Essays on the Trade-Offs for Non-Compliance and Deceptive Behavior

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Introduction

Opportunities for dishonest earnings are widespread, and the economic costs of non-compliance are well documented.[1] The list includes tax evasion, insurance and accounting fraud, service tampering, fare dodging or lies in personal communication. As for many decisions in economic life, people face a trade-off when encountering a misreporting opportunity. The monetary gain of a dishonest activity may be tempting, but it may come at serious consequences such as severe fines. However, even in absence of (monetary) punishment, people refrain from lying (e.g. Abeler et al. 2019 for a survey). Parents raise their children to be honest (Houser et al. 2016), and the bible highlights the importance of truthfulness for a society (Ephesians 4:25). Hence, there seem to be also non-monetary costs that prevent people from lying.

The Economic literature has investigated the trade-off between honest and dishonest reporting, and controlled laboratory experiments have turned out as a particularly useful tool. In the typical setup, a participant takes part in a random lottery. Each outcome of the lottery corresponds to a specific payoff. Importantly, the participant claims the payoff herself, offering a possibility for dishonest earnings by misreporting on the lottery outcome. Fischbacher and Föllmi-Heusi (2013) introduce this task as a die roll, and Bucciol and Piovesan (2011) as a binary coin flip. Other paradigms study dishonesty by misreporting on the performance in a real effort task (Mazar et al. 2008) or by sending self-serving deceptive messages to fellow participants (Gneezy 2005). Also closely related are tax compliance experiments (for early contributions see Friedland et

[1] See, for example, Slemrod (2007).

al. 1978, and Spicer and Thomas 1982), in which subjects evade a deduction in order to achieve higher earnings.

Subsequent research has built on these paradigms and has identified a variety of crucial behavioral aspects and determinants for dishonesty reporting and non-compliance. Contributions include, among others, positive and negative externalities of lying (e.g. Erat and Gneezy 2012), the role of emotions (e.g. Coricelli et al. 2010), the relationship of creativity and dishonesty (Gino and Ariely 2012), investigations into the structure of lying costs (e.g. Lundquist et al. 2009, Cappelen et al. 2013, Gneezy et al. 2018, Abeler 2019), treatment by peer subjects (Houser et al. 2012), dishonesty under scrutiny (van de Ven and Villeval 2015), the time delay in the reward for dishonest behavior (Ruffle and Tobol 2014), a subject's perception by others (Konrad et al. 2014), the characteristics of audit regimes to uncover lies (e.g. Beck et al. 1991, Alm et al. 1992, Alm et al. 1993), and socio-economic characteristics such as gender and religiosity (e.g. Friesen and Gangadharan 2012, Shalvi and Leiser 2013).[2]

Nevertheless, puzzles on dishonest reporting behavior remain. This dissertation builds on the previous literature and investigates into four of these puzzles using controlled laboratory experiments. In Chapter 1, we ask if there are heuristics for (un-)truthful reporting and address the multi-dimensionality of dishonest decision-making. Chapter 2 focuses on the consequences of self-selection into honest and dishonest earning opportunities. This also allows for an estimation of the distribution of behavioral lying costs. Chapter 3 highlights the role of ignorance of rules and regulations for dishonest reporting choices. Finally, chapter 4 investigates into the compliance decision of teams and disentangles the decision-making dimension from the liability dimension.

The trade-off on costs and benefits of a dishonest report may be a complex task. Finding the optimal solution for a specific reporting situation therefore requires cognitive resources and sufficient contemplation time (e.g. Mead et al. 2009, Gino et al. 2011, Shalvi 2012, and van't Veer et al. 2014). Both are not always available, and people have to rely on heuristics instead (cf. Kahneman 2011). Chapter 1 investigates into the heuristics for reporting decisions. To this end, we run a laboratory experiment and exogenously vary the available reflection time. The control group has enough contemplation time to consider the reporting decision carefully, while the treatment group is caught by surprise and needs to come up with a decision in a short time frame. Our

[2] For a more thorough account of the literature, we refer to the extensive surveys by Abeler et al. (2019), Alm (2019) and Gerlach et al. (2019).

results indicate that subjects are significantly less prone to make a dishonest report under time pressure. Dishonest subjects under time pressure also need more response time than their honest counterparts. These findings may be good news as most subjects have a heuristic of being honest.

We also discuss the multi-dimensionality of dishonest decision-making in chapter 1. A precondition for a deliberate dishonest report is the awareness of the misreporting opportunity. Only then, people can trade off the costs and benefits of the reporting decision. Further steps may include formulating a credible lie or keeping track of lies in the course of actions. We take a deeper look at the first two steps of the deceptive process. The recognition of the misreporting opportunity takes considerable time, and many subjects fail to do so under time pressure. In contrast, we find no evidence for further differences in the reporting behavior when accounting for this precondition of making a dishonest report. Hence, the lack of sufficient contemplation time may not affect the conscious trade-off on whether to misreport or to tell the truth.

A crucial question for the prevalence of dishonest behavior is whether people deliberately self-select into misreporting opportunities. Chapter 2 addresses the self-selection of honest and dishonest people. The career choice of a person might be a prominent example. While the banking industry has the reputation of attracting notorious liars (Cohn et al. 2014), nuns seem eager to be perceived honest (Utikal and Fischbacher 2013). Dishonest subjects are also more likely to self-select into the corrupt public sector in India (Banerjee et al. 2015, and Hanna and Wang 2017). In chapter 2, we ask if subjects self-select into honest and dishonest earning opportunities based on their individual behavioral lying costs. By offering subjects a choice between two income-generating lotteries in absence and in presence of a misreporting opportunity, we elicit the willingness to pay (WTP) for earning money in an honest fashion.

The good news is that most people are willing to spend some resources to earn their money in an honest fashion. However, a substantial minority of subjects takes advantage of the misreporting opportunity and drastically reduces their WTP as compared to the situation without such an opportunity. These subjects also have a large propensity of making a dishonest report. In contrast, their counterparts with a large WTP tend to be honest. Hence, individuals anticipate their behavioral lying costs, and self-select and report accordingly. The estimation of individual lying costs reveals that 30 percent of subjects have very small lying costs, while 50 percent of subjects seem to abstain from lying altogether. The remaining 20 percent prefers honesty to dishonesty but not at the costs of forgoing a significant monetary gain. This estimation directly connects to survey evidence that misreporting is mostly unresponsive to the incentive size for lying (Abeler 2019).

In Chapter 3, we manipulate the setup of the reporting situation itself. Subjects need to make a report in a state of ignorance on whether they are telling the truth or not. This mimics reporting decisions with a lack of information, for example when people are unsure if they are eligible to a deduction in their tax declaration or not. This may leave people with only two options. They may abstain from a self-serving report and forgo justified earnings. Or they may make the self-serving report at the risk of being a liar. We investigate such a reporting situation in a laboratory experiment. The outcome of a binary lottery qualifies either for a high payoff or for a low payoff. Subsequently, subjects claim one of the two payoffs. The control group knows which outcome is eligible to which payoff, while the treatment group lacks this knowledge.

Ignorance on the eligibility to payoffs leads to a larger propensity of claiming the high payoff, which almost doubles the fraction of unjustified claims as compared to the control group. Hence, subjects seem to suffer more from the behavioral costs of a deliberate lie than from a (potentially) dishonest report under ignorance. Nevertheless, some subjects feel obliged to claim only the low outcome. An increase in the ex-ante probability of being eligible to the high payoff crowds this behavior almost completely out, suggesting that people only need to be sufficiently certain to make the self-serving report. An explanation for the reporting behavior under ignorance might be the highly controversial social norms. While subjects consider a deliberate lie dishonest, the claim of the high payoff under ignorance ranges somewhere between honesty and dishonesty. Hence, subjects may fear negative consequences to their self- and social image less when making their reporting decision under ignorance.

Inspired by corporate scandals such as the 2015 Volkswagen emission scandal, chapter 4 discusses dishonest decision-making of teams. There is consensus in the deception literature that teams are more dishonest (Weisel and Shalvi 2015), for example due to the erosion of social norms by dishonest communication (Kocher et al. 2018) or the deferral of one's own responsibility (Conrads et al. 2013). We build on these insights but focus on the more complex compliance problem, which deviates from deception problems in one important aspect. A dishonest report may lead to monetary sanctions. This raises the question to not only who makes the compliance decision but also who is economically liable for a fraudulent report. Moreover, the focus may shift from the behavioral lying costs to the monetary fines. Our laboratory experiment investigates the team compliance decision along two dimensions. First, we vary whether individuals decide alone or in a team. Second, we either hold subjects individually liable or allow for sharing the costs and benefits of non-compliance between team members.

We confirm previous findings that teams are more dishonest than individuals are. The economic liability is the main driving force, as allowing for shared liability leads to a significant increase in dishonest reports. Team decision-making per se contributes little to the increase in non-compliance. Further results indicate behavioral spillovers within teams. Team members tend to convince each other of a specific reporting strategy, which leads to a positive correlation of individual reports. Finally, the risk dimension of the audit is the most important motivation for the compliance problem. In contrast to deception problems, the concept of being honest has less weight in the compliance decision. Hence, an audit might be suited to restore honesty in teams as long as each team member is individually liable.

1

Deception under Time Pressure – Conscious Decision or a Problem of Awareness?

This chapter is based of joint work with Tim Lohse and Kai A. Konrad.[1]

1.1 Introduction

Misreporting opportunities are common in everyday life. Some opportunities allow for a reflective decision while others come as a surprise and require an intuitive response. On the one hand, think of the problem of declaring taxable income or declaring the size of the monetary loss to an insurance company after a burglary. These problems leave plenty of time for consideration. On the other hand, picture a spontaneous decision to accept an excessive change (Azar et al. 2013) or a sudden control by customs at the airport when leaving the baggage claim area (Konrad et al. 2017). Here, time is always a crucial factor since dishonest activities involve coping with a trade-off between the associated costs and benefits. Benefits of a dishonest report are oftentimes immediate material or reputational gains. Costs may have not only a monetary dimension (such as fines) but also a psychological dimension due to violations of internal norms causing a bad conscience that dampens the utility of the material gain.[2] Thus, finding the

[1] The chapter is based on the article 'Deception under time pressure: Conscious decision or a problem of awareness?,' published 2018 in the *Journal of Economic Behavior & Organiztion*, Volume 146, p. 31–42. For the dissertation, this chapter has been editorially adapted. These changes are the sole responsibility of the author of this dissertation.

[2] The literature on deception has identified a variety of crucial behavioral aspects such as guilt aversion (Charness and Dufwenberg 2006), an aversion to lying (Lundquist et al. 2009, Cappelen et al. 2013), the behavioral differences depending on the type of lie (Erat and Gneezy 2012), the positive relationship between creativity and dishonesty (Gino and Ariely 2012), the role of emotions (Coricelli et al. 2010) or a subject's perception by others (Konrad et al. 2014).

optimal solution to this trade-off is a complex and potentially cognitively demanding task. Subjects might fail at this task under cognitive constraints such as time pressure. The issue is especially tricky if the misreporting decision is preceded by a cognition process that takes time in order to become aware of the misreporting opportunity. In fact, awareness of the misreporting opportunity is a precondition for the conscious decision to misreport and is hence an essential step in the misreporting process. The longer this cognition process takes, the less time there is to balance the costs and benefits of the actual report.

In this chapter, we use an innovative setting to shed light on this topic. We study a laboratory experiment of self-serving deceptive behavior which combines two distinct levels of reflection time with a cognition process about the opportunity to misreport. We ask the following research questions: first, what impact does time pressure have on misreporting behavior compared to a decision made with sufficient reflection time? Second, how is this effect of time pressure on misreporting mediated? Given the fact that there are two crucial components of dishonest reporting – namely, the process of gaining awareness of the misreporting opportunity and the conscious decision to misreport – which of these two is more important?

Our results are as follows. Comparing reporting behavior under time pressure with behavior with sufficient reflection time shows that time pressure has a large impact on the share of misreports: the number of dishonest reports significantly decreases by more than one third. Moreover, analyzing the timing of reports made under time pressure reveals that dishonest reports, on average, require 10 percent more time than honest reports. More specifically, the distribution of dishonest reports over time first-order stochastically dominates the distribution of honest reports over time. For the decomposition of the misreporting process, our results suggest that the differences between the decisions made under time pressure and the ones with extensive reflection time can be attributed to different levels of subjects' awareness of the misreporting opportunity. Restricting the analysis to those subjects who are aware of the misreporting opportunity and therefore make a conscious decision to misreport reveals that the share of dishonest reports is nearly the same under time pressure and with sufficient reflection time. This finding highlights the importance of the cognition process that leads to awareness, a component that has received only limited attention in the literature so far.

We contribute, on the one hand, to the experimental literature on dishonest decision-making under time constraints and, on the other hand, to the literature on the substeps of deception. The crucial novelty of our approach is that the reporting task comes as a perfect surprise. Due to this setting, subjects are unaware of the misreporting opportunity and cannot form strategies beforehand. Our rigorous implementation of the time dimension ensures that all steps involved with a deceptive strategy, i.e., the cognition process to become aware of the cheating opportunity and the balancing of the costs and benefits of a dishonest report, have to be carried out within a (rather short) time frame. Thus, we are able to study the true impact of time pressure on (mis)reporting behavior and we can isolate the impact of awareness.

The impact of time constraints on decision-making has only recently found itself in the spotlight of economic research.[3] By varying the available reflection time exogenously, one can identify behavior as either an intuitive response or as the result of a reflective process. Shalvi et al. (2012) use a modified version of the dice rolling experiment (Fischbacher and Föllmi-Heusi 2013) to address the effect of an exogenous introduction of time pressure on cheating. They find clear evidence of misreporting under time pressure, but less clear-cut evidence with unconstrained reflection time. Although there is a controversy as to whether participants were able to make up their mind on the decision prior to the actual report (Foerster et al. 2013 and Shalvi et al. 2013), the main insight provided by Shalvi et al. is that cheating is the automatic response. Gunia et al. (2012) also manipulate the time dimension by introducing an enforced contemplation period into a sender-receiver framework with honest and dishonest messages in the style of Gneezy (2005). Their results point in the same direction as Shalvi et al. (2012) since enforced reflection time leads to less dishonest behavior. However, the comparability of both settings is limited due to potential harm to other subjects and strategic truth-telling (Sutter 2009). In contrast, Greene and Paxton (2009) inform us that cheating takes more time and, hence, is not the intuitive choice. Most recently, based on an experiment on MTurk, Capraro (2017) comes to the same conclusion and observes more honest behavior under time pressure. As the payoff schemes and the reporting task were explained before subjects had to take an honest or dishonest decision, they were aware of the cheating opportunity and could form conditional strategies in advance. Since in our setting the misreporting opportunity comes as a surprise, we

[3] Among others, studies test the effect of time pressure on the quality of decision-making and on risky decisions (Kocher and Sutter 2006, Kocher et al. 2013) or the relationship between response time and cooperation in social dilemmas (e.g., Rand et al. 2012, Rand et al. 2014, Rand et al. 2016, Krajbich et al. 2015 or Stromland et al. 2016). For an overview see Spiliopoulos and Ortmann (2018). From a broader perspective, we contribute to the literature on the role of cognitive constraints (e.g., Mead et al. 2009, Gino et al. 2011, and van't Veer et al. 2014) and deliberation (Zhong 2011) on dishonest decision-making, which is surveyed by Bereby-Meyer and Shalvi (2015).

exclude this possibility and ensure that subjects have to make the actual decision as to whether or not to misreport within the short time frame.

A second literature strand takes account of the multi-dimensionality of dishonest decision-making, in particular the cognitive process that leads to awareness of the misreporting opportunity, the actual decision to misreport, and the construction of a credible dishonest report. While all decision steps potentially require cognitive resources, if subjects are not aware of the misreporting opportunity, then the subsequent decision steps become trivial. They may automatically lead to a truthful report. Gino et al. (2009) vary the saliency of the cheating option and find a decrease in cheating rates when a (fake) participant explicitly asks for permission to cheat in the presence of other subjects. In contrast, Fosgaard et al. (2013) use a more subtle procedure and show that facilitated understanding increases the share of dishonest reports by women. Walczyk et al. (2003) focus on the second and third step and find that the construction of a lie increases the response time of subjects. Our setting not only allows us to distinguish between the cognition process and the conscious decision to misreport, but also enables us to make inferences on the cognitive resources required for each step. Hence, we are able to identify the process of gaining awareness of the misreporting opportunity as a crucial determinant for dishonest decision-making.

1.2 The Experiment

1.2.1 Design

The experiment was programmed and conducted with the software z-Tree (Fischbacher 2007). Sessions took place at the econlab in Munich from December 2015 to April 2016. The pool of participants consisted predominantly of local Munich university students who were recruited using ORSEE (Greiner 2015). A total number of 411 subjects (average age 22.9; average payoff 13.8 EUR; 48 percent female participants) from various fields participated in the experiment. On average, a session lasted for 30 minutes and had 11 participants. The experiment was a one-shot game with two treatments, namely the 'Contemplation Treatment' and the 'Time Pressure Treatment.' We applied a between-subjects design. Throughout the experiment, care was taken that participants remained anonymous and did not exchange views or learned of other subjects' monetary payoffs neither during the experiment nor at the end of the experiment when payments were made.

Each participant was seated in a private cubicle at a computer. Some introductory screens provided the general instructions. An initial mock decision that was unrelated to the actual task in the experiment made them familiar with the technical choice of alternatives. Then, participants drew an individual income from a computerized private lottery shown as a binary wheel of fortune. Participants had an 80 percent chance of drawing a low income (400 Experimental Currency Units [ECU] = 4 EUR) and a 20 percent chance of drawing a high income (1000 ECU = 10 EUR). Probabilities were common knowledge. In both treatments the participants' task was to report their income simply by clicking either a button with 400 ECU or with 1000 ECU. There was no default report, i.e. not choosing one or the other option resulted in a payoff of zero and led to an exclusion from the analysis. As their (final) income report was the only determinant of their payoff, participants with a low income had a monetary incentive to misreport their lottery result.

Since we focused on misreporting on the individual level, the computer system registered both the true incomes from the lottery and the actual reports. Participants were informed that no individual screen was observable to the laboratory staff during the experiment, but they might have been aware that individual misreporting was detectable in the data. However, as recent literature has also found significant cheating in observable settings comparable to ours (Gneezy et al. 2018, and Kocher et al. 2018), this kind of observability should have no major influence on our results.[4] Moreover, we have chosen a procedure that physically separated the learning of the individual income from the actual reporting process.[5]

The two treatments, as illustrated in Figure 1.1, differ with respect to the available time to make the individual reporting decision after getting to know the outcome from the lottery.[6] In the Contemplation Treatment (CT) subjects read that they now had a fixed 60-second time period to think about the reporting decision and that only their

[4] In a comparison of observable and unobservable settings, Gneezy et al. (2018) find differences for certain aspects of cheating behavior, such as less partial and less absolute cheating in observable settings. Most importantly for our setting, the likelihood of reporting the maximum outcome is nevertheless broadly comparable. As our analysis focuses on treatment differences and partial cheating is ruled out by the binary design, the remaining differences are only of secondary importance in our case.

[5] While the main experiment including the reporting task was displayed on the main (center) monitor, the income-generating lottery took place on a separate notebook monitor (on the right).

[6] The research project originally involved a second treatment dimension. The purpose of this dimension was to evaluate the effect of social cues on the stimulation of certain heuristics. Specifically, we displayed a picture of a treetop (baseline) and a picture of human eyes (treatment) in the upper part of the monitor. Our hypothesis was that human eyes create a diffuse feeling of being observed, thereby shifting attention to (potential) negative consequences of cheating, such as a loss of reputation. However, this is not the case and we do not find a significant treatment effect along this dimension (χ^2-Test: $p = 0.42$). Hence, we pooled the respective data and focus entirely on the first dimension, namely the existence of time pressure.

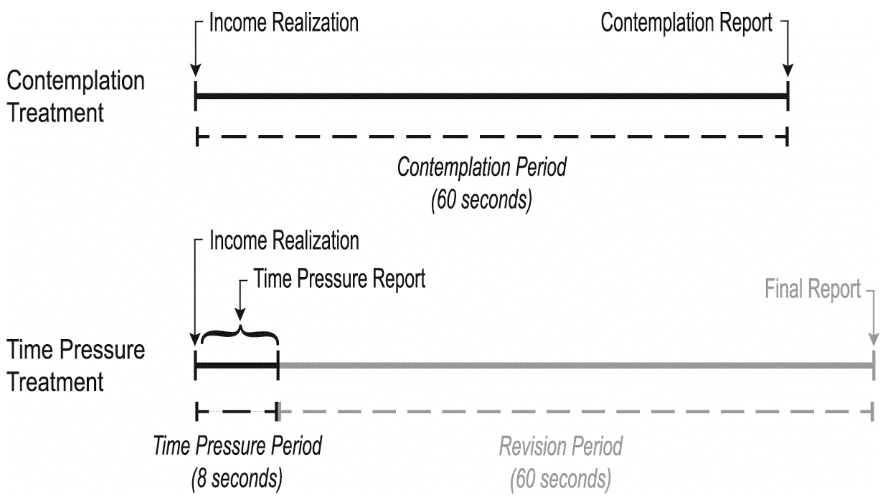


FIGURE 1.1: Structure of the Experiment

report determined their payoff. Only after the 60 seconds had elapsed, participants were asked to make their report. Hence, participants had sufficient reflection time for their decision. We refer to this report as the 'contemplation report.' In the Time Pressure Treatment (TPT), an initial income report had to be made under time pressure: participants read that they now had only eight seconds to report their income and that their report determined their payoff. This procedure made the reflection time of eight seconds a binding time constraint.[7] We refer to this report as the 'time pressure report.' Failure to give a report on their income led to a payoff of zero and to an exclusion from the analysis. Unknown to participants, the time pressure period in TPT was followed by an enforced revision period (displayed in gray in Figure 1.1): participants read that there was a break of 60 seconds until the experiment continued and that they could revise their time pressure report after the break. After the 60 seconds had elapsed, participants made a second report by clicking on one of the two income buttons, which determined their final payoff. We refer to this report as the 'final report.' Since informational circumstances in this second decision are different, we do not include this report in our main analysis. However, we briefly discuss the results in section 1.3.5.

It is important to notice that before reaching the contemplation period in CT or the time pressure period in TPT, respectively, participants knew neither the structure of the experiment in general nor that they would be asked to state the outcome of

[7] To determine an appropriate time threshold that would cause a relevant amount of time pressure, we ran pre-tests and a pilot. When restricting the available decision time to just six seconds it turned out that 2/3 of all participants failed to report in time. When the time threshold was set to 10 seconds it was not binding for anyone. Without further pre-tests or pilots we chose to allow for eight seconds in the actual experiment.

their private lottery. Hence, they were not able to anticipate the reporting problem. They could not make up their mind in advance on whether they wanted to misreport or state their income truthfully. In our subsequent analysis we will compare subjects' declaration behavior without time pressure in CT to behavior under time pressure in TPT. Focusing on the contemplation report versus the time pressure report in CT and TPT, respectively, will allow us to disentangle two effects: on the one hand, the cognition process of becoming aware of the possibility to misreport, and on the other hand, the conscious deception decision which involves trading off the costs and benefits of misreporting.

Right after the completion of the main experiment, we conducted a questionnaire concerning the experimental setting. One of the questions we asked was whether subjects were aware of the misreporting opportunity for the respective report. The answer to this question was used as a measure of awareness. In order to avoid any form of moral sentencing or other distorting influences on subjects' responses, we used a neutral framing for this question. A translation of the exact wording was the following: "Were you aware of the fact that you were able to influence your payoff since you were completely free to choose the answer that served you best?" The timing of this question right after the completion of the main experiment ensured that subjects still had the precise circumstances of reporting in mind but kept the report unaffected.[8]

The questionnaire was followed by several post-tests such as the Cognitive Reflection Test (Frederick 2005) to identify impulsive and reflective subjects. In the post-test section, subjects were able to earn an additional 300 ECU = 3 Euro. Each session concluded with two additional questionnaires, namely the Short Dark Triad (Jones and Paulhus 2014) and socio-economic questions. The purpose of the Short Dark Triad was to measure anti-social characteristics, specifically Machiavellianism, narcissism, and psychopathy. Most importantly for our setting were Machiavellian traits (9 items) such as manipulativeness or calculating, immoral behavior.

[8] As it is clear from the description above, our indicator of the awareness of the misreporting opportunity was not exogenously varied. Therefore, we applied a careful assessment of the reliability of responses (for a detailed discussion, please refer to section 1.3.3).

1.2.2 The Multi-Dimensionality of Dishonest Decision-Making

Misreporting is a complex and cognitively demanding task that consists of several steps and dimensions. The first and essential step is the cognition of the misreporting opportunity, which depends both on its apparentness and the possibility of a prior anticipation. For example, it is almost common knowledge that the filing of the income tax declaration may offer opportunities for misreporting. However, finding potential loopholes or possibilities to hide one's income is much more difficult. Hence, the opportunity to evade taxes can be anticipated, but is in most circumstances not very evident. In contrast, people sometimes face unexpected questions in their personal or professional life, such as whether a forgotten or overdue task has already been completed. Here, the misreporting opportunity is apparent. But the lack of anticipation might lead to its ignorance. In turn, subjects might (automatically) give an honest report. The latter case also matches the situation of subjects in our experiment, since the opportunity is easy to understand but comes as a surprise in TPT.

The subsequent step of the misreporting process is a careful evaluation of costs and benefits and the conscious decision of whether to misreport or not. While the benefit of a dishonest report is an immediate material gain in our setting, there are several potential costs of misreporting. These may include, among others, fear of an audit, an uncomfortable situation when receiving the payoff from the laboratory staff or the violation of internal norms. Since there are no audits and we ensure complete confidentiality of subjects' actions, the monetary incentive of misreporting should dominate countervailing incentives for the majority of subjects. But the trade-off leading to this insight potentially requires sufficient reflection time, which is not available under time pressure. Here, subjects are required to make an unreflected decision and have to rely on heuristics. The specific heuristic applied then might depend on the cost or benefit that comes to a subject's mind first. In contrast to the reflective decision, subjects that initially relate misreporting to the potential costs might try to avoid negative consequences and therefore give an honest report under time pressure. This is all the more true since honesty can also be a successful heuristic beyond the lab.

The theoretical underpinning for the dynamics of misreporting in our setting is captured best by Kahneman's (2011) dual framework of decision-making. "System 1" is responsible for quick, intuitive decision-making and requires (almost) no cognitive effort. In contrast, "System 2" choices show an in-depth evaluation of problems and lead to reflective decisions.[9] Typically, decisions made under time pressure are "System 1" decisions and are based on heuristics. In principle, this heuristic could be to misreport or it could be to tell the truth. However, a subject can misreport only if the subject is aware of this option. The crucial contribution of our analysis is to separate the impact of time pressure on recognition of the misreporting opportunity from the impact of time pressure on the reporting choice for those subjects who recognized the misreporting opportunity.

Both steps might require reflection time, and the time needed for each step should depend on the respective situation or the framework. In our setting, the misreporting opportunity cannot be anticipated by subjects. To gain awareness of the misreporting opportunity, subjects need to overcome their initial surprise of being in a reporting situation. Subjects are informed that their payoff is determined solely by their report. The misreporting opportunity should be sufficiently apparent to be recognized by a large share of subjects. Further, the reporting task makes truthful reporting and misreporting technically equally simple. Reporting is carried out by clicking one of two buttons. We expect that this makes the technical reporting task very fast and easy to perform, irrespective of the choice of report. As argued above, the choice of report is potentially more time consuming, as subjects must weigh the benefits against the various costs of misreporting.

1.3 Experimental Results

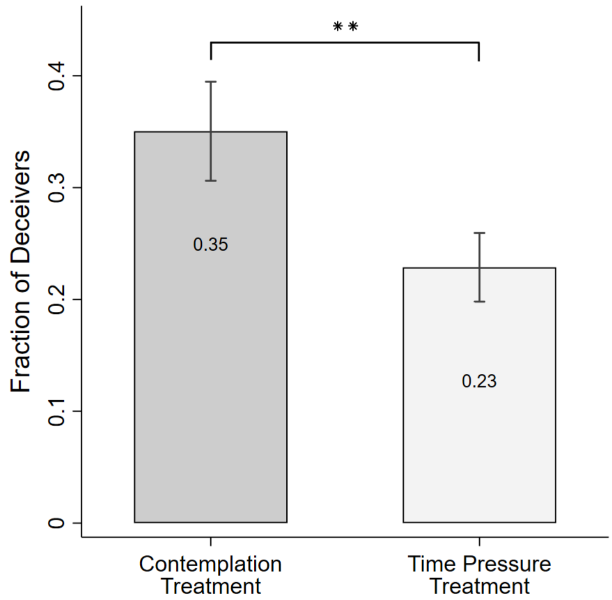
1.3.1 Overall Misreporting by Treatments

The analysis focuses on subjects that draw a low income and make their report within the time limit. A total of 32 subjects in TPT were not able to make a report within eight seconds, which indicates that time pressure was sufficiently high.[10] This leaves us with 305 subjects in total, of which 117 subjects are in CT and 188 subjects are in TPT.

We start our analysis with overall misreporting behavior, addressing the question of whether time pressure decreases the share of dishonest reports. Figure 1.2 displays the fraction of subjects that misreport a high income based on their low true income in CT

[9] Other explanations for this choice environment involve e.g. interpretations of the drift diffusion model (for a discussion, see Clithero 2018) or of the social heuristics hypothesis (Rand et al. 2014).

[10] We find no major differences in the characteristics of the group of non-responders and the group of those that responded on time. In particular, the level of awareness of the misreporting opportunity is comparable in both groups.



Note:. Error bars indicate mean +/- SEM.
Bracket indicates significant treatment difference, **p<0.05.

FIGURE 1.2: Misreporting by Treatment

and TPT. About 35 percent untruthfully report a high income with sufficient reflection time while only 23 percent dishonestly report a high income under time pressure. This difference is significant (χ^2-Test: $p = 0.02$) and shows that time pressure decreases the share of dishonest reports by more than one third.[11]

This estimate is confirmed by a multivariate analysis of misreporting (Table 1.1). For ease of interpretation, we report the results of a linear probability regression. The dependent variable is the share of dishonest reports and the reference group is CT for all specifications. The coefficient on the time pressure dummy is significant in both base specifications (with/without socio-economic control variables, columns (1) and (2))[12] and decreases the share of dishonest reports by 12 to 14 percentage points. This result contrasts with previous results of the literature.

[11] A preliminary conclusion is that honest reporting is the intuitive choice for a large share of subjects. An alternative explanation for behavior under time pressure might be that subjects choose one of the reports randomly, such that honest and dishonest reports occur with a probability of 0.5, respectively. A binomial test, however, rejects this hypothesis, i.e., the observed probability of misreporting significantly deviates from the expected probability of 0.5 in case of randomization ($p < 0.01$).

[12] Except for the number of siblings, none of the socio-economic control variables have a significant effect on misreporting.

VARIABLES	(1)	(2)	(3)	(4)
Time Pressure	-0.122**	-0.138**	-0.143***	-0.142***
	(0.054)	(0.054)	(0.054)	(0.054)
Cognitive Reflection Test			0.0294	
			(0.024)	
Machiavellianism				0.011**
				(0.005)
Socio-Economic Controls	NO	YES	YES	YES
Constant	0.350***	0.082	0.014	-0.260
	(0.044)	(0.228)	(0.239)	(0.278)
Observations	305	305	305	305
R^2	0.018	0.039	0.044	0.051

Notes: The table presents results of a linear probability regression with dishonest reporting as dependent variable (binary variable). Time Pressure is a dummy variable that is 1 for TPT and 0 for CT. Cognitive Reflection Test is the number of questions solved (0 to 3) and Machiavellianism is the score in the respective section of the Dark Triad (9 to 45). Robust standard errors in parentheses, *** p<0.01, ** p<0.05.

TABLE 1.1: Multivariate Analysis of Misreporting

In an extended specification (column (3)), we include the performance in the Cognitive Reflection Test (Frederick 2005) as an indicator of reflective thinking, which might be related to the ability to misreport. So far, there is mixed evidence on the relationship between reflective thinking and dishonest behavior (Fosgaard et al. 2013 and Ruffle and Tobol 2017). For overall misreporting, we find no significant effect of reflective thinking on misreporting. Also, subjects with Machiavellian traits (higher scores in the Machiavellianism section of the Short Dark Triad (Jones and Paulhus 2014)) have a significantly higher probability of misreporting (column (4)). This fits well with the definition of Machiavellianists as individuals who deceive for material gain, but only when potential costs are limited and it is thus beneficial to do so. We summarize our main finding as follows:

Result 1.1 *Time pressure decreases the share of dishonest reports by more than one third.*

1.3.2 *Response Time in the Time Pressure Treatment*

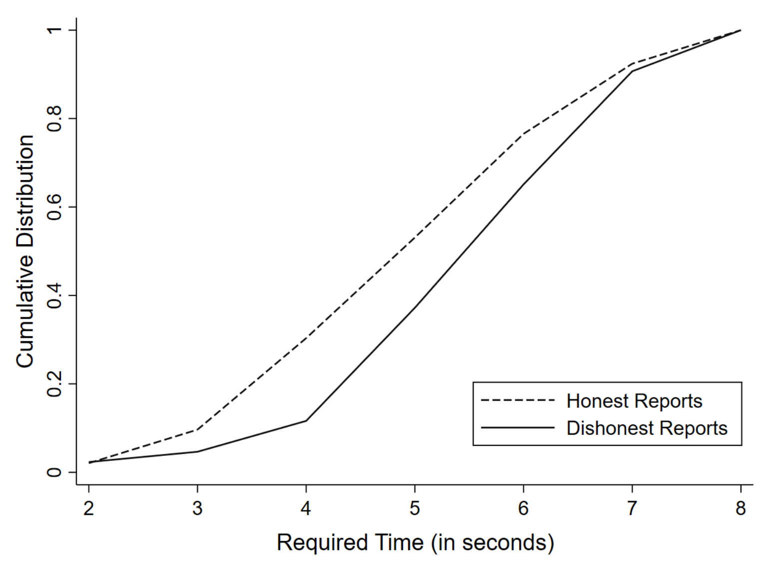


FIGURE 1.3: Cumulative Distribution of the Required Response Time in TPT

To shed further light on the question of whether honesty is the intuitive response, we analyze the required time of subjects *during the Time Pressure Period of TPT*, i.e., reporting behavior during the eight-second time frame. If misreporting indeed takes more time due to the cognition process and the balancing of costs and benefits, we should observe that honest subjects need less time than dishonest subjects.[13] Figure 1.3 plots the cumulative distribution of the response time of honest and dishonest subjects. A first inspection reveals that the distribution of response time for dishonest subjects first-order statistically dominates the distribution for honest subjects, i.e., it takes longer to make an untruthful report. On average, dishonest subjects (5.88 secs) need 10 percent more time than honest subjects (5.36 secs).

We conduct a number of non-parametric tests to assess the significance of these findings (Table 1.2). Since we are not able to reject the null hypothesis of the homogeneity of variances (Levene's test, Brown-Forsythe test), we apply the Wilcoxon rank-sum test and find that the difference between both groups is significant: a random honest subject is 61 percent more likely to need less time than a random dishonest subject.

[13] As Krajbich et al. (2015) point out, differences in reaction times might be due to option discriminability and not necessarily due to the existence of a dual process. Hence, our findings from this section should be interpreted as supporting evidence for the findings from section 1.3.1.

Test	Test Statistic	p-value
Homogeneity of Variances:		
Levene's Test	1.912	0.168
Brown-Forsythe Test	1.654	0.200
Equality of Distribution:		
Wilcoxon Rank-Sum Test	2.209	0.027
Kolmogorov-Smirnov Test	0.187	0.098

TABLE 1.2: Test-Statistics for Response Time under Time Pressure

Finally, the marginal significance of the Kolmogorov-Smirnov test on the equality of distributions (one-sided version) completes the picture. Hence, the analysis of required response time in the TPT further supports that honesty is the intuitive response for the majority of subjects. Our main finding from this section is:

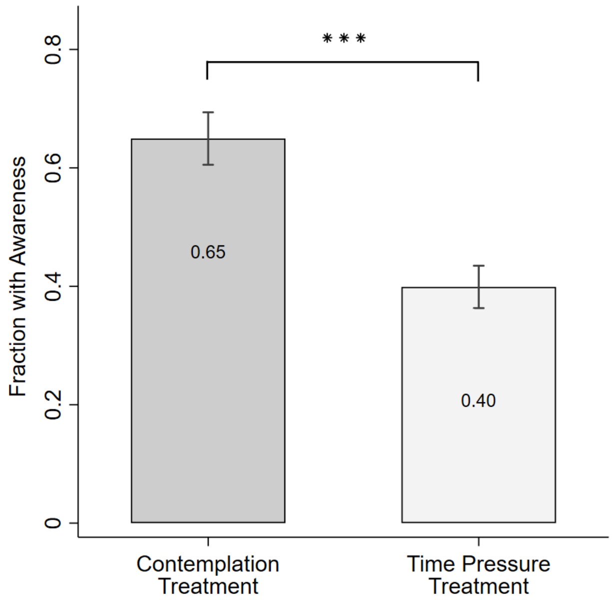
Result 1.2 *Dishonest subjects need 10 percent more time than honest subjects in the Time Pressure Treatment.*

1.3.3 Awareness of the Misreporting Opportunity

Results 1.1 and 1.2 suggest that misreporting is not the intuitive choice but rather requires sufficient reflection time. In the following two subsections, we decompose the process of misreporting into the cognition of the misreporting opportunity and the actual reporting decision of those subjects who are aware of the opportunity to misreport. Both steps are crucial for misreporting, but might be affected differently by time pressure. We start with the analysis of time pressure on the cognition process of the misreporting opportunity (awareness) and continue with the second step, the conscious decision to misreport or to tell the truth.[14]

Figure 1.4 displays the awareness of the misreporting opportunity in CT and TPT, respectively. While nearly two-thirds of subjects are aware of the misreporting opportunity in CT, only 40 percent report being aware of it in TPT. This difference is highly significant (χ^2-Test: $p < 0.01$) and suggests that time pressure reduces awareness of the

[14] Despite the two-step structure a joint regression analysis is not necessary: in all the probit models with a sample selection (Heckman probit) or recursive bivariate probit models that we analyzed, tests show that we cannot reject the null hypothesis that the two steps are in fact independent and should thus be estimated independently from each other. Therefore, we first report a regression analysis of the awareness process (this subsection) and then the misreporting behavior conditional on awareness (next subsection).



Note: Error bars indicate mean +/- SEM.
Bracket indicates significant treatment difference, *** p<0.01.

FIGURE 1.4: Awareness of the Misreporting Opportunity by Treatment

misreporting opportunity by 39 percent (or 25 percentage points). This estimate is confirmed by a regression analysis with awareness as the dependent variable (Table 1.3). In both base specifications (columns (1) and (2)), time pressure significantly reduces the level of awareness. Hence, subjects need considerable reflection time to overcome their surprise of the reporting situation and to identify the misreporting opportunity. This result highlights the importance of this first step of misreporting.

To ensure the validity of this result, we test for inconsistencies in the answers to the awareness question with respect to several dimensions. First of all, we focus on the group of subjects with a high income. These subjects have no incentive to misreport either on their true high income or on the question concerning the awareness of the misreporting opportunity. Hence, this group is perfectly suited to corroborate our findings above. While 61 percent are aware of the misreporting opportunity in CT in this group, only 43 percent report being aware in TPT. This clearly confirms our previous results. Secondly, awareness of the misreporting opportunity is a precondition for making a dishonest report. Therefore, a high percentage of dishonest subjects should state being aware for the respective report. This prediction is in line with the data: while 86 percent of dishonest subjects stated that they were aware in TPT, 88 percent of dishonest

VARIABLES	(1)	(2)	(3)	(4)
Time Pressure	-0.251***	-0.259***	-0.275***	-0.260***
	(0.057)	(0.058)	(0.057)	(0.058)
Cognitive Reflection Test			0.085***	
			(0.025)	
Machiavellianism				0.001
				(0.006)
Socio-Economic Controls	NO	YES	YES	YES
Constant	0.650***	0.410*	0.214	0.387
	(0.044)	(0.247)	(0.254)	(0.315)
Observations	305	305	305	305
R^2	0.059	0.073	0.104	0.073

Notes: The table presents results of a linear probability regression with self-reported awareness of the misreporting opportunity as dependent variable (binary variable). Time Pressure is a dummy variable that is 1 for TPT and 0 for CT. Cognitive Reflection Test is the number of questions solved (0 to 3) and Machiavellianism is the score in the respective section of the Dark Triad (9 to 45). Robust standard errors in parentheses, *** p<0.01, * p<0.1.

