The Impact of Self-Control on Investment Decisions[*]

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Abstract

This paper explores how reduced self-control affects individual investment behavior in two laboratory tasks. For this purpose, I exogenously reduce subjects' self-control using a well-established psychological treatment. In each task, I find no significant main treatment effect, but secondary effects consistent with findings on self-control from other studies and self-control's potential relevance in financial markets. In experiment 1, I find no significant change in the disposition effect following the manipulation. However, treated participants trade fewer different shares per round. In experiment 2, I look at the effect of self-control on myopic loss aversion by implementing a 2×2 design by varying investment horizon and self-control in a repeated lottery environment. Average behavior suggests that reduced self-control increases framing effects, but I cannot reject the null hypothesis of equal investment levels between the self-control treatments within each investment frame. Analyzing the dynamics of decision making in more detail, self-control depleted participants in the narrow frame reduce their investment levels on average over time which seems to be driven by more intense reactions to investment experiences.

JEL codes: G02, G11, D53, D81

Keywords: Self-control, loss aversion, disposition effect, trade clustering, myopic loss aversion

[*]Special thanks go to Klaus Hildebrand who provided valuable assistance in designing and conducting experiment 1 as part of his Master's thesis. His analysis of the dataset from experiment 1 can be found in his Master's thesis (Hildebrand, 2015). The analysis of this dataset in the current paper adds to his results by looking at heterogeneity in the effects and by looking at trade clustering. Daniel Kessler's patient responses and help in programming the letter-e-task are greatly appreciated. Special thanks also go to Gerlinde Fellner and Matthias Sutter for providing me with their z-Tree code and instructions for the MLA task. Furthermore, this paper benefited greatly from many helpful comments from participants at the ESA 2015 North America meeting and the Experimental Finance 2016 conference and at Brown Bag Workshops at Chapman University and University of Munich and in particular from comments by Brice Corgnet, Uri Gneezy, Austin Henderson, Lena Jaroszek, Martin Kocher, Simeon Schudy, Rudi Stracke and Joachim Winter. I acknowledge funding through the International Doctoral Program "Evidence-Based Economics" of the Elite Network of Bavaria and through the Center for Advanced Studies, LMU Munich.

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1 Introduction

> "Success in investing doesn't correlate with I.Q. once you're above the level of 25. Once you have ordinary intelligence, what you need is the temperament to control the urges that get other people into trouble in investing."[1]
>
> *Warren Buffett*

Investment guru Warren Buffett regards the 'temperament to control (...) urges' as necessary for investing sucessfully. Warren Buffett's statement matches psychologists' definition of self-control: the ability to override or inhibit undesired behavioral tendencies, such as impulses (Tangney et al., 2004). Interpreted in this way, Warren Buffett seems to suggest a relationship between self-control abilities and investment behavior.

This paper looks at the causal relationship between state self-control, i.e. temporary changes in self-control, and two investment biases on the individual level. I find no significant main effect of exogenously reduced self-control on neither the disposition effect (DE, cf. Shefrin and Statman, 1985) nor myopic loss aversion (MLA, cf. Benartzi and Thaler, 1995). However, reduced self-control increases traders' focus on trading fewer different stocks in the DE task and amplifies framing effects due to MLA. Looking at the dynamics of investment in the MLA task, behavior under reduced self-control becomes significantly more dependant on previous outcomes but only if subjects invest and receive feedback more frequently. Consequently, frequently investing subjects whose self-control has been depleted become more cautious over time. This finding suggests that a broad investment frame, i.e. investing infrequently, can serve as a shield against the influence of short lived emotions. The evidence in this paper contributes to the growing literature investigating determinants of heterogeneity in investment biases and indicates that fluctuations in state self-control have an effect on investment behavior.

The findings of Roy Baumeister and coauthors (e.g. Baumeister et al., 1998) indicate that an initial act of self-control can impair performance in succeeding tasks if these tasks also require self-control. Based on these findings, subjecting participants to a self-control demanding task in a first stage allows researchers to assess the effects of self-control on behavior in a second task. Self-control is needed to regulate behavior in different dimensions ranging from affective behavior to cognition (Hagger et al., 2010; Kotabe and Hofmann, 2015). Many of these dimensions are also relevant for financial decision making. Temporary fluctuations in self-control in financial markets could arise endogenously from making choices as part of normal market activities or from suppressing emotions connected with price fluctuations. Alternatively, they could arise exogenously from unrelated private demands. Psychological studies differentiate between *state* self control, i.e. temporary changes in the level of self-control in a person, and *trait* self-control, i.e. the relatively stable disposition to exert self-control. Besides the effect of state self-control studied in the present paper, self-control may also be relevant due to differences in personality traits between market participants. Schmeichel and Zell (2007) show that both state and trait self-control have similar effects on behavior.

[1] Source: `http://www.businessweek.com/1999/99_27/b3636006.htm` (accessed on March 15, 2016)

However, to the best of my knowledge there has been little research into the relationship between investment behavior and self-control; most of the existing evidence is correlational. Fenton-O'Creevy et al. (2011) conduct interviews with traders and senior managers at investment banks focusing on emotions and emotion regulation strategies. They find pronounced differences in emotion regulation strategies between inexperienced, low-performing and high performing traders. Similarly, Lo et al. (2005) conduct a survey on personality and emotions among participants in an online day trading course. In their study, traders with more intense emotional reactions to gains and losses exhibit a significantly worse trading performance. Furthermore, self-assessments of their participants' strengths and weaknesses suggest self-control as a highly relevant factor for investment success. The only other experiment which tests the role of self-control in a financial environment is the study by Kocher et al. (2016), who manipulate traders' self-control in the bubble market paradigm introduced by Smith et al. (1988). They observe a higher degree of overpricing in markets if either all or only half the participants' self-control is reduced. From these results it seems as if reductions in self-control can have an effect on aggregate market outcomes possibly by being reinforced through the interaction of market participants.

The rest of this paper is structured as follows: section 2 takes a deeper look at the literature related to the current studies both from economics and psychology, section 3 considers experiment 1, while section 4 covers experiment 2. I discuss the findings from both experiments in section 5 and conclude in section 6.

2 Related Literature

The present paper relates to the literature on self-control from economics and psychology. At the beginning of the sections on each experiment, I summarize research that relates to each experiment more specifically. Most studies in psychology and many papers in economics that use the same paradigm refer to the manipulation of self-control used in this paper as 'ego depletion', 'willpower depletion' or simply 'depletion'. I use these terms interchangeably.

2.1 Self-Control in Economics

In recent years, self-control has received considerable attention in behavioral economics, often as an explanation for time inconsistent decision making. Economic theory has modeled self-control in a number of ways in order to explain observations which are hard to reconcile with the rational model of (discounted) expected utility maximization (Samuelson, 1937; Von Neumann and Morgenstern, 2007).[2] In these models, a lack of self-control may cause decisions counteracting long-run interests of an individual, such as addictive behavior, under-saving and procrastination (Bucciol et al., 2010). Self-control features prominently in several models: in dual-self models of decision making, where multiple internal selves with diverging interests interact (e.g. Thaler and Shefrin, 1981; Fudenberg and Levine, 2006), models of quasi-hyperbolic discounting, i.e. relative overweighting of present utility (Laibson, 1997), and the temptation

[2]Some of the main departures from this rational decision making view are for example small stakes risk aversion (Rabin, 2000) and time-inconsistent behavior (Laibson, 1997; O'Donoghue and Rabin, 1999).

model of Gul and Pesendorfer (2001), which models self-control failures as cue-triggered mistakes (see also Benhabib and Bisin, 2005; Bernheim and Rangel, 2004; Kim, 2006). Lack of self-control may also be connected with overspending (Heidhues and Koszegi, 2010). More recently, willpower has been explicitly modeled as an internal depletable resource (see Ali, 2011; Fudenberg and Levine, 2012; Ozdenoren et al., 2012).

Meanwhile, the empirical literature in economics has considered the impact of self-control on decision making using two sets of methods: 1) self-reported survey measures of self-control and 2) experiments manipulating self-control. Ameriks et al. (2003) and Ameriks et al. (2007) look at the connection between wealth accumulation and the 'propensity to plan' and self-control respectively. Both studies attribute heterogeneity in savings and wealth among households to differences in these measures. Similarly, Gathergood (2012) uncovers a positive association of lack of self-control and consumer over-indebtedness in a UK sample. Various areas of economics have adopted the experimental paradigm of Baumeister et al. (1998) in recent years to evaluate the impact of ego depletion on economic outcomes, ranging from the impact of self-control on productivity (e.g. Bucciol et al., 2011, 2013), via time preferences (e.g. Burger et al., 2011; Kuhn et al., 2014) to social preferences (e.g. Achtziger et al., 2015; Xu et al., 2012).

More closely related to the current paper, self-control manipulations have been found to have mixed effects on risky decision making. Several studies find increased risk aversion following ego depletion, in particular in dynamic situations where losses are experienced immediately (De Langhe et al., 2008; Kostek and Ashrafioun, 2014)[3] or when the role of responsibility for decision making is stressed (Unger and Stahlberg, 2011). On the other hand, several studies also find an increase in risk taking following ego depletion. This pattern seems to be in particular present in one shot choices (Bruyneel et al., 2009; Friehe and Schildberg-Hörisch, 2014), questionnaire results and the balloon anaolgue risk task (both in Freeman and Muraven, 2010). Both Stojić et al. (2013) and Gerhardt et al. (2015) find no significant effect of ego depletion on risk preferences elicited from choice lists based on the procedure by Holt and Laury (2002). Finally, considering the interaction of framing effects with ego depletion, De Haan and Van Veldhuizen (2015) do not detect an effect of ego depletion on performance in several framed tasks: a prisoner's dilemma, an attraction effect task, a compromise effect task, and an anchoring task.

2.2 Ego Depletion in Psychology

An extensive body of research in psychology shows that self-control is needed to keep a check on certain impulses. This ability deteriorates after self-control effort has been exerted. Research on self-control was sparked off by Walter Mischel and coauthors (see e.g. Mischel et al., 1989) and has recently experienced a surge in attention, partly motivated by the work of Roy Baumeister and co-authors (e.g. Baumeister et al., 1998). Baumeister et al. (1998) introduced the dual task paradigm to look at the effect of an initial 'depletion' stage on a dependent measure in a second stage. Following up on these results, research considering ego depletion has mushroomed in recent years.[4] Initially, the 'strength model' of self-control, which posits that self-control works like a muscle, seemed to be a good fit due to a number of findings:

[3]Note that none of these studies systematically look at the effect of ego depletion on loss aversion.

[4]Inzlicht and Schmeichel (2012) mention more than 100 experiments; for an overview, see the meta study by Hagger et al. (2010) which is based on 198 experiments.

self-control regenerates through rest (Tyler and Burns, 2008), can be trained by regular exercise (Muraven et al., 1999), considerably differs between individuals (Tangney et al., 2004) and can be replenished via glucose intake (Masicampo and Baumeister, 2008).
However, this model cannot accommodate a number of more recent findings: first of all, ego depletion can be overcome by giving financial incentives (Muraven and Slessareva, 2003) and by inducing positive mood (Tice et al., 2007). Furthermore, merely gurgling a glucose laden drink is already sufficient to reverse the effects of ego depletion (Molden et al., 2012). Finally, believing that self-control acts as a limited resource predicts whether participants are susceptible to the ego depletion effect (Job et al., 2010). Due to this recent evidence, the 'process model' of self-control has emerged which distinguishes between motivational and attentional factors as responsible for ego depletion effects (Inzlicht and Schmeichel, 2012).
However, apart from increasing support for the concept of *state* self-control, studies on ego depletion effects have received a considerable amount of critique recently. Carter and McCullough (2014) found evidence for publication bias in studies on ego depletion by correcting for small study effects. Xu et al. (2014) fail to replicate the depletion effect using a typical dual task setting in four separate studies.

3 Experiment 1: The Disposition Effect

The disposition effect (DE) can be defined as the propensity to sell winners – i.e. stocks that have gained in price relative to some reference price – too early and to ride losers – stocks that have lost in price – for too long (Shefrin and Statman, 1985). It constitutes a violation of expected utility maximization, since the historical price at which an asset was acquired should not play a role for the decision to sell it. Shefrin and Statman (1985) explain the presence of the DE with four major elements – mental accounting, regret aversion, self-control and tax considerations.
The possible impact of self-control on the DE can be illustrated with the help of the idea of realization utility formalized by Ingersoll and Jin (2013) and Barberis and Xiong (2012): investors receive bursts of utility (disutility) right at the moment of selling an asset for a gain (loss) additionally to consumption utility. The DE arises from trading off long-run portfolio performance and short-term realization utility, e.g. realizing a loss is painful in the short-term, but pays off in the long-run because an inferior asset is sold. In this framework, self-control problems can affect discounting or the relative strength of utility vs. disutility bursts, i.e. loss aversion. In the former case, participants become more present-biased or more impatient in a state of low self-control, in other words they care more about present utility bursts and therefore speed up realizing gains and postpone realizing losses. In the latter case, a state of low self-control increases loss aversion due to more pronounced emotional reactions, or to put it differently it reinforces the negative utility bursts from realizing losses relative to the utility bursts from realizing gains, making realizing losses more aversive and postponing their realization more attractive.

3.1 Related Literature

Shefrin and Statman (1985) provide the first formal presentation of the DE hypothesis and suggest a theoretical framework. Three influential papers are among the first to convincingly confirm the DE:

Odean (1998) rigorously analyzes the DE establishing its presence in a sample of 10.000 accounts from a large discount brokerage, while Grinblatt and Keloharju (2001) find strong evidence for the DE in a comprehensive sample of all stock market investors in Finland. Weber and Camerer (1998) develop the experimental task for the DE that I use in the current experiment. In their setting, Bayesian updating of expectations would imply holding on to winning stocks and selling off losers. Thus, displaying the DE is a clear mistake. Nevertheless, subjects in this study behave in line with the DE. However, when shares are automatically sold after each period, the DE is greatly reduced.

Several studies have looked at factors responsible for heterogeneity in the DE, both experimentally and using market data. Professional investors seem to suffer from the DE to a lower degree (Shapira and Venezia, 2001), which is in line with the finding that measures of a trader's sophistication correlate negatively with the DE (Feng and Seasholes, 2005; Dhar and Zhu, 2006). Trading experience reduces the DE both in repeated trading experiments (Weber and Welfens, 2007) as well as following repeated investment decisions in real stock markets (Feng and Seasholes, 2005; Dhar and Zhu, 2006). Frydman and Rangel (2014) experimentally show that the DE is responsive to the saliency of a stock's purchasing price. Finally, commitment devices in the form of stop loss and take gain orders can reduce the scope of the DE (Fischbacher et al., 2015), which can be interpreted as evidence that (lack of) self-control plays an important role for the disposition effect.

3.2 Design

First, participants are randomly allocated to participate in two different versions of the letter-e-task[5] (Baumeister et al., 1998). I refer to participants with the difficult version of this task as *Low SC* participants and to participants with the easy version as *High SC* participants respectively. In what follows participants trade assets in the DE task (Weber and Camerer, 1998). Finally, they fill out a number of control tasks including: the cognitive reflection test (CRT, cf. Frederick, 2005), choice lists to elicit risk preferences and loss attitude (Tanaka et al., 2010), financial literacy questions (Van Rooij et al., 2011), the short self-control scale of Tangney et al. (2004) and a number of socioeconomic questions.

