The influence of moral costs and heuristics on individual decision making: Five essays in behavioral economics

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I Introduction

‘*A person who has not made peace with his losses is likely to accept gambles that would be unacceptable to him otherwise.*’

Daniel Kahneman/Amos Tversky (*Choices, Values and Frames*)

Behavioral economics is the science combining the evident unity of economics and psychology (Camerer et al., 2011). It increases the fit of (behavioral) economic theories to human decisions in the economic context. While the major theories and findings of behavioral economics were established in the last decades (Sent, 2004), psychological factors were already considered by economists during the classical period. Adam Smith (1759) raised psychological principles of individual behavior affecting human decisions in his book *The Theory of Moral Sentiments*. For example, he described the phenomenon subsequently named loss aversion (Kahneman and Tversky, 1979) in his book as follows:

‘we suffer more…when we fall from a better to a worse situation, than we ever enjoy when we rise from a worse to a better’

Building on these insights, the following generations of researchers, like Herbert Simon (Augier and March, 2004), who was awarded the Noble Prize, and George Katona (Hosseini, 2011), established a new economic mindset. They discovered a new approach to economic thinking that went beyond the borders of neoclassical theories. Combing psychology and economic analysis provided theoretical insights and improved policy recommendations from economics. Thus, behavioral economics is an evolutionary step in economics that enhanced the neoclassical approach by considering human behavior (Camerer et al., 2011).

In this thesis, I go into more detail on two specific aspects of behavioral economics, namely the disposition effect and immoral behavior. These topics reflect on popular theoretical approaches from behavioral economics, such as Prospect Theory (Kahneman and Tversky, 1979) and inequality aversion (Fehr and Schmidt, 1999). The disposition effect is a trading pattern characterized by investors that are reluctant to sell capital losses and realize capital gains early (Shefrin and Statman, 1985). The phenomenon is empirically well-documented for investors and substantially reduces their profits from trading (Odean, 1998). Likewise, unethical behavior is harmful for the economy. Recent controversial cases of large-scale dishonesty associated with high economic losses are the Volkswagen emission control defeat device (Wang et al., 2016) and the Libor manipulation (Abrantes-Metz et al., 2012). In the following parts of the introduction, I motivate the specific facets of these topics discussed in the five papers included in the thesis.

In the **first part** of the thesis, I focus on the disposition effect. The disposition effect in the finance sector has been examined for private investors (Odean, 1998; Frazzini, 2006), students (e.g., Weber and Camerer, 1998; Chui, 2001), professional traders (Shapira and Venezia, 2001; Garvey and Murphey, 2004; Fu and Wedge, 2011), and team investors (Rau, 2015). Indeed, the disposition effect is not only widespread, it is also detrimental for investors (Odean, 1998). Due to the economic harm of this behavior, it is worthwhile to investigate the disposition effect in general. In this thesis, I approach the disposition effect in three studies: i) The effect of deciding on behalf of others on disposition effect behavior (chapter II). ii) The effect of prenatal androgen exposure on loss aversion, which is a key determinant of the disposition effect (chapter III) and iii) the disposition effect in commodity selling (chapter IV).

In the paper '**The disposition effect when deciding on behalf of others**' (Chapter II), our special interest was whether prosocial investors exhibit a more pronounced disposition effect under investment conditions when they feel closer to the other person. Empirical evidence suggests that professional traders who trade on behalf of others are less susceptible to the disposition effect (Shapira and Venezia, 2001). However, there is a lack of knowledge regarding the underlying forces. The effects are also unclear when private investors are involved. A possible explanation based on the results of Shapira and Venezia (2001) is that the trading experience of professional traders leads to improved trading performance (Da Costa Jr. et al., 2013). However, a couple of behavioral effects matter, such as, for instance, the perceived degree of loss aversion (Kahneman and Tversky, 1979), emotional responses (Shefrin and Statman, 1985; Summers and Duxbury, 2012) and reputational effects (Heimer, 2016; Pelster and Hofmann, 2017). Another factor is an investor's perceived social concern for her client. There is evidence that social distance to the decision target is an important determinant for investment behavior when deciding on behalf of others (Motinari and Rancan, 2013) and can be detrimental in social trading (Hershfield and Kramer, 2017). This emphasizes the role the disposition effect could play when private investors trade the money of others. A related case was experimentally demonstrated by

Rau (2015), who found that the responsibility for trading in teams increases the occurrence of the disposition effect.

Our study experimentally builds on the idea that social concerns for others are detrimental for investors' performance in social trading. We studied the impact of subjects' social value orientation (prosocial vs. individualistic type) on the emergence of the disposition effect when deciding for others. We explored this research question experimentally, as this enabled us to tackle possible obstacles which are hard to isolate in the field. First, the setup allowed us to focus on the pure effects of intrinsic motivation from the investors' side. That is, the experimental framework of a *Responsibility* treatment avoided, by design, that investors' extrinsic motives could be a reason for behavioral changes in social trading. More precisely, as investors in the *Responsibility* treatment were not paid based on their trading performance, the possibility that they were striving for a (high) personal monetary benefit could be ruled out. Moreover, we exogenously matched the decision targets to the investors. Thus, extrinsic motives to attract traders were also ruled out. The latter can play an important role in online social trading platforms where 'leading' traders receive bonuses for each trader who decides to copy their trades (Pelster and Hofmann, 2017).

In chapter III entitled '**Determinants of financial loss aversion: The influence of prenatal androgen exposure (2D:4D)**', I focus on loss aversion. Loss aversion is allegedly one of the key drivers for behavioral biases in the financial economics context, e.g., the disposition effect. However, while loss aversion is important, little research has investigated its determinants. In the context of risk aversion and altruism, several determinants related to human development have been identified (Garbarino et al.; 2011; Buser, 2012). Therefore, it is possible that factors related to human development could also influence the magnitude of loss aversion. In particular, biological factors, such as prenatal testosterone exposure, are proven to influence (economic) behavior. Prenatal androgens affect the development of the brain and increase its future sensitivity to the activational effects of circulating testosterone (Breedlove and Hampson, 2002). The most suitable marker to measure fetal androgens is the second-to-fourth digit length ratio (2D:4D), with a relatively longer fourth finger (lower 2D:4D) indicating higher exposure (Manning et al., 1998).

With regard to economic preferences, Buser (2012) revealed significant effects of the digit ratios (2D:4D) on the monetary amounts given in ultimatum, dictator, trust and public good games. Garbarino et al. (2011) and Coates et al. (2009) found that risk-taking increases with a smaller 2D:4D for both females and males. Furthermore, trading success

(Coates et al., 2009), choice of occupation (Nye and Orel, 2015) and wages (Nye et al., 2017), as parameters describing economic behavior, are related to the digit ratio. However, the relationship between the 2D:4D digit ratio and loss aversion had not been investigated previously. In this context, the research objective was to examine whether prenatal testosterone exposure influences the degree of loss aversion, while also taking into account personal characteristics which potentially influence loss aversion. To answer this research question, we conducted a laboratory experiment and surveyed digit ratios. In the experiment, loss aversion was elicited using an incentivized task.

The relevance of the disposition effect to the finance sector has been thoroughly examined. However, the disposition effect also appears in other contexts. In chapter IV, entitled '**The disposition effect in farmers' selling behavior – An experimental investigation**', we conducted an incentivized online experiment with farmers from Germany to investigate selling behavior for commodities. There have been several experiments in which the disposition effect was analyzed for student samples. For example, Weber and Camerer (1998) conducted an experiment in which the shares of risky assets could be traded. They found that students showed a tendency to sell assets that gained in value and keep assets that had dropped in value. In the context of financial and housing markets, Weber and Welfens (2006) revealed that learning and greater trading experience lead to a mitigated disposition effect. Further experimental investigations of the disposition effect with student samples were carried out by Da Costa et al. (2008), Fischbacher et al. (2014), Rau (2014) and Rau (2015). However, results based on experiments with students cannot be easily applied to other groups of participants in general (Belot et al., 2015), entrepreneurs (Barr and Hitt, 1986; Haigh and List, 2005), or farmers (Maart-Noelck and Musshoff, 2014; Hermann and Musshoff, 2016).

In this context, our objective was twofold: i) to investigate the presence of a disposition effect in the decisions of farmers selling stored commodities and ii) the analysis of factors potentially influencing the disposition effect. Thus, we provided three contributions to the existing literature. First, we experimentally analyzed whether farmers reveal a disposition effect in their decision to sell stored commodities. While empirical studies – mainly based on aggregated price data – have revealed evidence of a disposition effect in the selling behavior of farmers, only an experimental setting allows the underlying beliefs regarding price developments to be controlled. Second, we adjusted the well-proven experimental setting of Weber and Camerer (1998) from the context of financial economics to agricultural economics. This experimental design had not yet been applied in the agricultural context. Third, we investigated factors that potentially influence the disposition effect, especially Prospect Theory components and socio-economic variables.

In the **second part** of the thesis, I focus on immoral behavior. Immoral actions are commonplace and their consequences affect everyone in the private, business and public sector. As mentioned, very topical demonstrations of large-scale dishonesty are the Volkswagen emission control defeat device (Wang et al., 2016) and the Libor manipulation (Abrantes-Metz et al., 2012). Volkswagen was dishonest regarding their emission values and thereby have harmed their customers and the public due to decreasing residual values of purchased cars and decreased air quality (Wang et al., 2016). The Libor manipulation, on the other hand, was a dishonest report of interest rates by large banks. These reports led to manipulated reference interest rates and damage to the global economy running into the millions of euros (Abrantes-Metz et al., 2012). Likewise, the so-called Panama papers revealed the great extent and serious nature of tax evasion and tax avoidance, which harm the public sector by reducing national budgets (Chohan, 2016). Besides these examples of lying, stealing also harms the economy. Worldwide, there is about $48 billion of retail loss annually due to employee theft and about $48 billion from shoplifting (The Smart Cube, 2015). Due to the substantial consequences of immoral action for the economy, it is worthwhile for economists to deal with this topic. With this thesis, I contribute to the literature by: i) investigating the influence of social distance on honesty and ii) analyzing differences in the moral costs of lying and stealing.

Honesty depends on both personal and situational factors. The primary focus of chapter V, entitled '**Be close to me and I will be honest: How social distance influences honesty**', was on the situational factor of social distance. Specifically, we had participants in a laboratory experiment allocate money between themselves and someone else, who was either another participant or the experimenter. In this context, the other participant (i.e., a fellow student), was socially closer to the participant than the experimenter. The allocation involved honesty because participants received a random 'suggestion' based on the outcome of a die roll of how to allocate money and they had to lie in order to depart from this suggestion. Technically speaking, our experiment combined a dictator game with Fischbacher and Föllmi-Heusi's (2013) die-rolling task.

Social distance – i.e., how close agents are to each other (Akerlof, 1997) – has barely been considered as an influential factor regarding honesty. By contrast, several studies have documented the effect of social distance on the outcome of social interactions. For instance, Buchan et al. (2006) showed that other-regarding preferences, such as trust, reciprocity, or altruism, are more pronounced with a lower degree of social distance. Eckel and Grossman (1996) found that altruism in dictator games varied with the distance between the dictator and the recipient, who was either an anonymous student or a reputable charity. Charness and Gneezy (2008) found that dictators are more willing to give to recipients as social distance decreases. Similarly, Zultan (2012) reported more cooperation in the ultimatum game after pre-game face-to-face communication, which apparently reduces social distance.

Prior research has shed light on contextual factors related to our experiment. In particular, there is evidence that face-to-face as opposed to anonymous interaction (Holm and Kawagoe, 2010; van Zant and Kray, 2014) and personalized as opposed to standardized messages increase honesty (Cappelen et al., 2013). Participants in experiments have also been found to lie more often when they feel they are treated unfairly (Houser et al., 2012), and there is even evidence of people engaging in ‘white lies’ to benefit others (Erat and Gneezy, 2012) and justifying their dishonesty (Lewis et al., 2012). The effect of social distance, in turn, had not been directly examined yet. Social distance is arguably an important driver of honesty or dishonesty, though, since lying occurs in the context of social interactions. It is therefore interesting to investigate whether dishonesty depends on social distance and if this effect hinges on personal preferences for honesty and fairness.

The influence of social distance on honesty is important because it relates to most interactions that involve honesty. For example, public authorities usually appear as a distant and impersonal interaction partner to people, and honesty is indeed a major concern in tax collection. In this and other areas, people often interact through intermediaries, who increase social distance between the interaction partners. More generally speaking, the wide use of the internet has profoundly simplified but also depersonalized communication.

In chapter VI, entitled ‘**I might be a liar, but not a thief: An experimental distinction between the moral costs of lying and stealing**’, we shed light on the different moral costs of dishonesty and stealing. Due to the importance of immoral behavior in all of its manifestations in the economic context, several studies have focused on lying as well as stealing in different situations (see Rosenbaum et al., 2014). These investigations provide evidence that non-pecuniary moral costs arise from lying and stealing. This enhances the (neo-)classical theory on immoral behavior as a product of income effects and the probability of getting caught and punished (Fischbacher and Föllmi-Heusi, 2013; Gneezy, 2005).

However, there is a lack of knowledge regarding the amount of lying and stealing in an experiment with constant incentives and risk of being caught as a consequence of immoral behavior. However, the studies of Belot and Schröder (2013) and Gravert (2013) indicate that a direct comparison of lying and stealing would be worthwhile. Gravert (2013) found that self-reported outcome tasks reduce stealing compared to an effort-based theft task. Furthermore, providing the possibility to lie and steal simultaneously (Belot and Schröder, 2013) leads to the avoidance of stealing by participants. The differentiation of the costs associated with these two immoral behaviors has relevant implications for social interactions and economic questions. If one of these immoral behaviors is associated with higher intrinsic costs for economic agents, it might be possible to reduce economic losses by re-framing decisions in the respective context. For instance, the transfer of responsibility to agents under conditions of asymmetric information and consequently the changed perception of the immoral action might increase behavior which is in accordance with moral convictions.

Our study contributes to the literature by investigating whether the intrinsic costs of lying and stealing differ. To achieve this, we implemented an experimental design in which participants in a lying treatment rolled a die on their computer screen and reported the outcome. In contrast, participants in a stealing treatment did not report the outcome, but rather allocated the money in private - i.e., it was possible to steal. Participants received an envelope containing the maximum possible payoff and were asked to take the money according to the die-rolling outcome. Based on the identical framework conditions and incentives, we were able to distinguish between moral costs of lying and stealing.

Since behavioral economics is a rather new area of economic research, several interesting and worthwhile research questions remain unanswered. All chapters in this thesis contribute to the existing knowledge in this field by filling research gaps in various areas of interest. The studies composing this thesis also brought to light further undiscovered topics for future research in this interesting field of economics.

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II The disposition effect when deciding on behalf of others

with Holger Rau and Oliver Mußhoff

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Abstract

This article presents experimental evidence on the disposition effect in a situation where an investor decides on behalf of another person. In our setting, trading effort should only be affected by investors' intrinsic motivation, as trading actions only influence the profits of a matched person. In a control treatment, trades directly influence investors' profits. We find that trading on behalf of others increases disposition effects. The effect is caused by inexperienced investors, characterized by a greater concern for others. Thus, trading responsibility results in an emotional burden for investors, which leads to weak trading performance.

Keywords: Disposition effect, experiment, decisions on behalf of others, social value orientation, loss aversion.

JEL Codes: C91, D14, D81, G41

1. Introduction

The disposition effect is a trading pattern characterized by investors that are reluctant to sell capital losses and realize capital gains early (Shefrin and Statman, 1985). The phenomenon is empirically well-documented for private investors (Odean, 1998; Frazzini, 2006), students (e.g., Weber and Camerer, 1998; Chui, 2001), professional traders (Shapira and Venezia, 2001; Garvey and Murphey, 2004; Fu and Wedge, 2011), and team investors (Rau, 2015). Indeed, the disposition effect is not only widespread it is also detrimental for investors (Odean, 1998).

Due to this importance a better understanding of the influencing factors of this phenomenon is crucial. There is evidence that disposition effects occur as a result of a combination of reference-dependent behavior (Kahneman and Tversky, 1979; Shefrin and Statman, 1985) and reference-dependent emotions (Summers and Duxbury, 2012). It follows from Prospect Theory (Kahneman and Tversky, 1979) that loss-averse investors might have problems with realizing capital losses (Barberis and Xiong, 2012). Genesove and Mayer (2001) report empirical evidence that loss aversion positively correlates with disposition effects. Summers and Duxbury (2012) reveal that such a behavior is amplified by reference-dependent emotions. That is, investors keep capital losses to avoid the feeling of regret when realizing that they invested in a disadvantageous stock. By contrast, if stocks exceed the purchase price, risk-averse investors quickly realize them (Shefrin and Statman, 1985). In this case, Summers and Duxbury (2012) find that rejoicing additionally stimulates the realization of capital gains. Overall, it turns out that disposition effects in particular occur as a result of self-control problems (Shefrin and Statman, 1985).

Laboratory experiments are appropriate instruments to test how institutional interventions can help to overcome self-control problems. There is evidence that automatic stop-loss orders (Weber and Camerer, 1998; Fischbacher et al., 2017) or the salient presentation of purchase prices (Frydman and Rangel, 2014) can attenuate the emergence of disposition effects. Another form of intervention which might impact disposition effects is 'trading on behalf of others.' The analysis of social trading is of importance, as private investors frequently delegate their decisions to professional traders (Garvey and Murphy, 2004). There is evidence that private investors with a low financial literacy are more likely to rely on family and friends (Van Rooij et al., 2011). The increasing popularity of delegated private investment choices is demonstrated by the popularity of stock investment clubs or 'social-trading networks' such as 'etoro' (see http://www.etoro.com). The etoro platform allows investors to copy the trades of other traders, which can be seen as a form of delegated investment decision-making.

Empirical evidence suggests that professional traders who trade on behalf of others are less susceptible to causing disposition effects (Shapira and Venezia, 2001). However, there is a lack of knowledge regarding what the underlying forces are. The effects are also unclear when private investors are involved. An explanation for Shapira and Venezia's (2001) results might be that the trading experience of professional traders leads to improved trading performance (Da Costa Jr. et al., 2013). However, a couple of behavioral effects matter, such as, for instance, the perceived degree of loss aversion (Kahneman and Tversky, 1979), emotional responses (Shefrin and Statman, 1985; Summers and Duxbury, 2012) or reputational effects (Heimer, 2016; Pelster and Hofmann, 2017). Another factor is an investor's perceived social concern for her client. There is evidence that social distance to the decision target is an important determinant for investment behavior when deciding on behalf of others (Motinari and Rancan, 2013) and can be detrimental in social trading (Hershfield and Kramer, 2017). This emphasizes the disposition effects, if private investors trade with their friends' money. A related case is experimentally demonstrated by Rau (2015), who finds that responsibility for trading increases the disposition effects in teams.

Our study experimentally builds on the idea that social concerns for others are detrimental for investors' performance in social trading. We study the impact of subjects' social value orientation (prosocial type vs. individualistic type) on the emergence of disposition effects when deciding for others. Our experiments also control for additional factors which might affect disposition effects, such as loss aversion and trade experience. Recent findings of Andersson et al. (2014) suggest that deciding for others lowers loss aversion when choosing between lotteries in a price-list design. We test whether a different degree of perceived loss aversion in social trading affects disposition effects. Importantly, in our experiment we isolate the effects caused by monetary incentives or reputational concerns. Investors are matched to an anonymous client and the decision-makers' payment does not depend on their trading performance. Our setup minimizes reputational concerns as decision targets do not select the investors and are not informed of their performance before the experiment is finished. To analyze the effect of trading on behalf of others, we conduct two treatments based on the design of Weber and Camerer (1998). In the main treatment called '*Responsibility*,' each subject is randomly matched with an anonymous other subject. All participants repeatedly take investment decisions on behalf of others and know that their profits depend only on the performance of another participant who is also trading on their behalf.[1] Our control treatment ('*Individual*') is an exact replication of Weber and Camerer (1998), i.e., investors trade only for their own benefits.

The results reveal that disposition effects are significantly higher in the *Responsibility* treatment. Furthermore, our findings show that the treatment effect can be entirely explained by differences in investors' social value orientation. It turns out that inexperienced investors, characterized by a prosocial attitude face self-control problems when taking decisions for others. The data show that this group exhibits a significantly higher disposition effect in social trading as compared to the case when trading for own benefits. By contrast, no treatment effect can be observed for individualists. The results add interesting new insights into the behavior of private traders who trade on behalf of their friends or are part of investment clubs.

[1] To study trading on behalf of others in isolation, we followed Andersson et al. (2014) and refrained from situations where a monetary conflict of interest exists between the investor and the stakeholder.

2. Hypotheses

In this section we derive our hypotheses. Experimental evidence of team investment decisions demonstrate that payoff externalities enhance self-control problems, if trading also affects the payoff of a matched partner. Rau (2015) shows that teams of two investors are reluctant to sell capital losses. Empirical evidence of a social-trading platform suggests that reputational effects induce higher disposition effects for traders who are followed by others (Pelster and Hofmann, 2017). In our *Responsibility* treatment, where subjects *only* trade for the benefit of another matched person, we expect that prosocial investors should be affected by this condition. The reason is that these traders have a low perceived social distance to the decision target (Montinari and Rancan, 2013) and therefore should have increased problems in regulating their emotions (Hershfield and Kramer, 2017). Hence, we expect that disposition effects are more pronounced when traders are responsible for other persons.