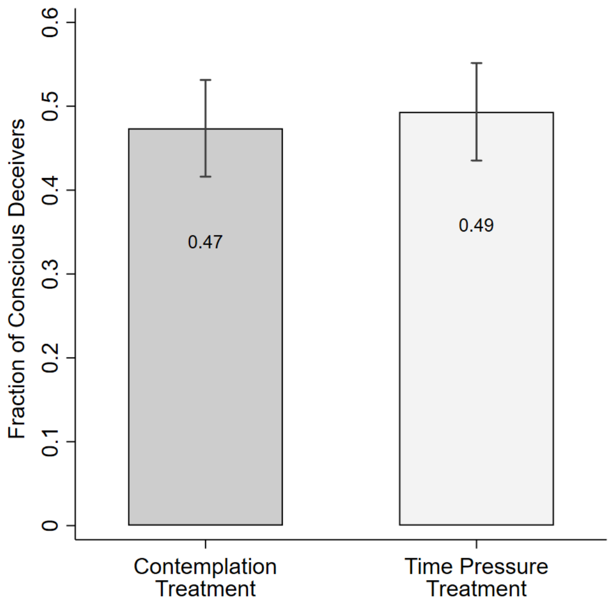
TABLE 1.3: Multivariate Analysis of Awareness of the Misreporting Opportunity

subjects were aware in CT. Finally, there are also no inconsistencies with respect to reporting behavior for the time pressure report and for the final report and answers to the awareness questions within TPT (for a detailed discussion, see section 1.3.5).

The decomposition of the decision process into two steps reveals that reflective thinking is related to a better ability to recognize the misreporting opportunity. The highly significant coefficient of the performance in the Cognitive Reflection Test in our alternative specification (column (3)) suggests that the likelihood of gaining awareness is 25 percentage points higher for a subject that answers all three questions correctly (reflective thinkers) compared to a subject that answers none of the questions correctly (impulsive thinkers). Machiavellian traits have no significant influence on the awareness of the misreporting opportunity (column (4)). In combination, both findings suggest that awareness is related to reflective thinking and independent of the moral attitude as captured by the Machiavellian section of the Dark Triad. We summarize our main findings concerning awareness as follows:

Result 1.3 *Subjects need reflection time to gain awareness of the misreporting opportunity. Time pressure reduces awareness by nearly 40 percent.*

1.3.4 The Conscious Decision to Misreport



Note: Error bars indicate mean +/- SEM.

FIGURE 1.5: Conscious Misreporting by Treatment

Recognition of the misreporting opportunity is seemingly a precondition for misreporting. We restrict our attention to subjects that consciously choose between honest and dishonest reporting. This restriction leaves us with 76 subjects in CT and 75 subjects in TPT. Figure 1.5 displays the fraction of conscious dishonest reports for CT and TPT, respectively. For the contemplation report, 47 percent of the aware subjects decide to report dishonestly, while 49 percent do so for the time pressure report. This difference between both treatments is not significant (χ^2-Test: $p = 0.81$). The nearly identical amount of misreporting in both treatments indicates that time pressure has hardly any effect on the conscious decision to misreport.

This finding is confirmed by regression analyses (Table 1.4) both of the subsample of 151 observations for subjects who stated that they were aware of the misreporting opportunity (columns (1)–(3)) and of the complete sample with all 305 observations that control for the effect of awareness (columns (4)–(6)). The dependent variable in Table 1.4 is the share of dishonest reports and the reference group is the CT. In all specifications of the subsample with subjects who are aware of the misreporting opportunity, time pressure has no significant effect on (conscious) dishonest reporting

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Time Pressure	0.016	0.008	0.024	-0.020	-0.033	-0.037
	(0.083)	(0.084)	(0.081)	(0.052)	(0.052)	(0.052)
Awareness				0.407***	0.407***	0.406***
				(0.048)	(0.048)	(0.048)
Cognitive Reflection Test		0.027				
		(0.041)				
Machiavellianism			0.028***			0.011**
			(0.009)			(0.005)
Socio-Economic Controls	YES	YES	YES	NO	YES	YES
Constant	0.030	-0.038	-0.878**	0.086*	-0.085	-0.417*
	(0.353)	(0.374)	(0.439)	(0.046)	(0.210)	(0.252)
Observations	151	151	151	305	305	305
R^2	0.046	0.049	0.101	0.213	0.231	0.243

Notes: The table presents results of a linear probability regression with dishonest reporting as dependent variable (binary variable). Specifications (1) - (3) refer to the subsample of subjects that stated being aware of the misreporting opportunity (n = 151), while specifications (4) - (6) use the complete sample (n = 305). Time Pressure is a binary variable that is 1 for TPT and 0 for CT. Awareness is a binary variable that is 1 if the subject reports being aware of the misreporting opportunity and 0 otherwise. Cognitive Reflection Test is the number of questions solved (0 to 3) and Machiavellianism is the score in the respective section of the Dark Triad (9 to 45). Robust standard errors in parentheses,
*** p<0.01, ** p<0.05, * p<0.1

TABLE 1.4: Multivariate Analysis of Conscious Misreporting

(columns (1)–(3)). This finding is confirmed for the complete sample: all specifications show that awareness of the misreporting opportunity is the main determinant of misreporting (columns (4)–(6)). In contrast, the time pressure dummy is insignificant and hence indicates that time pressure has no significant effect on misreporting beyond its effect on awareness. While reflective thinking (as captured by the Cognitive Reflection Test) has no effect on conscious misreporting (column (2)), Machiavellianists have a significantly higher probability of giving a dishonest report (column (4) and (6)). This depicts the other side of the coin compared to the finding for awareness: deceptive traits predict conscious misreporting, but this is independent of a subject's reflectiveness.

Result 1.4 *Controlling for subjects being aware of the misreporting opportunity, there is no evidence that time pressure has an effect on the conscious choice whether to misreport.*

In sum, we find evidence that the intuitive response for a majority of subjects is an honest report. The majority of subjects are not able to recognize the misreporting opportunity under time pressure, which in turn leads to a low share of dishonest reports. However, in our framework time pressure has no effect on balancing the costs and benefits of misreporting and on the final conscious decision whether to misreport.

1.3.5 Supporting Evidence: Revisions and the Final Report in TPT

In TPT, subjects make two reports over the course of the treatment: the initial time pressure report and the final report after the revision period (compare to Figure 1.1). Since the informational circumstances are different for the final report and not directly comparable to the time pressure report in TPT and the contemplation report in CT, our main analysis disregards data from the final report. However, the effect of reflection time in the revision period should be in line with the effect of reflection time in CT: more reflection time increases the share of dishonest reports. This is confirmed in the data: in the final report in TPT, 49 percent of subjects with a low income untruthfully report a high income. Hence, the share of dishonest reports more than doubles compared to the initial time pressure report (23 percent).[15] The difference is highly significant (McNemar's χ^2-Test: $p < 0.01$).

This direct, *within*-subjects comparison of the time pressure report and the final report also sheds light on the question if and in which direction subjects revise their initial report. Almost all subjects (95 percent) deceiving in the time pressure report also deceive in the final report. In contrast, a considerable share of 36 percent of honest subjects in the time pressure report revises the report and deceives in the final report. Taken together, this means that the reports are only revised in one direction, i.e., towards more deception in the final report. This revision behavior speaks against alternative explanations for misreporting under time pressure, such as a random choice of available options. If this was the case, we should observe no revisions at all (in case subjects would have remained by their randomly chosen choice) or revisions in both directions.

Moreover, the final report allows us to analyze the relationship of responses to the awareness questions and actual reporting behavior. As expected, awareness in the time pressure report implies awareness in the final report: 97 percent of subjects that state being aware of the deception possibility in the time pressure report also state being

[15] Although our structure for the revised report is different to settings with several sequential outcomes and reports, this finding relates to increased misreporting in repeated settings (e.g., Kocher et al. 2018) or for repeated participation (e.g., Fischbacher and Föllmi-Heusi 2013).

aware of it in the final report. Furthermore, partitioning the initially truthful subjects by the responses to the awareness questions reveals: subjects that state being not aware both for the time pressure report and for the final report stick to the initially truthful report (97 percent of subjects). A medium share of subjects that state being aware for both reports revises the honest time pressure report (22 percent of subjects). Most importantly, subjects that state being aware for the final report but not for the time pressure report are most likely to revise their truthful time pressure report (62 percent of subjects). These observations are in line with the results from section 1.3.3 and 1.3.4 and stress the importance of the cognition process for understanding the time pressure effect.

1.4 Discussion

In contrast to other settings, the subtlety and unpredictability of the misreporting opportunity in our framework allows for a more complete coverage of the misreporting process. It consists both of the process of gaining awareness of the misreporting opportunity, and the conscious decision on the actual report. We are able to separate both effects through the post-experimental questions on awareness in the respective reports. Since gaining awareness requires the mental availability of the concept of deception and its ignorance automatically implies an honest report, awareness is an essential part of misreporting. This cognition process has not been the focus of the literature so far. However, a decomposition of both steps is instructive for at least two reasons. First, misreporting opportunities differ considerably both with respect to their costs and benefits and with respect to their unexpectedness and apparentness. Hence, a decomposition leads to a better understanding of the dynamics of misreporting under different circumstances, such as time pressure. Second, the decomposition gives valuable insights for the prevention of deception. For example, in case of intuitive choices, is it more effective to highlight the immorality of deceptive actions or to make the deception opportunity as non-transparent as possible?

In contrast to previous results in the literature, we find that time pressure leads to significantly less dishonest reports. This is in line with misreporting to be not the intuitive response. For dishonest reports, subjects need to have in mind the concept of deception to recognize the misreporting opportunity. Then, they have to make a conscious decision of whether to misreport or not. Both steps potentially require cognitive effort and hence may be time-consuming. However, our results show that it is not the trade-off between costs and benefits of misreporting that is cognitively demanding and requires reflection time, but instead the cognition process of the misreporting opportunity. This finding is interesting, since subjects only have to overcome their initial surprise to gain awareness of the misreporting opportunity. Once we condition on awareness, the share of dishonest reports is nearly identical across treatments, which suggests that the conscious decision to misreport is intuitive and does not require ample cognitive resources. Although the moral dilemma might be more pronounced in other settings, our results indicate that the conscious misreporting decision might be determined by an inherent heuristic for honesty rather than a reflective process. Hence, decreasing the transparency of the misreporting opportunity could be the most effective prevention of dishonesty. In contrast, actions that highlight the potential costs of misreporting might have countervailing effects if they increase the awareness of the deception possibility at the same time.

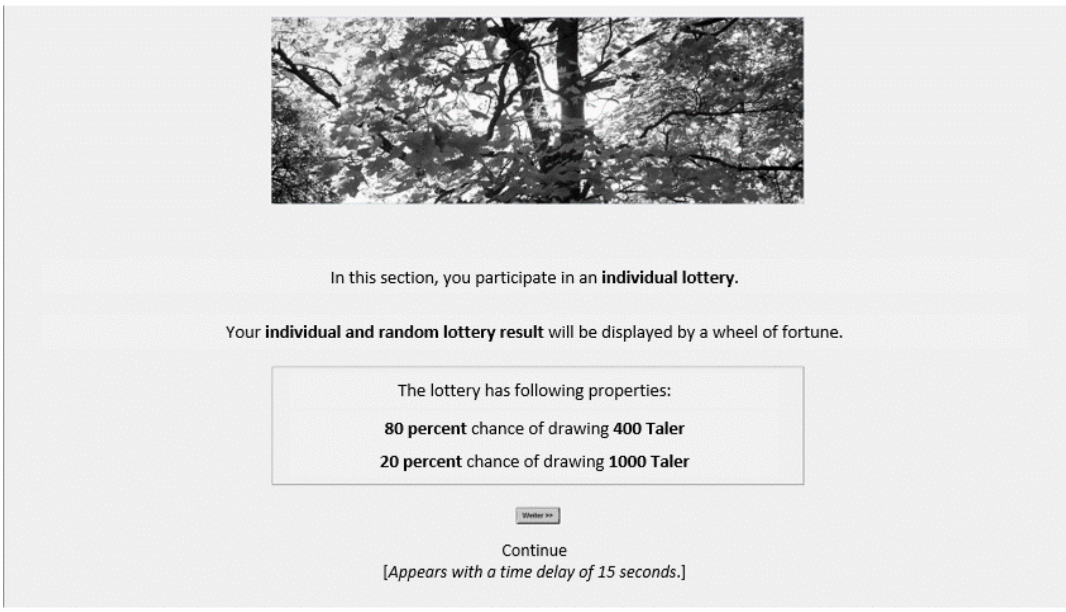
1.5 Conclusion

In this chapter, we study the role of the time dimension for dishonest decision-making in a one-shot experiment. The time dimension turns out to be a crucial determinant of deception. Some misreporting opportunities are unforeseeable and require an intuitive decision, while others allow for extensive reflection time. Our treatments exogenously vary the level of reflection time available to participants, inducing an intuitive versus a reflective decision. The novelty of our approach is that the misreporting opportunity comes as a surprise, thereby allowing us to cover the entire process of misreporting. Besides the actual decision of whether or not to misreport, gaining awareness of the misreporting opportunity is the crucial first step and a precondition for misreporting. First, we address the question of what impact time pressure has on dishonest reporting compared to sufficient reflection time. Secondly, we investigate which part of the misreporting process is affected by time pressure. Hence, we isolate the effect of time pressure on the cognition process that leads to awareness of the misreporting opportunity from its effect on the conscious decision to misreport.

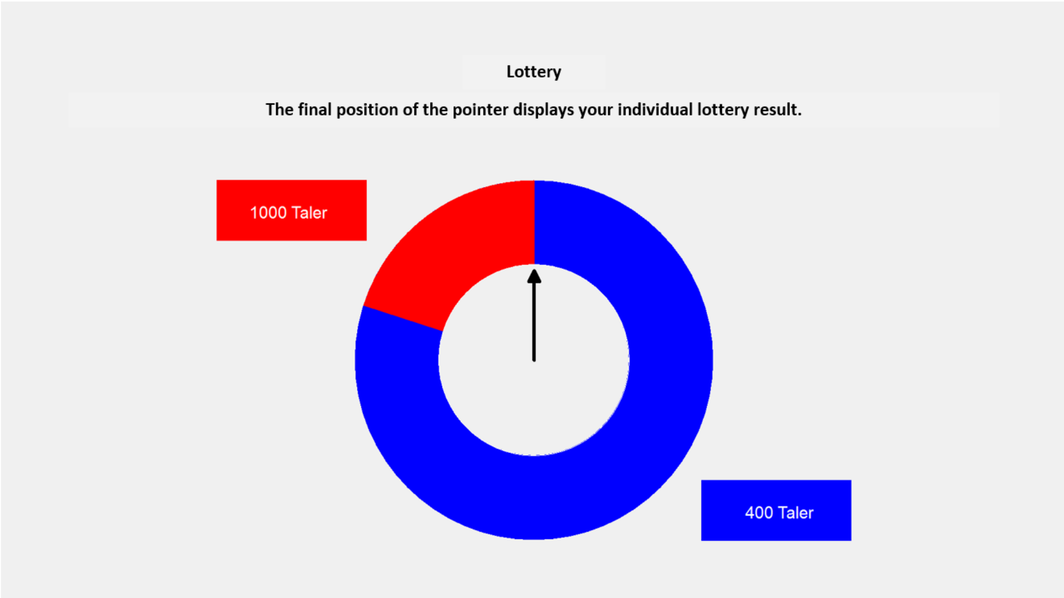
In contrast to the previous literature, we find evidence that misreporting need not be the intuitive choice when the misreporting opportunity is not apparent and subjects have the option to deceive for personal gain. This finding manifests in a lower share of dishonest reports as well as in an increased response time for dishonest subjects under time pressure. The decomposition into the process of gaining awareness of the misreporting opportunity and the conscious decision to misreport reveals that more reflection time increases the awareness of the misreporting opportunity, but has no effect on the conscious decision of whether to misreport or not. Hence, our results suggest that honesty is the automatic response since subjects need a considerable amount of reflection time to gain awareness of the misreporting opportunity. This is an important insight, since deception opportunities often come as a surprise and are not immediately obvious.

1.A Appendix – Screen-Shots from the Experiment

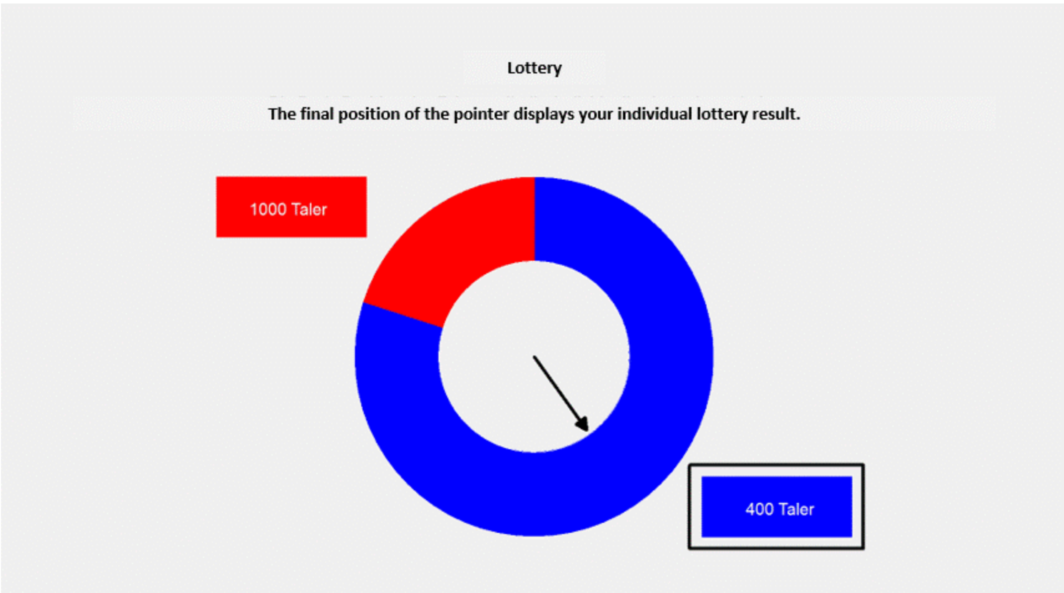
As noted in section 1.2.1, subjects found all instructions for the experiment on screen. This appendix presents a selection of screen-shots from the experiment. The experiment was conducted in German, and we provide an English translation here (additional explanations on screen-shots *in italics*). For brevity and clarity, we only show the most important or most characteristic screen-shots. The actual experiment consisted of screens before, in between, and after the selection of screens shown here. Captions of the respective screen-shot provide additional information to which stage and to which treatment the screen-shot belongs to.



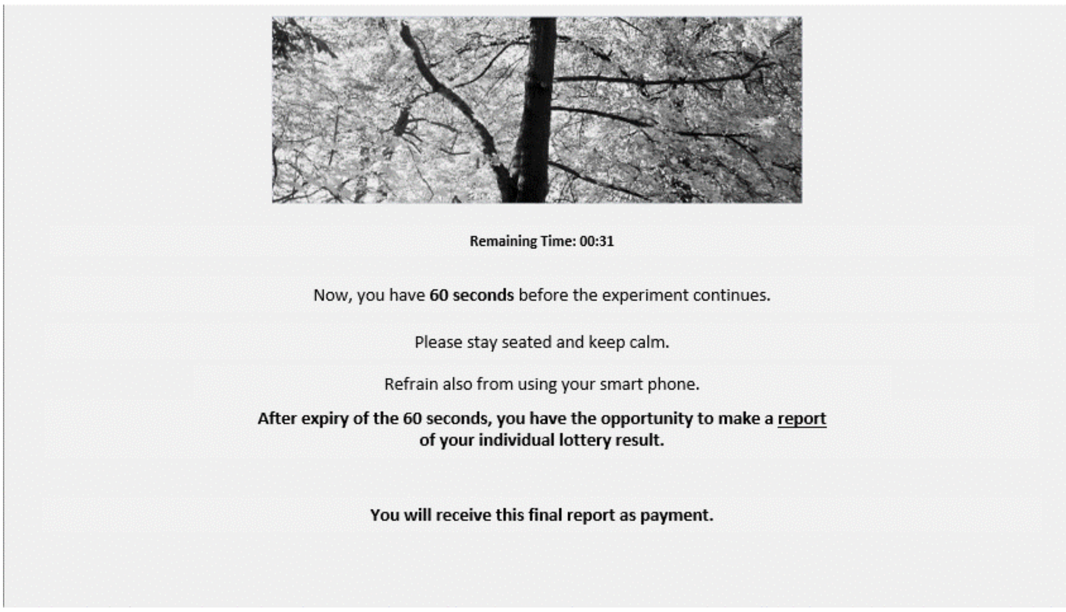
All Treatments: Explanation of the probability distribution before participation in the lottery.



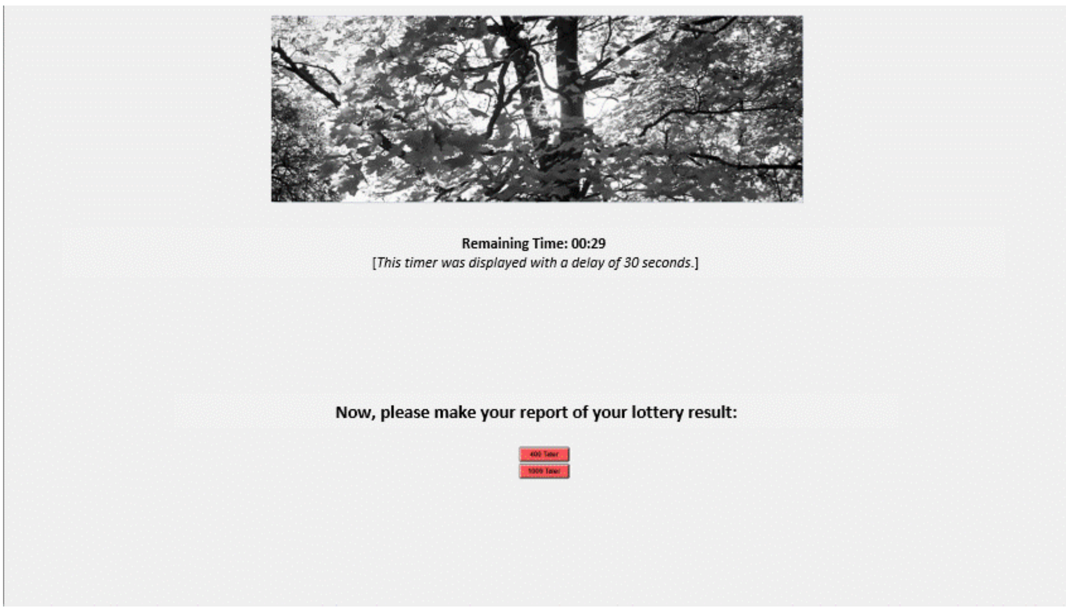
All Treatments: Initial state of the Wheel of Fortune before pointer starts spinning.



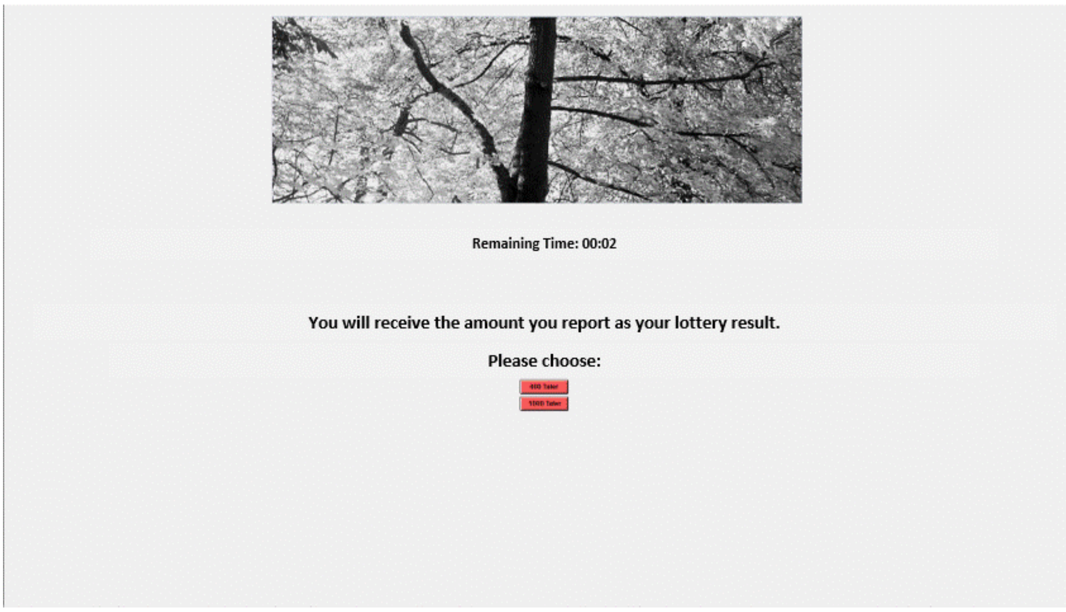
All Treatments: Final state of the Wheel of Fortune with individual lottery outcome.



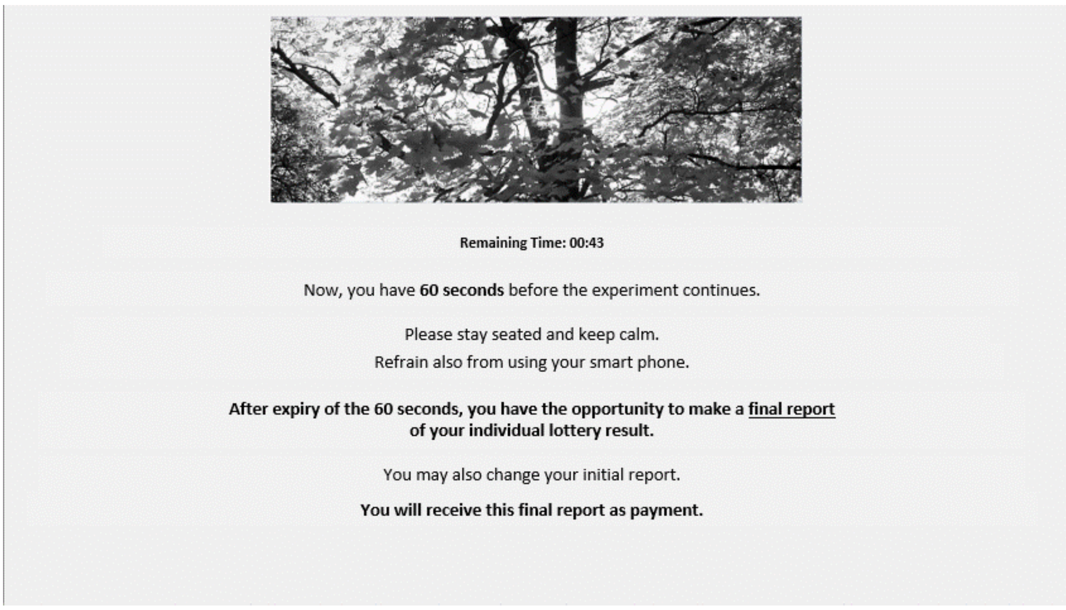
Contemplation Treatment: Contemplation Period after participation in the Wheel of Fortune.



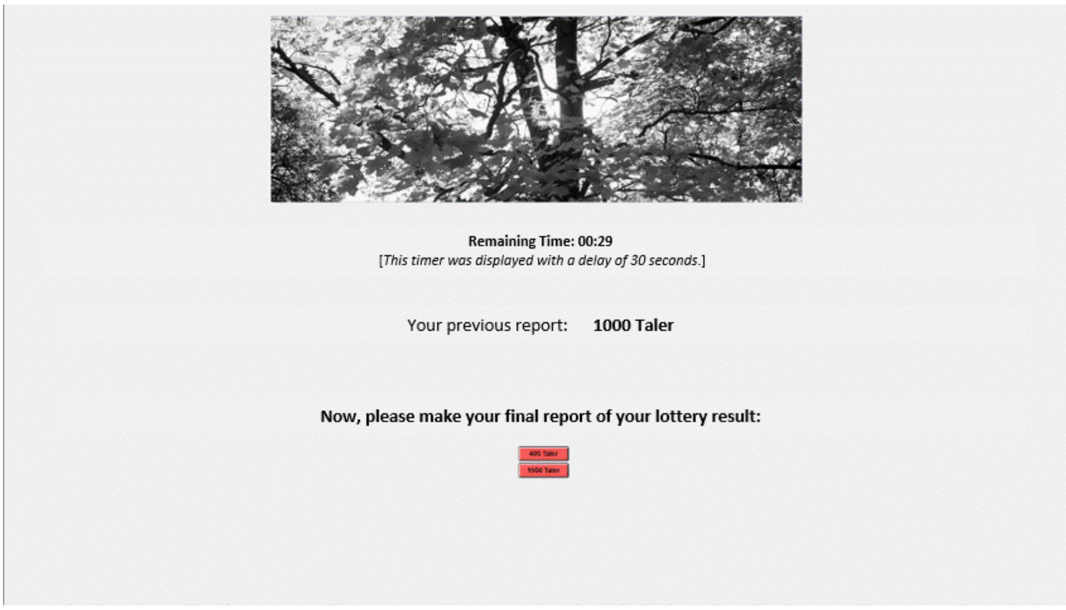
Contemplation Treatment: Contemplation Report after Contemplation Period.



Time Pressure Treatment: Time Pressure Period with Time Pressure Report.



Time Pressure Treatment: Revision Period after Time Pressure Report.



Time Pressure Treatment: Final Report after Revision Period.

2

Better to Win Honestly Than to Get Rich by Lying?

This chapter is based of joint work with Kai A. Konrad and Tim Lohse.

2.1 Introduction

As the biblical proverb (Proverbs, 21:6) tells us, "[m]aking a fortune through a lying tongue is a vanishing mist, a pursuit of death." But if the material reward from behaving dishonestly becomes large, the temptation might become irresistible. The proverb indicates: dishonestly earned money has a (behavioral) cost. For this reason, individuals may enjoy honestly earned money more than they enjoy dishonestly earned money.

This leads to empirical questions. Are individuals willing to pay to earn in an honest fashion? How badly do they suffer as a side effect of dishonest earnings? How is this lying cost related to their choice between an honest earning opportunity and one that allows them to make more money, but only if they are prepared to cheat? We resolve these questions in a theory-guided experiment in the laboratory.

Our work is related to the literature on cheating in general, and to two strands of this literature in particular. First, several papers study the role of monetary incentives for lying, which allows for insights on the distribution of individuals' lying costs and their preference for honest earnings. In particular, this literature investigates how subjects' truthfulness depends on the size of the stakes at risk. The findings are rather heterogeneous (e.g., Fischbacher and Föllmi-Heusi (2013), Kajackaite and Gneezy (2017), Gibson et al. (2013) and Vranceanu and Dubart (2019). See also the surveys by Abeler et al. (2019) and Gerlach et al. (2019)). Second, a recent literature strand touches on the issue of the self-selection of dishonest subjects into, or out of, lying opportunities. Several field experiments document a relationship between the occupational choice to work in corrupt sectors and the individual propensity to cheat (Banerjee et al. 2015 and Hanna and Wang 2017). Faravelli et al. (2015) and Gino et al. (2015) investigate the role of cheating opportunities for self-selection into competitive settings in the laboratory. Evidence on whether subjects deliberately self-select into cheating opportunities based on their lying costs is limited so far. In Shalvi et al. (2011) the individuals are offered a fixed payoff as an exit option over participating in the dice rolling game. In their framework the take-up of the exit option is not much affected by the possibility to lie, and lying behavior seems to be almost unrelated to the value of the exit option. Lying costs may not be the major driving force for self-selection here.

We elicit individuals' willingness to pay (WTP) to increase the odds of having an opportunity to earn honestly instead of earning money through a misreporting opportunity. This WTP can reflect how badly different individuals suffer from making dishonest earnings. Therefore, this WTP can be seen as a subtle way to measure the heterogeneity in lying costs. We also observe individuals' actual reporting behavior when the misreporting opportunity emerges. We study how the willingness to pay and the reporting behavior are related to each other. These findings give a good indication of whether individuals self-select into honest or dishonest earnings based on their lying costs.

More precisely, we conduct a laboratory experiment in which individuals choose between two lotteries in two different choice regimes. One lottery, (let us call it the 'Bad Lottery') has a zero outcome with high probability and a win outcome with a low probability. The other lottery yields the zero outcome with a low probability and the win outcome with a high probability. Let us refer to it as the 'Good Lottery.' The Bad Lottery is costless. To obtain the Good Lottery instead, individuals have to pay a price. Their lottery choice is recorded for two different regimes. One regime is truthful by design: the lottery outcome is identical to the individual's final payout. Here, an individual's attitude toward truth-telling is irrelevant. The other regime allows for misreporting. In particular, the individual may claim the win outcome even if the true lottery outcome is zero. Whether and for which price an individual is willing to acquire the Good Lottery may depend on the individual's valuation of a true win outcome compared to a dishonestly claimed win outcome in this regime.

Our results indicate that some, but not all individuals have a smaller WTP for the Good Lottery in the regime with the misreporting opportunity. Some individuals are not willing to spend money to obtain the Good Lottery. And these individuals also tend to misreport whenever the opportunity arises. Other individuals expend considerable resources to obtain the Good Lottery. These individuals are also less likely to misreport if the misreporting opportunity emerges.

2.2 The Decision Theoretic Framework

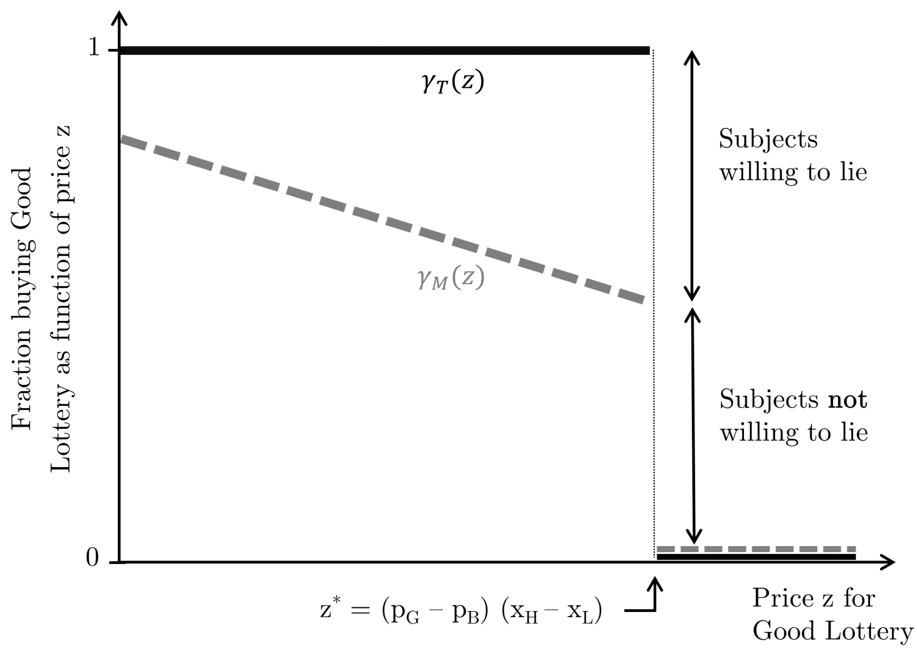
Consider an individual that maximizes its expected monetary payoff. The individual chooses between two binary lotteries (labeled 'Good Lottery' and 'Bad Lottery'). Each lottery yields either a high payout x_H (denoted as 'win outcome') or a low payout x_L (denoted as 'zero outcome'), with $x_H > x_L$. The Bad Lottery B yields the high outcome x_H with probability p_B and the zero outcome x_L with probability $1-p_B$. The Good Lottery G yields x_H with probability p_G and x_L with probability $1-p_G$. Lottery G is the better lottery because $p_G > p_B$.

2.2.1 The Honest Regime: Choosing in the Absence of a Misreporting Opportunity

An individual can choose the Bad Lottery B for a zero price. For a price $z \geq 0$ she can choose the Good Lottery G instead. We denote the two choice alternatives by $(B, 0)$ and (G, z). Once this choice is made, the individual learns the lottery outcome and is rewarded with this true outcome as her payoff. The expected payoffs from $(B, 0)$ and (G, z) are $\pi_B = p_B x_H + (1-p_B)x_L$ and $\pi_G = p_G x_H + (1-p_G)x_L - z$. The indifference price

$$z^* = (p_G - p_B)(x_H - x_L) \tag{2.1}$$

gives the individual the same expected payoff from $(B, 0)$ and from (G, z^*). For a price $z \leq z^*$ the purchase of the Good Lottery G leads to a higher expected payoff. Hence, the expected payoff-maximizing individual chooses (G, z) for $z \leq z^*$ and $(B, 0)$ for $z > z^*$. The fraction of individuals who choose (G, z) as a function of z is depicted as a function $\gamma_T(z)$ in Figure 2.1: $\gamma_T(z) = 1$ for $z \leq (p_G - p_B)(x_H - x_L)$ and $\gamma_T(z) = 0$ for any higher price.



Note: Figure displays the predicted fraction of individuals purchasing the Good Lottery G as a function of its price z in the URR and HR.

FIGURE 2.1: Predicted Demand for the Good Lottery G

2.2.2 The Untruthful Reporting Regime: Choosing with a Misreporting Opportunity

Let us change one assumption in the set-up. As before, the individual chooses between $(B, 0)$ and (G, z) and then learns the true lottery outcome. But then the individual is asked to report the lottery outcome. This report does not need to be truthful, i.e., the individual can choose any report $\xi \in \{x_H, x_L\}$. She receives exactly this reported value ξ as her payoff.

Suppose that the individual has a (behavioral) cost of reporting untruthfully. Let this cost be equal to $\theta \geq 0$ and a draw from a random variable with cumulative distribution function $F(\theta)$ with support $[0, \infty)$.[1] This θ might be interpreted as the sum of internal lying costs, including (self-)image concerns and the costs of violating (self-imposed) social norms, as modeled explicitly e.g., in Gneezy et al. (2018) or Abeler et al. (2019). For brevity, we refer to θ as the individual's lying costs.

[1] We assume that an individual's θ for reporting $\xi(x_L) = x_H$ is the same in both lotteries. In the experimental setting, we are able to address whether this is a valid assumption.

Based on these primitives the optimal reporting behavior of an individual with lying costs θ is the following. A win outcome x_H is always reported honestly, $\xi(x_H) = x_H$. In contrast, whether the individual reports truthfully if the lottery outcome is x_L depends on θ: $\xi(x_L) = x_L$ if $\theta \geq (x_H - x_L)$ and $\xi(x_L) = x_H$ if $\theta < (x_H - x_L)$. The individual misreports on x_L if and only if her lying costs θ fall short of the monetary benefits of misreporting. Taking this reporting behavior into account, the expected payoffs for the choices $(B, 0)$ and (G, z) are

$$\pi_B(\theta) = \begin{cases} x_H - (1 - p_B)\theta & \text{if } \theta < x_H - x_L \\ p_B x_H + (1 - p_B)x_L & \text{if } \theta \geq x_H - x_L \end{cases} \tag{2.2}$$

and

$$\pi_G(\theta) = \begin{cases} x_H - (1 - p_G)\theta - z & \text{if } \theta < x_H - x_L \\ p_G x_H + (1 - p_G)x_L - z & \text{if } \theta \geq x_H - x_L \end{cases} . \tag{2.3}$$

This makes the lottery choice straightforward: an individual pays z and chooses (G, z) if $\pi_G(\theta) - \pi_B(\theta) \geq 0$, and goes for $(B, 0)$ if $\pi_G(\theta) - \pi_B(\theta) < 0$, where

$$\pi_G(\theta) - \pi_B(\theta) = \begin{cases} (p_G - p_B)\theta - z & \text{if } \theta < x_H - x_L \\ (p_G - p_B)(x_H - x_L) - z & \text{if } \theta \geq x_H - x_L \end{cases} . \tag{2.4}$$

For the case of indifference, we assume that the individual chooses (G, z). The fraction of individuals who purchase (G, z) as a function of z is depicted as $\gamma_M(z)$ in Figure 2.1.