3.2.1 The Letter-E-Task

The letter-e-task (Baumeister et al., 1998) is one of the most commonly used and most effective tasks in the literature on ego depletion (Hagger et al., 2010). We use a computerized German version lasting 7.5 minutes closely resembling the one in Sripada et al. (2014). Participants are shown one word on a screen for 3 seconds and have to classify it according to a specific rule into one of two categories. They do so by pressing or refraining from pressing the 'e' button on their keyboard within the 3 seconds. In the no-regulation version, participants have to press the 'e' button if the word contains the letter 'e'. Participants in this condition are referred to as *High SC* participants, as their self-control capacities should not be impacted by the task (Baumeister et al., 1998). In the regulation version, participants are given a more complicated rule: they have to press the 'e' button if the word contains the letter 'e', but only if the 'e' is not either immediately next to or one more letter away from another vowel. Therefore, when participants

[5] A translation of the instructions can be found in appendix A.3.1.

see the letter 'e' they have to override their first impulse to press the 'e' button and check, whether there is another vowel up to two letters away from the 'e'. This exertion of self-control to override a dominant impulse impacts their ability to exert self-control in the experiment later on (Baumeister et al., 1998). Participants in this treatment are referred to as *Low SC* participants in the following. Participants from both treatment groups are shown exactly the same words in a fixed random order: 30 words containining no 'e', 60 containining an 'e' but with another vowel closeby, and 60 containing an 'e' with no other vowel closeby. Table 1 gives a hypothetical example for the classification of three English words for each treatment. Directly after the letter-e-task, participants have to evaluate as how strainful and difficult they perceived the task and how frustrated and tired they feel on a 7-point Likert scale. To avoid wealth effects, participants receive a flat payment of 3.00 € for this task.[6]

Table 1: Examples of Classifications in the Letter-E-Task

	High SC	Low SC
plastic	✗	✗
business	✓	✗
trouble	✓	✓

Note: ✓(✗) corresponds to (not) pressing the 'e' button

3.2.2 The Disposition Effect Task

Our DE task closely resembles the adaptation of Weber and Welfens (2007) of the DE task in Weber and Camerer (1998). Participants are given an initial endowment of 2,000 points – equivalent to 10.00 € – and observe the price movements of six different goods over three initial periods. Subsequently, they can buy and sell these goods over 14 periods. In the last period, subjects see their final portfolio of goods which is then automatically sold at its current price. The proceeds are added to the cash holdings and paid out to the participants at the end of the experiment.

The prices of goods move from period to period according to a random process. The price of every good either increases by 6% or decreases by 5% each period. This upward-moving price path incentivizes participants to actively trade goods (Weber and Welfens, 2007). Short selling and borrowing are not allowed. In the initial period, all goods start off at the same price of 100 points. Goods differ only by their underlying probability of a price change, which is held constant. Each good i is given exactly one of the following probabilities of a price increase: $p_i \in \{65\%, 55\%, 50\%, 50\%, 45\%, 35\%\}$. The order of the probabilities as well as the actual price realizations are randomly allocated to goods across pairs of subjects. Thus, two subjects in each session – one *Low SC* and one *High SC* subject – always receive the same price path, so that we can directly compare their behavior, but at the same time we avoid finding an effect which might be specific to a specific price path. The mechanics of the price movements are common knowledge, but subjects need to infer each good's probability of a price increase by observing the realized price paths.

[6]Note that Achtziger et al. (2011) test whether different incentive schemes during the depletion stage have a differential effect on ego depletion and find no difference between flat and piece rate incentives.

In order to determine which asset has the highest probability of a price increase, Bayesian updating requires subjects to count the number of price increases of each good, which corresponds to ordering goods according to their current price. Therefore, a risk neutral agent's optimal strategy would lead to the opposite of the DE – selling off assets that have previously lost in value and keeping assets that have previously gained in value. Hence, the DE is a mistake in this environment (Weber and Camerer, 1998). Our design differs from Weber and Welfens (2007) in a couple of points: First, subjects give their expectations about the probabilities of a price increase of each good at the beginning of three periods – the first trading period, the 7th trading period and the last trading period. They allocate each of the six probabilities to exactly one good and receive 20 points for each correctly allocated expectation at the end of the experiment. Due to missing responses for a number of participants who failed to make an input within the allowed time at least once, the answers from the expectations subtask are not further evaluated here.[7]. Secondly, in order to avoid long waiting times and to prevent the depletion effect from differentially wearing off across subjects, participants proceed automatically to the next period after the time allocated to the current period runs out. Participants have 20 seconds time to observe prices in non-trading periods, 40 seconds in trading periods and an additional 90 seconds for entering their expectations. Thirdly, to ensure understanding of the trading environment, participants complete three practicing tasks without a time limit and have to answer 7 multiple choice questions about the goods market correctly before the self-control manipulation in part 1 starts.

3.2.3 Additional Measures

After part 2, further experimental measures[8] are collected: First, participants answer the three questions of the CRT (Frederick, 2005) without incentivization. Then participants receive two sets of incentivized choice lists on two separate screens to measure risk preferences and loss aversion adapted from Tanaka et al. (2010). The switching point to the right option among the 11 choices on the first screen identifies risk preferences and the switching point to the right among the seven choices on the second screen identifies loss aversion (Tversky and Kahneman, 1992) with later switches to the right option on each screen implying higher degrees of risk aversion and loss aversion respectively. One of these 18 choices is randomly determined for payout and simulated at the end of the experiment. Thirdly, subjects answer five financial literacy questions adapted from Van Rooij et al. (2011) receiving 0.20 € for each correct response. At the end of the experiment, subjects fill out two sets of questionnaires: first the 13 items of the brief self-control scale on a 7-point Likert scale (Tangney et al., 2004) and then a number of socio-economic questions.

[7]61 out of 142 participants missed at least one expectation elicitation, 25 in the *Low SC* condition and 36 in the *High SC* condition. This difference is significant according to a χ^2 test ($p = 0.062$) Comparing the sum of absolute differences between the prescription of Bayesian updating and the actual expectation inputs for those subjects who made all inputs yields no significant differences between the treatments (Mann-Whitney-U test, $p = 0.8899$).

[8]The interested reader may refer to appendix A.1.1 for a more extensive explanation of these measures and for the rationale behind including them.

3.2.4 Procedure and Sample Size

In order to avoid restoration of self-control capacities on the one hand (Tyler and Burns, 2008) and information overload on the other hand, instructions to the experiment are handed out and read to participants in two blocks: first we do so for the letter-e-task and the disposition effect task and then, after the completion of these two parts of the experiment, for the rest of the experiment. After each part of the instructions, participants can ask questions in private.

Sessions were implemented using z-Tree (Fischbacher, 2007) and subjects were recruited using ORSEE (Greiner, 2015). We conducted the sessions in December 2014 and January 2015[9] at MELESSA in Munich. Both treatments were conducted within the same session by giving different on-screen instructions for the letter-e-task. Sessions lasted about 90 minutes and participants earned 20.55 € on average including a show-up fee of 4.00 €.

A total of 142 participants equally split between the two treatments took part in six experimental sessions. This sample size allows me to detect the average effect size $d = 0.62$ (Cohen's d) of studies on ego depletion contained in the meta analysis of Hagger et al. (2010) with 95.6% probability and an effect of size $d = 0.474$ with 80.0% probability. Only 6 of the 198 studies contained in Hagger et al. (2010) exceed this sample size, which might help to alleviate small-study concerns (e.g. in Carter and McCullough, 2014).

3.3 Results

Table 12 in the appendix reports manipulation checks of the treatment by comparing corrrectly classified words in the letter-e-task and the subjective measures asked immediately after the letter-e-task and at the end of the experiment between treatments. According to Mann-Whitney U-Tests (MWU) participants in the *Low SC* condition classified about 10 words less than those in *High SC* correctly (MWU, $p < 0.01$), experienced the task to be significantly more straining, more difficult and were more frustrated after the task (MWU, all $p < 0.01$). Neither tiredness nor measures for mood were significantly impacted by the task.[10]

3.3.1 The Disposition Effect

I apply the measurement of the disposition effect according to Odean (1998) based on the number of each asset sold at a gain or a loss with respect to a reference price. For this purpose, I relate actual sales to selling opportunities at a gain or loss, where gains and losses are measured with respect to the weighted average purchase price (WAPP) of an asset.[11] This ensures that the results are not affected by a lack of selling opportunities at a gain or loss. Proportion of gains realized (PGR), proportion of losses realized (PLR) and the disposition effect measure (DE) are calculated in the following way:

$$PGR = \frac{\text{\# of sales at gain}}{\text{\# of selling opportunities at gain}} \tag{1}$$

[9]The times of each session are summarized in table 11 of the appendix.

[10]One of the subjects in *Low SC* seems not to have complied with the letter-e-task having pressed the 'e' button only 14 times throughout the task. All the results reported in this section are robust to excluding this participant from the analysis.

[11]Results are not sensitive to using the alternative reference prices of highest purchase price, lowest purchase price, first purchase price or most recent purchase price. Additonally, results are not sensitive to using amounts of each asset traded or just the number of times an investor sells at gains or losses (i.e. quantity weighted or trade weighted measures).

$$PLR = \frac{\text{\# of sales at loss}}{\text{\# of selling opportunities at loss}} \tag{2}$$

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

DE is the difference between the percentage of gains realized and the percentage of losses realized and lies in the interval $[-1, 1]$. If an investor sells every position as soon as the price exceeds the purchasing price, i.e. $PGR = 1$, and keeps all the assets that have lost in value, i.e. $PLR = 0$, *DE* will take the value of 1. If an investor immediately exits every losing position and keeps all the positions that have gained in price, the *DE* measure will take the value of -1. Higher values of *DE* thus correspond to an investor displaying the disposition effect to a higher degree.

First, I reproduce the presence of the disposition effect. Figure 1 shows the *DE* measures including 95% confidence intervals for each treatment. Table 2 tests the presence of the disposition effect by comparing the *DE* measure to 0 indicating that there is a weakly higher tendency in the overall sample and in the two treatment groups to sell winners more frequently than losers. Note that due to Bayesian updating a risk-neutral investor should sell losers more frequently than winners. Depending on the specific price path, rationality implies a negative optimal value of *DE*. Thus, comparing the *DE* measure to 0 is a conservative test of the presence of the disposition effect.

Secondly, I compare the size of *DE* between *High SC* and *Low SC* participants. Figure 2 compares the individual *DE* measures of the two participants that saw an identical price development, thus controlling for heterogeneous effects of price paths.[12] If the *Low SC* treatment had a positive impact on the *DE* measure, the points in figure 2 would lie to the right of the 45° line more frequently, which is not the case. Wilcoxon signed-rank (WSR) tests reported in table 3 confirm for each of the components of the *DE* measure as well as for the number of shares traded that there are no statistically significant differences between *Low SC* and *High SC* participants.

Heterogeneity:

There is no evidence for heterogeneous treatment effects on different subgroups: Regressions of the *DE* measure on various explanatory variables and their interaction term with a dummy for the *Low SC* treatment in table 14 of the appendix, as well as MWU tests for subgroups in table 15 and table 16 confirm that there is also no heterogeneity in the treatment effect based on CRT scores of participants or based on the Self-Control-Scores (SCS) of participants. Thus, this null result is not driven by opposing effects for different subsamples.

3.3.2 Trade Clustering

In the exploratory analysis reported here, I consider the trade clustering (*TC*) measure suggested as a measure of endogenous narrow bracketing by Kumar and Lim (2008). Using discount brokerage data they find that investors who execute trades in a more clustered way exhibited weaker disposition effects

[12] For 3 participants – all of them in the *Low SC* treatment – no *DE* measure could be calculated, because they never had any loss opportunities, thus the data for three pairs of participants is lost when I look at the paired data.

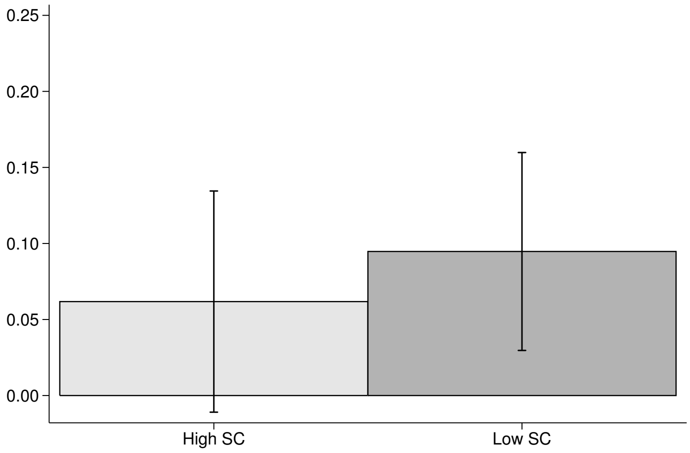

Figure 1: Disposition Effect Measure by Treatment

Table 2: Presence of the Disposition Effect

	Mean PLR	Mean PGR	Mean DE	$\#DE > 0$	$\#DE \leq 0$	p-value
All	0.166	0.239	0.078	82	57	0.023**
High SC	0.173	0.235	0.062	43	28	0.096*
Low SC	0.158	0.244	0.095	39	29	0.154

Note: p-values from binomial tests with $H_0 : p(DE > 0) = 0.5$; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

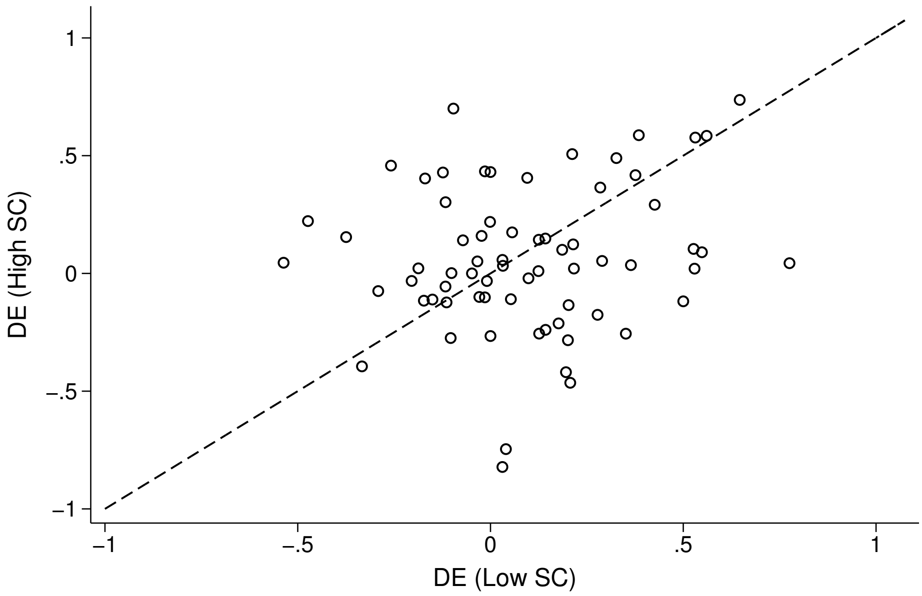

Figure 2: Paired Disposition Effect Measures across Treatments

Table 3: Effect of Self-Control Manipulation on Disposition Effect Measures

	High SC	Low SC	p-value
PLR	0.173	0.158	0.925
PGR	0.235	0.244	0.733
DE	0.062	0.095	0.625
shares traded	46.197	48.901	0.470

Note: Disposition Effect measures based on weighted average purchase price (WAPP); p-values from two-sided paired Wilcoxon Signed Rank (WSR) tests with participants matched by price path

and held better diversified portfolios, presumably because they consider trades executed on the same day together rather than separately. I look at a measure of narrow bracketing because some of the previous effects of ego depletion on economic outcomes (e.g. Kocher et al., 2016) could be the result of an increase in narrow bracketing, i.e. of a higher tendency to consider decisions separately from each other. TC can be calculated by using the following equation:

$$TC = 1 - \frac{\#\text{ of trading periods}}{\#\text{ of distinct trades}} \tag{4}$$

I define trading periods as periods in which participants execute trades and distinct trades as the sum of the number of distinct assets that a subject traded per period over all periods. Here, TC can lie in the range[13] $[0, \frac{5}{6}]$. If a subject executes distinct trades only in separate periods, i.e. # of distinct trades = # of trading periods, this measure takes the value 0. The more distinct trades a subject executes per trading period on average, the higher TC will be. Given the presence of risk aversion, it is impossible to compare TC to its optimal level and to compare deviations from this optimal level between participants. Therefore, I concentrate on the raw measure.[14]

Figure 3 displays mean TC and 95% confidence intervals by treatment on the left and TC measures paired by participants with the same price path on the right. The left part of this figure suggests that there is a slight treatment effect, i.e. TC is reduced by the treatment. The paired graph on the right does not display a clear pattern, even though the points seem to have a tendency to lie above the 45° line. The analyses contained in table 4 confirm that there is a weakly significant difference of nearly 5 percentage points in TC between *High SC* and *Low SC* traders (MWU, $p = 0.077$), which however becomes insignificant when exploiting the grouping of traders by price path (WSR, $p = 0.226$). *Low SC* insignificantly reduces the number of distinct trades by roughly 2 (MWU and WSR, $p > 0.1$), while the number of trading periods is slightly reduced, but again insignificantly (MWU and WSR, $p > 0.1$). Thus, the effect of *Low SC* on TC seems to be driven by the combined effect on distinct trades and number of trading periods.