Hypothesis 1:

(a) Disposition effects are higher in the Responsibility *treatment than in the control treatment.*

(b) In Responsibility, *disposition effects will be stimulated by investors with a prosocial attitude.*

Empirical and experimental evidence reveals that individual loss aversion stimulates disposition effects (Genesove and Mayer, 2001; Rau, 2014). Thus, we expect that subjects with a higher degree of loss aversion realize fewer capital losses and exhibit more pronounced disposition effects. Andersson et al. (2014) report that subjects behave less loss-averse when deciding for others. In *Responsibility*, investors trade on behalf of others. As trades do not affect their income, they should have a different perception of losses in this case. Hence, the correlation between loss aversion and disposition effects should be less pronounced in *Responsibility*.

Hypothesis 2:

(a) Individual loss aversion is positively correlated with disposition effects.

(b) In Responsibility, *the impact of loss aversion on disposition effects is attenuated.*

3. Experimental design

In this section we describe our experimental design. First, the experiment design of Weber and Camerer (1998) is introduced. Second, we discuss the differences between our treatments *Individual* and *Responsibility*. Third, we provide a brief overview of the additional experimental tasks implemented to measure individual preferences (risk aversion, loss aversion, and social value orientation) and personal characteristics.

3.1 The framework of Weber and Camerer

In the experimental framework of Weber and Camerer (1998) six different labeled assets A, B, C, D, E, and F can be traded over 14 periods. The asset prices are predetermined for all periods and follow a distinct random process. Participants' trading actions do not influence stock prices. The price sequences of all 14 periods are pre-determined before the experiment starts. The stocks are classified in different types according to their chances of a price increase. More precisely, exactly one stock follows a good/very good quality (labeled: +, ++), one stock follows a poor/very poor quality (labeled: −, − −), and two stocks fluctuated around the starting price with a 50% probability of rising prices (labeled: 0). Subjects were told about the existence of the types and their characteristics, but received no information on the allocation of the labels. Our experiment applied the same stocks (A to F), the same allocation of the stock types, and the same price sequences as in Weber and Camerer (1998). In each period, prices are determined in two stages: 1. determination of the direction of price movement; 2. determination of the change in the price magnitude. The two stages are explained in detail in the following sections.

3.1.1 Stage 1: Determination of the Direction of Price Movement

In the first stage the computer determines whether an asset will increase/decrease in value. The probability of a price increase/decrease depends on the assets' stock types. Weber and Camerer's (1998) random process allocates fixed probabilities of stock price increases/decreases for each type of each quality. This feature allows the predetermination of the sequence of the price changes. A random process determines whether a stock will increase or decrease in value. This depends on the underlying probabilities of price increases of the stock types. Although the participants are familiarized with the probabilities of a price increase or decrease, they do not know which probability belongs to which stock. Nevertheless, the participants can guess by counting and comparing the number of price increases in the previous periods. Table 1 presents the underlying allocation of the stocks (A to F) to the types. Our experiment follows Weber and Camerer (1998) and adopts their design.

Table 1: Stock characteristics

Stock		Probability of price change	
Name	Type	Increase	Decrease
A	+	55%	45%
B	–	45%	55%
C	– –	35%	65%
D; E	0	50%	50%
F	++	65%	35%

Note: Overview of the stock types and their probabilities of price increases and decreases (stock names are not shown to the participants).

3.1.2 Stage 2: Determination of the Price Magnitude

After the random process determines whether an asset will increase, the computer randomly determines the magnitude of the price change in the second stage. It can be either 1, 3 or 5 Talers. All outcomes occur with a probability of one third. The probability of a stock price increase is not correlated with the magnitude of the price change and the expected value of a price change for a randomly-chosen stock is zero (Weber and Camerer, 1998).[2] Weber and Camerer (1998) determined the price sequences of stocks according to this approach. They also computed the asset prices for four prior periods: -3, -2, -1, and 0. This information is presented to subjects prior to the start of the experiment. The purpose is to give participants an initial idea of the stocks' characteristics. In this experiment we also present this information to subjects prior to the start of the experiment. Figure 1 illustrates the resulting stock movements of Weber Camerer (1998) in periods -3 to 14.

[2] The framework easily allows the application of Bayesian Updating in each period. Bayesian subjects would repeatedly update their beliefs on the increase probability of all six shares, based on the actual observed price changes. Hence, investors might apply a simple heuristic of counting the number of times a stock increased to determine its type. The stock whose price has increased most often is most likely to be of the ++ type. The stock which had the second highest number of price increases has to be of type +, etc.

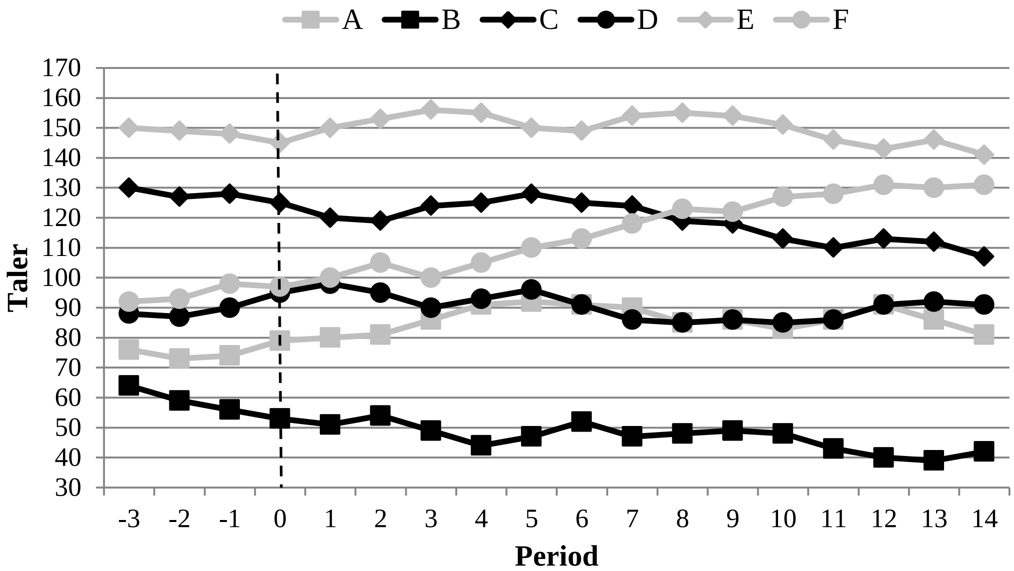

Figure 1: Price movements of stocks A to F over time.

Note: Dashed line marks the beginning of the trade period; Figure is not shown to the participants at the beginning of the experiment.

3.1.3 Elicitation of Guess Scores

In the experiment we follow Weber and Camerer (1998) to examine the possibility that subjects' disposition effects are caused by a misjudgment of the stock types. That is, after periods 7 and 14 subjects must guess the type of each of the six stocks. The estimates are used to derive delta (δ), a measure of fit between the best fit and a subject's guess of the stock type. The guesses of the six stocks are coded as follows: ++ = 2, + = 1, 0 = 0, − = -1, − − = -2. The coding corresponds to the rational estimate. Afterwards, the absolute value of the difference between a subject's guess and the rational estimate is calculated for each of the six stocks. The delta corresponds to the sum of the absolute differences of all six stocks. The δ measure ranges from 0 (best estimates) to 12 (worst estimates). For instance, if a subject guesses that the ++-type is stock 'F' then the subject's actual estimate equals the rational estimate. Thus, the difference is: 2−2 = 0. If the subject guesses that the 0-type is stock 'A' then the difference is: 1−0 = 1. It follows for the delta of this subject: δ = 0 + 1+ etc.

3.1.4 Measures of the Disposition Effect

When investors sell shares, the purchase prices are not always known. Hence, the paper reports two accounting principles to compute results: (i) Average Price; (ii) First-In-First-

Out (FIFO).[3] The Average-Price approach (e.g., Odean, 1998) determines the purchase price as the weighted average of all purchase prices. Whereas, the FIFO measure identifies the purchase prices by assuming that investors sell the stocks in distinct orders. That is, it assumes that investors first sell the stocks which were bought first.

Furthermore, the analysis follows Odean (1998) to investigate the occurrence of disposition effects. Therefore, we determine the proportion of gains realized (PGR) and the proportion of losses realized (PLR). The PGR (PLR) is the number of realized gains (losses) divided by the total number of possible gains (losses) that could have been sold. In accordance with Odean (1998), it can be defined as follows:

$$Proportion\ of\ Gains\ Realized\ (PGR) = \frac{Realized\ Gains}{Realized\ Gains + Paper\ Gains} \tag{1}$$

$$Proportion\ of\ Losses\ Realized\ (PLR) = \frac{Realized\ Losses}{Realized\ Losses + Paper\ Losses} \tag{2}$$

We calculated the individual-level disposition effects (DE) for all participants as the difference between the PGR and PLR:

$$DE = PGR - PLR \tag{3}$$

The DE measure is restricted to a range between -1 and 1. Participants with DE = 1 (-1) realized all gains (losses) immediately, whereas they never realized losses (gains). For investors with DE = 0, PGR and PLR are equal.

We also compute disposition effects with the measure of Weber and Camerer (1998) to provide a robustness check for the DE measure. We refer to this measure as the ‘alpha’ measure. Alpha examines whether participants used last period’s prices as reference points. More precisely, it is tested whether subjects prefer to sell stocks after price increases of the last period’s price. The alpha measure is defined as:

$$\alpha = \frac{(S_+ - S_-)}{(S_+ + S_-)} \tag{4}$$

S_+ (S_-) represents the sum of sales realized after price increases (decreases). Alpha corresponds to the difference in sales after a price increase and a price decrease, normalized by

[3] We also calculated LIFO (Last-In-First-Out) but results are not affected by using this accounting principle. Weber and Camerer (1998) and Rau (2015) also find no differences between these accounting principles.

the total number of sales. An alpha of 1 (-1) indicates that participants only sold after the price increased (decreased). If the alpha amounts to zero, the number of sells after price increases and price decreases is the same.

3.2 *Individual* versus *Responsibility* Treatment

In a between-subject design, we test for differences between the two treatments: *Individual* and *Responsibility*. *Individual* is identical to the experiment of Weber and Camerer (1998) as well as Rau (2015). It follows the framework described in section 3.1. A crucial difference applies to *Responsibility*, where participants decide on behalf of another participant from the respective experimental session. The experiment was programmed using z-Tree (Fischbacher, 2007) and carried out in fall 2016. The data encompasses four sessions of *Responsibility* with a total of 85 subjects and four sessions of *Individual* with a total of 85 subjects. In total, 170 participants took part in the experiment and were recruited with ORSEE (Greiner, 2004). The subject pool consisted of students from a German university from various fields who earned €16.70 on average. The majority studied economics, i.e., 37.6% of the subjects in *Individual* and 35.5% of the subjects in *Responsibility*. We explicitly excluded participants who heard a lecture about finance. The sessions lasted approximately 110 minutes.

3.2.1 Procedures of the Individual treatment

In *Individual* all participants received an endowment of 10,000 Talers. In both treatments we applied an exchange rate of 1,000 Taler = €1. In periods 1 to 13 (see Figure 1) subjects could buy or sell assets which were labeled with the neutral German word ‘Anteile’ (‘‘shares”). Subjects did not necessarily have to invest any amount of their endowment. There were no transaction costs for trading actions and subjects were not allowed to make short sales, i.e., they could only sell stocks which they owned. In period 14 subjects’ portfolios were automatically liquidated. Their final payoff corresponded to the value of the liquidated portfolio plus the money they owned in period 14. To evaluate whether subjects had a good understanding of the stock types, they had to guess the stock types after periods 7 and 14. Here, they received 200 Talers (€0.20) for each correct guess.

3.2.2 Responsibility

In *Responsibility* almost everything was identical to *Individual*. However, one crucial difference was that a trader decided on behalf of someone else and her outcome of the trading experiment also depended on the decisions of someone else. To establish this treatment environment, all participants acted in the role of an investor and recipient. More precisely, subjects determined with their trading actions the payoff of a randomly matched recipient. At the same time, they received a payoff which depended on the trading performance of another randomly matched participant. Our matching procedure ruled out that two subjects could mutually generate their payoffs. We explicitly informed our subjects on this matching procedure which prevents participants from evolving any kind of reciprocal mental connection to their matched partner. At the beginning of the experiment every participant received a note containing a letter from the alphabet attached to the instructions. Participants made aware that the letter served as their experimental identity.[4] Afterwards, participants were told in the instructions that they had to decide for another participant in the experimental session and that their earnings from the experiment would depend on someone else. Most importantly, to rule out any form of potential mental reciprocal relation, we stated in the instructions that: 'The participant who decides for you is not the same person as the person you are deciding for.' When the trading experiment started, participants received a screen message informing them of the letter of the participant which they would be deciding for. Moreover, they were shown the letter of the person who would be deciding for them. Our matching procedure worked as follows: for instance, a participant received the letter 'A', i.e., she knew that her name was 'A'. Moreover, the participant was informed on the computer screen that she would take the trading decisions on behalf of participant 'B'. However 'A' received the earnings according to decisions of a third participant, namely participant 'C'. We informed participants in the decision sheet of each period that they would decide on behalf of the matched recipient. Therefore, we repeatedly mentioned that they would be making their decisions on behalf of this participant.

3.3 Additional experimental tasks

Besides the main trading experiment, we conducted further elicitation tasks. Before the trading experiment started, a computerized risk-aversion, loss-aversion and social value orientation elicitation task was conducted. Finally, after the trading experiment had been carried out, subjects' empathy, levels of perceived regret, and rejoice were elicited in a post-experimental questionnaire.

To elicit risk aversion, we carried out a modified version of the measure, introduced by Eckel and Grossman (EG; Eckel and Grossman, 2008). In the EG task participants chose a preferred lottery from a set of lotteries. The classical form of the EG, however, allows us to

[4] Participants in *Individual* received a note containing a letter as well, in order to provide comparable framework conditions in both scenarios.

measure detailed distinctions exclusively for different levels of risk aversion. We therefore decided to extend the classical EG task to gather additional information on whether participants were risk-neutral or risk-seeking.[5]

In a next step we elicited subjects' loss aversion through an incentivized multiple price list, introduced by Gächter et al. (GJH; Gächter et al., 2007) and used in several other studies, e.g., Dutcher et al. (2015) and Koudstaal et al. (2016).[6] Participants had to decide 10 times whether they wished to take part in a lottery or not. In all 10 lotteries, participants could face a loss with a chance of 50%, or receive a gain with a probability of 50%. The probabilities for a loss or gain as well as the amount of the gain are constant across all lotteries. However, the potential loss increases across the lotteries. The GJH task allows for a characterization of participants regarding their degree of loss-aversion, which is expressed by lambda (λ), a coefficient indicating loss-aversion (Gächter et al., 2007). The task was incentivized and each participant received an endowment of €0.70[7] at the beginning of the experiment. This endowment ensured that no participant had a negative payoff. To determine each participant's final payoff, a random lottery was drawn. If the random lottery was accepted by the participant, the respective lottery was performed and the outcome was added to the initial endowment. If the random lottery was rejected, the participant did not take part in the lottery and received the endowment.

To answer our question of whether investors' social-value orientation (SVO) impacts disposition effects when deciding on behalf of others, we elicited subjects' SVO with an incentivized measure according to Murphy et al. (2011) (see appendix A.3). In the SVO Slider Measure, participants had to decide in six different decision situations ('sliders') which allocation of tokens they would like to choose from a given set of combinations. Each combination included an amount of tokens which would be received by the deciding participant and an associated amount of tokens which would be received by a randomly matched participant.[8] Every participant received a payment from the *SVO Slider Measure*. Participants were informed that the decision in one of the six sliders would be relevant for their payment. In this case, in a random draw half of the participants ('deciders') received

[5] For detailed illustration of the conducted task and possible outcomes of the lotteries, please refer to appendix A.1. To determine the payoff, the chosen lottery was performed and the outcome was paid to the participant at the end of the experimental session.

[6] The modified design of the GJH-task is adapted from Rau (2014) and can be found in appendix A.2.

[7] The monetary amounts used in the tasks were chosen according to an average wage approach: average wage per time unit multiplied by the expected time units to carry out the task.

[8] The randomly assigned participant was not the same as in the stock trading experiment.

the monetary amount according to their own decisions and the other half received a monetary amount depending on the decision of the randomly matched 'decider.'

In addition, we measured subjects' empathy using a 16-item questionnaire answered on a 5-point Likert scale.[9] The empathy measure is based on the Interpersonal Reactivity Index (IRI), introduced by Davis (1983). We used a modified version according to Paulus (2012). To calculate the empathy score of a participant the 5-point Likert scale was transformed into numbers. If participants chose the response 'never' the corresponding number was 1, if they chose 'rarely' the number was 2, and so on. The empathy score is the sum of the 16 items minus the numbers from items 3, 6, 8, and 13.

4. Results

In this section, we start with descriptive statistics on trading behavior in our two treatments. Afterwards, we apply non-parametric and parametric-test methods to validate our hypotheses. All reported tests are based on two-sided p-values, if not otherwise specified.

4.1 Trading Behavior

Table 1 presents the descriptive statistics of subjects' trading behavior in the *Individual* and *Responsibility* treatment. The third column displays the aggregate data. The table presents bought stocks and sold capital gains/losses which denote the average number of traded stocks. Whereas the variables processed gains, gain trades, and loss trades focus on the average number of processed trades.

A conspicuous finding is that investors in both treatments sell a significantly higher average number of capital gains than capital losses (Wilcoxon matched pairs tests, $p < 0.001$; both treatments). A similar pattern can be found when focusing on gain and loss trades. This is a first indication that disposition effects obviously occur in both treatments. We find that in *Responsibility* not statistically significant more stocks are bought and more capital gains and losses are sold than in *Individual.*

[9] The translation of the items is provided in appendix A.4.

Table 2: Descriptive statistics on trading behavior

	***Individual* (n=85)**	***Responsibility* (n=85)**	**All data (n=170)**
Bought stocks[a)]	141.01 (94.30)	148.88 (80.97)	144.95 (87.72)
Sold capital gains[b)]	45.74 (50.62)	47.80 (47.31)	45.74 (50.62)
Sold capital losses[b)]	33.75 (36.34)	35.93 (40.87)	34.84 (38.57)
Processed trades[c)]	26.79 (12.29)	26.75 (11.75)	26.77 (11.99)
Gain trades[d)]	4.67 (3.91)	5.13 (3.79)	4.90 (3.85)
Loss trades[d)]	4.45 (3.77)	4.38 (3.43)	4.41 (3.60)

Note: Standard deviation in parenthesis.
a) Number of stocks bought by participants. b) Number of stocks sold as gain (loss) according to the mean portfolio value (Odean, 1998). c) Total number of trades (buying and selling) d) Number of trades sold as gains (losses) according to the mean portfolio value (Odean, 1998).

4.2 Hypotheses tests

We turn to our main results. Figure 2 focuses on the disposition effects calculated with the method of Odean (1998). The figure depicts subjects' average Disposition Effects (DE), the Proportion of Gains Realized (PGR), and the Proportion of Losses Realized (PLR).

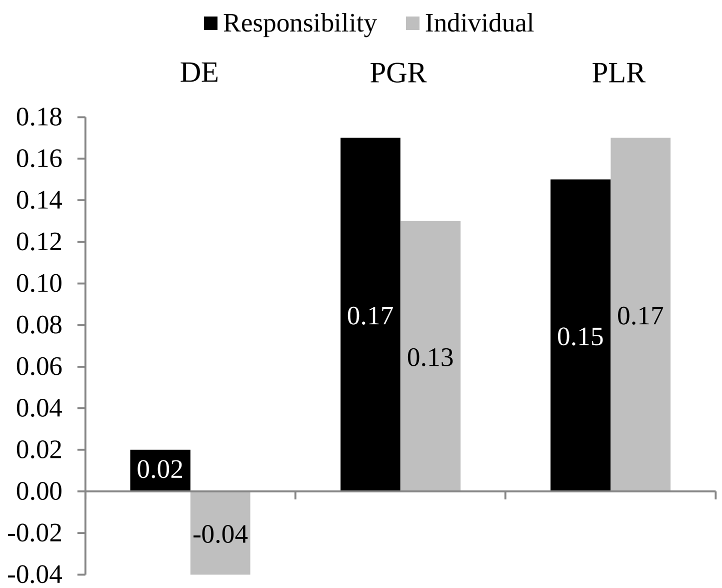

Figure 2: Disposition Effects (DE), Proportion of Gains Realized (PGR), and Proportion of Losses Realized (PLR) in the treatments *Responsibility* and *Individual*.

The data show that subjects exhibit higher disposition effects when deciding on behalf of others (0.02; SD: 0.25) than in the *Individual* treatment (-0.04; SD: 0.21) (Mann-Whitney test, p = 0.067). The finding is also supported by the Alpha measure of Weber and Camerer (1998). Figure 3 compares the Cumulative Distribution Functions (CDF) of subjects' Alphas in our two treatments.

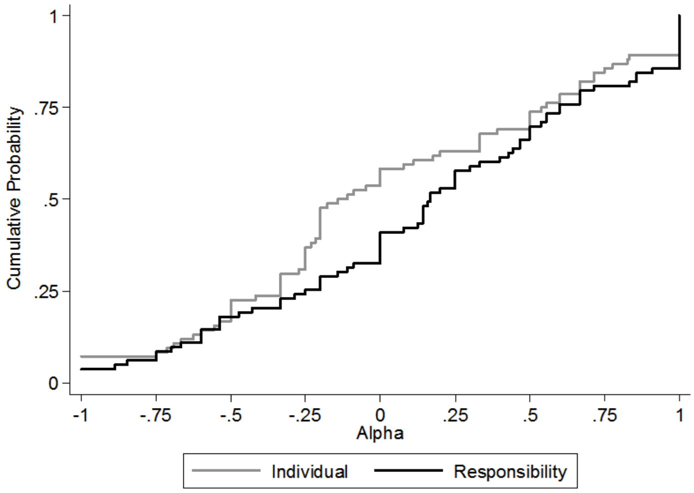

Figure 3: Alpha measure (Weber and Camerer, 1998) in our treatments.

