

# Risk, Luck and Deception: Three Essays in Behavioral Economics

By  
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# 1 Introduction

Two of the pillars in the field of behavioral economics which we are concerned with are motivation and rationality. In this thesis, which consists of three papers, I explore both these topics, and each of these papers are motivated by different aspects of money managers' profession; specifically, when they invest, give advice, or their motivation to work. Studying and understanding these aspects is important because money managers and investment decisions can have huge impacts on people's financial situations. In particular I am interested in investigating whether advice given to customers depends on the customer's wealth level. One reason for investigating deception in this setting is that financial advisors often have incentives which are misaligned with those of the customers. In addition, financial advisors do meet many different customers with varying levels of wealth and opportunities. Thus, I find it interesting to investigate this and how different clients affect advice. When it comes to the amount of work that money managers put in, research shows that some are able to create additional value. However, effort is likely not the only cause of this additional value, and for the majority of money managers they are likely paid for luck. So why then do they put in so much effort? Possible explanations include signaling, or they believe it creates value. Lastly, costs can have a huge impact on compound returns. As a result, I find it important to understand how people take these costs into consideration.

I use experiments as a means to identify the causal effects, because this empirical method allows for an unprecedented control over the environment and allows me to change one factor at a time.

As a starting point I will present the concepts central to the three papers in this thesis in the following sections. First, I start with deception, then move on to social preferences, mental accounting, and lastly, luck and motivation.

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## **1.1 Deception and Lying**

Before I start with how standard economics and behavioral economics differ in the way they view lying and deception, I zoom out a bit and look at what constitutes a lie; the Stanford Encyclopedia of Philosophy states that “To lie is to make a believed-false statement to another person with the intention that the other person believe that statement to be true.” (Mahon, 2016). This definition has four necessary conditions for a message to be considered a lie: the statement condition, untruthfulness condition, addressee condition, and lastly the intention to deceive the addressee condition. These necessary conditions require some clarification. The first condition, the statement condition, states that any form of delivering a message, be it spoken language, sign language or a written message, is considered a statement. The second condition requires that the Sender believe the statement to be false, e.g. if a person states something that is objectively true, but the statement is a lie if the Sender believes it is false. The central aspect here is the Sender’s belief regarding the truthfulness of the statement. Thirdly, the addressee condition requires that another person is the receiver of the statement. The fourth and final necessary condition requires that an untruthful message is sent to the other person with the intention that the Receiver believes it is true. One consequence of this is that altruistic lies are not considered lies, e.g. the message is not considered a lie if the Sender believes his message will be inverted and therefore sends an untruthful message to make the Receiver believe the opposite which is true. In addition, due to the word “intention” it is sufficient that the Sender intends to lie, and whether he is successful in lying is not a requirement. A distinction between lying and deception is therefore drawn. Deception depends on the Sender being successful in deceiving the Receiver, and the traditional definition states that “To deceive is to intentionally cause to have a false belief that is known or believed to be false” (Mahon, 2016). This definition, unlike the definition of lying, defines a true message as deception if it is able to instill a false belief in the Receiver,



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given that this was the Sender's intention. Lying is therefore a form of deception. However, not all forms of deception are lies.

According to standard economic theory, agents are self-serving and risk averse. Due to the assumption of self-serving agents, it is costless for agents to lie or deceive. Therefore, in the frame of standard economic theory, agents will only refrain from lying if the chance of being caught is high enough and the downside of being caught is large enough (Crawford & Sobel, 1982).

New evidence shows that lying carries a fixed intrinsic cost (Gneezy, Kajackaite, & Sobel, 2017; Kajackaite & Gneezy, 2015). In other words, there is a difference between the standard economic theory and the empirical findings. The source of this cost is likely morality, because in lab settings where subjects are faced with a one-shot interaction, a large portion of subjects forgo a larger payoff if they have to lie to receive it and instead settle for a smaller payoff. This implies that lying and deception are intrinsically costly to people. In addition, the research of Gneezy et al. (2017) shows that people can be categorized into two types: those who always lie, and those who lie whenever it is beneficial to them. Those who do lie, do so to the fullest. There would be more partial lies if people's cost of lying were variable. The people who lie whenever it is beneficial to them perform a cost-benefit calculation and lie to the full extent when the lie comes out on top. The behavioral economic model deviates from the standard economic model here due to the disutility people experience if they lie or deceive. In some regards this is similar to what the standard economic theory says people do. However, the reasons for refraining from lying are somewhat different and most likely we would see more lying if people were homo economicus, since there would be no intrinsic cost of lying.

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## **1.2 Social Preferences**

Standard economic theory states that people are self-serving, meaning that we are indifferent to the utility of other people. In this framework, people will not give to others unless it gives them increased utility in another way, for example better reputation.

However, a long line of research that started with Kahneman, Knetsch, and Thaler (1986)<sup>1</sup> shows that people have other-regarding preferences, meaning that an agent's utility is in fact affected by others' utility. Two of the most influential papers are Bolton and Ockenfels (2000) and Fehr and Schmidt (1999), which are both outcome-based social preference models. One important contribution was the formalization of inequity aversion. A person with inequity averse preferences experiences a larger disutility if they have less than others compared to if they have more. This theory has led to studies that have shown that some people are willing to pay a cost to reduce other people's rank when wages are flat (Charness, Masclet, & Villeval, 2013) and reject unfair offers in ultimatum games (standard economic theory predicts acceptance of any positive amount).

I investigate social preferences in an advice-giving setting, since a major part of money managers' work is to advise their customers. I wanted to see whether this advice differs depending on the inequity between the advisor and the receiver.

## **1.3 Mental Accounting**

In standard economic theory, people are thought to have no limitations in their cognitive capabilities. As a consequence, they are able to instantly update their beliefs, calculate everything, and are fully aware of all their preferences and what will maximize utility. In addition,

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<sup>1</sup> For a summary of this literature see e.g. D. J. Cooper and Kagel (2016).

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money is completely fungible to people and as a result is allocated to that which maximizes utility. However, according to behavioral economics, people have limits to their cognitive capabilities and use heuristics to alleviate the strain from thinking and to make decisions quickly. When it comes to money, people are thought to allocate budgets to different mental accounts (Thaler, 1985), so they do not have to evaluate all aspects of life. Mental accounting is a concept coined by Richard Thaler, which tries to explain why and how people evaluate, keep track of, and organize financial decisions. For example, when people are deciding whether or not to go to the cinema, they only have to check the balance of the ‘entertainment account’. One implication of this way of organizing budgets is that money is not perfectly fungible, as purchases of similar character are lumped together.

One central aspect of mental accounting is how often accounts are evaluated. As stated by Thaler, “Accounts can be balanced daily, weekly, yearly, and so on, and can be defined narrowly or broadly” (Thaler, 1999, p. 183). How often the account is evaluated can affect whether an account is “in the red” or not.

Another central aspect is the account’s reference point that prospects are evaluated against (Thaler, 1985). This point can be the status quo, one’s current wage, entitlement or brand attributes, to name a few. The reference dependence can manifest itself as the endowment effect, which according to Knetsch, Tang, and Thaler (2001, p. 257) has “...been among the most robust findings of the psychology of decision making”. The endowment effect is where people value an item more simply because they own it. More formally, the willingness to pay is lower than the willingness to accept.

Although there are some challenges to mental accounting, it has given valuable insights into people’s behavior (see e.g. Grinblatt & Han, 2005; Hossain & Morgan, 2006; Thaler, 2016).

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## **1.4 Lucky Motivation**

Luck is how we speak about the outcome of a random process, often a gamble. If you get the upside you are lucky, but if you get the downside you are unlucky. Here I examine how people attribute outcomes to luck or skill. No one is luckier than others, because probability affects us all equally. In turn, what governs people's attitude towards luck in the standard economic framework is their risk preference, and people correctly attribute the outcome to risk. Therefore, people do not receive any additional utility from being lucky, and no disutility from being unlucky, because all of these aspects are internalized, known, and evaluated before people take part in the gamble. Gambles can take many forms, e.g. playing the lottery, crossing the road, or investing in stocks, each with its own associated risk.

Behavioral economics has a different approach to luck and how people relate to it. At the foundation we have prospect theory, where people attribute weights to probabilities and use heuristics which can lead to different biases. One of the more relevant biases for this context is the illusion of control; as the word implies, people believe they have more control over outcomes than they actually do. Another closely related bias is the attribution bias, where people mistakenly attribute downside outcomes to bad luck and upside outcomes to skill. The attribution bias can skew the feedback people receive, which can make it harder to de-bias the illusion of control bias. One profession where this is perhaps more salient than for other professions is money managers. According to the efficient market hypothesis, stocks follow a random walk; money managers are therefore 'paid for luck' (Bhootraa, Dreznerb, Schwarzc, & Stohsd, 2015; Fama & French, 2010; Malkiel & Fama, 1970; Pástor, Stambaugh, & Taylor, 2017). However, I believe that if you ask a money manager, they will tell you that results are due to skill, despite the efficient market hypothesis being taught in all introductory finance courses. In addition, there is anecdotal evidence that money managers work long and hard hours (Michel, 2014); despite that, on average they

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would perform just as well by throwing darts at a list of stocks. Moreover, a study using a gift-exchange game saw decreasing efforts and rewards when signals became noisier (Rubin & Sheremeta, 2015). The way employers in the same study dealt with the introduction of noisier signals was to increase their use of fixed pay and reduce the use of performance-based pay. It appears that when luck is a known component of the output, employers do not reward workers for it. So why do we not see more fixed pay used for money managers' compensation?

There is evidence that some money managers are able to outperform their reference index (Bhootraa et al., 2015; Pástor et al., 2017), but little evidence supports the notion that effort is the cause of this. One explanation of the high effort of money managers could be that when signals become noisier and the link between effort and outcome is unclear, workers increase their effort to signal their moral type (Sloof & van Praag, 2010). Workers then increase their effort in the hope that the employer will infer and reward their intentions (Rand, Fudenberg, & Dreber, 2015). The worker may also be motivated to work hard if they expect their manager to adhere to a social norm of hard work and if the manager holds some power over their compensation. Under such conditions the worker may expect that effort will be rewarded, even if the role of luck and effort is common knowledge. This latter point is related to virtue ethics.

Many of the papers I have mentioned have used economic experiments as a way of gathering data, and this is also the method that I use in the papers in this dissertation. I therefore discuss in the following section what we can learn from economic experiments, some critiques against experiments, and whether these critiques are valid.

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## **1.5 Economic experiments and their ability to reveal causal effects**

In the section above, I have listed several factors that have previously been investigated in behavioral economics. One commonality among many of the behavioral economic studies is the use of lab experiments to test the prevalence of effects. One reason is that the experimenter has a high degree of control in lab experiments, which in turn means that causality can be identified.

In the following section, I discuss what causality is and in greater detail how lab experiments can reveal causal effects. I then point out some of the criticisms of lab experiments and their defense.

### **1.5.1 What is causality?**

Causality is defined in the Oxford Dictionary (2018) as the relationship between cause and effect. The cause must also precede the effect and there must exist a direct path between the cause and effect in every minimal underlying structure (Pearl, 2009). To fulfill the requirements of direct path and minimal underlying structure, one has to know in advance all the causal relevant factors. A causal structure can then be described, which shows how all of the variables are influenced; this forms the basis for the causal model. From the causal model we can determine and inspect a subset of observed variables which we in turn can use to infer causality. These subsets of observed variables of interest are most often found in theory. Theory also gives predictions about what kind of effect will occur and which direction it will take.

We run into some problems if we do not know every possible variable that could potentially have an effect. One solution to this problem is to do a *ceteris paribus* comparison (Levitt & List, 2007b). To do this, we need to hold all variables constant and change one variable at a time. The causal effect, or treatment effect as it is often called in economic

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literature, will be the difference between outcome for the treated and outcome for the non-treated (this is often called the control group) (Falk & Heckman, 2009). Because people's preferences and personalities can vary a lot, we run into potential challenges. Some people have self-interested, wealth maximizing preferences while others have preferences that are more philanthropic, as discussed above in the social preference section. If we have an unbalanced sample in one of the treatments, a sample with too many subjects with one distinct set of preferences can give a wrong estimate of the causal effect. So how can we protect ourselves from these variances in preferences and personalities? We can use randomization of subjects into treatments. When we randomize subjects into treatments, we do not hold everything else equal, but we achieve independence between the treatment variable and the potential outcome (Angrist & Pischke, 2008, 2014).

### *1.5.2 What is an Economic Experiment?*

Economic experiments are concerned with three things: the environment, institution, and observed behavior given the environment and institution. The environment and institution set the framework and rules that are allowed in the experiment; the economic experiment can therefore be viewed as a self-contained economy. The environment consists of agents, endowment of resources, information, and preference over outcomes. The institution governs which actions are allowed among the agents; it also contains a choice set for each agent and an outcome function which is contingent upon the choices made. For predicting and analyzing outcomes, the theoretical framework is used. This is done by having a set of assumptions and investigating if one can observe behavior supporting these predictions in the lab. The use of a theoretical framework enables replicability and also allows for comparative statistics (Cassar & Friedman, 2004). Another thing supporting replicability is the strict protocol regarding instructions. By having clear

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instructions in writing, all the sessions within the same treatment receive the same instructions.

To conduct an economic experiment, subjects must first be recruited and put in a controllable environment, then provided the desired choice set where the outcome function is enforced. What can be tricky to control is the endowment of preferences; here the induced value theory enters the picture. Induced value theory (Smith, 1989) allows the researcher "...to induce pre-specified characteristics in the subject so that their innate characteristics become irrelevant" (Cassar & Friedman, 2004, p. 26). Three conditions must be met to achieve this: monotonicity, salience, and dominance. Money is often used to satisfy all of these conditions, as more money is better (monotonicity) and it is something the subject cares about. Saliency is achieved if there is a clear link between choices made and what is rewarded. To overcome dominance, increments in the reward system have to be large enough so that they become more important than other utility yielding aspects relevant to the experiment. Privacy can help in this setting, because when subjects perform a task in private they are not being judged by other participants.

### *1.5.3 Critique against Economic Experiments*

When discussing my research with others, I have received some pushback as to the validity of economic experiments. Perhaps the most common one is the lack of realism critique. As a framework of critique against economic experiments, I use the papers of John List and Steven D. Levitt (Levitt & List, 2007a, 2007b). In these papers, Levitt and List raise some concerns with using economic experiments and generalizing the findings to the "real world", making the claim that generalizability is important for experiments. Levitt and List are critical to the use of economic experiments because they often use students as subjects and that behavior is affected by at least these five factors (Levitt & List, 2007a, 2007b): presence of moral and ethical consideration, nature of extent of scrutiny of one's action by others, the context in which a



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decision is embedded, self-selection of individuals making the decisions, and the stakes of the game.

### **1.5.3.1 Presence of moral and ethical consideration**

This concern is split into three aspects of moral determinants: financial externality that an action imposes on others, the set of social norms or legal rules that govern behavior in a particular society, and moral concerns depending on the nature and extent of how an individual's actions are scrutinized. These moral determinants affect how a person considers a choice and can be hard to mimic and capture in the lab. If the subjects subscribe to different norms, the researcher could get imprecise measurements.

### **1.5.3.2 Nature and extent of scrutiny of one's action by others**

The activity of being observed can lead some subjects to behave differently than if they make decisions in private. Some subjects will also try to guess what the researcher's hypothesis is and act according to the hypothesis; this is known as the demand effect. For example: if the subjects are faced with a dictator game, the dictators could guess that the researcher is interested in measuring altruism and give more than they would "in the real world". This would exaggerate the altruism measurement and not be a good basis for making inferences about the "real world"; it would instead measure the effect of monitoring. Some also raise concern with the "Hawthorne effect"<sup>2</sup> where people alter behavior just because they know they are being observed.

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<sup>2</sup> The Hawthorne effect is when people who participate in an experiment change their behavior because they are being observed and not necessarily because they are in the treatment.

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### **1.5.3.3 The context in which a decision is embedded**

No matter how abstract the task in the experiment is, the researcher cannot completely control how the subjects perceive it and what associations subjects will make. A one-shot game could be played as a repetitive game, or subjects may not believe that they are anonymous. Both these potential situations would lead to imprecise or wrongful estimates.

### **1.5.3.4 Self-selection of the individuals making the decisions**

One major concern is that there is a subject pool bias for economic experiments where the subject pool is populated with subjects that do not represent the general population. Students used as subjects in the majority of economic experiments often have different characteristics than the rest of the population. Namely, students have higher education, lower chronological age, and higher occupational status (Doty & Silverthorne, 1975). Students also lack the experience that professional agents present in the relevant market possess; this could be stockbrokers if one is interested in trading behavior, or car mechanics if one is interested in credence goods. This lack of experience could potentially lead to behaviors that are systematically different from the professionals. Economic experiments that use students as subjects would thus tell us little about how the professional actors behave, making it hard if not impossible to make any inferences about the “real world”.

### **1.5.3.5 The stakes of the game**

Typically in an economic experiment, subjects make choices with relatively small sums of money. This allows the researcher to get many observations on a limited budget. There are many real-life situations where we make choices with equal sums of money, but we are also faced with choices with much larger sums of money. Thus, it is important to take the stakes into consideration when performing the analysis.

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All of these critiques and concerns about economic experiments must be taken seriously, as we could potentially get effect estimates that are loosely rooted in reality and would serve us poorly when making inferences about “the real world”. Another critique is that economic experiments have an artificially short time span and few choices. Understandably, there will be limitations to what one can learn from a simple gift exchange game<sup>3</sup> if one is interested in long term employer-employee relationships.

#### ***1.5.4 Is the critique against Economic Experiments of real concern?***

To decide whether the concerns by Levitt and List detailed above are of concern or not, we must take the goal of the economic experiment into account. If the goal is to elicit preferences about a specific group in society, we may learn little about this group’s preferences based on an economic experiment that uses students as subjects. In addition, we can learn even less from the economic experiment if the students are systematically different from the group of interest. However, as stated by Colin Camerer (Camerer, 2011), the primary objective of most economic experiments is not to make generalizations from lab to field. Rather, the objective is to establish a general theory that can be linked to economic factors, e.g. incentives, rules, and norms of behavior. With this in mind, are the critiques listed above still of concern? Let us take a closer look at what the literature tells us.

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<sup>3</sup> In the gift exchange game there are two players: employers who represent the firm, and workers who are self-interested utility maximizers. The firm first commits to a wage level, then the workers can commit to a costly effort level which earns the firm profit.

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#### **1.5.4.1 Presence of moral and ethical consideration – answer to critique**

Early studies which used the dictator game did show a larger share of giving than what is gifted to charity. Therefore, it is not surprising that people have raised their eyebrows when presented with these facts. However, these two types of giving are not directly comparable; what is gifted to charity is people's hard-earned money, while the money subjects give in experiments is what is known as "house money"<sup>4</sup>. As Camerer (2011) writes:

...the extreme control in the lab suggests it is an ideal setting in which to learn about influences on sharing. The nature of entitlements, deservingness, stakes and obtrusiveness can all be controlled much more carefully than in most field settings (Camerer, 2011, p. 16).

#### **1.5.4.2 Nature and extent of scrutiny of one's action by others – answer to critique**

As in many other situations in life, actions and choices in the lab are subject to scrutiny and obtrusiveness. To determine if scrutiny in the lab leads to different behavior than outside the lab, we must compare situations with the same level of obtrusiveness and scrutiny. As mentioned above, the scrutiny can manifest itself as a demand effect where subjects try to act according to what they believe is the researcher's hypothesis. This effect can be mitigated by the use of instructions that clearly instruct the subjects on the rules of the game. Furthermore, those who are concerned with the "Hawthorne effect" should know that reanalysis of the data shows no such effect (Jones, 1992).

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<sup>4</sup> Derived from gambling when a gambler has won money and is gambling with this money.

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#### **1.5.4.3 The context in which a decision is embedded – answer to critique**

If researchers are concerned with controlling for context in decision making, then the lab is one's best bet because in no other place will researchers have the same level of control.

Economic models are also abstracted from the real world. Much of the reason for this is for the sake of simplicity, since the simplicity helps to make interaction between factors more salient. Deduction does not allow us to draw a conclusion based on results, however induction does. If the underlying assumption made by the theory is unchanged, we can expect to see the same behavior in the “real world” as in the laboratory. If a theory is not true in the laboratory but is assumed to be true in the “real world”, one should reevaluate the theory (Falk & Fehr, 2003). We can say this because a lab experiment will have control over the relevant parameters of the theory, and when results do not find any causal effect the theory has to be mis-specified.

#### **1.5.4.4 Self-selection of the individuals making the decisions – answer to critique**

Seeing how inexperienced agents act in a new market is in itself interesting and not a weakness with experiments, because it can give valuable insight into how agents learn. Students know that as participants they can often earn a decent hourly wage by participating in an economic experiment, and as Camerer (2011) notes, “In schools with active economic labs, subjects do see themselves as “market participants” whose traits allow them to excel in the marketplace” (p. 23). In this example it is better to look at participants as workers rather than students.

#### **1.5.4.5 The stakes of the game – answer to critique**

Several studies have shown that stakes do not have as much to say as critiques claim, since the same pattern is shown in economic experiments

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with low wages as in economic experiments in developing countries where several months' worth of wages are at stake. Other meta-studies have found that an increase in stakes leads to less noise in the data and people show more rational behavior (Camerer & Hogarth, 1999; Smith & Walker, 1993).

Overall, many of the concerns about economic experiments raised by Levitt and List are not as severe as one would initially think. To quote Falk and Heckman: "Ironically, most objections (concerning lab evidence) raise questions that can be very well analyzed with lab experiments, suggesting the wisdom of conducting more lab experiments, not fewer." (Falk & Heckman, 2009, p. 537). We should also note that students are real people that have real preferences and experience emotions just as any other person out in the field.

### *1.5.5 Replicability*

In section 1.5.2, where I explain what an economic experiment is, I mention that clear instructions are given to subjects in writing. By using these instructions, other research groups can easily replicate the studies. By replicating experiments, we can be more confident that findings are true. As in all research that relies on analyzing data, there is always a chance of committing a type I error; to wrongfully reject the null hypothesis, believing there is a significant treatment effect. There is also possibility, due to weak statistical power, to fail to discover a true relationship. If we can discover the same effect in multiple experiments, our confidence in uncovering a true causal relationship can increase.

A replication study performed by Camerer et al. (2016) shows just how important it is to conduct replication studies. They found that in the replicated studies, 61% of the effects were in the same direction as the original study, and the average effect size was 66% of what the original study reported. When looking at these numbers, it is clear that far more replications should be conducted; only then will we be able to determine

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what is statistical noise and what is the true causal relationship. Some of the problem of low replicability can be attributed to the publication bias; it is very hard to publish null findings in reputable journals, which gives “perverse incentives”. If a researcher knows that they will have a hard time publishing a null finding, they might spend more time running pilot studies, and sometimes these are not reported in order to tweak inputs to fit hypotheses. Alternatively, the researcher might perform some p-hacking, which is a process where one tests many relationships in the data to look for p-values less than 0.05, and find hypotheses that fit the data and not the other way around (Ioannidis, 2005).

It can be shown that as the number of research groups investigating the same phenomenon increases, “the probability that an *initial* declared research finding is true decreases” (Maniadis, Tufano, & List, 2014, p. 285). Therefore, if we can increase the number of replications and test the same hypothesis through different designs, and if the research finding still holds up, then we can be more confident that it is a true causal effect. In one of my experiments I replicate one of the treatments in Gneezy (2005) and find, almost on the decimal, the same proportion as he does. This therefore adds to my confidence that the finding is correct.

### ***1.5.6 Discussion – What can we learn from Economic Experiments?***

There will in most cases be some degree of uncertainty, whether the mechanism is the true causal mechanism or not. However, I would argue that economic experiments are a very good alternative that is both robust and the result is intuitive to interpret if done correctly. Within the natural sciences, experiments are considered the gold standard for uncovering natural laws that are valid outside the lab. Is it unreasonable to assume this for the social sciences as well? Economic experiments have a high degree of internal validity if the economic experiment is conducted in a satisfactory way, to include: clear instructions, subjects see a connection between action and reward, subjects are randomized into treatments,

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only one variable is changed at a time, and subjects are not deceived. When conducting experiments and making sure they meet the requirements just listed, the researcher can be sure that the observed difference in the outcome variable is due to the treatment, and not any other unobserved variable.