Heterogeneity

[13] Due to the maximum of 14 trading periods and the maximum of 6 distinct trades that can be executed per period, I get $\bar{TC} = 1 - \frac{14}{14\times 6} = \frac{5}{6}$ for the upper limit.

[14] In the present context, the optimal level of the TC measure for a risk neutral Bayesian updater is path dependent and can be easily obtained. Details can be found in appendix A.1.6. Since the assumption of risk neutrality is clearly not given in the data and precludes the diversification motive in trading, I consider it an implausible comparison and do not follow this approach.

Table 5 displays results from MWU tests, where participants have been split into three groups, according to their CRT responses, following the classification suggested in Cueva et al. (2016). Participants who gave at least two of the incorrect impulsive[15] responses in the CRT were classified as *impulsive*, participants who gave at least two correct responses were classified as *reflective*, while the third group consists of the residual. It turns out that the effect of *Low SC* on *TC* is only present and significant for the *reflective* group of participants, whose *TC* drops by nearly 14 percentage points from 0.510 to 0.373 (MWU, $p < 0.01$), while for the other two groups the effect goes in the opposite direction and is statistically insignificant.

Table 6 reports the results of a similar subgroup analysis for participants who were grouped according to their tercile in the self-control questionnaire. Directionally, it seems as if only the participants in the lowest and middle tercile of *SCS* responses are affected negatively by the self-control manipulation, while this effect is only marginally significant for the 2nd tercile (MWU, $p = 0.058$).

To corroborate these findings and to explore the explanatory value of the additional measures, I conduct tobit regressions which I report in table 7. I use the dummy variable *Low SC* taking the value 1 for participants in the *Low SC* treatment as the main explanatory variable in these regressions and control for the heterogeneity in price paths by including price path dummies.[16] Furthermore I successively add control variables, some of which I interact with the *Low SC* dummy:

- *female*: dummy taking the value 1 for females
- *ln(age)*: the natural logarithm of age
- *CRT*: number of correct responses to the CRT questions
- *SCS*: self-control score from the brief self-control scale
- *FLQ score*: number of correct responses to the financial literacy questions
- *switch LA*: switching point on the screen measuring loss aversion
- *switch RA*: switching point on the screen measuring risk aversion

These regressions confirm the negative effect of the treatment on *TC* on average ($p < 0.05$ in specification 1) and furthermore replicate the result that the negative effect of the treatment on *TC* is driven by subjects with a higher CRT score: Higher CRT scores are significantly related to higher degrees of *TC* in the *High SC* group ($p < 0.1$ in specifications 3, 5 and 6, $p < 0.05$ in specification 4) and significantly negatively correlated with *TC* in the *Low SC* group ($p < 0.01$ for post estimation Wald tests of H_0 : $\beta_{CRT} + \beta_{CRT \times lowSC} = 0$ in specifications 3 to 6). *SCS* and its interaction with the *Low SC* dummy are not significantly correlated with *TC* in these regressions. Similarly, none of the coefficients of *FLQ*, *switch LA* or *switch RA* is significant.

Overall, there is a weakly significantly negative effect of the self-control manipulation on trade clustering which seems to be primarily driven by a strong negative effect on highly reflective individuals.

[15] Impulsive responses are 10 for the ball question, 100 minutes for the machine question and 24 for the water lily question.

[16] The results are qualitatively similar when excluding the price path dummies.

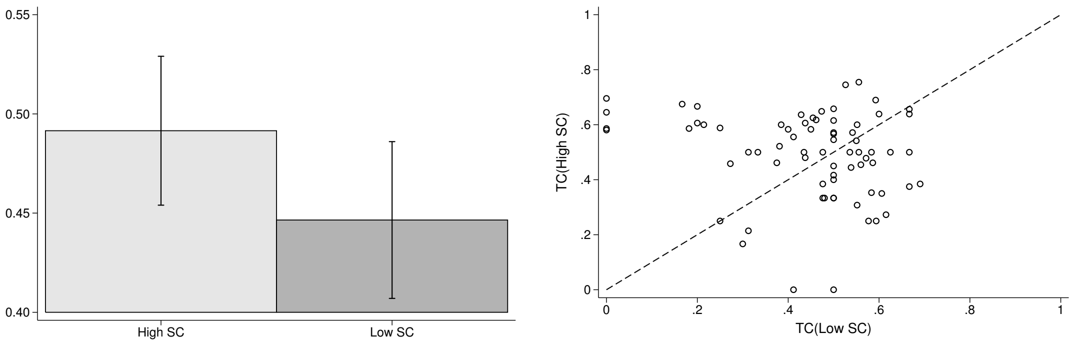

Figure 3: Trade Clustering Measures across Treatments

Table 4: Effect of Self-Control Manipulation on Trade Clustering and Related Measures

			p-values	
	High SC	Low SC	MWU	WSR
TC	0.492	0.447	0.077*	0.226
distinct trades	20.986	18.972	0.357	0.412
trading periods	9.338	9.239	0.910	0.986

Note: p-values from two-sided Mann-Whitney U Tests (MWU) comparing columns and paired Wilcoxon Signed Rank Tests (WSR) with participants matched by price path respectively; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: Heterogeneity of Effect of Self-Control Manipulation on Trade Clustering by Cognitive (Ir)Reflection

	High SC	N High	Low SC	N Low	p-value
impulsive	0.490	18	0.500	27	0.926
residual	0.459	19	0.522	12	0.273
reflective	0.510	34	0.373	32	0.001***

Note: impulsive individuals had at least 2 impulsively wrong responses in the CRT, reflective individuals had at least 2 correct responses; p-values from two-sided Mann-Whitney U tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Heterogeneity of Effect of Self-Control Manipulation on Trade Clustering by Self-Control Score

	High SC	N High	Low SC	N Low	p-value
1st tercile	0.470	27	0.428	26	0.407
2nd tercile	0.528	27	0.443	16	0.058*
3rd tercile	0.468	17	0.465	29	0.882

Note: p-values from two-sided Mann-Whitney U tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.3.3 Effect on Additional Measures

Finally, in line with the results in Kocher et al. (2016) and the null results of the effect of ego depletion on risk preferences elicited from choice lists (Stojić et al., 2013; Gerhardt et al., 2015), there was no significant effect on the CRT score (MWU, $p = 0.485$), risk aversion (MWU, $p = 0.616$ for switches in the gains list) or loss aversion (MWU, $p = 0.352$ for switches in the mixed list).[17]

Table 7: Tobit Regressions of Trade Clustering on low SC and other Explanatory Variables

	(1)	(2)	(3)	(4)	(5)	(6)
			TC			
Low SC	-0.0462**	-0.0427*	0.0903**	-0.0820	-0.0832	-0.0843
	(0.0219)	(0.0221)	(0.0411)	(0.137)	(0.138)	(0.138)
female		0.0308	0.0299	0.0329	0.0340	0.0333
		(0.0310)	(0.0307)	(0.0303)	(0.0313)	(0.0314)
ln(age)		-0.0306	-0.113	-0.131	-0.131	-0.132
		(0.0997)	(0.0974)	(0.0959)	(0.0959)	(0.0984)
CRT			0.0359*	0.0396**	0.0389*	0.0402*
			(0.0194)	(0.0191)	(0.0197)	(0.0204)
CRT $\times$ Low SC			-0.0941***	-0.0902***	-0.0898***	-0.0911***
			(0.0245)	(0.0240)	(0.0242)	(0.0247)
SCS				0.00116	0.00112	0.00116
				(0.00176)	(0.00178)	(0.00180)
SCS $\times$ Low SC				0.00296	0.00297	0.00303
				(0.00240)	(0.00240)	(0.00241)
FLQ score					0.00123	0.000909
					(0.00946)	(0.00954)
switch LA						-0.00134
						(0.0111)
switch RA						0.00142
						(0.00530)
Constant	0.685***	0.744**	0.928***	0.890***	0.890***	0.883***
	(0.0926)	(0.315)	(0.306)	(0.314)	(0.314)	(0.317)
Price Path Dummies	Yes	Yes	Yes	Yes	Yes	Yes
σ	0.130***	0.130***	0.123***	0.120***	0.120***	0.120***
	(0.00801)	(0.00798)	(0.00755)	(0.00741)	(0.00741)	(0.00740)
Observations	142	142	142	142	142	142

Note: Low SC is a dummy variable taking te value 1 for the low SC treatment and 0 otherwise; ln(age) is the natural logarithm of age; SCS stands for self-control score; FLQ stands for financial literacy questionnaire; switch LA and switch RA denote switching points on the list measuring loss aversion and risk aversion with later switches (higher values) indicating higher degrees of loss and risk aversion respectively; standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4 Experiment 2: Myopic Loss Aversion

Myopic Loss Aversion (MLA) can arise in dynamic decision making environments, e.g. when repeatedly deciding whether to invest in an asset or a gamble. It consists of loss aversion and myopia, and implies that (temporary) losses are weighted more strongly when presented in a more disaggregated way (cf. Benartzi and Thaler, 1995). Thus, presenting investment decisions in a more disaggregated way (or giving feedback more frequently) typically results in lower investment levels. Gneezy and Potters (1997)

[17] Appendix A.1.4 analyzes the impact of the self-control manipulation on these additional measures in more depth.

show that people who repeatedly invest in a specific binary mixed lottery invest higher amounts if they receive feeback and make their choices less frequently.

Since MLA is a combination of loss aversion and myopia, either of these aspects might be impacted by self-control: either subject's negative utility from losing money might be more pronounced or subject's tendency to evaluate gambles separately or jointly might be affected, i.e. subjects might be less likely to think about alternative viewpoints of their choice. The findings from experiment 1 suggest the latter explanation ex ante.

In the following, I refer to the reduction of decision frequency as a broad (investment) frame, and to more frequent decisions as a narrow (investment) frame.

4.1 Related Literature

Benartzi and Thaler (1995) introduce Myopic Loss Aversion (MLA) consisting of loss aversion and myopia as an explanation for the equity premium puzzle of Mehra and Prescott (1985): Salient price drops when frequently evaluating one's portfolio might cause stock owners high levels of discomfort. For such disutility, they need to be compensated by higher equity premiums than suggested by the simple model in Mehra and Prescott (1985). From an intuitive viewpoint, myopia or narrow bracketing is a prerequisite for loss aversion in a dynamic context to affect behavior and market prices, since if gambles would be considered as part of a larger portfolio or integrated with wealth, there would be hardly any scope for experiencing losses (see also Barberis et al., 2001).

The idea of narrow bracketing builds on the findings of Tversky and Kahneman (1981) who show that people may make dominated choices when evaluating two pairs of lottery choices separately rather than jointly. There seems to be a somewhat artificial distinction in the literature between narrow bracketing and MLA, with the former usually referring to a situation of simultaneous decision making and the latter to a dynamic decision making context. However, MLA can be considered as a special case of narrow bracketing (Read et al., 1999). Read et al. (1999) provide an overview about narrow bracketing showing that choice bracketing is an important determinant of behavior in a wide range of contexts. They discuss factors that determine whether people bracket broadly or narrowly and assert that there is a lack of knowledge of such factors. Thaler et al. (1997) and Gneezy and Potters (1997) first experimentally test MLA. In the following, I concentrate on factors influencing narrow bracketing that have emerged with a focus on the experimental paradigm of Gneezy and Potters (1997).

There is a number of studies that consider how MLA correlates with subject characteristics. It seems that there is considerable heterogeneity in MLA among different groups of people, but only few factors that affect the degree of narrow bracketing have been identified. According to Haigh and List (2005) professional investors react more strongly to an exogenous change in investment frames than students, which Eriksen and Kvaløy (2010) replicate using a sample of financial advisers. Glätzle-Rützler et al. (2015) do not detect the typical MLA pattern in a sample of adolescents.[18] Van der Heijden et al. (2012) conduct MLA experiments with a large representative sample in the Netherlands and reveal significantly

[18] One could speculate that these differences might be connected to differences in cognitive abilities, motivation of the subjects and timing of the experiments: the sessions in schools were always conducted in class in the morning, while those with traders in Haigh and List (2005) were conducted in the evening after trading (both from personal communication).

larger framing effects for more impatient individuals using a measure of time discounting. The authors speculate that accessibility of information accounts for the connection between the MLA measure and impatience, i.e. that intuitive thinkers both think less about less accessible consequences in the future as well as about less accessible characteristics of a repeated lottery such as the diversification it entails. Surprisingly, Van der Heijden et al. (2012) also find larger MLA effects for participants with a CRT score of at least 2. In the study by Hilgers and Wibral (2014), low maths grades and impulsivity as measured by the Barratt Impulsiveness Scale (Patton et al., 1995) are predictive of an increased MLA effect.

Outside the Gneezy and Potters (1997) paradigm, Rabin and Weizsäcker (2009) study the theoretical and empirical generality of the narrow bracketing result in Tversky and Kahneman (1981). The data from their experiments indicates a rather unifrom tendency towards narrow bracketing that does not vary much with observable background characteristics.

Other studies have directly manipulated features of the MLA task. Some authors disentangle the increased investment in the broad investment frame and attribute it to the effects of feedback frequency and investment horizon. However, they reach somewhat different conclusions: Fellner and Sutter (2009) find that both feedback frequency and investment horizon play similar roles, while Langer and Weber (2008) (using a multiplicative version of the MLA task) and Bellemare et al. (2005) attribute the more important role to investment horizon and feedback frequency respectively. Fellner and Sutter (2009) also analyze the effect of an endogenous choice of investment frames and how participants can be 'nudged' to remain in the broad investment frame. They find no effect of information provision about performance of previous participants, but default setting works to make subjects remain in the broad frame. Hilgers and Wibral (2014) consider the role of learning in the MLA paradigm by subjecting participants to two sets of MLA tasks with a potential switch of investment frame. In their setting, a broad frame increases investments, but switching to the narrow frame does not reduce them, thus making initial framing differences disappear in the second set of MLA tasks, if subjects had previously been in the broad frame. This learning effect is particularly strong for participants classified as impulsive and for individuals with high cognitive skills. Using an unincentivized variation of the Gneezy and Potters (1997) paradigm and only considering the narrow investment frame, De Langhe et al. (2008) find a reduction in investment levels following ego depletion. Some of the results in Benjamin et al. (2013) might also indicate a factor impacting narrow bracketing: differences in risk aversion over small stakes are related to heterogeneity in cognitive abilities. The task they use involves multiple choices between safe payoffs and 50:50 lotteries and between two 50:50 lotteries. Importantly, unlike in the standard procedure for choice list experiments (e.g. Holt and Laury, 2002), all the choices of a participant are paid out. In another part of their study, Benjamin et al. (2013) manipulate subjects' cognitive load, which reduces the number of risk neutral choices.[19] It is possible that this effect is driven by the reduced tendency of participants to jointly evaluate choices under cognitive load.

[19]Hofmann et al. (2009) propose that cognitive load and ego depletion tasks have a similar effect on decision making.

4.2 Design

I apply a 2×2 between subjects design: in one dimension, participants' self-control is manipulated by subjecting them to the letter-e-task, resulting in the two treatments *High SC* and *Low SC*. The investment frame is varied independently between frequent investments in the *Narrow* frame and infrequent investments in the *Broad* frame.

