590 (5.7066)	-0.0150 (0.0192)
household income>$60,000	177.6066 (122.3170)	0.0529 (0.0469)	4.3549 (6.3902)	0.0291 (0.0205)
nonwhite	145.6331 (129.7351)	0.0574 (0.0497)	18.5246 *** (6.4311)	0.0580 ** (0.0277)
SAH excellent	152.2851 (191.4282)	0.0593 (0.0773)	-4.1146 (9.9175)	-0.0138 (0.0291)
SAH (very) poor	24.6384 (105.6686)	0.0348 (0.0409)	10.1463* (5.4042)	0.0405 (0.0197)
N	2541	2541	2526	2526
R^2	0.0073	0.0039	0.0124	0.0116
F	2.2303	1.3728	3.5012	3.5764

Notes: Respondents with B2>=2000 were dropped. Heteroscedasticity robust standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

4.5 Risk Aversion as a Predictor of Enrollment in Medicare Part D

Out of the 2598 respondents in our sample, 443 are active deciders and had to make a decision. 349 of them enrolled in a Part D plan, while 94 remained uncovered. Since the RPS was targeted at older Americans, respondents were explicitly asked if a family member had decided for them. Those 21 persons who reported that someone else took the decision on their behalf were consequently excluded.[30] We also dropped 15 respondents with missing answers or a WTP for B2 of $2000 or higher.

In order to uncover the role of risk aversion in the Part D enrollment process we estimate probit models in which we regress the enrollment decision of the active deciders on our measures of risk attitudes and a set of covariates. We correct for potential bracketing effects in the elicitation procedure by including a treatment dummy in each regression. As a measure for the demand of prescription drugs we use expenditures for 9 frequent symptoms, calculated by Winter et al. (2010). The regressions further includes dummy variables for having been covered by a different type of medical insurance before the introduction of Part D, age (older than 75 years), low educational attainment (less than high school), sex, income (dummies for reported income below $30,000 or above $60,000), ethnicity, and excellent and poor/very poor self-reported health.

Table 4.3 presents our estimation results. Column (1) contains the coefficients of a baseline specification without our risk aversion measures. Not surprisingly, current expenses for prescription drugs are an important determinant for enrollment. We also find that respondents who previously had prescription drug coverage which was not equivalent to Part D coverage were more likely to enroll, which probably reflects a substitution of their previous insurance coverage towards the subsidized program. As expected, persons with excellent self-reported health were more likely to remain uncovered. Socio-demographic factors such as age, income, education, gender and ethnic background are all insignificant, which is in line with Heiss et al. (2010) and Levy and Weir (2010).

Columns (2) and (3) contain estimates for the sample of active deciders using either of the two measures for risk preferences individually, and the regression in column (4) uses both measures jointly. While α has no significant influence on the probability of enrolling, β is highly significant. This holds regardless if β is used individually or jointly with α. Individuals with a higher WTP for the hypothetical travel insurance are significantly more likely to obtain Part D

[30] All of them actually enrolled.

coverage. This result confirms that risk aversion is indeed an important determinant for enrollment in Part D.[31]

A comparison of columns (3), (5) and (6) sheds more light on the effects of different bracketing treatments. Across all active deciders (column 3) β is a highly significant predictor for enrollment (p-value = 0.005). Interestingly, this result is almost entirely driven by the wide bracketing treatment (column 6); the coefficient in the narrow treatment is much smaller and far from being significant (p-value = 0.301). We also tested whether this result could be driven by a different composition of both subsamples, but find no evidence for a failure of randomization. Apart from β, there is no variable for which we can reject the hypothesis of identical distributions across treatments. The only exception is the proportion of respondents who had a different kind of medical insurance before the introduction of Part D (p-value = 0.030). Thus, our treatment effect is not driven by non-random assignment.

[31] As already mentioned in section 4.1 we tested whether a more flexible functional form would be more appropriate for the risk aversion measures, but find no evidence which rejects our functional form assumption.

