Essays in Behavioral and Experimental Finance

Konstantin Egbert Lucks

München 2016

Essays in Behavioral and Experimental Finance

Konstantin Egbert Lucks

Inaugural-Dissertation
zur Erlangung des Grades Doctor oeconomiae publicae
(Dr. oec. publ.)
an der Volkswirtschaftlichen Fakultät
der Ludwig–Maximilians–Universität München

2016

vorgelegt von
Konstantin Egbert Lucks

Referent: Prof. Dr. Martin Kocher

Korreferent: Prof. Dr. Joachim Winter

Promotionsabschlussberatung: 16. November 2016

Tag der mündlichen Prüfung: 11. November 2016

Namen der Berichterstatter: Martin G. Kocher, Joachim Winter, Markus Glaser

Danksagung

Jetzt, wo es auf das Ende meines Lebensabschnittes ‘Promotion’ zugeht, möchte ich all den Menschen danken, die mich auf meinem Weg unterstützt und begleitet haben.

Als erstes möchte ich meinem Betreuer Martin Kocher danken, der sich immer Zeit für mich genommen hat, egal wo oder wie beschäftigt er gerade war, und der immer offen für neue Ideen war und mich bei deren Umsetzung unterstützt hat. Außerdem, möchte ich Marta Serra-Garcia danken, die mich zu einer sehr interessanten Zusammenarbeit eingeladen hat, aus der aber leider kein gemeinsames Projekt entstanden ist, und die mich großzügigerweise für ein halbes Jahr nach San Diego eingeladen hat. Hier möchte ich mich auch bei Uri Gneezy für seine Gastfreundschaft und die Aufnahme in seine Forschergruppe in San Diego bedanken. Ich hatte dort eine sehr schöne und sehr interessante Zeit! Außerdem möchte ich Joachim Winter danken, der stets neue und interessante Perspektiven bei Projekten aufgezeigt hat und der einen Großteil der Verpflichtungen, die bei unseren Schulexperimenten bei unserem Kooperationspartner entstanden sind, übernommen hat.

Außerdem möchte ich meinen Koautoren für die lehrreiche und spannende Zusammenarbeit danken. Die Arbeit an dem Projekt mit Martin Kocher und David Schindler hat mir sehr viel Freude bereitet und ich habe dabei sehr viel gelernt. Die Projekte mit Melanie Lührmann, Joachim Winter und Marta Serra-Garcia haben mir ebenso sehr viel Spaß gemacht und eine wunderbare Lernerfahrung dargestellt. Vielen Dank insbesondere für das hervorgebrachte Vertrauen bei der Durchführung der Schulexperimente!

Weiterhin möchte ich meinen Kollegen an der LMU danken, vor allem meinen Kollegen am Lehrstuhl und den Doktoranden des Evidence Based Economics Programms, mit denen ich die gemeinsame Zeit und den intellektuellen Austausch sehr genoßen habe. Besonders hervorheben will ich hier Simeon Schudy, der immer ein offenes Ohr für mich hatte und mir jederzeit bereitwillig sowohl konzeptionell als auch bei der Umsetzung von Projekten geholfen hat. Besondere Erwähnung verdienen auch meine wissenschaftlichen

Hilfskräfte Daniela Eichhorn, Julia Rose und Vivien Bonten, die mich zu unterschiedlichen Zeitpunkten meiner Dissertation tatkräftig bei meinen Dissertationsprojekten unterstützt haben, sowie Cristina da Glória und Angelica Schmidt.

Dann möchte ich mich noch bei der Munich Graduate School of Economics für die tollen Jahre und die hervorragenden Vorlesungen und Gastvorlesungen bedanken und bei My Finance Coach und Iris Siara für die tolle Zusammenarbeit während der Vorbereitung und der Durchführung unserer Schulexperimente.

Zuletzt möchte ich mich bei meiner Familie und meinen Freunden für den Rückhalt, den sie mir in den Jahren meiner Promotion gegeben haben, die Ermunterung und die gemeinsam verbrachte Zeit bedanken.

Konstantin E. Lucks, Juni 2016

Table of Contents

List of Figures

List of Tables

Preface

The efficient market hypothesis (Fama, 1970) is still the most prominent theory on price formation in asset markets. According to this hypothesis, asset prices reflect all available information. Thus, it is impossible to beat the market consistently. However, the efficient market hypothesis is explicitly based on the existence of rational actors who maximize expected utility based on rational expectations. Any mispricing would be exploited by such traders and irrational subjects would be pushed out of the market. Implicitly, such a mechanism assumes no trading constraints on rational market participants, who can thus borrow money and short sell assets to the degree necessary to correct mispricing (cf. Barberis and Thaler, 2003).

Since the emergence of the efficient market hypothesis, there has been a longstanding discussion on its empirical validity. Researchers have documented a variety of stock market anomalies which are inconsistent with the traditional finance paradigm of 'rational' agents (Barberis and Thaler, 2003). Several scholars provide evidence that 'seemingly irrelevant factors' (Thaler, 2015) affect aggregate market outcomes. For example, stock returns are positively correlated with the amount of sunshine in the location of the stock market (Hirshleifer and Shumway, 2003). On the individual level, investors are less willing to sell stocks whose price has dropped since purchase compared to those whose price has increased; this regularity is referred to as the disposition effect (Shefrin and Statman, 1985; Odean, 1998). Higher degrees of the disposition effect are correlated with inferior trading performance due to suboptimal trading with regards to tax considerations, but also due to subsequent underperformance of unsold 'loser' stocks which continue to underperform due to medium-term return momentum (Odean, 1998; Seru et al., 2010).

Many of such departures from rationality can be explained by incorporating behavioral principles from psychology, sociology and anthropology into financial research (Shiller, 1999). This research area is commonly referred to as 'behavioral finance'. Several explanations for market behavior originate from psychology, such as those treated in this dissertation – self-control and peer effects.

Motivated by psychological research, this dissertation experimentally investigates explanations for financial decision making outside of the 'rational' expected utility framework. Chapters 1 and 2 consider the role of self-control for financial decision making in different contexts. Chapter 3 looks at peer effects – the impact of social influence on risky decision making. All the chapters are based on economic experiments, which allow drawing causal inferences from exogenous treatment variations in the decision environment. The two chapters examining the effect of self-control on financial decision making are based on laboratory experiments. In such a context, the experimenter can temporarily reduce self-control abilities due to the close control over the decision environment. The last chapter studies how peers affect risky decision making and how social distance can account for heterogeneous effects. It is based on a lab-in-the-field experiment (Charness et al., 2013) in German lower tier high schools, because in this environment more distant peers (i.e. acquaintances) and less distant peers (i.e. friends) occur naturally.

Over the last years, self-control has attracted considerable interest in the field of behavioral economics – a trend mainly motivated by the existing psychological literature. Even though empirical evidence from behavioral finance is rather limited, practitioners and investment guides often stress the importance of self-control for financial decision making. For example, investment legend Warren Buffett sees "[...] the temperament to control the urges that get other people into trouble in investing [...]" as necessary for successful investing[1]. The psychological definition of self-control seems to match Warren Buffet's statement quite closely: self-control is the ability to override or inhibit undesired behavioral tendencies such as impulses and refrain from acting on them (Tangney et al., 2004). In the first two chapters of this dissertation, I aim to fill this research gap by investigating the effects of *state* self-control on financial decision making – a) in a market environment in chapter 1 and b) on individual decision making in chapter 2.