As mentioned in the defense of the critique against economic experiments in section 1.5.4, the main objective of most experiments is to establish a general theory that can be linked to economic factors, e.g. incentives, rules and norms, and observed behavior (Camerer, 2011). The general economic experiment does not promise to provide data that can be generalized to a wide variety of “real world” settings. It is much more important that economic experiments have a high degree of internal validity, so that the measurement of the effect is accurate and true within its own “universe”. However, if we encounter a setting in the “real world” that has the same conditions as the experiment, we can be confident that we will observe the same behavior in the field as in the lab.

A study conducted by B. J. Cooper, Kagel, Lo, and Gu (1999) showed that different subject pools behave in much the same way after some time and their behavior is not systematically different. It is therefore possible, to some degree, to generalize to other subpopulations based on findings from economic experiments that make use of student samples. In addition, if the experiment has been replicated numerous times where the same effect is found, then researchers can be confident that a true and causal relationship exists.

Overall, my perception of economic lab experiments is that they are a good and viable method to elicit preferences and uncover causal relationships. They allow the researcher to have an unprecedented degree of control over the environment. The addition of how replicable experiments are adds to my confidence in economic lab experiments. Although one should be careful when making some generalizations on a



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population based on a small sample, we can still learn valuable information about behavior and preferences that is important when developing theories.

## **1.6 Summary of the Three Essays**

In the first paper I investigate by use of a controlled lab experiment, if and how deception is affected by inequity in payoff opportunities. Subjects play both a dictator game and a cheap talk sender-receiver game in which the receivers' payoff opportunities vary and are either worse, the same, or better payoff opportunities than those of the sender. I find, not surprisingly, that the level of deception is highest when receivers have better payoff opportunities than the senders. However, the senders' lying aversion, as measured by the difference in behavior in the cheap talk sender-receiver game and the dictator game, is also higher when the receivers have better payoff opportunities. In contrast, lying aversion is not present when receivers have worse payoff opportunities than the senders. This indicates that it may be more costly for senders to deceive those who have more, than those who have less.

In the second paper, we investigate whether mental accounting affects decisions when buying risky prospects. We do this because an increasing number of people invest in actively managed mutual funds, despite the lack of evidence for these funds' ability to deliver returns above the index. These funds have higher fees than index funds that yield the same return as the underlying index. We predict that when making their investment decisions, people ignore fees through mental accounting in that the fee is segregated from the other attributes of the investment. We run an experiment on the online labor market Amazon mechanical turk (Mturk) to investigate this prediction. We do not find support for our main prediction, as our subjects act in accordance with standard economic theory and take the fee into consideration. We find that subjects take the same amount of risk and choose lotteries with the same

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after-fee expected return as subjects who do not have to pay a fee. In addition, how the fee is presented does not affect behavior at all.

In the third paper we look at motivation when the outcome is unaffected by effort, because in some jobs the correlation between effort and output is almost zero. For instance, many money managers are primarily paid for luck due to stocks following a random walk. Through the use of a controlled lab experiment, we investigate under which conditions workers are willing to put in effort even if output (and thus employers' earnings) is determined by pure luck. We vary whether the employer can observe the workers' effort, and whether the employer knows that earnings are determined by luck. We find that workers believe that the employer will reward effort even if effort does not affect earnings. Consequently, workers work harder if the employer can observe their (unproductive) effort. Moreover, we find that if the employer only sees earnings and not effort, workers work harder if the employer does not know that earnings are determined by luck. The latter effect is driven by female workers and suggests that (female) workers work hard in order to avoid undeserved rewards.

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## **2 Is Deception Affected By Inequity?**

By Bjørnar Laurila\*

### **2.1 Abstract**

This paper investigates, by use of a controlled lab experiment, if and how deception is affected by inequity in payoff opportunities. Subjects play both a dictator game and a cheap talk sender-receiver game in which the receivers' payoff opportunities vary. I find, not surprisingly, that the level of deception is highest when receivers have higher payoff opportunities than the senders. However, the senders' lying aversion, as measured by the difference in behaviour in the cheap talk sender-receiver game and the dictator game, is also higher when the receivers have higher payoff opportunities. In contrast, lying aversion is not present when receivers have lower payoff opportunities than the senders. This indicates that it may be more costly for senders to deceive those who have more, than those who have less.

### **2.2 Introduction**

In some occupations where customers rely on advice, advisors can be faced with the decision of whether or not to deceive. The advisors can give good advice which is in the customer's best interest, but not maximize their own earnings, or they can deceive the customer to maximize their own earnings. These advisors also meet people from different walks of life with more wealth, but also less wealth than what they have themselves. Do advisors behave differently, depending on who they are advising? We can think of three stylized cases which represent the three possible inequities: In the first situation, the customer always

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\* I am grateful to participants at the Rady School of Management seminar and especially to Uri Gneezy for helpful comments to this paper.

has less wealth than the advisor, in the second case they both have equal amounts of wealth, and in the third case the customer always has more wealth than the advisor. What is interesting in this stylized setting is to see if the advisors will deceive more or less, depending on the inequities.

In this paper, through the use of a controlled laboratory experiment, I investigate whether the decision to deceive is affected by differences in payoff opportunities.

Many philosophers have come up with and used different definitions of deception. In this paper I use the definition that is, according to the Stanford Encyclopedia of Philosophy, the most widely accepted definition. This definition states that “To deceive is to intentionally cause to have a false belief that is known or believed to be false” (Mahon, 2016)<sup>5</sup>. Consequently, it is possible to deceive by telling the truth if it instills a false belief in the Receiver<sup>6</sup>.

According to standard economic theory, lying and by extension, deception, is costless to people who will only refrain from lying if the chance of being caught is high enough and the downside of being caught is large enough (Crawford & Sobel, 1982). New evidence shows that lying<sup>7</sup> also carries an intrinsic convex (Fischbacher & Föllmi-Heusi, 2013; Mazar, Amir, & Ariely, 2008) or fixed cost (Gneezy, Kajackaite, & Sobel, 2017; Kajackaite & Gneezy, 2015). This intrinsic cost of lying likely stems from morality, because the studies which have investigated deception have used one-shot interactions. In these games, reputation does not play a part and still a sizable portion of subjects forgo the larger payment because they have to lie to receive it. Instead, they settle for a

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<sup>5</sup> Deception is a success term and in the context of this paper it is more correct to talk about intention to deceive. However, for ease of language and to not put too much emphasis on the Sender’s belief, I use the term deception throughout this paper.

<sup>6</sup> When referring to previous studies, I use the terminology of the relevant study.

<sup>7</sup> Gneezy (2005) uses a stricter definition of lying in his papers than I do, and therefore uses lying in some cases where I will call it deception.



lower payment which they do not have to lie for to receive. This implies that people dislike lying and deception and that it is intrinsically costly to them. The research of Gneezy et al. (2017) showed that when controlling for the probability of being caught, the pattern of a convex cost of lying (lie more in low stake situations than in high stake situations) disappeared. This favors a fixed cost of lying because when people lied, they did so to the fullest. This means that when the benefit of lying exceeds the cost of lying, people will go all in with the lie. People can be categorized into two types using the fixed intrinsic cost: those with an infinite cost of lying and those with zero cost of lying. Those with an infinite cost of lying will never lie, even if lying helps themselves and others (Erat & Gneezy, 2012). People with a zero cost of lying lie whenever it is beneficial to them, and when they lie they do so to the fullest. This intrinsic cost of lying is part of a cost-benefit analysis regarding whether or not to lie.

Deception often creates some sort of inequity because a usual goal of deception is to increase your own payoff, and it can come at others' expense. It is important to look at how deception is affected by inequity in payoff opportunities, due to the impact deception can have on others' payoff. A long line of research that started with Kahneman, Knetsch, and Thaler (1986)<sup>8</sup> shows that people have social preferences, meaning that a person's utility is affected by other people's outcome. Two important contributions show that the acceptance of differences is asymmetric (Bolton & Ockenfels, 2000; Fehr & Schmidt, 1999). In other words, people experience a larger disutility if they have less than others compared to if they have more than others. Moreover, if this disutility is large enough it can lead people to give up some of their own payoff in order to reduce the disutility from the difference in payoffs.

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<sup>8</sup> For a summary of this literature, see e.g. Cooper and Kagel (2016).

Deception is one of many things that can lead to inequity. In Dictator Games the average transferred amount is 28% of the endowment (Engel, 2011) and the dictators still keep the majority for themselves, even when the receivers are seen as deserving, (Cappelen, Moene, Sorensen, & Tungodden, 2008). If people really disliked differences in payoffs, there would only be 50/50 splits of the endowment between the two players. From this we can infer that people have a preference for and will create inequity if given the chance.

A few papers control for social preferences when investigating cost of lying, and find social preferences and cost of lying are connected and move in the same direction. More specifically, more lying is associated with negative social preferences and less lying with pro-social preferences (Cappelen, Sørensen, & Tungodden, 2013; Maggian & Villeval, 2016). One explanation is that the preference for fairness crowds out the preference for lying (Hurkens & Kartik, 2009). A second explanation is that experiencing the breaking of a norm justifies breaking of another norm (Houser, Vetter, & Winter, 2012). As an example, we can imagine a scenario where there is a norm favoring equity and another against lying. If the equity norm is violated, then it becomes more acceptable to violate the lying norm. A contradictory explanation states that lying is not connected to social preferences. Rather, people make a cost-benefit analysis on whether or not to lie (Kajackaite & Gneezy, 2015). In the latter explanation, people are said to be of a type with fixed cost associated with lying, and whenever this cost is smaller than the benefit from lying the person will lie. The probability of being caught, the potential benefit gained by lying, how large the lie is, guilt and social identity also play part in this cost-benefit analysis<sup>9</sup>.

A couple of different methods have been used when these other studies (Cappelen et al., 2013; Gino & Pierce, 2009; Houser et al., 2012;

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<sup>9</sup> People gain utility from being perceived as honest (Gneezy et al., 2017).

Hurkens & Kartik, 2009; Kajackaite & Gneezy, 2015; Maggian & Villeval, 2016) have investigated lying and used social preferences as a control. They either first let subjects play a game where lying can be measured, then play a Dictator Game to make inferences about the subject's social preferences (Cappelen et al., 2013; Maggian & Villeval, 2016). This method measures how lying and social preferences correlate. Kajackaite and Gneezy (2015) used another method and ran a treatment where lying had a direct impact on another player's payoff. However, the lie was not directed at this other player but was instead directed at the experimenter. The game in this experiment was to think of a number between 1 and 6, roll a die, and say if the die had the same number of eyes as you had thought of. If you answered "yes" the payoff went to you, and if you answered "no" then the payoff went to the other player. Kajackaite and Gneezy (2015) found that the introduction of this other player and by extension social preferences do not affect lying. Gino and Pierce (2009) have a somewhat similar approach as Kajackaite and Gneezy (2015) with respect to who the lie is directed towards. However, Gino and Pierce (2009) created inequity between two players (Solver and Grader). In this paper the lie is from the Grader to the Experimenter about the Solver's performance in a real effort task, and in addition the subjects are not anonymous. Gino and Pierce (2009) find that inequity between the two players leads to dishonest behavior.

These contradictory findings about and approaches to control for the connection between lying aversion and social preferences lead to the question: Is deception affected by inequity in payoff opportunities? To my knowledge there are yet no studies that investigate a situation where a subject lies to another subject and is faced with payoff opportunities where one of the subjects either always earns more (or less) than the other. I use a design that combines both the possibility to deceive and differences in payoff opportunities. The design uses a Cheap Talk Sender-Receiver Game where the Sender has to deceive the Receiver directly. The treatments are constructed such that the two players have

asymmetric and unequal payoff opportunities in two treatments, and equal but asymmetric payoff opportunities in the third. I also run a Dictator Game with the same treatments to control for preferences over the payoff distribution. What is more, by using the Dictator Game I get an upper bound estimate of the magnitude of lying aversion by examining the differences between the two games.

The main findings from the experiment indicate that deception are affected by inequity, but only a certain degree. There are more Senders who deceive when they are disadvantaged by having worse payoff opportunities than the Receiver. However, there are not fewer Senders who deceive when the picture is reversed; both cases compared to when both have the same, but asymmetric payoff opportunities. In addition, there is no lying aversion preset when Senders have better payoff opportunities than the Receivers. However, lying aversion is present when Senders have equal or worse payoff opportunities. This contradicts the findings of Cappelen et al. (2013), who found less lying for subjects with positive social preferences, and Kajackaite and Gneezy (2015), who found no relationship between social preferences and lying. Lastly, Senders deceive more when they have worse payoff opportunities, but deception is also most costly in this case.

### **2.3 Related Literature**

To my knowledge, the first paper which identifies cost of lying is the Gneezy (2005) paper. This paper uses a binary Cheap Talk Sender-Receiver Game<sup>10</sup> and a binary Dictator Game. The Dictator Game is used to control for the preferences over the payoff opportunities. Gneezy shows that people have an intrinsic cost of lying, consider the harm caused to others - relative to their own gain from lying, and that people are sensitive to the size of the benefit. In this type of design, the Sender's

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<sup>10</sup> This design will be explained in more detail in the next section, as this paper uses this design.

belief about the Receiver is something to be aware of as shown by Sutter (2009), who uses the same Cheap Talk Sender-Receiver Game as Gneezy (2005). Sutter shows that some Senders in a binary Cheap Talk Sender – Receiver game send the truthful message but do not expect the message to be followed or expect it to be inverted, in effect deceiving using a true message.

A different approach to measure lying comes from Fischbacher and Föllmi-Heusi (2013). In their cheating game, participants roll a die in private, report the number of eyes and are paid accordingly, except when rolling a six which pays zero. This design eliminates strategic considerations, but it is not able to identify lying on an individual level, only at an aggregated level. Fischbacher and Föllmi-Heusi (2013) find support for a convex cost of lying because subjects do not lie to the fullest. Lying cost with a convex shape is disputed by Kajackaite and Gneezy (2015), who argue that subjects do not lie to the fullest due to a fear of being caught and not because of the shape of the lying cost. They show that subjects lie to the fullest and behave according to a fixed cost of lying when removing the chance of being caught. A field experiment by Abeler, Becker, and Falk (2014) which is related to Fischbacher and Föllmi-Heusi (2013) uses the flip of a coin and finds that among the general population in Germany, lying aversion is large and widespread.

Gneezy, Rockenbach, and Serra-Garcia (2013) combine the Cheap Talk Sender-Receiver Game and the cheating game. Here, the Sender observes the state of the word, 1 - 6, and sends a message regarding this to the Receiver. The Receiver then has to choose a number and can follow the message or not. The Sender's payment is dependent on the message he sends and 6 pays more than 1. The Receiver is paid if she follows a true message, paid less if she does not follow the message, and paid nothing if she follows an untrue message. Gneezy et al. (2013) identify three types of participants: those who never lie, those who sometimes lie (when benefits are large enough), and those who always lie.

An alternative explanation to lying observed in the Cheap Talk Sender-Receiver Games is guilt aversion (Battigalli & Dufwenberg, 2007; Charness & Dufwenberg, 2006). According to this theory, people strive to meet others' expectation in order to avoid guilt, and lying in itself does not cause disutility. Actions are therefore based on beliefs and second order belief instead of individual preferences for lying. Another aspect of this theory is that context is important when analyzing lying, because if lying is expected in a given situation, it is not (as) costly compared to a situation where honesty is expected (Charness & Dufwenberg, 2010).

## **2.4 Design**

I use the Cheap Talk Sender-Receiver Game from Gneezy (2005) and have treatments with different inequities in order to investigate if the decision to lie is affected by the Receiver's relatively higher or lower payoff opportunities. In the Cheap Talk Game, the Sender has private information about the payoff structure and has to send one of two messages concerning which option the Receiver should choose. The Receiver, upon receiving the message, chooses one out of two options that determine both players' payoffs. The Sender can choose between the following two messages:

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

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

For the Receiver, option A always yields a higher payoff. However, for the Sender option B always yields the highest payoff. Senders are asked which option they expect the Receiver to choose in order to control for strategic behavior such as telling the truth, but expecting option B to be chosen. Senders are also asked how many out of 100 Receivers follow the message from the Sender.

When considering the design of Gneezy (2005), lying and deception can only be identified by the Sender's action while the intention behind the

message is not known. As pointed out by Sutter (2009), some Senders might deceive by telling the truth. His approach of asking the Senders which option they believe the Receiver will choose will serve as a proxy for the Senders' intent. The additional question that Sutter asks his Senders (how many out of 100 Receivers follow the message) serves as an indicator of the Senders' second order belief about Receivers' trust in Senders. Assuming that Senders are honest when answering the elicitation questions, Sutter classifies four Sender types. The Benevolent Truth-Teller sends the truthful message and expects it to be followed by the Receiver. The Sophisticated Truth-Teller sends the truthful message, but expects the Receiver to choose the other option. The Benevolent Liar sends the untruthful message, but expects the Receiver to choose the other option. The Liar both sends the untruthful message and expects it to be followed. This is summarized in Table 1 Types of Senders.

Table 1 Types of Senders

		Expects option	
		A	B
Sends message	A	Benevolent truth-teller	Sophisticated truth-teller
	B	Benevolent liar	Liar

Note: The table shows the classification of Senders, based on the message they send and what option they expect the Receiver to choose.

In this experiment I use Sutter's classification of Sender types and his elicitation questions. Therefore, the main emphasis of the analysis is on deception (Senders who expect option B to be chosen by Receivers, regardless of the message received). When I make comparisons to Gneezy (2005), I use his definition of deception which only uses the message for classification (Senders who choose to send message B). Gneezy's definition also serves as a conservative measurement of deception, as it does not rely on the Senders' stated expectations, only their behavior.

There are three treatments in the design and the only thing that changes across these treatments is the Receiver's payoff opportunities. The

Sender's payoff opportunities and therefore the gain from deception are held constant across treatments. The harm to the Receiver (in absolute terms) is also constant across the three treatments. This allows me to investigate how different inequities affect deception behavior. In *Poor* the Receiver always gets less than the Sender, regardless of the option chosen. *Equal* offers the same payoff opportunities to the two players, but depending on the option, one player will receive more than the other. In *Rich*, the Receiver gets more than the Sender regardless of the option chosen. Table 2 summarizes the payoffs to Senders and Receivers in the different treatments.

Table 2 Payoffs

Treatments	Option	Payoff to <sup>11</sup>	
		Sender	Receiver
<b>Poor</b>	A	50	30
	B	60	20
<b>Equal</b>	A	50	60
	B	60	50
<b>Rich</b>	A	50	90
	B	60	80

### 2.4.1 Dictator Game

The preferences over the payoff distributions are controlled with a Dictator Game with the same treatments as the Cheap Talk Sender-Receiver Game. I cannot identify what is due to disutility from deception and what is due to Senders' social preferences, if this is not controlled for. As in the Cheap Talk Sender-Receiver Game, the Dictator has private information about the payoff opportunities. In addition, based on

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<sup>11</sup>The payoffs were listed in NOK in the experiment, at the time 1 USD  $\approx$  8 NOK.



previous research (Gneezy, 2005; Hurkens & Kartik, 2009; Sutter, 2009), not all Receivers in the Cheap Talk Sender-Receiver Game will follow the advice. Therefore, to get approximately the same expected payoff from an option in the Dictator Game, the probability of implementing a Dictator's choice is 80%<sup>12</sup>. This is known by both players. A Dictator is considered selfish if she chooses option B, because this option always gives Dictators the highest expected payoff.

The difference between the two games are that in the Cheap Talk Sender-Receiver Game the Sender sends a message and in the Dictator Game the dictator makes a choice. A difference between the two games with respect to the fraction of selfish Dictators and Deceiving Senders is therefore likely due to a disliking of deception. This is also how lying is measured in the analysis.

*Lying*<sup>13</sup> *aversion*: there is smaller fraction of Senders who send message B than Dictators who choose option B.

#### **2.4.2 The Preference Survey Module**

As mentioned in the introduction, there are several factors that influence lying behavior and especially relevant for this paper is the interaction with others. Using a survey developed by Falk, Becker, Dohmen, Huffman, and Sunde (2016), I can control for risk aversion, discounting, trust, altruism, and positive and negative reciprocity. All of these factors, except for discounting, are directly relevant for lying and social preferences. Trust, altruism, and positive and negative reciprocity give a

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<sup>12</sup> 81% of Receivers in my sample followed the advice from the Sender.

<sup>13</sup> I use the word "lying" for ease of comparison to the other literature. Sutter (2009) uses deception and sometimes intended deception. Meanwhile Gneezy (2005) looks at the expected consequence, where everyone that sends the untruthful message is classified as liar. In the other literature (Abeler et al., 2014; Fischbacher & Föllmi-Heusi, 2013; Gneezy et al., 2013) the game is different and Sophisticated Truth-Telling has little to no strategic advantage.

richer insight into the social preferences of a subject than the Dictator Game alone. Risk aversion is relevant for the probability of being caught as a deceiver.

For a full list of the questions from the survey, please refer to the appendix. Two different approaches are used for eliciting risk preferences. In the first method, subjects self-report their willingness to take risks on an eleven-point scale going from "completely unwilling to take risks" to "very willing to take risks". The other method used is a decision tree, where subjects have to choose between a safe option and a 50/50 lottery between zero and varying amounts. There are five levels of these varying amounts, where the new amount is dependent on the previous amount and answer. A similar approach is used for discounting; however, the choice is between an amount today and an amount in 12 months. Subjects also self-report how willing they are to give up something today which will be more beneficial to them in the future. This elicitation is done with an eleven-point scale going from "completely unwilling to do so" to "very willing to do so". The same scale is used when subjects self-report how willing they are to punish others if they themselves are treated unfairly, if someone else is treated unfairly, or how willing they are to give without expecting anything in return. Subjects are then asked to indicate how well different statements regarding reciprocity and trust describe them. A couple of hypothetical scenarios are also used for measuring altruism and positive reciprocity. Altruism is measured by how much they will donate to a charity of their choosing if they receive 8000 NOK tomorrow. Positive reciprocity is measured by confronting them with a situation where they receive a favor from a stranger, and they have the opportunity to give this person one of six presents of different value or no present at all.

## **2.5 Procedure**

The experiment was conducted over three days, with a total of 15 sessions and 312 subjects participating. The average number of pairs in

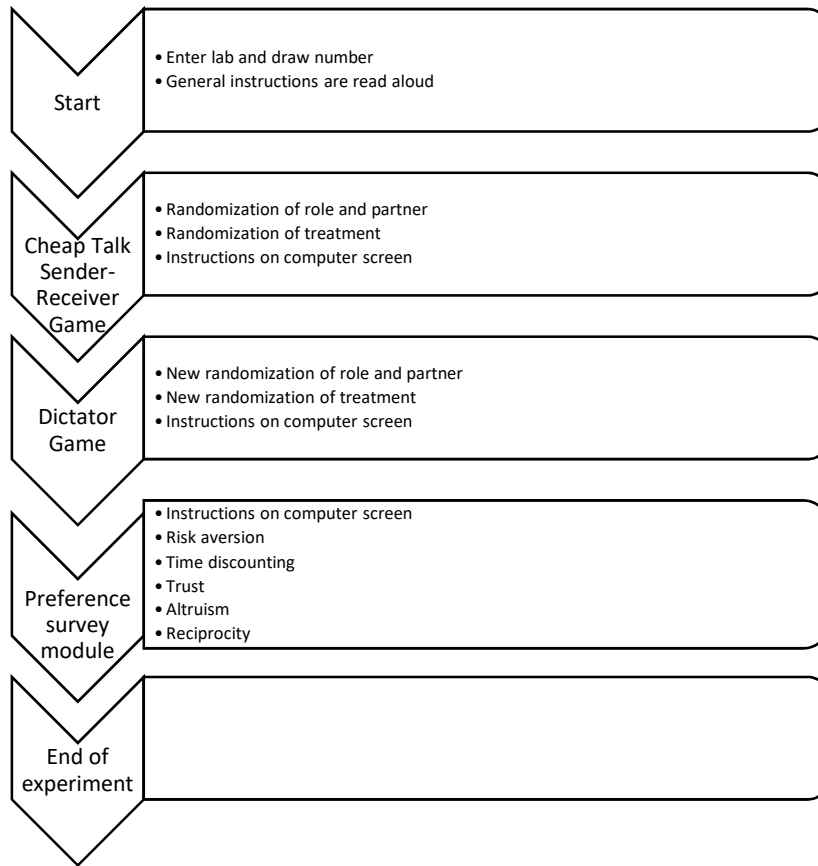
one session was 11 and the lowest number of pairs in one session was eight. All the subjects were recruited through an email sent to students at the University of Stavanger, and the email contained a link to a webpage where the students could register for their desired session. The distribution of subjects into treatments was even with 51 pairs in *Poor*, 53 pairs in *Equal* and 52 pairs in *Rich*. Approximately half of the Senders were female: 49% in the Cheap Talk Game and 51% in the Dictator Game. The median age of the participants was 24.5 years old. When testing the balance of the treatment using OLS regressions in Table 4, I find that all treatments are well balanced with respect to age and gender. In addition, all treatments except for *Poor* in the Cheap Talk Sender-Receiver Game are well balanced with respect to education. *Poor* in the Cheap Talk Sender-Receiver Game has weakly higher education than *Equal*. Apart from this difference, the sample is well balanced with respect to age, gender and education.