The CDFs of subjects' Alphas are significantly different in the *Responsibility* treatment (0.17) than in the case when subjects trade for their own profits (0.03) (Kolmogorov-Smirnov test, p = 0.034). Hence, we find support for Hypothesis 1a.

Result 1: *Deciding on behalf of others leads to significantly higher disposition effects as compared to trading for own benefits.*

Next, we concentrate on the impact of traders' social-value orientation (SVO) and test whether a lower social distance to the decision target leads to an increase in problems with controlling their emotional states (Montinari and Rancan, 2013). Consequently, difficulties with self-control might arise (Hershfield and Kramer, 2017) which cause pronounced disposition effects (Rau, 2015). We turn to Hypothesis 1b and test whether prosocial traders (Prosocials) with a low level of perceived social distance exhibit more pronounced disposition effects than traders with a high level of perceived social distance (Individualists). Fenton-O'Creevy et al. (2011) point out that investors with low trading experience especially have problems in the regulation of emotions. Therefore, we distinguish between the SVO of less-experienced traders and more-experienced traders. We classified our participants based on their self-reported trading experience[10] stated on a 10-point Likert scale in our post treatment questionnaire. A value of 1 indicate that participants had no or very little trading experience and a value of 10 that they perceived themselves as highly experienced regarding stock trading (see appendix A.5). Focusing on trading experience, it turns out

[10] We asked subjects: "how high do you estimate your experience in the private trading of stocks? Please answer on a scale 1-10 (1 = very low; 10 = very high)."

that 54% of our subjects stated that they had very low trading experience of 1. Therefore, we classify subjects with a trading experience of 1 (>1) as inexperienced (experienced).

Focusing on experienced traders, we do not find that social-value orientation statistically significantly determines the level of disposition effects in *Responsibility* (Pearson's correlation coefficient, ρ = -0.223; p = 0.178).[11] By contrast, we observe that the level of perceived social distance clearly matters for inexperienced traders. This finding is illustrated in Figure 4 which presents scatter plots of the correlation between subjects' SVO angle and the level of exhibited disposition effects. In the scatter plots data of the treatment *Responsibility* (left panel) and treatment *Individual* (right panel) is compared.

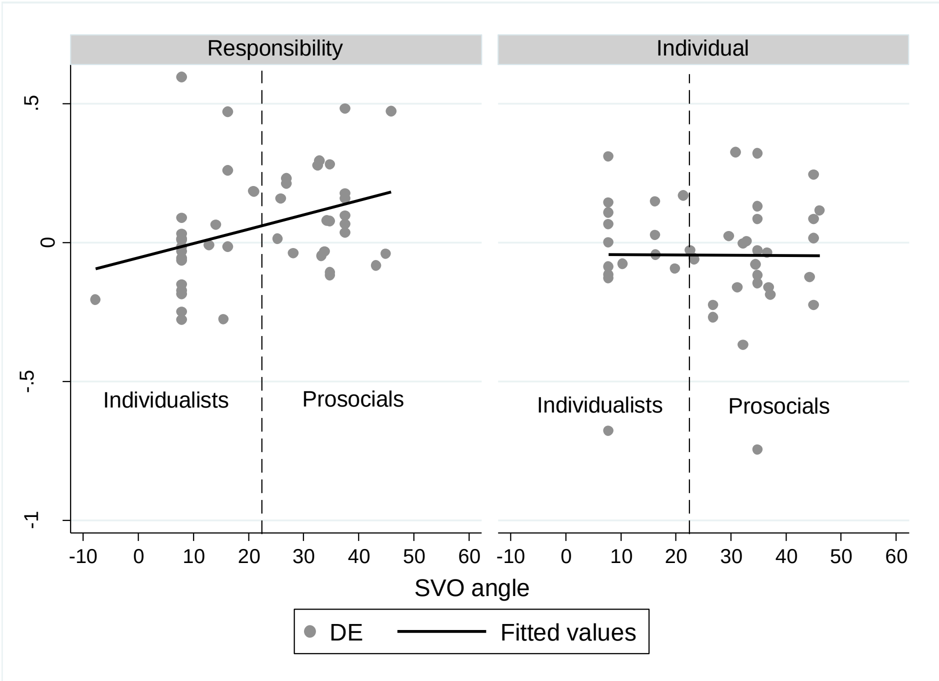

Figure 4: Scatter plots on the correlation of the SVO angle of inexperienced traders (trading experience = 1) and disposition effects in *Responsibility* (left panel) and *Individual* (right panel).

Note: Dashed line indicates the threshold of the SVO angle which divided individualistic (left) and prosocial (right) subjects (Murphy et al., 2011).

A conspicuous finding is that in *Responsibility* we find a significant positive correlation between subjects with a higher degree of social-value orientation (i.e., a higher SVO angle) and the level of exhibited disposition effects. We interpret these subjects in *Responsibility* as investors who perceive a lower level of social distance to the matched recipient. This pattern is confirmed by a significant Pearson's correlation coefficient (ρ = 0.346; p =

[11]A similar finding can be observed in *Individual* (Pearson's correlation coefficient, ρ = -0.005; p = 0.978).

0.019). Hence, inexperienced prosocial subjects exhibit higher disposition effects than inexperienced individualists (Mann-Whitney test, p = 0.038) when trading on behalf of others.

By contrast, in the right panel, it can be seen that this correlation does not exist when inexperienced investors trade for their own interests in treatment *Individual*. That is, the Pearson correlation coefficient is not statistically significant ($\rho = -0.006$; $p = 0.970$). When investors trade for their own benefits, the disposition effects of prosocial and individualistic subjects do not differ statistically significantly (Mann-Whitney test, p = 0.282). Importantly, for individualistic investors we do not find a significant correlation between SVO angle and DE, neither in the *Responsibility* treatment ($\rho = -0.223$; $p = 0.178$), nor in the *Individual* treatment ($\rho = -0.005$; $p = 0.978$). Thus, we find support for Hypothesis 1b when focusing on inexperienced traders. Whereas the hypothesis is rejected for subjects who had trading experience.

Result 2a: *When inexperienced investors are responsible for other people, prosocial types exhibit significantly higher disposition effects than individualistic types.*

Result 2b: *When inexperienced investors are responsible for other people, the level of investors' social-value orientation determines the level of disposition effects.*

Finally, we analyze whether deciding on behalf of others impacts the degree of perceived loss aversion which might affect the occurrence of disposition effects. Focusing on the aggregate data, we generally find that disposition effects are stimulated by loss aversion. That is, we observe a positive correlation between loss aversion and DE (Pearson's correlation test, $\rho = 0.201$; $p = 0.014$). This is in line with Genesove and Mayer (2001) and Rau (2014) and confirms Hypothesis 2a.

To test the effects of trading on behalf of others, we focus on a disaggregate analysis. If we focus separately on the *Individual* treatment, it becomes obvious that the relation between loss aversion and the level of the disposition effect also holds (Pearson's correlation test, $\rho = 0.201$; $p = 0.014$). By contrast, the relation becomes insignificant in the *Responsibility* treatment (Pearson's correlation, $\rho = 0.172$; $p = 0.137$). Thus, the effect of loss aversion is attenuated when subjects decide on behalf of others. This supports the findings of Andersson et al. (2014) and is in line with Hypothesis 2b.

Result 3a: *Loss aversion stimulates the occurrence of disposition effects.*

Result 3b: *The relation between loss aversion and disposition effects is attenuated in the Responsibility treatment.*

The results show that though deciding on behalf of others attenuates the impact of loss aversion on disposition effects, traders do not achieve an improved performance in this setting. However, our analysis of the impact of social-value orientation provides an answer: As it turns out, prosocial investors who perceive a lower distance to their matched person exhibit particularly high disposition effects.

4.3 Regression Analyses

To provide an in-depth analysis of disposition effects, we estimate Tobit regressions with heteroscedasticity robust standard errors on DE, PGR, PLR, and the Alpha-measure of Weber and Camerer (1998) for inexperienced participants. Table 3 illustrates the regression estimates for the four dependent variables DE (models 1–2), PGR (models 3–4), PLR (models 5–6) and Alpha (models 7-8).[12] In basic regression models, we include a treatment dummy (*Responsibility*) and subjects' preference parameters (loss aversion and risk aversion). Models (2), (4), (6) and (8) additionally control for the impact of interactions between treatment dummies (*Responsibility* and *Individual*) and subjects' social-value orientation (prosocial and individualistic). Furthermore, we control for subjects' gender, the stated emotions (regret/rejoice), the level of empathy, their math grade and whether they study economics.

The models (1) and (7) confirm Result 1 for inexperienced traders, i.e., the general treatment effect: when inexperienced participants decide on behalf of another participant, the disposition effect is significantly more pronounced than in the *Individual* treatment. Results show that the coefficient of *Responsibility* is positive and significant for the inexperienced traders, however, not for the experienced traders (see regressions in appendix A.6). The highly significant positive coefficient of *Loss Aversion* demonstrates that disposition effects are more pronounced for loss-averse traders. This confirms the result of Rau (2014). Models (2) and (8) reveal that loss aversion is still a significant determinant of DE, if we control for other variables. We find that the female gender is not a factor that increases DE or Alpha significantly for inexperienced traders.[13] Turning to emotions, we find a

[12] For similar regression estimates of high experienced traders, refer to appendix A.6

[13] Focusing on the regressions on the sample of experienced traders, we find that gender indeed matters. Females in the experienced sample reveal higher disposition effects according to DE and Alpha. On the one hand, this result confirms the results of Genesove and Mayer (2001) and Rau (2014). On the other

twofold pattern: feelings of regret seem to stimulate the disposition effect. That is, we find that the coefficient of Alpha is positive and highly significant. In other words, traders who feel high levels of regret have problems to sell stocks, after a stock-price decrease in the last periods (Rau, 2015). Regret has also an almost significant effect on DE ($p = 0.112$). At the same time, pride does not affect the disposition effect for inexperienced traders. By contrast, all other control variables do not statistically significantly affect DE and Alpha.

To better understand the driving forces of the treatment differences in disposition effects, we focus on subjects' selling behavior in more detail. In this respect, we run separate regressions on the Proportion of Gains Realized (PGR) (models 3–4) and on the Proportion of Losses Realized (PLR) (models 5–6). It becomes obvious from models (3) and (4) that the treatment effect is mainly caused by an increased PGR of inexperienced traders in the *Responsibility* treatment. In the *Responsibility* treatment we find a higher degree of PGR. This is highlighted by Model (3) which focuses on subjects' PGR as dependent variable. Here, we find that the coefficient of *Responsibility* is positive and significant. Thus, when inexperienced traders decide on behalf of another person, they show a more pronounced tendency to realize capital gains, compared to the situation where they trade for their own benefit. Furthermore, we find moderate effects of loss aversion and regret on the degree of PLR (see Model (5)). That is, a higher degree of loss aversion and regret lead to a smaller degree of PLR.

hand our results indicate that the gender differences are mainly driven by the fact that males improve their trading behavior to a higher extent through trading experiences.

Table 3: Tobit regressions with robust standard errors on DE, PGR, PLR and Alpha for subsample of inexperienced traders[a)]

	DE		PGR		PLR		Alpha	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	-0.144**	-0.210	0.053	-0.041	0.210***	0.189	-0.474*	-1.629**
Responsibility (1=yes)	0.099**	-	0.082**	-	-0.017	-	0.309*	-
Prosocial[b)](1=yes)	0.020	-	0.035	-	0.012	-	0.039	-
Individual x Prosocial	-	baseline	-	baseline	-	baseline	-	baseline
Individual x Individualistic	-	0.006	-	0.007	-	0.006	-	0.109
Responsibility x Prosocial	-	0.120**	-	0.123**	-	-0.004	-	0.509**
Responsibility x Individualistic	-	0.056	-	0.041	-	-0.010	-	0.249
Loss aversion (lambda)[c)]	0.042**	0.038*	0.021	0.020	-0.027*	-0.023	0.215*	0.229**
Risk aversion (CRRA)[d)]	0.018	0.011	-0.015	-0.016	-0.035*	-0.029	0.022	0.019
Female (1=yes)	-	0.013	-	-0.003	-	-0.012	-	-0.156
Pride[e)]	-	-0.013	-	-0.003	-	0.013	-	-0.021
Regret[f)]	-	0.019	-	0.003	-	-0.019*	-	0.085***
Empathy[g)]	-	0.001	-	0.002	-	0.001	-	0.016
Math grade (1–15)[h)]	-	0.005	-	0.003	-	-0.002	-	0.026
Study economics(1=yes)	-	0.014	-	-0.006	-	-0.023	-	-0.129
σ[i)]	0.196	0.189	0.160	0.157	0.148	0.143	0.674	0.615
Observations	80	80	80	80	80	80	80	80

Note: Ten participants show inconsistent choices (switching back and forth between accepting and rejecting lotteries) in the GJH task and are therefore omitted for the regression estimates. However, all results are qualitatively identical if we estimate the models without lambda. Level of significance: *** p <0.01, ** p <0.05, * p <0.10

a) According to a specific question with possible values ranging from 0 (no experience) and 10 (highly experienced). b) According to an incentivized Murphy task, possible values ranging from -16.26° to 61.39°. c) According to an incentivized GJH task, possible values ranging from 0.68 to 5.50. d) According to a modified and incentivized EG task, possible values ranging from -1.60 to 1.81. e) According to a specific question with possible values ranging from 0 (not proud at all) and 10 (very proud). f) According to a specific question with possible values ranging from 0 (no regret) and 10 (very much regret). g) According to the IRI based Saarbrücker personality questionnaire, possible values ranging from 12 to 60. h) Possible values ranging from 0 to 15; 15 is the best grade. i) The sigma (σ) value represents the estimated standard error of the interval regressions.

To better understand the driving forces of the treatment differences in disposition effects, we focus on subjects' selling behavior in more detail. In this respect, we run separate regressions on the Proportion of Gains Realized (PGR) (models 3–4) and on the Proportion of Losses Realized (PLR) (models 5–6). It becomes obvious from models (3) and (5) that the treatment effect is mainly caused by an increased PGR in the *Responsibility* treatment. More precisely, in the *Responsibility* treatment we find more PGR. This is highlighted by Model (3) which focuses on subjects' PGR as dependent variable. Here, we find that the coefficient of *Responsibility* is positive and significant. Thus, when participants decide on behalf of another person, they reveal a more pronounced tendency to realize capital gains, compared to the situation where they trade for their own benefit. Furthermore, the degree of loss aversion is a key driver of PLR (see Model (5)). That is, a higher degree of loss aversion leads to a smaller PLR.

If we ignore subjects' social value orientation (models (1), (3), (5)), it becomes obvious that the variable *Trade experience* is generally not significant. Nevertheless, trading experience is a crucial factor for the emergence of disposition effects. This becomes obvious if we incorporate subjects' social-value orientation in our analyses. In Model (2), the significant positive interaction term of *Responsibility x LOWexperience x Prosocial* Model demonstrates that the treatment effect is caused by inexperienced subjects characterized by a prosocial social-value orientation who exhibit more pronounced disposition effects when trading for others. Wald tests comparing the coefficients from Model (2), reveal that prosocial traders with low trading experience in the *Responsibility* treatment additionally show a higher DE than high experienced prosocial ($p = 0.088$) and low experienced individualistic ($p = 0.049$) investors in the *Responsibility* treatment. This again confirms our main findings of results 1a and 1b. Turning to Model (4), it becomes clear that this behavior is induced by an increased realization of capital gains by this group of investors, when trading affects a decision target. More precisely, we find a positive significant effect of the interaction of *Responsibility x LOWexperience x Prosocial* on PGR. Model (4) indicates that the elevated DE for the prosocial investors with low trading experience in the *Responsibility* treatment is caused by the greater proportion of divested paper gains in this group.

Result 4: *The driving force of the treatment effect can be attributed to a significantly higher proportion of gains realized in the responsibility treatment. This behavior is initiated by inexperienced traders with prosocial value orientation.*

4.4 The role of investor motivation

A further possible explanation for the treatment effect could be differences in investors' motivation to trade, as their performance in *Responsibility* will be revealed to a matched partner (Pelster and Hofmann, 2017). We assume that trader motivation could be reflected by the number of trades processed, as trading is associated with effort. Besides the first indications from Table 2 regarding the trading volume, we provide a more detailed overview of the trading volume. Table 4 focuses on the data of the *Responsibility* treatment. It illustrates the trading volume of traders with high and low trading experience in the *Responsibility* treatment.

Table 4: Trading volume in different subsamples of the *Responsibility* treatment

	Trading volume[a)]		p-value[b)]	**Trading volume**[a)] **low experienced**		p-value[b)]
	High experienced	*Low experienced*		*Indivi-dualistic*	*Pro-social*	
Total trades	226.3	241.0	0.822	211.9	270.1	0.244
	(122.1)	(161.3)		(153.5)	(167.0)	
Buying volume	146.1	151.3	0.774	135.4	167.2	0. 203
	(69.6)	(90.2)		(87.1)	(92.4)	
Selling (gains) volume	45.7	49.6	0.778	31.8	67.4	0. 083
	(39.7)	(53.3)		(29.2)	(65.5)	
Selling (losses) volume	33.6	37.9	0.727	44.1	31.8	0.930
	(34.5)	(45.9)		(60.7)	(23.3)	

Note: Standard deviation in parentheses. The table compares trading volume based on subjects' trading experience (columns 1–3) and based on the SVO of subjects with low trading experience (columns 4–5).
a) Number of traded shares in all stocks.
b) According to a Wilcoxon signed-rank test.

It becomes obvious that the total trading volume does not differ between the different levels of trading experience. A conspicuous finding can be observed when focusing on the trading volume of subjects based on their SVO. That is, prosocial inexperienced subjects even process a weakly significant higher trading volume of capital gains (67.4) compared to individualistic subjects (31.8) (Wilcoxon signed-rank test, p = 0.083). This underlines the finding that the significant treatment effect of the PGR only arises because prosocial traders with low trading experience process a higher trading volume. Hence, we find that this group is even more motivated when trading on behalf of others.

A further method to control for investors' motivation is to focus on their understanding of the stock types. In the Weber and Camerer (1998) framework subjects know that each of the six stocks follows a distinct type, i.e., stocks with a better rating are more likely to increase. As a consequence, subjects have the possibility to identify a stock's type by counting the number of stock price increases/decreases. Our idea is that, if an investor is motivated to achieve a good performance, she should therefore try to find out the stocks' characteristics. Subjects' evaluations of the stock types are measured by the guess score we elicited in the experiment. It turns out that Mann-Whitney tests focusing on the guess scores after period 7 and 14 find no significant differences between experienced and inexperienced investors (period 7: $p = 0.371$; period 14: $p = 0.210$). The same holds when comparing guess scores between prosocial vs. individualistic subjects (period 7: $p = 0.761$; period 14: $p = 0.316$). This adds further support against the idea that inexperienced prosocial investors exhibit more pronounced disposition effects because of a lack of motivation.

Result 5: *The higher disposition effect of inexperienced traders with prosocial value orientation in* Responsibility *is not driven by less motivation of these individuals.*

5. Conclusion

This paper analyzes the disposition effect of investors who decide on behalf of other subjects. A special interest was whether prosocial investors in this case exhibit more pronounced disposition effects, as they feel closer to the other person. We explored this research question experimentally, as this enables us to tackle possible obstacles which are hard to isolate in the field. First, the setup allows us to focus on the pure effects of intrinsic motivation from the investors' side. That is, the experimental framework of a *Responsibility* treatment avoids by design that extrinsic motives from the investors' side could be a reason for behavioral changes in social trading. More precisely, as investors in *Responsibility* are not paid based on their trading performance, it can be ruled out that they strive for a (high) own monetary benefit. Moreover, we exogenously match the decision targets to the investors. Thus, it cannot be that investors have extrinsic motives to attract traders that are following them. The latter can play an important role in online social trading platforms where 'leading' traders receive bonuses for each trader who decides to copy their trades (Pelster and Hofmann, 2017).

The results support our hypothesis, i.e., we observe stronger disposition effects when investors' trading decisions affect the payoff of other subjects. More concrete, inexperienced traders with a prosocial attitude more readily sell capital gains when taking decisions for others. By contrast, no treatment effect can be found for individualistic subjects and decision-makers with a high degree of trading experience. We find that deciding on behalf of others abolishes the correlation of individual loss aversion and the disposition effect (Andersson et al., 2014) which, however, has no effect on treatment differences. We focus on a couple of robustness checks on investors' trading volume and their understanding of the stock types to proxy their motivation when deciding for others. However, we find no evidence that inexperienced investors in the *Responsibility* treatment could be less motivated. That is, no differences in trading volume or between the precision of guess scores can be found for the different trader types.

The results are exciting, as we can conclude that even intrinsic motives (which may be shared by friends) are sufficient to cause pronounced disposition effects when deciding for others. It is noteworthy that social distance in our anonymous design should rather be less distinctive. Hence, it is likely that the effects are more pronounced when investors decide for their friends and therefore know the other person. Our findings have practical implications for everyday trading. That is, private investors who consider delegating their investment decisions to friends should be cautious, as prosocial investors with a low degree of trading experience exhibit pronounced disposition effects. Referring to the findings of Shapira and Venezia (2001), we conclude that professional traders exhibit lower disposition effects for two reasons: First, they are characterized by a higher degree of trading experience. Second, we speculate that individualistic persons might self-select to a career as a trader. Further research could add treatments testing whether a monetary incentive for traders deciding on behalf of others affects the results, and in which direction. It could be expected that the monetary incentive does not affect the (inexperienced prosocial) traders and they further perform worse.