Psychologists differentiate between two kinds of self-control: *trait* self-control (dispositional self-control) and *state* self-control. *Trait* self-control was popularized by the marshmallow experiments of Walter Mischel and coauthors (e.g. Mischel and Ebbesen, 1970). High dispositional self-control is correlated with many forms of behavior that contribute

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

to a successful and healthy life, while low self-control is correlated with problematic behavior including impulsive buying and procrastination (Mischel et al., 1989; de Ridder et al., 2012). From an economic perspective, *trait* self-control can be considered as capturing some stable preference. On the other hand, an extensive body of research shows that self-control can be temporarily weakened, thus self-control can also be considered as a time varying *state*. Following up on the work of Roy Baumeister and coauthors (e.g. Baumeister et al., 1998), this research has proliferated in recent years.

Economics complements the psychological perspective of self-control by formalizing theories in mathematical models. Economists have traditionally focused on intertemporal decision making as a manifestation of self-control. People often behave in time inconsistent ways, i.e. they might change their mind about previous plans once the moment of action arrives. For example, a student might postpone studying to the next day in order to indulge in binge watching the latest Netflix series, even though she had previously planned to study on that day (O'Donoghue and Rabin, 1999). Such behavior is hard to reconcile with rational intertemporal decision making – i.e. discounted expected utility maximization – and explanations have been modeled in various ways. One popular explanation is referred to as (quasi-) hyperbolic discounting (see e.g. Laibson, 1997). Compared to the benchmark of exponential discounting, hyperbolic discounting involves overweighting of present utility relative to future utility, which is also referred to as 'present bias'. Notable exceptions to this static view of self-control are Ali (2011), Fudenberg and Levine (2012) and Ozdenoren et al. (2012), who model temporary changes in the resource self-control based on psychological findings.

Even though anecdotal evidence suggests that present biased preferences are widespread among individuals (e.g. Laibson, 1997; O'Donoghue and Rabin, 1999), hyperbolic discounting is unlikely to impact prices on asset markets since higher short-term discount rates could be spotted and exploited easily by either unbiased market participants or algorithms. It is therefore unlikely to find direct evidence for hyperbolic discounting in financial markets. On the other hand, temporary fluctuations of self-control resources or stable aspects of self-control that are not directly related to intertemporal decision making could influence financial decision making and consequently outcomes on financial markets. To the best of my knowledge, there has been little research on the relationship between investment behavior and self-control. Most of the existing evidence is correlational and thus cannot provide causal inferences. For example, Fenton-O'Creevy et al. (2011) find pronounced differences in emotion regulation strategies between different groups of traders based on interviews with traders and senior managers at investment banks in London. Similarly, in Lo et al. (2005a), day traders with more intense emotional reactions to gains and losses exhibit significantly worse trading performances. Additionally, day traders' self-assessed strengths and weaknesses suggest self-control as a highly relevant factor for investment success.[2]

In this dissertation, I focus on the temporary *state* self-control by using established treatments from psychology of weakening self-control (Hagger et al., 2010) in financial decision making experiments. This approach enables me to identify causal effects of self-control on financial decision making. However, it is noteworthy that research comparing *state* and *trait* self-control usually finds both concepts to impact behavior in similar ways (e.g. Schmeichel and Zell, 2007).

Besides self-control problems, there is a variety of other factors that may affect financial decision making. In the last chapter of my dissertation, my coauthors and I consider one such factor – peer effects – in a stylized risky decision making experiment. We pay particular attention to social distance as a factor explaining heterogeneity in peer effects.

Traditional economics abstracts from social interactions and considers decision making as determined by the individual. However, more recent findings suggest that social interaction might play a bigger role than previously thought, because decision makers often rely on the opinions or actions of others when evaluating alternatives. Nobel prize winner Robert J. Shiller already pointed this out in the 1980s: "Investing in speculative assets is a social activity." (Shiller et al., 1984, p. 457). Peer effects[3] on decision making can be characterized as differences in behavior of an individual in isolation compared to a social setting where some interaction with others or observation of others is possible (Lahno, 2014).

Social psychologists have studied the effect of social interactions on decision making for a considerably longer time than economists. Leon Festinger's theory of social comparison for instance has been very influential in social psychology (Festinger, 1954). The main

[2]See table 3 in the working paper Lo et al. (2005b).

[3]In the following I use this expression interchangeably with the effects of social interaction.

idea of this theory is that human beings use social comparisons in order to evaluate own opinions and abilities in the absence of objective criteria.

Manski (2000) outlines an economic framework to conceptualize peer effects. Peer effects between two agents can occur if the action of one agent affects preferences, expectations or constraints of another agent. Preference interactions might arise due to direct influence of someone else's payoff on one's own utility, e.g. because of altruism, or due to the effect of social comparison on utility, e.g. because of jealousy or pride. Such preference interactions are for instance present in the outcome based social preference models of Fehr and Schmidt (1999) along with Bolton and Ockenfels (2000), where decision makers can derive additional utility from the payoffs of others or disutility from lagging behind others. Information exchange between agents can be thought of as either affecting expectations, e.g. by influencing another agent's belief about the likelihood of a certain state of the world, or as affecting constraints, e.g. by enlarging another agent's consideration set by introducing her to a new choice option (Manski, 2000). Both of these channels can be considered as forms of social learning.

Peers have been shown to exert considerable influence on financial decision making in several real world applications, including social interaction in the case of enrollment in retirement savings accounts and the level of retirement savings (Duflo and Saez, 2002, 2003; Beshears et al., 2015), the decision to participate in the stock market (Brown et al., 2008), asset allocation in stocks (Ivković and Weisbenner, 2007; Bursztyn et al., 2014), and insurance decisions (Cai et al., 2015).

The role of social distance for peer effects in financial decision making has received little attention, even though there are clear differences in peer relationships between studies. In the economic laboratory, peer effects are usually studied by grouping participants who have never interacted before with each other[4] (e.g. Falk and Ichino, 2006; Lahno and Serra-Garcia, 2015). Meanwhile field experiments and field studies usually consider the interaction between pairs of friends or family members (e.g. Bursztyn et al., 2014; Dahl et al., 2014). It seems plausible that the channel of peer effects might work quite differently depending on the relationship with the peer, e.g. choices of more distant acquaintances

[4]Manski (2000) criticizes this in the following way: "(...) the groups whose interactions are observed are formed artificially for the sake of the experiment. This raises obvious questions about the credibility of extrapolating findings from experimental settings to populations of interest."

could convey more information than those of closer peers. Similarly, people might care less about the payoffs of socially more distant peers.

My dissertation consists of one single-authored paper and two joint research projects.

The first chapter is a joint project with Martin Kocher and David Schindler. Together, we explore the impact of a self-control reducing manipulation (Baumeister et al., 1998) on prices and behavior on experimental asset markets, since increased impulsivity could reinforce bubble formation. We contribute to the literature exploring the psychological foundations of the results on overpricing in the experiments by Smith et al. (1988).

In three experimental treatments either no, all, or half of the traders in a market are exposed to the Stroop task (Stroop, 1935) before trading commences. This is one of the most widely used methods to weaken individuals' self-control (Hagger et al., 2010). We refer to these different treatments as *HIGHSC*, *LOWSC*, and *MIXED* markets respectively. Ten experimental participants then interact on an asset market that is known for its basic tendency to exhibit overpricing. It features a dividend-bearing asset with decreasing fundamental value. Additionally, we collect measures of risk attitudes and cognitive abilities to detect the channels through which individual self-control problems might translate into overpricing on the aggregate level.

We find significantly higher levels of overpricing on markets where traders' self-control abilities have been depleted compared to markets where no one's self-control has been depleted. Meanwhile, there is no direct effect of the manipulation on risk attitudes or cognitive abilities. Self-control depleted traders also do not trade significantly less than non-depleted participants. However, in *MIXED* markets, we find three patterns that suggest reasons for the increase in overpricing: first, traders with low self-control engage in more speculative behavior early on and others jump on this bandwagon. Second, our measure of cognitive abilities loses its predictive power for profits of traders with low self-control indicating that their behavior is based on controlled decision making processes to a lower degree. Third, traders with low self-control report higher levels of emotional arousal during the market, suggesting that they were driven by impulses more strongly.