In the first part of the experiment, the participants work on the same self-control depleting task as in experiment 1, which is followed by the MLA task (Gneezy and Potters, 1997). The third part contains a variety of background measures.[20]

4.2.1 The MLA task

I use a computerized version of the original task by Gneezy and Potters (1997) based on the implementation by Fellner and Sutter (2009). In each of 18 rounds, participants are endowed with 100 experimental currency units (ECU) (with 100 ECU corresponding to 0.50 €) out of which they can invest an arbitrary integer amount X from the interval $[0, 100]$ into a risky lottery. The outcome of the risky lottery depends on the throw of a simulated six-sided die and is independently drawn for each round. The 24 realization paths from the first session are used for all the following sessions, thus eight participants (two in each treatment) observed the same realization path. If the die shows the numbers 1 or 2, participants win the lottery and receive $100 + 2.5 \times X$ as earnings for that round. If the die shows any other number, participants lose the lottery and receive $100 - X$. Earnings for the individual rounds are added up to obtain earnings for the task.

There are two investment frames which impact the way participants make investment decisions and receive feedback. In the *Narrow* frame, participants make their investment choices X for each round separately and receive immediate feedback on each choice. In the *Broad* frame, participants decide about their investment X for the next three rounds. When they have made their choice, the same X is invested in each of the three rounds. Participants in this treatment receive feedback for all three rounds at once and are only shown their aggregated earnings over the three rounds.

Directly after finishing the investment task, participants receive four computation questions[21] on their screen. These are meant to test the participants' mathematical abilities to perform the calculations needed to discover the diversification properties given by the repeated investment in independent lotteries. All these questions require entering an integer and participants receive 0.25 € for each correct response.

4.2.2 Additional Measures

Following the computation questions, participants take part in a number of background measures: the loss aversion task from Trautmann and Vlahu (2013), which consists of 6 choices out of which one is implemented in the end, the extended CRT from Toplak et al. (2014) for which participants receive a flat payment of 2.50 €, an abbreviated version of the Barratt Impulsiveness Scale (BIS) (Spinella, 2007;

[20] A translated version of the instructions can be found in appendix A.3.2.
[21] See appendix A.2.2 for the wording and correct answers of these questions.

Stanford et al., 2009) and a number of socio-economic background measures. Please refer to appendix A.2.1 for detailed descriptions of these tasks and the rationale for including them.

4.2.3 Procedure and Sample Size

I handed out and read instructions to participants in two blocks: first for the letter-e-task and the MLA task and then – after finishing these two parts of the experiment – for the rest of the experiment. After each part of the instructions, I gave participants the opportunity to ask questions in private. Sessions and recruitment were implemented using z-Tree (Fischbacher, 2007) and ORSEE (Greiner, 2015) respectively. I conducted the sessions for this experiment in July 2015[22] at MELESSA in Munich. Sessions lasted roughly 60 minutes and participants earned 19.97 € on average including a show-up fee of 4.00 €.
A total of 191 participants took part in eight sessions – two sessions for each treatment cell. Each treatment cell thus has 48 observations, apart from *Low SC*×*Narrow* which has 47 observations. This sample size allows me to detect the following effect sizes between two cells of my treatments: the average effect size of studies on ego depletion in Hagger et al. (2010) of $d = 0.62$ is detected with power 85.2% and an effect of size $d = 0.58$ with power 80.0%. Only 2 of the 198 studies contained in Hagger et al. (2010) exceed the overall sample size of my study. Note however that the effective sample size of the current study is only 95.75 for comparisons between two treatment cells, which is still comparably high and is exceeded by only 16 of the 198 studies contained in Hagger et al. (2010).

4.3 Results

The manipulation checks – reported in table 19 of the appendix – yield very similar results as in experiment 1. Furthermore, appendix A.2.3 provides evidence that there was no significant impact of the self-control manipulation on the background measures that were collected in experiment 2, apart from a borderline statistically significant increase in loss aversion (MWU, $p = 0.103$).

4.3.1 Myopic Loss Aversion

Now, I turn to the main measure of interest of experiment 2 – the investment levels in the four different treatment cells. I am interested in whether the framing interacts with the self-control manipulation, i.e. whether the investment levels between the *Broad* and *Narrow* frame is significantly impacted by the self-control manipulation. Figure 4 displays the investment levels for the four treatments including 95% confidence intervals and table 8 tests the presence of the MLA effect by comparing investments beween *Broad* and *Narrow* within each self-control treatment and between *High SC* and *Low SC* within each investment frame using MWU tests. I obtain the expected effect that there is a larger wedge between the investment levels in the different frames within the *Low SC* participants (more than 17 ECU difference) than within the *High SC* participants (roughly 9 ECU difference). The MLA effect is only statistically significant within the *Low SC* treatment (MWU, $p = 0.007$ and $p = 0.135$ for *Low SC* and *High SC* respectively). However, I cannot reject the null hypothesis of equal investment levels between *High SC*

[22] Refer to table 18 of the appendix for the timing of each session.

and *Low SC* within each investment frame (MWU, $p = 0.425$ and $p = 0.557$ for *Narrow* and *Broad* respectively).

Heterogeneity:

In appendix A.2.5, I report results that are obtained when I divide the sample at the median impulsivity score (BIS). In line with Hilgers and Wibral (2014), I find that the framing effect is larger for more impulsive individuals who seem relatively unaffected by the *Low SC* treatment. Furthermore, the effect seems to be (insignficantly) larger for less impulsive individuals in *Low SC* compared to *High SC*, which seems to be primarily driven by a higher investment in the *Broad* frame. Thus, it seems as if *Low SC* participants who are usually not impulsive behave more similarly to impulsive individuals.

Finally, splitting the sample by CRT terciles, which I report in appendix A.2.6, indicates that the effect of the *Low SC* treatment is rather uniform across the CRT distribution with the spread being (insignificantly) larger for the *Low SC* treatment than the *High SC* treatment for every single tercile.[23]

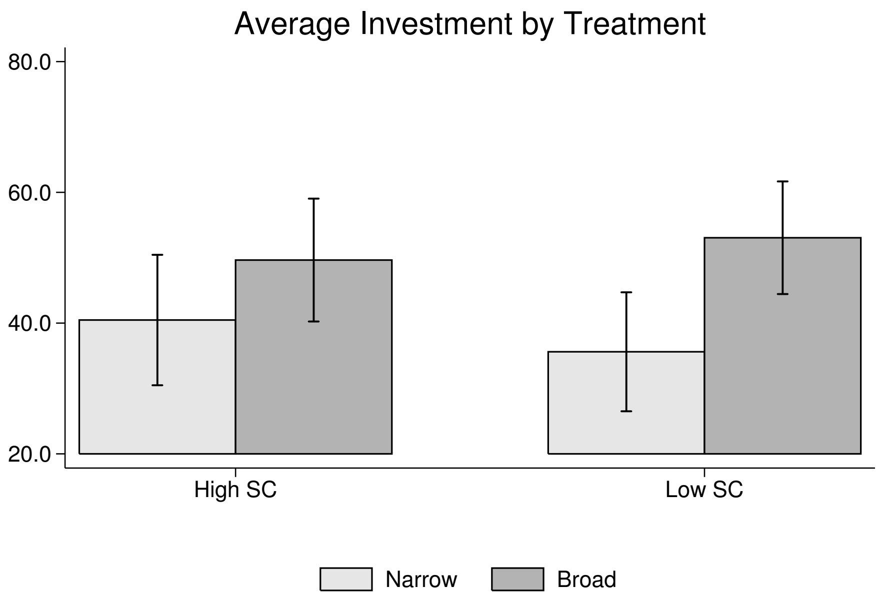

Figure 4: Average Investment by Treatment Condition

Table 8: Average Investment over all Periods by Treatments

	Narrow		*Broad*		
	mean	N	mean	N	p-value (CvC)
High SC	40.470	48	49.642	48	0.135
Low SC	35.612	47	53.056	48	0.007***
p-value (RvR)	0.425		0.557		

Note: p-values from two-sided Mann-Whitney U-tests; RvR stands for tests comparing rows i.e. depletion effects within frame, CvC stands for tests comparing columns i.e. comparing framing effects within each self-control manipulation; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[23] Similar results are obtained if subjects are divided into groups according to the CRT classification into *impulsive*, *reflective* and *residual* used in experiment 1, either based on CRT scores or on CRT7 scores.

4.3.2 Dynamics of Investments

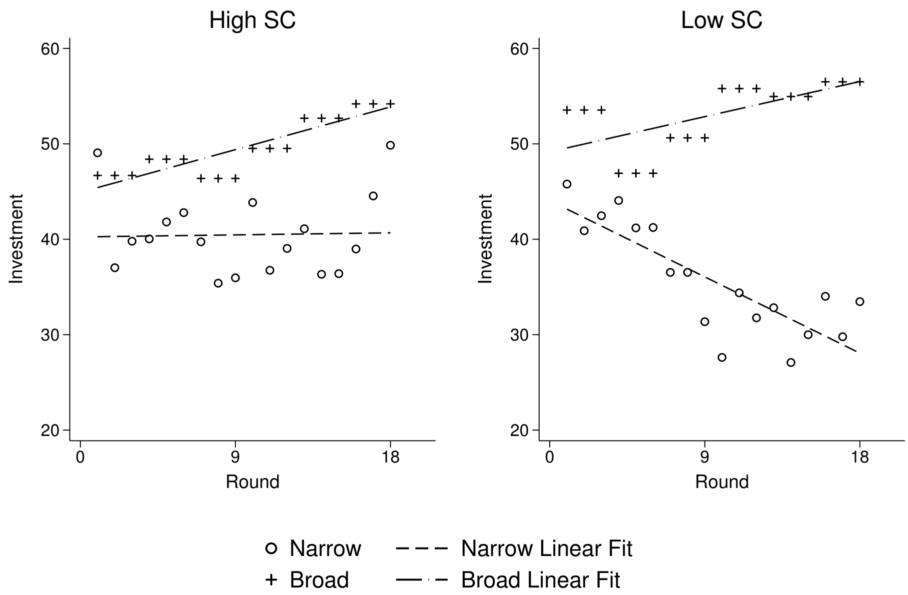

Figure 5: Average Investment per Round by Treatment Condition

Figure 5 displays the average evolution of investment levels by treatment condition over rounds. It seems like the differences between the frames within the *Low SC* treatments is driven by later investment rounds. Subjects in the *Low SC* × *Narrow* treatment were the only ones to reduce their investment levels over the course of the experiment.

Paneled tobit regressions can yield insights into what kind of investment experiences drive this divergence. I investigate different specifications of a tobit panel regression in table 9. The construction of my sample in these regressions differs from related studies that use a similar approach: Haigh and List (2005) include the investment of every round in their *Infrequent* treatment, i.e. even when participants did not make an active choice. Fellner and Sutter (2009) aggregate blocks of three choices per subject, even when subjects made three separate choices, and skipped the first block for each subject. I include all observations when a decision was made, i.e. for participants in the *Broad* treatments I only consider the first choice of each block, and do not leave out the first block of choices. My rationale for doing so is to maximize the number of active choices included per subject in order to increase the power of the regressions. For each subject in the *Narrow* (*Broad*) treatments I thus obtain 18 (6) observations.

The dependent variable in these regressions is the invested amount per round. The main explanatory variables are defined as follows:

- *Low SC*: dummy taking value 1 for observations in *Low SC*
- *Broad*: dummy taking value 1 for observations in *Broad*

All the regressions include dummies for the realization paths, since 2 subjects in each treatment observed the same realizations of the lotteries over the 18 rounds[24]. Throughout specifications 2–5, I successively add the following variables:

- *female*: dummy taking value 1 for female observation
- *ln(age)*: natural logarithm of age
- *CRT7*: extended CRT score
- *BIS*: Barratt Impulsivesness Score, higher values indicate more impulsivity
- *accepted lotteries*: number of accepted lotteries in the loss aversion task, higher values indicate lower loss aversion

For *CRT7* and *BIS*, I also add interaction terms with the treatment *Low SC*, since I hypothesized that these variables might interact with the treatment. Finally, in specification 6, I add the variables reflecting the investment history suggested by Fellner and Sutter (2009) and their interaction terms with the *Low SC* dummy:

- *previous wins*: number of all previous lottery wins
- *wins last 3*: number of wins in the three previous lotteries
- *wealth*: accumulated wealth over all previous periods in ECU

The coefficients of the treatment dummies have the expected direction throughout specifications 1–5, while they usually fail to reach significance. Women invest significantly less money in the MLA task than men, which has often been found in experiments involving risky decision making (Croson and Gneezy, 2009). Furthermore, the extended CRT score is significantly positively correlated with investment levels throughout specifications 3–6, while its interaction term with *Low SC* as well as both the variable *BIS* and its interaction with *Low SC* fail to significantly predict investment levels. Finally, a higher number of accepted lotteries – an indicator of lower levels of loss aversion – is also highly positively correlated with investment levels.

Within *High SC*, only the number of wins during the last three rounds is significantly correlated with investment levels ($p < 0.01$) – more wins in the previous three rounds correlate negatively with investment levels. Fellner and Sutter (2009) find strikingly similar results for the three history variables as I do within the *High SC* treatments. However, in the *Low SC* treatments, the effect of all these variables is more pronounced and significantly different from *High SC*. The number of previous wins in the *Low SC* treatments is more strongly correlated with the investment levels compared to *High SC* ($p < 0.05$), the number of previous wins during the last three rounds is more strongly negatively correlated ($p < 0.1$) and the current wealth level is significantly more negatively correlated ($p < 0.01$) with investment levels.[25]

[24] Results are not sensitive to excluding these dummy variables.

[25] Note that coefficients of the interactions of *previous wins* and *wins last 3* with *Low SC* become marginally insignificant ($p = 0.136$ and $p = 0.132$) if I exclude the first block of three choices. All the variables indicating the lottery realization history and their interaction terms with *Low SC* become insignificant if I apply the more conservative aggregation of choices applied by Fellner and Sutter (2009). Results including significance levels are qualitatively the same as those reported here if I apply the method of Haigh and List (2005).

I report the same set of regressions separately for each investment frame in appendix A.2.7. In line with figure 5, the difference in effects of outcome history on investment levels for *Low SC* participants is exclusively driven by the behavior of subjects in the *Low SC* × *Narrow* treatment. This result supports the interpretation that the *Broad* investment frame shields participants from the negative effects of emotions on their investment decisions, which are in turn enhanced for *Low SC* participants. A *Broad* investment frame might thus be regarded as a commitment device.

5 Discussion

5.1 Low Baseline Behavioral Effects

Compared to previous experimental studies collecting similar measures (e.g. Weber and Welfens, 2007), the DE measures in experiment 1 seem quite low.[26] Similarly, in the MLA task the differences between the two investment frames are low compared to other studies (e.g. Gneezy and Potters, 1997; Haigh and List, 2005). This indicates that participants behave in a relatively rational and controlled manner. Potentially, such low baseline effects make finding an impact of the self-control manipulation more difficult.

The differences in both experiments to previous experiments might have been driven by details of the instructions or by a higher degree of sophistication in our participants. One indicator for the relevance of the latter interpretation is that average CRT scores for the original three CRT questions of the participants in experiment 1 (experiment 2) were 1.42 (1.64) which is in the close neighborhood of the mean scores for Harvard and Princeton students reported in Frederick (2005). However, the distribution of self-control scores and scores on the Barratt Impulsiveness Scale in the present samples are very close to those reported in Tangney et al. (2004) and Spinella (2007) respectively.

5.2 Null Effects on Cognitive Abilities and Risk Attitudes

It might be surprising that the *Low SC* manipulation has no effect on CRT scores in either experiment, as it did in Kocher et al. (2016). In the experiments here this cannot be the result of direct incentivization (Muraven and Slessareva, 2003). However, participants might be motivated to perform well due to intrinsic motivation or social pressure from the experimenter. Also, it is known that self-control resources replenish with rest (Tyler and Burns, 2008) and the time delay between the depletion task and taking these measures might have been enough for a recovery. Furthermore, the CRT might not be suitable to evaluate the effect of a relatively subtle self-control manipulation, because the goal of obtaining the correct response is clear and there is little scope for distraction.