Table 4.3: Determinants of Part D Enrollment (Probit Regressions)

	All active deciders				B1 narrow brackets	B1 wide brackets
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
constant	0.5054** (0.1969)	0.3821 (0.2203)	-0.2671 (0.3251)	-0.4315 (0.3438)	-0.0609 (0.4972)	-0.7694* (0.4370)
α		0.0697 (0.0903)		0.0893 (0.0953)		
A1 treatment dummy		0.1495 (0.1492)		0.1572 (0.1514)		
β			0.8092*** (0.2967)	0.8366*** (0.2967)	0.4412 (0.4416)	1.2542*** (0.4200)
B1 treatment dummy			0.0865 (0.1506)	0.0883 (0.1511)		
prescription drug expenditure in $	0.0032** (0.0013)	0.0032** (0.0013)	0.0032*** (0.0012)	0.0032*** (0.0011)	0.0051*** (0.0017)	0.0028** (0.0014)
other medical insurance	0.6944*** (0.1716)	0.6891*** (0.1717)	0.6980*** (0.1735)	0.6942*** (0.1741)	0.3241 (0.2393)	1.1428*** (0.2739)
SAH excellent	-0.8050*** (0.2392)	-0.8047*** (0.2402)	-0.8075*** (0.2427)	-0.8095*** (0.2447)	-1.3441*** (0.3952)	-0.2465 (0.3639)
SAH (very) poor	-0.1719 (0.2029)	-0.1588 (0.2014)	-0.1887 (0.2036)	-0.1737 (0.2021)	0.0306 (0.2966)	-0.5333* (0.3007)
age > 75	0.1897 (0.1547)	0.1887 (0.1552)	0.1895 (0.1579)	0.1876 (0.1585)	0.1747 (0.2171)	0.4180* (0.2506)
education below highschool level	-0.2205 (0.1511)	-0.2122 (0.1504)	-0.2198 (0.1541)	-0.2106 (0.1539)	-0.1970 (0.2156)	-0.0958 (0.2352)
male	0.0760 (0.1530)	0.0636 (0.1532)	0.1111 (0.1544)	0.1009 (0.1547)	0.4780** (0.2195)	-0.0814 (0.2291)
household income <$30,000	-0.0703 (0.1699)	-0.0696 (0.1694)	-0.0617 (0.1719)	-0.0636 (0.1719)	-0.0485 (0.2454)	0.0307 (0.2624)
household income >$60,000	0.2873 (0.2257)	0.2930 (0.2290)	0.2408 (0.2291)	0.2434 (0.2329)	0.3268 (0.3174)	0.3215 (0.3206)
nonwhite	0.2967 (0.2675)	0.2652 (0.2678)	0.2642 (0.2691)	0.2199 (0.2660)	0.3014 (0.3714)	0.1949 (0.3936)
N	407	407	407	407	209	198
Pseudo-R^2	0.1282	0.1316	0.1466	0.1509	0.1962	0.1924
Log-pseudolikelih.	-187.467	-186.724	-183.506	-182.576	-92.434	-80.461

Notes: Dependent variable = 1 if respondent enrolled in Part D, 0 otherwise. Respondents with B2>=2000 were dropped. Heteroscedasticity robust standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

4.6 Conclusion

To the best of our knowledge, this is the first analysis which focuses on the role of risk aversion in the take-up process for Medicare Part D. We find that risk attitude for losses can explain why eligible persons decided not to enroll in Part D. Using the WTP for a hypothetical insurance from a non-health context we can show that risk tolerant respondents are indeed highly significantly less likely to obtain Part D coverage. Our analysis shows that together with low current prescription drug costs and excellent self-reported health status – two variables which directly reflect limited expected financial benefits associated with Part D – risk tolerance is the most important determinant for explaining why respondents decided to remain uncovered. Our analysis also shows that risk aversion is by far more important than socio-demographic characteristics to explain non-enrollment in Medicare Part D. These findings underscore the importance of heterogeneity of risk attitudes in the analysis of insurance markets, as argued by, inter alia, Finkelstein (2010) and Spinnewijn (2009).

From a survey design perspective, this analysis suggests a two-step procedure which provides anchoring values in a first step and fully exploits individual heterogeneity in the second step. Doing this achieves the goal of a very low non-response rate and enables us to perform consistency checks for questionable answers by comparing the responses from the open-ended and the bracket format. Whether such a two-step procedure is an efficient way to increase response rates and response quality in the context of a time-constrained internet survey, and how it compares to the same measure elicited in one step, is a question which merits further research.