For a price $z > z^* = (p_G - p_B)(x_H - x_L)$ the individual will never purchase (G, z). This behavior is independent of the individual's θ: for $\theta \geq x_H - x_L$ the individual reports truthfully, but we have $\pi_G(\theta) - \pi_B(\theta) = (p_G - p_B)(x_H - x_L) - z < 0$. For $\theta < x_H - x_L$ the individual reports $\xi = x_H$ for any true lottery outcome, and we have $\pi_G(\theta) - \pi_B(\theta) = (p_G - p_B)\theta - z < (p_G - p_B)(x_H - x_L) - z < 0$. Hence, $\gamma_M(z) = \gamma_T(z) = 0$ for all $z > z^*$.

For $z \leq z^*$, individuals with $\theta \geq x_H - x_L$ report truthfully and choose (G, z), as purchasing (G, z) gives them a higher expected payoff than $(B, 0)$. In contrast, individuals with $\theta < x_H - x_L$ report $\xi = x_H$ for any true lottery outcome. For these individuals, $\pi_G(\theta) - \pi_B(\theta)$ is weakly increasing in θ: the higher the individual's lying costs, the worse the zero outcome is compared to the win outcome, and the higher the individual's WTP for the Good Lottery. An individual is just indifferent between $(B, 0)$ and (G, z) for $(p_G - p_B)\theta - z = 0$, or $\theta = z/(p_G - p_B)$.[2] Individuals with a lying cost higher than (or equal to) $z/(p_G - p_B)$ purchase (G, z), while individuals with smaller lying costs choose $(B, 0)$. Accordingly, for a given z the fraction of individuals who choose (G, z) is equal to the probability that $\theta \geq z/(p_G - p_B)$, or,

$$\gamma_M(z) = 1 - F(\frac{z}{p_G - p_B}) \text{ for } z \leq z^*. \tag{2.5}$$

2.3 Experimental Analysis

2.3.1 Design

Overview. We conducted our laboratory experiment in September and October 2018 at the econlab Munich.[3] Each subject made decisions in two regimes in a sequential order.[4] In the 'Honest Regime' (HR), the individual chooses one of two lotteries and receives the true payout of this lottery. In the 'Untruthful Reporting Regime' (URR) the lottery choice is followed by a reporting stage with an opportunity to misreport and to potentially earn a dishonest income by reporting a win outcome although the lottery yielded the zero outcome.

Lotteries. In both regimes there are two lotteries, the Bad Lottery and the Good Lottery. The lotteries have the same binary outcomes: the zero outcome has a payout x_L of EUR 0 and the win outcome has a payout x_H of EUR 12. For the Bad Lottery, the probability p_B of the win outcome x_H is 1/6, and the probability $(1 - p_B)$ of the zero outcome x_L is 5/6. For the Good Lottery, the probability p_G of the win outcome x_H is 4/6 and the probability $(1 - p_G)$ of the zero outcome x_L is 2/6.[5] Evidently, the Good Lottery is better than the Bad Lottery by first-order stochastic dominance.[6]

[2] This relationship allows for an estimation of an individual's lying costs based on her articulated WTP. For a detailed discussion of the estimation procedure and results from the experiment, refer to section 2.4.5.

[3] A total of 308 subjects participated in 18 sessions of the experiment (average length of a session 75 to 90 minutes). These were predominately local university students (average age 24 years; 55.5 percent female students). Subjects were recruited from the laboratory's subject pool using ORSEE (Greiner 2015). The experiment was programmed and conducted with z-Tree (Fischbacher 2007). The average payoff from the experiment was EUR 17.90 (min. EUR 6.50, max. EUR 27.50) including a show-up fee of EUR 9 and earnings up to EUR 2.90 from the post-tests.

[4] We randomly varied the order of regimes: half the subjects started with one or the other regime. To exclude any income effects or hedging behavior, only one of the two regimes was chosen at random for payout at the end of the experiment.

[5] Probabilities of the outcomes were visualized as drawing numbers from '1' – '6' on a computerized wheel of fortune. Each number represented either the zero or the win outcome of the respective lottery. The wheel of fortune determined the true lottery outcome for the subject.

[6] Important to our design, subjects are not given the possibility to opt out completely, but only to improve their chances of an honest win outcome. Uncertainty over the outcome and the potential temptation to lie are not removed

Elicitation Method. In both regimes the subject chooses between the two lotteries. We use the strategy method (Selten 1967) to elicit an individual's reservation price for choosing the Good Lottery over the Bad Lottery: Subjects make a series of choices between the Good and the Bad Lottery for 18 different possible price levels of the Good Lottery. The price for the Bad Lottery was EUR 0 for each choice, while the price z of the Good Lottery declined in steps of EUR 0.50 from EUR 8.5 to EUR 0.[7] Accordingly, a subject's maximum WTP $z_i^{\max}$ is defined by the highest price level for which the subject chooses the Good Lottery over the Bad Lottery.

Implemented Choice. When making their choices, subjects know that the laboratory had pre-determined a seller price z_0 that would apply, and that would be announced by the laboratory once they made their binary buying decisions for all possible values of z. The seller price z_0 was set to EUR 4 in the Honest Regime and to EUR 1.5 in the Untruthful Reporting Regime.[8] As subjects only receive the information on the price z_0 once they have made their choices, it was in their best interest to consider their choices between $(B, 0)$ and (G, z) as potentially payoff-relevant for all possible $(B, 0)$-versus-(G, z) comparisons.[9]

Lottery Allocation. If a subject chooses the Bad Lottery for z_0 (i.e., $z_i^{\max} < z_0$), the cost-free Bad Lottery at a price of EUR 0 is implemented ('Bad Lottery - self-selected'). If a subject expressed a WTP that (weakly) exceeded z_0 (i.e., $z_i^{\max} \geq z_0$), then, with probability 2/3, the subject was assigned to the Good Lottery at a price of z_0 ('Good Lottery - self-selected'). With the remaining probability 1/3 the subject was assigned to the cost-free Bad Lottery instead ('Bad Lottery - assigned'). This procedure provides us with information on how individuals make their reporting choices if they have a high WTP for the Good Lottery but have received the Bad Lottery.[10]

completely. The WTP is therefore more likely to be due to the reduction of the likelihood of facing temptation, and not due to avoiding an uncertain outcome completely.

[7] The multiple price list employed here is a variant of the Becker-DeGroot-Marschak (1964) mechanism to elicit a subject's WTP in an incentive-compatible manner.

[8] The price EUR 1.50 in the URR was chosen to achieve a balanced number of subjects who face the misreporting opportunity in the three possible lottery allocations. We based our estimation on the results of a structurally related pilot with different parameterization.

[9] We addressed the potential concern that subjects might be suspicious about whether the seller price z_0 was pre-determined or not. In their cubicle, we placed a sealed letter which they were only allowed to open at the end of the experiment. The letter contained information on the predetermined price z_0.

[10] It also accounts for the potential effect of loss aversion, as buying the Good Lottery is costly but does not imply an honest success with certainty. The literature typically finds more cheating in the loss domain than in the gain domain (e.g., Grolleau et al. 2016 or Schindler and Pfattheicher 2017), with an increasing propensity to cheat the less likely the bad outcome is (Garbarino et al. 2019). Therefore, the lying rate in the group 'Good Lottery self-selected' may have an upward bias (that runs against our prediction). If so, focusing on the lying rate in the group 'Bad Lottery assigned' instead allows for a clean estimation of the selection effect.

The Regimes. In the Honest Regime, the lottery outcome of the allocated lottery is directly payoff-relevant and ends the regime. In the Untruthful Reporting Regime, the procedure continues. The subject learns the respective lottery outcome of the allocated lottery and is asked to make a report on this outcome. In turn, this report determines the payoff from the URR. The subject may report untruthfully, i.e., she might choose to report $\xi(x_L) = x_H$. There are no audits or formal sanctions for misreporting. Our design allows both for a comparison of the willingness to pay between-subjects (decision in the regime which was first in order) as well as within-subjects (decision in both regimes). Moreover, we are able to identify dishonest subjects in the URR by comparing their reported to their realized lottery outcome which are both observable in the data.

Post-Tests and Payoffs. The experiment concluded with an incentivized elicitation of beliefs about the choice and the reporting behavior of other participants in the respective session.[11] The Post-Test section also included the Cognitive Reflection Test (Frederick 2005), Murphy et al.'s (2011) test for social value orientation and a variant of the Ellsberg paradox (Ellsberg 1961) to test for ambiguity aversion. Finally, we conducted a short socio-economic questionnaire. Before subjects were paid out, they received a detailed overview of their earnings from the experiment.

2.3.2 Hypotheses

We develop two sets of hypotheses along our decision theoretic framework. The first set of hypotheses addresses the impact of the misreporting opportunity on individuals' WTP for choosing (G, z) over $(B, 0)$.

Using the actual numbers from the experimental set-up, risk neutral, payoff-maximizing subjects have a predicted indifference price z^* of EUR 6 in the Honest Regime.[12] For all prices $z > z^*$, subjects are predicted to choose the Bad Lottery. For all prices $z \leq z^*$, subjects are predicted to choose the Good Lottery. In the Untruthful Reporting Regime, subjects with high lying costs $\theta \geq 12 (= x_H - x_L)$ face a decision problem that is exactly equivalent to the honest regime. For these subjects, the misreporting opportunity does not matter and the choice behavior should be the same in both regimes. But subjects with low or intermediate lying costs $\theta < 12$ are likely to misreport on the zero outcome. Ex ante, because the misreporting opportunity makes the zero outcome

[11] We ran additional robustness sessions to account for potential confounders from the interaction effects of payment schemes or risk preferences (see Appendix A.2 for a detailed discussion).

[12] An assumption is risk neutrality of subjects for small stakes. We check this assumption with an additional lottery choice task. In fact, 72 percent of subjects can broadly be classified as risk neutral.

less bad for them, this leads to a smaller WTP $z_i^{\max}$ for the Good Lottery in the URR. However, the higher the individual's lying costs, the worse the zero outcome is and the closer $z_i^{\max}$ is to z^*.

Hence, the comparison of $\gamma_T(z)$ and $\gamma_M(z)$ in Figure 2.1 suggests that, for a given $F(\theta)$, the distribution of the WTP in the Honest Regime first-order stochastically dominates the WTP in the Untruthful Reporting Regime. For prices $z \in (z^*, \infty)$, the comparison is trivial as $\gamma_T(z)$ and $\gamma_M(z)$ coincide and all individuals choose $(B, 0)$ in both regimes. For prices $z \in [0, z^*)$, the fraction of individuals who purchase G for a given z deviate from each other. All types of individuals choose (G, z) in the HR. Their maximum WTP for the Good Lottery G is z^*. In contrast, the fraction of subjects choosing (G, z) in the URR is $1 - F(\frac{z}{p_G - p_B})$ for a given $z \in (0, z^*)$. This share is monotonically declining in z. At $z = z^*$, only individuals with high lying costs $\theta \in [x_H - x_L, \infty)$ would choose (G, z). Hence, $1 - F(\frac{z^*}{p_G - p_B})$ is strictly smaller than 1 at $z = z^*$. We summarize these considerations in the following

Hypothesis 2.1 *The WTP for the Good Lottery in the Untruthful Reporting Regime*

(i) is between-subjects and, on average, smaller than in the Honest Regime;

(ii) is within-subjects, smaller than or equal to the Honest Regime.

Our second set of hypotheses focuses on individuals with a lottery outcome x_L in the Untruthful Reporting Regime. The decision theoretic framework suggests a close relationship between the articulated WTP for the Good Lottery and the subsequent reporting behavior in this regime. As reporting behavior is observable in our setting, we are able to investigate this relationship by analyzing the WTP conditional on reporting behavior, and vice versa.

In the Untruthful Reporting Regime, misreporting only pays off for sufficiently small lying costs $\theta < x_H - x_L$. Accordingly, individuals base their WTP for the Good Lottery on their anticipated reporting behavior. Honest individuals disregard the misreporting opportunity and should therefore not alter their WTP $z^* = (p_G - p_B)(x_H - x_L)$. Their WTP for the Good Lottery reflects the increased chances of an honest win outcome. In contrast, as the misreporting opportunity makes dishonest individuals better off, dishonest individuals should express a smaller maximum WTP $z_i^{\max} = (p_G - p_B)\theta < (p_G - p_B)(x_H - x_L) = z^*$. Their WTP for the Good Lottery reflects their expected reduction of lying costs. Hence, dishonest individuals have a smaller WTP than honest individuals in the URR. As a consequence, individuals with $z_i^{\max} < z^*$ are expected to misreport $\xi(x_L) = x_G$ and individuals with $z_i^{\max} \geq z^*$ are expected to report truthfully $\xi(x_L) = x_L$.[13]

In the Honest Regime an individual's readiness to misreport is irrelevant by design. In this regime individuals with different attitudes toward misreporting should articulate the same maximum WTP: $z^* = (p_G - p_B)(x_H - x_L)$. A comparison of both regimes reveals that dishonest individuals (as identified by their reporting behavior in the URR) have a smaller WTP in the URR than in the HR. In contrast, the regime has no influence on the WTP of honest individuals.

Finally, consider the reporting behavior conditional on the lottery allocation. Let the seller price of the Good Lottery be $z_0 \in (0, z^*)$. Individuals with a WTP smaller than z_0 should self-select into $(B, 0)$. Since $z_0 < z^*$, these individuals should misreport if such an opportunity emerges. Individuals with a WTP (weakly) higher than z_0 self-select into (G, z_0). This group comprises two types of individuals: individuals with $\theta \geq \frac{z^*}{p_G - p_B}$ who will not misreport and individuals with $\theta \in [\frac{z_0}{p_G - p_B}, \frac{z^*}{p_G - p_B})$ who will misreport $\xi(x_L) = x_H$. In the aggregate, the fraction of dishonest reports in the group of individuals who select into $(B, 0)$ should therefore be higher than in the group who end up with (G, z_0). These considerations are collected as

Hypothesis 2.2 *Consider the group of individuals who have the zero outcome x_L in the Untruthful Reporting Regime. In the Untruthful Reporting Regime*

(i) individuals who misreported $\xi(x_L) = x_H$ articulate a smaller WTP than truthfully reporting individuals;

(ii) individuals who misreported $\xi(x_L) = x_H$ articulate a smaller WTP than they do in the Honest Regime;

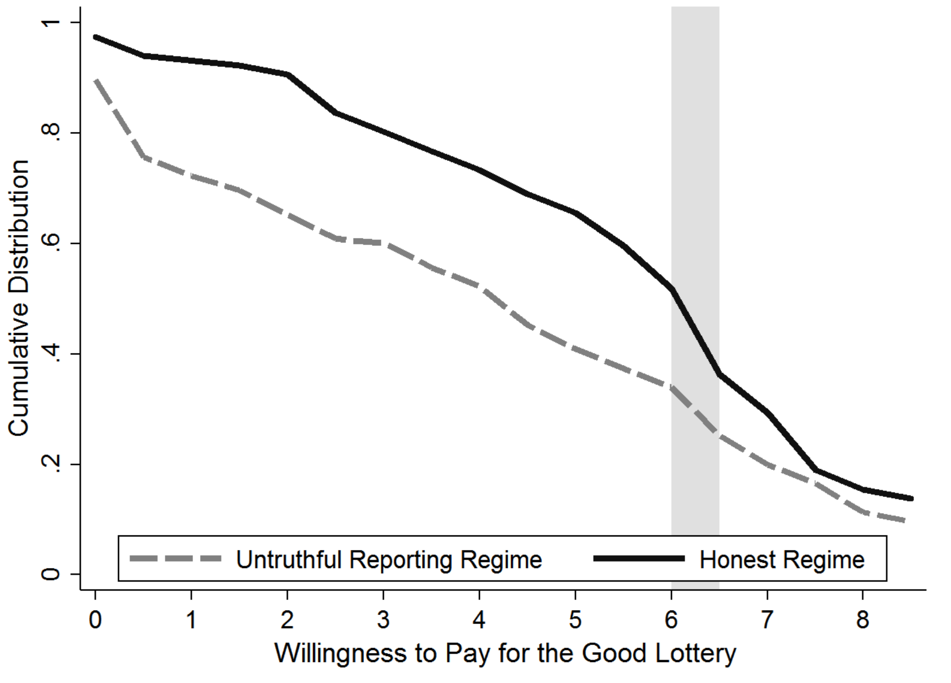
(iii) a higher fraction of individuals who select into $(B, 0)$ misreport as compared to individuals who select into (G, z_0).

[13] For many reasons (including risk aversion, lack of attention, calculation errors), the observed reporting behavior may deviate from this sharp prediction. In particular, for individuals with θ close to $\theta = \frac{z^*}{p_G - p_B}$, the difference between the payoff from lying, $x_H - \theta$, and the payoff from reporting truthfully, x_L, is small. Some of the individuals with a maximum willingness to pay above z^* might misreport, and some of the individuals with a maximum WTP below z^* may report honestly in the URR. A "softer" prediction that accounts for these effects is that the propensity to lie is a declining function of the maximum articulated WTP.

2.4 Results

The analysis of the experimental data evolves along the lines of our predictions. We start with a comparison of the WTP in the Untruthful Reporting Regime and the Honest Regime. We show that the WTP in the URR is significantly smaller. Then, we investigate the relationship of the WTP in the URR and the subsequent reporting behavior. We find evidence that the WTP is indicative of the reporting behavior and that subjects self-select based on their lying costs. For the most part of the analysis, we focus on subjects with a WTP smaller or equal to EUR 6 in both regimes and who make a consistent statement of the WTP.[14]

2.4.1 The Demand for the Good Lottery



Note: Graph displays cumulative distribution of a between-subjects comparison of the Willingness to Pay (in EUR) for the Good Lottery in the Untruthful Reporting Regime and the Honest Regime

FIGURE 2.2: Demand for the Good Lottery in the Regimes

[14] A higher valuation for the Good Lottery may not be explained by lying costs and, hence, is beyond the scope of our decision theoretic framework. The consistent statement of the WTP is defined as choosing either the Bad Lottery or the Good Lottery for all price levels, or as switching only once from the Good Lottery to the Bad Lottery at $z_i^{\max}$. Alternatively, we use a control variable in our regressions analyses. A detailed discussion and an analysis of the complete sample is provided in Appendix A.1.

We start with a (pseudo) between-subjects comparison of the WTP of the Good Lottery in the two regimes, focusing on the regime that was first in order. Figure 2.2 displays the aggregate distribution of subjects willing to buy the Good Lottery over the full support of possible prices. It has a similar interpretation as the theoretical demand curves $\gamma_T(z)$ and $\gamma_M(z)$ in Figure 2.1.[15] The solid line illustrates the empirical demand curve $\gamma_T(z)$ for the Good Lottery in the Honest Regime, and the dashed line the empirical demand curve for the Untruthful Reporting Regime.

Comparing the WTP in both regimes, Figure 2.2 suggests that the WTP in the HR first-order stochastically dominates the WTP in the URR. A stable gap between both regimes of around 20 percentage points emerges up to a WTP of EUR 6. Econometric tests for this range of prices confirm this observation ($N = 86$ in the URR and $N = 74$ in the HR): The average WTP of 2.45 EUR in the URR is significantly smaller than the average WTP of 3.88 EUR in the HR (Wilcoxon-Mann Whitney test: $p < 0.01$). A Kolmogorov-Smirnov test also rejects the null hypothesis of equal distributions ($p < 0.01$). Hence, subjects seem to take the misreporting opportunity into account when making their lottery choice, and they reduce their WTP accordingly. However, some subjects value the higher likelihood of an honest win outcome even in the presence of the misreporting opportunity. Only 14 percent have a non-positive WTP in the URR, and the average WTP in the URR is significantly larger than zero (one-sample median test: $p < 0.01$).[16] For prices higher than EUR 6, both empirical demand curves seem to converge, which may indicate that the decision is not related to lying costs in the range. Consistent with *Hypothesis 2.1(i)*, we summarize our findings in

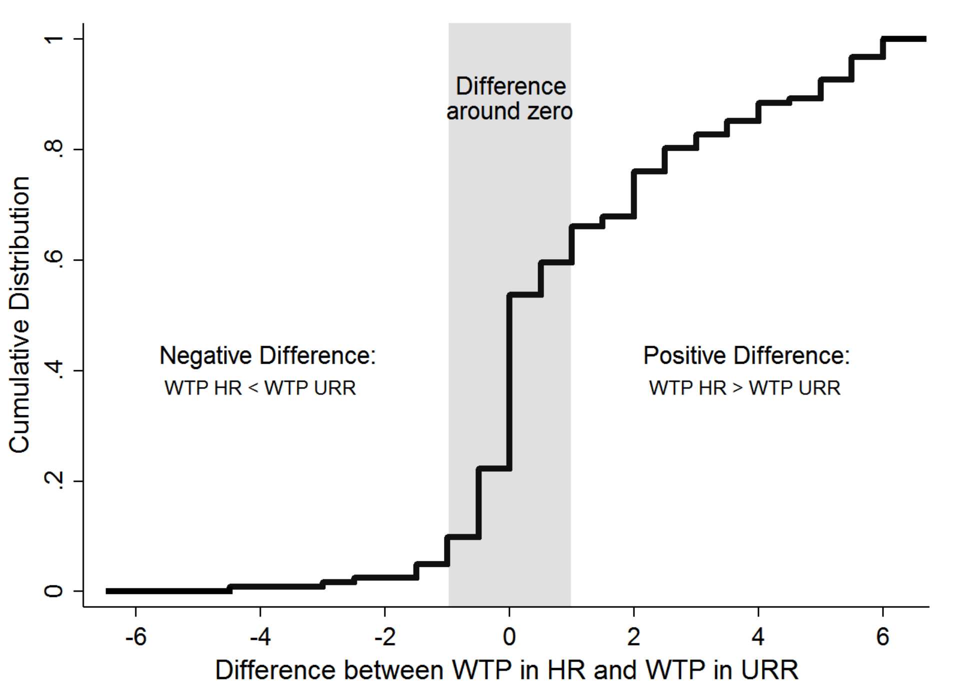
Result 2.1 *In a between-subjects comparison, the WTP in the Untruthful Reporting Regime is smaller than in the Honest Regime.*

2.4.2 The Within-Subjects Differences in the Willingness to Pay

Since all subjects state their WTP in the HR as well as in the URR, we are also able to make a within-subjects comparison of the valuation of the Good Lottery in both regimes. Figure 2.3 illustrates the within-subjects difference between the WTP in the Honest Regime and the Untruthful Reporting Regime, i.e., $z^{\max}_{i,HR} - z^{\max}_{i,URR}$. We

[15] The gray area between EUR 6 and EUR 6.5 represents the theoretical indifference price z^* as measurement of the WTP was implemented in intervals of EUR 0.50. Figure 2.2 displays the data without any restrictions to the level of the WTP in both regimes (URR: $N = 115$; HR: $N = 116$).

[16] A non-positive WTP in the URR may indicate subjects with lying costs of zero ($\theta = 0$) who are exactly indifferent between both lotteries.



Note: Graph displays cumulative distribution of within-subjects differences in the WTP between the Honest Regime and the Untruthful Reporting Regime

FIGURE 2.3: Within-subjects Differences in the WTP

abstract from subjects with a non-positive WTP in both regimes which leaves us with 121 subjects here.

In Figure 2.3, we mainly observe two groups of subjects. There is a large group with a difference in their WTP of around zero (between EUR -1 and EUR 1, 61 percent), indicated by the gray interval. These are subjects with a WTP which is broadly similar in both regimes.[17] Our decision-theoretic framework suggests that subjects in this group may have high lying costs ($\theta \geq 12$). In this case, subjects forgo the misreporting opportunity and both regimes are payoff-equivalent. Second, a group of 34 percent of subjects has a positive difference between EUR 1 and EUR 6, i.e., a higher WTP in the HR as compared to the URR. This group presumably consists of subjects with small to medium lying costs $\theta \in (0, 12)$. These subjects should be willing to misreport for the zero outcome. Their WTP for the Good Lottery is determined by the desire to avoid the costs of lying. The smaller the lying costs, the smaller the WTP in

[17] To account for small mistakes and fuzziness in choice behavior, we define the group that has a similar WTP in both regimes as having an absolute difference smaller or equal to EUR 1. A more narrow definition of this group (absolute difference smaller or equal to EUR 0.5) does not qualitatively change the results: 10 percent have a negative difference, 50 percent have a similar WTP in both regimes, and 40 percent have a positive difference.

the URR and the larger the difference between both regimes. The prevalence of these two groups of subjects with distinct differences in their WTP is in line with the prediction from our decision-theoretic framework.[18] Finally, the average within-subjects difference is EUR 1.07 between both regimes, which is significantly larger than zero (Wilcoxon signed ranks test: $p < 0.01$). In accordance with *Hypothesis 2.1(ii)*, we state:

Result 2.2 *In a within-subjects comparison, for the vast majority of subjects, the WTP in the Untruthful Reporting Regime is smaller than or equal to the WTP in the Honest Regime.*

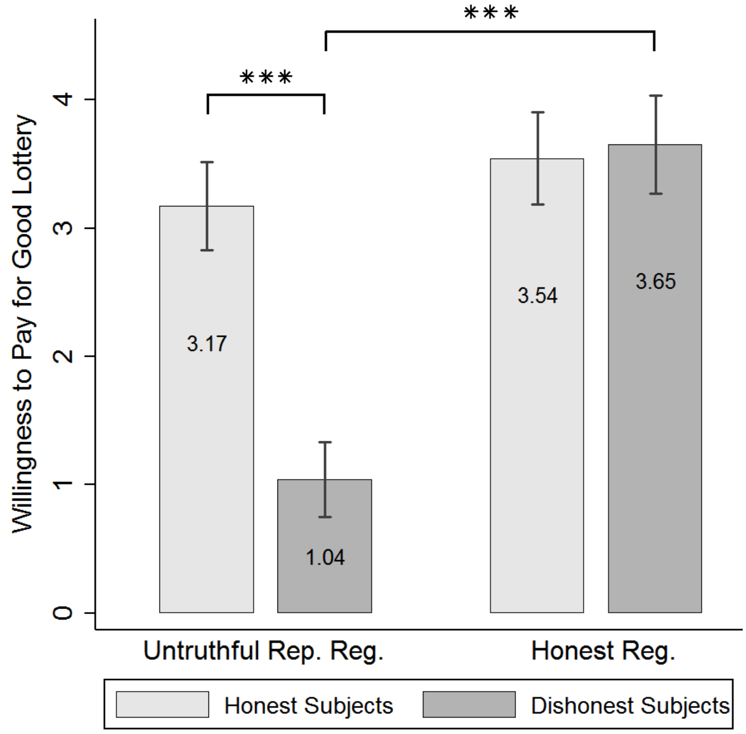
2.4.3 The Willingness to Pay Conditional on Reporting Behavior

The analysis so far has revealed that the misreporting opportunity in the URR decreases the demand for the Good Lottery. Now, we investigate the relationship of the WTP in the URR and the subsequent reporting behavior. In our decision-theoretic framework, both are determined by an individual's lying costs θ. Subjects hence may self-select based on their WTP, and report accordingly when facing the misreporting opportunity. To shed light on this, we focus on subjects with a zero outcome (for the respective lottery). These subjects had a monetary incentive to lie and to report the win outcome instead.[19]

Differences in the WTP Among Honest and Dishonest Subjects. We start by examining the WTP conditional on the observed reporting behavior in the URR ($N = 71$). Misreporting subjects are classified as dishonest subjects, and those who made a truthful report as honest. Figure 2.4 displays the average WTP in both regimes for honest and dishonest subjects, respectively. The between-subjects comparisons of the WTP reveal that dishonest subjects in the URR are willing to pay less than a third of what honest subjects would pay for the Good Lottery. This difference is highly significant (Wilcoxon-Mann Whitney test: $p < 0.01$). In contrast, there is no significant difference between honest and dishonest subjects in the HR (Wilcoxon-Mann Whitney test: $p = 0.74$). The within-subjects comparisons across regimes corroborate these findings: dishonest subjects reduce their WTP for the Good Lottery by more than 70 percent when they have the opportunity to misreport (Wilcoxon signed ranks test:

[18] There is a third group of subjects with a negative difference between EUR -4.5 and EUR -1. This group is beyond the scope of our framework, but with a population share of 5 percent it is also small in comparison to the other groups.

[19] For subjects with an honest win outcome ($N = 131$), we cannot observe whether they had lied or not, and therefore they are excluded here. However, none of these subjects lied downward by stating a lower number than actually drawn.



Note: WTP in the URR and HR conditional on reporting behavior in the URR.
Error bars indicate mean +/- SEM. Brackets indicate significant treatment differences, *** p<0.01.

FIGURE 2.4: Comparison of WTP conditional on Reporting Behavior

$p < 0.01$). For honest subjects, we find no such difference in the WTP between the regimes (Wilcoxon signed ranks test: $p = 0.45$).

In summary, as our decision-theoretic framework suggests, only in the URR is there a difference between dishonest and honest subjects, and only dishonest subjects have a smaller WTP in the URR than in the HR. Hence, our results indicate that dishonest subjects deliberately reduce their WTP in the presence of the misreporting opportunity. In contrast, both regimes are payoff-equivalent for honest subjects as they tend to state a similar WTP in the URR and HR. In line with *Hypothesis 2.2(i) and 2.2(ii)*, we summarize these findings in

Result 2.3 *Dishonest subjects have a smaller WTP in the Untruthful Reporting Regime*

(i) as compared to honest subjects;

(ii) as compared to their WTP in the Honest Regime.

VARIABLES	(1A) Honest	(1B) Dishonest	(2A) Honest	(2B) Dishonest
WTP Honest Regime	0.820***	0.162	0.649***	0.232
	(0.112)	(0.120)	(0.127)	(0.147)
Gender	-0.227	-0.652	0.331	-0.830
	(0.443)	(0.665)	(0.423)	(0.720)
Age	-0.027	0.078	-0.047	0.099
	(0.073)	(0.091)	(0.067)	(0.094)
Socio-Economic Controls	YES	YES	YES	YES
Procedural Controls	YES	YES	YES	YES
Post-Test	NO	NO	YES	YES
Constant	0.637	3.871	-0.946	4.058
	(1.993)	(2.900)	(1.774)	(2.915)
Observations	98	55	98	55

Notes: The table presents results of an interval regression. The depedent variable is the WTP in EUR in the Untruthful Reporting Regime. Specification "1A/2A" includes subjects that reported honestly a zero outcome and specification "1B/2B" includes subjects that dishonestly reported a win outcome and had a WTP smaller than or equal to EUR 6 in the URR. "WTP Honest Regime" is a subject's WTP for the Good Lottery in the Honest Regime in EUR, "Gender" is a dummy for female subjects, and "Age" is the age of the respective subject. "Socio-Economic Controls" include a dummy for business/economics students and a dummy for subjects with non-German mother tongue, "Procedural Controls" include a dummy for consistent choice behavior and control for order effects on both regimes, and "Post-Tests" include the number of correct answers (0 to 3) in the Cognitve Reflection Test (Frederick 2005), Murphy et al.'s (2011) measure of distributional preferences (-16.26 to 63.39), a dummy for ambiguity aversion (Ellsberg 1961) and an indicator for an individual's risk preferences. Robust standard errors in parentheses,
*** $p<0.01$.

TABLE 2.1: Multivariate Analysis of the WTP in the Untruthful Reporting Regime

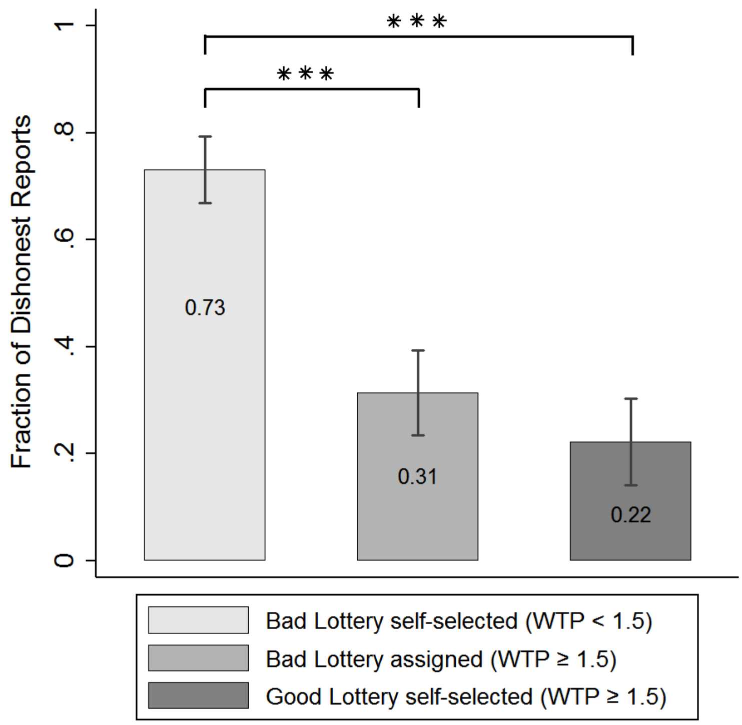
Multivariate Analysis of the WTP in the Untruthful Reporting Regime. We confirm these findings in an interval regression analysis on the WTP for the Good Lottery in the Untruthful Reporting Regime (Table 2.1).[20] The main result of interest is the coefficient on the WTP in the Honest Regime. For the subgroup of honest subjects (specifications (1A)/(2A)), we find a positive and highly significant relationship between the WTP in both regimes. The point estimate implies that an increase of EUR 1 in the WTP in the HR is associated with an increase of EUR 0.65 to EUR 0.82 in the WTP in the URR. As both regimes are payoff-equivalent for honest subjects, this is in line with our theory framework. In contrast, for the subgroup of dishonest subjects (specifications (1B)/(2B)), the coefficient on the WTP in the HR is small and not significantly different from zero. Hence, the WTP in the URR seems to be based

[20] Here, we control for a consistent statement of the WTP in 'Procedural Controls' directly. Moreover, as the misreporting opportunity is not relevant for honest subjects, we also include honest subjects with a WTP larger than EUR 6 in the URR (specification 1A/2A). Imposing the restriction of a WTP smaller or equal to EUR 6 for honest subjects does not qualitatively change the results in specification 1A/2A.

on a different motivation than in the HR, namely the subject's lying costs.

Result 2.4 *The WTP in the Honest Regime does not predict the WTP in the Untruthful Reporting Regime for dishonest subjects, which indicates that subjects who misreported based their WTP on their lying costs instead.*

2.4.4 Reporting Behavior Conditional on the Willingness to Pay



Note: Error bars indicate mean +/- SEM. Brackets indicate significant treatment differences, *** p<0.01.

FIGURE 2.5: Misreporting conditional on Lottery Allocation

(Dis-)Honest Reporting in Different Lotteries. As a next step, we analyze the reporting behavior based on a subject's WTP in the Untruthful Reporting Regime. The sample is again restricted to the subgroup of subjects with the zero outcome ($N = 114$).[21] Figure 2.5 displays the fraction of dishonest reports for the three possible lottery allocations. A comparison between the group 'Bad Lottery self-selected' ($N = 52$) and

[21] As the WTP is not the focus of the analysis, we do not impose the restriction of making a consistent statement of the WTP here. Results are robust to this selection choice.

the group 'Bad Lottery assigned' ($N = 35$) allows us to investigate the consequences of self-selection for dishonest behavior. Subjects in both groups take part in the cost-free Bad Lottery, but have a different WTP. In the former group, the WTP is below the predetermined price of EUR 1.5 for the Good Lottery, while subjects in the latter group were willing to acquire the Good Lottery but were randomly assigned to the Bad Lottery (prob. 1/3). Our decision-theoretic framework suggests that the first group consists predominantly of dishonest subjects and the second group of a mixture of honest and dishonest subjects. We find evidence in line with this prediction: Subjects with a small WTP are more than twice as likely to give a dishonest report as subjects with a high WTP. The difference of more than 40 percentage points is highly significant (χ^2-test: $p < 0.01$).

We also compare the first two groups to the group of subjects whose choice of the Good Lottery was implemented ('Good Lottery self-selected', $N = 27$, prob. 2/3). It is important to note that there are two core differences between the lotteries: the Good Lottery has a smaller probability of the zero outcome, and subjects pay EUR 1.50 for the Good Lottery independent of the outcome. Both aspects might affect the reporting behavior of subjects outside the scope of our model. The comparison of subjects that self-selected into the Bad Lottery with subjects that self-selected into the Good Lottery reveals that the latter are significantly less likely to use the misreporting opportunity (χ^2-test: $p < 0.01$). In contrast, we do not find a significant difference of the reporting behavior between subjects with a high WTP that were involuntarily assigned to the Bad Lottery and those who were assigned to their preferred Good Lottery (χ^2-test: $p = 0.42$).

These results provide evidence that lying costs do not depend on the implemented lottery, and that subjects do not select into the Good Lottery as it eases misreporting. Moreover, subjects with the zero outcome in the Good Lottery also seem to abstain from recovering their loss of EUR 1.5 by misreporting. Hence, self-selection into the Good Lottery reflects the preference for honest earnings: subjects with a high WTP in the Untruthful Reporting Regime have a lower propensity to misreport. In accordance with *Hypothesis 2.2(iii)*, we state

Result 2.5 *The fraction of dishonest subjects is significantly lower among subjects willing to self-select into the Good Lottery than among the set of subjects who self-select into the Bad Lottery.*

VARIABLES	(1)	(2)	(3)	(4)
WTP Untruthful Rep. Reg.	-0.300***	-0.312***	-0.364***	-0.329***
	(0.061)	(0.062)	(0.069)	(0.070)
Gender	-0.704**	-0.684**	-0.606**	-0.541*
	(0.276)	(0.277)	(0.282)	(0.321)
Age	0.024	0.017	0.018	0.038
	(0.039)	(0.039)	(0.042)	(0.033)
WTP Honest Regime			0.139**	
			(0.058)	
Socio-Economic Controls	YES	YES	YES	YES
Procedural Controls	NO	YES	YES	YES
Post-Test	NO	NO	NO	YES
Constant	0.430	-0.0407	-0.548	-1.404
	(0.981)	(1.085)	(1.199)	(1.056)
Observations	114	114	114	114

Notes: The table presents the results of probit specifications. The dependent variable is dishonest reporting (dummy variable). The dataset is restricted to subjects with a zero outcome and a WTP smaller than or equal to EUR 6 in the URR. "WTP Untruthful Rep. Reg." is a subject's WTP in the Untruthful Reporting Regime in EUR, "Gender" is a dummy for female subjects, "Age" is the age of the respective subject, and "WTP Honest Regime" is a subject's WTP in the Honest Regime in EUR. "Socio-Economic Controls" include a dummy for business/economics students and a dummy for subjects with non-German mother tongue, "Procedural Controls" include a dummy for consistent choice behavior and control for order effects on both regimes and "Post-Tests" include the number of correct answers (0 to 3) in the Cognitve Reflection Test (Frederick 2005), Murphy et al.'s (2011) measure of distributional preferences (-16.26 to 63.39), a dummy for ambiguity aversion (Ellsberg 1961) and an indicator for an individual's risk preferences. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

TABLE 2.2: Multivariate Analysis of Misreporting Behavior

Multivariate Analysis of the Reporting Behavior. Table 2.2 reports the results of a probit regression model with dishonest reporting as the dependent variable. Our model predicts a binary relationship between the WTP in the Untruthful Reporting Regime and reporting behavior. Here, we investigate a softer prediction, namely that a higher WTP in the URR is related to a smaller probability of misreporting. We find evidence for this prediction: the marginal effect of an increase of EUR 1 in an individual's WTP in the URR corresponds to a decrease in the probability of misreporting by 8 to 10 percentage points. Both the sign and the magnitude are robust to the inclusion of diverse control variables in the alternative specifications (2) to (4). Female subjects are approx. 13 to 20 percentage points less likely to misreport, while we do not find a significant effect on the age of subjects. The WTP in the Honest Regime significantly increases the probability of giving a dishonest report. Our model does not make any prediction on the relationship of both variables, but, importantly, the coefficient on the WTP in the URR remains unaffected. Finally, subjects with a higher cognitive ability (Frederick 2005) are significantly more likely to misreport, while none of the other post-tests (social value orientation, ambiguity aversion and risk preferences) has a significant effect.