Our study is the first to show a causal effect of a temporary depletion of self-control on overpricing and behavior in asset market experiments. We show that already a moderate number of participants with low self-control is sufficient to nearly double the extent of overpricing. Our findings have important implications: first, with differences in self-control levels, we add a new explanation for overpricing in asset markets to the literature. Second, our results are indicative of the role of self-control in real world markets, either due to temporary reductions in self-control or due to differences in dispositional self-control. Based on our results, investment decisions should not be taken in a state of reduced willpower. If such conditions are unavoidable, commitment devices might be a powerful tool to avoid potentially negative consequences of reduced self-control.

In the second chapter, I explore how reduced self-control affects individual investment behavior in two laboratory experiments. In the first experiment, I examine the effect of reduced self-control on the disposition effect, i.e. the tendency to be more willing to sell stocks that have gained in price after purchase than stocks that have lost in price (Shefrin and Statman, 1985). In the second experiment, I look at the effect of the same manipulation on myopic loss aversion, i.e. the inclination to shy away from investing money in an asset when its price fluctuations are evaluated more frequently (Benartzi and Thaler, 1995). I contribute to the literature by looking for explanations and exploring the heterogeneity of these two behavioral patterns.

I exogenously reduce subjects' self-control using the letter-e-task (Baumeister et al., 1998). In experiment 1, this is followed by an individual trading environment with multiple stocks where the disposition effect can be measured (Weber and Welfens, 2007). In experiment 2, an investment task follows where the investment horizon of participants is exogenously varied to explore myopic loss aversion (Gneezy and Potters, 1997). In each experiment, I additionally obtain measures of cognitive abilities, risk preferences and dispositional self-control or impulsivity.

In each experiment, I find no significant main treatment effect, but rather subtle secondary effects, which are in line with findings on self-control from other studies. In experiment 1, I find no significant change in the disposition effect following the self-control manipulation. However, treated participants concentrate on trading fewer different shares per round which is purely driven by subjects with high cognitive abilities. In experiment 2, I investigate the effect of self-control on myopic loss aversion by implementing a 2×2 design varying investment horizon and self-control between subjects in a repeated lottery environment. While lowered self-control seems to reinforce framing effects directionally, I cannot reject the null hypothesis of equal investment levels between the self-control treatments within each investment horizon. Analyzing the dynamics of decision making in more detail, I find that investment experiences drive behavior of participants with low self-control with the short investment horizon more strongly. On average, these subjects consequently reduce their investment levels over the course of the experiment. Finally, I find no significant impact of the self-control manipulation on the experimental measures of cognitive abilities and risk preferences in either experiment.

To my knowledge, this is the first paper to experimentally reduce self-control in order to look at the impact of this treatment on individual investment biases. The combined results of experiments 1 and 2 provide some indication that a reduction in self-control causally results in an increase in narrow framing, while the previous literature has identified a limited number of correlates of a person's tendency to frame repeated risky decisions narrowly. Furthermore, experiment 1 indicates that particularly those participants who are similar to financial traders in terms of cognitive abilities (cf. Thoma et al., 2015) are influenced by the self-control manipulation. Thus, despite selecting into markets based on high *trait* self-control, market participants might be particularly sensitive to temporary fluctuations in self-control *state*.

Chapter 3 is a joint project with Melanie Lührmann and Joachim Winter. In a field experiment, we study the role of peer effects in risky decision making among adolescents in the age range of 13 to 15. More specifically, we explore how the heterogeneity in peer effects might be moderated by social distance. We randomly allocated school classes to two interaction treatments in which students were allowed to discuss their choices with a natural peer – either a friend or a randomly selected classmate – before individually making choices in an incentivized lottery task that is comprised of both pure and mixed prospects. In a control group, adolescents made choices without being able to discuss them with a peer. We contribute to three strands of literature. Firstly, we complement the literature that documents how the typical patterns of risky choices develop during childhood and adolescence. Secondly, we document how the heterogeneity of peer effects is moderated by social distance and thus contribute to the literature on peer effects. Finally, we add to the scarce evidence on assortative matching based on risk preferences, i.e. whether friends are more similar to each other in terms of risk preferences.

We gave adolescents two incentivized choice lists eliciting risk preferences and attitudes towards losses adapted from the design of Tanaka et al. (2010). We randomly allocated 12 classes to one of three treatments – one control treatment, in which adolescents made decisions without interaction, and two interaction treatments, in which students were allowed to discuss their choices with one peer. In the interaction groups, we induced two different types of peers: endogenously chosen friends and randomly allocated classmates.

Our first finding is that a large majority of adolescents are both risk and loss averse (Kahneman and Tversky, 1979). Second, we find no evidence of assortative matching on (or convergence in) risk preferences – neither with respect to risk nor to loss aversion. In contrast, we do confirm the expected matching of friends based on observable characteristics, such as age, gender and family background. Third, peer communication increases choice similarity markedly in both treatment arms and in both types of lotteries. Increased similarity mainly arises due to perfect alignment of choices. Fourth, by investigating a private and a public signal of financial competency, we can investigate social learning. Our findings indicate that social learning effects differ according to social distance – randomly paired classmates with a high private signal are less likely to perfectly coordinate their choices, while larger differences in the public signal negatively predict perfect coordination. However, no such effects exist among paired friends. Finally, randomly paired participants become significantly more loss averse (and weakly more risk averse) compared to the control group, while there is no such effect among pairs of friends.

To the best of our knowledge, we are the first to systematically establish the presence of loss aversion among adolescents. Contrary to our initial hypotheses, we did not find that social interaction reduces the level of risk and loss aversion among adolescents, but quite the contrary: it may increase loss aversion if peers are less familiar with each other. Our study is the first to create experimental peer interaction among natural peers, while recognizing the need to account for assortative matching. This leads to a new finding about the nature of matching: we find no evidence that relationship formation is driven by similarity in risk preferences. Finally, we systematically document heterogeneity in peer effects according to social distance, both in social learning as well as in the direction of the effect on choices.

Each chapter of this dissertation is self-contained, as each includes its own introduction and can be read independently. The appendices of each chapter are collected in the last section of this dissertation arranged in the same order as the individual chapters. They contain additional analyses referred to in the main body, as well as the instructions of the respective experiments included in each chapter.

CHAPTER 1

Unleashing Animal Spirits – Self-Control and Overpricing in Experimental Asset Markets[1]

1.1 Introduction

> "Even apart from the instability due to speculation, there is the instability due to the characteristic of human nature that a large proportion of our positive activities depend on spontaneous optimism rather than mathematical expectations, whether moral or hedonistic or economic. Most, probably, of our decisions to do something positive (...) can only be taken as the result of animal spirits – a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities."[2]
>
> *John Maynard Keynes*

Keynes famously saw "animal spirits" at the root of many (financial) decisions, potentially causing price exaggerations on the aggregate market level. As often in Keynes' work, the term "animal spirits" is not well-delineated. It alludes to optimism, instincts, urges, emotions, and similar concepts. In this paper we assess the notion that a *lack of self-control abilities* may lead to price exaggerations on asset markets, and we analyze how the lack of self-control abilities is associated to emotions and trading behavior. In psychology, self-control abilities and willpower are defined as the capacities to override or inhibit undesired behavioral tendencies such as impulses and to refrain from acting on them

[1]This chapter is based on joint work with Martin Kocher and David Schindler.
[2]Source: Keynes (1936), p. 136.