Similar arguments might hold for the null effects of the self-control manipulation on the risk and loss aversion measures. Additionally, since these preference measures are derived from choice lists rather than in a spontaneous way and one by one, they might trigger a more rational response mode in subjects, even

[26] Figure 6 in the appendix compares the DE measures from experiment 1 to those in Weber and Welfens (2007) whose participants repeated the disposition effect task twice. Our DE measures seem to be very close to the DE measure in their second repetition. Expectations elicitation and waiting times were not the root of the low average DE measure: we ran two more sessions in February 2015 without these features. In these sessions, average DE is 0.0765 and cannot be distinguished from the rest of the sample (MWU, $p = 0.9511$).

Table 9: Tobit Panel Regressions of Lottery Investment

	(1)	(2)	(3)	(4)	(5)	(6)
	investment					
Low SC	-18.69	-20.65*	-13.97	-15.42	-10.41	1.146
	(13.03)	(12.28)	(21.50)	(21.43)	(20.89)	(21.42)
Broad	18.60	20.66*	16.93	18.17	19.89*	20.08*
	(13.12)	(12.38)	(12.20)	(12.15)	(11.85)	(12.03)
Broad × Low SC	22.89	22.99	27.48	25.58	19.24	20.05
	(18.59)	(17.47)	(17.19)	(17.15)	(16.80)	(17.07)
female		-48.31***	-40.08***	-39.80***	-35.05***	-36.46***
		(9.592)	(9.819)	(9.751)	(9.583)	(9.741)
ln(age)		0.157	10.13	7.456	0.405	0.278
		(30.46)	(30.04)	(29.89)	(29.18)	(29.64)
CRT7			7.577**	6.912**	5.738*	5.631*
			(3.106)	(3.119)	(3.053)	(3.101)
CRT7 × Low SC			-2.428	-1.690	-1.202	-1.227
			(4.454)	(4.475)	(4.355)	(4.423)
BIS				1.295	1.456	1.464
				(0.964)	(0.939)	(0.954)
BIS × Low SC				-1.758	-1.884	-1.864
				(1.373)	(1.337)	(1.358)
accepted lotteries					15.44***	15.38***
					(4.929)	(5.005)
previous wins						2.259
						(2.313)
previous wins × Low SC						7.038**
						(3.497)
wins last 3						-4.978***
						(1.886)
wins last 3 × Low SC						-4.745*
						(2.727)
wealth						-0.00367
						(0.00732)
wealth × Low SC						-0.0314***
						(0.0113)
Constant	44.73*	86.34	22.92	-10.49	-36.56	-32.59
	(23.53)	(99.99)	(100.4)	(102.9)	(100.6)	(102.1)
Price Path Dummies	Yes	Yes	Yes	Yes	Yes	Yes
σ_u	61.34***	57.29***	55.99***	55.57***	53.96***	54.91***
	(4.112)	(3.848)	(3.764)	(3.743)	(3.642)	(3.709)
σ_e	35.98***	35.99***	35.99***	35.99***	35.98***	35.34***
	(0.841)	(0.841)	(0.841)	(0.841)	(0.840)	(0.825)
Observations	2,286	2,286	2,286	2,286	2,286	2,286
Number of Subject	191	191	191	191	191	191

Note: Low SC is a dummy variable taking te value 1 for the low SC treatment and 0 otherwise; Broad is a dummy taking the value 1 if decisions were made in blocks of three; ln(age) is the natural logarithm of age; SCS stands for self-control score; CRT7 stands for the number of correct responses in the extended CRT; BIS stands for the score in the Barratt Impulsiveness Scale; accepted lotteries is the measure of loss aversion based on Trautmann and Vlahu (2013); Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

though their behavior in the following dynamic investment task might be driven by spontaneous urges to a larger degree. One indicator supporting this interpretation is the difference in the effect of the self-control manipulaton on the different measures of loss aversion between experiment 1 and 2: while differences in loss aversion were not detectable in the relatively more complex task in experiment 1 ($p = 0.352$), they became more pronounced and borderline significant in the arguably more spontaneous loss aversion elicitation in experiment 2 ($p = 0.103$).

5.3 The Effects of Low Self-Control on Trading Behavior

The presence of a treatment effect on trade clustering exclusively for *reflective* individuals, can be considered from two perspectives. Firstly, this is in accordance with the interpretation of Hofmann et al. (2009) that depletion affects the rational system in a dual systems perspective of decision making (see also Kahneman, 2011). Unreflective participants are unlikely to be affected by a treatment that reduces reflective thinking. Secondly, this heterogeneity highlights the possible relevance of the results, since professional participants in real world financial markets display high scores on tests such as the CRT (Thoma et al., 2015). However, note that I there are no similar heterogeneous treatment effects based on CRT in experiment 2.

The *Low SC* treatment increases the degree of history dependence of investments in the MLA task exclusively in the *Narrow* investment frame. These patterns suggest that the emotional reactivity towards past experiences is increased following reductions of self-control.[27] Additionally, these findings are in line with interpreting the *Broad* investment frame as a 'shield' against the influence of emotions on investment levels, since in this frame the outcome history variables and their interaction with the *Low SC* treatment have no significant impact on investment levels. A *Broad* investment frame might thus constitute some kind of a commitment device.

The interaction of emotions with the depletion effect might offer an explanation for the lack of a main treatment effect in both experiments reported here: self-control might play a larger role for tasks that induce a relatively higher emotional activation in participants, such as a 'social' trading environment, such as the market in (Kocher et al., 2016). Such an environment might induce stronger emotions e.g. due to feelings of competition. Similarly, it could be the case that reduced self-control makes participants more attentive to social cues in general and thus more likely to follow other persons' behavior. Thus relatively small effects on the individual level could be reinforced when traders interact leading to larger effects on aggregate. Taking one more step towards the real world, traders low in self-control might be more likely to follow social information such as rumours of a hot investment opportunity or a coming market crash than information about fundamentals.

The secondary effects reported here are in line with the more recent process view of self-control (Inzlicht and Schmeichel, 2012), which suggests that a reduction of self-control temporarily shifts both attention and motivation. The results from experiment 1 and 2 suggest that a reduction in self-control results in

[27] The current setup only allows for speculations why this might be the case: possible explanations could involve an increased tendency to display the gambler's fallacy for the more pronounced negative effect of the last three gamble wins on investment levels, and a larger role of regret for the coefficients of *previous wins* × *Low SC* and *wealth* × *Low SC*. More positive lottery realizations predict higher investment levels in the *Low SC* treatment, while higher actual positive investment experiences positively impact wealth levels and thus reduce investment levels.

a more narrow focus of participants, i.e. a more narrow attention to the facts at hand. Additionally, the enhanced history dependence of participants' investment decisions in experiment 2 indicates a stronger reliance on emotions for decision making, which is in line with the increase in reported emotional activation following ego depletion in Kocher et al. (2016). These effects can also be related to a dual-systems perspective of self-control and decision making (Hofmann et al., 2009). The fact that in experiment 1 only subjects with high cognitive abilities were impaired by the self-control manipulation and the enhanced history dependence in experiment 2 suggest a shift from rational processing to emotions in decision making (cf. Kahneman, 2011).

Taking a bird's eye view, one might hypothesize that the effect of lowered self-control might be stronger when experimental instructions are less clear, when impulsivity and emotionality within a task are more important, or when market participants interact.

6 Conclusion

Even though the effects on individual investment decisions seem to be relatively small in the present paper, the results in Kocher et al. (2016) suggest that such effects can be reinforced when traders interact resulting in larger effects on the market level. Besides the possible relevance of self-control on real world markets, the effects of the self-control manipulation can be a factor contributing to the heterogeneity often found in experiments on financial decision making due to previous cognitive engagements or self-control demands of experimental subjects. Similarly, dispositional differences in self-control between participants can contribute to the heterogeneity commonly found, even though they had no explanatory power in the present research.

The findings reported here and in Kocher et al. (2016) might indicate the relevance of self-control state for real world financial markets. In experiment 1, in particular those participants who are similar to financial traders in terms of CRT scores (Thoma et al., 2015) were the ones whose trade clustering was negatively affected by the self-control manipulation. It seems easier to argue that participants with low trait self-control or low CRT scores might be pushed out of the market over time, but then potentially the behavior of the remaining market participants might be the most sensitive to temporary fluctuations in self-control. Thus, state self-control might be particularly important for explaining real world financial market behavior. Furthermore, the presence of (temporary) self-control problems might also suggest reasons for the existence of commitment devices such as automatic selling devices (Shefrin and Statman, 1985; Fischbacher et al., 2015), internal rules of trading and traders' supervision (Fenton-O'Creevy et al., 2011) in financial markets.

The present paper opens up a number of directions for future research. Looking into explanations for the increase in history dependence in investments uncovered in experiment 2 might be a fruitful path. Further interesting research questions are how the effect of reduced self-control impacts the processing of social information, how enhancing self-control impacts financial decision making, what commitment devices can alleviate the negative effects of ego depletion on financial markets and how temporary reductions in self-control might be identified in real world stock market data.

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A Appendix

A.1 Appendix for Experiment 1

A.1.1 Additional Measures

Here, I explain the additional measures collected in experiment 1 in more detail and also outline the rationale for including them. After the end of part 2, participants first had to answer the three CRT questions, which have been shown to be correlated with risk and time preferences (Frederick, 2005) and to be predictive of trading performance in experimental asset markets (Corgnet et al., 2014; Noussair et al., 2014; Kocher et al., 2016). Since subjects have been shown to overcome the effect of lowered self-control if the monetary incentives are high enough (Muraven and Slessareva, 2003) and Kocher et al. (2016) found no effect of an ego depleting task on incentivized CRT performance, subjects received no incentivization for giving correct responses.

Secondly, participants received two choice lists to elicit risk preferences and loss attitude adapted from Tanaka et al. (2010). In order to save time and to reduce complexity, we abstracted from obtaining measures of probability weighting. All lottery choices thus only involved 50:50 lotteries. To identify loss and risk aversion, we therefore only required two screens rather than the three sheets in Tanaka et al. (2010). The experimental task consisted of 18 binary lottery choices on two separate screens. On the first screen, each choice was made between two lotteries with positive payoffs, while lotteries on the second screen involved both gains and losses. This design enables us to assign parameters of prospect theory utility according to a participant's switching points (cf. Tversky and Kahneman, 1992) or we can use "raw" switches as proxies for risk preferences and loss attitude. In the following analysis, I will use the latter method.[28] The 11 choices on the first screen involve only gains. The first lottery option (option A) is the same across choices on the first screen. Option B has a lower payoff in the adverse state than option A, while we increase its higher payoff from row to row thus making it increasingly attractive. An individual's switching point in these 11 choices identifies risk aversion with later switches implying higher degrees of risk aversion. The 7 choices on the second screen involve only mixed lotteries, with either option A deteriorating from row to row or option B improving from row to row, while option B always involved a higher possible loss than option A. Later switches to option B on the second screen given a certain switch on the first screen imply higher degrees of loss aversion. Participants were allowed at most one switch from option A to option B on each screen, but were allowed to choose option A or option B throughout all choices. After the experiment, one of the eighteen choice situations was randomly selected for payout and the outcome of the lottery selected by the participant was randomly determined. Participants learned about their lottery outcome at the end of the experiment.

After the elicitation of risk and loss attitudes, participants answered five financial literacy questions adapted from Van Rooij et al. (2011)[29] receiving 0.20 € for each correct response. The reason for including this measure was on the one hand to see whether financial literacy or financial sophistication, which have

[28]The results are not sensitive to using switching points or inferred parameters as measures of risk preferences and loss attitudes. Details of how to assign preference parameters based on switches on the two screens can be found in appendix A.1.2.

[29]The exact wording of the questions can be found in appendix A.1.3

been found to be predictive of the tendency to display the disposition effect in real world stock markets (e.g. Feng and Seasholes, 2005), also predicts the size of the disposition effect in our laboratory task and on the other hand to reduce noise by controlling for it in regressions.

Finally, participants replied to the 13 items of the brief self-control scale on a 7-point Likert scale (Tangney et al., 2004) before answering a number of socio-economic questions. The rationale for including the self-control scale was twofold: On the one hand, we wanted to evaluate whether it had a similar impact on the outcome measure as our self-control manipulation as previously shown in e.g. Schmeichel and Zell (2007). On the other hand, we wanted to include it as a possible explanatory variable for heterogeneity in the treatment effect, since treatment effects have been shown to depend on the underlying tendency of a participant to control herself or fall prey to impulses (Hofmann et al., 2009).

A.1.2 Assigning Preference Parameters from Lottery Choices

All lottery choices only involved 50:50 lotteries. To identify loss and risk aversion, we hence require two decision sheets (rather than three as in Tanaka et al. (2010)). In determining individuals' utility parameters we assume a prospect theory value function of the form

$$v(x) = \left\{ \begin{array}{cc} x^{\sigma} & \text{for } x \geq 0 \\ -\lambda(-x)^{\sigma} & \text{for } x < 0 \end{array} \right\}$$

where σ is the concavity parameter measuring the degree of risk aversion and λ denotes the loss aversion parameter, i.e. the kink of the value function at payoffs of 0. To determine prospect theory values of a lottery, each payoff x is inserted into the value function $v(x)$ and then weighted by the objective probability of 0.5. The 11 choices on the first screen involve only gains. The first lottery option (option A) is the same across choices on the first screen, while we vary increase the high payoff in the second lottery across choices. The individual's switching point in these 11 choices identifies the risk aversion parameter σ. Later switches to the second lottery imply lower σ, i.e. higher risk aversion. The 7 choices on the second screen involve only mixed lotteries, with either the first lottery (option A) deteriorating from row to row or the second lottery (option B) improving from row to row. These 7 choices enable us to assign a loss aversion parameter λ to each participant, given her choices over the 11 gains lotteries. Later switches to option B on the second screen imply higher loss aversion λ.

A.1.3 Financial Literacy Questions

1. Suppose you had \$100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?

    (a) More than \$102 (correct)

    (b) Exactly \$102

    (c) Less than \$102

    (d) I don't knoww.

2. Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money int his account?
   (a) More than today
   (b) Exactly the same
   (c) Less than today (correct)
   (d) I don't know
3. Do you think that the following statement is true or false? "Buying a single company stock usually provides a safer return than a stock mutual fund."
   (a) False (correct)
   (b) True
   (c) I don't know
4. Which of the following statements describes the main function of the stock market?
   (a) The stock market helps to predict stock earnings
   (b) The stock market results in an increase in the price of stocks
   (c) The stock market brings people who want to buy stocks together with those who want to sell stocks (correct)
   (d) None of the above
   (e) I don't know
5. Which of the following statements is correct?
   (a) Once one invests in a mutual fund, one cannot withdraw the money in the first year
   (b) Mutual funds can invest in several assets, for exampe invest in both stocks and bonds (correct)
   (c) Mutual funds pay a guaranteed rate of return which depends on their past performance
   (d) None of the above
   (e) I don't know

Note: Questions 1, 2, 3, 4 and 5 correspond to questions 1, 2, 15, 7 and 8 from Van Rooij et al. (2011) respectively.

A.1.4 Effect on Additional Measures

Here I check the effect of the self-control manipulation on the additional measures that were collected after the disposition effect task. Table 10 reports mean CRT scores, switching points from the two choice lists and the associated parameters of risk attitude σ and loss attitude λ by treatment condition. The last column displays p-values from MWU-tests testing equality of medians between the two groups.

First of all, CRT scores are not affected by the self-control manipulation (MWU, $p = 0.485$), which is in line with the results in Kocher et al. (2016) who also found no direct effect of their self-control manipulation on the CRT score. Note however, that unlike in Kocher et al. (2016), in this experiment correct responses on the CRT were not incentivized, as incentivizing effort can make people overcome the negative effects of ego depletion (Muraven and Slessareva, 2003). Therefore, this result can be taken as evidence that incentivization was not the reason for the null effect on the CRT scores in Kocher et al. (2016).