Result 2.6 *Self-selection by a higher WTP in the Untruthful Reporting Regime leads to a smaller propensity of misreporting.*

2.4.5 Estimation of Lying Costs

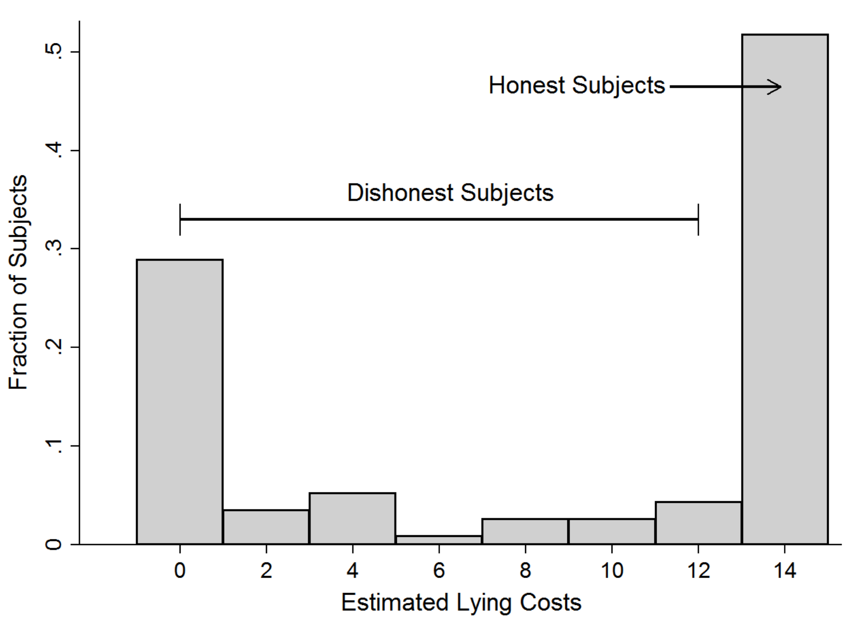
The final part of our analysis focuses on an estimation of the individuals' lying costs based on the WTP in the Untruthful Reporting Regime. This is a subtle way of measuring an individual's lying costs: first, subjects state their willingness to pay to improve their chances of drawing the win outcome honestly. Only then some subjects face the actual decision of whether to misreport or not. Importantly, the decision in both steps is determined by the individual's lying costs.

As lying is observable, the estimation procedure is based on the reporting behavior of subjects with the zero outcome.[22] Setting the left side of equation (2.4) equal to zero and solving for the lying costs θ, we recover a subject's lying costs as

$$\theta = \begin{cases} z/(p_G - p_B) & \text{if} \quad \xi(x_L) = x_H \\ x_H - x_L & \text{if} \quad \xi(x_L) = x_L \end{cases} . \tag{2.6}$$

Figure 2.6 displays the estimated distribution of the lying costs ($N = 114$). For honest subjects, the costs of lying outweigh monetary benefits, and hence, lying costs have to be equal to or higher than EUR 12. This is a lower bound of the true lying costs, and the proportion of these subjects is captured by the bar indicating lying costs of EUR 14. For dishonest subjects, the costs of lying are smaller than the monetary

[22] Our procedure may also allow the estimation of lying costs if the reporting behavior is not observable. The key challenge here is to separate honest subjects with prohibitively high lying costs but a smaller than predicted WTP (for behavioral reasons, such as risk preferences or loss aversion), from subjects that are willing to lie but have strictly positive lying costs. Both groups have a WTP above zero but below EUR 6 in the Untruthful Reporting Regime. However, since only dishonest subjects have a smaller WTP in the URR as compared to the HR, a similar WTP in both regimes may indicate honest subjects with prohibitively high lying costs. Classifying the latter group of subjects as honest allows for a separation of both groups and an estimation as proposed in equation (2.6).



Note: Estimation of lying costs is based on observed reporting behavior and the WTP in the URR.

FIGURE 2.6: Estimated Distribution of Lying Costs

benefits, and lying costs are recovered as of the WTP in the Untruthful Reporting Regime. The result of this estimation suggests an almost binary distribution of the lying costs. Roughly 30 percent of subjects have zero lying costs, and around 50 percent of subjects have prohibitively high lying costs. A remaining 20 percent of subjects have intermediate lying costs in the interval EUR $(0, 12)$. This is in line with survey evidence that subjects' reporting behavior is by and large unresponsive to increased incentives in cheating games (e.g., Abeler et al. 2019 and Gerlach et al. 2019).

Result 2.7 *The majority of subjects have either zero or prohibitively high lying costs, and only 20 percent of subjects fall in the intermediate range.*

2.5 Conclusion

The chapter studies differences between individuals who seemingly have no reservations of earning money by lying, and those who incur behavioral costs of misreporting and have a preference for honest money. Whether the former group of individuals are also more likely to deliberately select into misreporting opportunities is a crucial resulting question.

In our theory-guided experiment, we assess individuals' willingness to pay to earn money in an honest fashion. Based on this, we experimentally investigate whether subjects self-select into honest and dishonest earning opportunities based on their lying costs, and whether this self-selection is predictive of their reporting behavior. In the experiment individuals choose between a Bad and a Good Lottery. The Bad Lottery has a zero outcome with high probability. In contrast, the Good Lottery yields a zero outcome with a low probability. We elicit an individual's willingness to pay for choosing the Good Lottery over the Bad Lottery in two different regimes. One regime is truthful by design. The other regime allows for misreporting. In particular, individuals may claim the win outcome even if their true lottery outcome is zero. The choice of the Bad Lottery confronts individuals with the misreporting opportunity by a high chance whereas the Good Lottery involves misreporting only with a low chance.

Our results indicate that the presence of the misreporting opportunity leads to a strong reduction in the WTP for the Good Lottery. Both in a between-subjects as well as in a within-subjects comparison, the demand for the Good Lottery in the truthful regime first-order stochastically dominates its demand in the regime with a misreporting opportunity. However, this finding is not unique for all subjects: they seemingly anticipate their ability to withstand or surrender to the temptation of dishonest earnings and select the lottery accordingly. Some subjects are not willing to spend money on the Good Lottery, and tend to misreport whenever the opportunity arises. Other subjects expend considerable resources on making their earnings honestly. In turn, these subjects are also less likely to misreport on the low lottery outcome. We interpret these findings as evidence that subjects self-select into misreporting opportunities based on their individual lying costs.

Our findings may have implications outside the lab. While some situations in real life leave subjects with little influence on the opportunity to misreport, for instance, as the opportunity arises by chance or by complete surprise, others allow for an anticipation of the cheating opportunity and to pre-plan how to behave optimally. An example might be insurance fraud. Our results suggest that dishonest subjects are more likely to self-select into environments with cheating opportunities, and consequences for truth-telling may be detrimental if such self-selection is possible.

2.A Appendix – Robustness Results for the unrestricted Sample

In our main analysis, we impose a few restrictions on our data set in order to test the predictions of our theoretical framework. Apart from making a consistent statement of the WTP, i.e., not switching back and forth for different price levels or switching in the wrong direction, a further restriction is the limitation of the WTP in the Untruthful Reporting Regime, and consequently the WTP in the Honest Regime, to EUR 6.[23] This leads to a reduction in the total number of observations. In this appendix, we report (sub)section by (sub)section our findings for the unrestricted sample and show that they are in line with our results from the main analysis. It is important to note that testing our hypotheses in the unrestricted sample comes at the cost of analyzing data outside the scope of our theoretical framework. For the WTP in the URR and the HR (compare to subsections 2.4.1 and 2.4.2), no restrictions apply and we analyze the complete sample ($N = 308$). As subjects with an honest win outcome ($N = 131$) have no incentive to lie, the analysis of the relationship of reporting behavior and the WTP (compare to subsections 2.4.3, 2.4.4 and 2.4.5) is restricted to the group of subjects with a zero outcome ($N = 177$). In this group, we distinguish between honest subjects that truthfully reported the zero outcome ($N = 98$) and dishonest subjects that lied and reported the win outcome ($N = 79$).

Subsection 2.4.1: The Demand for the Good Lottery. The pseudo between-subjects comparison of the WTP in the first decision reveals that the average WTP in the URR is EUR 4.52 ($N = 154$) and the average WTP in the HR is EUR 5.38 ($N = 154$). The difference between both regimes is significantly different from zero (Wilcoxon-Mann Whitney test: $p = 0.03$) and a Kolmogorov-Smirnov test rejects the null hypothesis that both distributions are equal ($p = 0.01$). Hence, result 2.1 is robust.

Subsection 2.4.2: The Within-Subjects Difference in the Willingness to Pay. The average within-subjects difference between the HR and URR is EUR 0.56 and significantly larger than zero (Wilcoxon signed ranks test: $p < 0.01$, $N = 308$). For the decomposition into the three groups (compare to Figure 2.3), we find that 55.8

[23] Several potential explanations come to mind why the WTP might be larger than EUR 6 in the URR. First, the WTP for subjects with high lying costs ($\theta \geq 12$) is determined by risk preferences as in the HR. If a subject's WTP in the HR is larger than EUR 6, the WTP in the URR should also be larger than EUR 6. Second, some subjects may disregard the misreporting opportunity, for example, due to a lack of awareness (see Fosgaard et al. (2013) and Lohse et al. (2018)). These subjects will not misreport and therefore face the same decision problem as in the HR. Third, some subjects might have kleptomaniac traits, i.e., they misreport whenever there is an opportunity to do so, even if it does not pay off in the respective situation. These subjects may want to avoid the temptation to lie and may therefore have a WTP larger than EUR 6 in the URR.

percent have a similar WTP in the HR and in the URR, 28.6 percent have a higher WTP in the HR as compared to the URR and 15.6 percent have a higher WTP in the URR as compared to the HR. The third group has a slightly larger share than in the main analysis, but qualitatively the results are the same. Hence, result 2.2 is robust.

Subsection 2.4.3: The Willingness to Pay Conditional on Reporting Behavior. As for the main analysis, we compare the WTP conditional on reporting behavior. Dishonest subjects have a WTP of EUR 5.15 in the HR and of EUR 3.34 in the URR, while honest subjects have a WTP of EUR 5.39 in the HR and of EUR 5.19 in the URR (compare to Figure 2.4). Between honest and dishonest subjects, the difference in the URR is highly significant, while we find no such difference in the WTP in the HR (Wilcoxon-Mann Whitney test: $p < 0.01$ and $p = 0.52$, respectively). Within-subjects and between regimes, the difference in the WTP is not significant for honest subjects but is highly significant for dishonest subjects (Wilcoxon signed ranks test: $p = 0.45$ and $p < 0.01$, respectively). Hence, result 2.3 is robust. The interval regression analysis on the WTP in the URR (Table 2.1) reveals that a higher WTP in the HR leads to a significantly higher WTP in the URR, both for honest and for dishonest subjects. This finding for dishonest subjects deviates from our result in the restricted sample, potentially due to a change in the behavioral pattern for subjects with a high WTP in the URR.

Subsection 2.4.4: Reporting Behavior Conditional on the Willingness to Pay. Finally, we investigate the effect of self-selection on reporting behavior. 70 percent of subjects in the group 'Bad Lottery self-selected' are dishonest ($N = 60$), 31 percent of subjects in the group 'Bad Lottery assigned' are dishonest ($N = 67$), and 32 percent of subjects in the group 'Good Lottery self-selected' are dishonest ($N = 50$)(compare to Figure 2.5). The difference between the first and the second group is highly significant, as is the difference between the first and the third group (χ^2-test: $p < 0.01$, respectively). The difference between subjects with a high WTP that were involuntarily assigned to the Bad Lottery and those that were assigned to the preferred Good Lottery is small and not significant (χ^2-test: $p = 0.94$). The probit regression analysis on the reporting behavior (Table 2.1) reveals that a higher WTP in the URR is related to a higher propensity to misreport. Hence, result 2.5 and result 2.6 are robust.

Subsection 2.4.5: Estimation of Lying Costs. As in our main analysis, we focus on subjects with the zero outcome in the URR ($N = 177$). The estimation of the lying costs reveals a similar pattern as for the restricted sample: 19 percent of subjects have lying costs of zero, while 55 percent have prohibitively high lying costs and abstain from misreporting. A fraction 12 percent of subjects have an intermediate range of lying costs between $\theta \in (0, 12)$, while 14 percent of subjects have lying costs $\theta > 12$ but are dishonest. In sum, result 2.7 is robust.

2.B Appendix – Further Results on the Willingness to Pay

The design of our experiment faced two challenges: On one hand, our reasoning relies on the fact that any difference in the WTP between both regimes traces back to the presence of the misreporting opportunity. Hence, we minimized all other potential differences between the Honest Regime and the Untruthful Reporting Regime. However, a crucial precondition for the validity of our results was that subjects anticipated the lack/presence of the misreporting opportunity in the HR/URR when stating their WTP. This required some subtle changes in the implementation of both regimes. For example, the result of the lottery was visualized by a wheel of fortune in the URR, while there was no visualization in the HR. Moreover, the necessity to make a report per se may have affected the WTP in the URR.

In order to exclude any influence of these differences on subjects' WTP, we ran four additional sessions of a Robustness Regime (RR) and the Honest Regime.[24] This RR was identical to the URR in all aspects of the implementation, except that subjects were required to make an honest report by design. Instructions were modified such that subjects were able to anticipate that they would have no opportunity to misreport. In contrast, the HR was not modified and is directly comparable to the HR in our main experiment.

Figure 2.B shows the distribution of the WTP in the Robustness Regime and the Honest Regime for subjects with a consistent statement of their WTP. We focus on the regime that was first in order only (compare to section 2.4.1). Unlike for Figure 2.2, a visual inspection reveals no persistent 'gap' in the WTP between both regimes but rather suggests that both curves are equal. The average WTP in the RR is EUR 4.96 ($N = 26$), while the average WTP in the HR is EUR 5.13 ($N = 32$). The difference of EUR 0.16 is not significantly different from zero (Wilcoxon-Mann Whitney test: $p = 0.79$), and a Kolmogorov-Smirnov test does not reject the null hypothesis of equal distributions ($p = 1.00$). This result is robust to the inclusion of all subjects (EUR 5.47 vs. EUR 5.47, Wilcoxon-Mann Whitney test: $p = 0.97$, $N = 74$).

The average within-subjects difference between both regimes (compare to section 2.4.2) is EUR 0.15 and not significantly different from zero (Wilcoxon signed ranks test: $p = 0.56$). A vast majority of 77 percent of subjects have a similar WTP in both regimes (absolute difference is smaller than or equal to EUR 1), while the fraction of

[24] The sessions with a total of 74 subjects took place in January 2019 at the econlab Munich.

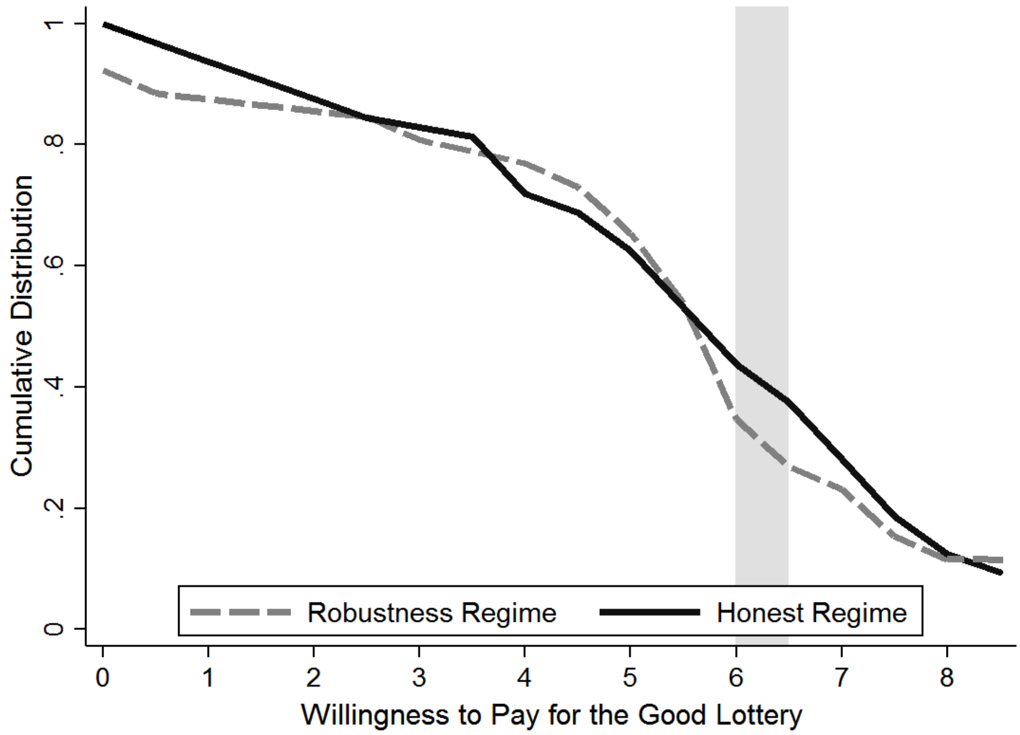
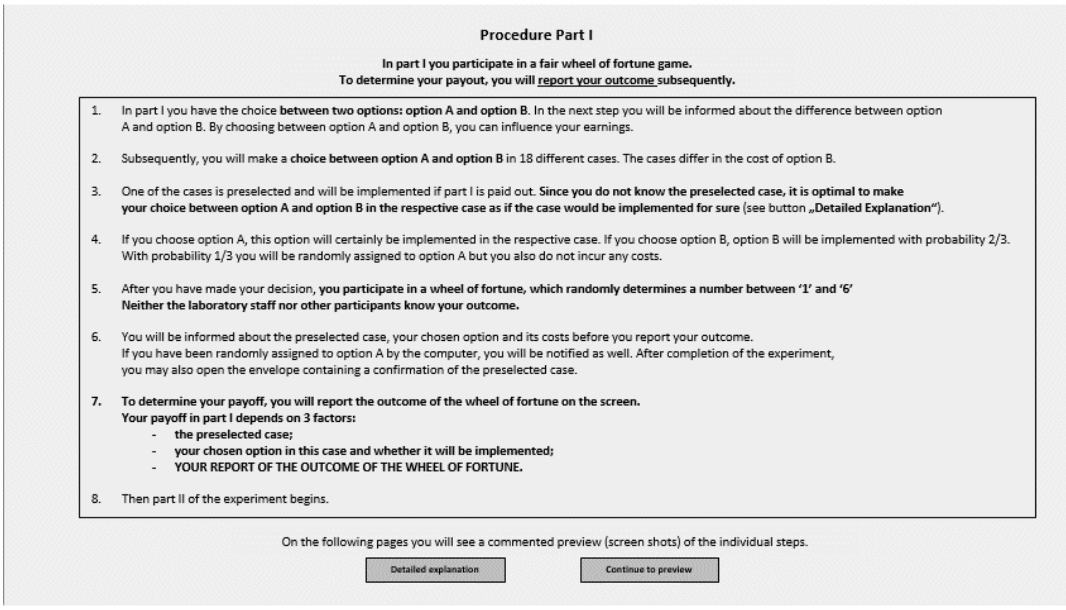


FIGURE 2.B: Comparison WTP in the Robustness Regime and the Honest Regime

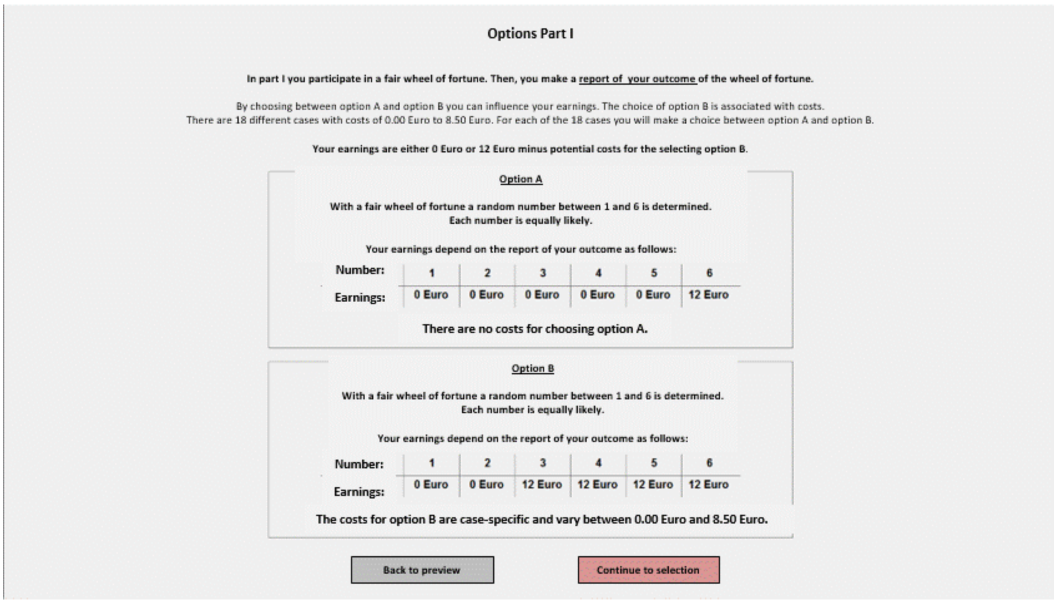
subjects with a negative and positive difference is 11 and 12 percent, respectively. We do not observe a prevalence of the two groups with a positive or a zero difference. In summary, the comparison of the Robustness Regime and the Honest Regime reveals no evidence of a difference in the WTP between both regimes which required truthfulness by design. This suggests that the technical differences in the implementation of the URR and the HR regime in our main experiment did not affect the WTP in a particular direction. Instead, it is rather the opportunity to misreport which leads to treatment effects.

2.C Appendix – Screen-Shots from the Experiment

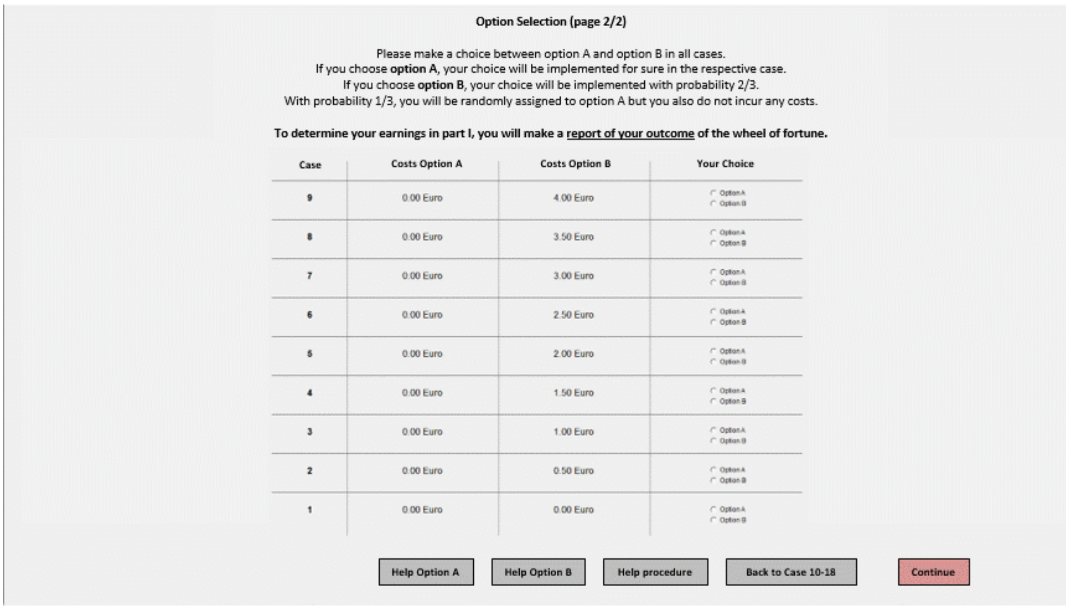
As noted in section 2.3.1, subjects found all instructions for the experiment on screen. This appendix presents a selection of screen-shots from the experiment. The experiment was conducted in German, and we provide an English translation here. For brevity and clarity, we only show the most important or most characteristic screen-shots. The actual experiment consisted of screens before, in between, and after the selection of screens shown here. Captions of the respective screen-shot provide additional information to which stage and to which treatment the screen-shot belongs to (URR for Untruthful Reporting Regime, HR for Honest Regime).



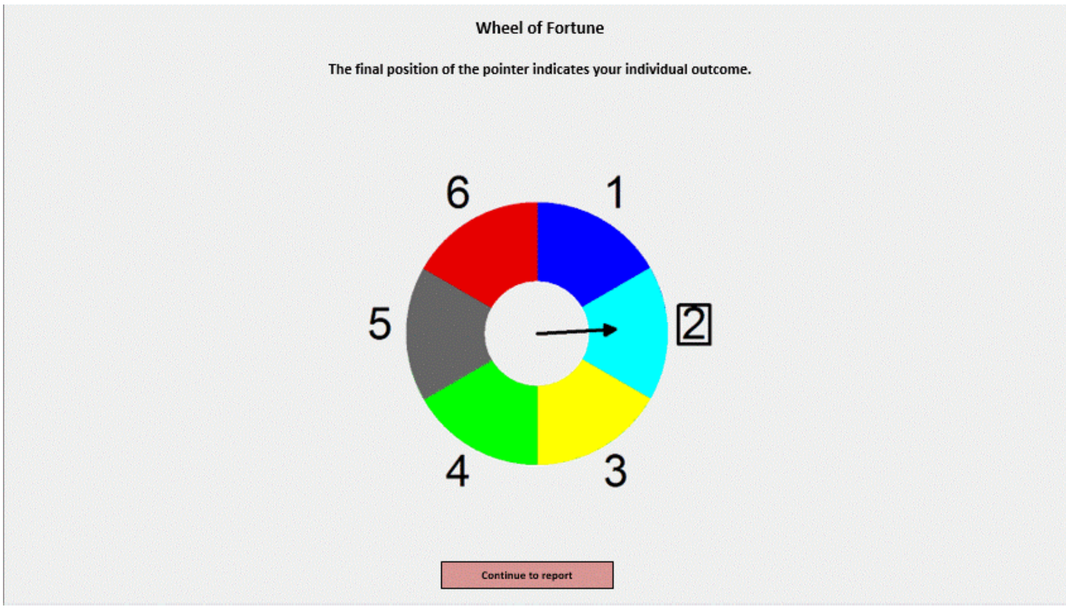
URR: Summary of the procedure. In addition, subjects saw a preview of each of the steps.



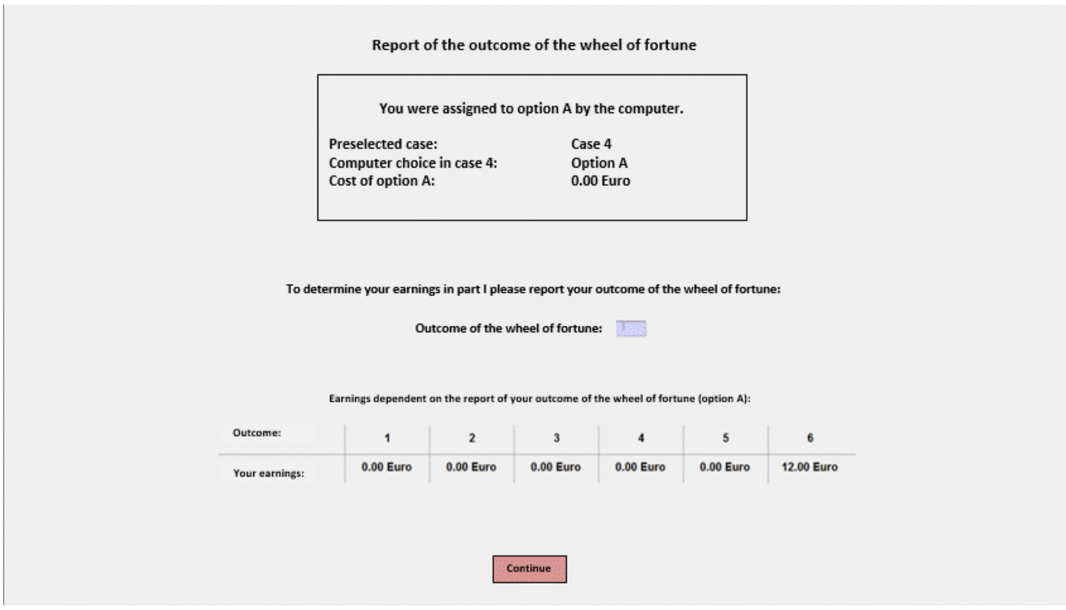
URR: Explanation of the 'Bad Lottery' and the 'Good Lottery'.



URR: Elicitation of the Willingness to Pay for the 'Good Lottery'.



URR: Lottery outcome (Wheel of Fortune, final state).



URR: Report of lottery outcome.

Instructions Part II

In part II you participate in different lotteries in four independent scenarios.

All scenarios follow the same procedure:

Process scenarios

1. In each scenario you have the **choice between two options: option A and option B.** At the beginning of each scenario, you will be told the difference between option A and option B. By choosing between option A and option B, you can influence your earnings.

2. Then you will make a **choice between option A and option B in different cases.** The cases differ in the cost of option B.

3. In each scenario, one of the cases is preselected and will be implemented if the scenario is paid out. **Since you do not know the preselected case, it is optimal to make your choice between option A and option B in the respective case as if the case would be implemented for sure** (see button **„Detailed Explanation"**).

4. If you choose option A, this option will certainly be implemented in the respective case. If you choose option B, option B will be implemented with probability 2/3. With probability 1/3 you will be randomly assigned to option A but you also do not incur any costs.

5. After you have made your choice**, the computer randomly determines a number between '1' and '6'**.

Your earnings in each scenario thus depends on 3 factors:
- **the preselected case;**
- **your chosen option in this case and whether it will be implemented;**
- **the randomly determined number.**

6. The next scenario begins. After scenario IV you will answer some more questions before the experiment is completed.

7. At the end of the experiment, you will be informed if part II (and if so which scenario) is payoff-relevant. For this scenario, you will be told which case was preselected, whether your chosen option was implemented, and which number was drawn. You can find a confirmation of the preselected case in the envelope.

On the following pages you will see a commented preview (screen shots) of the individual steps in the scenarios.

[Detailed explanation] [Continue to preview]

HR: Summary of the procedure. In addition, subjects saw a preview of each of the steps.

Options Scenario I

In this scenario you participate in a lottery with the two options A or option B.

By choosing between option A and option B, you can influence your earnings. The choice of option B is associated with costs.
There are 18 different cases with costs from 0.00 Euro to 8.50 Euro. For each of the 18 cases you will make a choice between option A and option B.

Your earnings are either 0 Euro or 12 Euro minus potential costs for the selecting option B.

Option A

The computer randomly determines a number between 1 and 6.
Each number is equally likely.

Your earnings depend on the number drawn as follows:

Number:	1	2	3	4	5	6
Earnings:	0 Euro	0 Euro	0 Euro	0 Euro	0 Euro	12 Euro

There are no costs for choosing option A.

Option B

The computer randomly determines a number between 1 and 6.
Each number is equally likely.

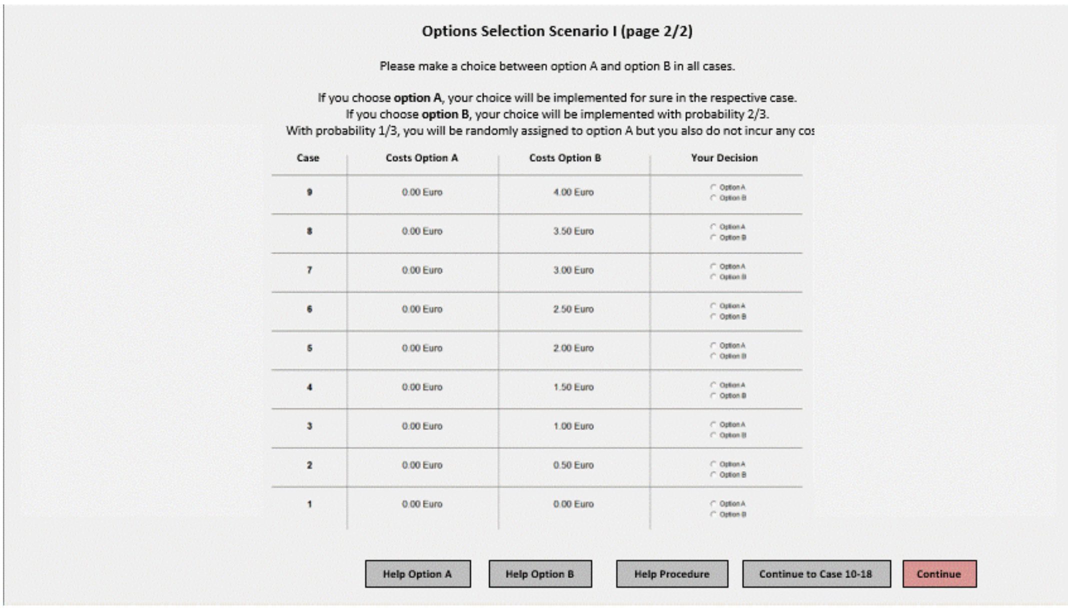
Your earnings depend on the number drawn as follows:

Number:	1	2	3	4	5	6
Earnings:	0 Euro	0 Euro	12 Euro	12 Euro	12 Euro	12 Euro

The costs for option B are case-specific and vary between 0.00 Euro and 8.50 Euro.

[Continue to selection]

HR: Explanation of the 'Bad Lottery' and the 'Good Lottery'.



Options Selection Scenario I (page 2/2)

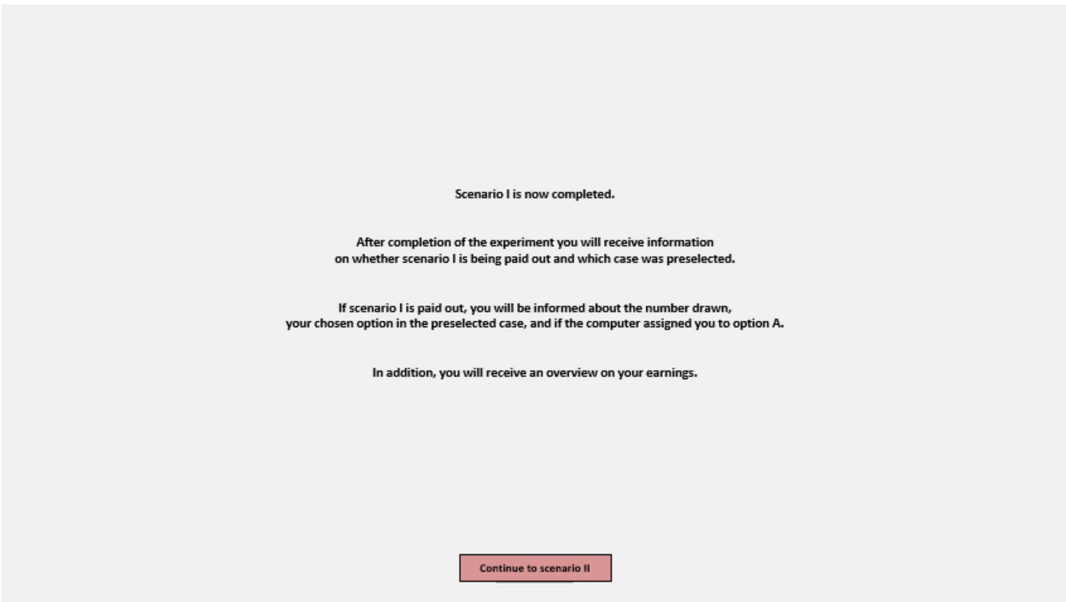
Please make a choice between option A and option B in all cases.

If you choose **option A**, your choice will be implemented for sure in the respective case.
If you choose **option B**, your choice will be implemented with probability 2/3.
With probability 1/3, you will be randomly assigned to option A but you also do not incur any cos

Case	Costs Option A	Costs Option B	Your Decision
9	0.00 Euro	4.00 Euro	Option A / Option B
8	0.00 Euro	3.50 Euro	Option A / Option B
7	0.00 Euro	3.00 Euro	Option A / Option B
6	0.00 Euro	2.50 Euro	Option A / Option B
5	0.00 Euro	2.00 Euro	Option A / Option B
4	0.00 Euro	1.50 Euro	Option A / Option B
3	0.00 Euro	1.00 Euro	Option A / Option B
2	0.00 Euro	0.50 Euro	Option A / Option B
1	0.00 Euro	0.00 Euro	Option A / Option B

Help Option A | Help Option B | Help Procedure | Continue to Case 10-18 | Continue

HR: Elicitation of the Willingness to Pay for the 'Good Lottery'.



Scenario I is now completed.

**After completion of the experiment you will receive information
on whether scenario I is being paid out and which case was preselected.**

**If scenario I is paid out, you will be informed about the number drawn,
your chosen option in the preselected case, and if the computer assigned you to option A.**

In addition, you will receive an overview on your earnings.

Continue to scenario II

HR: Final Stage.

3

Misreporting under Ignorance

3.1 Introduction

Is this item applicable to you? Many questions and forms require a binary answer, and ‘I don’t know’ is not an available option. So what do you do here? Obviously, not disclosing additional earnings from a speaker’s fee on a business trip to your employer is deceptive, but privately filling for compensation on delays or collecting miles on such trips is fine? Concealing losses from impatient investors is accounting fraud, but praising the new business strategy despite lacking the financial numbers is just how the game is played? The lack of knowledge in reporting situations like these creates a trade-off. On the one hand, people can abstain from making a potentially false claim. This leads to relief from all negative consequences of a dishonest claim in the future. Nevertheless, people might regret this choice when it turns out that the claim would have been justified. On the other hand, people can choose the self-serving option. However, the claim might be untruthful, and ignorance may not serve as a valid excuse in this case. Hence, people may incur a similar discomfort as if they had deliberately lied. They may suffer from intrinsic costs of violating (self-imposed) social norms. They may see their self-image as an honest person endangered. Or they might be afraid of losing their social image as a person of integrity.

Our research questions are motivated by exactly this trade-off. What do subjects report when they do not know whether their claim is justified? Is there a behavioral cost of making a potentially unjustified claim? And how does the reporting decision under ignorance relate to the reporting decision with full information? Additionally, we investigate into the determinants of the reporting decision under ignorance. Does the probability that a claim is justified affect the reporting behavior? Is there a social norm regarding reporting under ignorance and does it differ from the norm regarding reporting with information? Finally, do people prefer to stay ignorant when being able to self-select, or do they rather pay for information to avoid the ambiguity in the reporting decision?

We address these questions in a theory-guided laboratory experiment. Based upon an individual lottery outcome, subjects are eligible either to a high payoff or to a low payoff. Importantly, subjects can manipulate their earnings as they are asked to claim the payoff themselves. Along the first treatment dimension, we vary whether subjects know if their lottery outcome qualifies for the high or low payoff, or whether they need to make their claim under ignorance. Along the second treatment dimension, we vary the ex-ante probability that a lottery outcome is eligible to the high payoff (which is common knowledge). We also elicit subjects' beliefs on the reporting behavior of others and on the social norm under both information conditions. Moreover, subjects can self-select into information and ignorance in a modified reporting task at the end of the experiment.

We find a substantial effect of ignorance on reporting behavior. Ignorant subjects make almost twice as many unjustified claims as informed subjects. In contrast, the probability of being eligible to the high payoff itself has only a small impact, but boosts the fraction of unjustified claims to almost 90 percent under ignorance. Beliefs on the reporting behavior are accurate on average, but show a bias towards the own claiming choice. Especially subjects avoiding a potentially unjustified claim expect more other subjects to do so, too. Moreover, subjects agree that a deliberate unjustified claim of the high payoff is dishonest. However, the social norm on the reporting behavior under ignorance is more controversial, and beliefs are spread over the entire sphere between honesty and dishonesty. Finally, most subjects are information seeking when being allowed to self-select, but only willing to spend little resources on identifying their eligibility to payoffs.

3.2 Related Literature

Our contribution to the literature is twofold. A growing literature investigates the role of information avoidance and (deliberate) ignorance for decision-making in moral dilemmas (for recent surveys, see Hertwig and Engel 2016, and Golman et al. 2017). As dishonest reporting is a specific case of a moral dilemma, we contribute to this literature in a broader sense. Research on this topic has mostly focused on selfish behavior at the expense of others. For example, in Dana et al.'s (2007) binary dictator game, dictators may remain ignorant about the consequences of their allocation choice. Specifically, the payoff-maximizing choice for the dictator is either beneficial for the recipient, or leads to a reduction in the recipient's payoff. A significant proportion of dictators stay ignorant in order to take the (presumably) selfish option while keeping up the (self-)image of fair person. A number of studies corroborate and extend this finding. Grossman (2014) shows that the propensity to avoid information depends on whether it needs to be actively or passively chosen. As peers tend to be more lenient towards uninformed selfish choices, ignorance may also serve as a strategic device (Conrads and Irlenbusch 2013 and Bartling et al.'s 2014). Other papers investigate the role of self-signaling and social-signaling concerns for selfish decision-making under ignorance (Grossman 2015, and Grossman and van der Weele 2017), highlight differences in information seeking/avoiding behavior between objective and subjective norm compliers (Spiekermann and Weiss 2016) or emphasize the effect of self-selecting into ignorance (Kajackaite 2015, and Serra-Garcia and Szech 2018). We contribute to this literature by taking a step back. Our reporting task is structurally simpler as it does not involve any fairness or distributional concerns, any financial harm to other subjects or any strategic considerations. Making a (potentially) unjustified claim of the high payoff in our setting is a self-serving but not a selfish action. However, as subjects are asked to only claim what they are eligible for, claiming a high payoff represents non-compliance with explicit rules. Our results suggest that ignorance enables subjects to violate this requirement of an honest claim more often, but that some people feel obliged to refrain from a potentially unjustified claim in case of doubt.