(Tangney et al., 2004). Self-control is necessary to guard oneself against undue optimism, actions motivated by emotional responses, and impulsive decisions. Furthermore, self-control is required in order to stick to plans made in the past.

That self-control is considered relevant for investor success is also evident from statements of investors and from popular guidebooks on the psychology of investing. For instance, Warren Buffet emphasizes that "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."[3] Similarly, anecdotal evidence from rogue traders show that they completely lost their self-control abilities at some stage. In a study by Lo et al. (2005a) involving day traders from an online training program participants stated attributes related to self-control as the most important determinants of trading success.[4] In a similar spirit, Fenton-O'Creevy et al. (2011) report distinct differences in emotion regulation strategies among traders of different experience and performance levels from qualitative interviews with professional traders. Therefore, correlational evidence suggests that self-control matters for trading success on an individual level.

This paper is the first to provide empirical evidence on the *causal* effect of a variation in self-control abilities on trading outcomes.[5] The major challenge to overcome is to exogenously vary self-control abilities in order to obtain causal inference on the impact of self-control abilities on behavior and market outcomes. A first step is to use the experimental laboratory and affect *state* self-control levels of traders. Most of the available techniques draw on the concept of self-control depletion or exhaustion. Our experimental identification rests on the assumption that self-control is a limited resource and that it is variable over time on the individual level. Evidence for these two characteristics is abundant (e.g. Baumeister et al., 1998; Gailliot et al., 2012), although it has also been questioned lately (Carter and McCullough (2013)). While validated survey measures for *trait* self-control exist, they can only provide correlational inference.

In the spirit of Keynes we concentrate on aggregate market outcomes in a first experiment and extend our analysis to individual behavior and performance in a second experiment.

[3] `http://www.businessweek.com/1999/99_27/b3636006.htm`

[4] They quote attributes such as persistence, tenacity, perseverance, patience, discipline, planning, controlling emotions, and (lack of) impulsivity as crucial (Lo et al., 2005a, table 3).

[5] However, there is a quickly growing empirical literature on the effects of self-control abilities on decision making in other domains relevant to economists (see, for instance, Beshears et al. (2015).)

We use a well-established financial market setup in the experimental laboratory (Smith et al., 1988; Kirchler et al., 2012; Noussair and Tucker, 2013; Palan, 2013; Eckel and Füllbrunn, 2015) to investigate whether an exogenous variation in self-control abilities of traders leads to overpricing. This experimental asset market is known for its basic tendency to exhibit overpricing; it features a dividend-bearing asset with decreasing fundamental value.

In order to deplete self-control abilities before the start of the market, we employ the Stroop task (Stroop, 1935), which is one of the most commonly used tasks in psychology experiments for modulating self-control (Hagger et al., 2010). It is easy to administer, it can be implemented in an exhausting/depleting version and in an easy version (i.e. a placebo version), and it allows for additional controls. The majority of studies that use both survey measures and behavioral measures of self-control conclude that the effects of state self-control interventions are qualitatively similar to those of trait self-control levels (e.g. Schmeichel and Zell, 2007). Hence, even if our experiment is confined to the laboratory setting and to a variation in state self-control, it is likely that it extends to situations outside the laboratory in which also trait self-control matters.

A drop in self-control abilities can increase the extent of overpricing on a market through different channels. One psychological transmission mechanism runs through an increased influence of the impulsive decision making system. A consequence could be that traders' behaviors become more easily swayed by observing others' behaviors on the market (for instance, a more pronounced tendency to momentum trading). Another behavioral mechanism relates to an heightened influence of emotions (for instance, the excitement after seeing the prospect of making more money, or a stronger psychological reward of interim gains). Yet another option, potentially related to impulsivity, would be a stronger role of biases in decision making such as myopia, limiting the ability to correctly foresee the declining fundamental value and thus creating histories of overpricing on the market.

Our main finding is a significantly higher level of overpricing on markets where traders' self-control abilities have been depleted, compared to markets with traders whose self-control abilities have not been depleted. If markets are populated by both depleted and non-depleted traders the effect is similar in size and also highly significant. Obviously, having some self-control depleted traders on a market suffices to create the additional over-pricing effect.

Behavior on markets is path-dependent, choices are endogenous to other choices, and traders imitate each other. Nonetheless, we are able to provide robust evidence from control variables, from trading and from survey questions that can explain the additional overpricing with depleted self-control abilities. First, there is no direct effect of self-control depletion on risk attitudes or cognitive abilities of traders, which could explain our findings. Second, self-control depleted traders do not trade significantly less than non-depleted traders, ruling out a simple exhaustion effect. Third, several indicators show that self-control depleted traders follow stronger speculative motives earlier on when trading. In other words, they contribute more to the creation of overpricing histories, and non-depleted traders jump on this bandwaggon. Fourth, stronger emotional arousal of individuals on the market is related to being self-control depleted. In short, traders become more impulsive and potentially rely less on cognitive skills, when they cannot resort to their full self-control resources.

The remaining paper is organized as follows: Section 1.2 gives an overview of the related literature, and in section 1.3, we explain and motivate our experimental design. Consequently, section 1.4 presents the results from our main experiment, and section 1.5 reports on an additional experiment that allows us both to test the robustness of our initial results and to better understand how self-control depletion translates into overpricing and how traders' behavior and decision processes might be affected by the treatment. We discuss potential channels explaining our findings in section 1.6. Section 1.7 concludes the paper.

1.2 Related Literature

Our literature overview focuses on the two aspects in the economics and psychology literature that are most relevant for our study: self-control and experimental asset markets. As already said, self-control abilities and willpower are defined as the capacities to override or inhibit undesired behavioral tendencies such as impulses and to refrain from acting on them. There are different theoretical approaches in psychology and in economics that take self-control abilities and potential self-control problems into account.

First, self-control can straightforwardly be related to dual-systems perspectives of decision making. As outlined by Kahneman (2011), these perspectives share the general assumption that structurally different systems of information processing underlie the production of impulsive, largely automatic forms of behavior, on the one hand (system 1), and deliberate, largely controlled forms of behavior, on the other hand (system 2). System 2 is effortful and requires self-control resources.[6] Thus, if resources are low, reflective operations may be impaired, leading to a dominance of impulsive reactions that could be in conflict with objective reasoning. From this perspective, reducing self-control abilities can be interpreted as increasing the role of the (impulsive) system 1 in decision making (Hofmann et al., 2009).

Second, and very much related to dual-system perspectives, economists have used dual-self models of impulse control (see, for instance, Thaler and Shefrin (1981) and Fudenberg and Levine (2006)) in order to describe self-control problems. These models study the interaction of two selves, a rational (long-term) and an impulsive (short-term) self. Such models can account for time inconsistent behavior (for instance, in connection with quasi-hyperbolic discounting) and for the fact that cognitive load makes temptations harder to resist. Third, willpower as a depletable resource has been modeled directly in economics. Ozdenoren et al. (2012) look at a consumption smoothing model that views willpower as a depletable resource, and Masatlioglu et al. (2011) consider lottery choices.

Is there empirical evidence for self-control abilities or willpower to be indeed limited or depletable resources? Many researchers in psychology have shown that exerting self-control consumes energy and consequently diminishes the available resources for other acts that require self-control.[7] Self-control can involve either cognitive control, or affective control, or both (Hagger et al., 2010). Self-control abilities regenerate through rest, can be trained, and differ between people (Baumeister et al., 1998; Muraven et al., 1999; Muraven and Baumeister, 2000; Tangney et al., 2004; Muraven, 2010).