When entering the lab, participants drew a number from a cup that determined their place in the lab. The participants received the general instructions in writing, that were read aloud and which informed the participants of the two games. Subjects were explicitly told that roles and partners would be randomized between the two games, and that what happened in the first game had no consequence in the second game. As seen in Table 3, both Dictators who were a Sender or Receiver in the first game behaved similarly, but there are some differences. Testing this with a test of equal proportion shows no differences between choices made by Dictator that were Senders in the first game, and Dictators that were Receiver in the first game ( $z = 0.257$ ,  $p = 0.797$ ). See section 2.7.1 Consistency Between Games for a more detailed analysis of Dictators' behavior dependent on their role in the first game.

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Figure 1 Experimental procedure



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Table 3 Dictator's Choice Depending On Role In First Game

Choice in Dictator Game	Role in CTS-RG		
	Receiver	Sender	Total
A	33	29	62
	21.15 %	18.59 %	39.74 %
B	52	42	94
	33.33 %	26.92 %	60.26 %
Total	85	71	156
	54.49 %	45.51 %	100 %

Note: The table shows the choices made by the dictators, dependent on the role they had in the Cheap Talk Sender-Receiver Game.

The rest of the information came from the computer program. The games and the Preference Survey Module<sup>14</sup> were computerized using z-Tree (Fischbacher, 2007). The subjects then played the Cheap Talk Sender-Receiver Game and before the Dictator Game started, a new randomization of both role and counterpart took place. When the Dictator Game was over, subjects completed the preference survey module before they learned their earnings from both games. Lastly, subjects filled out a questionnaire with background variables. Each session lasted approximately 30 minutes. The design is summarized in Figure 1.

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<sup>14</sup> Due to a programming error, the answers for willingness to punish someone who treats others unfairly was not recorded.

Table 4 OLS Regression Testing For A Sample Balance

<b>Dependent variable:</b>	<b>Age above median</b>	<b>Age above median</b>	<b>Female</b>	<b>Female</b>	<b>Education above median</b>	<b>Education above median</b>
Poor	0.037 (0.099)	-0.039 (0.099)	-0.077 (0.099)	0.059 (0.099)	0.191* (0.097)	-0.061 (0.097)
Rich	0.066 (0.098)	0.048 (0.098)	-0.028 (0.098)	-0.048 (0.098)	-0.070 (0.092)	0.028 (0.098)
Constant	0.453*** (0.069)	0.509*** (0.069)	0.528*** (0.069)	0.509*** (0.069)	0.358*** (0.067)	0.434*** (0.069)
N	156	156	156	156	156	156
R <sup>2</sup>	0.003	0.005	0.004	0.008	0.050	0.006
Game	CTS-RG	DG	CTS-RG	DG	CTS-RG	DG

Note: The table shows the results from OLS regression on dummy variables for Age above median, Female and Education above median for both of the two games. Rich and Poor are dummy variables for the treatments, which take the value 1 if true and 0 otherwise. Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . CTS-RG: Cheap-Talk Sender Receiver Game. DG: Dictator Game.

## **2.6 Behavioral predictions**

Two factors play an important part when Senders make the decision about which message to send. Senders' and Dictators' social preferences determine how the differences in payoff opportunities are perceived. Senders' cost of deception determines how costly it is for them to deceive. I will first start with predictions regarding how social preferences can affect behavior, then I turn to the cost of deception and its presence and magnitude.

A rational self-serving Sender who is completely anonymous and who interacts with the Receiver only once will want to maximize his own payoff regardless of the Receiver's payoff opportunities. For these kinds of Senders no weight is put on social comparison. In this case, the Receivers will know this and the message sent from the Sender will be non-informative to the Receiver (Crawford & Sobel, 1982). However, as research has shown, Receivers consider the Senders' messages in Cheap Talk Sender-Receiver Games (Gneezy, 2005; Hurkens & Kartik, 2009; Sutter, 2009). Senders' beliefs are also in line with this. It is therefore possible for Senders to affect the choice of the Receiver through the message and Senders know this. In turn, this means that the message sent by the Sender will likely affect the Sender's own utility, because the message is followed and the choice made by the Receiver determines both players' payoffs.

When deciding which message to send, the Senders' social preferences will likely play an important role. Following Bolton and Ockenfels and also Fehr and Schmidt (Bolton & Ockenfels, 2000; Fehr & Schmidt, 1999), people tend to dislike inequity. Therefore, some Senders will likely use the message to even out the differences between the two payoffs because Senders experience disutility. Because people dislike having less more than they dislike having more, the disutility from differences in payoff opportunities will be largest in *Rich*. Additionally, Senders in *Rich* must send message B to minimize the difference in

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payoffs. These Senders will have to weigh the disutility from the differences in payoffs against the disutility from deception. Generally, a Sender will deceive if the utility from option B minus the cost of deception is larger than the utility from option A.

Senders in *Equal* will also experience disutility from differences in payoffs, more so if option A is chosen. Senders must therefore also weigh the disutility from the differences in payoffs against the disutility from deception. Following Bolton and Ockenfels and also Fehr and Schmidt (Bolton & Ockenfels, 2000; Fehr & Schmidt, 1999), disutility from differences in payoffs will be smaller than in *Rich* and fewer Senders in *Equal* will therefore deceive.

In *Poor*, Senders will likely also experience disutility from differences in payoffs. However, the disutility will likely be smaller than the disutility Senders in *Rich* experience. This is because Senders have better payoff opportunities in *Poor* than Receivers. In *Poor*, Senders can minimize the difference in payoffs by sending message A. One implication of this is that Senders in *Poor* do not have to weigh the disutility from the inequity in payoffs against the disutility from deception in order to reduce inequities in payoffs.

Previous empirical findings partly support that Senders will account for the Receiver's payoff. For example, Senders have been observed recommending the option that maximizes overall payoff, not their own payoff (Cappelen et al., 2013; Gneezy, 2005; Hurkens & Kartik, 2009). I therefore predict that:

**Prediction 1:** The treatments can be ranked with respect to the fraction of Senders who deceive  $Poor < Equal < Rich$ .

If Dictators do not care about the Receiver, then there will be the same number of Selfish Dictators in all three treatments, as the treatments all have the same payoff opportunities. However, Dictators will likely want



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to minimize the difference in payoffs due to the disutility from these differences in payoffs. This is likely true since social preferences among Dictators have been well documented (for a summary see e.g. Cooper & Kagel, 2016). That being said, Dictators usually allocate more to themselves (Engel, 2011). Therefore, Dictators will generally be selfish if the utility of option B is larger than the utility from option A.

The same argument for disutility from differences in payoffs from the Cheap Talk Sender-Receiver Game applies in this Dictator Game.

**Prediction 2:** The treatments can be ranked with respect to the fraction of Dictators who are selfish  
*Poor < Equal < Rich.*

Deception is costly to Senders and there will therefore be a smaller fraction of Deceiving Senders than Selfish Dictators within the same treatment. However, if social preferences are the only thing that affect Senders' and Dictators' decisions, then we will see the same number of Deceiving Senders and Selfish Dictators within the treatments. This is likely not true, and based on previous research as first shown by Gneezy (2005) and later a large body of research (see e.g. Erat & Gneezy, 2012; Gneezy et al., 2017; Gneezy et al., 2013; Houser et al., 2012; Hurkens & Kartik, 2009), deception (lying) is costly to people. Due to this intrinsic cost, which only Senders are subject to (Dictators only make a decision), there will be a smaller fraction of Deceiving Senders than Selfish Dictators. This difference between the fractions is known as "lying aversion".

**Prediction 3:** Senders have lying aversion and this presents itself through a lower fraction of Deceiving Senders than Selfish Dictators within each treatment.

People are likely to weigh the disutility from differences in payoff against the cost of deception because both affect utility. This will show itself in the size of the difference between the fraction of Selfish

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Dictators and Deceiving Senders. There are different findings regarding how social preferences and lying cost interact; Cappelen et al. (2013) find that more pro-social preferences are correlated with higher cost of lying. In addition, Maggian and Villeval (2016) find that envious children are more likely to lie. Related is the study by Gino and Pierce (2009), who find that people act dishonestly in order to level out inequality and more so if they are at a disadvantage. Contrary to these studies, Kajackaite and Gneezy (2015) find no correlation between social preferences and lying. The empirical evidence regarding the relationship between social preferences and deception is in other words inconclusive, as some find a relationship and some do not. On a different note, in the story about Robin Hood, the main character and his gang steal from the rich, give to the poor, and are celebrated for it. In other words, they use dishonest behavior to level out inequality. This story provides intuition as to how (some) people weigh dishonesty and social preferences against one another.

The cost of deception is likely the same in all three treatments, because the gain from deception is the same in all three treatments and so is the harm to the Receiver (in absolute terms). Therefore, there is likely a point where the disutility from the inequity is greater than the disutility from deception and senders will deceive. However, the effect is moderated/offset by the cost of deception. In other words, the disutility from inequity crowds out disutility from deception. As discussed above in predictions 1 and 2, the disutility is likely largest in *Rich* and smallest in *Poor*. Therefore, the size of the lying aversion (difference in fractions of Selfish Dictators and Deceiving Senders) is also likely to be smallest in *Rich* and largest in *Poor*. This is because the disutility from deceiving is the same in all three treatments, but the disutility from differences in payoffs is different between the three treatments.

If the cost of lying is larger than the disutility from the difference in payoff opportunities, social preferences will not matter and equally many Senders will deceive in all of the three treatments.

**Prediction 4:** The magnitude of lying aversion will be smallest in *Rich* and largest in *Poor*.

## **2.7 Results**

Figure 2 shows the fraction of different sender types in all three treatments. By adding together the Strategic Truth-Tellers and Benevolent Liars, we see that a sizable fraction of Senders expects the Receiver to choose the option that was not recommended. There is also a slight variation across treatments, however, the variation is not of a large magnitude. In *Poor*, most Senders are Benevolent Truth-Tellers (43.1%), the second most common type is Liars (23.5%), followed by Strategic Truth-Tellers (21.6%), and lastly Benevolent Liars (11.8%). The distribution of Sender types is similar in *Equal* with most Benevolent Truth-Tellers (43.4%), Liars (22.6%), Strategic Truth-Tellers (24.5%), and Benevolent Liars (9.4%). In *Rich*, there are most liars (42.3%), equally many Benevolent Truth-Tellers as there are Strategic Truth-Tellers (each 21.2%), and fewest Benevolent Liars (15.4%).

One commonality among the three treatments is that Benevolent Liars are the least common type and the proportions in the three treatments are not statistically different from one another ( $\chi^2=0.88$ , d.f. = 2,  $p=0.644$ ). The proportions of Strategic Truth-Tellers are the same across treatments ( $\chi^2 =0.20$ , d.f. = 2,  $p=0.903$ ), which implies that these types are not sensitive to the treatment differences. Based on these findings we can say that results are driven by Liars and Truth-Tellers. If we test for different proportions of Liars between the three treatments, we get the same result: no difference between the Poor and Equal treatment ( $p=0.914$ ). The proportion of Liars in *Rich* is significantly larger than both the Poor ( $p=0.043$ ) and *Equal* ( $p=0.031$ ).

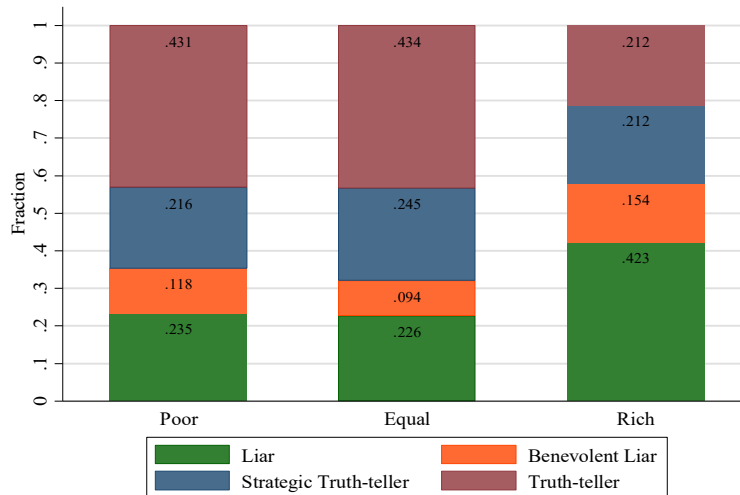
*Equal* is similar to Sutter (2009)'s T2 treatment, however, he finds a somewhat different pattern than I do. Sutter finds most Benevolent

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Truth-Tellers (36%), followed by Liars (33%), then Sophisticated Truth-Tellers (28%), and lastly Benevolent Liars (2%).

Figure 2 Fraction of different Sender types in treatments



Note:

Figure 2 shows the fraction of the different Sender types within each treatment. The Sender types are classified by using the message they sent and which option they expected the Receiver to choose, see Table 1 for details. N = 156.

I analyze the data based on the definition of Deception discussed previously by adding together Liars and Strategic Truth-Tellers, since both of these types expect option B to be chosen<sup>15</sup>.

As seen in Figure 3, there are some differences between the three treatments with respect to the fraction of Senders who deceive the Receiver. Most notably is *Rich* where a fraction of 0.635 Senders deceive. There are fewer Senders who deceive in the *Poor* and *Equal* treatments, which have fractions of 0.451 and 0.472, respectively. Testing<sup>16</sup> the differences between the treatments shows that there is no

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<sup>15</sup> See appendix for the analysis using Gneezy (2005)'s definition that only looks at the message.

<sup>16</sup> All tests are two-sided tests of equal proportions, unless noted otherwise.

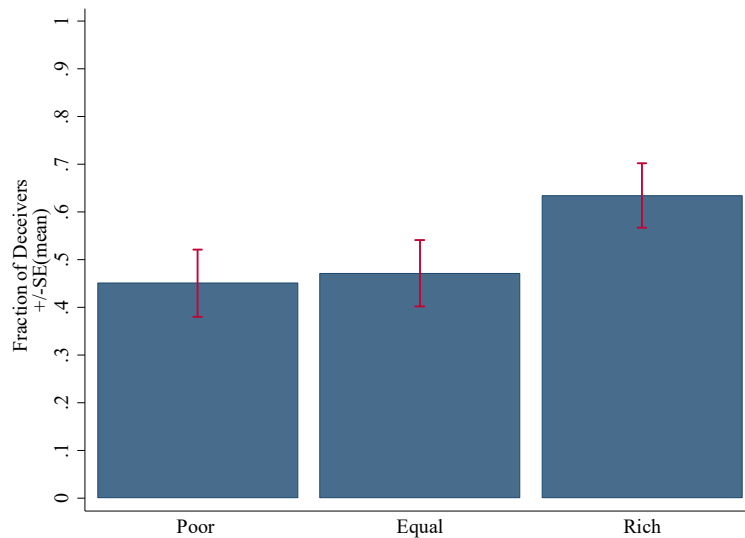
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difference in the fraction of Senders who deceive between Poor and Equal treatments ( $p=0.832$ ). The difference between Equal and Rich is weakly significant ( $p=0.093$ ) and so is the difference between Poor and Rich ( $p=0.061$ ). Based on these results, it appears that having worse payoff opportunities weakly makes Senders more inclined to deceive. However, if the Senders have better payoff opportunities, they are not less likely to deceive than if they have the same payoff opportunities.

I find that Senders in *Equal* are less inclined to deceive compared to the fraction of Deceiving Senders in the Sutter (2009) T2 treatment, where a fraction of 0.61 deceives. The difference of 0.14 ( $0.61 - 0.47$ ) implies that Sutter's sample either has lower disutility from deception or a stronger preference for getting a higher payoff than my sample.

Figure 3 Fraction of Senders who deceive



Note: The figure shows the fraction of Senders who expect the Receiver to choose option B within each treatment.

To test the difference between treatments I use dummy variables in the regression analysis. Rich is a dummy variable that takes the value 1 if a

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subject was in *Rich*. *Poor* is a dummy variable that takes the value 1 if a subject was in *Poor*. Unless noted otherwise, *Equal* is used as the base treatment. The variable *Female* is an indicator variable for gender, and *Age* is a continuous variable measuring the subjects' age in years.

Table 5 shows the results of testing the data using a probit model with expecting Option B as the dependent variable. We see that without controlling for the factors in the preference survey module, model 1 and 1m, Senders in *Rich* are weakly more likely to deceive than Senders in *Equal*. This is the same pattern as the non-parametric analysis above. When controlling for the factors in the preference survey module, model 2 and 2m, the weakly significant effect in model 1 still persists.

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Table 5 Probit Regression on Deceiving Senders

Dependent var. Deceiving Sender	(1)	(1m)	(2)	(2m)
Rich	0.429*	0.169*	0.474*	0.186*
	(0.249)	(0.095)	(0.259)	(0.099)
Poor	-0.045	-0.018	-0.070	-0.028
	(0.252)	(0.100)	(0.266)	(0.106)
Female	-0.190	-0.075	-0.317	-0.126
	(0.205)	(0.081)	(0.220)	(0.086)
Age	0.010	0.004	0.017	0.007
	(0.017)	(0.007)	(0.019)	(0.007)
Constant	-0.232		0.064	
	(0.514)		(0.569)	
Observations	156	156	156	156
Pseudo $R^2$	0.026	0.026	0.072	0.072
PSM Controls	No	No	Yes <sup>17</sup>	Yes

Note: Rich, Poor, Female are all dummy variables which take the value 1 if true and 0 otherwise. Age is a continuous variable. Marginal effects: m. Robust standard errors in parentheses. PSM: Preference Survey Module. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Result 1:** More Senders deceive when they have worse payoff opportunities, but do not lie less when they have better payoff opportunities than the Receiver.

As seen in Figure 4, there is a clear ranking of the treatments with regard to the fraction of Selfish Dictators. When I test the differences between the treatments, I find they are all significantly different from each other. Dictators in *Poor* are significantly less selfish than Dictators in *Equal*

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<sup>17</sup> I construct dummy variables (1 if above median) when running the probit. If I use the raw answers the significance level of Rich dips just underneath 0.1.

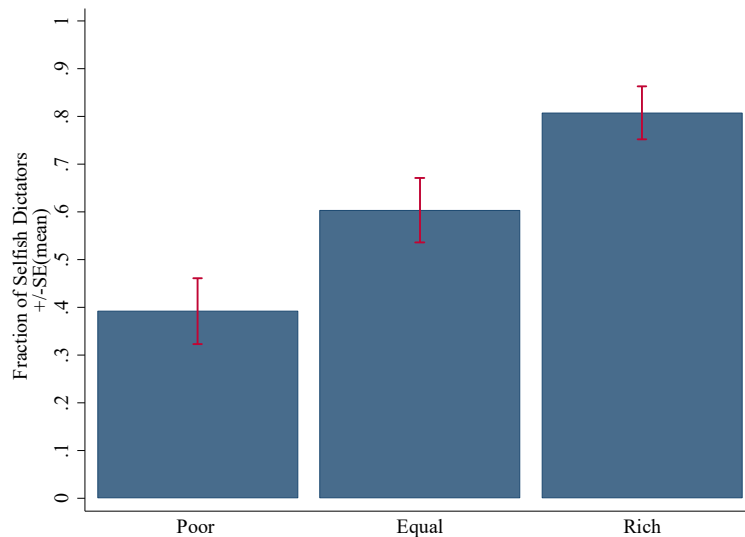
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( $p=0.031$ ) and Dictators in *Rich* are significantly more selfish than Dictators in *Equal* ( $p=0.022$ ). This result indicates that the Dictators' choices do depend on the Receivers' payoff opportunities.

Comparing *Equal* to treatment 1 in Gneezy (2005), I find almost the same fraction of Selfish Dictators, where I have a fraction of 0.604 and Gneezy a fraction of 0.66.

Figure 4 Fraction of Selfish Dictators



Note: Figure 4 shows the fraction of Dictators within each treatment who chose option B and thereby are classified as Selfish.

Investigating this in a probit model in Table 6 with Selfish Dictator as the dependent variable, I find the same results as in the non-parametric analysis. As seen in model 1, there are significantly fewer Selfish Dictators in *Poor* while there are significantly more Selfish Dictators in *Rich*, both compared to *Equal*. These results also persist when controlling for the factors in the preference survey module, model 2.



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Table 6 Probit Regression on Selfish Dictators

Dependent var.	(1)	(1 m)	(2)	(2 m)
<b>Selfish Dictator</b>				
Rich (d)	0.629** (0.266)	0.229** (0.090)	0.601** (0.273)	0.219** (0.093)
Poor (d)	-0.538** (0.251)	-0.208** (0.097)	-0.576** (0.262)	-0.222** (0.100)
Female (d)	0.039 (0.218)	0.015 (0.083)	-0.028 (0.225)	-0.011 (0.086)
Age	-0.013 (0.016)	-0.005 (0.006)	-0.017 (0.017)	-0.006 (0.007)
Constant	0.585 (0.479)		0.328 (0.563)	
Observations	156	156	156	156
Pseudo $R^2$	0.095	0.095	0.118	0.118
PSM Controls	No	No	Yes	Yes

Note: Rich, Poor, Female are all dummy variables which take the value 1 if true, 0 otherwise. Age is a continuous variable. Marginal effects: m. Standard errors in parentheses. PSM: Preference Survey Module. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . PSM: Preference Survey Module, also dummy variables (1 if above median).

**Result 2:** Dictators' choice is dependent on the Receivers' payoff opportunities. The fraction of Selfish Dictators increases together with the Receivers' payoff opportunities.

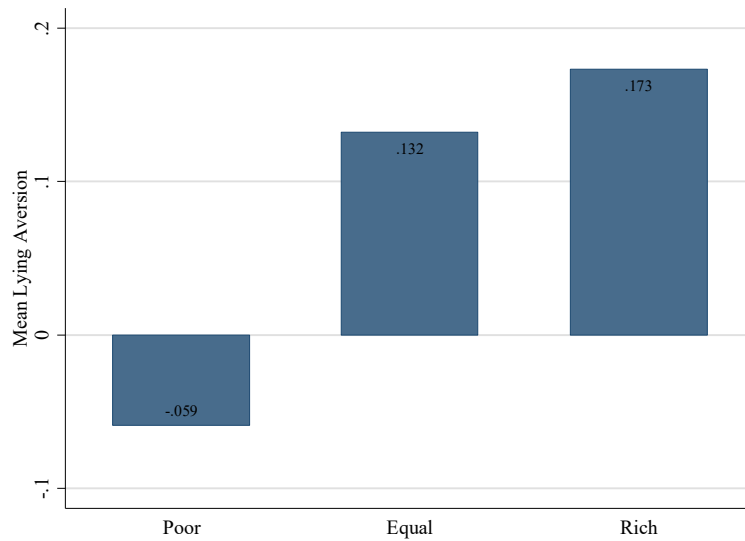
In all of the treatments there is a difference between the fraction of Deceiving Senders and Selfish Dictators, and this is the lying aversion mentioned in the introduction. This upper bound measurement is calculated by subtracting the fraction of Senders who deceive from the fraction of Selfish Dictators for each treatment, which is in line with what Gneezy (2005) did. This is illustrated in Figure 5, which shows the magnitude of the lying aversion. The odd one out is Poor, where lying aversion is negative. A test to see whether the two proportions, Senders who deceive and Selfish Dictators, are different shows that they are not ( $p=.843$ ). The other two treatments, Equal and Rich, have positive lying

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aversion, which means that there are fewer Deceiving Senders than there are Selfish Dictators. *Equal* with its lying aversion of 0.132 is both positive and significant ( $p=.003$ ). The same goes for *Rich* with a lying aversion of 0.173 ( $p=.027$ ).

Figure 5 Lying aversion



Note: The graph shows the difference between the mean fraction of Selfish Dictators and Deceiving Senders within a treatment.

The lying aversion of *Rich* appears to be larger than *Equal*, while testing if this is the case shows that they are not ( $p=.559$ ). This means that in the *Equal* and *Rich* treatments, deception is reduced by the same amount due to it being costly. However, deception is not reduced in *Poor* and it appears to be costless to the Senders in this treatment. One possible explanation for this pattern is that social preferences crowd out lying aversion when a Receiver is worse off than the Sender.