Secondly, we contribute to the experimental literature on lying behavior (see the seminal contributions by Gneezy 2005, Mazar et al. 2008, and Fischbacher and Föllmi-Heusi 2013). Recent surveys find that a substantial fraction of the experimental population abstains from lying for a monetary gain (Abeler et al. 2019, Gerlach et al. 2019). This abstention traces back to the behavioral costs of dishonest reporting, which may be composed of intrinsic costs, self- or social image concerns and the fear of violating social norms (e.g. Abeler et al. 2019, Gneezy et al. 2018). The literature has identified a number of behavioral determinants of dishonesty,[1] but evidence on uncertain lies is scarce. Dugar et al. (2019) show that senders are more likely to deceive receivers if the harm of the deceptive message is unknown. However, senders are perfectly aware of sending a deceptive message. We build on this literature. While dishonestly claiming the high payoff with information resembles the basic setting, the ignorance dimension is new to the literature. Making a claim of a high payoff under ignorance may be regarded ex-ante as a partial lie. Ex-post, however, the claim of the high payoff is either justified or unjustified. The investigation of this trade-off for honest people is the core contribution of this chapter.

3.3 Decision Theoretic Framework

Structure[2] Consider a risk-neutral, payoff-maximizing agent i. Suppose this agent takes part in a lottery with multiple outcomes. It is common knowledge that any lottery outcome is eligible to a high payoff x_H with probability p, and to a low payoff x_L with probability $(1 - p)$. However, agents differ with respect to the information on the eligibility of their specific lottery outcome. Some agents know about their eligibility to the high or low payoff, denoted by s_H or s_L, while other agents receive no information, denoted by $s_\varnothing$. The information is a draw from the information set $S = \{s_H, s_L, s_\varnothing\}$. Irrespective of their draw, agents have to claim either the high payoff x_H or the low payoff x_L, i.e. they need to claim $\xi(S) \in \{H, L\}$. This claimed payoff determines their monetary payoff from the lottery.

Lying Costs Each agent i is characterized by a tuple $(\theta_{Inf}, \theta_{Ign})$ with $\theta_{Inf} \in [0, \infty)$ and $\theta_{Ign} \in [0, \infty)$. Let θ_{Inf} denote an agent's behavioral cost of dishonestly claiming x_H with information, $\xi(s_L) = H$, and θ_{Ign} denote an agent's behavioral cost of making

[1] For illustration, these determinants include positive and negative externalities of lying (e.g. Erat and Gneezy 2012), the ex-ante probability of drawing the win outcome (e.g. Abeler 2019), individual versus team decisions (e.g. Kocher et al. 2018), the role of emotions (e.g Coricelli et al. 2010) or of feelings of being treated unfair (Houser et al. 2012), the time dimension (e.g. Lohse et al. 2018), the relationship of creativity and dishonesty (Gino and Ariely 2012), or socio-economic characteristics (e.g. Shalvi and Leiser 2013). The list is by no means complete.

[2] The notation and structure of the analysis in this section borrows from chapter 2.

a potentially unjustified claim of x_H under ignorance, $\xi(s_\varnothing) = H$.[3] Hence, payoffs from claiming $\xi(S) = H$ and $\xi(S) = L$ are the following:

	s_H	s_L	$s_\varnothing$
$\xi(S) = H$	x_H	$x_H - \theta_{Inf}$	$x_H - \theta_{Ign}$
$\xi(S) = L$	x_L	x_L	x_L

Both the existence and heterogeneity of lying costs θ_{Inf} is well documented (e.g. Gneezy et al. 2018 and Abeler et al. 2019) and might be interpreted as the sum of intrinsic lying costs and self- or social-image concerns. The costs of making a potentially unjustified claim under ignorance follow from a similar reasoning.[4] However, we assume $\theta_{Ign} \leq \theta_{Inf}$ for all agents. Ignorance may serve as an excuse for claiming the high payoff, leading to smaller intrinsic lying costs. Moreover, (self-imposed) social norms may require a less clear-cut reporting decision as compared to information, and the self- and social-image may be less threatened. Figure 3.1 displays the distribution of agents. All of the probability mass is located below the 45-degree line as of $\theta_{Ign} \leq \theta_{Inf}$.[5]

Reporting with Information The information on the eligibility to payoffs allows for a conditional reporting strategy. Whenever an agent observes s_H, she claims $\xi(s_H) = H$. In contrast, if an agent observes s_L, her reporting behavior depends on her lying costs θ_{Inf}. For lying costs $\theta_{Inf} \geq x_H - x_L$, she honestly claims $\xi(s_L) = L$.[6] For lying costs $\theta_{Inf} < x_H - x_L$, she dishonestly claims $\xi(s_L) = H$ and incurs the lying costs of θ_{Inf}. The dotted vertical line in Figure 3.1 illustrates this division into honest and dishonest agents.

The reporting behavior leads to the following ex-ante payoff from the participation in the lottery with information

$$\pi_{Inf} = \begin{cases} x_H - (1-p)\theta_{Inf} & \text{if} \quad \theta_{Inf} < x_H - x_L \\ px_H + (1-p)x_L & \text{if} \quad \theta_{Inf} \geq x_H - x_L \end{cases} . \tag{3.1}$$

[3] For the simplicity of notation, we refer to an agent's tuple as $(\theta_{Inf}, \theta_{Ign})$ instead of $(\theta_{Inf,i}, \theta_{Ign,i})$. θ_{Inf} and θ_{Ign} need not be correlated, e.g. higher lying costs with information do not necessarily imply higher costs of making a potentially unjustified claim under ignorance. If the latter costs were equal to the expected lying costs, i.e. $\theta_{Ign} = (1-p)\theta_{Inf}$, the decision theoretic framework would boil down to a model of information-seeking behavior only.

[4] As claiming the low payoff $\xi(s_\varnothing) = L$ under ignorance may be unjustified as well, an alternative interpretation of θ_{Ign} is the spread in the behavioral costs of claiming the high payoff $\xi(s_\varnothing) = H$ over the low payoff $\xi(s_\varnothing) = L$.

[5] In the following, we are agnostic about the distribution of agents, except that none of the subsets M_1 to M_5 in Figure 3.1 is empty.

[6] In the indifference case $\theta_{Inf} = x_H - x_L$, we suppose that an agent honestly claims $\xi(s_L) = L$.

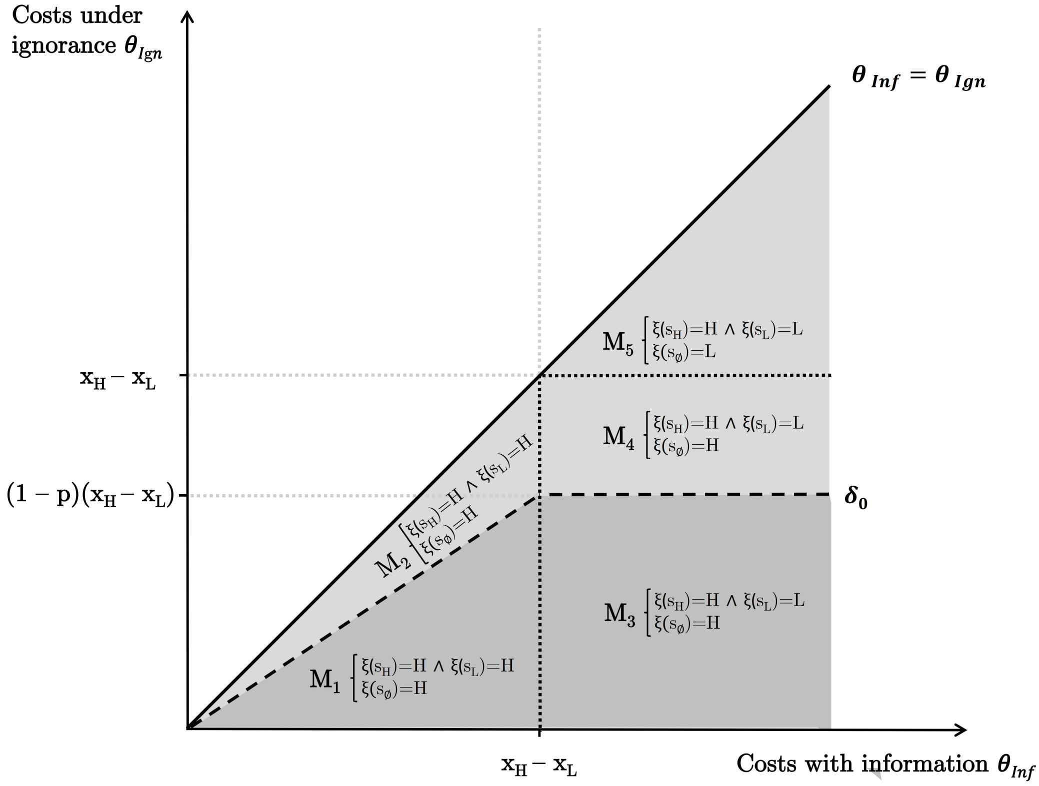


FIGURE 3.1: Distribution of Agents

Reporting under Ignorance Ignorance on the eligibility to payoffs does not allow for a conditional reporting strategy. Therefore, an agent's claim only depends on her costs of making a potentially unjustified claim θ_{Ign}, but is independent on whether her outcome qualifies for the high or the low payoff. For $\theta_{Ign} \geq x_H - x_L$, she claims $\xi(s_\varnothing) = L$, and for $\theta_{Ign} < x_H - x_L$, she claims $\xi(s_\varnothing) = H$. The dotted horizontal line in Figure 3.1 illustrates the threshold for making a potentially unjustified claim of x_H.

The ex-ante payoff from the participation in the lottery under ignorance is independent of the probability distribution of payoffs and is given by

$$\pi_{Ign} = \begin{cases} x_H - \theta_{Ign} & \text{if} \quad \theta_{Ign} < x_H - x_L \\ x_L & \text{if} \quad \theta_{Ign} \geq x_H - x_L \end{cases} . \tag{3.2}$$

Preferences for Information In accordance with their lying costs $(\theta_{Inf}, \theta_{Ign})$, agents may always dishonestly claim x_H (subset M_1 and M_2 in Figure 3.1), may only make a (potentially) dishonest claim under ignorance (subset M_3 and M_4), or may abstain from making a (potentially) dishonest claim altogether (subset M_5).[7] A comparison of ex-ante payoffs from the information conditions, (1) and (2), allows for a classification into information seekers and information avoiders. When having the choice, an agent is willing to pay exactly as much as the difference (1) – (2) for information. Defining an agents (maximum) WTP $z^* := \Delta((1) - (2))$, the difference reads

$$z^* = \begin{cases} \theta_{Ign} - (1-p)\theta_{Inf} & \text{if } \theta_{Ign} \leq \theta_{Inf} < x_H - x_L \\ \theta_{Ign} - (1-p)(x_H - x_L) & \text{if } \theta_{Ign} < x_H - x_L \leq \theta_{Inf} \\ p(x_H - x_L) & \text{if } \quad \theta_{Ign} \geq x_H - x_L \end{cases} . \tag{3.3}$$

In Figure 3.1, δ_0 displays agents with $z^* = 0$, i.e. combinations of $(\theta_{Inf}, \theta_{Ign})$ for which an agent is exactly indifferent between information and ignorance. Agents located below δ_0 are better off under ignorance (negative WTP, indicated by dark gray area), and agents above δ_0 are better off with information (positive WTP, indicated by light gray area).

Change in the Probability Distribution of Payoffs Now consider an increase in the probability of being eligible to the high payoff x_H from p to p', with $p' > p$. We distinguish two cases here.

First, suppose $(\theta_{Inf}, \theta_{Ign})$ does not depend on p. The increase in p neither changes the thresholds for claiming $\xi(s_L) = H$ and $\xi(s_\varnothing) = H$ nor leads to a change in the distribution of agents into the subsets $M_1 \cup M_2$, $M_3 \cup M_4$, and M_5. Reporting behavior with information is unaffected, while there are relatively less unjustified claims of x_H under ignorance.[8] Moreover, information on the eligibility to payoffs becomes more

[7] There are four different combinations of costs $(\theta_{Inf}, \theta_{Ign})$. First, consider agents with $\theta_{Inf} < x_H - x_L \wedge \theta_{Ign} < x_H - x_L$. These agents always claim $\xi(S) = H$, independent of their eligibility to x_H or the information condition. The difference (1) – (2), $\Delta = \theta_{Ign} - (1-p)\theta_{Inf}$, may be positive or negative. Agents in the subset M_1 are better off under ignorance as expected lying costs with information, $(1-p)\theta_{Inf}$, are larger than the costs θ_{Ign} under ignorance. In contrast, agents in subset M_2 prefer information. The chance of honestly claiming $\xi(s_H) = H$ with information outweighs the reduced lying costs under ignorance that occur with certainty. Second, consider agents with $\theta_{Inf} \geq x_H - x_L \wedge \theta_{Ign} < x_H - x_L$. These agents honestly claim $\xi(s_L) = L$ with information but claim $\xi(s_\varnothing) = H$ under ignorance. Hence, the smaller lying costs under ignorance ensure getting the high monetary payoff. The difference (1) – (2), $\Delta = \theta_{Ign} - (1-p)(x_H - x_L)$, may also be positive or negative. Agents in the subset M_3 prefer ignorance to information, while agents in M_4 are better off with information. The latter group rather enjoys an honestly claimed high payoff with probability p (and an honestly claimed low payoff with probability $(1-p)$) than a potentially unjustified high payoff under ignorance. Third, consider agents with $\theta_{Inf} \geq x_H - x_L \wedge \theta_{Ign} \geq x_H - x_L$. These agents honestly claim $\xi(s_L) = L$ with information but also claim $\xi(s_\varnothing) = L$ under ignorance. They rather forgo a justified high payoff than making a potentially dishonest claim $\xi(s_\varnothing) = H$ under ignorance. The difference (1) – (2), $\Delta = p(x_H - x_L)$, is strictly positive and agents are always better off with information. Finally, the case $\theta_{Inf} < x_H - x_L \wedge \theta_{Ign} \geq x_H - x_L$ is ruled out by assumption.

[8] The increase in p leads to two changes: first, as information seeking becomes more attractive, more agents are in M_2 and M_4 and fewer agents are in M_1 and M_3. Second, the overall mass of agents eligible to the low payoff x_L decreases. Therefore, relatively more agents forgo a justified high payoff x_H by claiming $\xi(s_\varnothing) = L$ under ignorance

attractive. The ex-ante expected payoffs with information increase both for honest and dishonest agents since they are eligible to x_H more often, but does not affect payoffs under ignorance (cf. equation (3.2)). Hence, δ_0 shifts downwards.

Second, suppose $(\theta_{Inf}, \theta_{Ign})$ depends negatively on p, i.e. an increase in p leads to smaller behavioral lying costs. In this case, it seems reasonable that $\theta_{Inf}(p) - \theta_{Inf}(p') < \theta_{Ign}(p) - \theta_{Ign}(p')$. The reduction in the costs of claiming x_H under ignorance, θ_{Ign}, is larger than the reduction of the costs with information, θ_{Inf}.[9] Consequently, the distribution of agents in Figure 3.1 changes. Less agents are in the subset M_5, while the composition of M_1 to M_4 depends on the relative change in θ_{Inf} to θ_{Ign}. The larger the relative reduction of θ_{Ign} to θ_{Inf}, the more agents are in M_1 and M_3 (and the less are in M_2 and M_4). Hence, while there are more unjustified claims of x_H both with information and under ignorance, the increase under ignorance is stronger. The shift in the distribution also counteracts the downward shift of δ_0, and more agents may become information avoiding.

(agents in M_5), which counterbalances agents claiming $\xi(s_\varnothing) = H$. Our empirical analysis accounts for this by adjusting for the expected number of agents eligible to x_L. An alternative measure that targets (unjustified) claims of x_H at the individual level would predict no change in reporting behavior under ignorance.

[9] With information, an agent perfectly knows when she is lying, which implies minor changes in intrinsic lying costs or self-image concerns. In contrast, it may become easier to convince herself that she is actually eligible to the high payoff x_H under ignorance, especially when $p' \geq 0.5$. The same may hold for the judgment of a social observer. Consequently, self- and social-image concerns are of less relevance under ignorance the larger p.

3.4 Experimental Setup

3.4.1 Design

1st Dimension \ 2nd Dimension	Information	Ignorance
Low Probability $p = 0.1$	Treatment 1 Subjects = 64	Treatment 2 Subjects = 78
High Probability $p = 0.5$	Treatment 3 Subjects = 85	Treatment 4 Subjects = 99

FIGURE 3.2: Treatment Allocation

Overview We use a one-shot, between-subjects design. The experiment ('Reporting Task') closely follows our decision-theoretic framework. Subjects randomly draw a lottery outcome and subsequently claim a high payoff or a low payoff based on their lottery outcome. The first treatment dimension of our 2-by-2 design varies the ex-ante probability of being eligible to the high payoff. 'Low Probability' indicates treatments with probability $p = 0.1$ and 'High Probability' indicates treatments with probability $p = 0.5$ of being eligible to the high payoff. The second treatment dimension varies the information on the eligibility to the payoffs. All subjects have common knowledge about the probability distribution. However, only half of the subjects know whether their lottery outcome is eligible to the high or the low payoff ('Information'), whereas the other half of subjects does not ('Ignorance'). The first dimension is implemented between sessions and the second dimension within sessions. Figure 3.2 summarizes the exogenous treatment allocation.

Procedure of the Reporting Task Figure 3.3 illustrates the procedure of the Reporting Task. It commences with a short explanation of the computerized lottery and the subsequent claiming stage. Subjects receive the information that one (five) of the lottery outcomes is (are) eligible to a payoff of EUR 8, and nine (five) of the lottery outcomes are eligible to a payoff of EUR 2. Subjects then randomly draw their

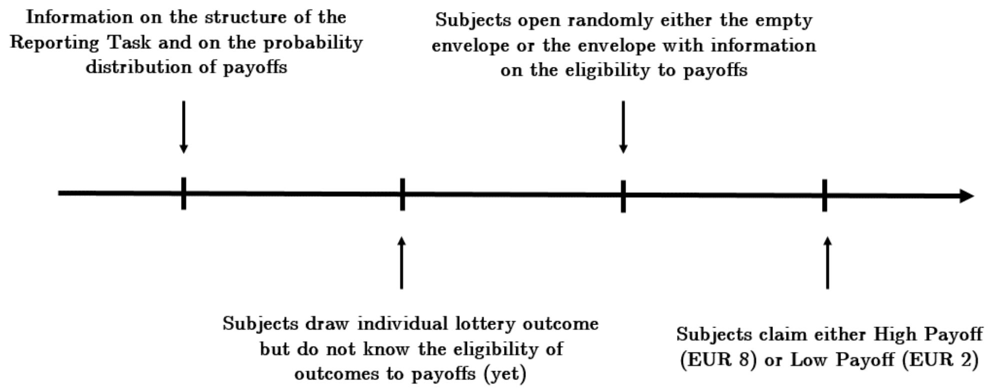


FIGURE 3.3: Procedure of the Reporting Task

individual lottery outcome.[10] However, they have no information on the correspondence of outcomes to payoffs yet. On the next screen, subjects find two closed envelopes and open exactly one of them. One envelope contains a list explaining which outcome is eligible to which payoff, while the other envelope is empty.[11] Finally, subjects have to claim either the high payoff of EUR 8 or the low payoff of EUR 2. They are specifically asked to claim the payoff they are eligible to based on their lottery outcome only. This concludes the Reporting Task, and subjects receive their claimed payoff as earnings. Unknown to subjects, they have the possibility to open the second envelope ex-post after completing the norm elicitation.

Post-Tests Next, we conducted a number of post-tests related to the Reporting Task.[12] First, subjects participate in a variant of the Ellsberg paradox (Ellsberg 1961) to measure ambiguity aversion. Second, we elicit subjects' willingness to pay (WTP) for two different binary lotteries. These lotteries resemble the Reporting Task in terms of the probability distribution and spreads of payoffs, but do not allow for a manipulation of payoffs. Third, we elicit the certainty equivalent of a binary lottery with payoffs EUR 0 (probability 0.5) and EUR 1 (probability 0.5). Fourth, we use a claiming task in style of Tjøtta (2019). Here, subjects are not formally bound by an eligibility restriction and may claim any amount between EUR 0 and EUR 0.5. Fifth, subjects choose whether

[10] We use a computerized Wheel of Fortune. Outcomes are represented by letters in random order.

[11] Prior to their choice of the envelope, subjects read that one envelope is empty and the other one contains the correspondence of outcomes to payoffs. Both envelopes appear identical when closed. For half of the subjects the right envelope was empty and for the other half the left envelope. Appendix 3.D provides screen-shots of the Reporting Task.

[12] We describe the sections/tasks and post-tests in chronological order. Section/tasks were referred to as 'parts' and post-tests as 'scenarios' in the experiment.

they receive information on the outcome of a compulsory lottery with binary payoffs of EUR -0.50 (probability 0.5) and EUR 0.50 (probability 0.5).

Beliefs on the Reporting Behavior and Social Norms The next section elicits subjects' beliefs on the fraction of high and low claims in both information conditions in the respective session. Moreover, we ask subjects on their beliefs regarding social norms (inspired by Krupka and Weber 2013). On a scale of 1 ('completely honest') to 6 ('completely dishonest'), subjects estimate the modal answer for two hypothetical reporting situations. In the first situation, a participant claims the high payoff under ignorance, and in the second situation, a participant claims the high payoff with the knowledge that she is only eligible to the low payoff. Subjects receive payment based on the precision of their estimation. Additionally, subjects state their personal perception on the social norm in the hypothetical cases (flat payment).

Implementation and Payoffs We conducted our laboratory experiment in January 2020 at the econlab Munich. In total, 16 sessions with 287 subjects took place (predominantly local university students; average age 23 years; 55 percent female subjects).[13] Subjects were recruited using ORSEE (Greiner 2015) and the experiment was programmed and implemented with z-Tree (Fischbacher 2007). The average sessions duration was 75 minutes, and the average payoff was EUR 17 (min. EUR 8.20, max. EUR 31.3) including a show-up fee of EUR 6 and average earnings of EUR 4.37 from the post-tests. After completing the experiment, subjects took part in a short socio-economic questionnaire, the section on Machiavellian traits of the Dark Triad (Jones and Paulhus 2014) and a short questionnaire regarding the experimental setup.

3.4.2 Hypotheses

Our hypotheses focuses on the Reporting Task with an exogenous allocation to the information conditions.[14] In Section 3.2, we argue that the costs of claiming $\xi(s_L) = H$ with information, θ_{Inf}, are larger than the costs of claiming $\xi(s_\varnothing) = H$ under ignorance, θ_{Ign}. The implications for reporting behavior for a given probability of being eligible to x_H follow directly.[15]

[13] We ran four pilot sessions ($N = 81$) in December 2019 and January 2020. As the reporting task was equivalent to the main experiment in two of the pilot sessions, we include respective sessions in our main analysis ($N = 39$). Results are robust to this selection choice.

[14] For a discussion on the self-selection into the preferred information condition, please refer to Appendix 3.A.

[15] Only agents in subset M_1 and M_2 in Figure 3.1 make an unjustified claim $\xi(s_L) = H$ with information, while agents in the subsets M_1 to M_4 make a (potentially) unjustified claim $\xi(s_\varnothing) = H$ under ignorance. Agents in M_5 who report $\xi(s_\varnothing) = L$ counteract the latter group. Denote the union of the five disjoint subsets M_1 to M_5 by M.

Hypothesis 3.1 *The fraction of unjustified claims under ignorance is larger than the fraction of unjustified claims with information.*

For the effect of an increase in p on reporting behavior, we state two competing hypotheses. When lying costs $(\theta_{Inf}, \theta_{Ign})$ are independent of p, the distribution of agents in M_1 to M_5 in Figure 3.1 does not change. The same holds for the threshold levels for making a (potentially) unjustified claim. With information, the change in p increases the number of agents who honestly claim $\xi(s_H) = H$, but leaves the reporting decision of agents with s_L unaffected. Hence, the fraction of unjustified claims does not change. Under ignorance, the same holds for agents' decisions on claiming $\xi(s_\varnothing) = H$ and $\xi(s_\varnothing) = L$. However, due to the increase in p, agents with large lying costs $\theta_{Ign} \geq x_H - x_L$ claim $\xi(s_\varnothing) = L$ despite being eligible to x_H more often, which decreases the aggregate fraction of unjustified claims of x_H.

Hypothesis 3.2A *A higher probability of being eligible to x_H does not change the fraction of unjustified claims with information but reduces the fraction under ignorance.*

An increase in p may also reduce agents' lying costs $(\theta_{Inf}, \theta_{Ign})$. In particular, it may lead to a reduction of θ_{Ign} relative to θ_{Inf}. The larger p, the better agents may convince themselves that they are eligible to x_H under ignorance. This may reduce intrinsic lying costs and dampen self- and social-image concerns, which is not possible for claiming with information.[16] Hence, the increase in the fraction of unjustified claims under ignorance outweighs the increase with information in this case.

Hypothesis 3.2B *A higher probability of being eligible to x_H increases the fraction of unjustified claims with information and under ignorance. The increase under ignorance is larger than the increase with information.*

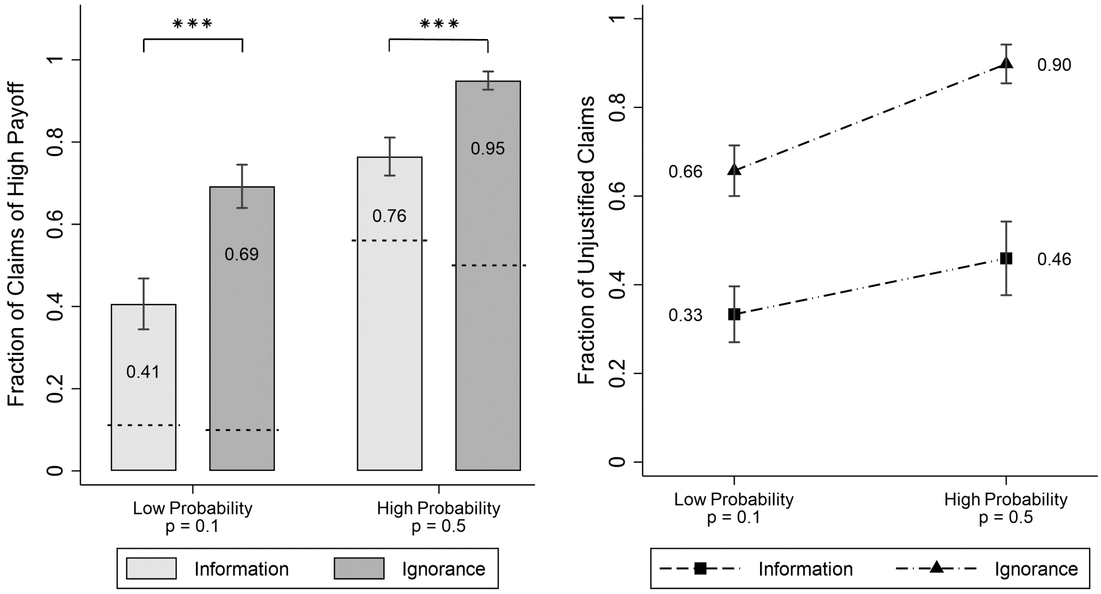
The resulting fractions of unjustified claims of x_H are given by $\eta_{Inf} = \frac{|M_1 \cup M_2|}{|M|}$ with information, and by $\eta_{Ign} = \frac{|M_1 \cup M_2 \cup M_3 \cup M_4|}{|M|} - \frac{p}{1-p}\frac{|M_5|}{|M|}$ under ignorance. Due to a small expected mass of agents in M_5 and our parameterization ($p = 0.1$ and $p' = 0.5$), the second term of η_{Ign} should be small. Hence, the fraction of unjustified claims under ignorance is larger.

[16] This crowds-out the subset M_5, and may change the distribution of agents in M_1 to M_4 towards M_1 and M_3 in Figure 3.1.

3.5 Results

For the most part, the following analysis focuses on subjects that have an incentive to make a (potentially) unjustified claim of x_H, which leads to an exclusion of subjects who are eligible to x_H with information. This leaves us with 94 subjects with information (57 subjects in Low Probability and 37 subjects in High Probability), and 177 subjects under ignorance (78 subjects in Low Probability and 99 subjects in High Probability).

3.5.1 Unjustified Claims with Information and under Ignorance



Note: Reporting behavior by treatment allocation. The left side shows the fraction of claims of the high payoff to total claims. The dashed lines refer to the ex-ante probability of being eligible to the high payoff. Brackets indicate significant treatment differences, $^{***}p<0.01$. The right side displays the fraction of (expected) unjustified claims of the high payoff among those (expected to be) eligible to the low payoff.

FIGURE 3.4: Reporting Behavior by Treatment Allocation

Fraction of claims of $\mathbf{x_H}$

Figure 3.4 (left side) shows the fraction of subjects claiming x_H. Here, we abstract from conditioning on the eligibility to payoffs. In Low Probability, 41 percent claim x_H with information ($N = 64$) and 69 percent under ignorance ($N = 78$). In High Probability, the respective fractions are 76 percent with information ($N = 85$) and 95 percent under ignorance ($N = 99$). In all treatments, subjects claim x_H significantly more often than the ex-ante probability $p = 0.1$ and $p = 0.5$ with honest reporting (Binomial-Test: $p < 0.01$, respectively).[17] More importantly, significantly more subjects claim x_H under ignorance as compared to information (χ^2-Test: $p < 0.01$, respectively). This finding holds for both probability dimensions and indicates more unjustified claims under ignorance.

Fraction of unjustified claims of $\mathbf{x_H}$

As a next step, we analyze the fraction of unjustified claims of x_H. By conditioning on the eligibility to payoffs, we can directly compare reporting behavior between treatments.[18] Figure 3.4 (right side) displays the fraction of unjustified claims for Low Probability on the left and for High Probability on the right. In both treatments, ignorance almost doubles the fraction of unjustified claims as compared to information. The increase from 33 percent to 66 percent and from 46 percent to 90 percent is highly significant (χ^2-Test: $p < 0.01$, respectively). In line with Hypothesis 3.1, subjects seem to have smaller costs of making a (potentially) unjustified claim under ignorance than with information, leading to more unjustified claims under ignorance.

Result 3.1 *Ignorance leads to significantly more unjustified claims of x_H as compared to information.*

Our findings on an increase in the probability of being eligible to x_H are more nuanced. There is only limited evidence for an effect on reporting behavior with information. In High Probability, informed subjects are 13 percentage points more likely to make an unjustified claim as compared to Low Probability. The increase is moderate in size and statistically not significant (χ^2-Test: $p = 0.22$). In contrast, the increase in unjustified claims in High Probability amounts to 24 percentage points under ignorance (χ^2-Test: $p < 0.01$). This suggests that the costs of making a (potentially) unjustified claim of x_H under ignorance decrease in the probability of being eligible to x_H.

Result 3.2 *Under ignorance, an increase in the probability of being eligible to x_H leads to more unjustified claims of x_H.*

[17] Due to the randomness of the lottery and the random allocation to information conditions, 56 percent were eligible to x_H in High Probability with information.

[18] The fraction of unjustified claims of x_H is given by $\hat{\eta}_{Inf} = \frac{\#(\xi(s_L)=H)}{\#(s_L)}$ for information and by $\hat{\eta}_{Ign} = \frac{\#(\xi(s_\varnothing)=H)-\#(E[\text{Eligible to } x_H])}{\#(E[\text{Eligible to } x_L])}$ for ignorance. Less formally, we compute the difference of the total number of claims of the high payoff subtracted by the (expected) number of justified claims of the high payoff and divide by the (expected) number of subjects eligible to the low payoff.

The distribution of lying costs ($\boldsymbol{\theta}_{Inf}, \boldsymbol{\theta}_{Ign}$)

The fraction of unjustified claims also allows for insights on the distribution of subjects' lying costs ($\theta_{Inf}, \theta_{Ign}$) as illustrated in Figure 3.1. In Low Probability, one third of subjects is willing to dishonestly claim x_H both with information and under ignorance ($M_1 \cup M_2$). Another third of subjects is willing to claim $\xi(s_\varnothing) = H$ under ignorance, but honestly claims $\xi(s_L) = L$ with information ($M_3 \cup M_4$). The last third is not willing to claim the high payoff under ignorance, $\xi(s_\varnothing) = L$, while these subjects also honestly claim $\xi(s_L) = L$ with information (M_5).[19] The increase of the ex-ante probability p shifts the distribution of subjects in equal parts to $M_1 \cup M_2$ and $M_3 \cup M_4$ (both around 45 percent of subjects) at the expense of M_5 (reduction to 10 percent of subjects) in High Probability. This indicates support for Hypothesis 3.2B.

Multivariate analysis of reporting behavior

Table 3.1 shows the results of a multivariate analysis of reporting behavior. We employ a linear probability model with (potentially) unjustified claiming of the high payoff as the dependent variable (binary variable).[20] In the two baseline specifications (1) and (2), moving from information to ignorance increases the fraction of (potentially) unjustified claims by 36 to 42 percentage points. This effect is highly significant and in line with Hypothesis 3.1. Hence, we confirm our finding on the smaller costs of making an unjustified claim under ignorance as compared to information. The coefficient on 'High Probability' in specification (1) also suggests a higher propensity of unjustified claims of x_H due to the increased probability of being eligible to x_H. However, the introduction of the interaction term in specification (2) points to a different conclusion. For ignorant subjects, the total effect of High Probability amounts to a significant increase of (potentially) unjustified claims by 26 percentage points. Hence, the increase in p leads to a reduction in the lying costs θ_{Ign}. For informed subjects, the effect is smaller and fails to reach statistical significance. The same holds for the interaction term 'Ignorance x High Prob.', which is limited evidence that the increase in the fraction of (potentially) unjustified claims of x_H is larger under ignorance.[21] Therefore, our results are only partially in line with hypothesis 3.2B.

[19] The empirical distribution is in line with our assumption that none of the subsets M_1 to M_5 are empty and that M_5 is not disproportionally large.

[20] In all specifications, the treatment Low Probability with information is the reference group. We use a linear probability model in order to obtain an unbiased estimate of the interaction term between the treatment dimensions 'Ignorance x High Prob.' (compare to Ai and Norton, 2003). A probit estimation of the reporting behavior is provided in Appendix 3.C. Our main results are robust except for the significance of the interaction term.

[21] As for the coefficient 'High Probability' in the treatments with information, this might be due to the limited sample size.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ignorance	0.418***	0.359***	0.365***	0.357***	0.371***	0.352***	0.366***
	(0.059)	(0.082)	(0.082)	(0.077)	(0.095)	(0.0960)	(0.098)
High Probability	0.213***	0.126	0.122	0.121	0.096	0.104	0.091
	(0.052)	(0.104)	(0.097)	(0.092)	(0.104)	(0.103)	(0.108)
Ignorance x High Prob.		0.131	0.154	0.143	0.152	0.161	0.151
		(0.118)	(0.114)	(0.108)	(0.122)	(0.122)	(0.126)
Awareness				0.242***			
				(0.053)			
Experience					0.180***		
					(0.049)		
Ambiguity Aversion						-0.050*	
						(0.030)	
Taking Task						0.019***	
						(0.007)	
Information Avoiders						-0.060	
						(0.074)	
Curiosity 2^{nd} Envelope						0.139*	
						(0.077)	
Machiavellianism							0.007
							(0.005)
Socio-Eco. Controls	NO	NO	YES	YES	YES	YES	YES
Constant	0.299***	0.333***	0.193	-0.167	-0.095	-1.052***	-0.305
	(0.053)	(0.063)	(0.225)	(0.231)	(0.204)	(0.336)	(0.237)
Observations	271	271	271	271	234	234	233
R^2	0.264	0.268	0.304	0.360	0.355	0.371	0.322

Notes: The table presents results of a linear probability regression with potentially unjustified claims as the dependent variable (dummy variable). The reference group is the treatment with information and a low ex ante probability of being eligible to the high payoff. "Ignorance" is a dummy variable that indicates treatments with ignorance on being eligible to the high or the low payoff, "High Probability" is a dummy variable that indicates treatments with high ex ante probability (prob. = 0.5) on being eligible to the high payoff, and "Ignorance x High Prob." is the interaction between both dummies. "Awareness" is self-reported awareness that only the claim determines the payoff (dummy variable), and "Experience" is self-reported previous participation in a related experiment as the reporting task (dummy variable). "Ambiguity Aversion" refers to ambiguity seeking, payoff-maximizing and ambiguity averse subjects (-1 to 1), "Taking Task" refers to the amount taken in a task in style of Tjøtta (2019) (Eurocents 0 to 50), "Information Avoiders" refers to subjects not willing to learn their lottery outcome in the post-test section (dummy variable), and "Curiosity 2nd Envelope" refers to subjects opening the second envelope ex post (dummy variable). "Socio-Eco. Controls" include a dummy for female subjects, the age of the subject, a dummy for subjects with German mother tongue, a dummy for Economics and Business students and the number of siblings. Robust standard errors are given in parentheses, *** $p < 0.01$, * $p < 0.1$.

TABLE 3.1: Multivariate Analysis of Misreporting Behavior

The alternative specifications (3) to (7) confirm the robustness of our main results in specification (2). Specification (3) introduces a number of socio-economic control variables. We do not find a gender effect, but Economics and Business students are significantly more likely to make an unjustified claim.[22] Self-reported awareness of the opportunity to misreport in specification (4) is correlated with a significantly larger propensity of making a (potentially) unjustified claim (compare to Lohse et al. 2018). A similar finding holds for self-reported previous experience in experiments akin to the Reporting Task (specification (5)). Specification (6) includes a number of post-tests. Ambiguity averse subjects are less likely to make a (potentially) unjustified claim, as well as subjects that are willing to take less in Tjøtta's (2019) taking task. Information avoidance on a lottery outcome is not related to reporting behavior, while subjects that open the second envelope ex-post are more likely to make a (potentially) unjustified claim. Finally, a higher score of Machiavellian traits (as measured in the Dark Triad by Jones and Paulhus 2014) is not correlated to reporting behavior.

3.5.2 Beliefs on the Reporting Behavior

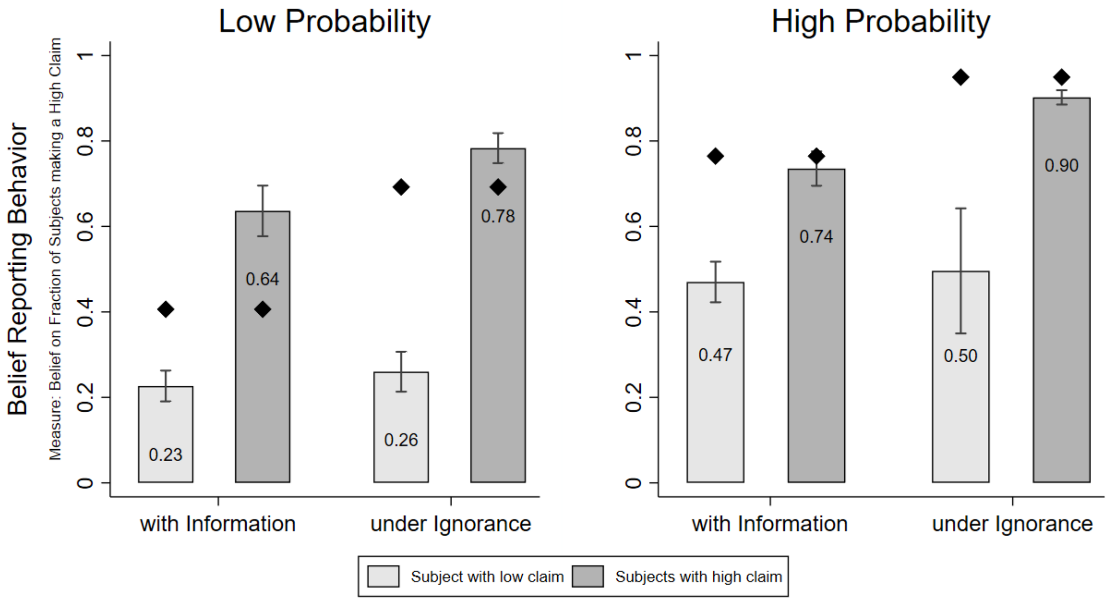
In the complete sample ($N = 326$), subjects' beliefs on the fraction claiming x_H and x_L with information and under ignorance match, on average, the actual reporting behavior.[23] Subjects correctly predict the treatment difference between informed and ignorant subjects in both treatments (Wilcoxon signed ranks test: $p < 0.01$, respectively).