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III Determinants of financial loss aversion: The influence of prenatal androgen exposure (2D:4D)

Single author

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Abstract

Loss aversion is allegedly one of the key drivers for behavioral biases in the financial economics context, e.g. the disposition effect and the equity premium puzzle. However, while loss aversion is important, little research has investigated its determinants. Therefore, loss aversion was elicited using an incentivized task. Results reveal that lower loss aversion is associated with greater prenatal testosterone exposure, as measured by the 2D:4D. Surprisingly, this correlation between loss aversion and the digit ratio only holds true for the right hand. Furthermore, a greater number of siblings, improved math skills and the male gender are all associated with reduced loss aversion. The results contribute to the understanding of loss aversion in general and the effect of prenatal testosterone on economic preferences in particular.

Keywords: Digit ratio; 2D:4D; loss aversion; gender differences.

JEL Codes: C91, D14, D81, J16

IV The disposition effect in farmers' selling behavior – an experimental investigation

with Elisabeth Vollmer and Oliver Mußhoff

Forthcoming in *Agricultural Economics*

Abstract

The identification of the optimal selling time of stored goods is among the most essential economic decisions on a farm. Beyond monetary aspects, behavioral factors may influence farmers' selling behavior. In financial economics, the disposition effect is a commonly observed phenomenon. It indicates that investors hold losing stocks too long, while they sell stocks that have increased in value too early. In the context of agriculture, this behavioral bias has not been analyzed thoroughly yet. To close this research gap, we conducted an incentivized online experiment with 112 farmers in Germany. The experimental design was based on well-proven experiments from financial economics and adapted to an agricultural decision context where stored goods must be sold. Farmers were provided information on the uncertain price developments. In addition, lotteries were conducted to elicit farmers' risk attitude, probability weighting, and loss aversion. Results indicate that there is a robust disposition effect in farmers' selling behavior. Furthermore, more loss-averse farmers exhibited a higher disposition effect.

Keywords: Disposition effect, experiments, farmers.

JEL Codes: C91, D81.

V Be close to me and I will be honest: How social distance influences honesty

with Andreas Ostermaier

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Abstract

We conducted a laboratory experiment to examine how honesty depends on social distance. Participants cast dice and reported the outcomes to allocate money between themselves and fellow students or the socially distant experimenter. They could lie about outcomes to earn more money. We found that dishonesty increases with social distance. However, responsiveness to social distance depends on personal preferences about inequity and honesty as a moral value. We observed selfish 'black lies' but not altruistic 'white lies' (outcomes were not understated to reduce inequality). Our results suggest that the reduction of social distance can promote honesty in social interactions.

Keywords: Cheating, honesty, social distance, experiment.

JEL Codes: C91, D63, D64.

1. Introduction

Dishonesty is common in social interaction. However, does it matter for honesty how close interaction partners are to each other? Dishonesty is costly because it reduces the value of the interaction and may even preclude it. Social distance is a likely influence on honesty and it is under the control of the interaction partners. It is therefore important to understand the role of social distance, which has largely eluded attention in prior research. We show, in a laboratory experiment, that dishonesty increases with social distance. Furthermore, we find that the effect of social distance depends on personal preferences about honesty and fairness. This study contributes to the growing literature that examines drivers of honesty, both on the personal and the situational level. It argues for the reduction of social distance to promote honesty in social interactions but also shows the contingency of this approach on personal preferences.

Honesty is, to a large extent, a matter of personal preferences (Gibson et al., 2013). Experimental research has consistently found individuals that did not lie despite strong economic incentives for dishonesty, whereas others lied readily. Motivated by pure aversion to lying, some even disregard potential favorable consequences of lying for themselves or others (López-Pérez and Spiegelman, 2013). In economic terms, there is an intrinsic cost to lying, which is prohibitively high for some (Arbel et al., 2014; Kajackaite and Gneezy, 2017). Refuting the idea of a simple distinction between economic and ethical types, Gibson et al.

(2013) traced honesty back to heterogeneous preferences. Specifically, they showed that the concept of protected values explains variation in honesty. People differ in how much they consider honesty a protected value, which they are reluctant to trade off for other values.

Although individual preferences matter, lying depends heavily on the situation. People lie to others in the context of social interactions, and most respond to situational factors. In particular, Erat and Gneezy (2012) showed that people, on average, consider the consequences of their lies for themselves and others. Their taxonomy of lies includes ‘selfish black lies,’ which benefit the liar at the expense of others, but also ‘altruistic white lies,’ which help others at the expense of the liar. A host of economic studies have shed light on social influences on honesty other than the consequences of lying (Rosenbaum et al., 2014). Important findings include that people lie more readily in groups than alone (Kocher et al., 2018) and that lying is responsive to the magnitude of incentives for dishonesty (Kajackaite and Gneezy, 2017). Hence, while few people never lie, most are ready to lie when it pays off.

Social distance—i.e., how close agents are to each other (Akerlof, 1997)—has barely been considered as a potential influence on honesty. By contrast, several studies have documented the effect of social distance on the outcome of social interactions. For instance, Buchan et al. (2006) showed that other-regarding preferences, such as trust, reciprocity, or altruism, are more pronounced with a lower degree of social distance. Eckel and Grossman (1996) found that altruism in dictator games varied with the distance between the dictator and the recipient, who were either anonymous students or reputable charities. Charness and Gneezy (2008) found that dictators are more willing to give to recipients as social distance decreases. Similarly, Zultan (2012) reported more cooperation in the ultimatum game after pre-game face-to-face communication, which apparently reduces social distance.

While these results suggest an interaction between social distance and other-regarding preferences, evidence for a potential interaction between social distance and honesty is limited. Related experiments use the sender–receiver game, where the sender has private information about two options and sends the receiver a potentially dishonest message about which option to choose (e.g., Erat and Gneezy, 2012; Lundquist et al., 2009; Sutter, 2009). Thus, van Zant and Kray (2014) found that pre-game face-to-face communication increases senders’ honesty. However, the sender–receiver game involves strategic considerations and leaves the final decision to the receiver. In die-rolling experiments, in turn, participants always lie to the socially distant experimenter. Kajackaite and Gneezy (2017) and Meub et al. (2016) had participants interact either with the experimenter or other participants, but had them make binary choices and did not observe lies on the individual level.

To explore the effect of social distance on honesty and, at the same time, account for the potential interaction with preferences about fairness, we combined a dictator game with the task of rolling a die and reporting the outcome to earn money (Fischbacher and Föllmi-Heusi, 2013). The outcome of the die roll can be seen as a 'suggestion' for the dictator of how to allocate money between herself and someone else. The dictator could override this suggestion to earn more (or less) money, but needed then to misstate the outcome—i.e., lie about it. Participants' reports depended therefore on their preferences for honesty and fairness. To manipulate social distance, we had them allocate money either between themselves and other participants or between themselves and the experimenter (Kajackaite and Gneezy, 2017). Like Kocher et al. (2018), we had them perform this task on the computer, which allowed us to observe honesty on the individual level.

Our findings contribute to prior research in several ways. First, we establish that honesty depends on social distance. Exploiting variation in outcomes, we do not find, in turn, that dishonesty is driven by inequality. Second, we tie honesty to heterogeneity in personal preferences about fairness and honesty. In particular, our data indicate that fairness concerns and social distance interact to influence honesty. This observation is in line with the finding that social distance affects altruism and cooperative behavior (Charness and Gneezy, 2008; Eckel and Grossman, 1996; Hoffman et al., 1996). Third, the observation of lies on the individual level enables us to identify 'altruistic white lies,' which reduce inequality at the expense of the liar (Erat and Gneezy, 2012). Such lies have been observed in sender–receiver games, but not in other settings, such as the die-rolling task.

The influence of social distance on honesty is interesting because it relates to most interactions that involve honesty. For example, public authorities usually appear as a distant and impersonal interaction partner to people, and honesty is indeed a major concern in tax collection. In this and other areas, people often interact through intermediaries, who increase social distance between the interaction partners. More generally speaking, the wide use of the internet has profoundly simplified but also depersonalized communication. Our findings suggest the reduction of social distance as an option to consider when honesty is an issue. In particular, this may be a worthwhile alternative to control mechanisms, which have been found to crowd out trust and social behaviors (Falk and Kosfeld, 2006).

2. Theoretical framework and hypotheses

Starting from a die-rolling task, we derive a simple utility function to model the agent's reporting choices (Kajackaite and Gneezy, 2017). We enhance the model by incorporating social distance. In the die-rolling task, the agent observes and reports a state of nature t (i.e., the outcome 1, 2, 3, 4, 5, or 6 of rolling the die), where different states are associated with different monetary payoffs. The agent's payoff m depends on whether she reports truthfully the state that she observes ($t' = t$), resulting in payoff m_t, or a different state ($t' \neq t$), resulting in payoff $m_{t'}$. If the agent misreports the state of nature by claiming it is more favorable for her than it actually is, lying earns her a monetary surplus of $m_{t'} - m_t$. At the same time, however, lying also has a psychological cost C_i.[1] Depending on how much the agent minds lying, this cost can be anywhere between zero and prohibitively high ($C_i \in [0, \infty]$). Taking into account both the benefits and costs of lying, the agent lies if her utility from lying is greater than from being honest. That is, she lies if

$$m_{t'} - C_i > m_t. \tag{1}$$

While lying depends on personal preferences for honesty, it often also affects others in some way (Erat and Gneezy, 2012). For example, a lie to increase an agent's payoff might reduce another agent's payoff. Relating the agent's report of t or t' to the maximum outcome k, so that t and t' are within $[0, k]$, individual j's outcome is then m_{k-t} if i is honest, and $m_{k-t'}$ if she is dishonest. As j's outcome depends on i's report, questions arise about how i's preference for honesty interacts with her social distance to j. Based on the observations of the effect of social distance on altruistic behavior (Buchan et al., 2006; Eckel and Grossman, 1996) and the effect of pre-game face-to-face communication on honesty in sender–receiver games (van Zant and Kray, 2014), we assume that the intrinsic cost of lying is a function of social distance. That is, people are more reluctant to lie to others who are closer to them. The individual psychological cost of lying can then be modeled as the sum of some basic cost of lying BC_i, which does not depend on social distance, and some additional cost, which is a function of social distance. Hence, we refine our notion of C_i in equation (1) to be calculated as:

[1] Kajackaite and Gneezy (2017) distinguish intrinsic costs C_i and extrinsic costs γ_i of lying, where the latter arise from being exposed as a liar. Our experimental framework keeps γ_i constant.

$$C_i = BC_i + f(SD_i). \tag{2}$$

Drawing on the aforementioned evidence, we propose the following hypothesis regarding the effect of social distance on honesty:

H1: Dishonesty increases with social distance.

Given the cost of lying, people would typically lie to earn monetary benefits that compensate this cost. Die-rolling experiments offer little empirical evidence, in turn, that people lie to reduce their payoff (Gneezy et al., 2018; Kocher et al., 2018). The only exception is a study by Utikal and Fischbacher (2013), whose data suggest that nuns told ‘disadvantageous lies’ in their experiment. It seems that the nuns in Utikal and Fischbacher’s experiment paradoxically lied to dodge the suspicion of lying, which truthfully high reports of their honesty might have raised. It should be noted, though, that regular die-rolling experiments do not allow us to observe lying on the individual level. Hence, there might be disadvantageous lies that are masked by a higher rate of advantageous lies.

Moreover, these experiments did not manipulate social distance. Instead, social distance between the participant and the experimenter was uniformly large. Kajackaite and Gneezy (2017) manipulated social distance in the sense that they had participants either interact with other participants or the experimenter. However, they did not observe lies on the individual level because they wanted to exclude that participants believed they could possibly be exposed as liars. There is evidence, though, that people lie for altruistic reasons from other experiments, where participants, unlike in die-rolling experiments, interact with each other rather than the socially distant experimenter. Specifically, Erat and Gneezy (2012) observed that senders lied to increase receivers’ payoffs, even when this reduced their own payoffs in the sender–receiver game. They named these lies, which were told by 33 percent of their sample, ‘altruistic white lies.’ Recollecting the evidence for disadvantageous and altruistic white lies, we derive two hypotheses to predict how social distance and favorable inequality combine to affect honesty:

H2a: Under a high degree of social distance, agents do not lie to reduce their own outcome for the benefit of others.

H2b: Under a low degree of social distance, agents lie to reduce their own outcome for altruistic reasons.

3. Experimental design

We conducted our experiment with two treatment conditions in the laboratory. The experiment was programmed in z-Tree (Fischbacher, 2007). We collected data from 120 participants (60 in either condition), whom we recruited with ORSEE (Greiner, 2015). One condition required an additional 60 ‘passive’ participants, from whom we did not collect any data. To describe the experimental design, we first explain the die-rolling task to test participants’ preferences for honesty. We then depict our manipulation of social distance. Finally, we provide a brief overview of the additional tasks that we implemented as part of our post-treatment questions.

3.1 Die-rolling task

Participants’ primary task consisted of rolling a die and reporting the outcome, which earned them money. Participants could usually earn more money by misreporting the outcome than by reporting it truthfully, which created an incentive for them to lie. This design was adopted from Kocher et al. (2018), who computerized Fischbacher and Föllmi-Heusi’s (2013) classical die-rolling task.

Participants first read the instructions (Appendices B.1 and B.2) and then answered comprehension questions to make sure that they understood their task (Appendix B.3). Next, they launched a short video of a six-sided die being rolled on their computer screen, resulting in one of six possible outcomes (⚀, ⚁, ⚂, ⚃, ⚄, or ⚅). To mimic a real die roll, we created a random mechanism to ensure that each outcome was equally likely to occur, and participants were informed of this. They also knew that the dice were rolled independently for each of them so that they could not infer the others’ outcomes from their own. After each die was rolled, the video was frozen so that the outcome remained visible for about 12 seconds. Participants then typed the outcome that they wanted to report into a field and submitted it. Regardless of the actual outcome, they could report any number—‘1,’ ‘2,’ ‘3,’ ‘4,’ ‘5,’ or ‘6’—, which allowed them to be honest as much as to be dishonest.

Participants’ payoff ultimately depended on their reported outcome and not the actual outcome of their die roll. Technically speaking, our experiment resembled therefore a dictator game, where the proposer or dictator splits some amount between herself and someone else. Unlike in the dictator game, however, where she splits the amount at her own discretion, the die-rolling task can be taken to suggest a random split. The participant could always neglect this suggestion and, like a dictator, implement any split by just reporting a different number than the outcome. This, however, required her to misstate the outcome—

i.e., to lie about it. We told participants specifically that their report determined their share of a fixed amount of €10. Table 1 shows how reports translated into payoffs. Clearly, a payoff maximizer would always report ‘5,’ regardless of the actual outcome, to earn the maximum payoff of €10.

Table 1: Payoff structure of die-rolling task

Report	Participant’s share	Remainder
‘1’	€2	€8
‘2’	€4	€6
‘3’	€6	€4
‘4’	€8	€2
‘5’	€10	€0
‘6’	€0	€10

Unlike in Fischbacher and Föllmi-Heusi’s (2013) original experiment, in which lying could not be observed on the individual level, Kocher et al.’s (2018) computerized version provided us with richer data, as we recorded both the actual and the reported outcomes. However, knowing that the outcome in the video was determined by the software, participants could easily infer that their lies could be detected by the experimenter, and instructions did not claim or suggest that the experimenter would not know the actual in addition to the reported outcomes. Of course, participation was anonymous so that lies could only be traced back to participants’ working stations but never to any individual person. Kocher et al. (2018, p. 3) acknowledge that the observability of dishonesty might reduce the level of lying. That said, prior evidence suggests that complete observability and complete privacy have only marginal effects on the absolute extent of lying (Bäker and Mechtel, 2015; Gneezy et al., 2018; Houser et al., 2016).[2] Nonetheless, we confined our analysis to relative comparisons.

3.2 Manipulation of Social Distance

We implemented a between-subject design with two treatment conditions. In the Low Degree of Social Distance condition, participants reported or misreported their outcomes to split €10 between themselves and ‘passive’ participants, who served as recipients. The re-

[2] It should also be noted that any effect of observability works against H1, which predicts that dishonesty increases with social distance. Our estimate of dishonesty in the High Degree of Social Distance condition is therefore best seen as a lower bound.

cipients were other participants from the same pool of students.[3] In the High Degree of Social Distance condition, they split €10 between themselves and the experimenter.

In the Low Degree of Social Distance sessions, participants were randomly assigned to one of two rooms when entering the laboratory. The participants in one room rolled dice and reported outcomes just like the participants in the High Degree of Social Distance treatment, as described in the previous section. The recipients in the other room were told about the participants' task while they waited for them to roll dice and submit reports. After receiving their instructions, the only task of the recipients consisted of drawing numbers that assigned a random participant who split the €10 to each of them. We had twice as many participants in the Low Degree of Social Distance sessions as to the High Degree of Social Distance sessions—one half of them participants, the other half recipients—and assigned one recipient to each participant. Hence, the number of participants who split the €10 were held constant across conditions. Participants and recipients were randomly matched and interacted anonymously.

The participants were told that their report would determine how a sum of €10 would be split between themselves and a recipient. Specifically, the 'participant in the other room' would receive the remainder of the €10. In the High Degree of Social Distance treatment, there were no recipients and the remainder of the €10 went to the experimenter instead. Clearly, social distance between fellow students is lower than between students and the experimenter. That said, anonymity saved participants from having to justify their decisions to recipients in the Low Social Distance condition. Table 2 summarizes the differences between the conditions.

Table 2: Comparison of the two treatments

	Treatment	
	High Degree of Social Distance	Low Degree of Social Distance
Participants per session	10	20
Reporting participants	10	10
Passive participants	0	10
Remainder (Table 1)	Remains with the experimenter	Goes to a passive participant

[3] We use the term 'recipient' for convenience in the paper. The instructions refer to all participants just as 'participants' and, to distinguish participants in one room from those in the other room, 'participants in the other room' (see Appendices A.1 and A.2).

3.3 Additional experimental tasks

After rolling the dice and reporting the outcomes, participants answered post-treatment questions. These questions included an incentivized multiple price list task, which we adopted from Blanco et al. (2011) to measure the participants' compassion parameter β (Appendix B.4). Participants were told only after completing the dice game and before starting the post-treatment questions that their answers to these questions could earn them additional money.

In addition, we had participants indicate their agreement with several statements adopted from Gibson et al. (2013) to measure the extent to which honesty was a 'protected value' for them. A value is 'protected' when an individual is reluctant to trade it for other values. In particular, someone who considers honesty a protected value would refrain from lying to earn money. By contrast, values that are not protected can readily be traded for each other (Appendix B.5). Finally, we gathered demographics as potential controls.

4. Results

4.1 Summary statistics

Our main interest was participants' honesty in reporting their outcomes. Figure 1 depicts the actual outcomes as well as the reported outcomes for each participant under both conditions. The figure shows that many participants reported their outcomes truthfully, placing them on the main diagonal. Under both conditions, there were, however, 'liars' who misstated their outcomes. In particular, lying was more frequent in the High Degree of Social Distance condition. It is striking that, whenever participants misreported their outcomes, they overstated rather than understated their outcome to earn more rather than give up any money.

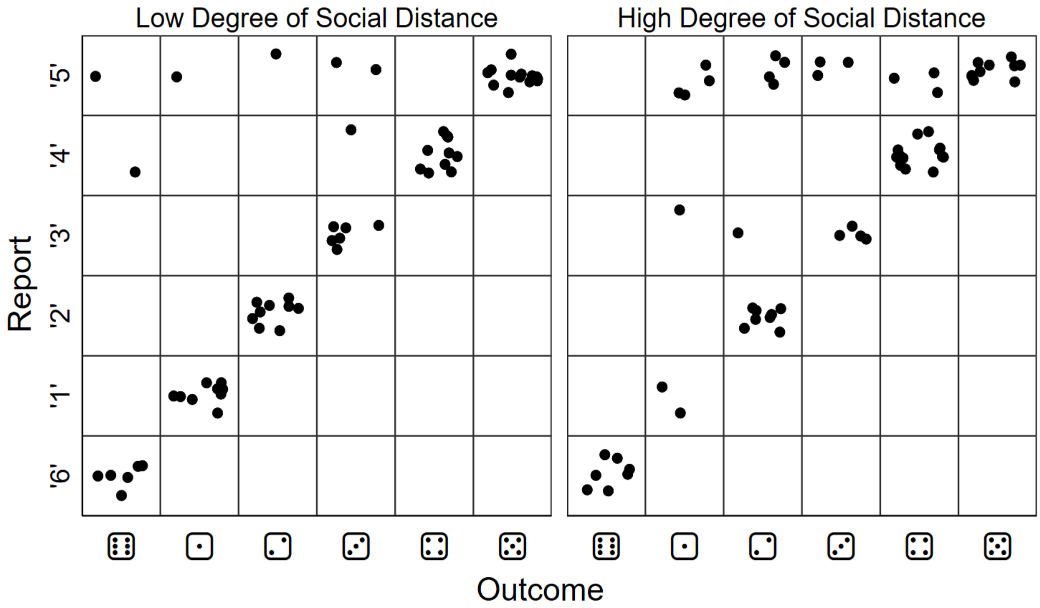

Figure 1: Outcomes and reports.

Note: Regardless of the outcome, a report of '1' earned the participant €2, a report of '2' earned €4, and so forth. A report of '6,' however, resulted in zero payoff.