**Result 3:** Senders have lying aversion when their payoff opportunities are equally large or smaller, compared to the

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Receiver’s payoff opportunities. Conversely, Senders do not have lying aversion when paired with a Receiver who has smaller payoff opportunities.

One interesting observation from the results is that even though Senders deceive Receivers, and the most in *Rich*, it is also in this treatment that lying aversion is largest.

**2.7.1 Consistency between games**

In this section I test whether the Dictators having the role as Senders in the Cheap Talk Sender-Receiver Game behave differently from the Dictators having the role as Receivers.

Of the 156 Dictators, 71 were also Senders in the Cheap Talk Sender-Receiver Game. 66% of the Selfish Dictators also deceived in the Cheap Talk Sender-Receiver Game. Of the Benevolent Dictators (chose option A), 48% did not deceive in the Cheap Talk Sender-Receiver Game. This is summarized in Table 7.

Table 7 Number of Dictators who were also Senders

		Dictator Game		Total
		A	B	
<b>Cheap Talk Sender-Receiver Game</b>	<b>A</b>	16 48 %	17 52 %	33 100 %
	<b>B</b>	13 34 %	25 66 %	38 100 %
	Total	29 41 %	42 59 %	71 100 %

Note: The table shows the number of Dictators who also had the role as Sender, along with their choice in both of the games. In the Cheap Talk Sender-Receiver Game, the table shows the message sent to the Receiver.

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One important caveat about this analysis is that there are few observations. Because of this I believe that few inferences can be made from this analysis.

Table 8 Players who had the role as both Sender and Dictator

<b>Dictator Game</b>				
<b>CTS-RG</b>	<b>Poor</b>	<b>Equal</b>	<b>Rich</b>	<b>Total</b>
<b>Poor</b>	5	11	6	22
<b>Equal</b>	10	8	7	25
<b>Rich</b>	7	7	10	24
<b>Total</b>	22	26	23	71

Note: CTS-RG: Cheap Talk Sender-Receiver Game. The table shows how many players had the role as both Senders and Dictator split into and the treatment they were in the two games.

Table 9 further investigates whether Dictator's behavior is influenced by the role they had in the previous game with probit regressions. In model 1, which pools all of the treatments in the Cheap Talk Sender-Receiver Game, we see that the coefficients for the interaction variable Sender \* Equal CTS-RG is significantly negative. This means that Senders in *Equal* are overall less likely to be a Selfish Dictator. Investigating this further by looking at each of the treatments in the Dictator Game (models 2, 3 and 4), we see that in models 2 and 3 that the variable Sender \* Equal CTS-RG is not significant. However, it is significantly negative in model 4. This means that a Dictator in *Equal* that also was a Sender in *Equal* is significantly less likely to be a Selfish Dictator. This can in turn affect the findings about lying aversion and the size of the lying aversion. However, this is based on few observations and additional experiments have to be run in order to find conclusive results. Notable is the paper by Hurkens and Kartik (2009), who randomize the order of the Cheap Talk Sender-Receiver Game and the Dictator Game and find no ordering effect. In light of this finding, it is not clear whether the difference between the Dictators is due to the role they had in the Cheap Talk Sender-Receiver Game.

Table 9 Probit Selfish Dictators controlling for role in Cheap Talk Sender-Receiver Game

<b>Model:</b>	<b>(1)</b>	<b>(1m)</b>	<b>(2)</b>	<b>(2m)</b>	<b>(3)</b>	<b>(3m)</b>	<b>(4)</b>	<b>(4m)</b>
<b>Treatment in DG:</b>	<b>All</b>	<b>All</b>	<b>Rich</b>	<b>Rich</b>	<b>Poor</b>	<b>Poor</b>	<b>Equal</b>	<b>Equal</b>
Poor DG (d)	-0.479*	-0.185*						
	(0.254)	(0.098)						
Rich DG (d)	0.666**	0.241***						
	(0.272)	(0.090)						
Sender (d)	0.503	0.189	4.685	0.774	0.561	0.215	0.070	0.027
	(0.345)	(0.126)	(393.481)	(38.681)	(0.615)	(0.232)	(0.477)	(0.183)
Sender * Equal CTS-RG (d)	-0.902**	-0.348**	-4.319	-0.963	-0.778	-0.267	-1.279**	-0.471**
	(0.410)	(0.147)	(393.482)	(12.770)	(0.704)	(0.206)	(0.629)	(0.187)
Sender * Rich CTS-RG (d)	-0.653	-0.255	-4.545	-0.976	-0.433	-0.156	-0.785	-0.305
	(0.414)	(0.159)	(393.481)	(11.199)	(0.741)	(0.246)	(0.625)	(0.230)
Constant	0.250		0.702***		-0.307		0.535**	
	(0.207)		(0.255)		(0.237)		(0.254)	
Observations	156	156	52	52	51	51	53	53
Pseudo $R^2$	0.117	0.117	0.061	0.061	0.019	0.019	0.097	0.097

Note: The table shows probit regressions on Selfish Dictators, controlling for the role in the Cheap Talk Sender-Receiver Game. Model 1 shows all treatments while models 2, 3 and 4 show sub-sample analysis of the treatments in the Dictator game. Marginal effects: m. Robust standard errors in parentheses, CTS-RG: Cheap Talk Sender-Receiver Game. DG: Dictator Game. Poor DG and Rich DG are dummy variables for treatment in the Dictator game. Sender is a dummy variable for those who had the role as Sender in the Cheap Talk Sender-Receiver Game. Sender \* Equal CTS-RG, Sender \* Rich CTS-RG are dummy variables that show the interaction effect for those who were Sender and for the relevant treatment in the Cheap Talk Sender-Receiver Game. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## **2.8 Conclusion**

In this paper I run an experiment consisting of two games: a Cheap Talk Sender-Receiver Game and a Dictator Game. Both of these games have binary choice sets and the same payoff opportunities. I use three different treatments to test whether lying is affected by inequities between the Sender's and the Receiver's payoff opportunities. In all treatments, the payoff opportunities are asymmetric. This means that the choice which is best for the Sender is not the best choice for the Receiver. Two of the treatments introduce inequities: one treatment in which the Receiver always has smaller payoff opportunities than the Sender, and in the other the Receiver always has higher payoff opportunities.

The results show that Senders are affected by differences in payoff opportunities. Senders in *Rich* deceive significantly more than Senders in the other two treatments despite deception being costly. Deception is also costly for Senders in *Equal*. However, deception is costless for Senders in *Poor*. One possible explanation is that deception is used to even out the difference in payoff in *Rich*, since the Receivers can afford it. In *Equal* the same argument for *Rich* applies, however, fewer Senders have a preference for option B. Lastly, in *Equal*, the same number of Senders and Dictators have a preference for option B and this might be due to social preferences crowding out lying aversion. Why Senders find it costless to deceive Receivers in *Poor* but not in the other treatments is unclear. This is also the opposite of what I expected to find. One reason for this unexpected finding could be due to the way lying aversion is measured.

Another finding is that none of the factors in the preference survey module helped explain Sender or Dictator behavior. This underscores the importance of running an experiment where interactions between effects are tested directly and not just controlled for.

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One insight from the findings in this paper is that people will deceive others, regardless of the other person's wealth level or payoff opportunities. However, wealthy people or those with high payoff opportunities are deceived much more than others. So, if you have been fortunate in life, you should take advice with a good grain of salt.

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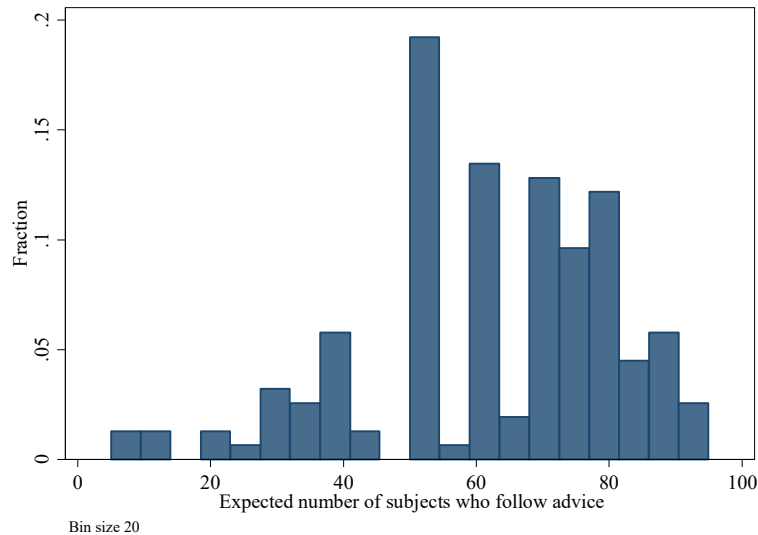
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## **2.10 Appendix**

Figure 6 Distribution of the Senders' beliefs about how many Receivers, out of 100, will follow the advice.

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Note: The figure shows the fractions of the Senders' answers to the question "How many Receivers out of 100 will follow the advice?"

### *2.10.1 Questions from the preference survey module.*

1.

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

2.

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

Please again indicate your answer on a scale from 0 to 10, where 0 means you are "completely unwilling to do so" and a 10 means you are "very

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willing to do so". You can also use any numbers between 0 and 10 to indicate where you fall on the scale, like 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

- How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?
- How willing are you to punish someone who treat you unfairly, even if there may be costs for you?
- How willing are you to punish someone who treat others unfairly, even if there may be costs for you?
- How willing are you to give to good causes without expecting anything in return?

3.

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

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

When someone does me a favor I am willing to return it.

If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so.

I assume that people have only the best intention.

4.

Please imagine the following situation: You can choose between a sure payment of a particular amount of money, or a draw, where you would have an equal chance of getting 3000 NOK or getting nothing. We will present to you with five different situations.

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31 outcomes – see original paper for details, the amount in Euro is multiplied by 10 to get the rounded amount in NOK.

5.

Please think about what you would do in the following situation.

You are in an area you are not familiar with, and you realize that you lost your way. You ask a stranger for directions. The stranger offers to take you to your destination. Helping you costs the stranger about 200 NOK in total. However, the stranger says he or she does not want any money from you. You have 6 presents with you. The cheapest present costs 50 NOK, the most expensive one costs 300 NOK. Do you give one of the presents to the stranger as a "thank-you"-gift? If so, which present do you give to the stranger?

- No present
- The present worth 50 NOK
- The present worth 100 NOK
- The present worth 150 NOK
- The present worth 200 NOK
- The present worth 250 NOK
- The present worth 300 NOK

6.

Imagine the following situation: Today you unexpectedly received 10,000 NOK. How much of this amount would you donate to a good cause? (Values between 0 and 10,000 are allowed)

7.

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Suppose you were given the choice between receiving a payment today or a payment in 12 months. We will now present to you 5 situations. The payment today is the same in each of these situations. The payment in 12 months is different in every situation. For each of these situations we would like to know which you would choose. Please assume there is no inflation, i.e. future prices are the same as today's prices.

31 outcomes – see original paper for details, the amount in Euro is multiplied by 10 to get the rounded amount in NOK.

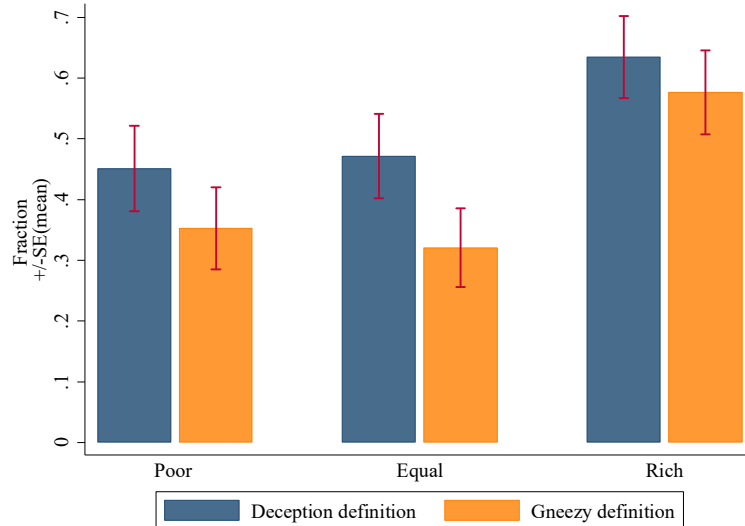
#### *2.10.2 Differences from definition of deception*

In this section I compare the two definitions of deception, namely the one that is used in the above analysis and also the definition used in Gneezy (2005). The difference between the two definitions is that the one in the previous analysis in this paper takes the Senders' stated expectation into account. Meanwhile, the definition Gneezy uses only looks at the message sent by the Sender. This comparison therefore acts as a robustness check.

Figure 7 Deceiving Senders - both definitions.

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Note: The figure shows the fraction of Senders who lied according to Gneezy's definition (orange) and according to my definition of deception (blue).

Looking at the graph, there are some differences between the two definitions of deception. Overall, the definition that I use always classifies more Senders as deceivers compared to Gneezy's definition. Investigating this further by comparing the proportions within each treatment, I find that none of the differences are significant. The largest difference which is found in *Equal* has a p-value of 0.112, while *Poor*'s test result is a p-value of 0.313. Lastly, *Rich* has the largest p-value of 0.547. Overall, the choice of definition does have weak implications, for the analysis as an overall test is weakly significant ( $p = 0.069$ ). This is also reflected in Table 10, which shows the results from probit regressions using the two different definitions as dependent variables. In the table we see that the coefficient for *Rich* changes significance level from weakly significant to strongly significant. Some of the other coefficients change direction, however, none of these coefficients are significant.

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The definition of deception does have minor implications on the Senders' social preference result and is worth mentioning. According to the definition I use, there is weak evidence for inequity averse or envious preferences; by using Gneezy's definition, the overall pattern remains the same but significant.

Table 10 Probit With The Two Different Definitions – Cheap Talk Sender-Receiver Game

<b>Dependent var:</b>	<b>(1)</b>	<b>(1m)</b>	<b>(2)</b>	<b>(2m)</b>	<b>(3)</b>	<b>(3m)</b>	<b>(4)</b>	<b>(4m)</b>
	<b>Deceive</b>	<b>Deceive</b>	<b>Deceive</b>	<b>Deceive</b>	<b>Gneezy def.</b>	<b>Gneezy def.</b>	<b>Gneezy def.</b>	<b>Gneezy def.</b>
Rich (d)	0.429*	0.169*	0.474*	0.186*	0.660***	0.257***	0.796***	0.308***
	(0.249)	(0.095)	(0.259)	(0.099)	(0.253)	(0.096)	(0.263)	(0.098)
Poor (d)	-0.045	-0.018	-0.070	-0.028	0.090	0.035	0.053	0.020
	(0.252)	(0.100)	(0.266)	(0.106)	(0.258)	(0.101)	(0.272)	(0.106)
Female (d)	-0.190	-0.075	-0.317	-0.126	0.125	0.049	0.093	0.036
	(0.205)	(0.081)	(0.220)	(0.086)	(0.208)	(0.081)	(0.223)	(0.087)
Age	0.010	0.004	0.017	0.007	-0.003	-0.001	0.005	0.002
	(0.017)	(0.007)	(0.019)	(0.007)	(0.016)	(0.006)	(0.017)	(0.006)
Constant	-0.232		0.064		-0.446		-0.534	
	(0.514)		(0.569)		(0.487)		(0.541)	
Observations	156	156	156	156	156	156	156	156
Pseudo $R^2$	0.026	0.026	0.072	0.072	0.041	0.041	0.079	0.079
PSM Controls	No	No	Yes	Yes	No	No	Yes	Yes

Rich, Poor, Female are all dummy variables which take the value 1 if true, 0 otherwise. Age is a continuous variable. Marginal effects: m. Standard errors in parentheses. PSM: Preference Survey Module. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

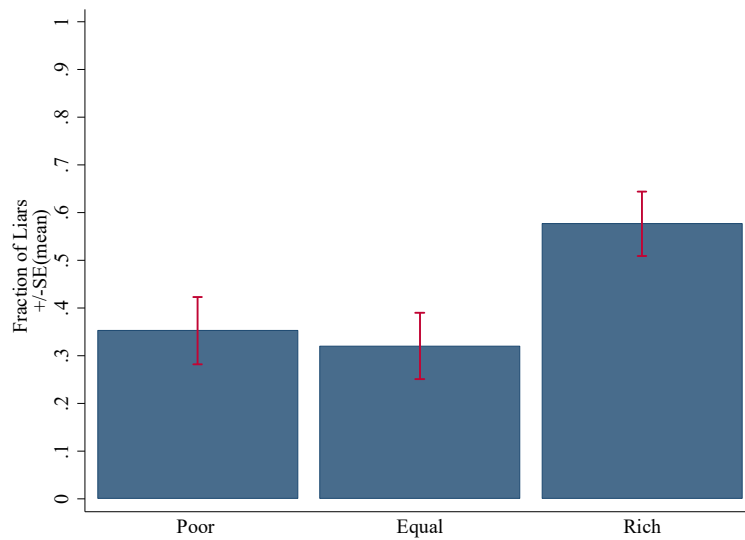


### 2.10.3 Results - Gneezy (2005)'s Definition of Lying

Using Gneezy (2005)'s definition of lying to see if Senders have social preferences yields the same result as the other definition, as shown in Figure 8. There are significantly fewer liars in Equal than there are in Rich ( $p=0.008$ ). However, there are no differences between the number of liars in Poor and Equal ( $p=0.728$ ). This means that Senders do have social preferences, and the difference is larger by using this definition.

**Result 1:** Lying only increases if the Sender has less than the Receiver, but it does not decrease if the Sender has more than the Receiver.

Figure 8 Fraction of Deceivers



Note: the figure shows the fraction of Senders within each treatment, who sent message B and therefore are classified as deceivers.

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Figure 9 In Figure 9, I show the difference between the number of Selfish Dictators and Liars, which gives an upper bound measure of the lying aversion. I show that lying aversion is almost non-existent in *Poor* with a difference of only 0.039 ( $p=0.682$ ) between the two games, meaning that the Senders in *Poor* have the same preference over the payoff distribution as the Dictators in the same treatment. In both the Equal and Rich treatments, there is significant lying aversion, with Senders in Equal having a larger lying aversion of 0.283 ( $p=0.003$ ) compared to Rich with 0.231 ( $p=0.011$ ). Testing if these two proportions are different reveals that they are not significantly different from each other ( $p=0.5422$ )<sup>18</sup>.

**Result 2:** Senders have lying aversion when their payoff opportunities are equally large or smaller, compared to the Receiver's payoff opportunities. Conversely, Senders do not have lying aversion when paired with a Receiver who has smaller payoff opportunities.

**Result 3:** Lying aversion has the same magnitude when it is present, which is in line with the theory of a fixed cost of lying.

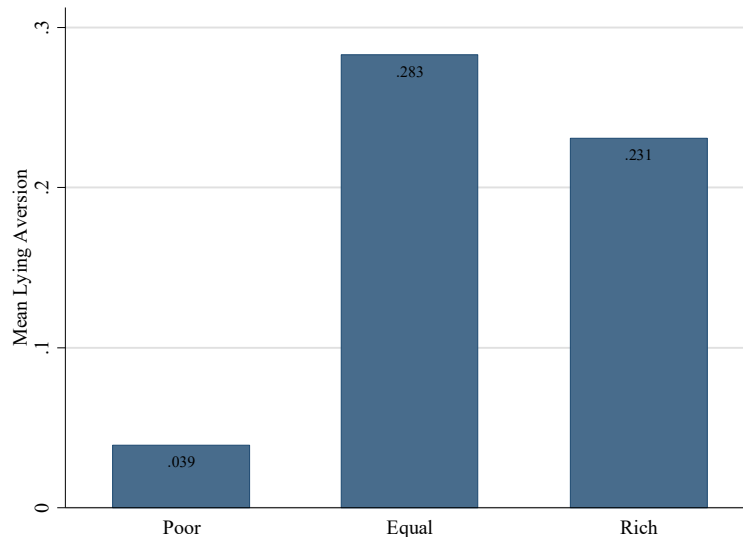
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<sup>18</sup> Difference between Poor and Rich is significant ( $p=0.005$ ).

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Figure 9 Size of lying aversion



Note: Shows the difference between the fraction of Selfish Dictators and Liars within each treatment, thus showing an upper bound estimate of the lying aversion's size.

### **2.10.4 Instructions**

#### **2.10.4.1 Greeting Instructions**

Welcome to this experiment in decision making.

The experiment will last approximately 30 minutes. During the experiment, you will earn money that will be transferred to your bank account during the next couple of days. It is important that you fill in the correct bank account number at the end of the experiment, or you will not receive payment. If you do not have a Norwegian bank account, please contact us at the end of the experiment.

The instructions for the experiment will appear on your computer screen. The experiment consists of three parts: two decision-making situations

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and one questionnaire. In the first part you will be matched with a random person in this room; you will then either be randomly assigned the role of sender or receiver. After the first decision-making situation, a new randomization will take place, and you will be matched with a new person and roles will be randomly assigned. You will earn money from both decision-making situations. Neither of the persons you are matched with, will ever learn your identity or what you earn. Situation one does not in any way affect situation two and vice versa. Lastly, you will complete a questionnaire and the experiment will be over.

Because this is part of a research project, we ask you to follow these rules so that we are able to use the data from the experiment.

The rules are:

- If you have a question, please raise a hand and we will come and help you.

Do not ask questions out loud!

- Do not communicate with the other participants.
- Turn off the sound on your phone.
- It is strictly forbidden to use the computer for other things than the experiment, due to the risk of technical malfunction.

Thank you for participating and good luck!

Do not take this outside of the lab.

#### **2.10.4.2 Cheap Talk Sender-Receiver Game: Sender**

Instructions cheap talk sender-receiver game, Equal Treatment, Sender.

This is the first decision-making situation. You have been randomly matched with another person in this room, hereby referred to as your

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counterpart. Two possible monetary payments are available to you and your counterpart in this situation. The two payment options are:

Option A: kr 50 to you and kr 60 to the other person

Option B: kr 60 to you and kr 50 to the other person

The choice rests with the other person, who will have to choose either option A or option B. The only information your counterpart will have is information sent by you in a message. That is, he or she will not know the monetary payments associated with each choice.

We now ask you to choose one of the following two possible messages, which you will send to your counterpart:

Message 1: "Option A will earn you more money than option B."

Message 2: "Option B will earn you more money than option A."

We will show the other person your message, and ask him or her to choose either A or B. To repeat, your counterpart's choice will determine the payments in the experiment. However, your counterpart will never know what sums were actually offered in the option not chosen (that is, he or she will never know whether your message was true or not). Moreover, he or she will never know the sums to be paid to you according to the different options. You will learn the result after situation two is completed.

We will pay the two of you according to the choice made by your counterpart. I choose to send (please click one):

Message 1

Message 2

#### **2.10.4.3 Cheap Talk Sender-Receiver Game: Receiver**

This is the first decision-making situation. You have been randomly matched with another person in this room, hereby referred to as your

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counterpart. Two possible monetary payments are available to you and your counterpart in this situation. The payments depend on the option chosen by you. We showed the two payment options to your counterpart. The only information you will have is the message your counterpart sends to you.

Two possible messages could be sent:

Message 1: "Option A will earn you more money than option B."

Message 2: "Option B will earn you more money than option A."

Your counterpart decided to send you message: X

We now ask you to choose either option A or option B. Your choice will determine the payments in this situation. You will never know what sums were actually offered in the option not chosen (that is, if the message sent by your counterpart was true or not). Moreover, you will never know the sums your counterpart could be paid with the other option.

We will pay the two of you according to the choice you make. I choose (click one):

Option A      Option B

#### **2.10.4.4 Dictator Game: Dictator**

You have been matched with a new person in this room and roles have been randomly assigned.

Part 2

In this situation, only you have to make a decision. Your decision will determine your payment, as well as your counterpart's payment. Two possible monetary payments are available to you and your counterpart in this situation, they are:

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Option A: Kr AS to you and Kr AR to the other student

Option B: Kr BS to you and Kr BR to the other student.

Your counterpart does not know which options are available, nor will your counterpart ever learn what you earn in this situation. Your counterpart only knows that you will make a decision that determine both payments.

Once you have made your choice, the computer will (by a random procedure) determine if your choice is used for the payments in this situation. The probability of your choice being used is 80%, while in the other 20% the other choice will be used.

For example: if 10 people choose Option X instead of Option Y, then Option X will be used 8 times and Option Y will be used 2 times to determine the payments. Your counterpart also know this.

I choose (please click one)

Option A      Option B

#### **2.10.4.5 Dictator Game: Receiver**

You have been matched with a new person in this room and roles have been randomly assigned.