However, the accuracy of average beliefs hides a strong bias towards the own claiming choice. Figure 3.5 (left side: Low Probability ($N = 135$); right side: High Probability ($N = 136$)) displays a between-subjects comparison of subjects' belief conditional on the own treatment allocation and claiming choice.[24] As a reading example, informed subjects claiming $\xi(s_L) = L$ in Low Probability believe that 23 percent of informed subjects claim x_H in this treatment. The black diamonds indicate the actual reporting behavior. Subjects claiming x_L strongly underestimate the fraction of subjects claiming x_H in all treatments, while the overestimation by subjects claiming x_H is less pronounced. In Low Probability (Figure 5 left side), subjects claiming x_L have a downward

[22] For a discussion on the correlation of different socio-economic factors and dishonest behavior, see the survey by Gerlach et al. (2019).

[23] In Low Probability, the estimated fraction reporting x_H with information is accurate and under ignorance underestimated by 7 percentage points. In High Probability, the fraction of claims of x_H is underestimated by around 10 percentage points.

[24] We elicited beliefs on both information conditions for each subject. Here, we restrict to subjects' belief for the own information condition.



Note: Beliefs of reporting behavior conditional on subjects' treatment allocation and claiming choice. The left figure shows Low Probability, the right figure shows High Probability. The left side in each figure displays beliefs of informed subjects, and the right side of ignorant subjects.

FIGURE 3.5: Beliefs on Reporting Behavior by Treatment Allocation

bias of 18 percent with information and of 43 percent under ignorance. The downward bias for subjects claiming x_L in High Probability is comparable and amounts to 29 percent with information and 45 percent with ignorance. We cannot reject that their belief equals the ex-ante probability of being eligible to x_H, $p' = 0.5$ (One-Sample-Median test: $p = 0.83$ and $p = 0.69$), which implies no unjustified claims of x_H. In contrast, the upward bias for subjects claiming x_H is smaller in Low Probability, and beliefs are accurate in High Probability.

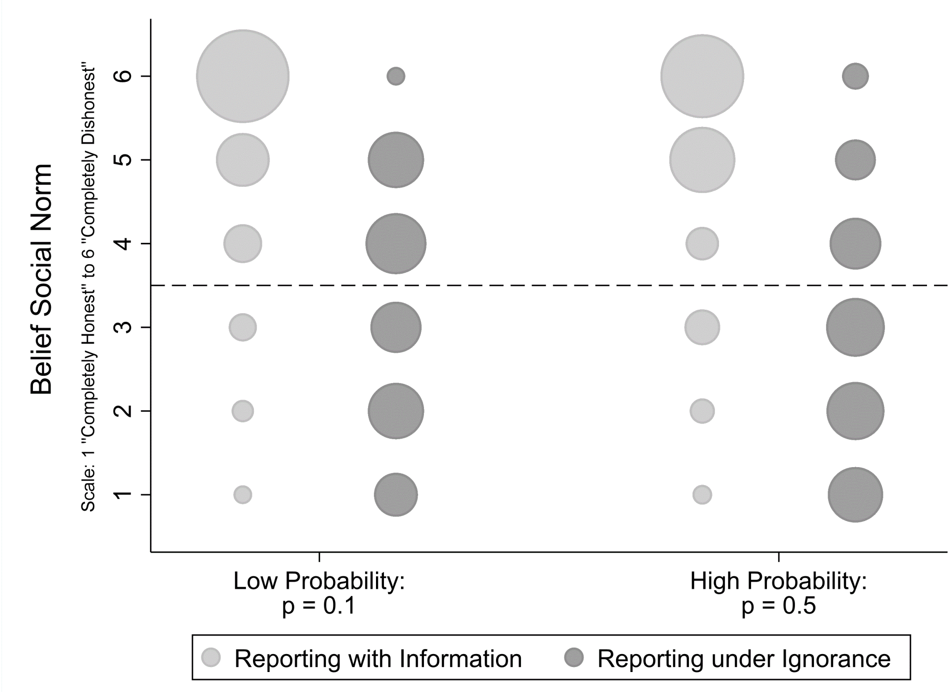
In both Low Probability and High Probability, we do not find a treatment difference in beliefs between informed and ignorant subjects claiming x_L (Wilcoxon-Mann Whitney test: $p = 0.75$ and $p = 0.95$).[25] Hence, subjects claiming $\xi(s_L) = L$ or $\xi(s_\varnothing) = L$ may consider the reporting situation with information equivalent to the reporting situation under ignorance. In contrast, we find a strong treatment difference in beliefs for subjects claiming x_H (Wilcoxon-Mann Whitney test: $p < 0.01$, respectively). As subjects claiming $\xi(s_L) = H$ or $\xi(s_\varnothing) = H$ expect other subjects to claim the high

[25] Since most ignorant subjects claim $\xi(s_\varnothing) = H$ in High Probability, only 5 out of 99 subjects claim $\xi(s_\varnothing) = L$ in this treatment. Hence, results for this subgroup may not be robust and should be interpreted cautiously.

payoff under ignorance more often, they may fear less negative consequences for their self- and social image.

Result 3.3 *Beliefs on the reporting behavior are accurate, on average, but biased towards subjects' own claiming choice.*

3.5.3 Social Norms regarding Reporting Behavior



Note: Figure shows the weighted distribution of beliefs on the social norm (as grades from '1' to '6').

FIGURE 3.6: Social Norms regarding Reporting Behavior

Adherence to social norms may be an important motivation for subjects' reporting decision. While the true eligibility is a fixed point for reporting behavior with information, social norms for reporting under ignorance lack such a benchmark. To shed light on this matter, we ask subjects on their belief of the modal answer for two hypothetical reporting situations ($N = 287$). In the first situation, subjects state their belief on the social norm for claiming $\xi(s_{\varnothing}) = H$ under ignorance on a scale form 1 ('completely honest') to 6 ('completely dishonest'). In the other scenario, they evaluate claiming $\xi(s_L) = H$ with information on the same scale. We discuss the beliefs on social norms here, which are broadly in line with the individual perception of subjects.

Figure 3.6 displays the distribution of beliefs on the social norm by treatment weighted by the probability mass of each grade. Grades above the dashed line are in the dishonest sphere, while grades below are in the honest sphere. Claiming $\xi(s_L) = H$ with information is considered dishonest (average grade for Low Probability and for High Probability is 5.25 and 5.05 out of 6). The mass of the two grades '6' ('completely dishonest') and '5' comprise around 80 percent of subjects in both treatments, leaving considerable less mass on all other possible grades. For the hypothetical case with information, we find no major treatment differences between Low Probability and High Probability (Wilcoxon-Mann Whitney test: $p = 0.09$), except for a slight shift from grade '6' to grade '5' in High Probability.

In comparison to information, the average belief on the social norm for making a (potentially) unjustified claim under ignorance is significantly shifted towards honesty (Wilcoxon signed ranks test: $p < 0.01$, respectively). Claiming $\xi(s_\varnothing) = H$ is considered somewhere between honesty and dishonesty, and average grades are 3.27 (Low Probability) and 2.89 (High Probability). Importantly, there is much less consensus on the social norm. Subjects' beliefs are distributed equally along the grades '1' ('completely honest') to '5', and only grade '6' ('completely dishonest') has less probability mass than the other grades. For Low Probability, 49 percent of subjects belief that claiming $\xi(s_\varnothing) = H$ is considered more dishonest than honest, while 33 percent do so for High Probability. Hence, the increase in the probability of being eligible to x_H significantly shifts the social norm under ignorance (Wilcoxon-Mann Whitney test: $p = 0.02$).[26] Due to the more dispersed social norm under ignorance, some of the ignorant subjects may not feel bound to a strict social norm. This might be one explanation for the increased fraction of unjustified claims under ignorance.

Result 3.4 *In contrast to information, social norms for reporting under ignorance are controversial. The claim of a high payoff under ignorance is considered in between honesty and dishonesty.*

[26] In contrast to the beliefs on the reporting behavior, beliefs on social norms are less biased towards the own claiming choice.

3.6 Discussion

While the lack of knowledge increases the propensity of making unjustified claims, a substantial minority prefers to forgo a justified payoff to the risk of being dishonest. Some people seem to consider ignorance not as a valid excuse for self-serving dishonest behavior, and face similar costs as for deliberate lying. The bad news is that this behavior is almost crowded-out completely when the likelihood of being eligible to the high payoff appears sufficiently high. This indicates that the reporting decision under ignorance may crucially depend on the ability to convince oneself of the eligibility to the high payoff. The higher the ex-ante probability of being eligible to x_H, the easier ignorant subjects can do so. The channel potentially works via intrinsic lying costs and self-image concerns, and is in line with our finding that the probability dimension has only a significant impact under ignorance. In addition, adherence to social norms and social image concerns may explain reporting behavior as well. As there is no consensus regarding the social norm under ignorance, subjects may feel less obliged to make a justified claim and may be less afraid of negative consequences to their image.

Implications arise to the design of forms and to the accessibility of information. While some subjects may take their ignorance as a strategic device to choose to self-serving option, other subjects may make an unjustified claim accidently due to their lack of knowledge. Providing options such as 'I don't know' in official forms may not be satisfactory from the point of the inquirer at first glance, but can reduce the propensity of the self-serving (dishonest) reporting behavior. Especially when consequences of a dishonest report have severe consequences, the indication of a lack of knowledge may be beneficial for both sides. This is also in line with the finding that most subjects are information seeking when information is free of costs (cf. Appendix 3.A). Consequently, information on rules and regulations should be as accessible as possible. A prominent example might be forms for the tax declaration, which people oftentimes perceive as complicated. However, our findings on self-serving behavior under ignorance should not be overstressed, as ignorance does not prevent legal prosecution and punishment (Ignorantia legis non excusat). This may counterbalance or even reverse our findings, and subjects may try to avoid a conflict with the law by abstaining from making a potentially unjustified claim altogether.

3.7 Conclusion

People refrain from making a self-serving lie more often than not. However, missing information or a lack of knowledge on laws and regulations might leave people insecure on whether they tell the truth. The answer 'I don't know' may not be satisfactory to conversation partners, and forms often only allow for binary yes-or-no decisions. Here, honest people face a trade-off between forgoing a potentially justified claim and the risk of being a liar. We investigate into this trade-off and ask what people report when they do not know whether their claim is justified. In our laboratory experiment, subjects have to claim either a low payoff or a high payoff in an income-reporting task. Importantly, they are only eligible to one of the payoffs. Along the first dimension, we vary whether subjects know of their eligibility to payoffs. Along the second dimension, we vary the ex-ante probability of being eligible to the high payoff, which is common knowledge. Independent of their treatment allocation, subjects have to claim one of the payoffs, which in turn determines their earnings. We elicit subjects' beliefs on the reporting behavior and on social norms, and corroborate our findings by a number of post-tests.

We find that ignorance of the eligibility to payoffs has a large effect on reporting behavior. The fraction of unjustified claims of the high payoff almost doubles as compared to informed subjects. In contrast, the probability dimension in itself has a minor impact. However, subjects are particularly prone to claim the high payoff under ignorance when the probability of being eligible is high. These findings suggest that the behavioral costs of making an unjustified claim under ignorance are smaller than lying costs with information. Still, some of the ignorant subjects feel obliged to only claim the low payoff. Further results indicate that beliefs on the reporting behavior are accurate, on average, but biased towards the own claiming choice. In particular, subjects that refrain from making a (potentially) unjustified claim underestimate the actual claiming rate of high payoffs. Social norms on the reporting behavior under ignorance are controversial, and subjects' beliefs are distributed over the entire scale from honesty to dishonesty. In contrast, there is consensus that unjustified claiming the high payoff with information is considered dishonest. Hence, subjects are more comfortable to make an unjustified claim under ignorance as negative consequences for their self- and social-image may be less pronounced. After all, taking a controversial and self-serving action with a lack of knowledge may be judged less harshly.

3.A Appendix – The Self-Selection Task

Appendix 3.A. presents results on the 'Self-Selection Task', which allowed subjects to choose their preferred information condition. Results should be taken with a grain of salt as we conducted the Self-Selection Task at the very end of our experiment. Hence, the preceding belief and norm elicitation may affect the WTP for information. As the Self-Selection Task is structurally similar to the Reporting Task, previous reporting behavior may also contaminate behavior in the Self-Selection Task.[27]

Design

The Self-Selection task is structurally similar to the Reporting Task but allows for self-selection into ignorance or information. Along the probability dimension, half of subjects are allocated to Low Probability ($p = 0.1$) and half of subjects to High Probability ($p' = 0.5$), irrespective of the treatment allocation in the Reporting Task.[28] After participation in the lottery, subjects state their willingness to pay (WTP) for opening both envelopes over leaving both envelopes closed. If their WTP was larger or equal to a pre-determined price (EUR 0.20), subjects open both envelopes but have to pay the pre-determined price. In contrast, if their WTP was smaller than the pre-determined price, subjects have to leave both envelopes closed and do not pay anything. We elicited the WTP for information in the range of EUR -2 to EUR 2 using the strategy method (Selten 1967). As in the Reporting Task, subjects had to claim their payoff based on their lottery outcome in the final stage of the Self-Selection Task. The Self-Selection Task was paid out only with 20 percent probability and subjects were not able to anticipate the task.

Hypotheses

This second set of hypotheses focuses on the self-selection into the preferred information condition (Self-Selection Task). Agents with costs $\theta_{Ign} \geq x_H - x_L$ claim $\xi(s_\varnothing) = L$ under ignorance and $\xi(s_L) = L$ with information (subset M_5 in Figure 3.1). They pay for the chance of an honest high payoff x_H, and have a WTP $z = p(x_H - x_L)$ for information (cf. equation (3.3)). Agents with $\theta_{Ign} < x_H - x_L$ claim $\xi(s_\varnothing) = H$ under

[27] For other settings with repeated misreporting opportunities, see for example Fischbacher and Föllmi-Heusi (2013), Abeler et al. (2014) or Effron et al. (2015).

[28] We inform subjects that their lottery outcome and the correspondence of outcomes to payoffs in the Self-Selection Task is independent to the lottery outcome and the correspondence in the Reporting Task.

ignorance. However, they may claim $\xi(s_L) = L$ or $\xi(s_L) = H$ with information as $\theta_{Inf} \geq \theta_{Ign}$ (subsets M_1 to M_4). These agents have a WTP $z < p(x_H - x_L)$ for information (cf. equation (3.3)). For a price $\bar{z} = 0$ (as depicted in Figure 3.1), all subjects that self-select into ignorance (subsets M_1 and M_3) claim $\xi(s_\varnothing) = H$, while subjects that self-select into information may claim $\xi(s_L) = L$ or $\xi(s_L) = H$. The larger the pre-determined price $\bar{z}$, the more subjects select into ignorance (and claim $\xi(s_\varnothing) = H$), and the less subjects remain in subsets M_2 and M_4 relative to M_5. At the boundary price $\bar{z} = p(x_H - x_L)$, all agents with information honestly claim $\xi(s_L) = L$. Consequently, the fraction of unjustified claims under ignorance is larger as with information. More importantly, subjects that self-selected into ignorance are more prone to make an unjustified claim as compared to ignorant subjects under exogenous allocation.[29]

Hypothesis 3.A1 *When agents self-select into their preferred information condition, ignorant subjects make more unjustified claims of* x_H

(i) than subjects that self-select into information;

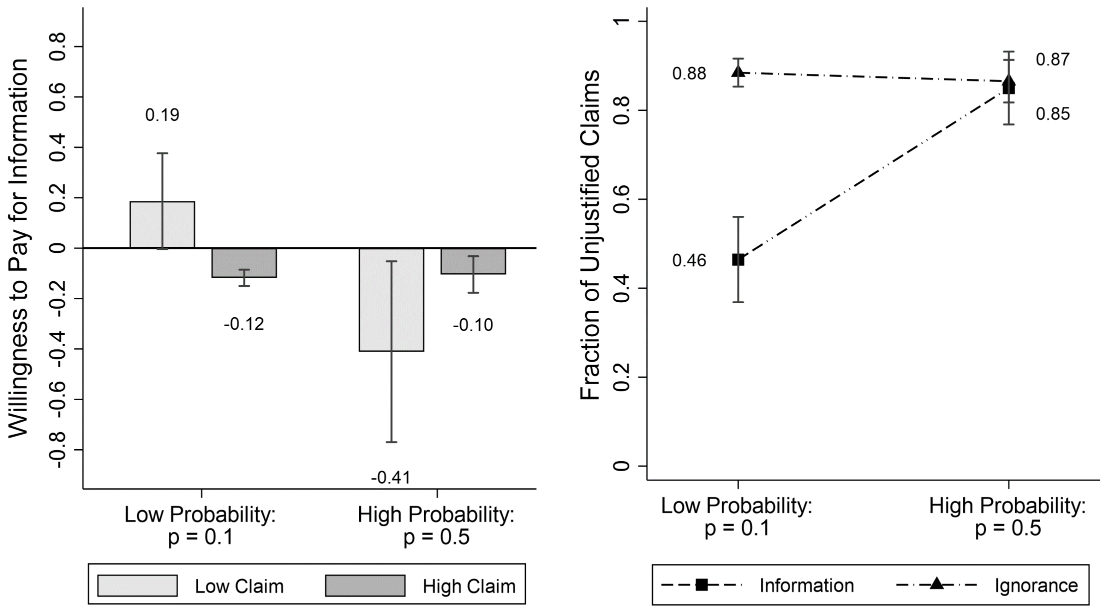
(ii) than ignorant subjects under exogenous allocation.

Finally, the effect of an increased probability of being eligible to x_H on the WTP for information depends on the effect of the increase in p on the distribution of lying costs $(\theta_{Inf}, \theta_{Ign})$. If $(\theta_{Inf}, \theta_{Ign})$ is independent of p, information becomes more attractive relative to ignorance as of the larger probability of being eligible to x_H. Hence, more agents are willing to pay for information. In contrast, when $(\theta_{Inf}, \theta_{Ign})$ depends on p, the direction of the change in the WTP is ambiguous and depends on the relative change of θ_{Inf} to θ_{Ign}. As argued, it seems reasonable that $\theta_{Inf}(p) - \theta_{Inf}(p') < \theta_{Ign}(p) - \theta_{Ign}(p')$, which may imply less information seeking behavior. However, we abstain from a competing hypothesis due to the ambiguity here.

Hypothesis 3.A2 *The increase in the probability of being eligible to* x_H *increases information seeking behavior.*

[29] The effect of self-selection for informed subjects remains ambiguous and depends on the distribution of agents in the subsets M_1 to M_4.

Results for the Self-Selection Task



Note: The left side displays the WTP for information conditional on reporting behavior. The right side displays the fraction of (expected) unjustified claims of the high payoff among those (expected to be) eligible to the low payoff.

FIGURE 3.A1: Behavior in the Self-Selection Task.

First, we analyze the WTP for information based on reporting behavior (Figure 3.A1 left side). We restrict the analysis to subjects with a consistent statement of the WTP that have an incentive to make an unjustified claim ($N = 254$).[30] In Low Probability, subjects with a high claim x_H are information avoiders, on average, and subjects with a low claim x_L are information seekers. We find a small but significant difference in the WTP for information between both groups (Wilcoxon-Mann Whitney test: EUR -0.12 vs. EUR 0.19, $p < 0.01$). In contrast, we find no such difference in High Probability (Wilcoxon-Mann Whitney test: $p = 0.91$). While subjects with a high claim x_H have a similar WTP for information as in Low Probability, subjects with a low claim x_L reduce their WTP to EUR -0.41 and are avoding information as well.[31]

[30] We elicit subjects' WTP with a variant of the Becker-DeGroot-Marschak (1964) mechanism in form of a multiple price list. Inconsistent statements include multiple switches or switches in the wrong direction. This restriction leads to exclusion of 14 out of 268 subjects and does not affect our results.

[31] The behavior in this group is not in line with any of our predictions. As honestly claiming $\xi(s_L) = L$ and $\xi(s_H) = H$ with information leads to a strictly larger payoff than claiming $\xi(s_\varnothing) = L$ under ignorance, these subjects should have a WTP of at least EUR 0 for information. In combination with the small number of subjects in this cell ($N = 9$), our findings for this group may not be robust and should be treated cautiously.

As a next step, we compare the reporting behavior under self-selection. For a predetermined price level of EUR 0.20, 19 percent of subjects self-select into information in Low Probability and 16 percent in High Probability. Figure 3.A1 (right side) displays the rate of unjustified claims dependent on the treatment allocation (compare to Figure 3.4 in section 3.5.1). We confirm the strong increase in the fraction of unjustified claims under ignorance in Low Probability (χ^2-Test: 46 percent vs. 88 percent, $p < 0.01$).[32] In line with Hypothesis 3.A1(ii), we also find that almost 90 percent of ignorant subjects make an unjustified claim of x_H. This is close to the theoretically predicted corner solution under self-selection, and a significant increase as compared to the exogenous allocation in section 3.5.1 (χ^2-Test: 88 percent vs. 66 percent, $p < 0.01$). In High Probability, effects are less clear-cut. While the fraction of unjustified claims under ignorance is comparable to the fraction under exogenous allocation, the fraction of unjustified claims with information significantly increases to 85 percent (χ^2-Test: 46 percent to 85 percent, $p < 0.01$). Consequently, we do not find a significant difference between information and ignorance (χ^2-Test: $p = 0.87$).[33] Nevertheless, results are broadly in line with Hypothesis 3.A1(i) and (ii).

Result 3.A1 *Subjects that self-select into ignorance are most likely to make a (potentially) unjustified claim of the high payoff.*

We conclude with a comparison of the WTP for information. We focus on subjects with a consistent statement of the WTP, which leaves us with 137 subjects in Low Probability and 133 subjects in High Probability. Figure 3.A2 shows the cumulative distribution of subjects choosing information over the entire range of prices (EUR -2 to EUR 2). It has a similar interpretation as a demand curve for information. A substantial minority of 31 and 35 percent avoids information (WTP $z < 0$), while the majority of subjects are information seekers (WTP $z \geq 0$). However, almost 79 and 73 percent of subjects have a WTP in the interval [EUR -0.2, EUR 0.2]. This suggests rather small costs of making a (potentially) unjustified claim both with information and under ignorance. We do not find a significant difference in the average WTP between both

[32] As the treatment allocation in the Self-Selection Task was independent of the Reporting Task, a within-subjects comparison is not suitable. The econometric approach of a between-subjects comparison chosen here is not completely appropriate to the data structure either, but seems to be the best alternative.

[33] Both for Low Probability and High Probability, the larger propensity of informed subjects to make an unjustified claim as compared to the exogenous allocation in 3.5.1 seems un-intuitive. A natural prediction would be that mostly honest subjects self-select into information. However, a large probability mass in M_2 and M_3 may lead to the observed reporting behavior. As we were agnostic about the distribution into the subsets, and due to discussed methodological issues of the Self-Selection Task, the findings for informed subjects should not be overstressed.

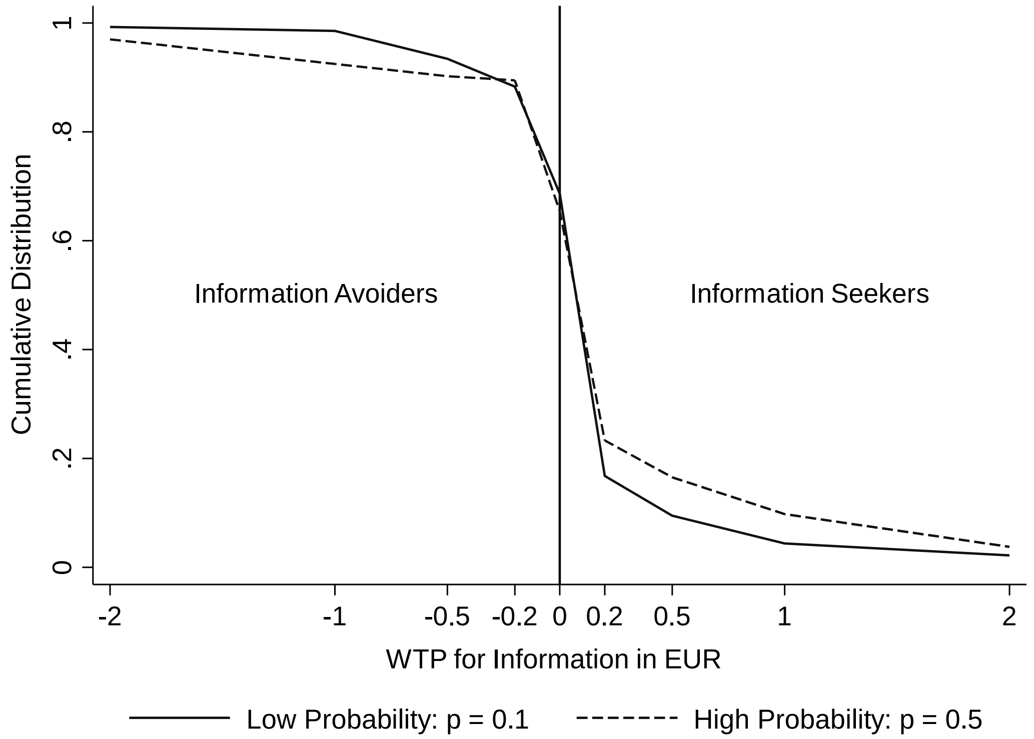


FIGURE 3.A2: Willingness to Pay for Information

treatments (Wilcoxon-Mann Whitney test: $p = 0.71$), and a Kolmogorov-Smirnov-Test does not reject the null hypothesis of equal distributions ($p = 0.89$). The distribution of the WTP for information in Low Probability is somewhat more concentrated around EUR 0, while there are both slightly more subjects with a positive and negative WTP for information in High Probability. Nevertheless, the WTP for information is largely unresponsive to the probability dimension, leading to no support of Hypothesis 3.A2.

Result 3.A2 *When being able to choose, most subjects prefer information to ignorance. The probability dimension does not influence this choice.*

3. B Appendix – Robustness Results for WTP for Information

We argue that the WTP for information is closely related to the (spread of) lying costs $(\theta_{Inf}, \theta_{Ign})$ with information and under ignorance (equation (3.3)). However, other factors might influence the WTP for information as well. For example, some subjects may consider their ignorance on the eligibility to payoffs as being eligible to the low payoff x_L. In this case, the WTP for information is equivalent to the WTP for participation in a lottery with the same probability distribution and spreads of payoffs.[34] To exclude this possibility and to draw further inferences about the WTP for information, subjects were offered to participate in two binary lotteries in the post-test section. The first (second) lottery yields EUR 0 with probability $p = 0.9$ ($p^{'} = 0.5$) and EUR 6 with probability $p = 0.1$ ($p^{'} = 0.5$). We impose the restriction of making a consistent statement of the WTP, which leaves us with 131 subjects in Low Probability and 125 subjects in High Probability.[35]

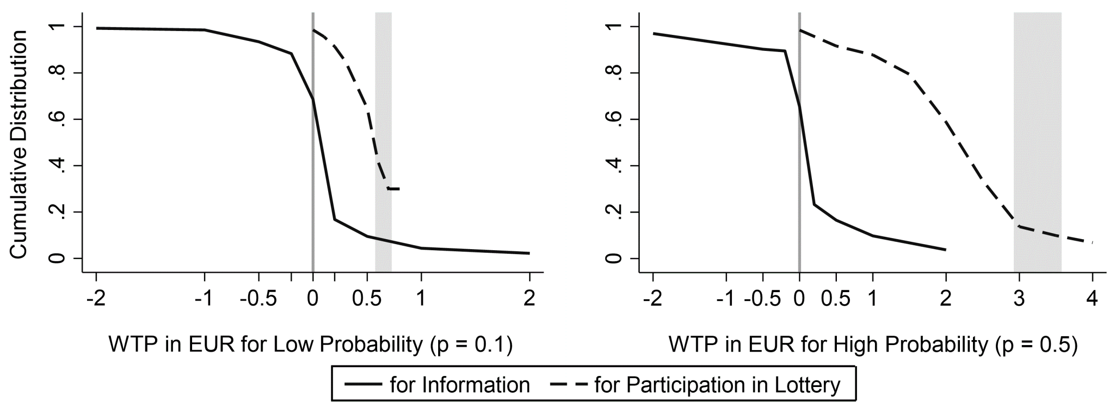


FIGURE 3.B: Comparison of the WTP for Information to the WTP for participation

Figure 3.B compares the WTP for information in the Self-Selection Task (solid line) to the WTP for the participation in the equivalent lottery (dashed line). The gray shaded area indicates the WTP for participation under maximization of expected payoffs.[36] The left side displays the comparison for Low Probability and the right side for High Probability. The average WTP for participation in both lotteries is significantly larger than the average WTP for information (Wilcoxon signed ranks test, EUR 0.51 vs. EUR -0.05 and EUR 1.90 vs. EUR -0.05, $p < 0.01$ and $p < 0.01$). For a price of EUR

[34] Moreover, risk aversion and loss aversion may affect the WTP.
[35] The restriction applies only to 9 to 10 percent of subjects. Our results are robust to this restriction.
[36] As we measure the WTP in intervals, we do not indicate a point prediction here.

0, almost all subjects participate in the respective lottery, whereas only 69 percent and 65 percent are willing to receive information. This difference becomes even larger for strictly positive prices (91 percent vs. 17 percent at a price of EUR 0.20 for Low Probability and 92 percent vs. 17 percent at a price of EUR 0.50 for High Probability). The gap in the WTP persists over the whole range of relevant price levels and only closes when prices approach the theoretical maximum WTP for participation. This indicates that the WTP for information originates from a different source than the WTP for participation in the lotteries, namely a subject's costs of making a potentially unjustified claim under ignorance and with information.

3.C Appendix – Robustness Results for Section 3.5.1

Table 3.C displays the results of a probit regression analysis of the reporting behavior. Specifications (1) to (7) are equivalent to the specifications in Table 3.1 and show the robustness of our results. In the two baseline specifications (1) and (2), moving from information to ignorance significantly increases the rate of (potentially) unjustified claims. The marginal effect amounts to 25 to 34 percentage points. As for the linear probability estimation, the coefficient on 'High Probability' in specification (1) suggests an increase in the fraction of unjustified claims. However, the introduction of the interaction term in specification (2) reveals that this effect only holds for subjects in the ignorance treatments. The marginal (total) effect in this group is 30 percentage points. Besides the total effect for ignorant subjects, specification (2) also indicates evidence for a larger increase in the propensity of making a (potentially) unjustified claim of x_H than with information. As of the potentially biased estimation of interaction terms in probit specifications (Ai and Norton 2003), this result should be interpreted cautiously.

Concerning the alternative specifications (3) to (7), results are in line with the linear probability estimation. We confirm the significant positive correlation of Economics and Business students to (potentially) unjustified claiming, but also find a significant correlation with age and gender in some of the specifications. Self-reported awareness of the opportunity to make unjustified claims as well as self-reported previous experience with the Reporting Task are related to a higher propensity to make a (potentially) unjustified claim (specification (4) and (5)). Results on the post-tests in specification (6) are broadly in line with the linear probability estimation, while there is no evidence for a correlation with Machiavellian traits in specification (7).

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ignorance	1.248***	0.933***	0.999***	1.055***	1.151***	1.072***	1.068***
	(0.180)	(0.228)	(0.242)	(0.243)	(0.303)	(0.297)	(0.297)
High Probability	0.764***	0.329	0.345	0.367	0.278	0.351	0.257
	(0.176)	(0.269)	(0.268)	(0.282)	(0.316)	(0.303)	(0.304)
Ignorance x High Prob.		0.809**	0.906**	0.917**	1.035**	1.126**	0.964**
		(0.374)	(0.375)	(0.402)	(0.432)	(0.452)	(0.425)
Awareness				0.901***			
				(0.205)			
Experience					0.892***		
					(0.252)		
Ambiguity Aversion						-0.196	
						(0.133)	
Taking Task						0.074***	
						(0.026)	
Information Avoiders						-0.393	
						(0.317)	
Curiosity 2nd Envelope						0.554*	
						(0.311)	
Machiavellianism							0.027
							(0.020)
Socio-Eco. Controls	YES	YES	YES	YES	YES	YES	YES
Constant	-0.611***	-0.431**	-0.902	-1.433**	-2.911***	-6.604***	-3.561***
	(0.159)	(0.172)	(0.640)	(0.674)	(0.920)	(1.417)	(1.053)
Observations	271	271	271	271	234	234	233

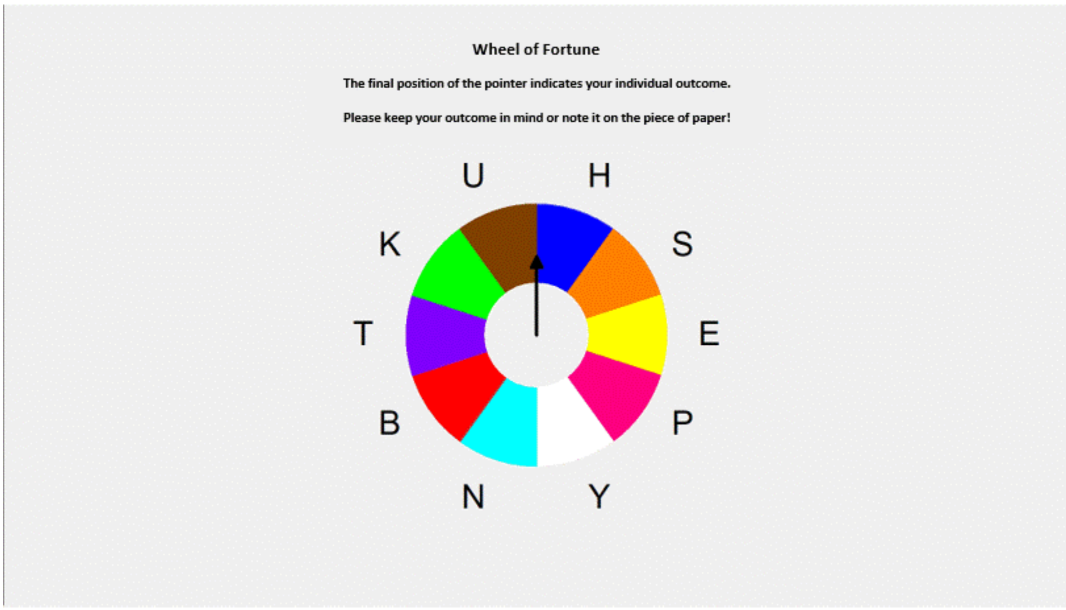
Notes: The table presents results of a probit regression with potentially unjustified claims as the dependent variable (dummy variable). The reference group is the treatment with information and a low ex ante probability of being eligible to the high payoff. "Ignorance" is a dummy variable that indicates treatments with ignorance on being eligible to the high or the low payoff, "High Probability" is a dummy variable that indicates treatments with high ex ante probability (prob. = 0.5) on being eligible to the high payoff, and "Ignorance x High Prob." is the interaction between both dummies. "Awareness" is self-reported awareness that only the claim determines the payoff (dummy variable), and "Experience" is self-reported previous participation in a related experiment as the reporting task (dummy variable). "Ambiguity Aversion" refers to ambiguity seeking, payoff-maximizing and ambiguity averse subjects (-1 to 1), "Taking Task" refers to the amount taken in a task in style of Tjøtta (2019) (Eurocents 0 to 50), "Information Avoiders" refers to subjects not willing to learn their lottery outcome in the post-test section (dummy variable), and "Curiosity 2nd Envelope" refers to subjects opening the second envelope ex post (dummy variable). "Socio-Eco. Controls" include a dummy for female subjects, the age of the subject, a dummy for subjects with German mother tongue, a dummy for Economics and Business students and the number of siblings. Robust standard errors are given in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 3.C: Robustness Analysis of Misreporting Behavior

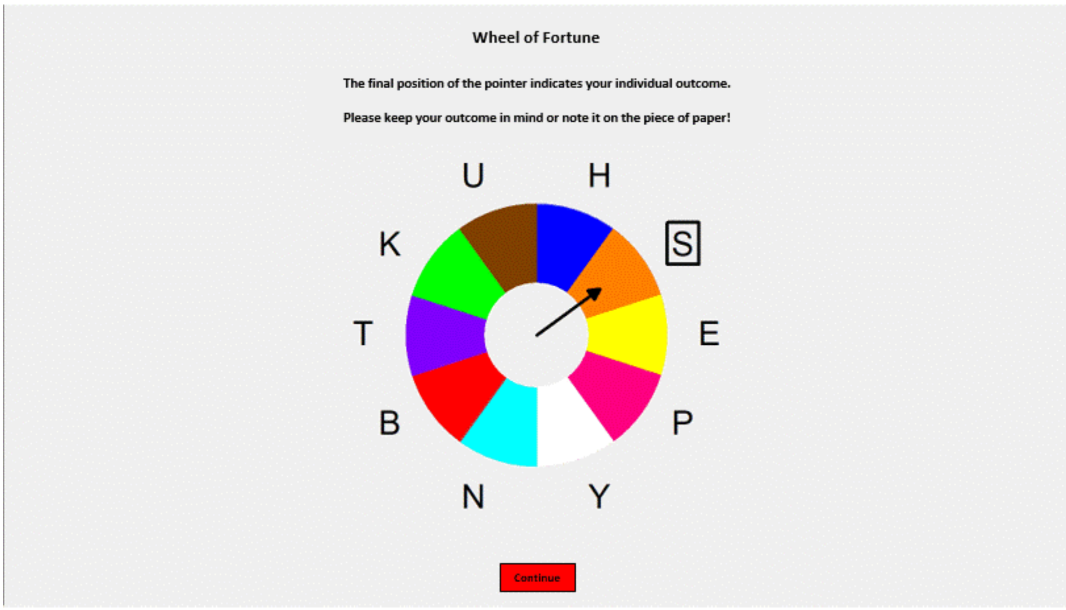
3.D Appendix – Screen-Shots from the Experiment

As noted in section 3.4.1, subjects found all instructions for the experiment on screen. This appendix presents a selection of screen-shots from the experiment. The experiment was conducted in German, and we provide an English translation here. For brevity and clarity, we only show the most important or most characteristic screen-shots. The actual experiment consisted of screens before, in between, and after the selection of screens shown here. Captions of the respective screen-shot provide additional information to which stage and to which treatment the screen-shot belongs to.

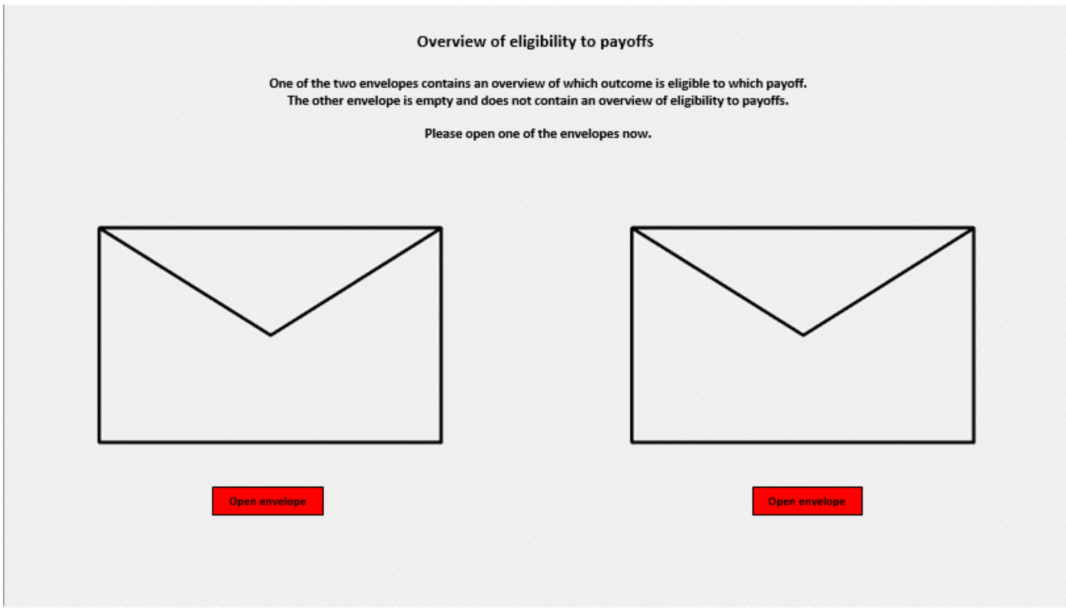
All Treatments: Explanation of the probability distribution before participation in the lottery (here: Low Probability).