As a measure of dishonesty, we related the additional payoff that a participant created by overstating her outcome to the maximum additional payoff that she could have possibly created. For example, the participant in the left panel of Figure 1, who reported a '3' for an outcome of ⚀ under the High Degree of Social Distance condition created a relative additional payoff of .5 = (3 − 1) ÷ (5 − 1). While the range of the (absolute) additional payoff depends on the actual outcome (e.g., it is 5 with an outcome of ⚅ and a report of '5'; 4 with an outcome of ⚀ and a report of '5,' etc.), the relative difference is restricted to range from 0 to 1. Obviously, the relative difference is 0 for participants whose outcome is ⚄ and who therefore cannot lie, unless they understate their outcome (which none of them did according to Figure 1).

The relative additional payoff averaged .11 (SD .30) in the Low Degree of Social Distance condition and .25 (SD .43) in the High Degree of Social Distance condition. Similarly, the average proportion of liars was .12 (SD .32) in the former condition and .27 (SD .45) in the latter. The numbers are necessarily similar since most liars reported an outcome of '5' in order to maximize their payoff rather than choosing a value somewhere between their actual outcome and the maximum outcome. Figure 1 shows exactly two participants in each condition who overstated their outcomes but reported a value less than '5.'

4.2 Hypothesis tests

H1 states that honesty increases when social distance decreases and vice versa. Figure 1 suggests that this is indeed the case because there were more participants above the main diagonal in the High Degree of Social Distance than in the Low Degree of Social Distance condition. In line with this observation, the Mann–Whitney reveals that the relative difference between reports and outcomes is higher in under a high than under a low degree of social distance on average and thus confirms H1 (.25 > .11, p = .033). Likewise, Fisher's exact test reveals that the proportion of liars was significantly larger in the former than in the latter condition (.27 > .12, p = .062).[4] Hence, there are more liars and there is more lying under a high degree of social distance compared to a low degree of social distance.[5]

Note that these two effects—the increase in lying and the increase in the proportion of liars—cannot be distinguished statistically. Besides the aforementioned two people in each condition who overstated their outcomes but reported something below '5,' all other participants claimed the maximum payoff once they decided to lie. Therefore, while there were certainly more liars under a high degree of social distance, we did not find that these liars also felt encouraged to tell more extreme lies as social distance increased and vice versa.[6] This observation is in line with related research, which shows that people care about how they are perceived by others when they are caught lying (Gneezy et al., 2018; Kocher et al., 2018). Knowing that lies could always be detected by the experimenter, participants either refrained from lying or, once they had decided to lie, did not bother reporting anything below '5' to increase the probability of being perceived as an honest person despite lying.

One might expect that reduced social distance leads to lies that balance outcomes as fairness preferences are more pronounced for peers (López-Pérez, 2012). However, we found no evidence that fairness considerations in a classical sense (Fehr and Schmidt, 1999) mediate the effect of social distance on honesty. Figure 2, which depicts the percentage of liars for each outcome both under a low and high degree of social distance, illustrates this result. It shows that the percentage of liars declined in both conditions as the outcome approached ⚂, which paid the participant more than the recipient or experimenter. This trend is eclipsed, however, by a large number of participants who overstated the outcome ⚂ in

[4] In the following we use Mann–Whitney test to compare relative additional payoffs and the Fisher's exact test to compare the proportion of liars.

[5] Regression estimates confirm these results, both with and without covariates (see Appendix B.6).

[6] The relative additional payoff created by lying is slightly higher for the high degree of social distance treatment. However, a Mann–Whitney test for the subsample of liars does not show this difference to be statistically significant (p = 0.448).

the Low Degree of Social Distance condition. Moreover, all participants reported the outcome ⚅ honestly in the High Degree of Social Distance condition, earning them zero payoff. Hence, we did not find that honesty hinges on outcome inequality.

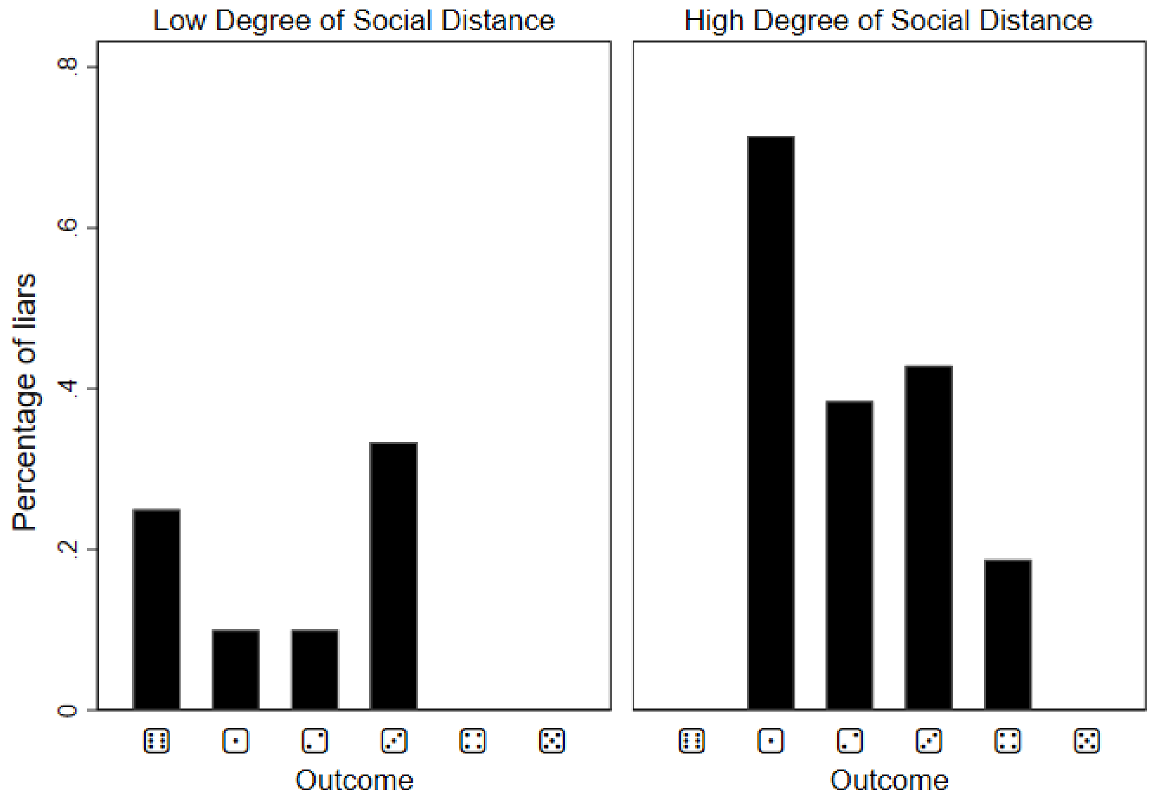

Figure 2: Outcomes and lying.
Note: Percentage of lying participants for each outcome and condition. A report of '1' earned the participant €2, a report of '2' earned €4, and so forth. A report of '6,' however, resulted in zero payoff.

H2a and H2b refer specifically to favorable ($t > k - t$) as opposed to unfavorable inequality ($t < k - t$)—i.e., the situation in which the actual outcome favors the participant over the recipient or experimenter. While these predictions do not allow for a direct statistical test, Figure 1 readily confirms H2a and refutes H2b. The figure shows that participants never lied to reduce favorable inequality, which would require them to understate their outcomes. There was no participant who understated her outcome, neither under a high nor low degree of social distance. This result is in line with H2a, but contradicts H2b.

Given that participants did not lie to reduce favorable inequality, we examine whether they lied more under unfavorable than under favorable inequality depending on social distance. Specifically, while participants can always increase their payoffs by overstating their outcomes, lying is arguably easier to justify when it reduces unfavorable inequality rather than increases favorable inequality (i.e., envy is supposed to be more substantial than compassion). Figure 1 shows that more participants lied across conditions when their outcomes were ⚅, ⚀, or ⚁ than when they were ⚂, ⚃, or ⚄, leaving them with less than half of the €10 in the former case. However, neither the difference in the percentage of liars (.25 > .14) nor the relative increase in payoffs because of lying (.23 > .13) was significant. That said, the difference in the percentage of liars in the High Degree of Social Distance condition comes close to being significant (.37 > .18, p = .144).

4.3 Preferences for honesty and compassion

To test for potential factors causing the main effect of social distance, we conducted further analyses. First, we examined how honesty depends on the extent to which participants consider it a protected value which they would not trade for other values. We split the sample at the median of the protected values scores, which we determined according to Gibson et al. (2013), to distinguish participants who are more and less reluctant to trade honesty for other values. Unsurprisingly, the relative increase in payoffs (.10 < .25, p = .015) because of dishonesty and the percentage of liars (.10 < .28, p = .019) were overall lower among participants with an above-median score. This said, participants who are more inclined to trade honesty should be particularly responsive to the effect of social distance. The increase in payoffs because of dishonesty (.12 < .41, p = .006) and the percentage of liars (.15 < .46, p = .010) were indeed significantly lower under a low as opposed to a high degree of social distance among these participants.

Second, to follow up on our analysis of outcome inequality, which we mentioned did not mediate the effect of social distance on honesty, we investigated the influence of fairness preferences. Specifically, we distinguished between participants with high and low levels of compassion, which quantifies the disutility of favorable inequality (Fehr and Schmidt, 1999). We used Blanco et al.'s (2011) multiple price list task to obtain the compassion parameter β for each participant and split our sample again at the median. We found that the relative additional payoff obtained by lying and the portion of liars differed between conditions for participants with above-median aversion to favorable inequality. Specifically, in the High Degree of Social Distance condition, the relative difference (.23 > .03, p = .020) and the proportion of liars (.25 > .03, p = .028) were significantly higher compared to the Low Degree of Social Distance condition. In contrast, neither the relative difference (.26 > .17, p = .368) nor the proportion of liars (.28 > .19, p = .542) differed significantly between conditions for participants with below-median levels of β.[7] This observation suggests that the effect of social distance on honesty at least partially interacts with fairness preferences.

Taken together, these results imply that the effect of social distance on honesty is driven by people with specific preferences about honesty and fairness—namely participants who are willing to trade honesty for other values (i.e., whose protected values score is low for honesty) and participants who have a strong aversion against favorable inequality (i.e., high levels of the compassion parameter β). As a final analysis, we examined whether the effect of social distance on honesty can be attributed, more specifically, to participants with a combination of these preferences. In support of this conjecture, the difference of the relative additional payoffs (.00 < .45, p = .003) and the percentage of liars (.00 < .50, p = .005) between the High Degree of Social Distance and the Low Degree of Social Distance condition turned out significantly higher among those participants who combined these preferences.[8] We retain therefore that social distance, which is a factor that arises from the situation, interacts with preferences that pertain to the person.

5. Conclusion

Honesty depends on both personal and situational factors. The primary focus of this study was on the situational factor of social distance. Specifically, we had participants in a laboratory experiment allocate money between themselves and someone else, who was either another participant or the experimenter. Consequently, the other participant (i.e., a fellow student), was socially closer to the participant than the experimenter. The allocation involved honesty because participants received a random 'suggestion' of how to allocate money and they had to lie in order to depart from this suggestion. Technically speaking, our experiment combined a dictator game with Fischbacher and Föllmi-Heusi's (2013) die-rolling task. The outcome of the die roll suggests how money should be allocated, but participants can misreport outcomes to achieve a different allocation.

We predicted and found that dishonesty increases with social distance. Specifically, participants were less willing to lie at the expense of fellow students than at the expense of the experimenter. Evidence for the effect of fairness is less conclusive. We did not see outcome

[7] The same result holds for participants whose β exceeded the threshold of .5 from the literature (Blanco et al., 2011; Müller & Rau, 2016). The median of β was .53 in our sample and therefore close to this threshold.

[8] Again, regressions—without and with covariates—confirm this result (see Table B.2 in Appendix B.6).

inequality mediate the effect of social distance on honesty. That said, we found that participants lied more readily to increase their payoff when it was less than half of the available money, and they never lied to benefit others, irrespective of social distance. Hence, we observed what Erat and Gneezy (2012) call selfish ‘black lies’ but not altruistic ‘white lies.’ Beyond these results, which were related to the situation, participants’ personal preferences about honesty and fairness offered further insights into the drivers of our findings.

Specifically, additional analyses revealed that social distance did not matter to participants who tended to consider honesty a protected value, which they would not trade for other values. The effect of social distance was therefore only significant among those participants who were inclined to trade honesty and who therefore responded to the different situations that arose from our manipulation. Likewise, the effect of social distance turned out to depend on participants’ aversion to favorable inequality. Only participants who were sufficiently uneasy with inequality, even when it favored themselves over others, responded to our manipulation. These analyses reveal that the effect of social distance on honesty is mainly driven by the combination of personal preferences.

Taken together, our results offer evidence that social distance interacts with personal preferences about honesty and fairness to influence honesty. Further research is needed to improve our understanding of this interaction. In particular, future investigations may use tasks that offer participants a richer action space to allow them subtler choices. It might thus be possible to distinguish between different types of liars (Gneezy et al., 2013). Moreover, social distance can be manipulated in different ways. For example, participants might be put to cooperatively interact in order to reduce social distance before they perform the die-rolling task. The effect of social distance would also be interesting to investigate in the field, although this would make it harder to consider personal preferences.

Prior research has shed light on contextual factors related to ours. In particular, there is evidence that face-to-face as opposed to anonymous interaction (Holm and Kawagoe, 2010; van Zant and Kray, 2014) and personalized as opposed to standardized messages increase honesty (Cappelen et al., 2013). Participants in experiments have also been found to lie more often when they feel they are treated unfairly (Houser et al., 2012) and there is even evidence for people engaging in ‘white lies’ to benefit others (Erat and Gneezy, 2012) or to justify their dishonesty (Lewis et al., 2012). The effect of social distance, in turn, had not been directly examined yet. Social distance is arguably an important driver of honesty or dishonesty, though, since lying occurs in the context of social interactions. It is therefore interesting to see both that dishonesty depends on social distance and that this effect hinges on personal preferences for honesty and fairness.

Our findings have implications for the design of social interactions. In particular, there are many interactions that require honesty to a certain extent but that involve social distance. For example, citizens interact with ‘anonymous’ tax authorities—a case which has long created concerns about dishonesty. Measures to decrease social distance may be a worthwhile alternative to costly control, which is a common response to dishonesty. Likewise, intermediaries are often used in social interactions, such as transactions between firms. Although there are compelling reasons to rely on intermediaries, they also increase social distance and create room for dishonesty (Erat, 2013). While our results imply that social distance does not matter to some, it is an inexpensive means to elicit honesty from those who respond to it. Wherever honesty is important but hard to ensure, the reduction of social distance is worth considering as a way of promoting honesty.

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VI I might be a liar, but not a thief: An experimental distinction between the moral costs of lying and stealing

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Abstract

In this paper, we shed light on the different moral costs of dishonesty and stealing. To accomplish this, we set up a die-rolling task which allowed participants to increase their own payout through dishonesty or theft. The results show that participants have fewer reservations about dishonesty compared to stealing, which implies higher intrinsic costs for stealing. We found that gender contributes to this effect, as women distinguish significantly between lying and stealing, while men do not.

Keywords: Lying, Deception, Stealing, Laboratory Experiment, Behavioral Economics.

JEL Codes: C91, D63, D82.

1. Introduction

Immoral actions are commonplace and their consequences affect everyone in the private, business and public sector. Very topical demonstrations of large-scale dishonesty are the Volkswagen emission control defeat device (Wang et al., 2016) and the Libor manipulation (Abrantes-Metz et al., 2012). Volkswagen was dishonest regarding their emission values and thereby have harmed their customers and the public due to decreasing residual values of purchased cars and decreased air quality (Wang et al., 2016). The Libor manipulation, on the other hand, was a dishonest report of interest rates by large banks. These reports led to manipulated reference interest rates and damage to the global economy running into the millions of euros (Abrantes-Metz et al., 2012). Besides these examples of lying, stealing also harms the economy. The 2017 National Retail Security Survey indicated that a loss of $14.67 billion arises annually from inventory–related employee theft in the retail industry of the U.S. alone (National Retail Federation, 2017). Worldwide, there is about $48 billion of retail loss annually due to employee theft and about $48 billion from shoplifting (The Smart Cube, 2015). Such theft is mainly committed by men (Centre of Retail Research, 2010).

Due to the importance of immoral behavior in all of its manifestations in the economic context, several studies have focused on lying and stealing in different situations (see Rosenbaum et al., 2014). There is evidence that non-pecuniary moral costs arise from lying and stealing. This enhances the classical theory on immoral behavior as a product of income effects and the probability of getting caught and punished (Fischbacher and Föllmi-Heusi, 2013; Gneezy, 2005). Further research found that the intrinsic costs of lying depend on the context: Kocher et al. (2018) reported that teams lie more often than individuals. This effect is driven by communication which seems to reduce moral costs regarding dishonesty. Moreover, pay schemes affect the intrinsic costs of lying. Belot and Schröder (2013) showed that competition fosters lying compared to fixed wage and piece-rate incentives. Furthermore, participants in experiments have also been found to lie more often when they feel they are treated unfairly (Houser et al., 2012). The same context dependency holds true for the intrinsic costs of stealing. Greenberg (1993) revealed that unfairly treated participants who are underpaid show a higher tendency to steal. Moreover, it is more likely that people steal from their companies compared to individual coworkers, indicating varying intrinsic costs of stealing which are conditional on the victim (Greenberg, 2002). Summarizing these findings, there is evidence that costs of immoral behavior reasonably depend on factors allowing the self-justification of the decisions (Gravert, 2013).

While the framework conditions are crucial for lying as well as stealing, there has not been any attempt to compare the level of lying and stealing under identical incentives so far. However, there are two studies illustrating that such a comparison seems worthwhile. Gravert (2013) investigated stealing with two experimental tasks: the effort-based theft task according to Mazar et al. (2008) and a chance-based modified die-rolling task (Fischbacher and Föllmi-Heusi, 2013). The author found that the rate of theft was lower in the die-rolling setting, as moral costs are reduced by the effort put forth in the theft task. In this case, effort provides a self-justification for stealing. Therefore, this contribution provides evidence that different experimental settings complicate a comparison of lying and stealing between studies. In a further study conducted by Belot and Schröder (2013), the authors introduced a design which allowed participants to simultaneously lie and steal from the experimenters. Furthermore, they compared different pay schemes using this design. Their results indicate that there was no evidence for theft, while lying in various forms arose and amounted to 10% of participants' productivity in a previously carried out real effort task. Due to the different types of immoral behaviors which are possible in such a setting, stealing might be the less favorable because of higher moral costs.

To the best of our knowledge, there is no study comparing the amount of lying and stealing in an experiment with constant incentives and risk of being caught as a consequence of immoral behavior. The differentiation of the costs associated with these two immoral behaviors has relevant implications for social interactions and economic questions. If one of these immoral behaviors is associated with higher intrinsic costs for economic agents, it might be possible to reduce economic losses by reframing decisions in the various contexts. For instance, the transfer of responsibility to agents under conditions of asymmetric information and consequently the changed perception of the immoral action might increase behavior which is in accordance with moral convictions. To address this point, our study contributes to the literature in two ways: 1) We investigated whether intrinsic costs of lying and stealing differ. To achieve this, we implemented an experimental design in which participants in a lying treatment rolled a die on their computer screen and reported the outcome. In contrast, participants in a stealing treatment did not report the outcome, but rather allocated the money in private, i.e., it was possible to steal. Participants received an envelope containing the maximum possible payoff and were asked to take the money according to the die-rolling outcome. Based on the identical framework conditions and incentives, we were able to distinguish between moral costs of lying and stealing. 2) We investigated gender differences regarding intrinsic costs of lying versus stealing. Gender differences regarding the intrinsic costs of immoral behavior are a matter of discussion in the literature (Childs, 2012; Grosch and Rau, 2017). Results regarding a potential effect of gender on lying (Childs, 2012; Grosch and Rau, 2017; Gylfason et al., 2013; Houser et al., 2012; Kajackaite and Gneezy, 2017) as well as stealing (Friesen and Gangadharan, 2013; Gravert, 2013) are ambiguous. Thus, we shed light on gender-specific costs of immoral behavior in three fields: lying, stealing and distinctions between lying and stealing.

2. Theoretical framework and behavioral predictions

We will first define a theoretical framework explaining dishonesty as well as stealing as immoral behavior from an economic point of view. This framework is based on the model introduced by Kajackaite and Gneezy (2017). We start by considering a situation in which an individual i observes a state of nature t. This individual has the possibility to report t and earn the associated financial payoff m_t. It is also possible to report t', which results in payoff $m_{t'}$. The benefit of the immoral behavior then is $m_{t'} - m_t$.

However, immoral behavior is not solely associated with benefits. Disutility arises from violating a moral concept (Gneezy, 2005). These intrinsic costs are defined as C_i and are heterogeneous across individuals but constant for each individual i (Kajackaite and Gneezy, 2017). The range of C_i is restricted to $0 \leq C_i \leq \infty$, where $C_i = 0$ and $C_i = \infty$ characterize an individual without any intrinsic value for moral behavior and an individual with pure aversion to immoral behavior, respectively (López-Pérez and Spiegelman, 2013). A second disutility arises from immoral behavior if the individual i is exposed when acting immorally. This disutility can be described by a function $f(m_{t'}, m_t, p_i)$ which is increasing with the probability p_i of being exposed and the size of the immoral action $m_{t'} - m_t$ (Kajackaite and Gneezy, 2017). Individuals are heterogeneous regarding their disutility of getting caught. Therefore, we denoted this by including parameter γ_i with $\gamma_i \geq 0$.