Part 2

Two possible monetary payments are available to you and your counterpart in this situation. The payments depend on the option chosen by your counterpart, who we also showed the two payment options to. You will never know what sums were actually offered in the option not chosen. Moreover, you will never know the sums your counterpart was paid or could be paid with the other option. We will pay the two of you according to the choice made by your counterpart.

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Once your counterpart have made their choice, the computer will draw a random number that will determine if the choice is implemented.

The probability of the choice being implemented is 80%, while in the other 20% the other choice will be implemented. That is, on average the choice is implemented 8 out of 10 times.

You will learn the result from this and the first situation once your counterpart has made their decision.

Press "OK" to indicate that you understand that you have no choice in this situation



## **3 Fee Versus Return: An Experimental Investigation**

Kristoffer W. Eriksen, Sebastian Fest, and Bjørnar Laurila\*

### **3.1 Abstract**

An increasing number of people invest in actively managed mutual funds, despite the lack of evidence for these funds' ability to deliver returns above the index. These funds have higher fees than index funds that yield the same return as the underlying index. We predict that when making their investment decisions, people ignore fees through mental accounting in that the fee is segregated from the other attributes of the investment. We run an experiment on the online labor market Amazon mechanical turk (Mturk) to investigate this prediction. We do not find support for our main prediction, as our subjects act in accordance with standard economic theory and take the fee into consideration. We find that subjects take the same amount of risk and choose lotteries with the same after fee expected return as subjects who do not have to pay a fee. In addition, how the fee is presented does not affect behavior at all.

### **3.2 Introduction**

How do costs in investment settings influence our decisions? Do costs serve as a signal of quality or do we simply ignore costs? Fees are on average 7 times higher for actively managed funds compared to index funds, yet during the last 25 years the number of US households owning mutual funds has more than doubled, (from 23.4 million in 1990 to 53.2 million in 2014), and as of 2014 total mutual fund assets weigh in at \$ 12 496 billion (Investment Company Fact Book 2015). While the share

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\* We are grateful to Nina Hjertvikrem for helpful comments.

of index equity mutual funds has risen the past 15 years, it still only constitutes 20.2% of net assets relative to actively managed equity mutual funds (Investment Company Fact Book 2015; Institute, 2015). Studies show that actively managed funds underperform their benchmark (Carhart, 1997; French, 2008; Gruber, 1996; Jensen, 1968), thus it seems like a puzzle why such a large proportion of investors still opt for actively managed funds. In other words, people who invest in actively managed funds earn less return on their investment and pay higher fees compared to if they had invested in index funds. This observation is known in the literature as “the actively managed fund puzzle” (Gruber, 1996). Researchers usually explain this puzzle with either financial illiteracy (Choi, Laibson, & Madrian, 2010), trust in money managers (Gennaioli, Shleifer, & Vishny, 2015), agency problems (Inderst & Ottaviani, 2009, 2012a, 2012b) or ignoring fees (Choi et al., 2010).

Ignoring fees is not in accordance with standard economic theory, because a fee lowers the expected value of an investment and a rational person will take the fee into consideration. One possible explanation as to why people potentially ignore fees is mental accounting (Thaler, 1985). Mental accounting can also lead people to take more risk, for example in a setting when faced with a decision between a safe option and a costly lottery. The hypothesized way this happens is that the fee is segregated from the lottery by being attributed to a different mental account than the lottery. Because the lottery and fee are attributed to two different mental accounts, they are not evaluated together and the fee can be ignored. The separation of fee and lottery can happen in the editing phase where people get a preliminary evaluation of prospects (Thaler, 1985). In turn, this can lead subjects who have to pay a fee to choose the lottery to take more risk than subjects who do not have to pay a fee. This is because the lotteries look the same after the editing phase. This explanation is also hypothesized by Kahneman and Tversky in their seminal paper about prospect theory, where they write “... people are

unlikely to perform the operation of subtracting the cost from the outcomes in deciding whether to buy a gamble. Instead, we suggest that people usually evaluate the gamble and its cost separately...” (Kahneman & Tversky, 1979, p. 288).

The mental accounting explanation is supported by Hossain and Morgan (2006). In their field experiment, a product’s total cost was varied by changing the proportion that was listed as shipping cost. They found that people were willing to pay a higher total price when the proportion of shipping costs increased. Hossain and Morgan see this as evidence that some of the product’s fee is attributed to a different mental account than the other attributes.

Choi et al. (2010) also found that people ignore fees, but they claim that people seek past return and do not discuss mental accounting. They ran an experiment where subjects had to invest an amount in one or up to four different S&P 500 index funds. The four funds differed in the fee that was charged and the annualized return since inception. In addition, funds are chosen so that fees and past returns are positively correlated. This had the effect that if subjects chased past returns, they also paid more in fees.

We use a consumer-based approach where we test the mental accounting explanation with a stripped but clear design. We use a modified multiple price list risk-elicitation task (Holt & Laury, 2002). The task in this design is to decide between two prospects: one prospect always contains a safe option and the other always contains a lottery. Subjects have to make six of these decisions in succession and as the decisions progress, the expected value of the lottery prospect also increases in value. The safe option remains unchanged throughout the decision. At the end of the experiment, one of the six decisions is randomly selected as payment from the experiment.

We run four treatments where we change the way that fees are listed to test the effect of fees on lotteries. In the first treatment the cost is

explicitly listed together with the lottery outcomes. In the second the fee is integrated in the lottery outcomes. In the third the fee is only mentioned in the introduction. Lastly, we have a control treatment without a fee.

According to standard economic theory, there should be no difference in risk-taking between the treatments because people take the fee into consideration. However, if people have difficulties in incorporating the fee into the lottery then we expect people who have to pay a fee to take more risk due to the fee being segregated to a different mental account than the other lottery attributes. At the same time, we expect that subjects who are faced with reduced lottery outcomes due to the hidden cost will take less risk than the other subjects, because when the subjects faced with the hidden fee evaluate the lottery, the outcomes are already reduced. In addition, they are faced with the same type of information as subjects who do not have to pay a fee.

The main result from the experiment is that subjects do not segregate the cost from the other lottery attributes, thus the cost is not posted to a different mental account. Instead, subjects react in accordance with standard economic theory and take cost into consideration. They want the same expected value from the lottery after the cost is deducted, as subjects who do not have to pay the cost in order to choose the lottery. The listing of the fee had no effect, and we do not find support for Kahneman and Tversky's hypothesis that costs and gambles are evaluated separately.

Another finding from the main experiment contributes to the literature on cognitive ability and risk-taking (Dohmen, Falk, Huffman, & Sunde, 2010). We found that subjects who had poor fee-recollection had significantly higher switching points than subjects with good fee-recollection, meaning that they are significantly more risk-averse.

### **3.3 Design and Procedure - Main Experiment**

We use a modified multiple price list method (Holt & Laury, 2002) to investigate if fees are ignored (segregated). In this design, subjects choose between two prospects, one safe alternative and one lottery, and they make six of these decisions in succession. As the decisions progress, the safe option remains unchanged. For the lottery prospect the upside of the lottery increases, while the downside of the lottery remains the same. We can then see when subjects switch from the safe option to the lottery. This switching point is our estimate of the subject's risk-preference. Because the upside of the lotteries increases, the expected value of the lottery also increases. Therefore, a lower switching point will be associated with less risk-aversion than higher switching points.

We use four treatments in this design to test whether the presentation of the fee affects risk-taking. In *Free*, choosing the lottery is free and therefore this treatment acts as our control. Subjects in this treatment receive information about the safe option, the probabilities, and the associated outcomes for the gamble. In the second treatment, hereafter *Fee*, choosing the lottery costs 5 points; this is true for all of the six situations. The subjects receive information about the safe option, the lottery's probability, outcomes, and fee at the same time. In addition, they are informed that the fee must only be paid if they choose the lottery in the situation that is selected for payment. The other situations do not affect subjects' payment. Subjects are also told that the fee has to be paid regardless of whether the high or the low outcome of the lottery is drawn; in effect this lowers the lottery's expected value. In the third treatment, hereafter *Hidden Fee*, the same fee as in *Fee* is incorporated into the outcomes of the lotteries. The subjects receive the same information as in *Free*, namely the probability and the associated outcomes. However, the outcomes of the lotteries in *Hidden Fee* are reduced compared to *Free* and the fee is not mentioned. In the fourth and final treatment, hereafter *Fee Recollection*, we test fee recollection through a variant of *Fee*. Subjects receive information about the fee only in the introduction.

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When the subjects are faced with the six decisions, they receive the same information as the subjects in *Free*. After they have completed the six decisions, we ask them how much they had to pay in order to choose the lottery. This question let us categorize the subjects with good and poor fee recollection.

The table below shows the lotteries in the treatments. The fee is 5 points and reduces both the high and low outcomes as it is deducted from the subject's earnings regardless of the lottery's outcome.

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Table 1 Treatment payoffs

Lottery	Free	Fee	Hidden Fee	Fee Recollection
High	155	155	150	155
1 Low	45	45	40	45
Exp. Value	100	95	95	95
High	165	165	160	165
2 Low	45	45	40	45
Exp. Value	105	100	100	100
High	175	175	170	175
3 Low	45	45	40	45
Exp. Value	110	105	105	105
High	185	185	180	185
4 Low	45	45	40	45
Exp. Value	115	110	110	110
High	195	195	190	195
5 Low	45	45	40	45
Exp. Value	120	115	115	115
High	205	205	200	205
6 Low	45	45	40	45
Exp. Value	125	120	120	120
Cost	0	5	0	5
Cost listed	N/A	Every situation	N/A	Instructions only

Note: The table shows the lottery outcomes within each treatment. The expected value is listed here for convenience, but it was not listed for the participants.

Table 2 shows the classification of subjects' relative risk aversion; based on the situation they switch to the lottery prospect.

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Table 2 Classification of Subjects' relative risk aversion

Lottery	Without fee		With fee	
	<i>range of relative risk aversion</i>	<i>classification</i>	<i>range of relative risk aversion</i>	<i>classification</i>
1	$r < 0$	risk neutral	$r < -0.305$	Somewhat risk loving
2	$0 < r < 0.269$	slightly risk averse	$-0.305 < r < 0$	risk neutral
3	$0.269 < r < 0.471$	somewhat risk averse	$0 < r < 0.266$	slightly risk averse
4	$0.471 < r < 0.629$	risk averse	$0.266 < r < 0.400$	somewhat risk averse
5	$0.629 < r < 0.756$	fairly risk averse	$0.400 < r < 0.651$	risk averse
6	$0.756 < r < 0.859$	Very risk averse	$0.6251 < r < 0.744$	fairly risk averse

We conducted the main experiment over three batches, one at the end of March and two during April 2017. We recruited a total of 1200 subjects from the Amazon Mechanical Turk (Mturk). The selection criteria for workers stipulated that subjects on Mturk needed to have a total number of 500 previously approved human intelligence tasks (HITs) and a HIT approval rate of 95 percent. In addition, only subjects who indicated their location as the United States were eligible for participation. The subjects were redirected from Mturk to Qualtrics where the experiment took place. Subjects were then randomized into one of the four treatments and received the relevant information to that treatment in writing on their computer screen. In order to continue to the lotteries, the subjects had to answer correctly on three control questions regarding the design<sup>19</sup>.

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<sup>19</sup> How many prospects can you choose from in each situation? How many situations will you have to decide on? How many of the decision problems will have real monetary payoff consequences for you?



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Table 3 Mean, standard error of the mean and median- Descriptive statistics

<b>Treatment</b>	<b>Age</b>	<b>Education</b>	<b>Female</b>
	36.3	4.19	0.49
Hidden Fee	0.655	0.0822	0.0289
	33	5	0
	37.1	4.11	0.463
Fee	0.65	0.0811	0.0288
	34	5	0
	35.5	3.98	0.373
Fee Recollection	0.665	0.0771	0.028
	32	4	0
	36.9	4.24	0.43
Free	0.609	0.0762	0.0286
	34	5	0
	36.4	4.13	0.439
Total	0.323	0.0396	0.0143
	34	4.5	0

Note: The table shows the mean, standard error of the mean and median of descriptive statistics. Age is measured in years. Education is the number of years with higher education. Female is an indicator variable for the subject's self-reported gender.

After the subjects had answered all six decisions, they completed a quick survey with demographic variables and self-reported their financial literacy. Finally, one decision was randomly selected as payment for their participation in the experiment.

From the regression in Table 4 we see that our randomization was successful, as our sample is well balanced with respect to both age and education. However, there are significantly fewer females in *Fee Recollection* while the other two treatments are balanced in this aspect.

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Table 4 OLS Regression testing for balance between treatments using Descriptive statistics

<b>Dependent variable:</b>	<b>Age above median</b>	<b>Female</b>	<b>Education above median</b>
Fee	0.010 (0.041)	-0.027 (0.041)	-0.003 (0.041)
Fee Recollection	-0.050 (0.040)	-0.117*** (0.040)	-0.057 (0.041)
Free	0.030 (0.041)	-0.060 (0.041)	0.020 (0.041)
Constant	0.450*** (0.029)	0.490*** (0.029)	0.510*** (0.029)
Observations	1200	1200	1200
$R^2$	0.004	0.008	0.003

Note: The table tests for balance between the treatments with respect to Gender, Age and Education. Fee, Fee Recollection, Free and all the dependent variables are dummy variables for the treatments and take the value 1 if true. Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

### **3.4 Behavioral Predictions**

The standard economic theory prediction is that people take all factors into consideration. In this case, subjects in *Fee* and *Fee Recollection* will perfectly integrate the fee into the lottery. They will therefore take the same amount of risk, as subjected in *Hidden Fee*. Subjects in these three treatments will also start to choose the lottery prospect later than subjects in *Free*. However, the after fee expected return in the fee treatments at the switching point will be the same as the expected return at the switching point in *Free*.

As mentioned in the introduction, based on the work of Thaler, Kahneman and Tversky, subjects are likely to evaluate the lottery and the fee separately (Kahneman & Tversky, 1979; Thaler, 1985). The predicted mechanism here is that fees are segregated from the lotteries' outcomes and they are attributed to two different mental accounts. If subjects perfectly segregate the fee from the lottery, then we predict that

subjects in both *Fee* and *Fee Recollection* will take the same amount of risk as subjects in *Free*. This is because subjects in these two fee treatments evaluate the gain from the lottery in the same manner as their counterparts in *Free*. Additionally, subjects in *Hidden Fee* will take less risk compared to the other three treatments. This is because subjects in *Hidden Fee* do not have to integrate the fee into the outcomes of the lottery, as they only see the outcomes reduced by the fee. These predictions would be consistent with the findings of Hossain and Morgan (2006).

In addition, we expect subjects in *Fee Recollection* with poor fee recollection to take less risk than subjects who have good fee recollection, because their ability to recall the fee correctly can be seen as a crude proxy of cognitive ability. This prediction springs from the findings of Dohmen et al. (2010) which show that subjects with weaker cognitive abilities are less willing to take risks.

### **3.5 Results**

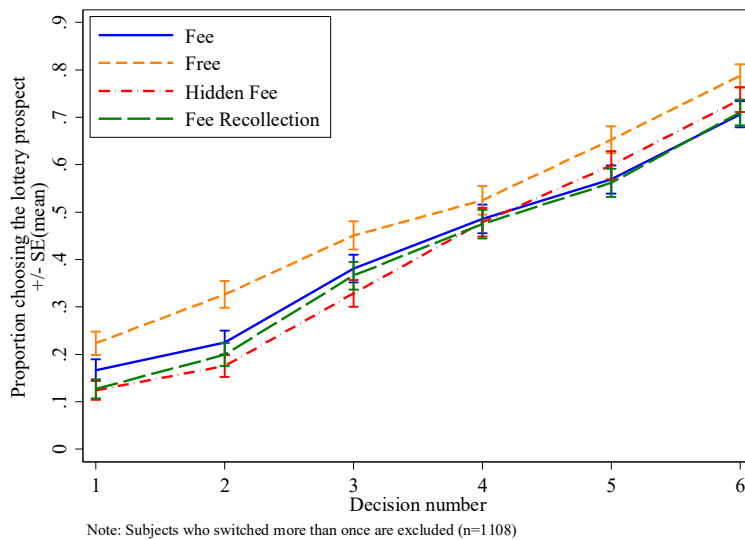
Our data consists of the answers from all subjects. However, in the analysis we drop subjects who are not consistent by switching back and forth between the safe prospect and the lottery prospect. Of the 1200 subjects, 92 subjects were inconsistent by switching multiple times. Excluding these inconsistent subjects does not change the results in a meaningful way, as they are equally distributed between the treatments (Kruskal-Wallis adjusted  $\text{Chi}^2(3) = 1.693$ ,  $p = 0.638$ ).

Figure 1 shows the proportion of subjects who choose the lottery in each of the six decisions. In the figure we see a clear pattern that an increasing number of subjects, regardless of treatment, chose the lottery. This pattern does not come as a surprise as the expected value increases with the lottery. Overall, there are always more subjects in *Free* who choose the lottery compared to the three fee treatments *Fee*, *Hidden Fee*, and *Fee Recollection*. In the first situation 22% of subjects in *Free*, 12% of

subjects in *Hidden Fee* and *Fee Recollection* and 17% of subjects in *Fee* choose the lottery. *Free* lies above both *Hidden Fee* and *Fee Recollection* until subjects reach lottery 4 and the three fee treatments converge. Despite differences among the fee treatments, none of these are significant. In the last decision, 78% of subjects in *Free* choose the lottery while the fee treatments are in the lower 70s, percentage wise.

We use a two-limit TOBIT regression together with a regular OLS regression later in the analysis. This is because there are sizable fractions of both subjects who always choose the lottery prospects and subjects who always choose the safe prospect.

Figure 1 Proportion of subject choosing lottery –consistent subjects only



Note: The figure shows the proportion of subjects in each of the treatments and in each of the decision situations that chose the lottery prospect.

Table 5 shows the results of pairwise testing the proportions of subjects who choose the lottery prospect in two consecutive situations. We see that all increases are strongly significant, except for three that are significant at the 10 % level. This means that significantly more subjects

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choose the lottery as the situations progress and that switching points are well suited for use in the coming analysis.

Table 5 Two-Sample Test of Proportions of subjects who chose the lottery prospect in each situation – Only Consistent Subjects

<b>Situation</b>	<b>Free</b>	<b>Fee</b>	<b>Fee Recollection</b>	<b>Hidden Fee</b>
1 - 2	-2.74***	-1.72*	-2.30**	-1.68*
2 - 3	-3.02***	-3.98***	-4.35***	-4.13***
3 - 4	-1.7*	-2.49**	-2.59***	-3.57***
4 - 5	-3.08***	-1.96**	-2.04***	-2.83***
5 - 6	-3.56***	-3.36***	-3.63***	-3.45***

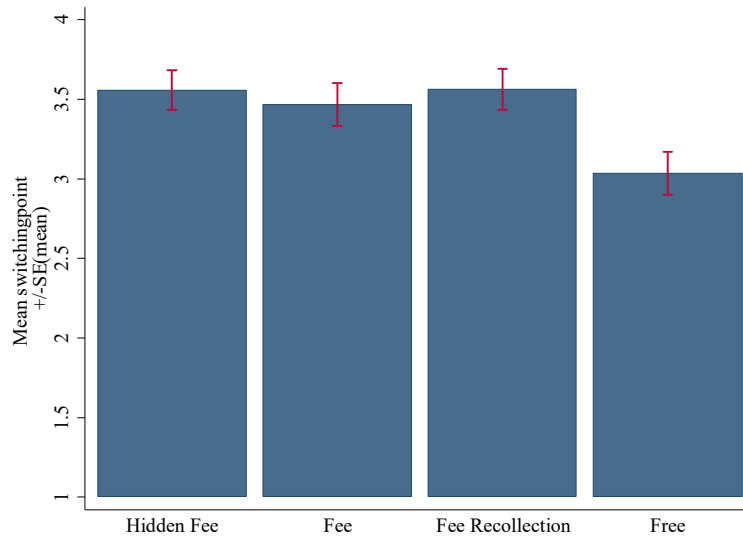
Note: The table presents z-values from two-sample test of proportions comparing the proportion of subjects who chose the lottery in each of the situations. \*:  $p < 0.10$ , \*\*:  $p < 0.05$ , \*\*\*:  $p < 0.01$

Because this method of risk elicitation is a form of multiple price list, selecting one lottery and drawing inferences from this would not be correct, and we therefore use switching points as our dependent variable when testing the behavioral predictions. A switching point is when a subject switches from the safe prospect to the lottery prospect. The switching points in this dataset range from 0 to 6 and are defined as 0 if the subject chooses the lottery prospect in the first situation, 1 if they choose the lottery prospect in the second situation, and so forth. Lastly, it is defined as 6 if they always choose the safe prospect. Therefore, a higher switching point is associated with higher risk-aversion.

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Figure 2 Mean switching point in treatments – only consistent subjects



Note: The figure shows the mean switching point in each of the four treatments. The switching point is where a subject switches from the safe prospect to the risky prospect, thus a higher switching point is associated with higher risk-aversion.

The average switching point and the standard error of the mean in the four treatments is shown in Figure 2. From a visual inspection of Figure 2 we see that the different fee treatment has about the same mean switching point. *Free's* mean switching point is lower. This pattern indicates that subjects integrate the fee with the lottery.

We find that at least one treatment's switching point is significantly different from the others from a Kruskal-Wallis test (adjusted  $\text{Chi}^2(3) = 10.82$ ,  $p = 0.01$ ). As observed in Figure 2, *Free's* switching point lies below the other three treatments' switching points, and by excluding *Free* we find no difference in switching points among the three fee treatments (adjusted  $\text{Chi}^2(2) = 0.190$ ,  $p = 0.91$ ). As a consequence, we can be certain that *Free* caused the significance in the first Kruskal-Wallis test. In addition, pairwise comparisons with Mann-Whitney U tests show that *Free's* switching point is significantly lower than all three

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fee treatments' switching point. Moreover, none of the three fee treatments have different switching points from each other. These results are summarized in Table 6. These non-parametric tests support the indices from the graph, that subjects integrate the fee in the evaluation phase.

Table 6 Mann-Whitney Test of Switching Points Between Treatments – Only Consistent Subjects

	Fee	Hidden Fee	Fee Recollection
<b>Free</b>	2.355***	2.728***	-2.861***
<b>Fee</b>		-0.316	-0.419
<b>Hidden Fee</b>			-0.96

Note: The table presents z-values from Mann-Whitney tests comparing the switching points between the treatments. \*: p<0.10, \*\*: p<0.05, \*\*\*: p<0.01.

Investigating the switching points further with regressions in Table 7, results show that regardless of the model's specification and analyzing method, subjects in *Free* have a significantly lower switching point than subjects in *Hidden Fee*. Looking at the TOBIT model with control variables, we see that *Free's* coefficient is -0.908. This means that subjects in this treatment switch to the lottery prospect one decision before subjects in the fee treatments do. At situation 3 in *Free*, the expected value of the lottery prospect is 110. At situation 4, where the average subject in the fee treatments switches, the after fee expected value of the prospect is also 110. This means that both groups are somewhat risk-averse as seen in Table 12. This lack of difference in expected value and risk preference further supports the indices that subjects integrate, not segregate, the fee in the evaluation phase.

Overall, we see that the OLS regressions tend to underestimate the effects compared to the TOBIT regressions, but the effects' directions are the same for both types of regression. In both the OLS and TOBIT regressions, being older than the median significantly increases the switching point with one situation. This means that older subjects are

more risk-averse than the median subject. We also find that more educated subjects have lower switching points; in other words, they are less risk-averse than the median educated subject. In addition, there is weak evidence that females in the OLS model have higher switching points than males— they are more risk-averse than men are, however, this is not supported by the TOBIT model.

Taken together, analyses are not able to reject the standard economic prediction that subjects will take the fee into account. The expected value at the switching points is in line with risk-averse preferences, because the average subject is not indifferent between the lottery prospect with expected value of 100 units and the safe option, but wants a premium of 10 units for choosing the lottery prospect. A difference in risk preference between subjects in the different treatments cannot explain the results. Thus, we reject the explanation that subjects segregate fees and lotteries and post them to different mental accounts. Subjects in our experiment seem to evaluate the fee and lottery together and post them to the same mental account.