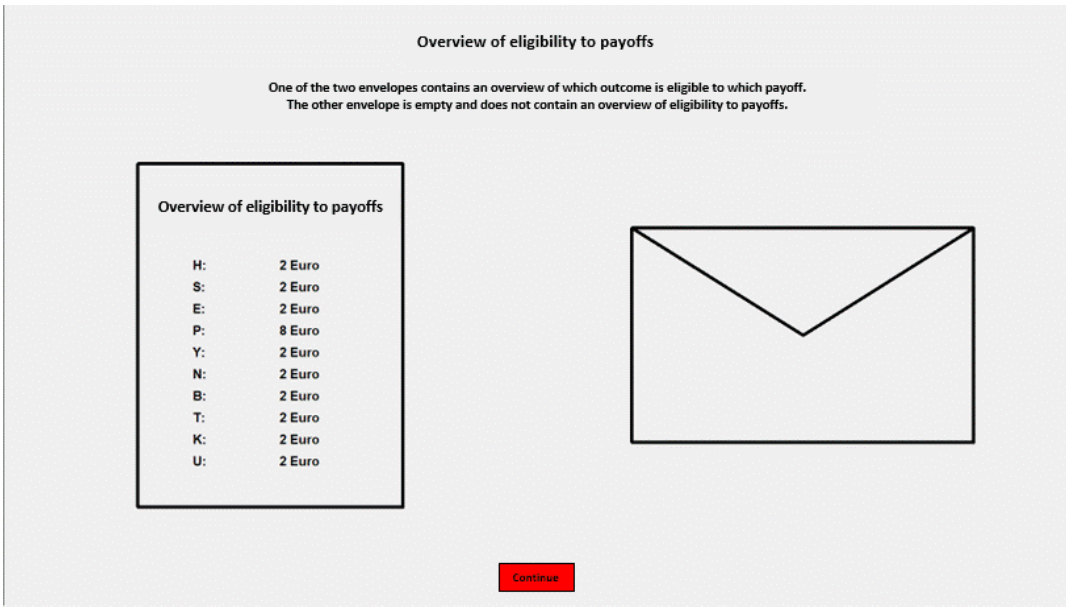
All Treatments: Initial state of the Wheel of Fortune before pointer starts spinning.



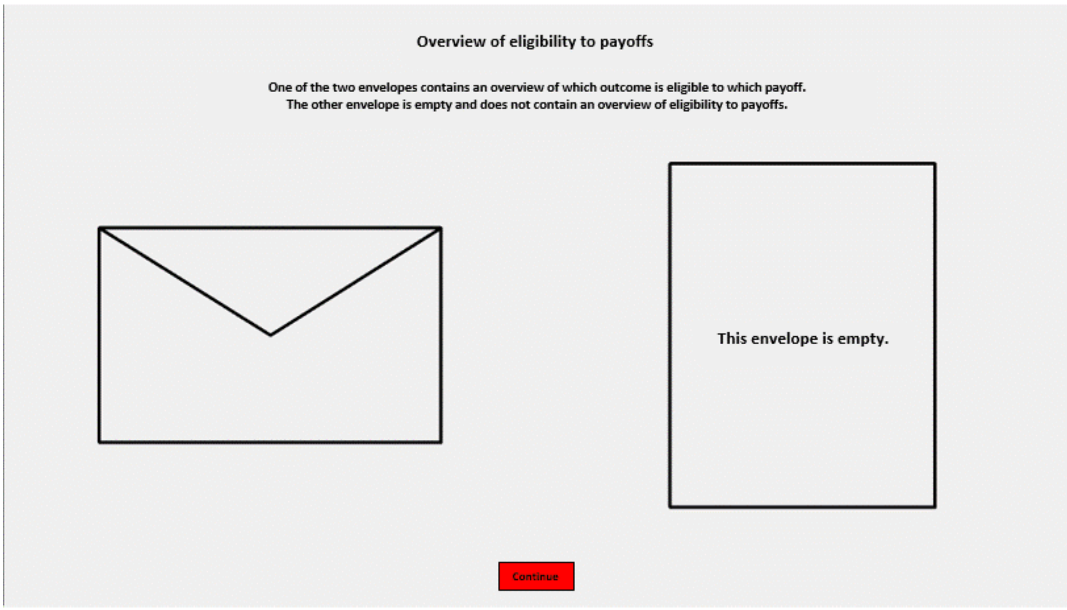
All Treatments: Final state of Wheel of Fortune with individual lottery outcome.



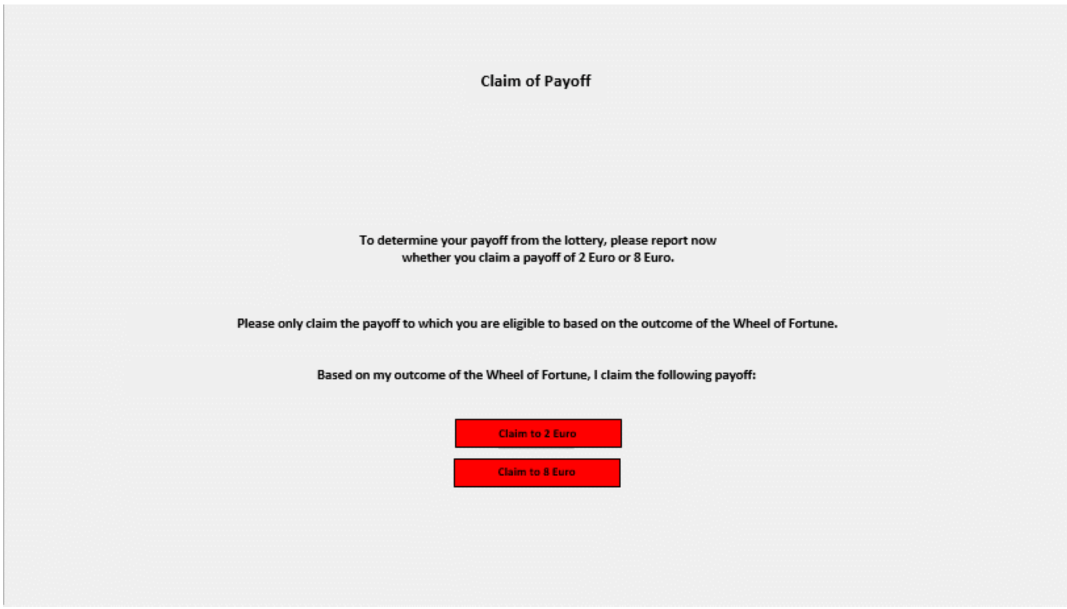
All Treatments: Initial situation before subjects open one of the envelopes.



Information: Subject opened the envelope with information (by chance).



Ignorance: Subject opened the envelope without information (by chance).



All Treatments: Subjects claim their payoff from the lottery.

4

Compliance in Teams – Implications of Joint Decisions and Shared Consequences

This chapter is based of joint work with Tim Lohse.[1]

4.1 Introduction

To comply, or not to comply: that is a question potentially worth billions of dollars as the dividend arbitrage trading scheme 'CumEx' or the Volkswagen emissions scandal show. These violations of tax laws and pollution standards give cause for concern. Despite enormous fines levied on companies, the well-documented deterring effect of audits seems to lose its bite when teams make compliance decisions. One crucial difference with an individual compliance problem is the involvement of several agents or parties. For example, in order to run the highly complex share deals of the CumEx tax fraud scheme (which had no economic purpose other than to receive tax reimbursements for taxes that were never paid) employees both within, as well as between, banks, stock traders, and law firms had to cooperate. Apart from this team decision-making, the concomitant economic consequences – the gains from undetected wrongdoing or the losses in the case an audit reveals the misconduct – oftentimes do not arise to a single decision-makers alone. Typically, the entire company and potentially also third parties

[1] The chapter is based on the Max Planck Institute for Tax Law and Public Finance Working Paper 2018–03 'Compliance in Teams – Implications of Joint Decisions and Shared Consequences.' For the dissertation, this chapter has been editorially adapted. These changes are the sole responsibility of the author of this dissertation.

can be affected. Thus, compliance decisions within teams might also hinge on the fact that the economic liability is shared among several agents.

This anecdotal evidence leads to several research questions. Are dyads as the smallest but yet important size of a team more prone toward non-compliance than single individuals? How is the team decision different from the decision by individuals and what is their motivation for (non-)compliance? Most importantly, what are the key determinants of the compliance behavior of teams? Is it the fact that the compliance decision is made by several agents in a team, or rather the fact that the monetary consequences are shared between team members? Answering these questions may give valuable insights into how non-compliance can effectively be reduced and under which conditions an auditing mechanism is suited to tackle non-compliance by teams.

In order to answer these questions, we run a laboratory compliance experiment. Non-compliance, i.e., falsely claiming to have a low income although the true income is high, and thereby evading a deduction, can pay off. However, an exogenous audit may apply, revealing the underreporting and leading to a fine. Along the first dimension of our 2-by-2 design, subjects either decide alone or in a team on their income reports. The two team members may coordinate before their decision-making via a chat. Along the second dimension, subjects are liable for the economic consequences either alone or in a team, i.e., team members either share their final earnings or not. Both dimensions are crucial but they are also intertwined.

Our results are as follows. Dyads are significantly more non-compliant than single individuals. Joint decision-making per se contributes little to this increase in non-compliance. In contrast, it is the shared (rather than individual) liability which leads to significantly more misreporting and accounts for most of the increased dishonesty of teams. Hence, our findings suggest that holding each team member fully (rather than only partially) liable might effectively restore compliance in teams. Further results indicate that teams successfully coordinate by making their decisions consensually. We also find evidence for behavioral spillovers between team members. More often teams tend to be dishonest, or sometimes honest, for both declarations. Evaluating the team chat protocols reveals that team members are mostly concerned about the risk of an audit and the monetary consequences of the report. Moral concerns about violating social norms of honesty only play a minor role.

4.2 Related Literature

We contribute to the well established literature on tax compliance (see Alm 2019 and Mascagni 2018 for surveys), which recently started to link the analysis of compliance behavior with methods and theories from psychology. For example, Christian and Alm (2014) show that the promotion of empathy and sympathy may help to increase compliance. Dulleck et al. (2016) provide evidence that psychic stress, arising from the contemplation of the possibility to evade, can increase compliance. So far, only a few papers experimentally study the role of social interactions within a group for tax evasion. Fairness seems to play an important role, but not social conformity (Fortin et al. 2007). Publicly exposing the deceivers in the laboratory deters evasion (Coricelli et al. 2010, 2014) and can have both a negative contagion effect and a positive shaming effect (Laury and Wallace 2005). As opposed to individual tax compliance, Abraham et al. (2017) show that joint tax compliance is positively affected by social norms. An inherent feature of team decision-making is also the transmission of information by taxpayer communication about the audit rate and compliance behavior of others, as studied by Alm et al. (2009, 2017). We contribute to the experimental compliance literature by explicitly studying differences in the compliance behavior between individuals and teams. Our differentiation between the collective decision and the shared economic consequences among the team members may advance our understanding of non-compliance and tax evasion. Given compelling evidence concerning the external validity of tax compliance studies undertaken in the laboratory (Alm et al. 2015), a lab experiment is particularly well suited to shed light on this matter. Importantly, we provide evidence that it is the shared liability that boosts non-compliance and suggest that enforcement policies aiming at this dimension might be particularly promising.[2]

As the compliance decision is a specific case of the more general question of whether or not to be honest, our study also adds to the literature on dishonesty in teams. Whereas Sutter (2009) shows that teams are as deceptive as individuals, Cohen et al. (2009) argue that groups might cheat even more. Findings by Weisel and Shalvi (2015) in a setting of a sequential dice roll (Fischbacher and Föllmi-Heusi 2013) point in the same direction, but stress the role of aligned incentives. Closest to our setting, Kocher et al. (2018) conduct a variation of the dice-rolling experiment and confirm higher

[2] We abstract from, first, other non-individualistic features of compliance problems such as the provision of public goods or voting on taxation parameters (for a survey on some aspects, see Alm 2014), and second, from theoretical discussions of corporate tax evasion (Chen and Chu 2005, Crocker and Slemrod 2005), or collaboration between two agents with heterogeneous lying aversion (Boadway et al. 2002).

misreporting in teams. Communication in teams is crucial for coordination. Inherent to team decisions is also the question of who benefits and who loses from a dishonest report. There is evidence that subjects lie more often if others benefit (Wiltermuth 2011, Gino et al. 2013, Conrads et al. 2013), but they lie less often when it comes at the expense of oneself or of others (Erat and Gneezy 2012). Compliance problems differ from mere cheating opportunities, and we contribute to this literature which typically abstracts from auditing mechanisms. Including both the liability dimension and the decision-making dimension allows us to investigate how these two key determinants impact the decision of being (dis)honest. The possibility of an audit also introduces extrinsic costs such as monetary fines, and dishonest reporting imposes a (monetary) risk for the team partner. Moreover, regret or shame may be more pronounced in a compliance framework. Finally, based on the analysis of the chat protocol we can identify the underlying motivations of the compliance decision in a team.

In light of the real world examples of non-compliance sketched above, our findings provide answers to important policy questions such as if, and under which circumstances, audits might be able to restore honesty in team decisions. More specifically, do companies need to take a setback and rely more on the decisions of single employees rather than teams to prevent dishonesty? Should law enforcement hold both parties of a joint non-compliance decision (partially) liable, or rather focus on punishing one party to the full extent?

4.3 The Experiment

4.3.1 Design of the Laboratory Experiment

1st Dimension / 2nd Dimension	Individual Decision	Joint Decision
Indiv. Liability	**Treatment 1 (T1)** Subjects = 56 Observations = 428	**Treatment 3 (T3)** Subjects = 78 Observations = 632
Shared Liability	**Treatment 2 (T2)** Subjects = 52 Observations = 450	**Treatment 4 (T4)** Subjects = 82 Observations = 656

FIGURE 4.1: Treatment Allocation

Overview. The experiment follows a 2-by-2 between-subjects design as illustrated in Figure 4.1. First, we vary whether subjects decide on their declared income individually or jointly in a dyad.[3] Second, we vary whether subjects are liable individually or collectively, i.e., on an individual level to the full extent or on a team level to proportional amounts. All treatments use a tax compliance task with 10 periods. In each period, a subject i earns with probability 0.2 a low income Y^l (400 Experimental Currency Units [ECU] = EUR 8) and with probability 0.8 a high income Y^h (ECU 1000 = EUR 20). While no deduction applies to a low income, subjects are supposed to pay a deduction Δ of ECU 400 on a high income. The deduction returns to the laboratory. For declarations of a high income, the deduction of ECU 400 is subtracted automatically from the income without further investigation. In contrast, declarations of a low income are independently from each other audited by the computer with a fixed probability $p = 0.5$.[4] In the case of no audit or a truthful declaration of a low income, no deductions or fines apply. However, if an audit reveals non-compliance, i.e., if a high

[3] The crucial behavioral differences we are studying primarily depend on whether a person is acting alone or not. We define a team as a dyad (cf. Conrads et al. 2013, Muehlheusser et al. 2015, or Weisel and Shalvi 2015) being the smallest but yet important size of a group. Firms, public administrations or the military frequently rely on dyads in the form of dual control in the course of which two persons have to agree on a certain decision.

[4] We have chosen a random audit because it is the easiest to understand. Other papers study alternatives, such as endogenous audit mechanisms where the audit probability increases with the estimated degree of underreporting (Gilpatric et al. 2011; Cason et al. 2016) or subjects' perceived untrustworthiness (Konrad et al. 2017). Our parametrization of incomes, deduction and fine borrows from Konrad et al. (2017).

income has been falsely declared as low, subjects must remargin the deduction of ECU 400 topped up by a fine Θ of ECU 200.[5] At the end of each period, subjects receive an overview of their earnings of this period (and of their partners' reporting and earnings in T2 to T4). Figure 4.2 summarizes the five stages of the decision process in a single period of the experiment.

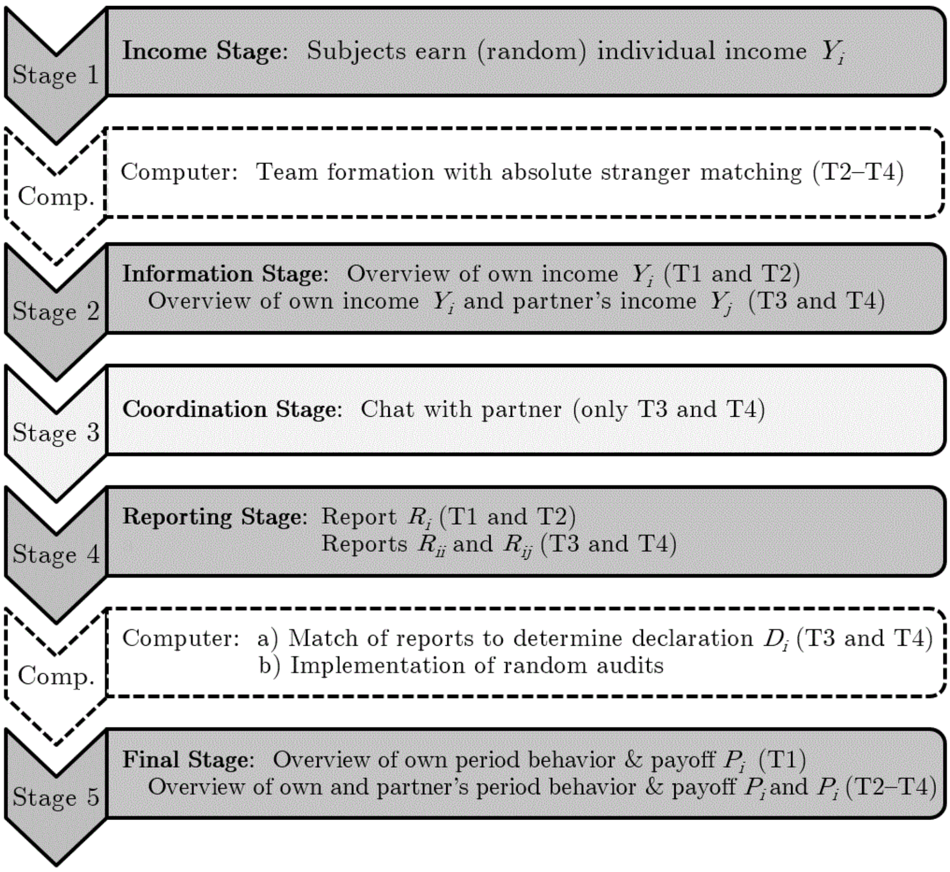


FIGURE 4.2: Procedure of one Period in the Experiment

First dimension: Decision-making. In treatments with individual decision-making (T1 and T2), subject i's only task is to make a report R_i of her own income Y_i with $R_i \in \{h, l\}$. This report automatically determines the declaration $D_i \in \{h, l\}$ of her income as $D_i = R_i$. In treatments with joint decision-making (T3 and T4), the computer forms a team by randomly matching a subject i with a partner j right after the income-generating stage. Team members i and j are mutually informed about their

[5]The unique best choice for subjects with a low income is to be honest about their income. Therefore, most of the subsequent analysis focuses on subjects with a high income since they face a trade-off of whether to be honest or not.

incomes Y_i and Y_j.[6] Each team member must then make two reports: one report of her own income and one report of the income of her partner. Hence, i reports R_{ii} and R_{ij}, whereas j reports R_{jj} and R_{ji}. In order to allow for a coordination on the reports before submitting them, team members enter a coordination stage (stage 3) and have the opportunity to exchange messages in a chatbox for 120 seconds. Then, both team members simultaneously make their reports on both incomes. For subject i the two reports R_{ii} and R_{ji} determine the declaration D_i of subject i's income in the following way: if $R_{ii} = R_{ji}$, i.e., if the own income report matches with the partner's report on one's own income, this matched report is registered as declaration D_i of subject i: $D_i = R_{ji} = R_{ii}$. However, if $R_{ii} \neq R_{ji}$, the true income of subject i is taken as i's declaration: $D_i = l$ if $Y_i = Y^l$ and $D_i = h$ if $Y_i = Y^h$ (and is never subject to an audit if $Y_i = Y^h$). Therefore, a dishonest declaration is only feasible if both team partners unanimously agree on being non-compliant.[7] The described procedure applies vice versa to team partner j's declaration and, thereby, implies the joint decision-making. Finally, a declaration of a low income $D_i = l$ is audited independently of the team partner's declaration of a low income $D_j = l$ with probability $p = 0.5$.

Second dimension: Liability. The second dimension varies to whom the payoff consequences of a declaration accrue. In treatments with individual liability (T1 and T3), subject i's period payoff P_i consists only of the earnings E_i from the declaration of the own income including the potential deduction and fine. There are two possible earnings for a subject with a high income: first, in the case of an honest declaration $D_i = h$ earnings are $E_i = Y^h - \Delta$; second, in the case of a dishonest declaration $D_i = l$ subject i faces a potential audit with probability 1/2 and expected earnings are $E_i = Y^h - \frac{1}{2}(\Delta + \Theta)$. Treatment 1 resembles the baseline setting in standard compliance games. In Treatment 3, individual liability means that if one team member conspires to underreport the other team member's income, she herself neither benefits from it nor would she face any fines should an audit reveal the underreporting.

In treatments with shared liability (T2 and T4), both team members mutually share their earnings. A subject i's period payoff is given by $P_i = \frac{1}{2}(E_i + E_j)$ with E_i and

[6] As the main focus of our experiment is to isolate the effect of joint decision-making from the effect of shared liability, we abstract from team dynamics over time and use a pre-announced absolute stranger matching protocol. In addition, we ensure that matched team members have the same income in order to rule out status or fairness considerations.

[7] Two alternative designs for the default mechanism of non-matching reports are conceivable but less suitable than our approach. In case an audit is automatically triggered, the best response to a team partner's report would be to state the same, i.e., comply if she complies and do not comply if she does not comply. In contrast, it is the weakly dominant strategy to base the report on one's true preference for our coordionation mechanism. In case non-matching reports lead to the implementation of only one of the reports with equal probability, incentives to reach an agreement on dishonest reports would be reduced and joint decision-making would essentially be incomplete.

E_j denoting i's and j's earnings including potential deductions and fines, respectively. Thus, both team members share the potential monetary benefits but also the monetary costs of a dishonest declaration in case the random and independent audits target D_i or/and D_j. This procedure keeps the range of potential earnings comparable to treatments without shared liability and does not change the total stake size of the individual reporting decision.

Implementation and payoffs. We conducted the experiment in July 2017 at the econlab Munich with 268 subjects (predominantly local students; average age 22.6; 51 percent female participants). Fourteen sessions with up to 24 subjects took place, each lasting for up to 100 minutes. Subjects were recruited using ORSEE (Greiner 2015) and the experiment was programmed and implemented in z-Tree (Fischbacher 2007). Experimental instructions were given in hard copy and were also read aloud by the same instructor. After the successful completion of some trial questions on the experimental setup, the main experiment with 10 periods started. At the end, one period was randomly chosen for the payoff. Following the final period, subjects took part in several incentivized post-experimental tests (Holt and Laury's (2002) risk elicitation task, the Cognitive Reflection Test (Frederick 2005) and a variant of Murphy et al.'s (2011) test for social value orientation). Each session concluded with a socio-economic questionnaire. Subjects earned an average of EUR 22 (min. EUR 9, max. EUR 31) including a show-up fee of EUR 6 and earnings from the post-experimental tests up to EUR 3.90.

4.3.2 Conceptual Framework

In our framework the compliance decision rests on two factors. First, we include the utility from the monetary payoff of non-compliance. Non-compliance has the same expected payoff along all dimensions, but shared liability leads to a more condensed distribution. Second, subjects may incur psychological costs from an untruthful report which may very well depend on the specific experimental treatment.[8] Along the first dimension, L_i^{ID} and L_i^{JD} denote subject i's lying costs for individual and joint decision-making, respectively. Lying costs can also occur along the second dimension and depend

[8] Given the constant size of a lie, we confine our framework to fixed lying costs. Without loss of generality, the lying costs are denoted separately according to the two dimensions of the experiment. This approach provides a clearer picture of the different costs at work. For a detailed investigation on the structure of lying costs, see for example Gneezy et al. (2018) and Abeler et al. (2019).

on whether subject i is individually liable (L_i^{IL}) or shares the economic consequences with the team partner (L_i^{SL}).

We focus on identical compliance behavior by both team partners and define $\widetilde{Y} := Y^h - \frac{1}{2}(\Delta + \Theta)$. We assume $u(\cdot)$ to be strictly concave and increasing in the earnings P_i. The treatment specific spread of expected payoffs (in utility terms) of subject i for non-compliance minus compliance read:

$$
\begin{array}{llcll}
 & \textit{Non-Compliance} & & \textit{Compliance} & \\
\text{T1:} & (1-p)u\left(\widetilde{Y}+\frac{1}{2}(\Delta+\Theta)\right)+pu\left(\widetilde{Y}-\frac{1}{2}(\Delta+\Theta)\right)-(L_i^{ID}+L_i^{IL}) & - & u\left(Y^h-\Delta\right) & (1)\\
\text{T2:} & (1-p)^2u\left(\widetilde{Y}+\frac{1}{2}(\Delta+\Theta)\right)+2p(1-p)u\left(\widetilde{Y}\right)+p^2u\left(\widetilde{Y}-\frac{1}{2}(\Delta+\Theta)\right)-(L_i^{ID}+L_i^{SL}) & - & u\left(Y^h-\Delta\right) & (2)\\
\text{T3:} & (1-p)u\left(\widetilde{Y}+\frac{1}{2}(\Delta+\Theta)\right)+pu\left(\widetilde{Y}-\frac{1}{2}(\Delta+\Theta)\right)-(L_i^{JD}+L_i^{IL}) & - & u\left(Y^h-\Delta\right) & (3)\\
\text{T4:} & (1-p)^2u\left(\widetilde{Y}+\frac{1}{2}(\Delta+\Theta)\right)+2p(1-p)u\left(\widetilde{Y}\right)+p^2u\left(\widetilde{Y}-\frac{1}{2}(\Delta+\Theta)\right)-(L_i^{JD}+L_i^{SL}) & - & u\left(Y^h-\Delta\right) & (4)
\end{array}
$$

For a risk-neutral subject without any moral concerns (i.e. lying costs of zero), the spreads as given by (1) to (4) are strictly positive and all equal, suggesting that the subject would underreport in all treatments. Aspects of risk aversion due to the monetary punishment in case of a detected misconduct on the one hand and strictly positive lying costs on the other hand may lead to negative signs of some or even all spreads. In such a case subject i would comply. The subsequent behavioral predictions about the effect of our two dimensions on compliance behavior are based on a comparison of the spreads across treatments.

Following the rich experimental evidence for an erosion of social norms in groups (Kocher et al. 2018, among others) and the possibility to defer the blame for anti-social behavior onto others (Dana et al. 2007, among others), we suppose $L_i^{ID} > L_i^{JD}$. Comparing the spreads of T3 and T1, Hypothesis 4.1 then follows directly from the positive sign of (3)−(1):

$$
\begin{aligned}
& (1-p)u\left(\widetilde{Y}+\tfrac{1}{2}(\Delta+\Theta)\right)+pu\left(\widetilde{Y}-\tfrac{1}{2}(\Delta+\Theta)\right)-(L_i^{JD}+L_i^{IL})-u(Y^h-\Delta)\\
- & \left[(1-p)u\left(\widetilde{Y}+\tfrac{1}{2}(\Delta+\Theta)\right)+pu\left(\widetilde{Y}-\tfrac{1}{2}(\Delta+\Theta)\right)-(L_i^{ID}+L_i^{IL})-u(Y^h-\Delta)\right]\\
= & L_i^{ID}-L_i^{JD}>0
\end{aligned}
$$

Hypothesis 4.1 *Joint rather than individual decision-making leads to more non-compliance.*

Further behavioral considerations can point in the same direction as Hypothesis 4.1. Teams are strategically more sophisticated (Kocher and Sutter 2005 or Sutter et al.

2013) and act with more self-interest (Kugler et al. 2007, among others). It is also for these reasons that teams might be more prone toward the economically rational decision to non-comply.

Evidence for Hypothesis 4.1 would cast a severe shadow on the concept of team work and joint decision-making in general and would provide a potential explanation for the recent corporate scandals of non-compliance. An impending implication for the design of governance rules would be to strengthen the role of individual decision-making and, thereby, possibly bringing back deep hierarchies with lonely leaders.

The derivation of Hypothesis 4.2 rests on two factors: Sharing the potential benefits of a dishonest report has been shown to increase misreporting (Wiltermuth 2011, Gino et al. 2013), and it is reasonable to suppose that $L_i^{IL} > L_i^{SL}$. Secondly, the more condensed distribution of earnings may induce more non-compliance for risk averse subjects. Comparing the spreads of T2 and T1, Hypothesis 4.2 follows from the positive sign of the difference (2)−(1):

$$\begin{aligned}
& (1-p)^2 u\left(\tilde{Y}+\tfrac{1}{2}(\Delta+\Theta)\right)+2p(1-p)u\left(\tilde{Y}\right)+p^2 u\left(\tilde{Y}-\tfrac{1}{2}(\Delta+\Theta)\right)-(L_i^{ID}+L_i^{SL})-u(Y^h-\Delta) \\
- & \left[(1-p)u\left(\tilde{Y}+\tfrac{1}{2}(\Delta+\Theta)\right)+pu\left(\tilde{Y}-\tfrac{1}{2}(\Delta+\Theta)\right)-(L_i^{ID}+L_i^{IL})-u(Y^h-\Delta)\right] \\
= & \left[(1-p)^2-(1-p)\right]u\left(\tilde{Y}+\tfrac{1}{2}(\Delta+\Theta)\right)+2p(1-p)u\left(\tilde{Y}\right)+(p^2-p)u\left(\tilde{Y}-\tfrac{1}{2}(\Delta+\Theta)\right)-L_i^{SL}+L_i^{IL} \\
= & (p^2-p)\left[u\left(\tilde{Y}+\tfrac{1}{2}(\Delta+\Theta)\right)+u\left(\tilde{Y}-\tfrac{1}{2}(\Delta+\Theta)\right)\right]+p(1-p)2u\left(\tilde{Y}\right)+L_i^{IL}-L_i^{SL} \\
= & p(1-p)\left[2u(\tilde{Y})-\left[u\left(\tilde{Y}+\tfrac{1}{2}(\Delta+\Theta)\right)+u\left(\tilde{Y}-\tfrac{1}{2}(\Delta+\Theta)\right)\right]\right]+L_i^{IL}-L_i^{SL}>0
\end{aligned}$$

The first two factors are positive, and the term in square brackets is strictly positive by Jensen's inequality for risk averse subjects. Since $L_i^{IL} > L_i^{SL}$, the sign of (2)−(1) is strictly positive. In an analogous way, the same holds for the sign of (4)−(3) when comparing T4 to T3.

Hypothesis 4.2 *Shared rather than individual liability leads to more non-compliance.*

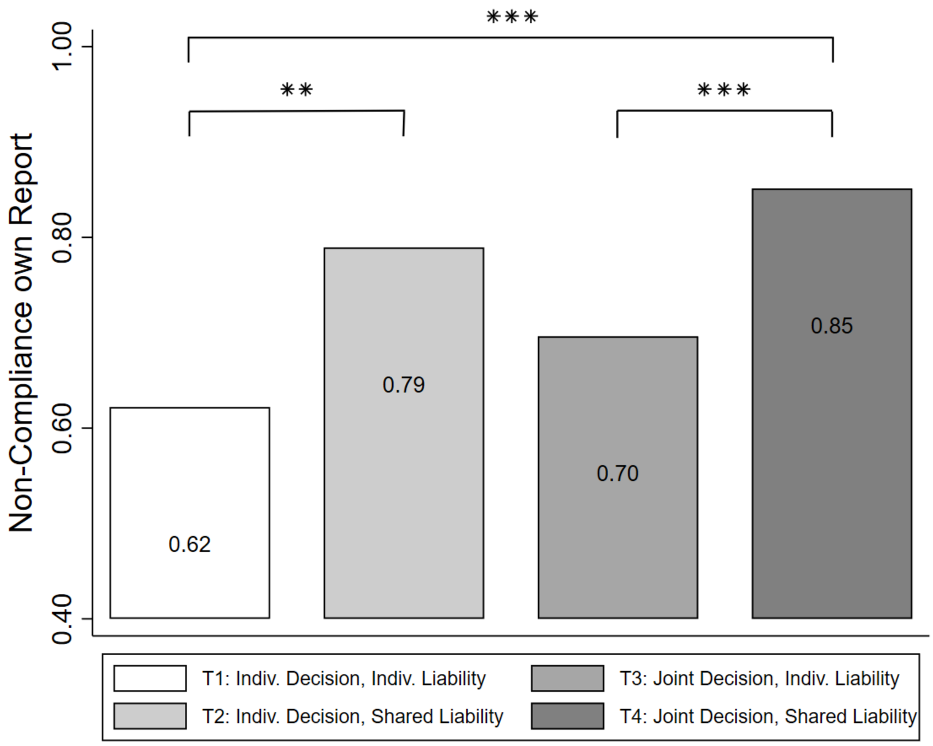
Beyond our framework, other aspects support Hypothesis 4.2. Sharing the potential fine may increase non-compliance because the per capita size of the loss per declaration is smaller and, therefore, appears individually less threatening.

Evidence for Hypothesis 4.2 would not question working in teams or flat hierarchies in general, but allude to a more subtle source of non-compliance. Sharing or even fully externalizing the costs of one's misconduct would provide exactly the wrong incentives since it would lead to more non-compliance.

4.4 Experimental Results

4.4.1 The Report of the own Income

We start our analysis with subjects' reports R_i (Treatment 1 and 2) and R_{ii} (Treatment 3 and 4) of their own income Y_i. This measure of compliance is directly comparable across all treatments.[9] In the following, we focus on subjects with a high income Y^h as they face a trade-off between reporting honestly or evading the deduction and reporting dishonestly. This leaves us with 2,166 observations from 268 subjects over 10 periods.



Note: Brackets indicate significant treatment differences, *** p<0.01, ** p<0.05.

FIGURE 4.3: Misreporting of own Income by Treatment

Figure 4.3 displays average non-compliance for the report of the own income in each treatment.[10] A comparison of Treatment 1 with individual liability and individual

[9]As outlined in section 4.3.1, the structure of the declaration D_i of income Y_i varies along the decision-making dimension. Our results in section 4.4.1 are robust to using the declaration as a measure of non-compliance instead. For a detailed analysis, please refer to appendix 4A.2.

[10](Non-)Compliance is computed as follows: we first calculate the non-compliance rate of each subject over the respective periods. This leaves us with one observation per subject for non-parametric tests ($N = 268$). Non-compliance per treatment, as displayed in Figure 4.1, is the average over individual rates. Results of a more conservative approach

decision-making to Treatment 4 with shared liability and joint decision-making reveals that the fraction of dishonest reports significantly increases from 62 percent to 85 percent (Wilcoxon-Mann-Whitney test: $p < 0.01$). Exploring the intermediate treatments refines the picture: joint decision-making has only a small effect and increases non-compliance by 7 percentage points on average. These differences are not statistically significant (Wilcoxon-Mann-Whitney test: $p = 0.29$ and $p = 0.41$). In contrast, effects for the liability dimension are clear-cut. Moving from individual to shared costs and benefits raises the fraction of dishonest reports significantly by 16 percentage points on average (Wilcoxon-Mann-Whitney test: $p = 0.02$ and $p < 0.01$, respectively). Importantly, this increase in non-compliance is nearly identical along the decision-making dimension.[11]

Table 4.1 confirms the findings in a multivariate analysis. The dependent variable is the misreporting of the own income (coded as a dummy variable). We show effects for a random effects probit regression that takes account of the panel data structure and clustering at the session level. Column (1) is a baseline specification and includes dimension dummies and a set of socio-economic control variables.[12] Joint decision-making has no significant effect on non-compliance, and we cannot confirm Hypothesis 4.1. In line with Hypothesis 4.2, shared liability significantly increases the probability of giving a dishonest report. Column (2) indicates the results of a specification that includes an interaction term between both treatment dimensions. Results from column (1) are validated, but we find no evidence of an interaction effect. The coefficient on the interaction term is small and not significant. The average marginal effect of shared liability in both specifications amounts to 12 percentage points and highlights the economic relevance of this dimension, while the non-significant average marginal effects of joint decision-making and the interaction term are smaller (4–5 percentage points and 1 percentage point, respectively). Hence, the two baseline regressions only point toward a negative effect of shared liability on compliance.

The alternative specifications in Table 4.1 (columns 3 to 6) include post-experimental tests and further robustness checks. Importantly, the main results from the baseline

with averaging at the session level ($N = 14$) are broadly in line with tests on the individual level. While we find no significant effect along the decision-making dimension, joint liability leads to significantly more non-compliance (Wilcoxon-Mann-Whitney test: $p = 0.20$ and $p = 0.03$, respectively).

[11] The observed effects emerge as a stable outcome after a few initial periods of learning. See appendix A.1 for detailed information about compliance behavior over time.

[12] Control variables include gender, age, number of siblings, and a dummy for students of economics and business administration. In line with earlier literature, we find that women are less likely to misreport. Moreover, age is positively correlated with compliance.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Joint Decision	0.233	0.219	0.215	0.203	0.237	0.233	0.236
	(0.168)	(0.244)	(0.225)	(0.271)	(0.235)	(0.260)	(0.254)
Shared Liability	0.594***	0.575***	0.554***	0.533***	0.612***	0.614***	0.600***
	(0.187)	(0.167)	(0.172)	(0.206)	(0.143)	(0.172)	(0.187)
Joint Dec. x Shared Liab.		0.0306	0.016	0.035	-0.010	0.045	-0.024
		(0.348)	(0.343)	(0.392)	(0.346)	(0.378)	(0.406)
Risk Attitude			0.122**				0.136***
			(0.048)				(0.048)
Cog. Refl. Test				0.243***			0.214**
				(0.079)			(0.084)
Soc. Val. Ori.					-0.007		-0.007
					(0.005)		(0.005)
Lagged Audit						0.310***	0.314***
						(0.090)	(0.090)
Socio-Eco. Controls	YES	YES	YES	YES	YES	YES	YES
Constant	2.524***	2.532***	2.066***	2.010***	2.604***	2.590***	1.647**
	(0.581)	(0.599)	(0.575)	(0.535)	(0.645)	(0.637)	(0.652)
Observations	2,166	2,166	2,166	2,166	2,166	1,958	1,958
Number of Subjects	268	268	268	268	268	268	268

Notes: The table presents results of a random effects panel probit specification with dishonest reporting (binary variable) as the dependent variable. "Joint Decision" is a dummy variable that indicates treatments with joint decision-making, "Shared Liability" is a dummy variable that indicates treatments with shared payoffs, and "Joint Dec. x Shared Liab." is the interaction between both dummies. The reference group is Treatment 1 with individual payoffs and individual decision-making. "Risk Attitude" refers to the number of risky decisions (0 to 10) in Holt and Laury (2002). "Cog. Refl. Test" is the number of correct answers (0 to 3) in the Cognitve Reflection Test (Frederick 2005). "Soc. Val. Ori." indicates Murphy´s et al. (2011) measure of distributional preferences (-16.26 to 63.39) and "Lagged Audit" is a dummy for being audited in the previous period. "Socio-Eco. Controls" include a dummy for female subjects, the age of the subject, a dummy for Economics and Business students and the number of siblings. Standard errors in parentheses account for clustering at the session level,
*** $p < 0.01$, ** $p < 0.05$.