Our experimental identification relies on self-control depletion. We reduce self-control abilities by exposing experimental participants to a self-control demanding task before the main task (known as the dual task paradigm). Such setups have been used in other domains in economics, mainly in the context of individual decision making. For example, the consequences of self-control variations in decision making under risk have been studied. Several papers report increased risk aversion following self-control depletion (Unger and Stahlberg, 2011; Kostek and Ashrafioun, 2014). However, a number of studies also

[6]Note that the division of system 1 as automatic and system 2 as controlled describes a tendency; there are both automatic and conscious processes involved in exerting self-control and giving in to temptation, respectively (cf. Kotabe and Hofmann, 2015).

[7]For recent overviews about the ongoing discussion in psychology and models of the underlying processes involved in self-control see Inzlicht and Schmeichel (2012) and Kotabe and Hofmann (2015).

reveal an increase in risk taking following similar manipulations (Bruyneel et al., 2009; Freeman and Muraven, 2010; Friehe and Schildberg-Hörisch, 2014). Both Stojić et al. (2013) and Gerhardt et al. (2015) find no significant effect of self-control manipulations on risk preferences elicited from choice lists. Bucciol et al. (2011, 2013) show in field experiments with children and adults that self-control depletion leads to reduced productivity in subsequent tasks. De Haan and Van Veldhuizen (2015) find no effect of a repeated Stroop task on the performance in an array of tasks in which framing effects – such as anchoring effects and the attraction effect – are typically observed.

Recently, experiments have looked at the effects of self-control variations on other-regarding preferences. Achtziger et al. (2016) report a strong but heterogeneous impact of reduced self-control on offers and accepting behavior in ultimatum games, presumably depending on what an individual's more automatic reactions are. In a similar vein, Achtziger et al. (2015) provide evidence for reduced dictator giving after a reduction in self-control abilities.[8]

Existing studies also suggest a relationship between self-control abilities and financial decision making. However, we are not aware of experimental studies in this context. Using survey evidence, Ameriks et al. (2003, 2007) consider the connection between wealth accumulation and trait self-control in a sample of highly educated US households. Ameriks et al. (2003) attribute differences in savings among households to differing "propensities to plan" – i.e. different individual costs of exerting self-control. Ameriks et al. (2007) use the difference between planned behavior and expected behavior in a hypothetical scenario as a measure for self-control problems. They find a positive correlation between better self-control abilities and wealth accumulation, in particular for liquid assets. Gathergood (2012) conducts a similar study in the UK with a representative sample. He reports a positive association between lower levels of self-control and consumer over-indebtedness.

Our asset market is based on the seminal paper by Smith et al. (1988), who were the first to observe significant overpricing in an experimental double auction market. Many studies have followed up on these early findings.[9] Trader inexperience and confusion have been considered as one of the aggravating factors of overpricing (Dufwenberg et al.,

[8]Martinsson et al. (2014) analyze the relationship between self-control and pro-sociality in an indirect way, but their findings are also in line with the idea that pro-social behavior requires self-control. A similar result is provided by Kocher et al. (2016b).

[9]Recent surveys can be found in Noussair and Tucker (2013) and Palan (2013).

2005; Kirchler et al., 2012), and Bosch-Rosa et al. (2015) for example show that grouping traders by cognitive skills leads to increased overpricing for groups with low cognitive sophistication. Nadler et al. (2015) provide evidence that giving testosterone to a group of male participants significantly increases prices, and Petersen et al. (2015) find that inducing stress decreases overpricing.

Since emotion regulation is correlated with self-control abilities (Tice and Bratslavsky, 2000), the influence of emotions on prices in asset markets is also relevant to our research question: Andrade et al. (2015) find that inducing excitement before trading triggers overpricing in asset markets stronger in magnitude and higher in amplitude than other emotions and a neutral condition. In a similar study, Lahav and Meer (2012) show that inducing positive mood leads to higher deviations from fundamental values and thus more overpricing. The role of emotions in experimental asset markets has also been evaluated using self-reported emotions on Likert scales (Hargreaves Heap and Zizzo, 2011) and face reading software (Breaban and Noussair, 2013), instead of inducing specific emotions exogenously. Results from these experiments indicate that excitement and a positive emotional state before market opening are correlated with increased prices relative to fundamental values. Moreover, fear at the opening of the market is correlated with lower price levels.

Finally, Smith et al. (2014) analyze neurological correlates of asset market behavior using fMRI. They show that aggregate neural activity in the nucleus accumbens (NAcc) tracks overpricing and that aggregated NAcc activity can predict future price changes and crashes. In their study, the lowest-earning traders exhibited a stronger tendency to buy as a function of NAcc activity. They also report a signal in the anterior cingulate cortex (ACC) in the highest earners that precedes the impending price peak and that is associated with a higher propensity to sell. These findings might be related to our experiments, since ACC activation functions can work as an internal “alarm bell” (Smith et al., 2014) that triggers subsequent adjustment, i.e. ACC activation might be a requirement for exerting self-control (Kotabe and Hofmann, 2015).

1.3 Experimental Design

Our paper reports the results from two experiments. The design of Experiment I is described in this section. Experiment II is a natural extension of Experiment I and

Figure 1.1: Treatment Differences in the Stroop Task

described in greater detail in section 1.5. Experiment I consists of four independent parts: (i) instructions and dry runs of the asset market without monetary consequences and without the possibility to build reputation for the parts to come; (ii) the main treatment variation in self control, the Stroop task (Stroop, 1935) in two treatment versions; (iii) elicitation of risk attitudes and cognitive abilities, both incentivized; and (iv) a fully incentivized experimental asset market.

Our identification of the effects induced by a variation in self-control abilities on market prices relies on the comparison of behavior on markets following two different versions of the Stroop task. A tough version lowers self-control abilities, whereas a placebo version should leave self-control abilities largely unaffected. We implement a condition in which all market participants are subjected to the tough version of the Stroop task (henceforth *LOWSC* for low self-control) and a condition in which all participants were subjected to the placebo version (henceforth *HIGHSC* for high self-control). Except for this treatment variation in part (ii), the two experimental conditions are identical in all other parts.

The Stroop task follows a simple protocol: participants are instructed to solve correctly as many problems as possible within five minutes. An example of such a problem is displayed on the left-hand side of Figure 1.1. The task is to select the color of the font the word is printed in. A selection of six color buttons – always the same and in the same order – is given on the bottom right of the screen, and subjects are instructed to click on the correct one. As soon as they make a selection, the next word-color combination appears. Consecutive word-color combinations always differ from each other. The difficulty of this task is that the words always describe one of the six colors; the incongruence between the color of the word and the word itself causes a cognitive conflict, since reading the word is the dominant cue. Common explanations for the conflict are automaticity of reading the word or relatively faster processing of reading than color perception (MacLeod, 1991). The conflict has to be resolved, and resolution requires self-control effort. Applying this effort depletes self-control resources and leaves participants with lower levels of willpower and/or self-control resources after the five minutes.

The Stroop task is one of the most commonly applied methods to deplete self-control resources (Hagger et al., 2010). It can be easily implemented in a computer laboratory, is straightforward to explain, requires only basic literacy skills, and generates additional data on the number of correctly solved problems and the number of mistakes. The difference between the Stroop task in *LOWSC* and *HIGHSC* is the frequency with which a conflicting word-color combination occurrs.[10] All screens in *LOWSC* exhibit such a conflict, while in *HIGHSC* only every 70th screen does. Experimental participants do not receive any information on the frequency of such a conflict, and the instructions for the two versions of the task are identical. By having an occasional word-color incongruence in *HIGHSC* we are able to ensure that subjects take the task seriously. If anything, our setup reduces the potential treatment difference, because in *HIGHSC* some self-control depletion might still take place, making the potential result of a significant difference between the two conditions more difficult to obtain.