Consequently, an individual will take immoral action whenever:

$$m_{t'} - C_i - \gamma_i f(m_{t'}, m_t, p_i) > m_t \,. \tag{1}$$

Starting from this theoretical framework, it becomes obvious that, on the one hand, incentives influence immoral behavior, and, on the other hand, intrinsic as well as extrinsic costs of immorality can influence a participant's decision to state t' instead of t. With this in mind, our experimental design becomes relevant. We held γ_i and $m_{t'}$ constant across treatments, but shifted the responsibility for the assignment of money, i.e., enabling lying in one experimental setting and stealing in another.

A review of the relevant literature does not yield information regarding the assumption of varying intrinsic costs associated with lying and stealing. In Table 1, we provide a brief overview about honesty levels in studies using self-reported outcomes (e.g. Fischbacher and Föllmi-Heusi, 2013) and theft tasks (e.g. Mazar et al., 2008). Here, self-reported outcomes measure lying, while theft tasks disclose the proportion of theft. It becomes obvious that honesty levels in both tasks are rather comparable. This is underlined by the literature review of Rosenbaum et al. (2014), which derived average full honesty levels from reviewed studies. They indicate overall full honesty of 52.9% for self-reported outcomes (6 observations) and 64.7% for theft tasks (8 observations), both with a relatively large spread: 33.5% - 74.0% for self-reported outcomes and 37.3% - 85.0% for theft tasks.

However, the studies of Belot and Schröder (2013) as well as Gravert (2013) indicate that a direct comparison of lying and stealing might be recommendable. Gravert (2013) found that self-reported outcome tasks reduce stealing compared to an effort-based theft task. Furthermore, providing the possibility to lie and steal simultaneously (Belot and Schröder, 2013) leads to the avoidance of stealing by participants. Thus, we assume that the intrinsic moral costs are higher for stealing, while theoretical predictions and previous research would imply constant intrinsic costs for both types of immoral behavior.

Table 1: Proportion of fully honest decisions in self-reported outcome and theft tasks of past studies (cf. Rosenbaum et al., 2014)

Study	Type	Sample	Overall full honesty in %
Gino and Wiltermuth (2014)	SRO	178 US citizens	76%
Fischbacher and Föllmi-Heusi (2013)	SRO	478 students	39%
Kocher et al. (2018)	SRO	273 students	59% - 69%
Gneezy et al. (2018)	SRO	916 students	67% - 74%
Friesen and Gangadharan (2013)	TT	115 students	67%
Gino and Pierce (2009)	TT	53 students	37%
Shu and Gino (2012)	TT	56 students	68%
Gino and Wiltermuth (2014)	TT	153 US citizens	41%

Note: SRO = self-reported outcome; TT = theft task

3. Experimental design

In this section, we describe our experiment. First, the design allowing participants to lie is introduced. Second, we explain the changes made in the experiment in order to test for theft. Third, we provide insights into the experimental procedure.

3.1 Die-rolling task

The general design was based on the die-rolling task introduced by Kocher et al. (2018) which is an extension of the Fischbacher and Föllmi-Heusi (2013) task. Before the task was carried out, participants had to correctly answer control questions regarding the task to ensure their understanding (see appendix C.1). Afterwards, participants saw a video of a die roll on their respective computer screens. A classic six-sided die was rolled in the video leading to six possible outcomes: ⚀ , ⚁ , ⚂ , ⚃ , ⚄ or ⚅. By drawing a random number, the software determined which outcome was shown. Participants were informed that the outcome of the die roll was determined by a random draw from six outcomes with an equal probability of occurrence. After the video played, the outcome of the die roll was visible on the participants’ screens for about 12 seconds. Subsequently, the participants’ task was to report the outcome of the die roll (‘Die number seen: ___’). Any possible outcome (1, 2, 3, 4, 5 or 6) could be entered by the participants. Thus, participants had the possibility to report dishonestly. The reported number determined the payoff of the participant, while the outcome of the die roll did not affect the payoff. We used a specific payoff structure for the die roll outcome (see Table 2). As in previous research, the highest payoff was associated with rolling a '5' and the lowest payoff with rolling a '6' (Fischbacher and Föllmi-Heusi, 2013). Therefore, a payoff maximizer would always report the number associated with the highest payoff, i.e., '5'.

Table 2: Payoff structure of die-rolling task

Entered number	Your payoff	Residual
'1'	€2	€8
'2'	€4	€6
'3'	€6	€4
'4'	€8	€2
'5'	€10	€0
'6'	€0	€10

As the participants were aware that the random number determining the outcome in the video was derived by the software, they could conclude that their lies were observed by the experimenter (Kocher et al., 2018).

3.2 Treatments

To answer our research questions, we implemented two treatments: *Lying* and *Stealing*. The general design of the die-rolling task described in the last section was used to observe *Lying* and was thus a replication of the Kocher et al. (2018) design. For the *Stealing* treatment, the die-rolling task was also carried out in order to determine the participants' payoffs and provide comparable framework conditions for both treatments. However, we then introduced specific modifications in the design to observe stealing: in the *Stealing* treatment, the participants were responsible for the allocation of money. Therefore, the outcome of the die-rolling task was not entered in the computer. Instead, each participant received an envelope containing five €2 coins, i.e., €10. In accordance with their respective outcomes, participants were asked to allocate the money in the envelope by removing their payoff and leaving the residual. Afterwards, they were asked to close the envelope. It was obvious from the instructions of the *Stealing* treatment (appendix C.1) that the envelopes were not collected during the experiment. The closed envelopes were simply left behind at the respective cabins.[1] In order to avoid any potential misleading with regard to the attribu-

[1] At the end of the experiment, one experimenter paid participants the remaining payment and a second experimenter made sure no one took the envelopes from other cabins.

tion of envelopes to cabins after the experiment, we prepared our envelopes with specific signs on the corners which were only visible under UV-light (see appendix C.2). We prepared each envelope with a specific sign combination associated with a particular cabin number. With this modification in the *Stealing* treatment, we let participants allocate the money in the envelope by themselves and therefore allowed theft. Table 3 illustrates the characteristics of both treatments.

Table 3: Comparison of treatments

	Treatment	
	Lying	*Stealing*
Control questions	x	x
Die roll video	x	x
Outcome of die roll suggests payoff	x	x
Enter die-rolling outcome in computer	x	-
Receive an envelope containing €10	-	x

3.3 Experimental procedure

The experiment was conducted in autumn 2017 at the University of Göttingen. A total of 80 participants (46.25% female) took part in the experiment, with 40 individuals randomly assigned to each treatment. The participants were recruited using ORSEE (Greiner, 2015) and the experiment was programmed in z-tree (Fischbacher, 2007).

We favored a between-subject design for our treatment comparison for the following reasons: i) In a within-subject design, we would not be able to randomize the payoff-relevant treatment for the individuals as in Kocher et al. (2018). In our case, we would have to hand over the envelope containing real money, which would disclose the payoff-relevant task within the experiment directly. ii) It would be possible to pay individuals for all treatments in a within-subject design; however, this would lead to order effects induced by wealth changes as well as compensation effects, which are difficult to control for, making the within-subject design pointless. iii) In a within-subject comparison, it would be favorable that framework conditions were equal, i.e., the two outcomes of the die-rolling tasks matched, but this contradicts the premise of a random draw.

At the beginning of the experiment, participants received written instructions (see appendix C.1) and were requested to raise their hands if questions arose. Questions were asked and answered in private. Afterwards, participants carried out the die-rolling task followed by additional experimental tasks. In addition to the die-rolling task, we measured protected values regarding honesty according to Gibson et al. (2013). This detailed questionnaire is available in appendix C.3. Afterwards, participants answered two short incentivized fairness tasks which are not related to this paper. Subsequently, participants were asked to answer another short questionnaire. Finally, participants received their payoffs from the experimenter privately, which were composed of: i) a fee for showing up and ii) payoffs for the die-rolling task (*Lying* treatment only) and the additional tasks.

4. Results

In order to provide an overview of the participants' decisions, we illustrate the reports and the associated outcomes for both treatments in Figure 1. The outcome is the number which was shown by the dice. The report is the number the participant entered in the computer in the *Lying* treatment. As participants in the *Stealing* treatment did not report directly, we counted the amount of money remaining in the envelope to determine the report.[2] On the one hand, Figure 1 demonstrates that in the *Lying* treatment, a considerable share of participants reported a higher pay-off than their outcomes indicated. Furthermore, most participants who lied reported a roll of '5' instead of their true outcome. On the other hand, there were only a few participants in the *Stealing* treatment who took advantage of the opportunity to withdraw more money from the envelope than their outcome suggested.

[2] For simplicity, we also use the term 'report' for the *Stealing* treatment in the following.

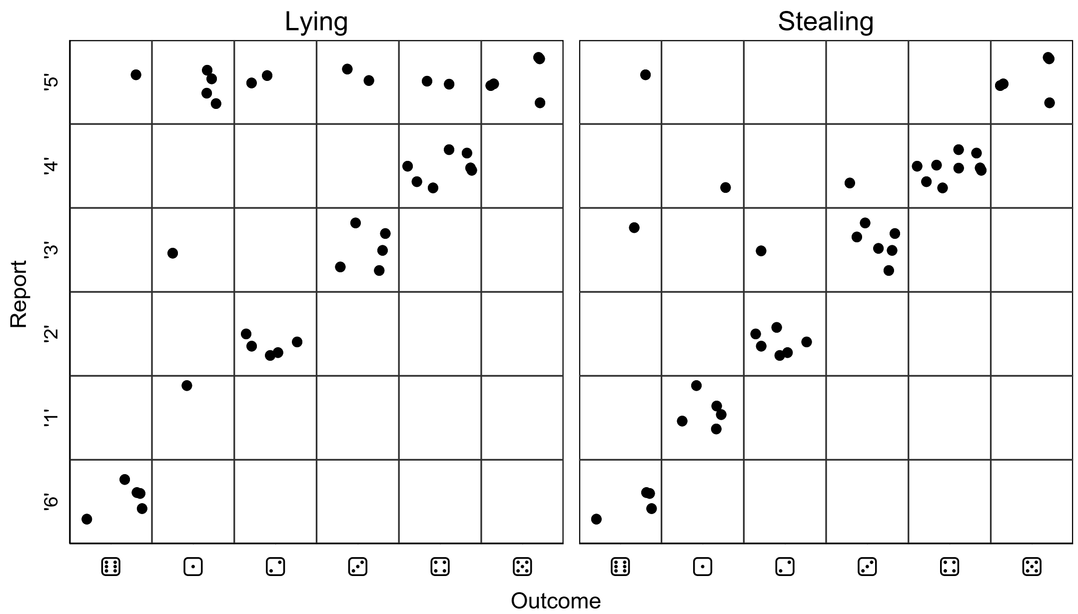

Figure 1: Scatterplot with jitter for outcomes and reports in the two treatments (n per treatment = 40).

Note: Reports in the stealing treatments were recorded based on the money removed from the envelope; honest reports fall along the main diagonal.

In total, 30% of the participants lied while only 12.5% of participants stole. This difference is statistically significant according to a Chi² test[3] ($p = 0.056$). Based on the different relative amounts of lying and stealing, we also calculated a measure for the relative additional payoff.[4] Unsurprisingly, comparing the relative additional payoffs, we also found a statistically significant difference according to a Mann-Whitney test ($p = 0.030$). For *Lying*, the relative additional payoff was 0.288, while for Stealing it was only 0.080.

Result 1: *Stealing is less frequent than lying.*

Subsequent to the investigation of differences in the incidence of lying and stealing, we further analyzed whether this effect was justified by varying intrinsic costs between lying and stealing. To accomplish this, we measured the extent to which honesty is a protected value for participants according to Gibson et al. (2013). The lower the score of this protected value measure, the more easily participants will trade honesty for other goods. Thus, we expected a correlation between the protected value score and lying in the respective treatment. Indeed, the correlation of protected values with the relative additional payoff was

[3] We used the Chi² test if possible and the Fisher's exact test otherwise.

[4] The relative additional payoff is defined as: (report-outcome) / (10 euros-outcome). Thus, it is restricted between 0 (a participant reporting the true outcome) and 1 (a participant with an outcome < 5 reporting a 5).

highly significant for the *Lying* treatment (Spearman rank correlation = -0.462; p = 0.003). Surprisingly, this correlation was not significant for the *Stealing* treatment (Spearman rank correlation = -0.182; p = 0.261). Therefore, the score in the protected value measure can be used to predict lying but not stealing (for illustration, see Figure 2). The reason for the lack of a significant correlation for *Stealing* was revealed by participants with a low protected value measure (median split). We compared the relative additional payoff of these participants between treatments. Indeed, we found a significant decrease in immoral behavior of participants with a low protected values score in the *Stealing* treatment (p = 0.044; Mann-Whitney test). Thus, participants who are willing to trade honesty as a moral value for other goods reveal less immoral behavior as they are faced with higher costs of immorality in the *Stealing* task. In contrast, those who regard honesty as a highly protected value were already acting according to their morale values in the *Lying* treatment, as their intrinsic costs for immoral behavior are generally high.

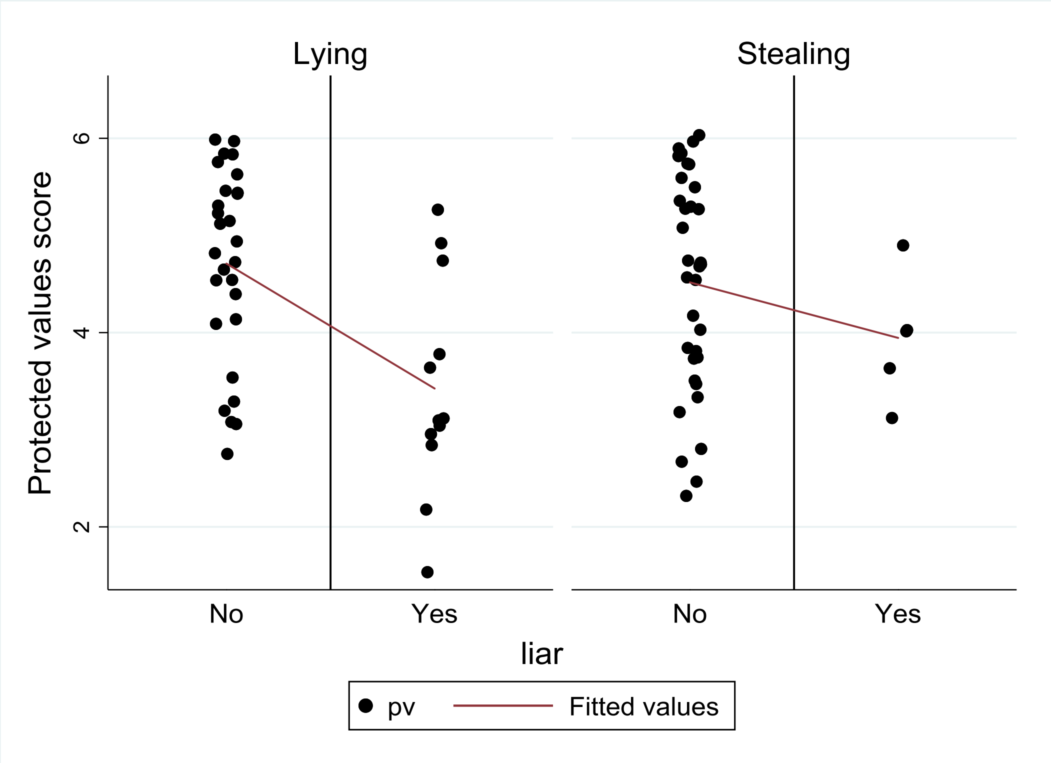

Figure 2: Scatterplot with x-axis jitter for the protected value scores of honest and dishonest participants in the two treatments (n per treatment = 40).
Note: Protected value score for honesty according to Gibson et al. (2013); possible values ranging from 0-6.

Result 2: *Moral values on honesty predict lying but not stealing.*

Result 3: *Stealing is associated with higher intrinsic costs compared to lying.*

Additionally, we investigated individual characteristics which explain the significant difference in immoral behavior we observed. An important characteristic of agents regarding their moral behavior is gender (Gylfason et al., 2013). The proportion of women and men showing immoral behavior in both treatments is shown in Table 4. Surprisingly, we did not find evidence for a gender effect in either treatment, i.e., gender does not predict lying or stealing. In both the *Lying* treatment (p = 1.000; Fisher's exact test) and the *Stealing* treatment (p = 0.345; Fisher's exact test), women showed a rate of moral behavior that was not significantly different from that of men. These results also hold true for the amount of the additional payoff (p = 0.695, p = 0.229; Mann-Whitney tests). However, comparing the treatment effect for genders separately, we found that women's behavior differed significantly (p = 0.090; Fisher's exact test) between the two treatments, but men's behavior did not (p = 0.488; Fisher's exact test). This result is also supported considering the relative additional payoff (women: p = 0.071, men: p = 0.167; Mann-Whitney tests). Thus, our treatment effect was driven by women who lie but avoid stealing.

Table 4: Moral and immoral behavior in both treatments divided by gender

	Lying		*Stealing*	
	Men (n = 22)	Women (n = 18)	Men (n = 21)	Women (n = 19)
Honest	68.2%	72.2%	80.9%	94.7%
Lie/Steal	31.8%	27.8%	19.1%	5.3%

Result 4: *There is no gender effect for the moral costs of lying or stealing.*

Result 5: *Women steal less frequently than they lie*.

5. Conclusion and Discussion

Lying and stealing are immoral actions disturbing social interactions and harming economic agents in various ways. We found a variation in the rate of immoral action between lying and stealing when all incentives and the risk of consequences from immoral actions were kept constant. Results suggest higher intrinsic costs associated with stealing compared to lying. Recent studies focus on self-justification as a main determinant of intrinsic costs of immoral behavior. We conclude, in line with these studies, that the authority receiving the immoral act provides participants with a potential self-justification. If someone is dishonest to someone else, the victim has the opportunity to convict the liar. This (possibly unrealistic) opportunity might be operationalized by liars to justify their immoral behavior. However, removing this authority leaves agents with less self-justification and increases their intrinsic costs of immoral behavior.

Focusing on the extent of lying in our experiment, the results are comparable to other studies using a die-rolling task with observable outcomes. Kocher et al. (2018) reported between 31% and 41% of participants lying under the same experimental conditions. Comparable results were also obtained by Gneezy et al. (2018), who did not use die-rolling, but rather a related task with observable outcomes. They found dishonesty levels between 26% and 33%. In contrast, for a theft task, different levels of stealing were reported. The proportion of thefts in our sample was more comparable to those elicited by Gravert (2013) with a similar design, i.e. randomly determined outcomes. However, the rate of theft in the studies applying the task of Mazar et al. (2008) was higher, ranging between 30% and 60% (Friesen and Gangadharan, 2013; Gino and Pierce, 2009; Shu and Gino, 2012). Gravert (2013) provided evidence for this difference, namely that the effort made in the classical theft task seems to be responsible for the higher levels of theft.

A further comparison of our results can be made regarding the gender difference in lying and stealing. As we did not find evidence for a statistically significant gender difference for the amount of lying, we contradict findings of other researchers (Fosgaard et al., 2013; Grosch and Rau, 2017; Houser et al., 2012; Kajackaite and Gneezy, 2017). Nevertheless, the gender difference in honesty is a matter of discussion, as there are also studies that rejected the hypothesis of gender differences (Childs, 2012; Gylfason et al., 2013; Kajackaite and Gneezy, 2017). Our results support the latter studies; however, the tendency of the gender difference we observed was in line with predictions. In contrast, previous studies on stealing indicate a weak gender difference in stealing (Friesen and Gangadharan, 2013; Gravert, 2013). Indeed, our results tend in the direction that women steal less than men; however, they support the weak correlation found in the literature. Consequently, we contribute to the insights about gender differences regarding intrinsic costs of immoral behavior by finding evidence that women have more difficulty self-justifying stealing compared to lying.

The results obtained in this study could be used as a starting point for future investigations regarding moral costs of lying and stealing. It might be a worthwhile approach to apply our comparison of lying and stealing to the theft task of Mazar et al. (2008). Since Gravert (2013) reported that stealing varies from a die-rolling task to an effort task where stealing is possible, it might be feasible to investigate whether the different moral costs of lying and stealing are maintained in an effort-based framework. Furthermore, framework conditions for decision problems could be stylized to reflect applied decision situations which allow for immoral behavior. For instance, an interesting context could be a tax avoidance framing where participants have to report income and costs (lying) or have to pay the respective tax on their own (stealing). It could also be helpful to investigate the association of effort and intrinsic costs as well as moral compensation for lying and stealing in more detail.

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VII Conclusion

In this section, the main findings and implications of the studies composing this thesis are presented. The structure of the conclusion follows the previous organization by addressing the respective articles consecutively.

The **first part** of the thesis focuses on various aspects of the disposition effect in the boarder and stricter sense. In Chapter II, entitled '**The disposition effect when deciding on behalf of others**', we compared the disposition effect for private investments and investments on behalf of others. The results reveal stronger disposition effects when investors' trading decisions affect the payoff of other subjects rather than their own payoffs. More concretely, inexperienced traders with a prosocial attitude more readily sell capital gains when making decisions for others than for themselves. By contrast, no treatment effect could be found for individualistic subjects and decision-makers with a high degree of trading experience. These results are exciting, as we can conclude that even intrinsic motives are sufficient to cause pronounced disposition effects when deciding for others. It is worth considering that social distance might be reduced if an investor decides on behalf of a person she knows; hence, it is likely that the effects are more pronounced when investors decide for their friends. Our findings have practical implications for everyday trading—i.e., private investors who consider delegating their investment decisions to friends should be cautious, as prosocial investors with a low degree of trading experience exhibit pronounced disposition effects. Furthermore, we found that deciding on behalf of others abolishes the correlation of individual loss aversion and the disposition effect (Andersson et al., 2014). We additionally tested for the motivation of investors under both conditions. However, we found no evidence that inexperienced investors were less motivated when deciding on behalf of others—i.e., no differences in trading volume or between the precision of guess scores regarding the performance of shares were found for the two treatments.