Secondly, again in line with Kocher et al. (2016) and in line with the mixed results in the psychology literature (Bruyneel et al., 2009; Unger and Stahlberg, 2011), I do not find an effect of the self-control manipulation on risk aversion (MWU, $p = 0.616$ and $p = 0.834$ for switches in the gains list and the inferred σ parameter respectively). I also find no significant effect of the self-control manipulation on the two measures of loss aversion (MWU, $p = 0.352$ and $p = 0.569$ for switches in the mixed list and the inferred λ parameter respectively).

Thus, the manipulation seems to have had no impact on the background measures collected in this study.

Table 10: Effect of Treatment on Background Measures

	High SC	Low SC	p-value
CRT score	1.493	1.366	0.485
switch gains	5.845	5.620	0.616
σ	0.613	0.629	0.834
switch mixed	3.563	3.296	0.352
λ	2.949	2.797	0.569

Note: p-values from two-sided Mann-Whitney U-tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.1.5 Additional Tables and Figures

Table 11: Starting Times of the Sessions of Experiment 1

Session	**Date**	**Weekday**	**Time**	**Treatments**
1	December 12, 2014	Friday	4:30 PM	*High SC*, *Low SC*
2	January 9, 2015	Friday	10:00 AM	*High SC*, *Low SC*
3	January 9, 2015	Friday	12:30 PM	*High SC*, *Low SC*
4	January 9, 2015	Friday	3:00 PM	*High SC*, *Low SC*
5	January 12, 2015	Monday	1:00 PM	*High SC*, *Low SC*
6	January 12, 2015	Monday	3:00 PM	*High SC*, *Low SC*
7	February 4, 2015	Wednesday	3:00 PM	*Low SC*×*No Exp. No Wait*
8	February 4, 2015	Wednesday	4:30 PM	*Low SC*×*No Exp. No Wait*

Table 12: Manipulation Checks for Experiment 1

Task Performance	High SC	Low SC	p-value
correct letters	149.085	138.817	0.000***
Questions asked directly after letter-e-task			
strain	2.690	3.704	0.000***
difficult	1.268	2.958	0.000***
tired immediate	3.803	4.127	0.208
frustrated	1.662	2.690	0.000***
Questions asked in the questionnaire			
general mood	3.507	3.394	0.360
experiment mood	3.310	3.282	0.746
tired end	3.493	3.479	0.851
effort end	2.986	3.225	0.271

Note: Variables "strain"-"frustrated" were answered by participants on a 7 point Likert scale "general mood" and "experiment mood" on a 5 point Likert scale and "tired end" and "effort end" on a 6 point Likert scale; p-values from two-sided Mann-Whitney U-tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 13: Randomization Checks for Experiment 1

	High SC	Low SC	p-value
age	23.648	23.521	0.724
known CRT	1.070	0.718	0.116
risk	3.845	3.676	0.460
math	2.338	2.662	0.170
financial knowledge	4.085	4.070	0.987
stock experience	5.056	4.761	0.219
self-control score	53.099	55.676	0.184
FLQ score	3.451	3.408	0.965
study subject	3.761	4.099	0.625
female	0.634	0.521	0.174

Note: p-values from two-sided Mann-Whitney U-tests; for study and female p-values from Chi2 tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

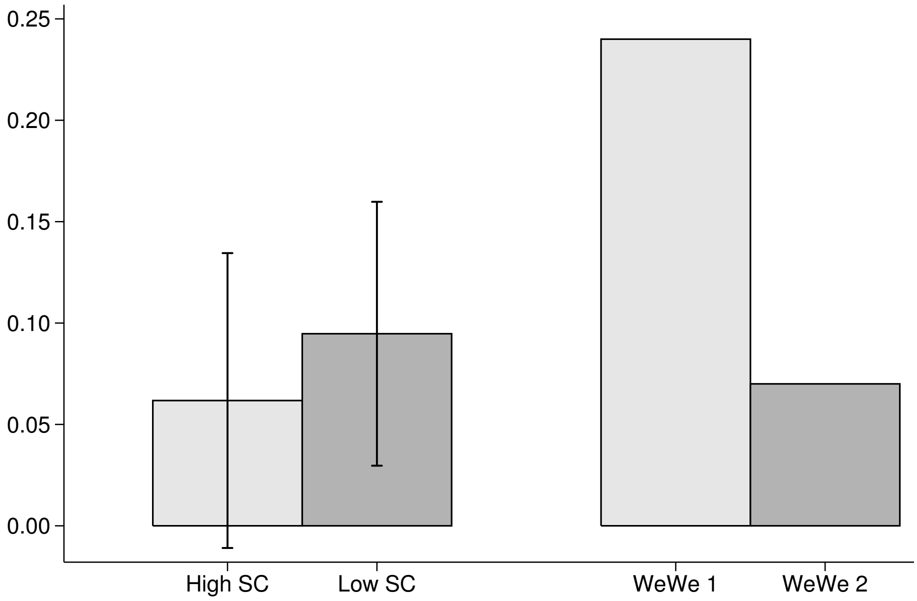


Figure 6: Disposition Effect Measure by Treatment and Comparison with Weber and Welfens (2007)

Table 14: Regressions of Disposition Effect Measure

	(1)	(2)	(3)	(4)	(5)	(6)
				DE		
Low SC	0.0308	0.0378	0.0132	0.272	0.275	0.288
	(0.0455)	(0.0450)	(0.0887)	(0.318)	(0.320)	(0.326)
female		0.0847	0.0683	0.0646	0.0555	0.0568
		(0.0648)	(0.0676)	(0.0686)	(0.0710)	(0.0723)
ln(age)		0.315	0.281	0.301	0.290	0.275
		(0.204)	(0.215)	(0.219)	(0.221)	(0.231)
CRT			-0.0360	-0.0335	-0.0282	-0.0324
			(0.0421)	(0.0428)	(0.0441)	(0.0465)
CRT × Low SC			0.0123	0.00810	0.00458	0.00594
			(0.0538)	(0.0549)	(0.0556)	(0.0579)
Self-Control Score (SCS)				0.00189	0.00230	0.00234
				(0.00394)	(0.00404)	(0.00414)
SCS × Low SC				-0.00464	-0.00467	-0.00490
				(0.00549)	(0.00552)	(0.00564)
FLQ score					-0.0117	-0.0113
					(0.0217)	(0.0223)
switch LA						0.0106
						(0.0255)
switch RA						-0.00233
						(0.0122)
Constant	0.0186	-1.020	-0.890	-1.023	-1.002	-0.956
	(0.189)	(0.643)	(0.672)	(0.709)	(0.714)	(0.733)
Price Path Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	139	139	139	139	139	139
R-squared	0.588	0.614	0.620	0.624	0.626	0.627

Note: Missing observations due to missing *DE* variable, standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 15: Heterogeneity of the Effect of Self-Control Manipulation on the Disposition Effect by Cognitive (Ir)Reflection

	High SC	Low SC	p-value
impulsive	0.017	0.099	0.445
residual	0.104	0.132	0.776
reflective	0.061	0.076	0.994

Note: Impulsive individuals had at least 2 impulsively wrong responses, reflective individuals had at least 2 correct responses; p-values from two-sided Mann-Whitney U-tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 16: Heterogeneity of the Effect of Self-Control Manipulation on the Disposition Effect by Self-Control Score Tercile

	High SC	Low SC	p-value
low score	0.040	0.117	0.481
medium score	0.076	0.157	0.248
high score	0.074	0.038	0.485

Note: p-values from two-sided Mann-Whitney U-tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.1.6 Optimal Trade Clustering

Since a risk neutral Bayesian investor would always shift all her wealth to the asset with the highest price, if she executes a trade, she should have one distinct trade in the first trading period (when she buys the most expensive asset) and then always exactly two trades (when she shifts her assets to the most expensive assets). Therefore the optimal TC for each price path can be calculated as follows:

$$TC^* = 1 - \frac{\#\text{ of changes at the top} + 1}{\#\text{ changes at the top} \times 2 + 1} \tag{5}$$

Therefore, I can introduce a measure how far the actual trade clustering differed from the optimal level of trade clustering.

$$|\Delta TC| = |TC - TC^*| \tag{6}$$

It turns out that treatment effects between *Low SC* and *High SC* disappear when looking at $|\Delta TC|$, details are available upon request.

Since risk neutrality seems to be an extreme assumption in the data, the theoretically optimal level for a risk-neutral agent will be a lower bound for the optimal level TC of a risk-averse agent. Modelling a risk-averse agent would be necessary, since the results from part 3 of the experiment suggest that there is a high degree of heterogeneity in risk attitudes, but makes the analysis untractable. Therefore, I refrain from going further in this direction and only compare the actual TC parameter in the data.

A.2 Appendix for Experiment 2

A.2.1 Additional Measures

Following the computation questions, participants fill out the loss aversion task from Trautmann and Vlahu (2013) which is based on the simple task in Fehr and Goette (2007). The task consists of 6 binary choices that are displayed on one screen. Subjects have to accept or reject one binary lottery in each row. In each lottery, there is a 50% chance of winning 4.50 € while there is a possible loss that increases from row to row by 1.00 €, starting at 0.50 € in the first row and finishing at a potential loss of 5.50 € in the last row. Thus the expected value from accepting the lottery drops from row to row by 0.50 € and becomes negative in the last row. For each participant, one of these rows is randomly selected at the end of the experiment and if the participant has accepted the respective lottery, the lottery outcome is simulated. Subjects learn about the outcome of this task at the end of the experiment. The number of accepted lotteries in this task can serve as a proxy for loss aversion (Fehr and Goette, 2007; Trautmann and Vlahu, 2013). I decided to use this task rather than the longer parametric task used in experiment 1 for two reasons: first, I wanted to reduce the time participants spend on this task to avoid waiting times and thus rest. Second, I wanted to reduce the complexity of the task measuring loss aversion.

Directly following the loss aversion task, participants are given the original CRT questions (Frederick, 2005) plus the four CRT extension questions from Toplak et al. (2014). Participants receive a flat payment of 2.50 € for answering these questions.[30] I decided to include the extended version of the CRT for two reasons: first of all, once a participant has seen the CRT questions they might have learned the answers. 'Thinking fast and slow' by Daniel Kahneman (Kahneman, 2011) has by now spread knowledge of the original CRT questions to a wide audience. Thus, some the CRT questions might be known to many participants, which might make finding a treatment effect in these questions harder due to added noise. Secondly, the extended CRT offers a finer-grained measure of cognitive abilities, making it easier to detect a possible treatment effect.

Before learning about their payouts, participants filled out the abbreviated version of the Barratt Impulsiveness Scale (BIS) (Spinella, 2007; Stanford et al., 2009), which according to Hilgers and Wibral (2014) can account for heterogeneity in the impact of the MLA framing. Finally, participants filled out a number of socio-economic background measures.

A.2.2 Computation Questions

1. Imagine you throw two fair-sided coins. You win 2.00 € every time you throw heads, while you lose 1.00 € every time you throw tails. How many € do you expect to win? (Correct: 1.00 €)

2. If you repeat the game above 8 times, on average how often will you win money? (Correct: 6 times)

3. If you throw three fair dice at once and you win 5.00 € for every 5 or 6 you roll, while you lose 2.00 € for any other number, how much money will you win on average? (Correct: 1.00 €)

4. If you repeat the game above 27 times, in how many games will you lose money? (Correct: 8 times)

[30] Refer to the design of experiment 1 for the reasons why these questions were not incentivized.

A.2.3 Effect on Additional Measures

Table 17 displays the means of the background measures for *High SC* and *Low SC* participants and p-values from MWU tests comparing these two treatment dimensions. From these tests it is obvious that the self-control manipulation had no effect on the two subsets of the seven CRT questions that were included in the study and neither on the four computation questions that were asked directly following the MLA task. The results for the number of accepted lotteries indicate a slight increase in loss aversion in the *Low SC* participants who accepted about 0.2 lotteries less on average. However, this effect marginally fails to reach significance (MWU, $p = 0.103$).

These tests replicate the results from experiment 1 and from Kocher et al. (2016) that the self-control reducing treatment does not impact cognitive abilities significantly.

Table 17: Effect of Ego Depletion on CRT scores, Computation Scores and Accepted Lotteries

	High SC	Low SC	p-value
CRT7	4.021	4.147	0.702
CRT	1.615	1.674	0.711
CRT4	2.406	2.474	0.783
computation score	1.156	1.337	0.335
lotteries accepted	2.854	2.653	0.103

Note: CRT7 scores encompass the original CRT scores plus the extension, CRT4 scores refers to just the 4 extension questions; p-values from two-sided Mann-Whitney U-tests comparing columns; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.2.4 Additional Tables and Figures

Table 18: Starting Times of the Sessions of Experiment 2

Session	**Date**	**Weekday**	**Time**	**Treatment**
1	July 21, 2015	Tuesday	10:00 AM	*High SC×Broad*
2	July 21, 2015	Tuesday	12:00 PM	*High SC×Narrow*
3	July 28, 2015	Tuesday	9:30 AM	*Low SC×Narrow*
4	July 28, 2015	Tuesday	11:30 AM	*High SC×Narrow*
5	July 28, 2015	Tuesday	1:00 PM	*Low SC×Broad*
6	July 28, 2015	Tuesday	3:00 PM	*Low SC×Narrow*
7	July 29, 2015	Wednesday	2:30 PM	*Low SC×Broad*
8	July 29, 2015	Wednesday	4:00 PM	*High SC×Broad*

Table 19: Manipulation Checks for Experiment 2

Task Performance	High SC	Low SC	p-value
correct letters	148.042	141.579	0.000***
Questions asked directly after letter-e-task			
strain	2.542	3.758	0.000***
difficult	1.375	2.832	0.000***
tired immediate	3.917	3.989	0.737
frustrated	1.917	2.221	0.011**
Questions asked in the questionnaire			
general mood	3.344	3.400	0.867
experiment mood	3.406	3.516	0.442
tired end	3.156	3.000	0.484
effort end	2.646	2.779	0.369

Note: p-values from two-sided Mann-Whitney U-tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 20: Randomization Checks for Experiment 2

	D=0,B=0	D=0,B=1	D=1,B=0	D=1,B=1	p-value
age	23.688	24.625	24.957	25.063	0.327
known CRT	1.604	1.458	1.532	1.188	0.425
known CRT4	0.146	0.438	0.213	0.125	0.744
risk	3.542	3.521	3.660	3.479	0.912
math	2.417	2.188	2.255	2.188	0.875
financial knowledge	3.313	3.563	3.511	3.458	0.805
stock exp	4.583	5.042	4.957	4.646	0.375
BIS	32.146	31.500	31.277	29.854	0.348
BIS extend	34.500	33.438	33.660	32.000	0.346
income relative	1.813	2.146	1.979	1.896	0.404
math	2.417	2.188	2.255	2.188	0.875
meal	3.542	3.104	2.872	2.604	0.045**
money available	1.188	0.771	0.809	0.750	0.346
working hours	9.760	13.172	9.809	9.490	0.585
study subject	4.083	4.521	4.702	3.833	0.452
female	0.604	0.646	0.574	0.604	0.916
native german	0.729	0.771	0.723	0.792	0.841
making ends meet	0.542	0.583	0.787	0.833	0.382
money origin1	1.250	0.771	0.766	0.896	0.336
money origin2	0.542	1.083	0.532	0.792	0.192
previous depletion experiment	0.563	0.583	0.468	0.479	0.585
previous MLA experiment	0.750	0.604	0.681	0.813	0.130

Note: each column represents one cell of the 2×2 design, where $D = 0$ and $D = 1$ stand for High SC and Low SC and $B = 0$ and $B = 1$ for narrow and broad frames respectively; p-values until working hours from two-sided Kruskall-Wallis tests, from study subject to previous MLA experiment from Chi2 tests comparing all columns to each other; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.2.5 Heterogeneity – BIS