We decided to provide participants with a flat payment of € 3 for the Stroop task in order to signal that we were interested in their performance. We do not use a piece-rate or any other competitive payment scheme because it might create different wealth levels after the treatment variation, and wealth differences might be correlated with the treatment. Hence, treatment differences might potentially be confounded with wealth effects.[11] Upon completion of the five minutes, we ask experimental participants how hard they perceived the task on a six-point Likert scale.

Self-control resource depletion can influence several relevant variables for the subsequent experimental asset market. We control for two mechanisms directly: cognitive ability and

[10] The right-hand side of Figure 1.1 shows an example of congruence between font color and word, as we use it in the placebo Stroop task in *HIGHSC*.

[11] Achtziger et al. (2015) find no differences in depletion effects between flat payments and incentivized versions of a related self-control manipulation. We are confident that subjects took the task seriously; only two participants in Experiment I tried less than 114 screens and one answered less than 110 items correctly. Most of our subjects answered many more – see appendix A.3 for details.

risk attitudes.[12] Eliciting control variables takes place after the self-control manipulation but before the experimental asset market for two reasons: Firstly, if these measures were to follow the asset market, there might be spillover effects due to experiences during the asset market and secondly the effect of our self-control manipulation might wear off since the asset market part of the experiment lasts a considerable amount of time during which self-control could start to regenerate (Muraven and Baumeister, 2000). In order to avoid that the self-control variation wears off before the asset market interaction starts, it is a requirement that measuring the control variables does not take much time. Two tasks that fit this requirement are the Cognitive Reflection Test (CRT) for measuring individual cognitive abilities (Frederick, 2005) and a simple multiple price list lottery design for eliciting individual risk attitudes (Dohmen et al., 2011).

First, our subjects answer the three questions of the standard CRT. It is well-known that CRT responses are correlated with more time-consuming measures of cognitive ability, risk and time preferences (Frederick, 2005), as well as with decisions in a wide array of experimental tasks such as entries in p-beauty-contest games (Brañas-Garza et al., 2012) and performance in heuristics-and-biases tasks (Toplak et al., 2011). Furthermore, Corgnet et al. (2014) and Noussair et al. (2014) find that the CRT is a good predictor of individual trader's profits in asset market experiments.[13] Subjects are paid € 0.5 for every correct answer but do not learn their CRT results and thus earnings until the end of the experiment.

Second, we elicit individual certainty equivalents (CE) for a lottery using a multiple price list as a measure for individual risk attitudes. Differences in risk attitudes can be a rational reason for trade (Smith et al., 1988) and might explain initial underpricing of assets on the market, thus sparking off later price increases and overpricing (Porter and Smith, 1995; Miller, 2002). Furthermore, Fellner and Maciejovsky (2007) find that more risk averse individuals trade less frequently. On a single computer screen, our experimental participants have to choose ten times between a lottery that pays either € .20 or € 4.20 with equal probability and increasing certain amounts of money that are equally spaced

[12] For evidence of potential effects of self-control depletion on complex thinking see Schmeichel et al. (2003). As mentioned in the previous section, evidence on the relationship between self-control abilities and risk attitudes is rather inconclusive. Emotions as a potential transmission mechanism will be assessed in Experiment II.

[13] The CRT is regarded as a measure of cognitive ability and thinking disposition (Toplak et al., 2011). We will discuss the CRT results and their implications in more detail when we discuss our results in section 1.6.

between the two outcomes of the lottery. Subjects are allowed to switch at most once from the lottery to the certain amounts. At the end of the experiment, the computer randomly picks one of the ten decisions of each individual as payoff-relevant and implements the preferred option, potentially simulating the lottery outcome.

Immediately after risk elicitation the main part of the experiment, the asset market, opens. The asset market featurs a dividend-bearing asset with decreasing fundamental value over ten trading periods (lasting 120 seconds each) in a continuous double-auction market design with ten traders and with open order books, following Kirchler et al. (2012).[14] This is a simplified version of the markets in Smith et al. (1988). Before the first trading period, five subjects in a given market receive 1000 experimental points in cash and 60 assets, and the other five receive 3000 points in cash and 20 assets as their initial endowment. Assignment to the two initial asset allocations is random.

During each trading period, traders can post bids and asks as well as accept open bids and asks. Partially executed bids and asks continue to be listed with their residual quantities and inactive orders remain in the books until the end of the current period. At the end of every period, the asset pays a dividend of either ten or zero experimental points with equal probability. The dividend payment is added to each trader's cash holdings. Assets have no remaining value after the last dividend payment, i.e. they display a declining (expected) fundamental value. This design feature is explicitly stated and highlighted in the instructions. To make things clear, the instructions provide a detailed table with the sum of remaining expected dividend payments per unit of the asset at any point in time. Assets and cash are carried from period to period. Short selling and borrowing experimental points are not allowed. After every period, the average trading price as well as the realizations of the current and all past dividends are displayed on a separate feedback screen. At the end of the ten periods, experimental points are converted into euros, using an initially announced exchange rate of 500 points = € 1 .

At the end of the experiment, subjects learn about their payoffs from all parts of the experiment. We ask them to fill in a short questionnaire concerning demographics and background data. We also ask participants how tired they feel after the experiment and how hard they have perceived decisions over the course of the entire experiment on a

[14] Appendix A.7 provides the experimental instructions, including a screen shot and a description of the trading screen.

6-point Likert scale. Then, all earnings are paid out in private, and the subjects are dismissed from the laboratory.

Experiment I was conducted in October 2013. 160 participants took part in ten experimental sessions. Hence, we obtained 16 independent observations, eight for each of our treatment conditions. The experiment was programmed using z-Tree (Fischbacher, 2007), and recruitment was done with the help of ORSEE (Greiner, 2015). Experimental sessions lasted for about 90 minutes, and participants earned € 18.18, on average. We only invited students who had never participated in an asset market experiment before. We also excluded students potentially familiar with the CRT or the Stroop task.[15] Prior to the start of the experiment, subjects received written instructions for all parts of the experiment. These were read aloud to ensure common knowledge. Remaining questions were answered in private.

1.4 Experimental Results

1.4.1 Manipulation Check

The data suggest that our treatment manipulation was successful: First of all, during the Stroop task participants attempted fewer problems, achieved fewer correctly solved problems and made more mistakes in the *LOWSC* condition than in the *HIGHSC* condition (all Mann-Whitney tests $p < 0.01$).[16] Participants perceived the Stroop task as significantly more demanding in the *LOWSC* condition than in the *HIGHSC* condition (Mann-Whitney test $p < 0.01$). Finally, we do not find any differences in background characteristics such as field ($p = 0.416$) and year of study ($p = 0.9162$), age ($p = 0.1709$) and gender ($p = 0.9558$) between our two treatments (Mann-Whitney tests and Pearson's χ^2 test for field of study), suggesting that random assignment to treatments was successful.

[15] Of our 160 subjects, one suffered from some form of dyschromatopsia, i.e. a color vision impairment. We asked for it in the post-experimental questionnaire in order to make sure that it is not a common phenomenon.

[16] Detailed distributions on these variables can be found in section A.3 of the appendix. All tests reported in this paper are two-sided unless stated otherwise.

1.4.2 Definitions and Measures

In order to calculate mean prices one can use either an adjustment that takes trading volumes into account (henceforth: volume-adjusted prices) or an adjustment that takes the number of trades into account (henceforth: trade-adjusted prices). The former is an average price per asset, whereas the latter is an average price per trade. Our results remain unaffected by the choice of adjustment; in line with the literature, we mainly display results based on volume-adjusted prices in the following.