Appendix A: Questionnaire Section on Risk Preferences

Question A1:

Suppose your supermarket announces that in a drawing from its customers you are the winner of the grand prize. This prize gives you a fifty-fifty chance of getting $10,000 or getting nothing. Alternately, you may choose an amount for certain rather than taking a chance of getting nothing.
Please indicate for each of these amounts whether you would take it for certain (rather than taking a fifty-fifty chance of getting $10,000 or nothing):

Wide brackets treatment:

	No	**Yes**
$6,500	☐	☐
$5,000	☐	☐
$3,500	☐	☐
$2,000	☐	☐

Narrow brackets treatment:

	No	**Yes**
$6,000	☐	☐
$5,500	☐	☐
$5,000	☐	☐
$4,500	☐	☐

Question A2:

Still thinking about the super market prize that gives you a fifty-fifty chance of getting $10,000 or getting nothing, as just described. What is the minimum amount you would choose for certain?
Please enter the dollar amount here: $ ______________

Question B1:

Suppose you are planning a vacation that costs $2,000 up front. There is a five percent chance that something will come up, and you will be unable to go. Your travel agent offers you insurance that will refund your $2,000 if you can't go.
Please indicate below whether you would buy this insurance at various costs rather than taking a chance:

Wide brackets treatment:

	No	**Yes**
$40	☐	☐
$70	☐	☐
$100	☐	☐
$130	☐	☐

Narrow brackets treatment:

	No	**Yes**
$85	☐	☐
$100	☐	☐
$115	☐	☐
$130	☐	☐

Question B2

Still thinking about a vacation that costs $2,000 up front and assuming there is a five percent chance that you will be unable to go. What is the maximum amount you would pay for insurance that would refund the cost, as just described?
Please enter the dollar amount here: $ _____________

Appendix B: Summary Statistics for Full Sample & Active Deciders

Table 4.4: Summary statistics: Full sample (N=2598)

Variable	**Mean**	**Std. Dev**	**Min**	**Max**
Demographic Characteristics				
Age	69.96	8.06	50	97
Age > 75	0.28	0.45	0	1
Gender (1=male, 0=female)	0.45	0.50	0	1
Education below highschool level	0.55	0.50	0	1
Income < 30,000 USD	0.45	0.50	0	1
Income > 60,000 USD	0.27	0.45	0	1
Currently living with a partner	0.61	0.49	0	1
Currently married	0.58	0.49	0	1
# children > 0	0.90	0.30	0	1
Currently working (full or part-time)	0.15	0.36	0	1
nonwhite	0.17	0.37	0	1
immigrant	0.04	0.20	0	1
Health Characteristics				
Self-assessed health (1=excellent, 5=very poor)	2.92	1.02	1	5
Self-assessed health excellent (0/1)	0.06	0.24	0	1
Self-assessed health poor or very poor (0/1)	0.28	0.45	0	1
# different prescription drugs regularly taken	1.30	1.67	0	15
Avg. monthly prescription drug costs (USD)	120.96	162.59	0	1280
Other type of medical insurance (unspecified)	0.74	0.44	0	1
Risk Aversion measures				
A2	3063.36	2286.86	0	9999
α	0.76	0.86	0.08	6.58
Risk seeking for gains, i.e. A2>5000 (0/1)	0.13	0.34	0	1
B2	120.31	345.81	0	8500
β	0.95	0.41	0.39	4.32
Risk seeking for losses, i.e. B2<100 (0/1)	0.53	0.50	0	1

Table 4.5: Summary statistics: Only active deciders (N=443)

Variable	Mean	Std. Dev	Min	Max
Demographic Characteristics				
Age	72.61	6.20	64	93
Age > 75	0.34	0.47	0	1
Gender (1=male, 0=female)	0.38	0.49	0	1
Education below highschool level	0.59	0.49	0	1
Income < 30,000 USD	0.50	0.50	0	1
Income > 60,000 USD	0.21	0.40	0	1
Currently living with a partner	0.59	0.49	0	1
Currently married	0.56	0.50	0	1
# children > 0	0.91	0.29	0	1
Currently working (full or part-time)	0.13	0.33	0	1
nonwhite	0.09	0.29	0	1
immigrant	0.03	0.18	0	1
Health Characteristics				
Self-assessed health (1=excellent, 5=very poor)	2.70	0.98	1	5
Self-assessed health excellent (0/1)	0.10	0.29	0	1
Self-assessed health poor or very poor (0/1)	0.20	0.40	0	1
# different prescription drugs regularly taken	0.93	1.36	0	9
Avg. monthly prescription drug costs (USD)	82.46	129.75	0	795
Other type of medical insurance (unspecified)	0.35	0.48	0	1
Risk Aversion measures				
A2	2896.50	2188.44	0	9999
α	0.70	0.81	0.08	6.58
Risk seeking for gains (A2>5000)	0.11	0.31	0	1
B2	107.98	277.36	0	4000
β	0.92	0.31	0.39	4.32
Risk seeking for losses (B2<100)	0.53	0.50	0	1

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