Table 21: Average Investment over all Periods by Treatments for Participants with below median BIS

	Narrow		*Broad*		
	mean	N	mean	N	p-value (CvC)
High SC	44.058	23	42.679	27	0.977
Low SC	41.364	25	53.250	30	0.148
p-value (RvR)	0.664		0.139		

Note: p-values from two-sided Mann-Whitney U-tests; RvR stands for tests comparing rows i.e. depletion effect within frame, CvC stands for tests comparing columns i.e. comparing framing effect withing self-control manipulation

*** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$

Table 22: Average Investment over All Periods by Treatments for Participants with above median BIS

	Narrow		*Broad*		
	mean	N	mean	N	p-value (CvC)
High SC	37.169	25	58.595	21	0.038*
Low SC	29.076	22	52.731	18	0.028**
p-value (RvR)	0.417		0.701		

Note: p-values from two-sided Mann-Whitney U-tests; RvR stands for tests comparing rows i.e. depletion effect within frame, CvC stands for tests comparing columns i.e. comparing framing effect withing self-control manipulation

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.2.6 Heterogeneity – CRT

Table 23: Average Investment by Treatment for low CRT Participants

	Narrow		*Broad*		
	mean	N	mean	N	p-value (CvC)
High SC	32.273	22	40.635	16	0.359
Low SC	32.577	18	50.921	19	0.055*
p-value (RvR)	0.913		0.289		

Note: p-values from two-sided Mann-Whitney U-tests; RvR stands for tests comparing rows i.e. depletion effect within frame, CvC stands for tests comparing columns i.e. comparing framing effect withing self-control manipulation

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 24: Average Investment by Treatment for medium CRT Participants

	Narrow		*Broad*		
	mean	N	mean	N	p-value (CvC)
High SC	27.419	11	51.696	17	0.109
Low SC	24.348	11	42.853	17	0.043**
p-value (RvR)	0.429		0.641		

Note: p-values from two-sided Mann-Whitney U-tests; RvR stands for tests comparing rows i.e. depletion effect within frame, CvC stands for tests comparing columns i.e. comparing framing effect withing self-control manipulation

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 25: Average Investment by Treatment for high CRT Participants

	Narrow		*Broad*		
	mean	N	mean	N	p-value (CvC)
High SC	62.063	15	56.922	15	0.882
Low SC	45.531	18	70.889	12	0.050**
p-value (RvR)	0.226		0.125		

Note: p-values from two-sided Mann-Whitney U-tests; RvR stands for tests comparing rows i.e. depletion effect within frame, CvC stands for tests comparing columns i.e. comparing framing effect withing self-control manipulation

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.2.7 Tobit Regressions by Frame

Table 26: Tobit Panel Regressions of Lottery Investment in the Narrow Frame

	(1)	(2)	(3)	(4)	(5)	(6)
			investment			
Low SC	-21.98	-21.81	-34.47	-32.52	-26.57	-12.21
	(14.43)	(13.65)	(31.54)	(30.31)	(29.28)	(29.89)
female		-58.66***	-46.68***	-40.21**	-37.38**	-41.63**
		(16.16)	(17.33)	(16.89)	(16.33)	(16.59)
ln(age)		-67.58	-62.65	-48.38	-57.71	-60.85
		(50.83)	(52.33)	(51.14)	(49.45)	(50.16)
CRT7			6.051	6.661	3.856	3.042
			(5.611)	(5.413)	(5.319)	(5.392)
CRT7 $\times$ Low SC			2.446	2.571	3.419	4.462
			(7.318)	(7.052)	(6.805)	(6.901)
BIS				3.042*	3.714**	3.653**
				(1.587)	(1.555)	(1.576)
BIS $\times$ Low SC				-6.015***	-6.202***	-6.313***
				(2.274)	(2.195)	(2.226)
accepted lotteries					18.16**	17.97**
					(7.085)	(7.178)
previous wins						3.350
						(2.893)
previous wins $\times$ Low SC						10.20**
						(4.401)
wins last 3						-7.185***
						(2.432)
wins last 3 $\times$ Low SC						-7.156**
						(3.564)
wealth						-0.00931
						(0.00923)
wealth $\times$ Low SC						-0.0476***
						(0.0144)
Constant	60.35*	320.8*	271.8	125.5	89.20	115.2
	(34.95)	(166.4)	(176.2)	(178.6)	(172.6)	(175.1)
Price Path Dummies	Yes	Yes	Yes	Yes	Yes	Yes
σ_u	67.57***	62.80***	61.22***	58.69***	56.43***	57.33***
	(6.312)	(5.855)	(5.718)	(5.497)	(5.295)	(5.370)
σ_e	39.93***	39.95***	39.94***	39.93***	39.92***	38.50***
	(1.130)	(1.130)	(1.130)	(1.129)	(1.129)	(1.086)
Observations	1,710	1,710	1,710	1,710	1,710	1,710
Number of Subject	95	95	95	95	95	95

Note: Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 27: Tobit Panel Regressions of Lottery Investment in the Broad Frame

	(1)	(2)	(3)	(4)	(5)	(6)
			investment			
Low SC	3.859	1.466	8.653	2.536	0.639	1.504
	(9.482)	(8.718)	(23.32)	(23.62)	(23.14)	(23.62)
female		-42.70***	-39.55***	-39.92***	-35.37***	-35.87***
		(11.13)	(11.73)	(11.63)	(11.56)	(11.63)
ln(age)		47.40	56.75*	66.70*	52.10	54.87
		(33.95)	(33.74)	(34.47)	(34.33)	(34.59)
CRT7			6.218*	5.677*	5.911*	5.828*
			(3.332)	(3.431)	(3.360)	(3.384)
CRT7 × Low SC			-1.531	0.541	1.123	1.120
			(5.394)	(5.543)	(5.438)	(5.477)
BIS				0.349	0.0167	0.0494
				(1.052)	(1.043)	(1.050)
BIS × Low SC				1.237	1.380	1.376
				(1.517)	(1.491)	(1.501)
accepted lotteries					14.27**	14.12**
					(6.519)	(6.556)
previous wins						1.611
						(3.444)
previous wins × Low SC						7.240
						(5.474)
wins last 3						-1.211
						(2.603)
wins last 3 × Low SC						-2.839
						(3.713)
wealth						0.00378
						(0.0107)
wealth × Low SC						-0.0196
						(0.0169)
Constant	48.45**	-59.60	-116.2	-154.0	-141.8	-157.8
	(23.12)	(111.0)	(112.1)	(114.3)	(112.1)	(113.1)
Price Path Dummies	Yes	Yes	Yes	Yes	Yes	Yes
σ_u	43.86***	39.82***	38.81***	38.37***	37.44***	37.81***
	(4.192)	(3.851)	(3.758)	(3.715)	(3.630)	(3.656)
σ_e	26.07***	26.06***	26.06***	26.07***	26.08***	25.30***
	(1.084)	(1.084)	(1.084)	(1.085)	(1.085)	(1.054)
Observations	576	576	576	576	576	576
Number of Subject	96	96	96	96	96	96

Note: Standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.3 Instructions

A.3.1 Disposition Effect: Instructions (translated from German)

Note that the additional measures from this experiment correspond to parts III and IV in the instructions.

Welcome to the experiment and thank you for your participation!

Please do not talk to other participants of the experiment from now on

General information on the procedure

This experiment is conducted to investigate economic decision making. You can earn money during the experiment. This will be paid to you privately and in cash right after the experiment. Your financial payoff depends on the decisions you make. Therefore, it is very important to read the instructions carefully.

The whole experiment takes about 90 minutes and consists of 4 parts. At the beginning you receive the instructions in detail for all parts of the experiment, which we will read aloud in two blocks. If you have further questions after the instructions are read or during the experiment, please raise your hand. One of the experimenters will come to you and answer your questions in private. Due to linguistic simplicity, we only use male names.

You will make multiple decisions. While you make your decisions a clock is running down on the right top of the screen. This is for your orientation on how much time you should take for your decisions. Mostly you can exceed this countdown, if you need more time for your decisions. However, in some parts you will be limited to act within the time limit (We will point that out before). Information screens, where you do not need to make any decision will be faded out after the countdown.

Payment

In one part of the experiment we do not talk about Euros but points. At the end of the experiment they will be converted to Euros. The exchange rate will be announced at the beginning of this part.

For your arriving on schedule and the answering of the questionnaires you get 4 Euros extra on top of your income that you can earn during the experiment.

Video recording

During the experiment we will record you with the camera at your screen.

Anonymity

None of the other participants get any information on how much you have earned in the experiment. We never connect names with data from the experiments. Even the data from the video recording will be made anonymous and be only used in aggregation. At the end of the experiment you need to sign a receipt about your earnings, which is only used for our accounting and does not allow any conclusions about your decisions.

Devices

At your place you will find a pen. Please leave it on the table after the experiment.

Part I

Task

The first part of this experiment is a word recognition task. You will see 1 word in black letters for 3 seconds on the screen.

Within these 3 seconds, you need to classify the shown word into 2 categories according to a certain rule. The exact rule will be shown to you on the screen at the beginning. Please take your time to read the rule carefully, as it will not be displayed again later.

If the displayed word satisfies the rule, you can classify the word into the first category by pressing the "e" key on your keyboard. If the displayed word does not satisfy the rule, do not press any key. Hence, it will be classified into the second category. After 3 seconds the next word will be displayed automatically.

For your classification you have 3 seconds. As soon as the time runs out a new word will be displayed. Even if you have chosen a categorization right at the beginning of the period, e.g. after 1 second by pressing the "e" key, you must wait for the next word until the period time is expired.

Please note, that your entry is ultimate. If you press the "e" key by mistake, you cannot correct your decision. Therefore, a focused approach is essential.

After all 150 words have been displayed the first part will end automatically and the second part starts.

Trial phase

Before the task begins, there will be displayed 3 words on trial, which you need to categorize. In the trial phase there is no time limit of 3 seconds. You have an extended time limit of 10 seconds.

If you choose the wrong categorization in the trial periods, the computer shows you the right solution with an appropriate justification. Please note that you need to wait the 10 seconds either way. After a wrong answer you get a feedback or the next test word will be displayed. After you have read your feedback (if any), please click on "OK" to proceed to the next test word. Please note that you will not get any feedback on your answers during the actual tasks.

After the trial phase you have one last chance to ask questions about this task. If you have a question, please raise your hand.

Payoff

For your concentrated machining in part I you get 3 Euros.

Part II

Payoff

In the second part of the experiment we do not talk about Euros but of points. At the end of the experiment they will be converted into Euros. The exchange rate is:

200 points =1.00 €

2 points =0.01 €= 1 cent

General description

The second part of the experiment is a replica of a goods market. In this part you can buy and sell 6 different goods: Good 1 to good 6. The game consists of 18 periods (periods -3 to 14). During the first 3 periods (-3 to -1) you cannot trade goods, i.e. buy or sell, but only observe the price development of all 6 goods. In the last period (14) you also cannot trade, as all remaining goods will be sold automatically at the end of this period.

In period -3 you get an endowment of 2,000 points, but no goods. With these points you can buy goods in periods 0 to 13. Furthermore you can sell goods from your possession.

Determining the prices of goods

In the starting period (-3) all 6 goods have the same price of 100 points. The price of every good will change in the following period: Either the price increases 6% or it drops down 5%. Hence, the price never stays constant from one period to the following.

Every of the 6 goods has his own probability for a price increase or price decrease. The probabilities are constant for every good and for all 18 periods. The probability for a price increase is for one of the goods 35% (−−), for one good 45% (−), for two goods 50% (0), for one good 55% (+) and for one good 65% (++). With the counter probability the prices of the goods decrease, for example, the price of good "−−" decreases with probability 65%.

Here you can see an overview of the distribution of probabilities for a price increase:

Description	–	-	0	+	++
Probability of a price increase	35%	45%	50%	55%	65%
Number	1	1	2	1	1

You will not know what good has what probability for a price increase, but you may find this out with the price history. The computer determines before the start of period -3, which good is associated with what probability randomly.

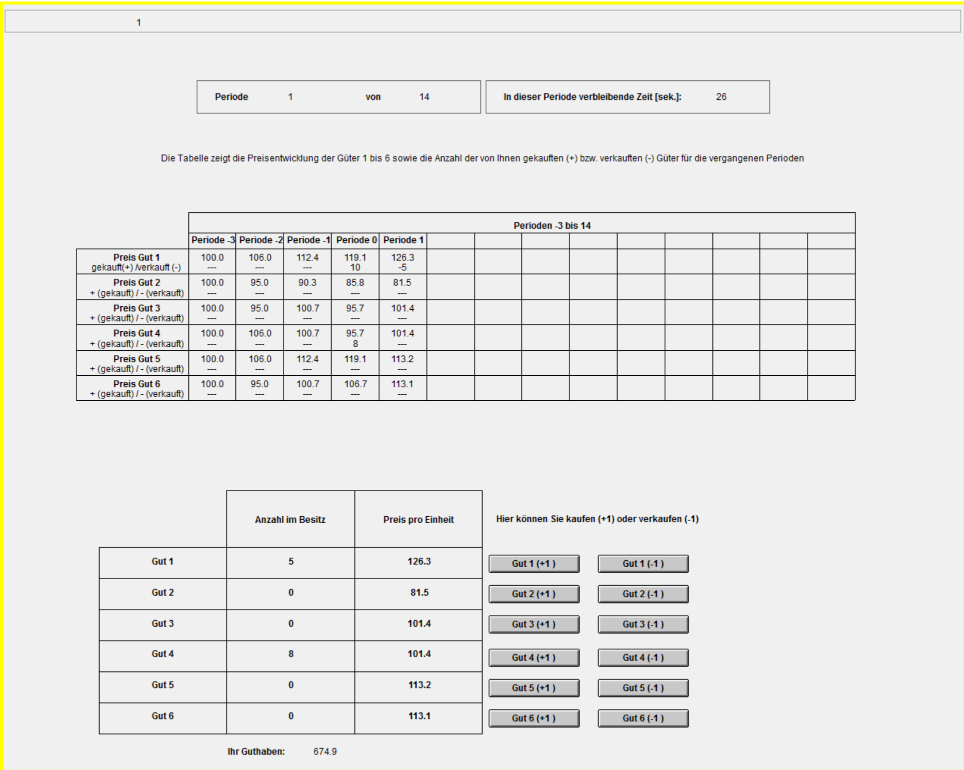
Price changes in one period are independent from price changes in other periods, i.e. the probability of a price increase of a certain good stays constant within all periods. In addition the price changes between two different goods are independent. Furthermore neither you nor other participants can influence the development of prices by your actions.

Period

The first three periods (-3 to -1) take 20 seconds each, as you cannot trade, but you can observe the prices of the goods. A regular trading period (periods 0 to 13) takes 40 seconds. If there is an allocation of goods in a period (more on this in the section after the next), you get 90 seconds additionally. After the countdown you will succeed into the next period automatically.

Trading periods

During a trading period your screen will look like this:



1

Periode 1 von 14

In dieser Periode verbleibende Zeit [sek.]: 26

Die Tabelle zeigt die Preisentwicklung der Güter 1 bis 6 sowie die Anzahl der von Ihnen gekauften (+) bzw. verkauften (-) Güter für die vergangenen Perioden

	Perioden -3 bis 14													
	Periode -3	Periode -2	Periode -1	Periode 0	Periode 1									
Preis Gut 1 gekauft(+) /verkauft (-)	100.0 ---	106.0 ---	112.4 ---	119.1 10	126.3 -5									
Preis Gut 2 + (gekauft) / - (verkauft)	100.0 ---	95.0 ---	90.3 ---	85.8 ---	81.5 ---									
Preis Gut 3 + (gekauft) / - (verkauft)	100.0 ---	95.0 ---	100.7 ---	95.7 ---	101.4 ---									
Preis Gut 4 + (gekauft) / - (verkauft)	100.0 ---	106.0 ---	100.7 ---	95.7 8	101.4 ---									
Preis Gut 5 + (gekauft) / - (verkauft)	100.0 ---	106.0 ---	112.4 ---	119.1 ---	113.2 ---									
Preis Gut 6 + (gekauft) / - (verkauft)	100.0 ---	95.0 ---	100.7 ---	106.7 ---	113.1 ---									

	Anzahl im Besitz	Preis pro Einheit	Hier können Sie kaufen (+1) oder verkaufen (-1)	
Gut 1	5	126.3	Gut 1 (+1)	Gut 1 (-1)
Gut 2	0	81.5	Gut 2 (+1)	Gut 2 (-1)
Gut 3	0	101.4	Gut 3 (+1)	Gut 3 (-1)
Gut 4	8	101.4	Gut 4 (+1)	Gut 4 (-1)
Gut 5	0	113.2	Gut 5 (+1)	Gut 5 (-1)
Gut 6	0	113.1	Gut 6 (+1)	Gut 6 (-1)

Ihr Guthaben: 674.9

At the top of the screen you can see in which period you are. Besides that the countdown of the period is shown.