In order to quantify the tendency of markets to exhibit irrational exuberance we compare trading prices with the fundamental value of the asset. In the following we adopt the approach of Stöckl et al. (2010) and assess the market price developments using *Relative Absolute Deviation* (RAD) (in equation 1.1) and *Relative Deviation* (RD) (in equation 1.2) as measures for general mispricing and overpricing, respectively.

$$\text{RAD} = \frac{1}{T}\sum_{t=1}^{T}\frac{|P_t - FV_t|}{\bar{FV}} \tag{1.1}$$

$$\text{RD} = \frac{1}{T}\sum_{t=1}^{T}\frac{P_t - FV_t}{\bar{FV}} \tag{1.2}$$

P_t is the volume-adjusted mean price in period t, FV_t is the fundamental value of the asset in period t, and $\bar{FV}$ denotes the average fundamental value of the asset over all periods.

RAD is constructed as the ratio of the average absolute difference of mean market price and fundamental value, relative to the average fundamental value of the asset. RD is the ratio of the average difference between mean market price and fundamental value, relative to the average fundamental value. The difference between the two measures is how the difference between mean market price and fundamental value enters the calculation: For RAD the difference enters in absolute terms, thus all deviations from the fundamental value – overpricing and underpricing – increase RAD, making RAD a measure of average mispricing. For RD the wedge between market price and fundamental value retains its sign, thus periods with overpricing and underpricing can cancel each other out. Hence, RD provides the dominant direction of mispricing, making it, in effect, a measure of average overpricing.

Both measures are straightforward to interpret: A RAD of .1 means that prices are on average 10% *off* the fundamental value, while a RD of .1 indicates that prices are on average 10% *above* the fundamental value. Both measures are independent of the number of periods and the fundamental value.

1.4.3 Aggregate Price Development

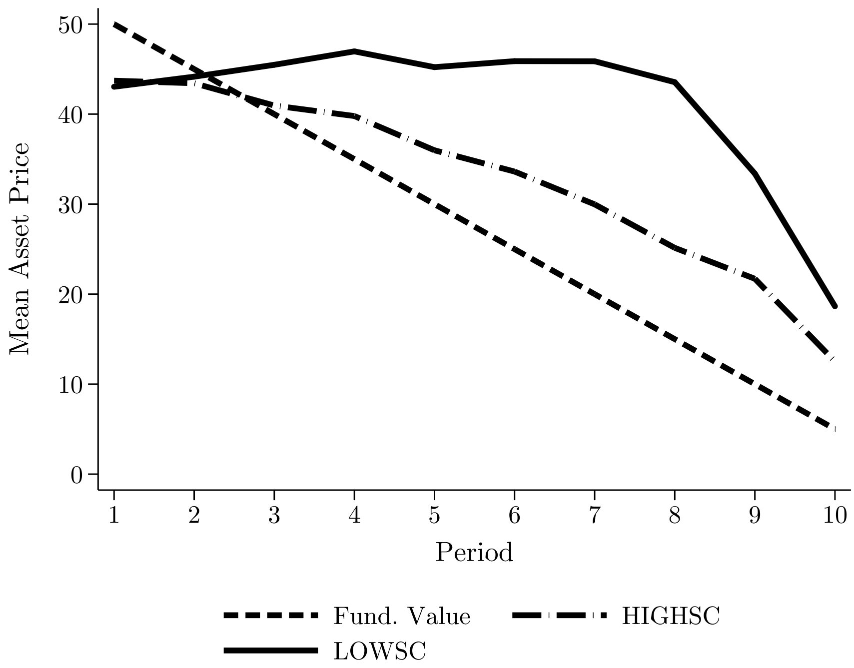


Figure 1.2: Mean (Volume-adjusted) Trading Prices in the Two Treatments

Figure 1.2 shows how average market prices in *LOWSC* and *HIGHSC* evolve over the ten trading periods. In both conditions, average market prices start out at a similar level, displaying a moderate level of underpricing. However, from the third period onwards, average prices in both conditions exceed the fundamental value. Eventually, average market prices drop sharply, but do not drop below the fundamental value again.

The most conservative comparisons between the two treatments are based on market averages over all traders and over all ten periods. This is the approach we apply for all non-parametric tests regarding aggregate market outcomes. These averages are statistically independent in the strict sense, and test statistics are based on eight observations for each treatment. A Wilcoxon signed-ranks test confirms the impression from eyeballing, i.e. that market prices in both conditions are significantly different from the fundamental value (*HIGHSC*: $p = 0.0929$, *LOWSC*: $p = 0.0173$). Figure 1.2 suggests more pronounced overpricing in the *LOWSC* condition than in *HIGHSC*, which is confirmed by a Mann-Whitney test (*HIGHSC*: $\bar{RD} = 0.1885$, *LOWSC*: $\bar{RD} = 0.4990$; $p = 0.0742$)[17]. A comparison of RD tells us that while in *HIGHSC* overpricing is on average 19%, in *LOWSC* prices exceed the fundamental value by almost 50%. Thus, trade among individuals with low self-control leads to overpricing which is more than twice as high as in the baseline *HIGHSC*.

Furthermore markets in the *LOWSC* condition exhibit higher levels of mispricing (*HIGHSC*: $R\bar{A}D = 0.3253$, *LOWSC*: $R\bar{A}D = 0.5890$; Mann-Whitney test: $p = 0.0460$). According to RAD, prices in the *HIGHSC* condition deviate by about 33% from the fundamental value, whereas they deviate by about 59% from the fundamental value in the *LOWSC* condition.

Figure 1.3 displays the price evolution of single markets in the two conditions. There is a high degree of path-dependence and endogeneity in price evolution in the markets and a lot of heterogeneity among markets in the same condition. Therefore, finding a significant difference between the two conditions for the most conservative test in terms of statistical independence is the more striking. The left panel represents the markets from the *HIGHSC* condition, while the right panel shows the *LOWSC* markets. Price paths in *HIGHSC* markets often follow a rather flat or declining development, while in *LOWSC* a number of markets display a hump-shaped price evolution that initially increases and peaks in later trading periods. The emergence of overpricing oftentimes can be attributed to constant prices despite decreasing fundamental values (Huber and Kirchler, 2012; Kirchler et al., 2012) – a description that fits price paths in our *HIGHSC* markets better than those in *LOWSC* markets.[18]

1.4.4 Potential Transmission Mechanisms of the Treatment Effect

Having established a significant treatment effect, the next step is to look at potential channels via which self-control variations could have had an effect on market outcomes. Detailed descriptive results on the variables considered in this section can be found in sections A.4ff. of the appendix.

[17]Both measures are significantly different from zero for both conditions.

[18]Section A.1 in the appendix shows a comparison of overpricing measures across treatments for each period separately. Overpricing in *LOWSC* significantly exceeds overpricing in *HIGHSC* in periods 6-9.