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Table 7 Regressions on Switching point – only consistent subjects

Dependent variable: switching point	OLS	OLS	TOBIT	TOBIT
Fee	-0.091 (0.183)	-0.081 (0.180)	-0.112 (0.314)	-0.112 (0.309)
Fee Recollection	0.003 (0.179)	0.033 (0.178)	0.063 (0.305)	0.104 (0.302)
Free	-0.523*** (0.183)	-0.524*** (0.180)	-0.893*** (0.311)	-0.908*** (0.306)
Age above median		0.586*** (0.130)		1.021*** (0.223)
Female		0.224* (0.131)		0.355 (0.224)
Education above median		-0.295** (0.130)		-0.473** (0.222)
Constant	3.558*** (0.124)	3.335*** (0.163)	3.909*** (0.212)	3.526*** (0.274)
Sigma			3.526*** (0.119)	3.471*** (0.117)
Observations	1108	1108	1108	1108
R2	0.010	0.037		

Note: Fee, Fee Recollection, Free, Age above median, Female and Education above median are all dummy variables, taking the value 1 if true and 0 otherwise. Robust standard errors in parentheses \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

We now turn to investigate behavior in *Fee Recollection*. Here, subjects only received information about the fee in the instructions. We classify subjects according to whether they were able to recall the fee correctly<sup>20</sup> after they had completed all six decisions. Of the 276 subjects, 160 (58%) correctly recalled the fee. From the graph below we see that these

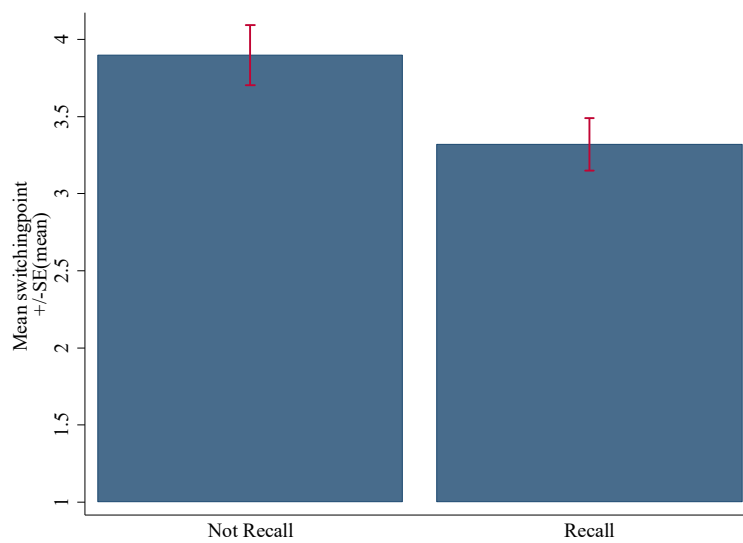
<sup>20</sup> Note: The subjects that did not correctly recall the fee could either not remember the fee or else they remembered the wrong amount.

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subjects switch to the lottery prospect before the subjects with poor fee recollection do. A non-parametric test shows that this difference in switching points is indeed significant (Mann Whitney  $z = 2.12$ ,  $p = 0.03$ ), meaning that subjects with good fee recollection are less risk-averse than subjects with poor fee recollection.

Figure 3 Average switching point in fee recollection treatment – only consistent subjects



Note: The figure shows the mean switching point for subjects in the Fee Recollection treatment that either were able to correctly recall the fee or were not able to correctly recall the fee.

The difference in switching points between recall and no recall subjects persists in regressions as seen in Table 8. We can therefore rule out that some of the other factors are causing the difference in switching points. We see that subjects who recall the fee correctly switch to the lottery prospect one situation before subjects who do not correctly recall the fee. In the TOBIT, we see that older subjects weakly have higher switching points, meaning that they are more risk-averse; this is in line with the regressions in Table 5.

Table 8 Regression on Switching point in Fee Recollection treatment consistent subjects only

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<b>Dependent variable: switching point</b>	<b>OLS</b>	<b>TOBIT</b>
Recalls fee correctly	-0.593** (0.258)	-0.905** (0.435)
Age above median	0.410 (0.266)	0.884* (0.457)
Education above median	0.000 (0.260)	-0.054 (0.439)
Female	0.183 (0.265)	0.263 (0.441)
Constant	3.668*** (0.254)	4.045*** (0.418)
sigma		
Constant		3.364*** (0.223)
Observations	276	276
R2	0.029	

Note: Recalls fee correctly, Age above media, Education above median and Female are all dummy variables, taking the value 1 if true and 0 otherwise. Robust standard errors in parentheses \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Based on this sub-sample analysis and our crude measure of cognitive ability, we find that subjects with better cognitive abilities are less risk-averse than subjects with poorer cognitive abilities. Looking at the expected value for the situation where subjects with good fee recollection switch, or situation 3, we see that the after fee expected value is 105. This value is lower than the expected value for the switching points for the rest of the sample. This finding also adds support to the hypothesis that these subjects take more risk.

### **3.6 Conclusion**

Previous studies have suggested several explanations as to why people still buy actively mutual funds; among these is financial illiteracy (Choi et al., 2010), ignoring costs (Choi et al., 2010), trust (Gennaioli et al.,

2015) or agency problems (Inderst & Ottaviani, 2009, 2012a, 2012b). Choi et al. (2010) also find that people seek to maximize past returns in addition to ignoring fees, and that the former seems to be the driving force. In this paper we investigate whether people segregate fees from lotteries as suggested by Kahneman and Tversky (1979), or whether people integrate the fee with the lottery.

In our experiment we utilize a clean design with binary choices between a safe option and a lottery. The data show that people in the three fee treatments have significantly lower switching points and take the same amount of risk as subjects who do not have to pay a fee. Thus, we are unable to find support for the prediction that the fee is segregated from and attributed to a different mental account than the lottery. Consequently, we must conclude that people act according to standard economic theory when choosing between a costly lottery and a safe option. These findings contradict the findings of Hossain and Morgan (2006), who found that some of a product's fee is ignored. One potential reason for these contradicting findings is that our experiment has a very clean design and no time limit. Hossain and Morgan on the other hand study auctions, and perhaps the time element causes a sense of urgency which in turn causes people to not pay attention to the whole cost. Another difference is that our study has a single fee, whereas Hossain and Morgan's study has two; namely, the actual price and shipping, which can also make it harder for people to calculate the total cost.

In addition, we also confirm our prediction that subjects with poor fee recollection have higher switching points than subjects with good fee recollection. This result is in line with the previous literature on cognitive ability and risk-aversion, such as Dohmen et al. (2010).

Real world implications from these findings are that hiding the cost does not matter, so we suggest that it is better for firms to be up-front with the cost and show that they have nothing to hide. Displaying honesty in this

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way can build a stronger relationship and give the firm extra goodwill with the customers.

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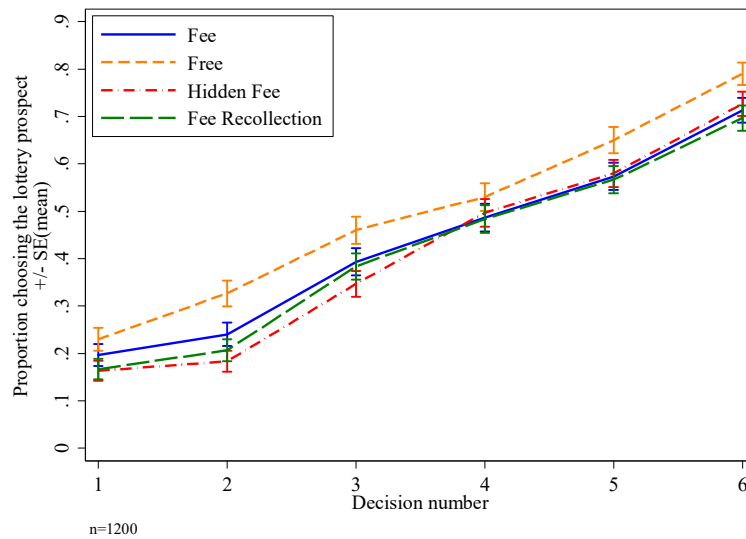
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### 3.8 Appendix

#### 3.8.1 Additional graph from main experiment

Figure 4 Proportion of subjects choosing lottery prospect - all subjects



Note: The figure shows the proportion of subjects in each of the treatments and in each of the decision situations that chose the lottery prospect.

#### 3.8.2 Mturk Instructions

##### 3.8.2.1 Introduction

Welcome to this research project! We very much appreciate your participation.

This is a survey about the economics of decision-making. Several research institutions have provided funds for this research.

Your payment will consist of the participation fee plus the amount of bonus points that you accumulate throughout the study. The exact



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amount of bonus points that you receive will depend on your decisions. At the end, each bonus points is converted into USD at a rate of 1 cent per bonus point.

Your bonus will be paid to you using the bonus system within a few days after the completion of this HIT. Your payment for taking the HIT will be sent to you shortly after the completion of this HIT.

#### **Procedures**

The survey consists of two parts and you will be given instructions on your screen before every single part of the survey. Please always make sure to read the instructions carefully before you continue.

#### **Participation**

Participation in this research study is completely voluntary. You have the right to withdraw at any time or refuse to participate entirely without jeopardy to future participation in other studies conducted by us.

#### **Confidentiality**

All data obtained from you will be kept confidential and will only be reported in an aggregate format (by reporting only combined results and never reporting individual ones). All questionnaires will be concealed, and no one other than the primary investigator will have access to them. The data collected will be stored in the HIPAA-compliant secure database until it has been deleted by us.

#### **Verification**

At the end of this survey, you will be given a completion code. You will need to copy this code to the survey code field on the AMT web page that directed you here at the beginning.

I have read and understood the above consent form and desire to participate in this study.

- Yes
- No

### **3.8.2.2 Part One**

The survey consists of two parts and you will be given instructions on your screen before each part of the survey. Please always make sure to read the instructions carefully before you continue.

In the first part, you will face six decision problems. Each decision problem involves a choice between two prospects, Prospect A and Prospect B.

Prospect A is always a sure payment, while Prospect B always involves a lottery. If you choose Prospect B the program will perform a lottery draw, and you will learn the outcome of this draw by the end of the survey.

Your task is to choose your preferred prospect by clicking on it.

At the end of the survey, one decision will be randomly selected and you will be paid according to the outcome of this decision. Thus, only one of the six decisions will have real payment consequences for you.

Before you start, we would like you to answer a few questions about the survey.

When you are ready, please continue.

How many prospects can you choose between at each decision problem?  
Options 2 – 4.

In total, how many decision problems will you have to decide on?  
Options 1 – 6.

How many of the decision problems will have real monetary payoff consequences for you? Options 0 – 4.

**DECISION PROBLEM 1:**

Your task is to choose your preferred prospect by clicking on it.

Prospect A is a sure payment of 100 bonus points.

Prospect B involves a lottery. With a probability of 50% you will earn 45 bonus points, and with a probability of 50% you will earn 155 bonus points.

Please spend some time to make your decision.

**DECISION PROBLEM 2:**

Your task is to choose your preferred prospect by clicking on it.

Prospect A is a sure payment of 100 bonus points.

Prospect B involves a lottery. With a probability of 50% you will earn 45 bonus points, and with a probability of 50% you will earn 165 bonus points.

Please spend some time to make your decision.

**DECISION PROBLEM 3:**

Your task is to choose your preferred prospect by clicking on it.

Prospect A is a sure payment of 100 bonus points.

Prospect B involves a lottery. With a probability of 50% you will earn 40 bonus points, and with a probability of 50% you will earn 170 bonus points.

Please spend some time to make your decision.

**DECISION PROBLEM 4:**

Your task is to choose your preferred prospect by clicking on it.

Prospect A is a sure payment of 100 bonus points.

Prospect B involves a lottery. With a probability of 50% you will earn 40 bonus points, and with a probability of 50% you will earn 180 bonus points.

Please spend some time to make your decision.

**DECISION PROBLEM 5:**

Your task is to choose your preferred prospect by clicking on it.

Prospect A is a sure payment of 100 bonus points.

Prospect B involves a lottery. With a probability of 50% you will earn 40 bonus points, and with a probability of 50% you will earn 190 bonus points.

Please spend some time to make your decision.

**DECISION PROBLEM 6:**

Your task is to choose your preferred prospect by clicking on it.

Prospect A is a sure payment of 100 bonus points.

Prospect B involves a lottery. With a probability of 50% you will earn 40 bonus points, and with a probability of 50% you will earn 200 bonus points.

Please spend some time to make your decision.

In Problem 1 you selected.

The outcome resulting from your decision is bonus points.

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In Problem 2 you selected.

The outcome resulting from your decision is bonus points.

In Problem 3 you selected.

The outcome resulting from your decision is bonus points.

In Problem 4 you selected.

The outcome resulting from your decision is bonus points.

In Problem 5 you selected.

The outcome resulting from your decision is bonus points.

In Problem 6 you selected.

The outcome resulting from your decision is bonus points.

Recall that one of your decisions is randomly selected and that you will be paid according to the outcome of this decision.

The computer has now randomly selected Decision 1.

Your bonus payment is XX bonus points.

**3.8.2.3 Part Two**

You have completed the first part of the survey. We would now like to ask you a few more questions before we conclude this survey.

How knowledgeable an investor do you consider yourself to be? (5 point scale: Very knowledgeable - Not at all knowledgeable)

Please rate each of the following investments' riskiness on a scale of 1 to 5. (1 indicates "no risk" and 5 indicates "very high risk".)

A large U.S. stock mutual fund

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A savings account at your bank

U.S. corporate bonds

Stable value/money market fund

Stock of a typical Fortune 500 company

An international stock mutual fund

An emerging markets stock mutual fund

A savings account at your bank

What is your gender?

Male

Female

How old are you?

What is the highest level of education you have completed?

Less than High School

High School / GED

Some College

2-year College Degree

4-year College Degree

Masters Degree

Doctoral Degree

Professional Degree (JD, MD)

Finally, if you have any comments or suggestions related to this study please write them down in the field below. Your feedback is very important to improve our research.

You have successfully finished the survey and we thank you for your participation!

We will calculate and pay your bonus as soon as this full batch of HITs is finished. It generally takes us a few days to match the data and pay out the bonuses.

### **3.8.3 Pilot**

We consider a consumer approach to investment decisions. As a pilot study, we look at a situation where people make an investment decision. In this situation, investors buy a product and consume its risk and return; investors may care about other things in addition to monetary gains, such as the thrill of taking risk or trust. If these other aspects give them sufficient extra utility, then they will be willing to pay for them and self-select into buying actively managed funds. The extra fee can trigger people who are sensation seekers (Zuckerman, 1979) to buy actively managed funds because they view it as a more exciting alternative or as a signal of quality, as opposed to index funds.

In the pilot study we run a controlled laboratory experiment to test if the introduction of fees triggers people's risk taking. The task in the pilot is to choose between four prospects. In the treatment, a fee is deducted from the earnings if the subject chooses one of the two riskiest prospects (those with the highest standard deviation). In the control, all prospects are free to choose. We find that subjects in the treatment do not choose significantly different from subjects in the control, which means that the cost does not matter. Testing if a difference in risk-preference can explain the lack of differences between the treatment and control using

a paid risk-elicitation task<sup>21</sup> shows no difference between the two groups. Therefore, different risk-preferences cannot explain the lack of difference between the two groups. Another possible explanation is that fees are ignored by subjects in the treatment. However, the pilot study is neither able to distinguish whether the fee is ignored, nor whether the fee introduces some excitement. We therefore investigate the ignoring of fees further in the main experiment.

### 3.8.3.1 Pilot Design and Procedure

The main task in the pilot was to choose one of four boxes. Each of the boxes contained a different prospect, all with the same expected value. Box A has a risk-free prospect and the other three boxes contained risky prospects with increasing standard deviations. This is summarized in Table 9.

Table 9 Box Choices

Box	Control		Treatment		
	Low	High	Low	High	Cost
A	200	200	200	200	0
B	180	220	180	220	0
C	120	280	120	280	10
D	50	350	50	350	10

Note: The table shows the payoffs associated with each box.

The pilot took place at the University of Stavanger on the 30th of November, and on the 3rd and 4th of December in 2015. The experiment was computerized using Z-tree (Fischbacher, 2007), which displayed all the information that subjects received. We recruited 118 students from the University of Stavanger through e-mail a week in advance, where we told them they could earn a decent hourly wage by participating in an

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<sup>21</sup> Subjects were asked to choose between six lotteries of increasing expected value and standard deviation.



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economic experiment. Students were free to register for the session they wanted, as long it was not already fully booked. Each day allowed for four sessions, which allowed us to run both treatment and control each day. The average age was 24.5 years old, and the oldest and youngest were 42 and 18 years old, respectively.

Table 10 Total number of subjects

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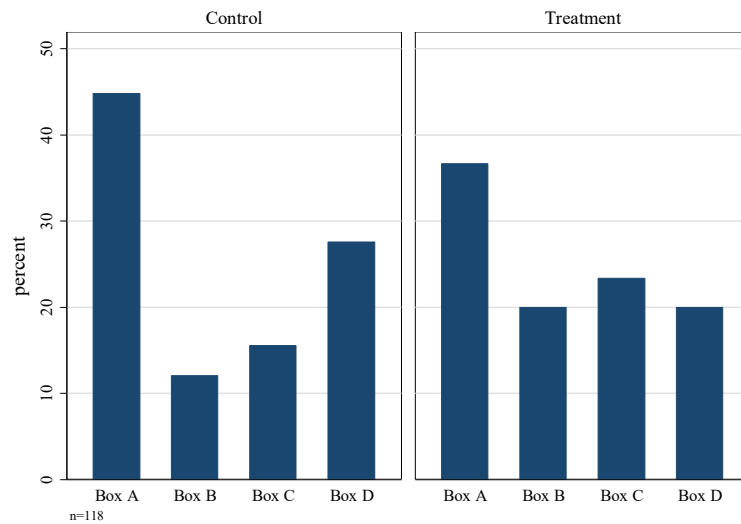
	<b>Number of subjects</b>
Control	58, (60% male)
Treatment	60, (60% male)

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When the participants came to the lab they were seated in cubicles so that other participants could not see their screen. When the session started, written instructions were read aloud and these were also given in writing. Then the subjects made their decisions on the computer using zTree (Fischbacher, 2007). After the subjects had chosen a box, they were faced with a risk-elicitation task, which was to choose between six lotteries of increasing expected value and standard deviation. Lastly, the subjects completed a survey about financial literacy and demographics.

### 3.8.3.2 Pilot Results

Figure 5 Box choices in pilot



Note: The figure shows the percent in each group that chose the different boxes.

From the graph above, we see that for all the boxes there is a level difference in the treatment and control. In both the treatment and control, box A is the most popular option, followed by box D for the control and box C for the treatment. Pairwise comparisons using Mann-Whitney U tests show that none of the choices between the treatment and control are statistically different. Results are summarized in Table 11.

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Table 11 Choices in treatments

Box	Control	Treatment	Test of equal proportions between Control and Treatment	
			z	p
A	26	22	-0.90	0.367
B	7	12	1.17	0.241
C	9	14	1.07	0.284
D	16	12	-0.97	0.333
Total	58	60		

Note: The table shows the number of subjects in each group that chose the different boxes. In addition, the table shows the results from tests of equal proportions which test if the proportions in the two groups are different from each other.

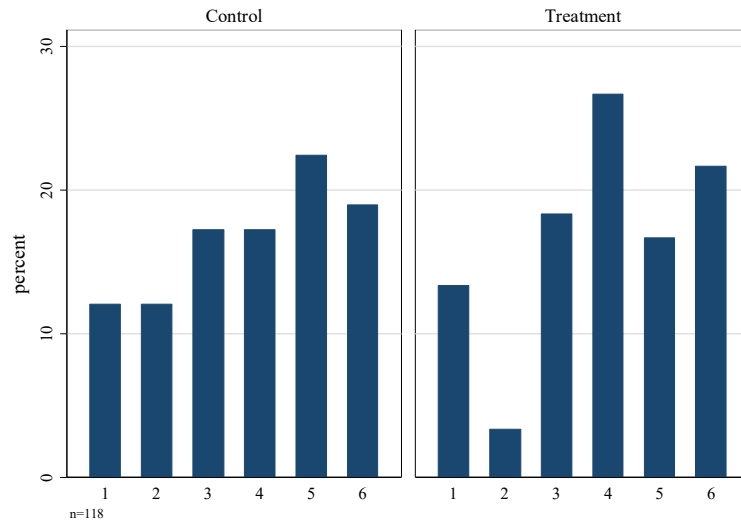
As mentioned in the pilot design and procedure, we included a lottery choice which allows us to control for subjects' risk preference.

Both of the graphs of the lottery choice are skewed to the right, testing whether one group selecting higher lotteries using a Mann-Whitney test shows no statistical difference between the two groups ( $Z=0.381$ ,  $p=0.703$ ); in other words, the overall risk preference is the same in the two groups.

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Figure 6 Lottery Choices in Pilot



Note: The figure shows the percent, in each of the two groups, that chose the different lotteries in the risk-elicitation task.

## **4 Effort Provision in a Game of Luck**

Mads N. Arnestad, Kristoffer W. Eriksen, Ola Kvaløy and Bjørnar Laurila

### **4.1 Abstract**

In some jobs the correlation between effort and output is almost zero. For instance, money managers are primarily paid for luck. We investigate, by the use of a controlled lab experiment, under which conditions workers are willing to put in effort even if output (and thus employer's earnings) is determined by pure luck. We vary whether the employer can observe the workers' effort, and whether the employer knows that earnings are determined by luck. We find that workers believe that the employer will reward effort even if effort does not affect earnings. Consequently, workers work harder if the employer can observe their (unproductive) effort. Moreover, we find that if the employer only sees earnings and not effort, workers work harder if the employer does not know that earnings are determined by luck. The latter effect is driven by female workers and suggests that (female) workers work hard in order to avoid undeserved rewards.

### **4.2 Introduction**

In most types of work, increased effort will lead to improved results. However, in some jobs the relationship between effort and outcome is almost zero. For instance, the performance of many money managers is mostly a measure of luck (Bhootraa, Dreznerb, Schwarzc, & Stohsd, 2015; Fama & French, 2010; Malkiel & Fama, 1970; Pástor, Stambaugh, & Taylor, 2017). A substantial body of research suggests that although some investors do outperform their relevant indexes, there is little evidence that suggests that effort is what sets successful money managers apart from unsuccessful ones. Money managers' effortful behavior may

even be negatively related to performance.<sup>22</sup> Also in other jobs, the correlation between effort and performance can be quite small. Effort may be positively correlated with observable outcomes, but these outcomes are also a function of random events outside the workers' control.

Given a very low and possibly negative relationship between effort and performance, one might think that e.g. money managers work very little. However, evidence suggests that they put substantial amounts of effort into their work, both in terms of time and dedication (Michel, 2014). These two sets of seemingly incompatible observations lead us to pose the question: *What motivates effort in a game of pure luck?*

Now, if effort is *believed* to be positively correlated with performance, and if high levels of noise are compensated by high-powered incentives like tournament theory predicts (Lazear & Rosen, 1981), then people work hard even in very noisy environments where luck is important. But what if the workers know that their effort does not help performance? Will they still work hard? We investigate this in a laboratory experiment. That is, we investigate under which conditions workers are willing to put in effort, even if output (and thus employer's earnings) is determined by pure luck. We vary whether the employer - who rewards the workers - can observe the workers' effort, and whether the employer knows that earnings are determined by luck.

Standard economic theory predicts that the workers will not exert effort in any of the conditions we investigate. However, we propose a moral

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<sup>22</sup> Firstly, transactional activity is negatively related to outcomes, because transaction costs tend to outweigh the gains associated with the trades. Moreover, paying close attention to the market may be associated with more frequent and more myopic transactions. This effect is prolific among both students and professional investors (Gneezy, Kapteyn, & Potters, 2003; Gneezy & Potters, 1997; Haigh & List, 2005; Thaler, Tversky, Kahneman, & Schwartz, 1997), and has been demonstrated in both lab and field experiments (Larson, List, & Metcalfe, 2016).

psychology account to explain when and why people will exert effort in a game of pure luck. Moral psychology relies upon three normative ethical theories as a point of departure for moral judgement: *consequentialist ethics*, whereby the moral value of an action is evaluated on the basis of its material outcomes; *deontological ethics*, whereby the moral value of an action is judged on the basis of rules, duties and obligations; and *virtue ethics*, in which the individual, not the action, is the unit of moral evaluation (Uhlmann, Pizarro, & Diermeier, 2015). Consequentialist theories of ethics hold that an act is permissible only if it maximizes good outcomes on quantifiable metrics. Typical examples include maximizing welfare and flourishing, minimizing resources spent, and maximizing lives saved (Smart & Williams, 1973). Consequentialist ethics provide little in terms of explaining effortful work in a game of pure luck. In such a game the efforts of the worker are, by definition, unrelated to outcomes. The deontological theories state that an action is right or wrong based on whether it violates a set of rules, duties and obligations that are seen as foundational to morality (Kant, 1785 / 2012). According to this view, an action can be wrong despite bringing about good consequences, and right despite bringing about bad consequences. Some actions may also be duty-bound even if they are unrelated to outcomes. So, in a game of luck a worker should work hard if she adheres to a moral norm that states that hard work is an obligation in and of itself, regardless of its efficacy. Based on this assertion, we hypothesize that *even when effort is unobservable to employers and unrelated to outcomes, most workers will choose to exert some effort (H1)*.