In Chapter III, entitled '**Determinants of financial loss aversion: The influence of prenatal androgen exposure (2D:4D)**', I investigated the influence of prenatal androgen exposure on financial loss aversion as a key driver of the disposition effect. The results provide evidence that increased prenatal testosterone exposure reduces loss aversion. Thus, the results of this study contribute by providing a detailed picture of the connection between biological factors and (behavioral) economics preferences, i.e., loss aversion.

Awareness of this relationship could improve the economic performance of agents with low prenatal testosterone exposure. For instance, they could consider setting ex-ante stop loss rules in order to reduce the occurrence of the disposition effect (Fischbacher et al., 2017) in the case of stock trading. Besides the relationship with the right-hand digit ratio, loss aversion was negatively associated with a greater number of siblings and better math grades. Thus, it could be concluded that the intensity of competition during childhood and mathematical education can reduce the importance of loss aversion in determining economic behavior. The results also revealed that females have greater loss aversion than males. However, prenatal testosterone exposure, siblings and better mathematical skills are possible factors that can counteract the greater loss aversion of females compared to males.

In the last chapter (IV) of the first part, entitled '**The disposition effect in farmers' selling behavior – An experimental investigation**', we analyzed the disposition effect in disinvestment decisions for commodity stocks. We applied an experimental setting to a sample of farmers. Our results show a robust disposition effect in farmers' selling decisions. We measured disposition effects with two different experimental designs. In both of them, participants revealed disposition effect behavior to a comparable extent. In addition, we investigated Prospect Theory parameters as potential determinants of the disposition effect. Surprisingly, the level of risk aversion does not affect the trading bias. However, we found that probability weighting and loss aversion are drivers of the disposition effect. A surprising result is that loss aversion was positively related to the proportion of gains realized, but not the proportion of realized losses. The results have implications for farmers and consultants of farm enterprises. As the losses associated with acting according to the disposition effect are substantial, farmers who are able to reduce this behavior might have an advantage in competition with others. While our results reveal that education is not a factor reducing occurrence of the disposition effect, experience is relevant. Thus, it might be a worthwhile approach for seasoned commodity sellers to share their experiences with inexperienced agents.

In the **second part** of this thesis, the focus of the investigations was immoral behavior. In chapter V, entitled '**Be close to me and I will be honest: How social distance influences honesty**', we studied the consequences of different levels of social distance for honesty. We found reduced lying as social distance decreases. Specifically, participants were less willing to lie at the expense of fellow students than at the expense of the experimenter. We did not find that outcome inequality mediated the effect of social distance on honesty. That said, we found that participants lied more readily to increase their payoff when it was less than half of the available money, and they never lied to benefit others, irrespective of social distance. Hence, we observed what Erat and Gneezy (2012) call selfish ‘black lies’ but not altruistic ‘white lies.’ Beyond these results, additional analyses revealed that social distance did not matter to participants who tended to consider honesty a protected value which they would not trade for other values. Social distance only mattered among those participants who were inclined to trade honesty for other values and who therefore responded to the different situations that arose from our manipulation. Likewise, the effect of social distance depended on participants’ aversion to favorable inequality. Only participants who were sufficiently uneasy with inequality which favored themselves over others changed their behavior according to the circumstances. Our findings have implications for the design of social interactions. In particular, there are many interactions that require honesty to a certain extent but involve social distance. Measures to decrease social distance might be a worthwhile alternative to costly control efforts, which are a common response to dishonesty. Our findings are also relevant in the context of intermediaries, which are often used in social interactions, such as transactions between firms. Although there are compelling reasons to rely on intermediaries, they also increase social distance and create room for dishonesty (Erat, 2013).

In the last chapter (VI) of this thesis, entitled ‘**I might be a liar, but not a thief: An experimental distinction between the moral costs of lying and stealing**’, different moral costs of lying and stealing were analyzed. We found a variation in the rate of immoral action between lying and stealing. Results suggest higher intrinsic costs associated with stealing compared to lying. Recent studies have focused on self-justification as a main determinant of the intrinsic costs of immoral behavior. We conclude, in line with these studies, that the authority receiving the immoral act provides participants with a potential self-justification. If someone is dishonest to someone else, the victim has the opportunity to convict the liar. This opportunity might be operationalized by liars to justify their immoral behavior. However, removing this authority leaves agents with less self-justification and increases their intrinsic costs of immoral behavior. We did not find evidence for a gender difference regarding the amount of lying or stealing. However, we contribute to the insights about gender differences regarding intrinsic costs of immoral behavior by providing evidence that women have more difficulty self-justifying stealing compared to lying. As stealing is associated with higher intrinsic costs for economic agents, it might be possible to reduce economic losses by reframing decisions in various contexts. For instance, the transfer of responsibility to agents under conditions of asymmetric information and consequently the changed perception of the immoral action might increase behavior which is in accordance with moral convictions.

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Appendices

Appendix A

A.1 Eckel and Grossman task

In this part you have to choose one of 9 lotteries which you prefer most. After your decision the computer will perform your selected lottery. Both payoffs arise with a probability of 50%. At the end of the experiment you will be informed of the outcome of the draw.

Table A.1: Conducted EG-task

Lottery	**Expected value of the lottery**[a)]	**Payoff A probability 50%**	**Payoff B probability 50%**	**Please choose your preferred lottery**	**Range of constant relative risk aversion if choosing this lottery**[b)]
1	€2.00	€2.00	€2.00	○	$1.37 \leq r \leq \infty$
2	€2.08	€2.56	€1.59	○	$0.97 < r \leq 1.37$
3	€2.26	€3.28	€1.24	○	$0.68 < r \leq 0.97$
4	€2.46	€4.00	€0.92	○	$0.41 < r \leq 0.68$
5	€2.55	€4.35	€0.74	○	$0.15 < r \leq 0.41$
6	€2.58	€4.59	€0.57	○	$-0.15 < r \leq 0.15$
7	€2.57	€4.65	€0.48	○	$-0.49 < r \leq -0.15$
8	€2.55	€4.67	€0.42	○	$-0.95 < r \leq -0.49$
9	€2.45	€4.68	€0.22	○	$-\infty \leq r \leq -0.95$

a) Column was shown in the modified version.

b) Column was not shown. A power utility function of the form $U(x) = \frac{x^{(1-r)}}{1-r}$ is assumed (Eckel and Grossman, 2008).

A.2 Gächter et al. task

You receive an endowment of 70 cents for this part. In the following you are faced with 10 lotteries. Assume that for each of the 10 questions a coin is thrown. The coin can either land at ‘heads’ or ‘tail.’ To answer each of the 10 questions you will either have to choose ‘accept’ or ‘reject’ taking part in the respective lottery. After you submit your decisions the computer will randomly draw one of the lotteries. If you reject this specific lottery you will receive the endowment after the experiment. If you accept the randomly chosen lottery the computer will flip a coin and the outcome of this coin toss will be added to your endowment. At the end of the experiment you will be informed of the randomly selected lottery and the outcome of the draw.

Table A.2: Conducted GJH-task

Lottery		Accept	Reject	Range of loss aversion coefficient (λ) if switching to reject in this row[a)]
1	If the coin turns up heads, then you lose 12 cents; if the coin turns up tails, you win 60 cents.	( )	( )	$5.00 \leq \lambda \leq \infty$
2	If the coin turns up heads, then you lose 15 cents; if the coin turns up tails, you win 60 cents	( )	( )	$4.00 \leq \lambda \leq 5.00$
3	If the coin turns up heads, then you lose 20 cent; if the coin turns up tails, you win 60 cents	( )	( )	$3.00 \leq \lambda \leq 4.00$
4	If the coin turns up heads, then you lose 25 cents; if the coin turns up tails, you win 60 cents.	( )	( )	$2.40 \leq \lambda \leq 3.00$
5	If the coin turns up heads, then you lose 30 cents; if the coin turns up tails, you win 60 cents.	( )	( )	$2.00 \leq \lambda \leq 2.40$
6	If the coin turns up heads, then you lose 35 cents; if the coin turns up tails, you win 60 cents.	( )	( )	$1.71 \leq \lambda \leq 2.00$
7	If the coin turns up heads, then you lose 40 cents; if the coin turns up tails, you win 60 cents.	( )	( )	$1.50 \leq \lambda \leq 1.71$
8	If the coin turns up heads, then you lose 50 cents; if the coin turns up tails, you win 60 cents.	( )	( )	$1.20 \leq \lambda \leq 1.50$
9	If the coin turns up heads, then you lose 60 cents; if the coin turns up tails, you win 60 cents.	( )	( )	$1.00 \leq \lambda \leq 1.20$
10	If the coin turns up heads, then you lose 70 cents; if the coin turns up tails, you win 60 cents.	( )	( )	$0.86 \leq \lambda \leq 1.00$

a) Column was not shown. As in Gächter et al. (2007), equal curvature parameters in the gain and the loss domain are assumed for deriving λ.

A.3 SVO slider measure

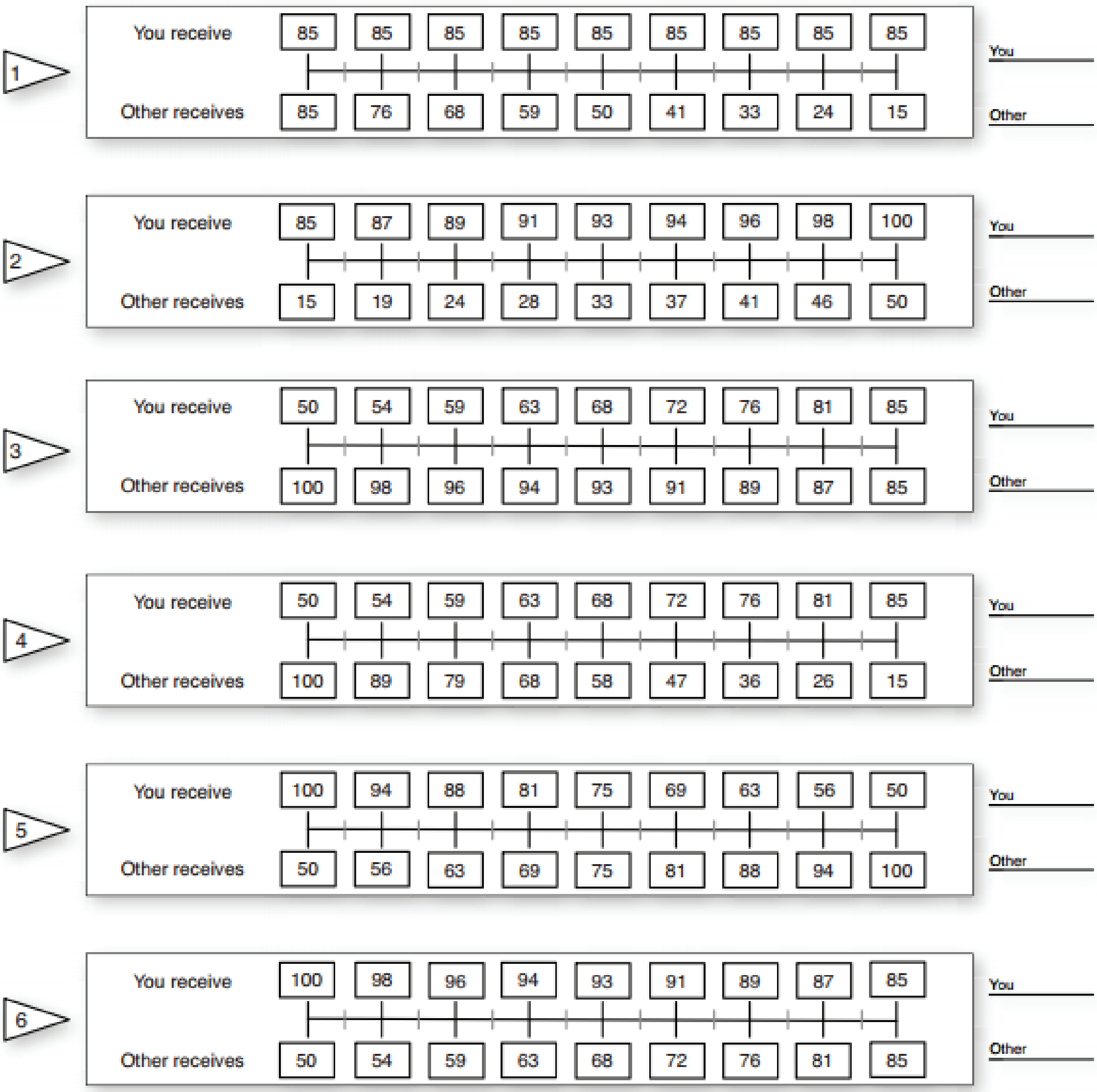


Figure A.3: SVO Sliders that were presented gradually to the participants. Values are by a conversion factor of 100 token = 2 €.

A.4 Questionnaire for measuring empathy

You will now read several statements which describe specific (generalized) human attitudes or reactions. All of them are associated with emotions. Please mark on the 5-point-scale to what extent they apply to you. Higher numbers indicate greater agreement. Perhaps you remember a specific incident for the specific statements. **There is no right or wrong.**

Answer scale:

	Never	Rarely	Sometimes	Frequently	Always
Statement # 1-16	○	○	○	○	○

Statements:

1. I often have tender, concerned feelings for people less fortunate than me. (EC)
2. I really get involved with the feelings of the characters in a novel. (FS)
3. In emergency situations, I feel apprehensive and ill-at-ease. (PD)
4. I try to look at everybody’s side of a disagreement before I make a decision. (PT)
5. When I see someone being taken advantage of, I feel kind of protective toward them. (EC)
6. I sometimes feel helpless when I am in the middle of a very emotional situation. (PD)
7. After seeing a play or movie, I sometimes feel as though I were one of the characters. (FS)
8. Being in a tense emotional situation scares me. (PD)
9. I am often quite touched by things that I see happen. (EC)
10. I believe that there are two sides to every question and I try to look at them both. (PT)
11. I would describe myself as a pretty soft-hearted person. (EC)
12. When I watch a good movie, I can very easily put myself in the place of a leading character. (FS)
13. I tend to lose control during emergencies. (PD)
14. When I’m upset at someone, I usually try to ‘put myself in his shoes’ for a while. (PT)
15. When I am reading an interesting story or novel, I imagine how I would feel if the events in the story were happening to me. (FS)
16. Before criticizing somebody, I try to imagine how I would feel if I were in their place. (PT)

A.5 Questionnaire for measuring socio demographics and trade experience

Please answer the following questions. Afterwards we will give out the payments.
[…]

How do you assess your personal experience in the trading of stocks? Please answer on the following scale from 1–10 (where 1 = no experience in trading and 10 = very extensive experience in stock trading)

( ) 1 …. ( ) 10

[…]

Table A.6: Tobit regressions with robust standard errors on DE, PGR, PLR and Alpha for subsample of experienced traders[a)]

	DE		PGR		PLR		Alpha	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	-0.204**	0.286	0.099	0.005	0.3030***	-0.274	0.025	0.083
Responsibility (1=yes)	0.020	-	0.024	-	-0.001	-	0.000	-
Prosocial[b)](1=yes)	-0.043	-	-0.053	-	-0.001	-	-0.237	-
Individual x Prosocial	-	baseline	-	baseline	-	baseline	-	baseline
Individual x Individualistic	-	-0.011	-	-0.012	-	-0.013	-	0.325
Responsibility x Prosocial	-	-0.047	-	-0.004	-	0.032	-	0.101
Responsibility x Individualistic	-	-0.006	-	0.075	-	0.076	-	0.151
Loss aversion (lambda)[c)]	0.130***	0.102***	0.031	0.026	-0.104***	-0.083**	0.124	0.045
Risk aversion (CRRA)[d)]	-0.048	-0.080**	-0.015	-0.034	0.035*	0.050**	-0.069	-0.103
Female (1=yes)	-	0.195***	-	0.030	-	-0.171***	-	0.277
Pride[e)]	-	0.021	-	0.023**	-	0.002	-	0.025
Regret[f)]	-	-0.006	-	0.003	-	0.007	-	0.020
Empathy[g)]	-	-0.014	-	-0.003	-	0.012*	-	-0.017
Math grade (1–15)[h)]	-	-0.011	-	-0.006	-	0.003	-	0.002
Study economics(1=yes)	-	0.119*	-	0.122**	-	-0.002	-	0.282
σ[i)]	0.231	0.206	0.158	0.146	0.174	0.154	0.659	0.619
Observations	69	69	69	69	69	69	69	69

Note: Nine participants show inconsistent choices (switching back and forth between accepting and rejecting lotteries) in the GJH task and are therefore omitted for the regression estimates. However, most results are qualitatively identical if we estimate the models without lambda (*Study economics* is not significant in model (2) if lambda is not taken into account and therefore n = 78). Level of significance: *** p <0.01, ** p <0.05, * p <0.10

a) According to a specific question with possible values ranging from 0 (no experience) and 10 (highly experienced). b) According to an incentivized Murphy task, possible values ranging from -16.26° to 61.39°. c) According to an incentivized GJH task, possible values ranging from 0.68 to 5.50. d) According to a modified and incentivized EG task, possible values ranging from -1.60 to 1.81. e) According to a specific question with possible values ranging from 0 (not proud at all) and 10 (very proud). f) According to a specific question with possible values ranging from 0 (no regret) and 10 (very much regret). g) According to the IRI based Saarbrücker personality questionnaire, possible values ranging from 12 to 60. h) Possible values ranging from 0 to 15; 15 is the best grade. i) The sigma (σ) value represents the estimated standard error of the interval regressions.

A.7 Experimental Instructions: Responsibility treatment

[Translation from German]

Welcome to this experiment about decision theory and thank you very much for you participation. Please read these instructions to the experiment carefully.

At the beginning of the experiment, every participant will receive a note containing a neutral letter (e.g. ‘B’ or ‘U). These letters are assigned randomly and the alphabetic order is not linked to the numbers of the cabins.

Due to the following part of the experiment you will decide for someone else. The other participant is randomly assigned to you by the computer. This other participant will remain anonymous during and after the experiment. The only information available to you is the randomly assigned letter. Furthermore, you are assigned to another participant who decides for you.

Note: The participant who decides for you is not the same as the participant you are deciding for.

For the main experiment:

- Your decisions do not have any consequences for your payoff. However, your decisions have direct consequences for the payoff of the randomly assigned participants.
- Your payoff depends on the decisions of the randomly assigned participant. This participant decides for you and is not the participant you are deciding for.

Before the experiment starts you are informed about the anonymous name (letter) of the participant you are deciding for. Moreover, you are informed about the name (letter) of the participant who decides for you.

During the experiment you will earn Talers which are converted to € by the following exchange-rate

1,000 Talers = €1

at the end of the experiment. The assigned participant whose payoff depend on your decisions will receive your Taler earnings converted in € and you receive the € amount the other participant earned for you. You will be informed about the earnings of the assigned participant at the end of the experiment before you are paid.

After the experiment, please wait at your desk until we will ask you to come to get your payoff. The payment is anonymous. Thus you will not get information about payments of other participants or their assigned letter. Please notice that it is not allowed to talk. If you will talk to other persons the experiment will be finished immediately. If you have a question, please raise your hand. We will come to your desk to answer it individually.

Description of the experiment

The experiment consists of 14 periods. In every period you have the possibility to buy shares of the firms A, B, C, D, E, and F. Every share has a certain value in Talers in every period.

You start the experiment with an endowment of **10,000 Talers**.

Performance of shares

The shares A-F will change in prices at the beginning of each of the 14 periods, i.e., in the subsequent period there will be no share which will have the same price as in the previous period. The share price changes have been predetermined before the experiment started. That is, all price changes of all shares are completely independent of all your buying and selling decisions. The same is true for all buying and selling decisions of the other participants of the experiment. Each of the shares A-F is of a certain type. The share types differ regarding their probability of increasing (decreasing) in value at the beginning of the period. The distributions of the types are given in the table below. In the experiment there will be exactly one share (of the shares A-F) which follows type '++' and the same is true for one share of type '+', '–', and '– –'. There will be two types (of the shares A-F) which follow type '0'. All types are displayed at the below table.