TABLE 4.1: Multivariate Analysis of Dishonest Reports of the own Income

specifications (1) and (2) are robust. Since the audit introduces a risk dimension in the compliance decision, we control for subjects' risk preferences (Holt and Laury 2002) in column (3). In line with the previous literature, we find that being less risk-averse is positively correlated to higher non-compliance. Cognitive ability, as measured by Frederick (2005), might influence the compliance decision as more impulsive subjects may over- or underestimate potential costs. We find that reflective thinking is positively correlated to a higher probability of giving a dishonest report (column (4)). Due to the monetary consequences for the team partner, social preferences might also alter the compliance decision. In column (5), social preferences as measured by the Social Value Orientation (Murphy et al. 2011) are not significantly related to the compliance decision. As an additional robustness check, we include lagged audits in our regression analysis (column 6). In line with earlier literature on the bomb crater effect

(Mittone 2006), we find that lagged audits on the report of the own income increase non-compliance. We complete our analysis with the full model including all independent variables and show that results are robust (column 7). We summarize the findings above in our main result providing support for Hypothesis 4.2 but very limited evidence for Hypothesis 4.1:

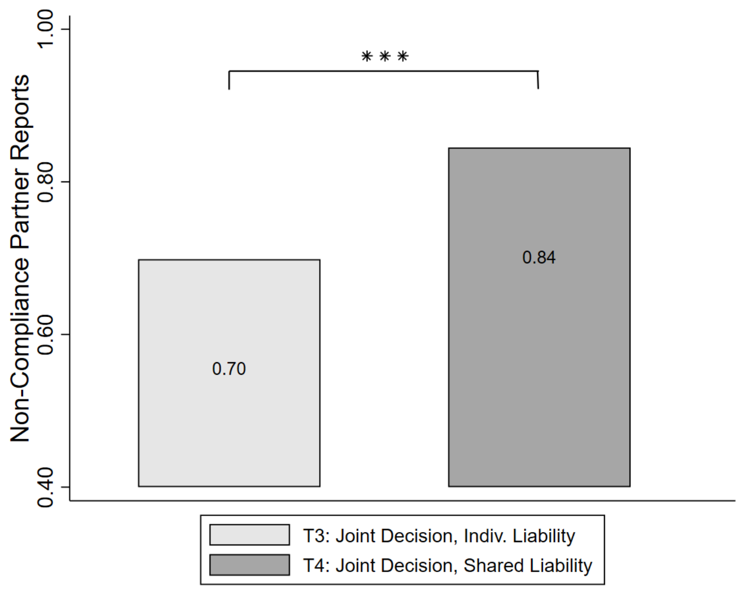
Result 4.1 *For the report of the own income*

(i) joint decision-making has only little effect on the compliance rate;

(ii) shared liability significantly increases non-compliance.

4.4.2 The Report of the Partner's Income

Besides the report for their own income R_{ii}, subjects are required to make a report R_{ij} for their partner's income Y_j in treatments with joint decision-making (Treatment 3 and 4). The procedure leaves us with two reports per subject in these treatments: a report for the own income and a report for the income of their partner.



Note: Bracket indicates significant treatment difference, *** p<0.01.

FIGURE 4.4: Misreporting of Partner's Income

Figure 4.4 displays the non-compliance rate for the reports on the partner's income. Aggregate non-compliance is comparable to the report of the own income. In Treatment 3 with joint decision-making and individual liability, 70 percent of reports for the partner's income are dishonest. This fraction increases to 84 percent in Treatment 4 with joint decision-making and shared liability (Wilcoxon-Mann-Whitney test: $p < 0.01$). The (nearly) perfect similarity of the aggregate compliance rates for the report of the partner's income (Figure 4.4) with compliance for the report of the own income (Figure 4.3) disguises the fact that not all subjects who make honest reports for their own income R_{ii} do so for their partner, R_{ij} (and vice versa). While 91 percent do so in Treatment 4, 28 percent choose deviating reports in Treatment 3. Moreover, the vast majority of teams successfully coordinate on the reports. Reports match with 95 percent probability in Treatment 3 and with 92 percent probability in Treatment 4.[13]

Result 4.2 *Subjects are willing to make a dishonest report on behalf of their partner, especially when they may benefit from joint non-compliance under shared liability.*

4.4.3 The Declaration Behavior within Teams

Although joint decision-making has no effect on the aggregate compliance rate, it does influence the composition of declarations within teams. Figure 4.5 summarizes the two declarations of a team in Treatment 2 to 4.[14] For each treatment, the upper bar ('Observed') displays the actual composition of declarations as observed in our data. On the lower bar ('Predicted'), we display a pseudo-prediction on the composition of declarations. This prediction assumes that coordination is not possible, and consequently, there are no behavioral spillovers between the team members. The composition of declarations follows a simple calculation based on the observed aggregate compliance rate in the respective treatment.[15] The slightly gray shaded area on the very left ('honest teams') refers to the fraction of teams with two honest declarations, the one in the middle ('mixed teams') refers to the fraction of teams with one honest and one dishon-

[13] In the few cases of non-matching reports, a subject's partner does not systematically deviate in one direction. The probability of a subject making a dishonest report and her partner making an honest report on the subject's income is roughly comparable to the opposite case of a subject making an honest report and her partner making a dishonest report on the respective income.

[14] Since Treatment 1 (individual decision and liability) has no team characteristics, we do not include this treatment here. If the same matching protocol as for the other treatments is applied, the results for artificial teams read as follows: 14.0 percent of teams are classified as honest, 46.3 percent of teams are classified as mixed, and 39.7 percent of teams are classified as dishonest.

[15] In absence of coordination, the predicted fraction of honest teams is the square of the compliance rate (1 subtracted by the non-compliance rate), the predicted fraction of mixed teams is two times the compliance rate multiplied by the non-compliance rate, and the predicted fraction of dishonest teams is the square of the non-compliance rate. By illustration, the calculation for Treatment 2 is the following: fraction of honest teams is $(1 - 0.78)^2 = 0.05$; fraction of mixed teams is $2 \cdot (1 - 0.78) \cdot 0.78 = 0.34$; fraction of dishonest teams is $0.78^2 = 0.61$.

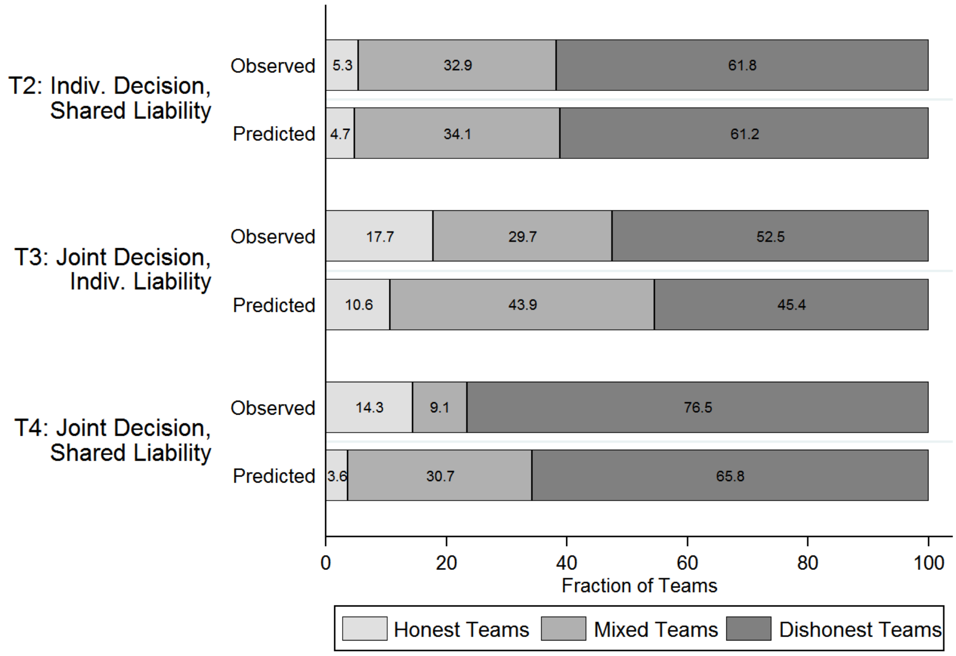


FIGURE 4.5: Composition of Declarations within Teams

est declaration, and the dark gray area on the far right ('dishonest teams') refers to the fraction of teams with two dishonest declarations.

A comparison of the observed and the predicted composition of declarations allows for detailed insights on the impact of joint decision-making. In Treatment 2, decision-making occurs individually and coordination among team members is not possible. The distribution of honest, mixed and dishonest teams closely follows the theoretically expected distribution based on the actual compliance rate. Consequently, individual declarations of team members are not correlated.[16] In contrast, both treatments with joint decision-making are characterized by a medium to high correlation of declarations within teams. The positive correlations suggest that joint decision-making is marked by spillovers between team members which results in more consequential declaration behavior. In comparison to the predicted composition in absence of coordination, teams

[16] We measure the correlation of declarations by the phi coefficient, which has a similar interpretation to the Person correlation coefficient but is suited for binary variables. Calculation is based on a 2x2 contingency table of pairs of declarations in teams. Rows reflect the declaration of the own income and columns the declaration of the partner's income. A correlation of zero results when the probability in each of the four cells (honest, mixed and dishonest teams) equals to the product of marginal probabilities (the aggregate (non-)compliance rate). While the phi coefficient is not different from zero in Treatment 2 (corr=0.04, χ^2-statistic: $p = 0.60$), the phi coefficient is positive in Treatment 3 and 4 (corr = 0.32 and corr = 0.70, χ^2-statistic: $p < 0.01$, respectively).

choose more often to be honest or to be dishonest for both declarations, but less often the intermediate case of mixed declarations. For Treatment 3 with joint decision-making and individual liability, the observed fraction of mixed declarations is 14 percentage points lower than predicted (29.7 percent vs. 43.9 percent), while both the fraction of two honest or two dishonest declarations is higher than predicted. For Treatment 4 with joint decision-making and shared liability, this effect is even more pronounced and the case of mixed declarations is almost crowded-out completely (9.1 percent vs. 30.7 percent). This pattern is also evident from a direct comparison of the observed composition of declarations between Treatment 2 and Treatment 4: moving from individual to joint decision-making leads to a reduction of mixed declarations, and a corresponding increase in mainly dishonest but also some honest declarations.

In summary, the compliance behavior under joint decision-making is potentially two-directional: some teams may be less compliant, and some teams may be more compliant. As the magnitude of both directions is roughly comparable and cancels out, this might explain why joint decision-making has no effect on the aggregate compliance rate.

Result 4.3 *Joint decision-making is characterized by behavioral spillovers between team members. As a consequence, teams are prone to be either honest or dishonest.*

4.4.4 Evaluation of the Team Chats

The analysis of chat protocols allows for further insights into team decision-making in Treatment 3 and 4.[17] Less than 2 percent of teams abstain from using the chat. Usage of the chat leads to a successful coordination on the mutual income reports (99 percent in Treatment 3 and 96 percent in Treatment 4). The finding is in line with the low rate of non-matching reports (compare to section 4.4.2). In half of all chats, the chat is not only a mere instrument for coordination (e.g., typing in a single number as a proposed report), but it is also used for the exchange of arguments in favor or against non-compliance. In early periods this exchange of arguments is slightly more pronounced (57 percent) as compared to final periods (46 percent).

Conditional on subjects giving an honest or dishonest report, Figure 4.6 provides an overview of the frequency with which a particular argument is mentioned in the

[17] The evaluation of chats was carried out independently by five research assistants according to an analysis protocol (available upon request). For each chat, we take the median evaluation of graders.

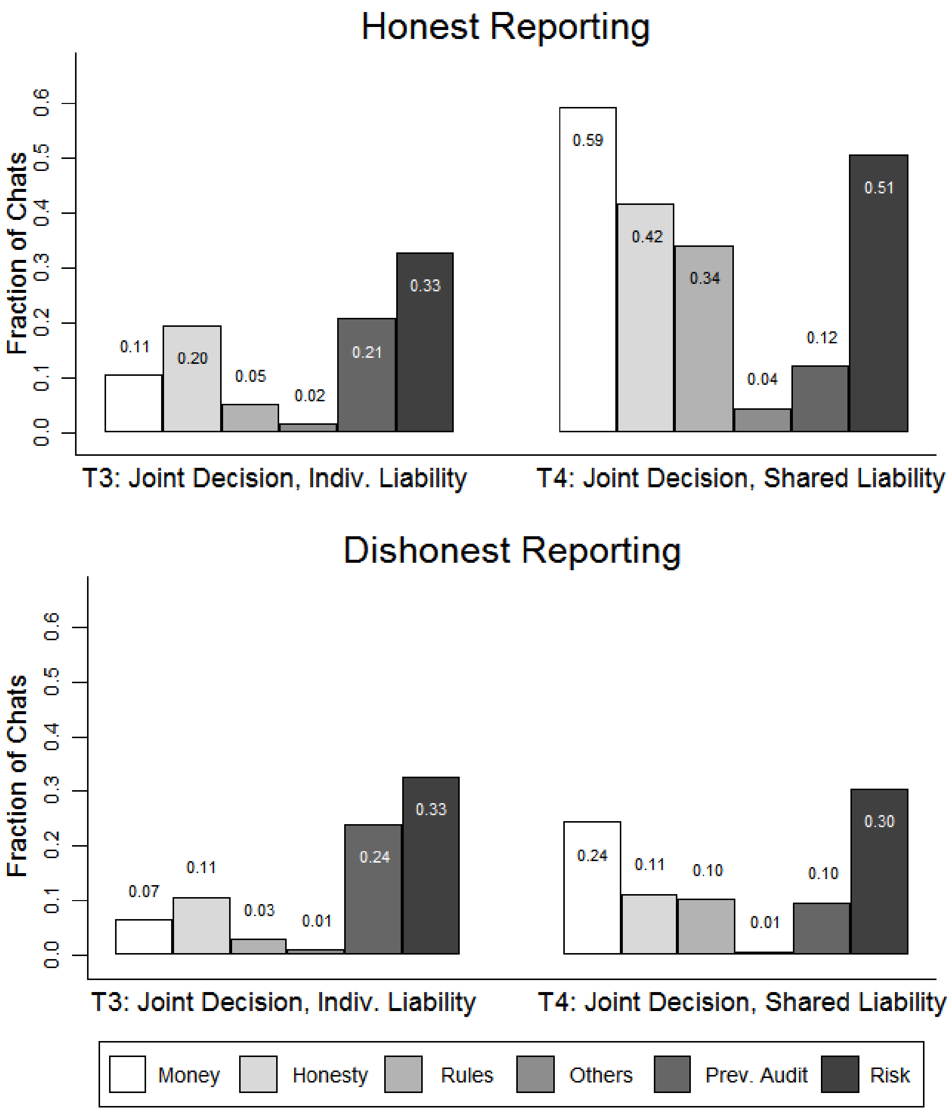


FIGURE 4.6: Frequency of Arguments mentioned in Chat

chat.[18] Inspired by Kocher et al. (2018), we use following categories: "Money" refers to the frequency with which the monetary consequences of a report are addressed, while "Honesty" indicates discussions on being truthful. "Rules" refers to the experimental instructions, in particular that subjects were supposed to report their actual income. "Other" indicates discussions about the compliance behavior of other subjects, while "Prev. Audit" captures the experience with audits in previous periods. Finally, "Risk" refers to the risk dimension of the compliance task.

In quantitative terms, the risk dimension is the most important argument, followed by the monetary consequences in case of shared liability (Treatment 4) or experience with previous audits in case of individual liability (Treatment 3). The social norm of honesty is discussed much less and only ranks third. This observed chat behavior is in line with Result 4.1. Moral aspects play a minor role as compared to economic aspects, implying that the tendency to disguise one's own responsibility for underreporting takes a back seat. Adherence to rules and the behavior of other subjects are mentioned only in a few cases (except for honest subjects in Treatment 4). We do not observe strong treatment differences for dishonest reporting, while there are evident differences for honest reporting. Both in terms of the frequency and the distribution of arguments, honest reporting in Treatment 4 is different compared to the other cases and seems to be the most controversial option.

As we partially focus on categories of arguments comparable to Kocher et al. (2018), we are able to relate our compliance task to their computerized deception problem. This gives insights into the similarities and differences of compliance and deception problems. We find differences mainly in two aspects: first, the frequency of argumentation is higher for honest reporting (and not for dishonest reporting) in the compliance task. Second, the most frequent argument for dishonest reporting is the risk dimension, as compared to the monetary consequences in the deception problem. However, monetary consequences are also relevant for the compliance problem, though mostly for honest reporting. Moreover, both the discussion of monetary consequences and of risk considerations are related to the economic consequences of a report, and therefore indicate a similarity rather than a difference between both problems. Both settings share that the concept of being honest is identified for an intermediate number of

[18] We focus on subjects with a high income that used the chat. For honest reporting, this leaves us with 189 observations in Treatment 3 and 91 observations in Treatment 4. For dishonest reporting, we have 439 observations in Treatment 3 and 551 observations in Treatment 4. There might be zero, one or multiple arguments per chat (hence, frequencies do not add up to 1), and some statements may fall into more than one category. This procedure allows for a direct assessment of the importance of arguments in the respective four cases.

chats. Adherence to rules and the behavior of others are only of minor importance for both settings. In summary, the introduction of an audit shifts attention toward the risk dimension and the audit itself, while other motivations, such as the monetary consequences or the concept of being honest, remain important for the compliance task.

Result 4.4 *The vast majority of subjects use the chat to coordinate on the mutual reports. Arguments refer primarily to the economic consequences but less to moral concerns.*

4.5 Discussion

The small insignificant increase of dishonest reporting along the decision-making dimension is good news and bad news at the same time. On the one hand, the deception literature's doom assessment of team behavior as rather deceptive does not entirely carry over to the more complex framework of compliance. A possible explanation is that if partner j is devoted to honesty while i isn't, the latter would need to convince the former of joint non-compliance. As a team partner can always enforce on honest declaration by making an honest report, only deceiving subjects have an incentive to persuade their partner of giving a dishonest report, too. This might be quite cumbersome, morally challenging as a deferral of responsibility for non-compliance is difficult, and, thus, could drive up i's psychological costs of joint decision-making. Moreover, a social observer may assign the moral responsibility for a dishonest declaration on the simple question "Cui bono?" In this case, it would not matter how the compliance decision was made but only who economically benefits. For these reasons, the inequality of the associated lying costs ($L_i^{ID} > L_i^{JD}$) could become less strict or even reverse, and Hypothesis 4.1 would not materialize. On the other hand, a negative implication is that third-party reporting seems to be ineffective in reducing dishonest behavior when agents have the possibility to coordinate beforehand. Non-compliance in the joint decision treatments requires not only a silent approval or non-objection, but an active statement by the team member. This condition leads neither to a large fraction of non-matching reports nor does it prevent successful evasion. Although honest subjects are not dependent on their partner to achieve a truthful declaration, a substantial fraction of subjects are willing to actively assist their partner in being non-compliant.

Unlike for joint decision-making, our results on shared liability are clear-cut. Independent of the decision-making dimension, sharing the costs and benefits of a dishonest report decreases compliance. In quantitative terms, this dimension accounts for over two thirds of the difference in the compliance rate between Treatment 1 and Treatment 4. An explanation is that the concept of "White Lies" (Erat and Gneezy 2012) transfers to compliance problems, i.e., subjects are more likely to not comply if it benefits not only themselves on expectation but also their partners. The magnitude of the effect would be in line with previous findings from the deception literature.[19]

Implications arise to the design of the internal structures of companies or organizations. Importantly, team decision-making per se is not the major source of non-compliance in group settings. Hence, there is no need for firms to forgo the benefits of team work in order to induce more honest decisions by its employees. Instead, our results suggest that it is sufficient to set liability rules right, meaning that each team member could be fully rather than just partially economically liable. This should deter misconduct ex ante. In the case of the Volkswagen emission scandal, for example, full individual liability might have prevented engineers and managers from committing their offenses in the first place. The recommendation of full individual liability does not only hold for teams, but also for compliance decisions by single employees that have potentially large external effects on the company: our findings from Treatment 2 show that shared liability substantially increases non-compliance even under individual decision-making. Famous rogue traders Nick Leeson (Barings Bank) and Jérôme Kerviel (Société Générale), for example, caused huge losses (and, in Leeson's case, the collapse of the bank) in an attempt to increase company profits through trading activities which violated internal regulations. Importantly, while acting on their own, successful non-compliance would have been individually advantageous for them as well as for the entire company. However, these cases also show the limits of individual liability rules ex post. The losses caused by Leeson or Kerviel outweighed their individual economic capability by far. Ultimately, such settings call for tight internal monitoring of employees' activities and external prosecution of misconduct for which the culprit could even face imprisonment. For other applications, such as joint tax evasion, individual liability is potentially a feasible policy: fines for unreported employment or evaded sales taxes might be imposed on either party to the full extent, and thereby deployed as an effective deterrence mechanism.

[19] Since the possibility of an audit in our setting is a major difference to the deception problem, a direct comparison should be interpreted cautiously. Erat and Gneezy (2012) find an increase of cheating of 13 percentage points (treatment T[10,0] vs. T[10,10]), while Wiltermuth (2011) reports an increase between 10 and 15 percentage points (treatment self-alone vs. self-and-other, study 2 and 3). Conrads et al. (2013) show that the average reported number in a dice-rolling experiment is inflated by 17 percent for teams (treatment Individual vs. Team).

4.6 Conclusion

Team decisions are widespread, and firms heavily rely on team work. However, recent corporate delinquencies and tax scandals suggest that team decisions may also have a dark side: teams seem to be more dishonest than individual decision-makers. In this chapter, we study the (tax) compliance decision of dyads as the smallest form of teams in a laboratory experiment. We ask whether the compliance behavior of teams is different from the compliance behavior of individuals and identify the effect of key differences to individual decisions. The first dimension of our 2-by-2 between-subjects design varies whether the compliance decision is made individually or in a team. Teams coordinate via a chat and each member subsequently reports both the own income and the income of the team member. The second dimension varies whether the monetary consequences of the (non-)compliance decision, in particular gains from undetected non-compliance and losses in the case of an audit, accrue only to the respective subject alone or if they are shared between both team members. This allows us to answer an even more important question, namely how non-compliance by teams can effectively be deterred. Specifically, we ask under which conditions an auditing mechanism is suited to address non-compliance by teams.

We find significantly more dishonesty in dyads as compared to individuals. This effect can mainly be attributed to the liability dimension: independent of whether the compliance decision is made on an individual basis or in teams, shared economic consequences among team members lead to significantly increased non-compliance. In contrast, the impact of team decision-making is small and we find no significant effect along this dimension. Further results indicate that decisions in teams are characterized by a low rate of disagreement in terms of non-matching reports and a medium to high correlation of reporting behavior. Most important motivations for (non-)compliance include the risk dimension of the compliance task and the monetary consequences of a report. Our findings are corroborated by different robustness checks. An implication of these insights for firms or organizations in general, is that team structures do not necessarily lead to more non-compliance. In order to restore compliance within teams, it is advisable to prohibit sharing of consequences but to place an emphasis on the full liability of each individual team member.

4.A Appendix – Evolution of Compliance Behavior over Time

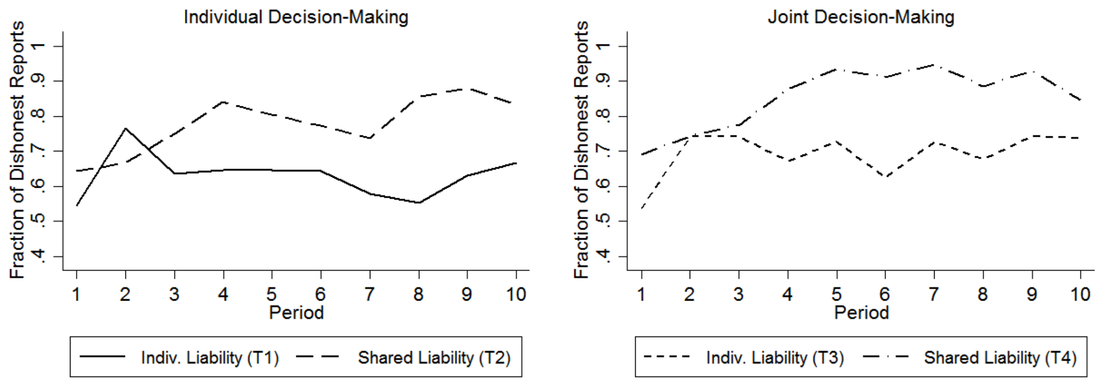


FIGURE 4.A1: Compliance over Time conditional on Individual (left side) and Joint Decision-Making (right side).

Figure 4.A1 compares compliance behavior over periods. The left side displays the evolution of non-compliance for individual and shared liability conditional on individual decision-making (Treatment 1 and 2). The right side displays this evolution conditional on joint decision-making (Treatment 3 and 4). The insights from aggregate misreporting are in line with these observations over time: a 'compliance gap' of 10 to 20 percentage points along the liability dimension emerges from period three onwards.

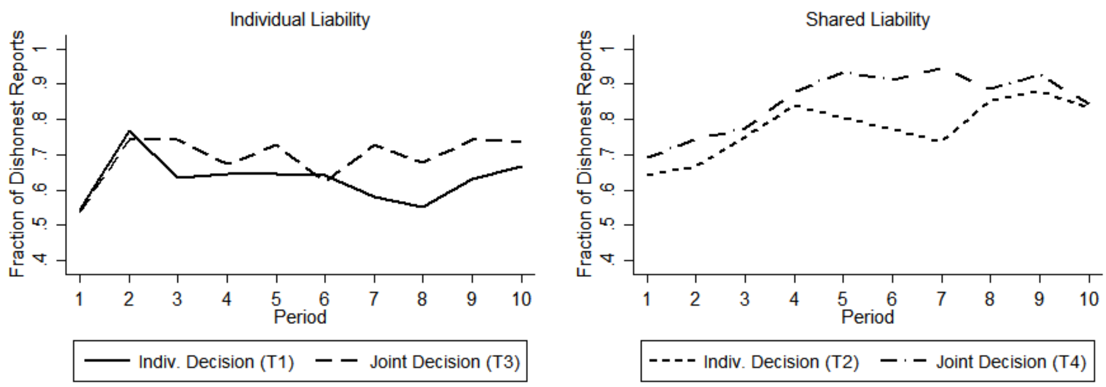


FIGURE 4.A2: Compliance over Time conditional on Individual (left side) and Shared Liability (right side).

Comparing the evolution in Treatment 1 to Treatment 3 and Treatment 2 to Treatment 4 reveals that the effects for decision-making are weaker. There seems to be no stable 'compliance gap' along this dimension (Figure 4.A2). The graphs also reveal no evidence of an interaction of the liability and the decision-making dimension, since the effect of shared liability for both individual and team decision-making are fairly similar.

Both graphs exhibit the same pattern over periods. Compliance behavior still fluctuates in the first three periods while the rate of dishonest reports increases in three out of four treatments. Subsequently, the compliance rate converges to a treatment-specific level.[20] We cannot observe strong reversals in the compliance behavior or end-round effects. Therefore, compliance behavior is not the outcome of some specific outlier periods but it is a stable pattern.

[20]Although we ran pre-experimental practice questions, the most probable explanation is the learning of subjects. Since the shared liability treatments are slightly more complicated than individual liability treatments, convergence takes one to two periods longer. Nevertheless, the between-treatment differences in compliance behavior in period 1 are already broadly in line with following periods.

4.B Appendix – The Declaration of Incomes

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Joint Decision	0.024	0.074	0.070	0.057	0.088	0.076	0.069
	(0.162)	(0.235)	(0.221)	(0.263)	(0.227)	(0.252)	(0.258)
Shared Liability	0.497***	0.560***	0.541***	0.526***	0.589***	0.594***	0.578***
	(0.182)	(0.160)	(0.164)	(0.191)	(0.140)	(0.168)	(0.179)
Joint Dec. x Shared Liab.		-0.104	-0.111	-0.095	-0.135	-0.083	-0.122
		(0.332)	(0.327)	(0.370)	(0.330)	(0.348)	(0.378)
Risk Attitude			0.104**				0.104**
			(0.043)				(0.045)
Cog. Refl. Test				0.198**			0.177**
				(0.078)			(0.080)
Soc. Val. Ori.					-0.005		-0.005
					(0.004)		(0.004)
Lagged Audit						0.241***	0.243***
						(0.089)	(0.088)
Socio-Eco. Controls	YES	YES	YES	YES	YES	YES	YES
Constant	2.207***	2.179***	1.794***	1.734***	2.235***	2.245***	1.489***
	(0.551)	(0.573)	(0.535)	(0.510)	(0.613)	(0.599)	(0.574)
Observations	2,166	2,166	2,166	2,166	2,166	1,958	1,958
Number of Subjects	268	268	268	268	268	268	268

Notes: The table presents results of a random effects panel probit specification with dishonest declarations (binary variable) as the dependent variable. "Joint Decision" is a dummy variable that indicates treatments with joint decision-making, "Shared Liability" is a dummy variable that indicates treatments with shared payoffs, and "Joint Dec. x Shared Liab." is the interaction between both dummies. The reference group is Treatment 1 with individual payoffs and individual decision-making. "Risk Attitude" refers to the number of risky decisions (0 to 10) in Holt and Laury (2002). "Cog. Refl. Test" is the number of correct answers (0 to 3) in the Cognitve Reflection Test (Frederick 2005). "Soc. Val. Ori." indicates Murphy s et al. (2011) measure of distributional preferences (-16.26 to 63.39) and "Lagged Audit" is a dummy for being audited in the previous period. "Socio-Eco. Controls" include a dummy for female subjects, the age of the subject, a dummy for Economics and Business students and the number of siblings. Standard errors account for clustering at the session level and are given in parentheses,
*** $p < 0.01$, ** $p < 0.05$.

TABLE 4.B: Multivariate Analysis of Dishonest Declarations of the own Income

An analysis of the final declaration of incomes reveals that the fraction of dishonest declarations is almost identical to the fraction of dishonest reports. For treatments with individual decision-making, the fraction of dishonest reports equals the fraction of dishonest declarations by design. For treatments with joint decision-making, the declaration mechanism itself may increase compliance: if subjects disagree and state different reports for an income, the mechanism automatically leads to a truthful declaration. Due to high coordination within teams, the tendency toward compliance is very moderate in size but decreases the effect of joint decision-making even further. Non-compliance for declarations amounts to 67 percent in Treatment 3 and 81 percent in Treatment 4 (as compared to 70 percent and 85 percent dishonest reports, respectively).[21]

In Table 4.B, we show the results of a panel probit regression with dishonest declarations as the dependent variable (coded as a dummy variable, standard errors account for clustering on the session level). The results are broadly in line with our previous analysis of dishonest reports (Table 4.1). The two baseline specifications include dimension dummies (column 1) and an interaction term (column 2). Shared liability significantly increases the probability of a dishonest declaration. In contrast, we find no evidence of an effect of joint decision-making or an interaction between both dimensions. For declaration behavior, the coefficient for joint decision-making is even smaller than for reporting behavior. This is also reflected by the average marginal effects: the effect of shared liability is stable and between 11 to 13 percentage points, while the (non-significant) effect of joint decision-making is only 1–2 percentage points. The alternative specifications confirm the robustness of our findings: specifications in column (3) to column (5) include the post-experimental tests while specification (6) controls for audits in the previous period. Results of a full model with all independent variables is displayed in column (7). As for reporting behavior, more risk-taking and better performance in the Cognitive Reflection Test is correlated to a higher non-compliance for declarations. In contrast, we do not find an effect for social preferences. Finally, being audited in the previous period significantly increases non-compliance. As for the report of the own income, we find no evidence for Hypothesis 4.1, but we confirm Hypothesis 4.2.

[21] A Wilcoxon-Mann-Whitney test confirms the earlier findings for the report of the own income: the differences along the decision-making dimension are not significant (T1 vs. T3 and T2 vs. T4, $p = 0.63$ and $p = 0.54$), while joint liability significantly increases non-compliance (T1 vs. T2 and T3 vs. T4, $p = 0.02$ and $p < 0.01$).

4.C Appendix – Robustness Results for Section 4.4.1

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Joint Decision	0.053	0.055	0.051	0.054	0.057	0.058	0.054
	(0.043)	(0.061)	(0.060)	(0.066)	(0.060)	(0.060)	(0.063)
Shared Liability	0.142***	0.145**	0.135**	0.140*	0.149**	0.147**	0.137**
	(0.042)	(0.066)	(0.065)	(0.072)	(0.066)	(0.065)	(0.069)
Joint Dec. x Shared Liab.		-0.005	-0.002	-0.008	-0.011	-0.004	-0.008
		(0.086)	(0.084)	(0.093)	(0.085)	(0.084)	(0.089)
Risk Attitude			0.031***				0.032***
			(0.009)				(0.008)
Cog. Refl. Test				0.053***			0.044***
				(0.015)			(0.015)
Soc. Val. Ori.					-0.001		-0.001
					(0.001)		(0.001)
Lagged Audit						0.054***	0.054***
						(0.016)	(0.016)
Socio-Eco. Controls	YES	YES	YES	YES	YES	YES	YES
Constant	1.129***	1.127***	1.013***	1.019***	1.137***	1.107***	0.902***
	(0.156)	(0.157)	(0.157)	(0.159)	(0.158)	(0.156)	(0.157)
Observations	2,166	2,166	2,166	2,166	2,166	1,958	1,958

Notes: The table presents results of a multi level mixed effects regression with clustering at the session and at the individual level. The dependent variable is dishonest reporting (binary variable). "Joint Decision" is a dummy variable that indicates treatments with joint decision-making, "Shared Liability" is a dummy variable that indicates treatments with shared payoffs, and "Joint Dec. x Shared Liab." is the interaction between both dummies. The reference group is Treatment 1 with individual payoffs and individual decision-making. "Risk Attitude" refers to the number of risky decisions (0 to 10) in Holt and Laury (2002). "Cog. Refl. Test" is the number of correct answers (0 to 3) in the Cognitve Reflection Test (Frederick 2005). "Soc. Val. Ori." indicates Murphy s et al. (2011) measure of distributional preferences (-16.26 to 63.39) and "Lagged Audit" is a dummy for being audited in the previous period. "Socio-Eco. Controls" include a dummy for female subjects, the age of the subject, a dummy for Economics and Business students and the number of siblings. Standard errors are given in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 4.C: Robustness Analysis of Dishonest Reports of the own Income

In our main analysis (Table 4.1 in section 4.4.1), we use a panel probit regression to account for the binary structure of the reports. To show that the estimation of interaction terms is not biased, Table 4.C reports the results of a multi-level mixed effects regression with clustering on both the session and the individual level. We do not find any major differences to the probit estimation in section 4.4.1. In particular, the coefficient on the interaction term between the decision-making dimension and the liability dimension stays very small and is not significant.

4.D Appendix – Experimental Instructions

This appendix provides an English translation of the instructions for treatment 4 (joint decision-making and shared liability). The experiment was originally conducted in German. Instructions for the other treatments were adapted according the two treatment dimensions.

Instructions

1. General Information

Please read the instructions completely and carefully. A precise understanding of the instructions may allow you to earn more money.

Your anonymity is preserved during the whole experiment. Please note that you are not allowed to communicate with other participants except for usage of the chat window.

During the experiment, it may happen that you have to wait for the experiment to continue. However, we ask you not to use your smartphone during the entire experiment. Anything that distracts your and the other participants' attention from the experiment is prohibited. In case you break these rules, you are excluded from the experiment and you do not receive any payout.

All participants of the experiment received the same instructions.

If you have any questions, please raise your hand. We are going to help you.

2. Introduction

You will repeatedly make decisions in this experiment. Your earnings in this experiment depend on your decisions.

Your earnings in this experiment are denoted in Taler. At the end of the experiment your Talers are converted into Euros and you are paid out by the laboratory. The following exchange rate applies: 50 Talers = 1 Euro. In addition every participant receives a show-up fee of 6 Euro.

Before the experiment starts, you are asked to answer a few questions. They exemplify situations that could arise in the experiment.

The actual experiment consists of 10 rounds. Each round is independent of previous and subsequent rounds. The figure below (Subsection 3.2) illustrates the different steps of each round.

In the aftermath of the experiment, you are asked to answer additional questions and to make additional decisions. The respective instructions will then be displayed.

3. Description of the Experiment

3.1 General Procedure

In this experiment, you and your partner repeatedly take the role of a taxpayer who decides whether to report his actual income or not.

Your income:

- At the beginning of each round, you are asked to count digits on the screen and thereby earn your income.

- Randomly you are either asked to count many digits, and as a consequence you will earn a high income (1000 Talers), or you are asked to count few digits, and therefore you will earn a low income (400 Talers).

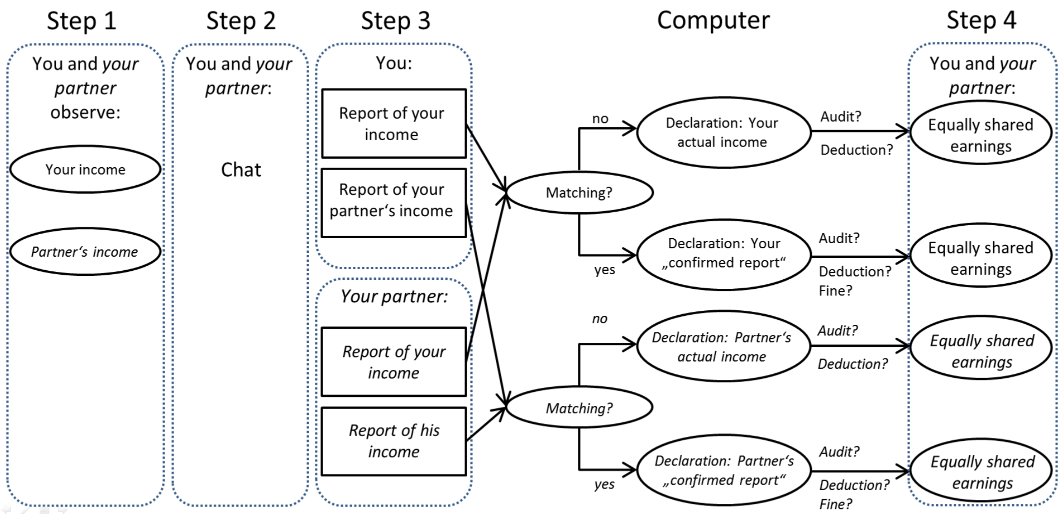
- You can either earn a high income with 80% probability or a low income with 20% probability.

After you have earned your income, you and your partner have to report your income at the computer to the tax authority. The tax authority does not know whether you earned a high or a low income.

For a high income a deduction of 400 Talers is due, which is returned to the laboratory. Whenever a high income is not correctly declared and this is revealed by a subsequent audit, you have to pay a fine of 200 Talers on top of the deduction.

In contrast, a low income is exempt from deduction. Whenever you earned a low income, but instead declare a high income, you have to pay the deduction of 400 Talers. Since declarations of high incomes are not audited, there is no fine in this case.

3.2. Graphical Overview of the Experiment



3.3 Procedure of Income Reporting

At the beginning of each round, another participant who has never been your partner before is randomly matched with you as your team partner. You will make the report of your income jointly with him. Conversely, your partner makes the report of his income jointly with you.

First, you will be informed about the income that you have earned and the income that your partner has earned (Step 1). Subsequently, a chat window will open on screen and you will get the opportunity to chat with your partner for two minutes (Step 2).

After the chat you will submit two reports. For each report, you may either state a high or a low income (Step 3):

a report which income you would like to state for yourself.

- a report which income you would like to state for your partner.

- Conversely, your partner will also make a report of his income and a report of your income.

You and your partner will share the financial consequences of your reports. This means that both of you will receive half of your own and half of your partner's earnings. Consequentially, both partners share deductions and potential fines equally.

The declaration of your income to the tax authority:

- If your report and your partner's report of your income match, this confirmed report is transmitted as the declaration of your income.

- If your report and your partner's report of your income do not match, there is no confirmation. In this case, your actual income is transmitted as the declaration of your income.

- Conversely, the same procedure applies for the declaration of your partner's income.

3.4. Individual Audits and Round's Earnings

After the declaration of incomes, a partial audit is conducted:

- Declarations of a low income are randomly audited with a 50% probability.

- Declarations of a high income are not audited.

Your earnings and your partner's earnings depend on the declarations and the potential audits (Step 4):

- Whenever a declaration is audited and the declaration of a low income is true, neither the deduction nor the fine are due.

- Whenever a declaration is audited and the declaration of a low income is untrue, the deduction of 400 Talers and the fine of 200 Talers are due.

- Whenever a declaration is not audited and the declaration is a low income, neither the deduction nor the fine are due, irrespective of the actual income.

- Whenever the declaration is a high income, the deduction of 400 Talers is due.

- The financial consequences of the respective declaration are equally shared between you and your partner.

At the end of the round, the earnings from the declaration of your income and the earnings from the declaration of your partner's income are shared equally between you and your partner. This determines your individual earnings of the respective round.

Afterwards, the next round starts (Step 1).

4. Concluding remarks

One of the ten rounds is randomly selected by the computer. Your earnings from this selected round determine your earnings (in Talers) of the experiment.

The payout is carried out at the end of the experiment. You will be called up by your seat number by the laboratory staff.

In case a medical or technical emergency occurs during the experiment, please press the F1 button.

Any more questions?

If not, ...

... thank you for your participation in this experiment and good luck!

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