The worker may also be motivated to work hard if she expects her manager to adhere to a social norm of hard work. Under such conditions the worker may expect that effort will be rewarded, irrespective of output. This latter point is related to the third ethical theory that informs moral judgement: virtue ethics. Virtue ethics is less concerned with evaluating actions, and more concerned with evaluating people and

whether or not they possess moral traits. A growing body of psychological research suggests that people intuitively and automatically make inferences about people's moral traits (Fiske, Cuddy, & Glick, 2007; Todorov & Uleman, 2003; Willis & Todorov, 2006). This tendency is observed across cultures (Choi & Nisbett, 1998; Lieberman, Jarcho, & Obayashi, 2005) and even in young children (Hamlin, Wynn, & Bloom, 2007). Observing effort is especially salient in judgements of virtue. Effort influences people's perception of the worker's goals, intentions and moral character (Pizarro & Tannenbaum, 2011; Uhlmann et al., 2015). According to Heider's classical work (1958 / 2013), exertion of effort (i.e. how hard the person is trying to do something) signals a worker's motivational force and the relative importance of the goal to the worker. Later research has supported the assertion that people infer goals from effortful behavior (Hassin, Aarts, & Ferguson, 2005). Many studies have highlighted the mediating role of attribution of motivation in the judgement of moral character (Reeder, 2009; Reeder, Kumar, Hesson-McInnis, & Trafimow, 2002; Reeder & Spores, 1983). The more effort exerted by the worker, the more likely perceivers are to make inferences about the goal of the worker (Dik & Aarts, 2007). Furthermore, the more effort a worker exerts in pursuing a goal, the more people perceive that goal as important to the worker (Bigman & Tamir, 2016; Dik & Aarts, 2008). Thus, as long as the goal is seen as morally good, increased effort leads to improved judgements of moral virtue (e.g., Cushman, 2008). If it is "the thought that counts" (Rand, Fudenberg, & Dreber, 2015) and effort is taken as a proxy for that thought, then workers should be motivated to work hard in a game of luck in order to demonstrate their virtue. We thus hypothesize that *when the employer can observe efforts, workers will work harder and expect to be rewarded for working hard (H2)*.

Our first two hypotheses outline that both "inward-focused" deontological ethics and "outward focused" virtue ethics may motivate effort in a game of luck, and that the virtue ethics would provide the most



powerful motivation. However, it is less obvious whether employees will work hard if the role of luck is common knowledge, i.e. when both the employer and the workers know that effort is unrelated to earnings. Under these conditions, it is possible to argue for competing predictions with regards to worker's beliefs and consequent effort. On the one hand, a worker may believe that the employer will be unimpressed with efforts that are explicitly unproductive. If this is the case, the worker may assume that higher efforts will fail to elicit higher compensation from the employer. The worker may even suspect that the employer will punish unproductive efforts and provide *lower* compensation for *higher* efforts, as the employer may see it as his job to correct misguided behavior through reductions in compensation. On the other hand, it is conceivable that workers will rely on a common work-ethic heuristic, in which even explicitly unproductive efforts will be rewarded by the employer. Past research has demonstrated that the link between effort and judgements about moral virtue is unrelated to outcomes (Bigman & Tamir, 2016). However, this effect has never been tested in a setting in which the lack of relationship between effort and earnings is common knowledge. Our hypothesis is that workers will rely on the virtuous heuristic and expect that high effort will be rewarded, even when it is common knowledge that effort does not help performance. We therefore formulated our third hypothesis: *Even when the lack of relationship between effort and earnings is common knowledge, workers will expect employers to reward effort, and consequently work hard (H3).*

Lastly, we investigate the role of potential “undeserved rewards” in worker's effort provisions. When effort is unobservable, and the role of effort and luck is *not* known by the employers, workers may worry about being given “undeserved rewards”; that is, rewards that the worker believes would not have been given had the employer been informed about the lack of correlation between effort and luck. It is natural for the worker to believe that the employer, when not given any prior information, will assume that whatever earnings are produced will at

least be partly related to the worker's effort level. And if the earnings are substantial, it is natural for the worker to expect a substantial reward. This setting may be uncomfortable to some workers, who may feel negatively about being rewarded for earnings they did not cause. In such an event, a form of inaction aversion could materialize (see Anderson, 2003), whereby workers work hard as a way to avoid the negative feeling of being rewarded for something they did not earn. Having exerted effort, even if it was unproductive, may make whatever reward "taste better" to the employee. We therefore propose our fourth hypothesis: *When effort is not observable to employers, workers will work harder when employer is unaware of the lack of relationship between effort and earnings (H4).*

Our experimental results give support to all of our four hypotheses: 1. Most subjects exert positive effort even when effort is unproductive, 2. They exert more effort when effort is observable, 3. They expect employers to reward effort even if it is common knowledge that output is determined by luck, and 4. In the case where effort is unobservable, they work harder if the employer does not know that earnings are determined by luck. The latter results are driven by female workers.

To the best of our knowledge, these results are novel. There is a growing literature on how people reward luck vs effort, see e.g. Cappelen, Moene, Skjelbred, and Tungodden (2017) and Cappelen, Sørensen, and Tungodden (2013). However, in contrast to this literature, we focus on the workers: How do they expect their effort to be rewarded when luck is decisive, and how does this affect effort provision. A few papers investigate the effect of noise on effort under various incentive systems, see Sloof and van Praag (2010), Eriksen, Kvaløy, and Olsen (2011), (Delfgaauw, Dur, Sol, & Verbeke, 2013), Rubin and Sheremeta (2015) and Corgnet and Hernán-González (2018). However, no one has investigated effort provisions in environments where noise is everything, i.e. when effort is completely unrelated to earnings and purely determined by luck.

The rest of the paper is organized as follows: In Section 2 we present Experimental Design and Procedure, in Section 3 we present the results, while Section 4 concludes.

### **4.3 Experimental design and procedure**

We investigate in a setting where output is purely random, how a worker's decision to put in effort is affected by the effort's observability. We also test whether this decision to put in effort is affected by the employer's knowledge of the output's cause.

There are two types of players in the experiment: workers and employers. The workers work individually in pairs on behalf of the employer on a real effort task. For each worker, a random draw made by the computer determines the worker's output from the working period. This is done after the working period is over so that the workers do not know their output before they start working. After the output is drawn, the output is converted into real money and added together with the money from the other worker in the worker pair. This sum is transferred to the employer, who is tasked with distributing two-thirds of the money between the two workers, while keeping the last third for herself. We will explain below what information the employer has when making the distribution. The complete set of instructions can be found in the appendix.

The working period lasts for 20 minutes. The real effort task is to decode a random string of ten letters into a sequence of ten numbers, using a code sheet that lists the letters and their numbers. All numbers have to be correct in order to move on to the next string, and there is an infinite number of strings for the worker to decode. Workers can decide the amount of effort they want to put in and decode as many or as few strings as they want. We use the number of strings a worker decodes as a measure of that worker's effort.

*Example of the real effort task:*

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A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z
8	12	14	10	9	6	24	22	7	5	11	3	18	1	21	16	23	2	13	19	25	4	26	17	20	15

Decode the following letters: A | E | H | Q | J | M | R | Z | T | W

The correct numbers are: 8 | 9 | 22 | 23 | 5 | 18 | 2 | 15 | 19 | 26

#### 4.3.1 Treatments

The workers always observe their own effort and always know that output is random. Workers also know what information is available to the employer prior to the working period. The treatment variations are based on what information the employer has available when distributing the reward. The employer always sees the individual worker's output when making the distribution, but knows only in two of the treatments that the output comes from a random draw made by the computer. This lets us test the effect of both an informed and uninformed employer on workers' effort. The other aspect we want to investigate is the effect of effort being observable. We therefore have two treatments where the employer sees the individual worker's effort and two treatments where the employer does not see effort. Table 1 summarizes the design.

Table 1 Treatments

	Cause of output is common knowledge (Luck known)	Only workers know the cause of output (Luck unknown)
<b>Effort is observable to employer</b>	EOLK = Effort Observable, Luck Known	EOLU = Effort Observable, Luck Unknown
<b>Only workers know effort. Effort is unobservable to employer</b>	EUKL = Effort Unobservable, Luck Unknown	EULU = Effort Unobservable, Luck Unknown

These two dimensions, information about cause of output and observability of effort, make for a two-by-two design with four

treatments: full information, not see effort, not know cause, and no information. In the *full information* treatment, the employer knows the cause of output and sees the workers' effort. In the *not see effort* treatment the employer knows the cause of output, just as in the full information treatment, but does not see the workers' effort. In the *not know cause* treatment the employer sees the workers' effort but is unaware of the fact that effort does not affect output. In the last treatment, *no information*, the employer does not receive information about the cause of output and does not see the workers' effort, but only sees the sums of money the workers generate.

#### **4.3.2 Procedure**

We ran 18 sessions in two batches with 255 participants in 2017, the first batch in June with 11 sessions and the second batch in August with 7 sessions. Each session had a maximum of 23 participants and only one treatment. The participants were Norwegian-speaking students at the University of Stavanger who were recruited through an e-mail sent to all students at the university. When the participants entered the lab, they drew a number from a cup that determined their place in the lab and subsequently their role; this also acted as a salient randomization device. An experimenter then read aloud general instructions informing about the rules and the participants had 10 minutes to read the printed instructions carefully before the z-Tree (Fischbacher, 2007) program started. The written instructions contained information about the game, but these were not read aloud due to the nature of the treatments. To verify that they had understood the instructions, workers had to answer correctly four true or false questions about the design. Then beliefs about how the employer would distribute rewards were elicited. The workers then worked for 20 minutes. Although there was no way for the workers to know the identity of their partner or how much effort their partner put in, the sound from keyboards being used did give a clue as to how much

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effort the other workers in the lab were putting in<sup>23</sup>. After the working period, workers saw their effort and learned their output, drawn by the computer. When the employer made the distribution, we elicited the same beliefs as before the working period to test if beliefs had changed during the working period. We also asked the workers how they thought the employer should distribute the money.

Table 2 Background characteristics

Treatment	Age		Female		Grade		N
	Mean	SE	Mean	SE	Mean	SE	
Effort observed luck known	23.86	0.54	0.59	0.06	3.59	0.09	59
Effort unobserved luck unknown	24.50	0.60	0.45	0.07	3.29	0.10	58
Effort observed luck unknown	24.59	0.95	0.57	0.07	3.36	0.07	56
Effort unobserved luck known	24.31	0.89	0.64	0.06	3.22	0.08	64
All	24.21	0.38	0.57	0.03	3.36	0.04	237

Note: The table presents background characteristics of subjects in the experiment by treatment. “Age” is a variable measuring subjects’ age in years; “Female” presents the proportion of females; “Grade” measures self-reported average grade, ranging from 0 (=F) to 5 (=A).

There was only one employer in each session. This employer was asked to distribute the money between the two workers for all the pairs in that session. We made the decision to have only one employer in each session because workers’ behavior was our primary interest. We did not communicate to the workers that one employer was responsible for all the pairs<sup>24</sup>, only that they worked for a participant that was randomly selected to be an employer. When distributing the money, the employer saw the sums of money the workers had generated, and depending on the

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<sup>23</sup> All the computer workstations were upgraded during the summer, so the fall sessions had different keyboards that made marginally less noise.

<sup>24</sup> Excerpt from workers’ instructions: “There are two types of players in this experiment, employers and workers. You have been randomly selected to be a worker...”. “A third participant has been randomly chosen to be an employer”. “You and the other worker are invited to work individually on a task on behalf of the employer”. See appendix for full instructions.

treatment, the number of strings each worker had decoded. The employer could not take money from one pair and distribute it to another pair; the whole amount of the 2/3 from the pair had to be distributed between the two workers in that particular pair for the computer to accept the distribution. After the employer had made all the distributions, the computer randomly selected one pair from which the employer received payment. The sessions lasted approximately 50 minutes and average payment was approximately NOK 230.

#### **4.4 Results**

Our experiment consists of both employers and workers, but our main research question pertains to what motivates effort when effort is unproductive; thus, the following main analysis focus on the beliefs and behavior of workers. We start by looking at workers' beliefs, and importantly to what extent workers believe that effort will be rewarded by the employer. Although effort is unproductive in the sense that it does not affect earnings to the employer, it could affect earnings for the worker.

Table 3 (and Figure 1) presents responses to the following statement: *The employer will give the most money to the worker who has solved most decoding tasks during the working period.* First, we see that when the employer can observe effort, the majority of workers believe that effort will be rewarded. This is true irrespective of whether the employer is informed about the role of luck, or not. By looking at the combination of columns 5 and 6 in Table 3, respectively, we find no significant differences in the frequency of subjects that believe that effort will be rewarded (or are sure that effort will be rewarded), comparing EOLK (59.3%) and EOLU (64.3%) (Test of equal proportions,  $z=-0.55$ ,  $p=0.58$ ). In the two treatments where effort is not observable for the employer, the majority of subjects correctly believe that the employer will not reward effort. Looking at row three and row five in Table 3, we see that 93% (EULK) and 89% (EULU) of the subjects are either neutral,

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do not believe effort will be rewarded, or are sure that effort will not be rewarded, respectively. While the responses in Table 3 for EULK and EULU indicate that most workers understand that the employer cannot be affected by effort when deciding on worker earnings, four workers in EULK and seven workers in EULU still indicate that they believe that effort will be rewarded by the employer.<sup>25</sup>

Table 3 Belief about whether the employer will reward effort

	<b>0 = sure that it will not happen</b>	<b>1 = do not believe it will happen</b>	<b>2 = neutral</b>	<b>3 = believe it will happen</b>	<b>4 = sure that it will happen</b>	<b>N</b>
Effort observed luck known (EOLK)	0 (0.00%)	10 (16.95%)	14 (23.73%)	30 (50.85%)	5 (8.47%)	59
Effort unobserved luck known (EULK)	34 (58.62%)	11 (18.97%)	9 (15.52%)	3 (5.17%)	1 (1.72%)	58
Effort observed luck unknown (EOLU)	0 (0.00%)	8 (14.29%)	12 (21.43%)	24 (42.86%)	12 (21.43%)	56
Effort unobserved luck unknown (EULU)	18 (28.13%)	16 (25.00%)	23 (35.94%)	6 (9.38%)	1 (1.56%)	64
<b>Total</b>	<b>52 (21.94%)</b>	<b>45 (18.99%)</b>	<b>58 (24.47%)</b>	<b>63 (26.58%)</b>	<b>19 (8.02%)</b>	<b>237</b>

Note: The table presents the frequencies (percentages) of the statement: “The employer will give the most money to the worker who has solved the most decoding tasks during the working period”. The responses are measured on a Likert scale from 0 (sure that it will not happen) to 5 (sure that it will happen). The rightmost column gives the number of subjects.

Figure 1 Beliefs: The employer will give the most money to the worker who has solved most decoding tasks during the working period

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<sup>25</sup> One can only speculate why these subjects present beliefs that are at odds with the information given about the employer information set. However, for the analysis we will include these workers if not stated otherwise.



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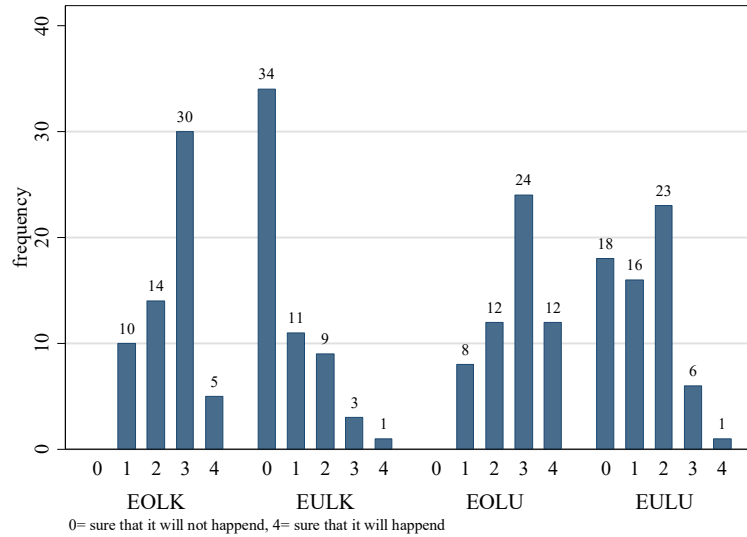


Table 4 supports the findings above. Making use of Mann-Whitney U-tests we find that workers believe that the employer will reward effort when effort is observable to the employer, but not when effort is unobservable. And importantly, results point in the direction that when effort is observed by the employer, workers' beliefs are insensitive to employers' knowledge about the role of luck, as shown by the insignificant difference between EOLK and EOLU. However, though the majority of workers believe that effort will not be rewarded in EULK and EULU, workers tend to be more confident if the employer has knowledge about the role of luck compared to when the employer is uninformed on the role of luck. This can be seen in rows three and five in Table 3, where more workers believe the employer will not reward effort in EULK compared to EULU. Also, the difference in responses is significant, as presented in Table 4, comparing EULK and EULU.

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Table 4 Comparison of belief about whether the employer will reward effort

	<b>Effort unobserved luck unknown EULU</b>	<b>Effort observed luck unknown EOLU</b>	<b>Effort unobserved luck known EULK</b>
<b>Effort observed luck known EOLK</b>	7.47***	-1.23	5.90***
<b>Effort unobserved luck unknown EULU</b>		-7.62***	-3.31***
<b>Effort observed luck unknown EOLU</b>			-6.23***

Note: The table presents z-values from Mann-Whitney U-tests comparing responses to the statement: “The employer will give the most money to the worker who has solved most decoding tasks during the working period”. Responses are reported on a Likert scale from 0 (sure that it will not happen) to 5 (sure that it will happen). \*:p<0.10, \*\*:p<0.05, \*\*\*:p<0.01.

We now present how workers actually behave in the experiment, i.e what effort they provide. First, workers provide on average positive effort in all treatments despite effort being unproductive. This is shown in Table 5, which presents relevant summary statistics. Our experiment is designed to distinguish between two main aspects that may motivate effort. The first is effort observability, the other is employer knowledge about the relationship (or lack of relationship) between effort and outcome. The distribution of effort is presented in Figure 2. Here we see that only four workers chose to refrain from providing effort, and these workers all participated in treatments where effort was not observed by the employer.

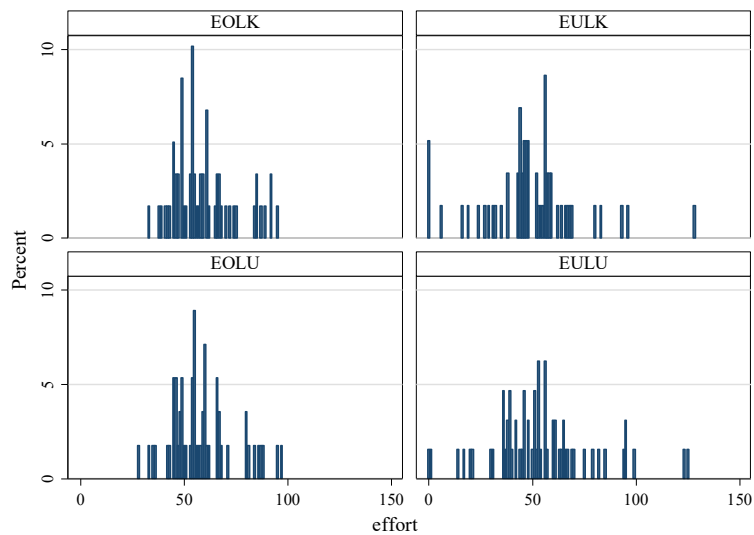
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Table 5 Summary Statistics: Effort provisions

	Mean	SD	Median	Min	Max	N
Effort observed luck known EOLK	59.2	14.8	55	33	95	59
Effort unobserved luck unknown EULU	49.0	22.5	48	0	128	58
Effort observed luck unknown EOLU	58.4	15.4	55	28	97	56
Effort unobserved luck known EULK	53.8	24.1	53	0	125	64
Effort observed	58.8	15.0	55	28	97	115
Effort unobserved	51.5	23.4	51	0	128	122
Luck known	54.1	19.7	54	0	128	117
Luck unknown	56.0	20.5	54	0	125	120

Note: The table presents summary statistics of effort provisions in the experiment for each treatment. In addition, we also include combinations of treatments. Here *Effort observed* (*Effort unobserved*) consists of the two treatments where effort is observable (unobservable). *Luck known* (*Luck unknown*) consists of the two treatments where the role of luck was known (unknown) to the employer.

Figure 2 Distribution of Effort in treatments



A second observation is that workers work harder if the employer can observe their effort (compared to when the employer does not see effort). As shown in Table 5, and also supported by Mann-Whitney U-tests in Table 6, effort provision in the two treatments where effort is observed by the employer is significantly different from the two treatments where the employer cannot see effort.<sup>26</sup> In the lower part of Table 5 we also combine the two treatments where effort is observed by the employer, and compare them to the two treatments where effort is not observed by the employer. Again, we find significantly higher effort when the employer can observe effort, with a mean effort of 58.84 (median of 55) compared to 51.51 (median of 51) for the combination of treatments where effort is not observed by the employer (Mann-Whitney U-test,  $z=-3.25$ ,  $p<0.01$ ). Thus, effort provision is clearly affected by whether the employer gets to observe effort, or not. However, even when effort is unobservable to employers, most workers will choose to exert effort. This is consistent with norms advocating that hard work is a moral duty and an obligation in and of itself.

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<sup>26</sup> The Shapiro-Wilk W test for normality shows that *effort* is not normally distributed. Thus, we make use of the Mann-Whitney U-test when we compare effort between treatments (t-tests give qualitatively similar results).

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Table 6 Comparison of effort

	Effort unobserved	Effort observed	Effort unobserved	Effort unobserved	Luck unknown
	luck unknown	luck unknown	luck known		
	EULU	EOLU	EULK		
Effort observed luck known	2.97***	0.21	1.92*		
Effort unobserved luck unknown		-2.74***	-0.91		
Effort observed luck unknown			1.61		
Effort observed				-3.25***	
Luck known					0.46

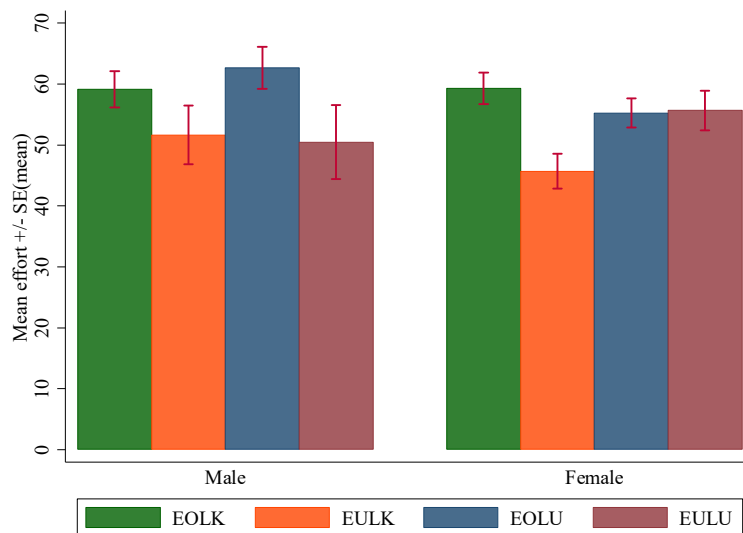
Note: The table presents z-values from Mann-Whitney U-tests. \*:p<0.10, \*\*:p<0.05, \*\*\*:p<0.01. In addition, we also include combinations of treatments. Here Effort observed (Effort unobserved) consists of the two treatments where effort is observable (unobservable). Luck known (Luck unknown) consists of the two treatments where the role of luck was known (unknown) to the employer.

Another observation, which is also consistent with the workers' beliefs presented above, is that employers' knowledge about the role of luck does not affect effort provision at all, given that effort is observable. Mean effort in EOLK is 59.24, while it is 58.43 in EOLU. This difference is not significant (Mann-Whitney U-test, z=0.21, p=0.84). Hence, even when the employer is informed that effort is unproductive (in the sense of not affecting earnings), workers still provide as much effort as when the employer only observes effort and does not know that the effort is unrelated to earnings. One interpretation of this is that effort provision is used as an important signaling device informing the employer that the worker is "deserving" and a virtuous worker.