Shares in the market	Type	Probability of price increase	Probability of price decrease
1	++	65%	35%
1	+	65%	45%
2	0	50%	50%
1	–	45%	55%
1	– –	35%	65%

Example:

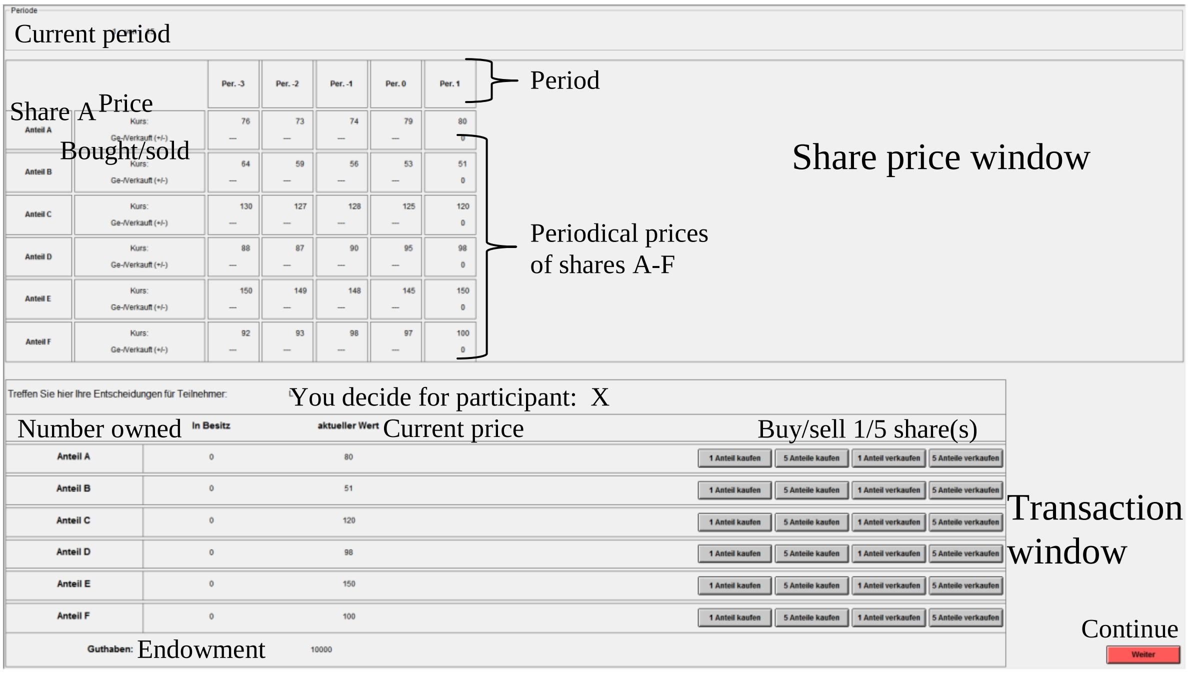
- assume that share X is of type: '++'
- at the beginning of each period the probability of a price increase of X is: 65%
- at the beginning of each period the probability of a price decrease of X is: 35%

The share price is determined as follows:

1. At the beginning of each period a share either increases (decreases). The probability depends on the share's type (see table).
2. Afterwards the magnitude of the price change (increase/ decrease) will be determined. The magnitude of the price change can either be of 1, 3 or 5 Talers. Every magnitude (1, 3 or 5 Talers) can happen with the same probability. That is, every magnitude (1, 3 or 5 Talers) can happen with a probability of one-third. This is the same for every share, independent of its type.

Buying and selling actions of shares

In each of the 14 periods you have the possibility to buy and sell shares for the portfolio of the other participant. You will find a screen shot at the next page which depicts all of your decision possibilities in the course of the experiment. In the upper part you will find the share price window, displaying shares A-F. The price changes of shares A-F in periods 1-14 will be displayed here. To give you an idea of shares' past price changes, you will also find the prices of periods -3, -2, -1 and 0. In the following you are given an overview of the price changes of the shares A-F in the periods -3, -2, -1 and 0.



Possibilities of decisions in the experiment

The upper part of the window is the share price window:

- The array labeled ‘price’ displays the exact price of a share in the current period. For instance, in the screen shot share A had a price of 76 Talers in period -3.
- Furthermore, the array ‘Bought/sold’ displays the number of bought/sold shares in the current period. The screen uses the following symbols: ‘– –‘ which means that there was no transaction. ‘1’ which means that one share was bought. ‘–1’ which means that one share was sold.

The window at the bottom is the transaction window. Here, you can decide in each period whether you would like to buy/sell one or more shares of shares A-F.

- The array ‘number owned’ displays the current number of shares owned
- The array ‘current price’ depicts the price which has to be paid in order to buy new shares. At the same time you would receive this price for each share sold.
- The array ‘endowment’ displays your endowment.

If you decide to buy shares of a firm then you have to pay for each share its current price. The sum of your expenditures cannot exceed your actual endowment.

Example:

- Share A's current price in period 1 is 110 Talers. You decide to buy five shares of A.

- The expenditures for this transaction are given by: 5 * 110 Talers = 550 Talers and are immediately subtracted of your endowment

To buy one unit of a share you can use the button ‘Buy 1 unit’. If you intend to buy 3 units you have to push the button ‘Buy 1 unit’ three times, etc. If you want to buy five units you can push the button ‘Buy 5 units’ instead of push the button ‘Buy 1 unit’ five times.

If you already own some shares at the beginning of a period, then you have the possibility to sell these shares. You will receive the current price of each share which is sold added to you endowment. Selling shares follow the same principles as buying shares. However, the numbers of sold shares cannot exceed the total number of shares owned.

Example:

- Share C's current price in period 5 is 90 Talers. Assume, you own a total of four shares C and decide to sell 3 shares C.
- This will lead to a payoff of: 3 * 90 Talers = 270 Talers. This amount will be directly credited to your endowment. Afterwards you will still own one share of C.

To sell one unit of a share you can use the button ‘Sell 1 unit’. If you intend to sell 3 units you have to push the button ‘Sell 1 unit’ three times, etc. If you want to sell five units you can push the button ‘Sell 5 units’ instead of push the button ‘sell 1 unit’ five times.

The experiment ends after 14 periods. Then you and your matched participant do not have the possibility to buy or sell shares. Afterwards, all shares that you own at this point in time are automatically liquidated. The resulting money amount will automatically credited to your endowment.

Additional earnings and payoff

Additional earning

Before the main experiment begins, you have the possibility to earn additional money by carrying out three tasks. The instructions for the tasks are displayed on the computer screen respectively.

After the three tasks are carried out the main experiment starts (instructions in written form).

Due to the main experiment, after the end of period 7 and 14, you have to guess which stock A-F followed the types:'++', '+', '0', '– –'and '–'. You will be credited 200 Talers to your endowment for every correct guess.

Payoff

The total payoff you will earn in the experiment is calculated as follows:

Total payoff = endowment which was not invested by the participant who decides for you + value of the shares in the portfolio of this participant + earnings of your guesses + your earnings from the three additional tasks.

The total payoff of the participant on behalf of which you are deciding will earn in the experiment:

Total payoff = your endowment which was not invested + value of the shares in your portfolio + earnings of this participants guesses + earnings of this participant from the three additional tasks.

Appendix B

B.1 Instructions for the experiment (Low Degree of Social Distance condition)

The following instructions were distributed in print. The task was completed on the computer.

General instructions

Welcome to today's experiment.

Please keep quiet throughout the experiment and follow the experimenter's instructions. Please don't talk unless asked to talk.

If you have any questions, raise your hand. The experimenter will come to you and answer your questions confidentially. Please turn off your mobile devices and stow them.

Participants who fail to comply with these instructions will have to leave the room and will only be paid €2.

Task

While entering the lab, all participants were randomly assigned to one of two rooms. There is the same number of participants in each room.

Every participant in this room will see a video which shows a six-sided die being rolled. Each outcome (⚀, ⚁, ⚂, ⚃, ⚄, and ⚅) is equally likely to occur.

The die is rolled for each participant independently of the others. That is, the die is *not* rolled once for all participants, but for each participant individually.

You must remember the outcome of your throw and enter it subsequently into a field on your computer screen.

The number that you enter determines your share of the €10. This share is your compensation for this experiment.

Number entered	Your share	Remainder
'1'	2 €	8 €
'2'	4 €	6 €
'3'	6 €	4 €
'4'	8 €	2 €
'5'	10 €	0 €
'6'	0 €	10 €

A participant in the other room will be randomly assigned to you. This participant receives the remainder of the €10 as compensation.

You won't meet the other participant. You won't learn this person's identity and the other participant won't learn your identity.

Payment

After the experiment, you will answer a number of questions. Your answers can earn you additional money.

You will receive your compensation at the end of the experiment in return for your numbered badge. In addition to your compensation from the task, you'll receive a fixed €4 payment.

You will be paid confidentially. Participants will be called out individually to go into the reception room.

B.2 Instructions for the experiment (High Degree of Social Distance condition)

The instructions for the High Degree of Social Distance condition were identical to those for the Low Degree of Social Distance condition except for the section ‘Task,’ which read as follows.

Task

Every participant in this room will see a video which shows a six-sided die being rolled. Each outcome (⚀, ⚁, ⚂, ⚃, ⚄, and ⚅) is equally likely to occur.

The die is rolled for each participant independently of the others. That is, the die is *not* rolled once for all participants, but for each participant individually.

You must remember the outcome of your throw and enter it subsequently into a field on your computer screen.

The number that you enter determines your share of €10. This share is your compensation from this experiment.

Number entered	Your share	Remainder
‘1’	2 €	8 €
‘2’	4 €	6 €
‘3’	6 €	4 €
‘4’	8 €	2 €
‘5’	10 €	0 €
‘6’	0 €	10 €

B.3 Comprehension questions

Participants cannot proceed until they have answered 1 – a, 2 – 6, 3 – 4, and 4 – 8.

1. What's your task?

a. To enter the displayed number that you have memorized
b. To enter a different number than the displayed number that you have memorized
c. To enter an arbitrary number

2. Suppose you see a ⚂ and you enter a '3.' How many euros do you earn?

___

3. Suppose you see a ⚂ and you enter a '2.' How many euros do you earn?

___

4. Suppose you see a ⚂ and you enter a '4.' How many euros do you earn?

___

B.4 Measurement of the compassion parameter

The multiple price list task to measure the compassion parameter β was adopted from Blanco et al. (2011). Participants were provided with a list of 22 choices, each between two allocations of payoffs between themselves and another participant (‘left’ and ‘right’).

First, all participants made 22 decisions in the role of Person A. Half of the participants were then randomly assigned to be Person B and type-A and type-B participants were matched. Finally, one of the 22 choices of the type-A participant was randomly selected and implemented.

Table E.4: Beta elicitation task

Person A’s Payoff	Person B’s Payoff	Decision	rson A’s Payoff	rson B’s Payoff
20	0	Left—Right	0	0
20	0	Left—Right	1	1
20	0	Left—Right	2	2
20	0	Left—Right	3	3
20	0	Left—Right	4	4
20	0	Left—Right	5	5
20	0	Left—Right	6	6
20	0	Left—Right	7	7
20	0	Left—Right	8	8
20	0	Left—Right	9	9
20	0	Left—Right	10	10
20	0	Left—Right	11	11
20	0	Left—Right	12	12
20	0	Left—Right	13	13
20	0	Left—Right	14	14
20	0	Left—Right	15	15
20	0	Left—Right	16	16
20	0	Left—Right	17	17
20	0	Left—Right	18	18
20	0	Left—Right	19	19
20	0	Left—Right	20	20
20	0	Left—Right	21	21

Note. The payoff is illustrated in units of ‘thaler.’ Each ‘thaler’ in the experimental task was remunerated with €0.15 at the end of the experiment.

B.5 Protected Values question

What is your opinion on lying for one's own benefit?

I find this …

Not at all praiseworthy	1–2–3–4–5–6–7	very praiseworthy
Not at all shameful	1–2–3–4–5–6–7	very shameful
Not at all acceptable	1–2–3–4–5–6–7	very acceptable
Not at all outrageous	1–2–3–4–5–6–7	very outrageous
Not at all blameworthy	1–2–3–4–5–6–7	very blameworthy
Very immoral	1–2–3–4–5–6–7	very moral

Honesty is something …

… that one should not sacrifice, no matter what the (material or other) benefits.

Strongly disagree 1–2–3–4–5–6–7 strongly agree

… that cannot be measured in monetary terms.

Strongly disagree 1–2–3–4–5–6–7 strongly agree

… for which I think it is right to make a cost–benefit analysis.

Strongly disagree 1–2–3–4–5–6–7 strongly agree

… about which I can be flexible if the situation demands it.

Strongly disagree 1–2–3–4–5–6–7 strongly agree

… which is about things or values that are sacrosanct.

Strongly disagree 1–2–3–4–5–6–7 strongly agree

B.6 Regressions results

Table B.6.1: Effect of social distance on honesty

	Relative additional payoff[a)]		**Liar**[b)]	
	(1)	(2)	(3)	(4)
High Degree of Social Distance	1.029***	1.462**	1.013**	1.502**
Outcome[c)]		−.422**		−.460**
Protected value score[d)]		−.773***		−.860***
Compassion[e)]		−2.535**		−2.536**
Age		.003		−.010
Female		−.545		−.484
Income		.000		.000
Number of siblings		.334		.331
Math grade[f)]		.024		.013
Constant	−2.142**	1.884	−2.024***	2.825
Observations	120		120	

Note: The table reports coefficients of GLM and logit regressions of relative additional payoff and liar on the degree of social distance; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

a) (Report − Outcome) ÷ (5 – Outcome) if Outcome ≠ 5; 0 otherwise.

b) 1 if Report ≠ Outcome; 0 otherwise.

c) Outcome of the die roll; possible values ranging from 0 to 6 (where ⚅ corresponds to ‘0’; ⚀, to ‘1’; …; ⚄, to ‘5’ under truthful reporting).

d) Protected value score for honesty according to Gibson et al. (2013); possible values ranging from 0 to 6.

e) Compassion β according to Blanco et al. (2011); possible values ranging from −.075 to 1.

f) Possible values ranging from 0 to 15, where 15 is best.

Table B.6.2: Effect of social distance and social preferences on honesty

Panel A: Effects of social distance, compassion, and protected values score on honesty

	Relative additional payoff[a)]		**Liar**[b)]	
	(1)	(2)	(3)	(4)
HD[c)]–HP[d)]–HC[e)]	−.090	−.133	−.811	−1.716
HD[c)]–LP[d)]–HC[e)]	.224	.188	1.030	.927
HD[c)]–HP[d)]–LC[e)]	−.143	−.214	−1.368	−2.220*
HD[c)]–LP[d)]–LC[e)]	.170	.174	.778	.994
LD[c)]–HP[d)]–HC[e)]	−.155	−.186	−1.535	−2.191*
LD[c)]–LP[d)]–HC[e)]	−.226*	−.218*	—[f)]	—[f)]
LD[c)]–HP[d)]–LC[e)]	−.143	−.232*	−1.368	−2.313
LD[c)]–LP[d)]–LC[e)]	—[g)]	—[g)]	—[g)]	—[g)]
Outcome[h)]		−.052**		−.548**
Age		.003		.023
Female		−.072		−.548
Income		.000		.000
Number siblings		.037		.339
Math grade[i)]		.006		.069
Constant	.226	.232	−1.030	−1.169
Observations	120		120	

Panel B: Wald tests for differences (*F*-statistics in parentheses)

	HD[c)]–HP[d)]–HC[e)]	**HD[c)]–LP[d)]–HC[e)]**	**HD[c)]–HP[d)]–LC[e)]**	**LD[c)]–LP[d)]–LC[e)]**
LD[c)]–HP[d)]–HC[e)]	.659 (0.20)			
LD[c)]–LP[d)]–HC[e)]		**.005** (8.16)		
LD[c)]–HP[d)]–LC[e)]			.900 (0.02)	
LD[c)]–LP[d)]–LC[e)]				.152 (2.08)

Note: Panel A table reports coefficients of GLM and logit regressions of relative additional payoff and liar on the degree of social distance. Panel B reports the *p*-values and, in parenthesis, *F*-statistics, of Wald tests for differences between subgroups under a high and low degree of social distance for Model 2 in Panel A; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

a) (Report − Outcome) ÷ (5 – Outcome) if Outcome ≠ 5; 0 otherwise.
b) 1 if Report ≠ Outcome; 0 otherwise.
c) HD versus LD: High versus Low Degree of Social Distance.
d) HP versus LP: High versus Low Protected Values Score for Honesty.
e) HC versus LC: High versus Low Compassion β.
f) There are no liars in this cell. Coefficients and standard errors therefore cannot be calculated.
g) Baseline.
h) Outcome of the die roll; possible values ranging from 0 to 6 (where ⚅ corresponds to ‘0’; ⚀, to ‘1’; …; ⚄, to ‘5’ under truthful reporting).
i) Possible values ranging from 0 to 15, where 15 is best.

Appendix C

C.1 Instructions and comprehension questions

Instructions for the experiment (*Lying* condition)

[The instructions were distributed in print. The task was completed on the computer.]

General instructions

Welcome to today’s experiment.

Please keep quiet throughout the experiment and follow the experimenter’s instructions. Please don’t talk unless asked to talk.

If you have any questions, raise your hand. The experimenter will come to you and answer your questions confidentially. Please turn off your mobile devices and stow them.

Participants who fail to comply with these instructions will have to leave the room and will only be paid €2.

Task

Every participant in this room will see a video which shows a six-sided die being rolled. Each outcome (⚀, ⚁, ⚂, ⚃, ⚄, and ⚅) is equally likely to occur.

The die is rolled for each participant independently of the others. That is, the die is <u>not</u> rolled once for all participants, but for each participant individually.

You must remember the outcome of your throw and enter it subsequently into a field on your computer screen.

The number that you enter determines your share of €10. This share is your compensation from this experiment.

Number entered	Your share	Remainder
‘1’	2 €	8 €
‘2’	4 €	6 €
‘3’	6 €	4 €
‘4’	8 €	2 €
‘5’	10 €	0 €
‘6’	0 €	10 €

Payment

After the experiment, you will answer a number of questions. Your answers can earn you additional money.

You will receive your compensation at the end of the experiment in return for your numbered badge. In addition to your compensation from the task, you’ll receive a fixed €4 payment.

You will be paid confidentially. Participants will be called out individually to go into the reception room.

Instructions for the experiment (*Stealing* condition)

[The instructions for the *Stealing* condition are identical to those for the *Lying* condition except for the section 'Task' and 'Payment' which reads as follows:]

Task

Every participant in this room will see a video which shows a six-sided die being rolled. Each outcome (⚀, ⚁, ⚂, ⚃, ⚄, and ⚅) is equally likely to occur.

The die is rolled for each participant independently of the others. That is, the die is not rolled once for all participants, but for each participant individually.

You must remember the outcome of your throw and open the envelope in your cabin afterwards.

The number that you remember determines your share of €10. This share is your compensation from this experiment. You will find €10 in €2 coins in the envelope.

Number entered	Your share	Remainder
'1'	2 €	8 €
'2'	4 €	6 €
'3'	6 €	4 €
'4'	8 €	2 €
'5'	10 €	0 €
'6'	0 €	10 €

Please remove your determined share from the envelope and subsequently close the envelope. This part of the experiment is then finished.

The envelope will not be collected immediately and you do not have to submit it at the end of the experiment. Please leave the envelope in your cabin. We will collect the envelopes once all participants have left the laboratory.

Payment

After the experiment, you will answer a number of questions. Your answers can earn you additional money.

You will receive your compensation for the additional tasks at the end of the experiment in return for your numbered badge. In addition to your compensation from the tasks, you'll receive a fixed €4 payment. This payment is independent from the already completed payment from the die-rolling task.

You will be paid confidentially. Participants will be called out individually to go into the reception room.

Comprehension questions

Participants cannot proceed until they have answered 1 – a, 2 – 6, 3 – 4, and 4 – 8.

1. What is your task?

a. To enter the displayed number that you have memorized
b. To enter a different number than the displayed number that you have memorized
c. To enter an arbitrary number

2. Suppose you see a ⚂ and you enter a ‘3.’ How many euros do you earn?

___

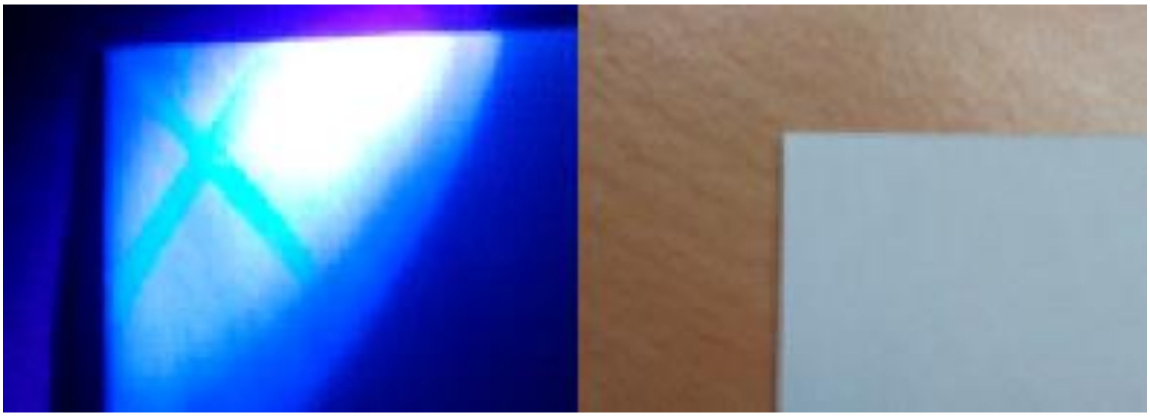
3. Suppose you see a ⚂ and you enter a ‘2.’ How many euros do you earn?

___

4. Suppose you see a ⚂ and you enter a ‘4.’ How many euros do you earn?

___

C.2 Picture of a marked envelope with and without UV light



C.3 Protected Value questions

What is your opinion on lying for one's own benefit?

I find this …

Not at all praiseworthy	1–2–3–4–5–6–7	very praiseworthy
Not at all shameful	1–2–3–4–5–6–7	very shameful
Not at all acceptable	1–2–3–4–5–6–7	very acceptable
Not at all outrageous	1–2–3–4–5–6–7	very outrageous
Not at all blameworthy	1–2–3–4–5–6–7	very blameworthy
Very immoral	1–2–3–4–5–6–7	very moral

Honesty is something …

… that one should not sacrifice, no matter what the (material or other) benefits.

Strongly disagree 1–2–3–4–5–6–7 strongly agree

… that cannot be measured in monetary terms.

Strongly disagree 1–2–3–4–5–6–7 strongly agree

… for which I think it is right to make a cost–benefit analysis.

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… which is about things or values that are sacrosanct.

Strongly disagree 1–2–3–4–5–6–7 strongly agree