Underneath you can see a table which displays the development of prices of the 6 goods up to the current period (here as example until period 1). The remaining cells of this period will be filled with each period. The respective price is the top number in the cell. The price in this example for good 1 in period 1 is 126,3 points. Directly below this figure your sales activities are reported: a positive number below the price means, that you have bought units of this good. A negative number means, that you have sold units of these goods. In the example you can see, that in period 0 ten units of good 1 and eight units of good 4 were bought. In period 1 five units of good 1 were sold.

The table below shows you how many units of goods you currently hold ("Anzahl im Besitz") and the current price of the goods ("Preis pro Einheit"). You can also read your current balance in the last line. In this example there are five units of good 1 and eight units of 4 in your possession and the remaining balance is 674.9 points.

To the right you find 12 buttons. These are 6 buy buttons (left column) and 6 sale buttons (right column). These buttons appear from period 0, as you can only trade from this period on.

By clicking on the respective buy button you can bring one unit of the good in your possession, as long as your credit is sufficient. By pressing the sell button you can sell units of the corresponding good, the corresponding price will be credited in your balance.

You cannot sell goods that you do not own and you can only buy goods as long as your point balance is sufficient. That is, neither your stock nor your balance may fall below zero. Moreover, it is only possible to buy or sell whole units.

Allocation of goods to probabilities of a price increase

At the beginning of periods 0, 7 and 14 you will be asked for an allocation of probabilities of a price increase to the single goods. For the rating of the goods you get additional 90 seconds in the corresponding periods, after the expiry of 90 seconds the field for the ranking is hidden.

Specifically, you should assign all 6 goods with one of the 5 descriptions ("−−", "−", "0", "+", "++"). You can only assign one description to one good and you should use every description except "0" only ones and the description "0" two times. The description "++" you should assign to the good from which you think it has the highest probability for a price increase (65%). Proceed accordingly to the other goods, e.g. for "−−" you should assign the good from which you think that it has the lowest probability for a price increase (35%).

In the corresponding periods you will see the following illustration in the lower right corner of your screen:

Bezeichnung	--	-	0	+	++
Wkt. für Preissteigerung	35%	45%	50%	55%	65%
Anzahl	1	1	2	1	1

By clicking the appropriate fields, you assign the goods to the corresponding description. For example, if you want to assign good 2 to the term "+", select the fourth opportunity in the line of good 2. If you have made all your assignments, please confirm your decision by clicking on the "Eingeben" button on the bottom of the screen. You still have to wait for the expiration of the 90 seconds. If you do not click "Eingeben", the computer will not count your entry and you will not make money for the allocation of goods. After 90 seconds the field to enter the descriptions disappears and you can trade (in period 0 and 7), respectively after the expiry of the remaining time part II ends (in period 14, the final period).

For every correct assignment you have made, you get 20 points at the end of part II (they will not be credited directly to your balance). Since you will be asked in three periods to make 6 assignments, you can earn up to 360 points with your assignments of goods.

Payoff

At the end of period 14 all goods are sold at the displayed price. This sale is added to your points account. Furthermore you get 20 points for each correct allocation of a probability. At the end of the experiment your payoff will be converted, displayed and then paid in cash.

Trial market and comprehension questions (BEFORE PART I!)

After the instructions are read aloud and before you start with part I of the experiment, you get the opportunity to familiarize yourself with the surface of the goods market. Please follow the instructions displayed in red in the upper part of the screen. These are small tasks to give you an understanding of the goods market. The price development of the sample periods differs from the subsequent price development, that is you cannot make any conclusions about the price development of the goods in part II. Please note that in this sample periods as opposed to the later main tasks the timeout is not binding, so you can exceptionally exceed the time in the upper right corner of the screen if you do not comply with the instructions in time.

After this you get 7 questions on your screen, which make sure if you have understood fully the rules of the goods market. Your answers to the control questions have no influence on the payment at the end of the experiment. As we said, we want you to understand how the goods market works as good as possible. Therefore, all questions are displayed until you have answered all questions correctly. If you are in doubt, please raise your hands. An experimenter can answer your questions in private.

Part III

Part III consists of two sections.

Section 1

In the first section we will ask you questions. You will see all 3 questions at once on your screen. In total you have 4 minutes to write your responses in the fields provided. If you have not made an entry after 4 minutes, the questions are faded out.

Section 2

The second section consists of two screens. On both screens, you get a certain number of decision problems. On the first screen there are 11 decision problems and on the second 7. Thereby, you are not connected with another person; hence you decide only for yourself.

In each decision problem you can choose between two alternative options, each either resulting in a high or a low payment. The probability of occurrence of a high or a low payment is each 50%. By clicking on your preferred options (Option A or Option B) you make your decision. By clicking on the OK-button you confirm your entries. You can only once jump from option A to option B. Afterwards you need to stay at option B for all following situations. However, it is possible that you choose consistently option A or option B. For example it is not possible if you have opted in decision problem 1 for option A, in decision problem 2 for B and in decision problem 3 again for A. Take your time for your decision, because your choice – as described below – determines your payout from part III.

Here are examples for each screen:

Screen I: 1st Decision Problem

	Option A:		Option B:
1.	mit 50% Wahrscheinlichkeit **1.00 Euro** mit 50% Wahrscheinlichkeit **2.00 Euro**	A ○ ○ B	mit 50% Wahrscheinlichkeit **0.20 Euro** mit 50% Wahrscheinlichkeit **2.40 Euro**

Screen II: 2nd Decision Problem

	Option A:		Option B:
1.	mit 50% Wahrscheinlichkeit **-0.40 Euro** mit 50% Wahrscheinlichkeit **2.50 Euro**	A ○ ○ B	mit 50% Wahrscheinlichkeit **-2.10 Euro** mit 50% Wahrscheinlichkeit **3.00 Euro**

Payout

Your profit is determined as follows: The computer randomly chooses with the same probability one of the 18 decision problems (11 on screen I and 7 on screen II). The lottery that you have chosen in your decision problem is then simulated by the computer by drawing a number between 0 and 10. You get the high payout, if the randomly drawn number is less than or equal to 5 (50% probability) and the low payout, if the random number is greater than 5 (50% probability).

Example: Assume the computer randomly selects the first decision problem on screen I. Suppose you have chosen option B. Then the computer simulates option B and you either get 0.20 EUR (with probability 50%) or you get 2.40 EUR (also with probability 50%) as payment for part III of the experiment.

The result of this part will be provided at the end of the experiment. After the end of part III part IV starts automatically.

Part IV & end of the experiment

In the last part we present 5 multiple-choice questions about your financial knowledge. For each correct answer you get 20 cent (0.20 €), which will be paid to you at the experiment in connection. For incorrect answers you get nothing.

In the following, please answer some questions on your person complete and honest, as they are very important for our investigation. After answering the questions your payments for all parts of the experiment will be displayed.

Finally, we will pay you your earnings in cash and in private. Please stay seated until we call you at random order. Please leave the pen and the instructions at your place and take your place card with you.

Good luck and thank you for your participation in today's experiment!

A.3.2 Myopic Loss Aversion: Instructions (translated from German)

Note that what is referred to in the main part of the paper as part 1 and part 2 correspond to Part I and Part II – Part III of the instructions, while part 3 refers to Part IV – Part V of the instructions.

Welcome to the experiment and thank you for your participation!

Please do not talk to other participants of the experiment from now on

General information on the procedure

This experiment is conducted to investigate economic decision making. You can earn money.
Today's experiment takes about 1 hour and consists of 5 parts. Before every part you get detailed instructions. If you have any questions after the instructions or during the experiment, please raise your hand or push the red button, if no experimenter is in the room. One of the experimenters will come to you and answer your questions in private. During the experiment you will be asked to make decisions. Only your own decisions determine your payoff. This results in accordance with the rules, which are explained below.
While you make your decisions, a clock is running down on the right top of the screen. This offers you an orientation, how much time you should need for your decisions. Mostly you can also exceed this time, if you need more time for your decision. If you cannot exceed the predetermined time, we will inform you explicitly.

Payment

In part II your income is expressed in thalers. Here the exchange rate is 100 talers = 0.5 €. The remuneration for the remaining parts will be directly indicated in Euros.
For your punctuality and answering the questionnaire at the end of the experiment you get 4 € in addition to the income you can earn during the experiment. Every part is relevant for your payment. Your earnings from this experiment will be paid at the end in cash.

Anonymity

We evaluate all the data of the experiment only anonymous and do never connect names with the data from the experiment. At the end of the experiment you need to sign a receipt for the payment, which serves for the accounting.

Devices

At your place you will find a pen. Please leave it on the table after the experiment.

Part I

Task

This part of the experiment is a task about letters. You will see exactly 1 word in black letters for 3 seconds on the screen.

Within the 3 seconds, it is your task to classify the displayed word into 2 categories according to a certain rule. The exact rule will be shown to you on the screen at the beginning. Please take your time to read the rule carefully, as it will not be displayed again later!

If the displayed word satisfies the rule, you can classify the word into the first category by pressing the "e" key on your keyboard.

If the displayed word does not satisfy the rule, do not press any key. That is, it will be classified into the second category. After 3 seconds the next word will be displayed automatically.

For your classification you have 3 seconds. As soon as the time runs out a new word will be displayed. Even if you have chosen a categorization right at the beginning of the period, e.g. after 1 second by pressing the "e" key, you must wait for the next word until the countdown is expired.

Please note, that your entry is ultimate. If you press the "e" key by mistake, you cannot correct your decision. Therefore, a focused approach is essential.

After all 150 words have been displayed – after 7 minutes and 30 seconds – the first part will end automatically and the second part starts.

Trial phase

Before the task begins, there will be displayed 3 words on trial, which you need to categorize. In the trial phase there is no time limit of 3 seconds. You have an extended time limit of 10 seconds.

If you choose the wrong categorization in the trial periods, the computer shows you the right solution with an appropriate justification. Please note that you need to wait the 10 seconds either way. After a wrong answer you get a feedback or the next test word will be displayed. After you have read your feedback (if any), please click on "OK" to proceed to the next test word. Please note that you will not get any feedback on your answers during the actual tasks.

After the trial phase you have one last chance to ask questions about this task. If you have a question, please raise your hand or press the red key on your keyboard.

Payoff

For your concentrated work in part I you get 3 €.

Part II

Part II consists of 18 rounds. In each round you get an endowment of 100 thalers (100 talers = 50 Cent = 0.5 €). In every round you have to decide how many thalers (X) of your endowment (from 0 to 100 talers) you want to set in the subsequent lottery.

If you choose to set the amount X in the lottery, then

- You lose this amount X with probability of two thirds (66.67%) and get a payment of $100 - X$ at the end of the round
- You win with a probability of one third (33.33%) 2.5-times the amount of X. Then you get a payment of $100 + 2.5 * X$ at the end of the round.

The decision on the outcome of the lottery is dependent on a random number that is drawn new in each round. The computer simulates a single-shot with a six-sided dice, in which each number has the same probability of $1/6 = 16,67\%$. You win every time the computer tosses the dice with numbers 1 or 2. You lose, if the computer tosses the dice with numbers 3, 4, 5 or 6. The computer plays every round a random and independent toss.

Therefore the probability to win $2.5 \times X$ is one third. With two thirds probability you lose the amount X.

Determination of the amount X for 3 rounds each

At the beginning of the 1st, 4th, 7th, 10th, 13th and 16th round you have to define an amount X for the lottery, which is fixed in every of the subsequent 3 rounds (so in rounds 1-3, 4-6, 7-9, 10-12, 13-15 respectively 16-18). During the random number is drawn new for every round you set the same amount X for each of 3 rounds. After you have set in your amount X, you learn the random numbers of the 3 rounds and how often you have won or lost on an extra screen.

This determines your earnings for the round. You will also see your whole earnings on the screen. For the payoff of this round the earnings of all rounds will be added.

Please note, that you cannot use the earnings from earlier rounds for the lottery. That is, that your input X is 100 talers maximum in every round. However, the input has to be identical for 3 rounds each (1-3, 4-6, 7-9, 10-12, 13-15 respectively 16-18).

(**Narrow:**

Part II consists of 18 rounds. In each round you get an endowment of 100 talers (100 talers = 50 Cent = 0.5 €). In every round you have to decide how many thalers (X) of your endowment (from 0 to 100 talers) you want to set in the subsequent lottery. If you choose to set the amount X in the lottery, then

- You lose this amount X with probability of two thirds (66.67%) and get an payment of $100 - X$ at the end of the round
- You win with a probability of one third ($33,33\%$) 2.5-times the amount of X. Then you get an payment of $100 + 2.5 * X$ at the end of the round.

The decision of on the outcome of the lottery depends on a random number that is drawn new in each round. The computer simulates a single-shot with a six-sided dice, in which each number has the same probability of $1/6 = 16.67\%$. You win every time the computer tosses the dice with numbers 1 or 2. You lose, if the computer tosses the dice with numbers 3, 4, 5 or 6. The computer plays every round a random and independent toss. Therefore the probability to win $2.5 * X$ is one third. With two thirds probability you lose the amount X.

After you have set in your decision about the amount X you get the information about the random number of this round and of you have won or lost on an extra screen.

This constitutes your round payment. You will also see your whole payment. For the payment in this part, payments from all rounds are added.

Please note, that your payment from earlier rounds cannot be used in the current round as input for the lottery. That is, in each round your amount X cannot exceed 100 talers.)

Part III

After part II has ended part III begins automatically. You will see the instructions for part III on your screen.

Part IV

Task

Part IV consists out of 6 decision making situations. In each situation you have to decide if you accept or decline a lottery. Every lottery results either in a win or in a loss. The probability of winning or losing is 50% each. By choosing your favored option (accept or decline) you make your decision. You confirm your decision by clicking the button "zu Teil V"-Button ultimately. Take your time for your decisions as your decision determines your payoff from part IV.

Payment

Your payment is determined as follows: The computer chooses randomly and with same probability one of the 6 decision making situations and simulates a fair coin toss. If you have accepted the lottery in the decision making situation you get the win with heads (with 50% probability) and the loss with tails (with 50% probability). If you have declined the lottery your payment is 0 Euro in this part.

Possible losses from part IV will be charged with the earnings of the other parts of the experiment. The result of this part will be provided at the end of the experiment.

After the end of part IV automatically part V begins.

Part V

Task

You are asked 7 questions in part V. You will get these displayed on two screens in succession. On the first screen you get 4 questions, on the second 3 questions. For the first 4 questions you have 4 minutes to enter your answers in the fields provided. For the following 3 questions you have 3 minutes. Please click on OK at the end of each timeout, so your input is saved and the experiment can be continued.

Payment

For answering the 7 questions you get 2.50 €.

End of the experiment

We please you to answer a questionnaire honest and complete, as these information are very important for our investigation.

Afterwards your earnings from the experiment will be displayed on a separate screen.

Finally, you have the opportunity to give us feedback about today's experiment. Then your payment is paid to you private and in cash. Please stay seated at your place as we will call you at random order. Please leave the pens and instructions at your place and take your place card with you.