In contrast, when effort is not observed by the employer, the role of luck seems to matter. Comparing EULK and EULU, which are the two

treatments where effort is not observed by the employer, we see from Table 6 that workers work harder when the employer also does not know that earnings are determined by luck. While the mean effort in EULU is 10% higher than in EULK, the difference is not statistically significant. However, the effort difference seems to be driven by females, suggesting that females work hard to avoid underserved rewards. Splitting our data by gender, we see from Figure 3 that effort provision from females is clearly higher in EULU, compared to in EULU. This difference is statistically significant at the 10% level (Mann-Whitney U-test; 55.68 vs. 45.69;  $z=-1.74$ ,  $p=0.08$ ). In Appendix B we present summary statistics of effort for females, and corresponding tests of significance.

Figure 3 Mean effort in treatments for both genders



#### 4.4.1 Regressions

We run regression analysis to check the robustness of the results from the non-parametric test. Also, recall that Table 2 indicated that the dataset regarding both gender and the subjects' grades were not perfectly

balanced.<sup>27</sup> Thus, in the following regression analysis we include: *Age*, which is a continuous variable measuring the subject's age; *Female*, which is an indicator variable for gender; and *Grade*, which is an ordinal variable measuring self-reported average grade, ranging from 0 (= F) to 5 (= A). To indicate our different treatments we use two dummy variables: *Effort observed* is equal to 1 if the employer gets to observe the worker's effort, zero otherwise. *Luck unknown* is a dummy variable equal to 1 if the role of luck is unknown to the employer, zero otherwise. Lastly, we include the interaction variable *Effort observed x Luck unknown*. This interaction variable alone presents the difference-in-difference coefficient for our treatments; that is, the difference in effort between EOLK and EULK and between EOLU and EULU. The reference group in our models is the condition where effort is unobserved by the employer, but where the employer is aware that workers are participating in a game of luck (EULK).

Regression results are found in Table 7. From model 3 we see that Grade is positive and significant. This means that workers with better grades tend to exert more effort. However, we do not find treatment differences concerning effort provision and grades. So, even though grades affect effort, the effect is not different between treatments. We also find that older workers exert less effort, as can be seen by the negative and significant coefficient for *Age*. More interesting are the dummy variables determining our treatments in model 1. Here we see that *Effort observed* is positive and significant, informing us that workers exert significantly more effort when effort is observable by the employer. The coefficient for *Observed Effort* alone gives the comparison of EOLK and EULK, while combining all coefficients ( $10.27 + 4.85 - 5.65 = 9.47$ ) gives the

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<sup>27</sup> See Table A1 in Appendix B for non-parametric tests of differences in the background variables between treatments.

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comparison of EOLU versus EULK.<sup>28</sup> Thus, controlling for worker's age and academic performance, the regressions in Table 7 support the results from the previously presented non-parametric tests; namely, that workers work harder if the employer can observe their unproductive effort.

Table 7 OLS Regressions over effort provisions

	<b>1</b>	<b>2</b>	<b>3</b>
Observed effort	10.272*** (3.659)	10.545*** (3.682)	9.005** (3.682)
Luck Unknown	4.847 (3.587)	5.209 (3.626)	5.471 (3.583)
Observed effort * Luck Unknown	-5.656 (5.147)	-6.059 (5.183)	-5.015 (5.131)
Female		-1.884 (2.623)	-1.955 (2.590)
Age			-0.394* (0.217)
Grade			4.331** (1.887)
Constant	48.966*** (2.598)	49.810*** (2.854)	40.897*** (9.753)
$R^2$	0.041	0.043	0.075
F	3.337	2.626	3.129
Observations	237	237	237

Note: Observed effort, Luck Unknown and Observed effort \* Luck Unknown are dummy variables. Female is a dummy variable indicating gender. Age is measured in years. Grade is the subjects' self-reported grade point average. Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

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<sup>28</sup> An F-test on whether the coefficients are jointly equal to zero is rejected: Wald test:  $F(3, 233) = 3.34, p=0.02$ .



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Neither *Luck unknown* nor the interaction variable are significant, indicating that the employer's knowledge about the role of luck is of less importance for the workers when they decide on effort provision. However, as suggested by the analysis above, male and female workers differ in their response to whether the employer knows it is a game of luck, or not. In Table 8 we investigate this further, and present OLS regressions for male and female workers separately. Focusing on male workers first (model 5 and model 7), we see from the insignificant coefficient *Luck unknown* that there is no difference in effort provision between EULK and EULU. The same is true when effort is observed, and when we compare the two treatments with and without employer knowledge about the role of luck (EOLK $\approx$ 59 and EOLU $\approx$ 63, Wald test:  $F(1, 99) = 0.28, p=0.60$ ). Finally, we look at how effort observability affects male workers. As shown by the coefficient *Observed Effort*, the difference between EOLK and EULK is not significant, while when we compare EOLU( $\approx$ 63) and EULU( $\approx$ 50), we find a significant difference at the 10 percent level (Wald test:  $F(1, 99) = 3.24, p=0.07$ ). So, our results indicate that male workers are affected by employer observability of effort when the employer is not informed about the role of luck.

Next, we turn to female workers. Looking at Model 4, we see that female workers are significantly affected by whether the employer can observe effort or not. In EOLK, female workers solve significantly more decoding tasks than they do in EULK, as shown by the positive coefficient of 13.62 for *Effort observed*. We also find that female workers in EOLU solve close to 10 ( $13.62+9.99-14.5=9.56$ ) more decoding tasks, compared to what they do in EULK. This difference is also significant (Wald test:  $F(3, 130) = 3.47, p=0.02$ ).

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Table 8 Effort OLS regression split on gender

	(4) Female	(5) Male	(6) Female	(7) Male
Observed effort	13.622*** (4.323)	7.500 (6.266)	12.796*** (4.136)	6.734 (6.579)
Luck Unknown	9.991** (4.186)	-1.147 (6.344)	11.303*** (4.008)	-0.989 (6.461)
Observed effort * Luck Unknown	-14.055** (5.848)	4.688 (9.226)	-13.260** (5.584)	4.961 (9.366)
Age			-0.543*** (0.206)	0.003 (0.520)
Grade			5.822*** (1.996)	1.720 (3.604)
Constant	45.692*** (3.275)	51.625*** (4.102)	32.952*** (10.306)	44.283** (18.966)
$R^2$	0.074	0.047	0.171	0.049
F	3.470	1.612	5.262	0.997
Observations	134	103	134	103

Note: Observed effort, Luck Unknown and Observed effort \* Luck Unknown are dummy variables. Female is a dummy variable indicating gender. Age is measured in years. Grade is the subjects' self-reported grade point average. Standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Interestingly, the positive and significant coefficient for *Luck unknown* shows that female workers work harder if they are paired with an employer that does not observe effort, and *also* does not know the role of luck (EULK $\approx$ 45 versus EULU $\approx$ 45+10). This result indicates that female workers are sensitive to whether the employer knows that it is a game of luck, or not. However, we only find evidence for such an effect when effort is unobserved by the employer. When the employer can see effort provision, the effort observability effect seems to dominate and we observe no significant difference between the treatments where the employer is informed or not about the role of luck (EOLK $\approx$  59 and EOLU $\approx$ 55, Wald test:  $F(1, 130) = 0.99, p=0.32$ ). One interpretation of

this result is that for female workers, effort observability is less effective when the employer does not know that it is a game of luck. This suggests that females work hard in order to avoid undeserved rewards. Another way of looking at this is by the significant interaction term, which informs us that the difference in effort between EOLK and EULK is different from the difference in effort between EOLU and EULU.

#### **4.5 Conclusion**

This paper presents results from a controlled lab experiment investigating under which conditions workers are willing to put in effort, even if output (and thus employer's earnings) is determined by pure luck. We vary whether the employer can observe the worker's effort, and whether the employer knows that earnings are determined by luck. Standard economic theory predicts that the workers will not exert effort in any of the conditions we investigate. However, we propose a moral psychology account to explain when and why people will exert effort in a game of pure luck: deontological ethics, whereby the moral value of an action is judged on the basis of rules, duties and obligations; and virtue ethics, in which the individual, not the action, is the unit of moral evaluation.

We find the following results: First, subjects exert positive effort even when effort is unproductive. Second, subjects exert more effort when (the unproductive) effort is observable than when it is not. Third, subjects expect employers to reward effort even if it is common knowledge that output is determined by luck. Fourth, in the case where effort is unobservable, subjects work harder if the employer does not know that earnings are determined by luck. The latter result is driven by female workers and suggests that (female) workers work hard in order to avoid undeserved rewards.

Our experimental design is rather stylized. In the real world, neither workers nor employers will have full knowledge about the relationship

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between effort and output, and they will typically hold beliefs that effort to some extent, or in some cases, leads to higher performance. However, the advantage with our lab experiments is that we make an environment where *only luck* matters, and where we can control whether and to whom this information is available. Thereby we can rule out confounding factors that may matter in real world environments where luck is important, but not everything. This also makes it possible to rule out standard economic theory as potential explanations for the results we achieve.

If effort is something that is important for the firm, they should make it easy to observe. However, if effort is not important, making it hard to observe will minimize employees' effort but not get rid of it.

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## **4.7 Appendix**

### **4.8 Appendix A**

In this section, due to the long instructions, a complete set of instructions is listed for the first treatment, then for the rest of the treatments only what is different is listed.

#### **4.8.1 Common welcoming text for all participants and treatments.**

Welcome To The Experiment!

#### **Introduction**



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The experiment will last for about 45 minutes. During the experiment you will be able to earn money that will be transferred to your bank account when the experiment is over. It is therefore important that you enter the correct account number at the end of the experiment. If you do not have a Norwegian account number, please contact us after the experiment is completed. Instructions for the experiment can be found in this instruction manual. You will also get a summary of the instructions on the PC screen when the experiment itself starts.

This experiment is part of a research project and it is important for us that the following rules are followed:

- If you have questions, please raise your hand and we will come help you.
- Do not ask questions in plenary.
- It is not allowed to communicate with the other participants while the experiment is in progress.
- You choose how to spend your time in the experiment. Nevertheless, we require that you stay in the designated space throughout the experiment.
- You can use your mobile phone to browse the internet, but make sure it is on silent mode before we start.
- It is strictly forbidden to use your PC for anything other than the experiment, as other uses may lead to technical problems with the experiment.

You will now have time to read through the instructions for the experiment. Are there any questions about what has been said up to now?

Please turn to the next page.

Good luck!

### 4.8.2 Worker Instructions

#### 4.8.3 Full information: Effort is visible to everyone, Cause of output is common knowledge.

In this experiment there are two types of participants, workers and employers. You are randomly drawn the role of a **worker**. Together with another randomly drawn participant, you form a worker pair. A third participant is randomly drawn the role of employer. You will not know the identity of either your fellow employee or employer, nor will they know your identity.

#### **The task**

You and the other worker are invited to work separately with a work assignment on behalf of the employer. The assignment consists of decoding letters to numbers. The PC screen will display a table of letters and corresponding numbers. Your job is to find the number in the table that matches the letters to be decoded (see example below). Once you have answered correctly on a task, the next task will appear on the screen. The working period is 20 minutes. Along the way, a clock in the top right corner will show how many seconds remain (20 minutes = 1200 seconds).

Example: Given the following table

A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z
8	12	14	10	9	6	24	22	7	5	11	3	18	1	21	16	23	2	13	19	25	4	26	17	20	15

*Decode the following letters: A | E | H | Q | J | M | R | Z | T | W*

*The correct answer is: 8 | 9 | 22 | 23 | 5 | 18 | 2 | 15 | 19 | 26*

When the work period is over, you will know how many decoding tasks you have solved.

## **Payment**

When the work period is complete, the computer will draw a random number between 1 and 6, where each number is equally likely. In other words, the computer throws a die. It is this die and only this die, which determines how much money you earn / generate for your employer. If your computer throws a high number, you earn a lot of money for your employer, while low numbers earn less money for your employer. You will know your die throw after the working period and at the same time you will see how many letters you decoded during the work period.

Likewise, the computer will also roll a die for the other worker. The employer thus receives money from you and from the other worker. How much money the employer receives in total will depend on the two dice thrown.

The employer will retain  $1/3$  of the money you and the other worker generate. The remaining  $2/3$  of the money will be distributed by the employer between you and the other worker. Employers can freely choose the amount of money that will go to you and how much will go to the other worker.

Before the employer decides how he / she will allocate the money between you and the other worker, he / she will see how many decoding tasks each of you solved and how many eyes on the die each of you got. The employer is aware that the amount you have earned / generated is due to the die, and not the effort in the task before the die is thrown. The employer also knows you have learned that how much money you generate for him / her is due to the die and not how many decoding tasks you solved during the working period.

The employer thus has the following information when distributing the money between you and the other worker:

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- How many decoding tasks you have solved during the working period.
- How many decoding tasks the other worker has solved during the working period.
- How much money (eyes on the die) you generated for the employer.
- How much money (eyes on the die) the other worker generated for the employer.
- The employer knows that it is only the random dice that determine how much money is generated and that you and the other worker are also aware of this.

#### **Question and summary**

Both before and after the work period you will be asked to answer some questions. Please try to answer as well as you can.

Once you have done this, you will get a screen that summarizes the outcome of the experiment. You will again see how many decoding tasks you have solved during the work period, which dice the computer threw for you after the work period, and how much you have earned in this experiment (the amount assigned to you by the employer). This section of the experiment may take some time, so please be patient.

Finally, you will be asked to answer a single questionnaire on your PC. This questionnaire asks you, among other things, to enter your account number. Be sure to fill in the correct account information so that you will receive the proceeds from the experiment. If you do not have a Norwegian account number, please contact one of us.

Good luck!

**4.8.3.1 Not see effort: Only workers see their effort, Cause of output is common knowledge.**

Before the employer chooses how he / she will allocate the money between you and the other worker, he will see which die each of you got. He will not see how many decoding tasks either you or the other worker solved.

The employer is aware that the amount you have earned / generated is due to the die, and not the effort in the task before the die is thrown. The employer also knows you have learned that how much money you generate for him / her is due to the die and not how many decoding tasks you solved during the working period.

The employer thus has the following information when distributing the money between you and the other worker:

- How much money (eyes on the die) you generated for the employer
- How much money (eyes on the die) the other worker generated for the employer
- The employer knows that it is only the random dice boxes that determine how much money is generated and that you and the other worker are also aware of this.
- The employer does not know how many decoding tasks you solved during the work period.
- The employer does not know how many decoding tasks the other worker solved during the working period.

**4.8.3.2 Not know cause: Effort is visible to everyone, Only workers know the cause of output.**

Before an employer chooses how he / she will distribute the money between you and the other worker, he / she will see how many decoding tasks each of you has solved and how many eyes on the dice each of you got.

The employer does not know that the amount you have earned / generated is due to the dice, and not the effort in the task before the dice are thrown. Employers also do not know you have learned that how much money you generated for him / her is solely due to the dice, and not how many decoding tasks you have solved during the work period.

The employer has the following information when distributing the money between you and the other worker:

- How many decoding tasks you have solved during the working period.
- How many decoding tasks the other worker has solved during the working period.
- How much money (eyes on the die) you generated for the employer.
- How much money (eyes on the die) the other worker generated for the employer.
- The employer does not know that it is only the random dice throws that determine how much money is being generated, nor that you and the other worker are aware of this information.

**4.8.3.3 No information: Only workers see their effort, Only workers know the cause of output.**

Before the employer chooses how he / she will allocate the money between you and the other worker, he will see which dice each of you got. He will not see how many decoding tasks you or the other worker solved.

The employer does not know that the amount you have earned / generated is due to the dice, and not the effort in the task before the dice are thrown. Employers also do not know you have learned that how much money you generated for him / her is solely due to the dice, and not how many decoding tasks you have solved during the work period.

The employer thus has the following information when distributing the money between you and the other worker:

- How much money (eyes on the die) you generated for the employer
- How much money (eyes on the die) the other worker generated for the employer
- The employer does not know how many decoding tasks you solved during the work period
- The employer does not know how many decoding tasks the other worker solved during the working period
- The employer does not know that it is only the random dice throws that determine how much money is being generated, nor that you and the other worker are aware of this information.

#### **4.8.4 Employer instructions**

##### **4.8.4.1 Full information: Effort is visible to everyone, Cause of output is common knowledge.**

In this experiment there are two types of participants, workers and employers. You are randomly drawn the role of **employer**. The other participants in the experiment are randomly drawn to be workers. The workers are assembled in anonymous worker couples. You will not know the identity of the workers and they will not know your identity.

The workers' task

In the experiment, there are several worker couples who all have you as an employer. Workers are invited to work individually with a work assignment on your behalf. The work assignment consists of decoding letters to numbers. The working period is set to 20 minutes. In this 20-minute period you have no tasks. You can use your mobile to browse the internet, but make sure it is on silent mode.

For each worker couple:

When the work period is completed, the computer will draw a random number between 1 and 6, where each number is equally likely to be drawn. The computer makes such a draw for each worker. This is, in other words, as if the computer throws a die. Only the dice determine how much money the workers earn for you. If the computer draws a high number, they earn a lot of money for you while low numbers earn you less money. Specifically, each eye on the die equates to 100 NOK (1 eye = 100 NOK, 2 eyes = 200 NOK, etc.). After the working period, you will learn how much money each worker has earned (dice thrown) and how many decoding tasks each worker has solved.

Payment and your task



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You will receive  $1/3$  of the money each worker-couple earns. Your task will be to distribute the remaining  $2/3$  of the money that each worker pair earns between the two workers. You choose completely freely how much money you will give to each of the two workers. This must be done for all worker pairs. For all workers, this is real money and the only profit in the experiment.

IMPORTANT: After you allocate the money between the two workers for all the pairs, the computer will draw a random worker pair that determines your profits in the experiment. Your profit in the experiment is thus  $1/3$  of the money for a randomly drawn worker pair (not the sum of all worker pairs).

To make the distribution, you will be given the following information for each worker pair:

- How many decoding tasks each worker solved during the working period
- How much money (eyes on the die) each worker generated for you
- Workers know that you have this information.

#### **4.8.4.2 Not see effort: Only workers see their effort, Cause of output is common knowledge.**

For each worker couple:

When the work period is completed, the computer will draw a random number between 1 and 6, where each number is equally likely to be drawn. The computer makes such a draw for each worker. This is, in other words, as if the computer throws a die. Only the dice determine how much money the workers earn for you. If the computer draws a high number, they earn a lot of money for you while low numbers earn you

less money. Specifically, each eye on the die equates to 100 NOK (1 eye = 100 NOK, 2 eyes = 200 NOK, etc.). After the working period, you will learn how much money each worker has earned (dice thrown).

To make the distribution, you will be given the following information for each worker pair:

- How much money (eyes on the die) each worker generated for you
- Workers know that you have this information.

**4.8.4.3 Not know cause: Effort is visible to everyone, Only workers know the cause of output.**

For each worker couple:

You as an employer will know how many decoding tasks each worker has solved during the working period and how much money each worker has earned for you.

To make the distribution, you will be given the following information for each worker pair:

- How many decoding tasks each worker solved during the working period
- How much money (eyes on the die) each worker generated for you
- Workers know that you have this information.

**4.8.4.4 No information: Only workers see their effort, Only workers know the cause of output.**

For each worker couple:

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You as an employer will know how much money each worker has generated for you.

To make the distribution, you will be given the following information for each worker pair:

- How much money (eyes on the die) each worker generated for you
- Workers know that you have this information.

### 4.9 Appendix B

Table 9 Comparison of background characteristics

Age Female Grade	Effort unobserved luck unknown	Effort observed luck unknown	Effort unobserved luck known
Effort observed luck known	-0.62 1.57 2.17**	0.32 0.24 2.00**	1.00 -0.54 3.00***
Effort unobserved luck unknown		0.90 -1.31 -0.52	1.54 -2.13** 0.58
Effort observed luck unknown			0.38 -0.77 1.28

Note: The table presents z-values from Mann-Whitney U-tests (Age and Grade) and tests for the equality of proportions (Female), comparing Age/Female/ Grade by treatment. "Age" is a variable measuring subjects age in years; "Female" presents the proportion of females; "Grade" measures self-reported average grade, ranging from 0(=F) to 5 (=A). \*:p<0.10, \*\*:p<0.05, \*\*\*:p<0.01.

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Table 10 Descriptive statistics of effort provided

	Mean	SD	Median	Min	Max	N
Effort observed luck known	59.3	15.2	55	38	95	35
Effort unobserved luck unknown	45.7	14.6	46.5	0	69	26
Effort observed luck unknown	55.3	13.5	54.5	28	95	32
Effort unobserved luck known	55.7	20.8	54	14	125	41
Effort observed	57.4	14.5	55	28	95	67
Effort not observed	51.5	23.4	51	0	128	122
Luck known	53.5	16.3	52	0	95	61
Luck unknown	55.5	17.9	54	14	125	73

Table 11 Mann-Whitney U-tests Effort provided – adjusted

	Effort unobserved luck unknown	Effort observed luck unknown	Effort unobserved luck known	Effort unobserved	Luck unknown
Effort observed luck known	2.85***	0.81	1.06		
Effort unobserved luck unknown		-2.38**	-1.74		
Effort observed luck unknown			0.14		
Effort observed Luck known					-2.06**
					0.54

Note: The table presents z-values from Mann-Whitney U-tests. \*:p<0.10, \*\*:p<0.05, \*\*\*:p<0.01. . In addition, we also include combinations of treatments. Here Effort observed (Effort unobserved) consists of the two treatments where effort is observable (unobservable). Luck known (Luck unknown) consists of the two treatments where the role of luck was known (unknown) to the employer.

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Figure 4 Mean Effort

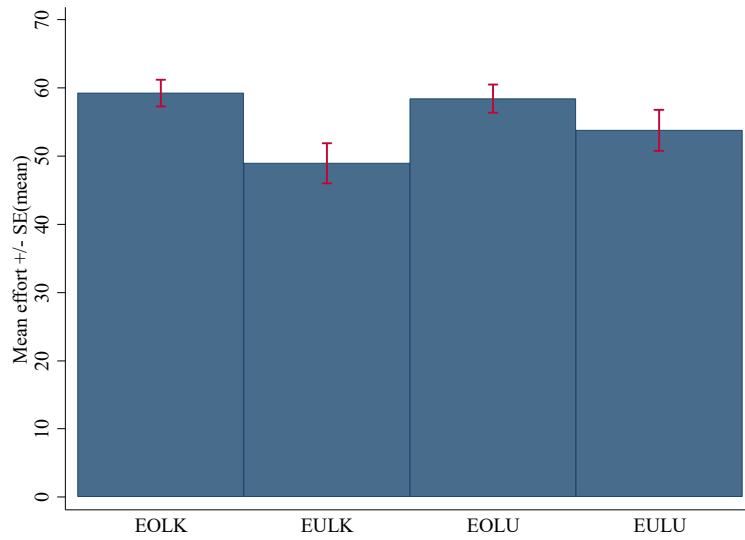
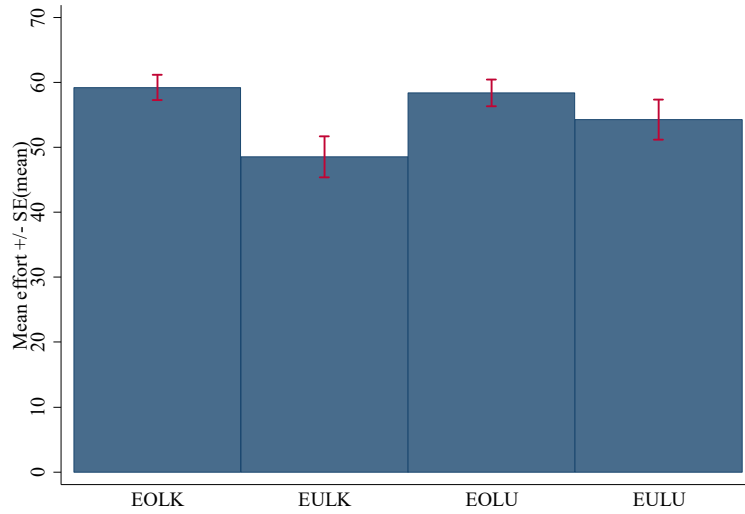


Figure 5 Mean Effort - Adjusted



Note: Not containing subject in treatments where effort is unobservable, but believed that effort will be rewarded