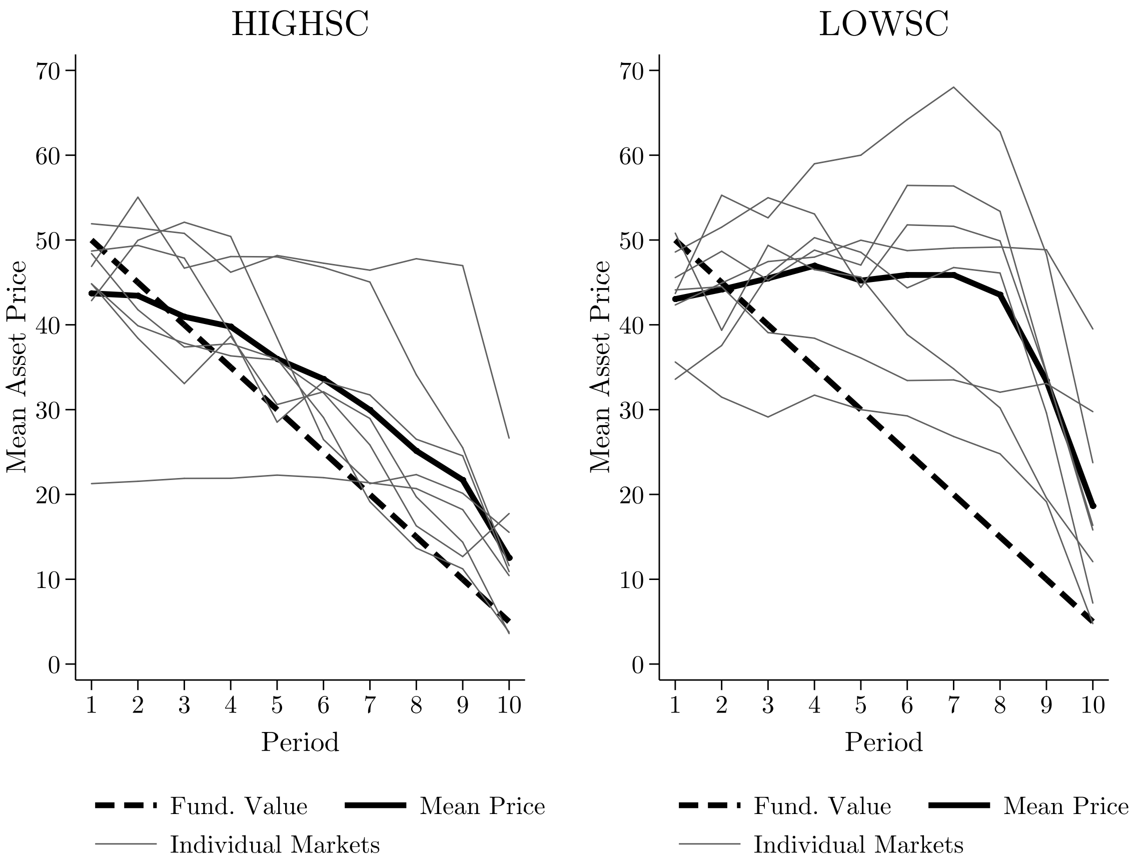


Figure 1.3: Evolution of Individual Market Prices in *HIGHSC* and *LOWSC*

Cognitive Abilities and Risk Attitude

Self-control depleted participants might not be willing to think as hard and thus provide the (wrong) intuitive answers in the CRT. The average number of correct answers in the CRT was 1.05 in *HIGHSC* and 1.14 in *LOWSC*. The difference in CRT score between the two conditions is not significant according to a Mann-Whitney test (p = 0.7223). We conclude that the Stroop task did not have an impact on our incentivized version of the CRT.[19] Risk attitudes might be affected by self-control depletion. The average certainty equivalent we elicited is close to the lottery's expected value: 2.2 in *HIGHSC* and 2.15 in *LOWSC*. Like the literature exploring the effect of reduced self-control on risk attitude that has come to inconclusive results (e.g. Bruyneel et al., 2009; Unger and Stahlberg, 2011; Gerhardt et al., 2015), we also find no significant effect (Mann-Whitney

[19] If we include the observations from our second experiment, the CRT scores of the two groups become 1.0875 and 1.1375 respectively with $p = 0.7442$ from a Mann-Whitney test.

test, $p = 0.4083$) of our treatment variation on risk attitudes as measured by the multiple price list certainty equivalent elicitation.[20]

Trading Activity

An additional channel through which our results could be explained is changes in trading activity, i.e. the number of traded shares per trading period. People low in self-control have been reported to become more passive (Baumeister et al., 1998, Experiment 4). Increased passivity and thus a thinner market in *LOWSC*, where few trades could drive overpricing, could be responsible for our results. Thus we compare the number of shares traded in the two conditions. Figure 1.4 illustrates the evolution of average shares traded per period. Traders in *HIGHSC* traded slightly more overall: while the average trader traded 13.02 shares per period in *HIGHSC*, only 11.39 shares changed hands on average per trader in each period in *LOWSC*. However, according to a Mann-Whitney test, there is no significant difference between amounts traded between the two conditions ($p = 0.3446$).[21] When analyzing the results of Experiment II, we shall take a closer look at trading strategies of self-control depleted traders versus non-depleted traders.

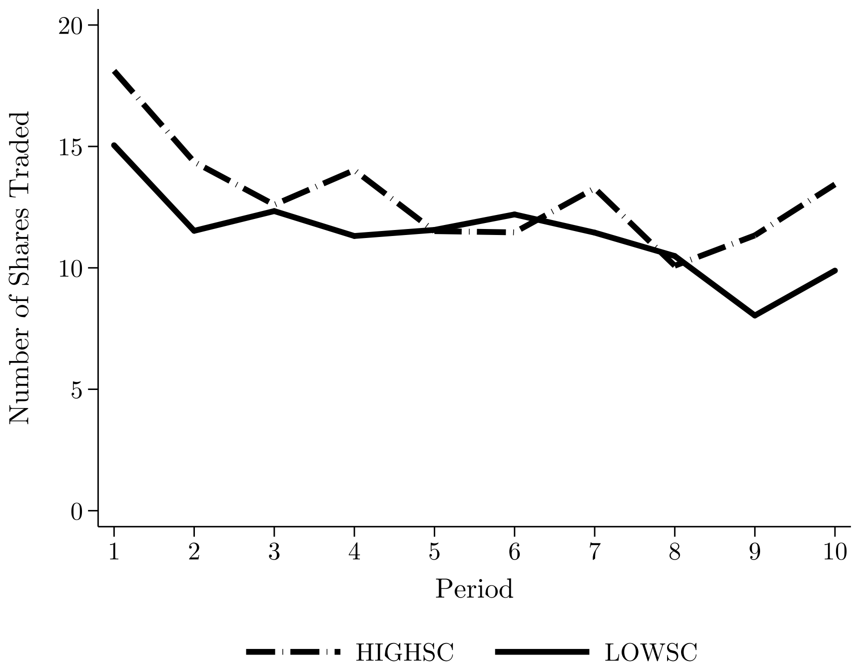


Figure 1.4: Evolution of Average Shares Traded per Trader by Condition

[20]Including observations from Experiment II does not provide significant differences between the two groups.
[21]An additional regression analysis in Table A.2 in appendix A.2 reinforces this conclusion.

Regressions Controlling for Potential Channels

Although our control variables seem unaffected by our treatment, they could still possess explanatory power for the difference in overpricing that we observe. We therefore run regressions, including controls as indepedent variables. To avoid endogeneity problems across trading periods and between subjects, respectively, we aggregate overpricing measures over all ten periods on the individual level and use robust standard errors clustered at the market level. We do this separately for sales and purchases, since selling above fundamental value results in an expected profit, while buying above fundamental value results in an expected loss. We define measures for individual overpricing for purchases and sales, which we call $IndRD_{purchases}$ and $IndRD_{sales}$, respectively. Similar to the measure RD they are defined as the percentage of buying (selling) prices exceeding the asset's fundamental value pooled over all periods, but for each subject's buying (selling) activity separately instead of on the market level as before. We report results on $IndRD_{purchases}$ as the dependent variable in the regressions in Table 1.1. In appendix A.2, we provide robustness checks for our chosen approach for sales and both aggregated sales and purchases.

In all four models we are interested in the effect of the explanatory variables on $IndRD_{purchases}$, our measure of an individual's overpricing tendency. Throughout all specifications, we observe a significant treatment effect: Being in *LOWSC* increases an individual's propensity to buy at excessive prices. In specification (2), our measure of risk attitude is not significant, but if we also include interactions with our treatments in specifications (3) and (4), relative risk seeking is correlated with lower individual overpricing when self-control capabilities are reduced. Performance on the CRT has the expected effect of reducing the tendency of buying at prices above fundamental value in all specifications where it is included, and its effect does not significantly differ between participants in *LOWSC* and *HIGHSC* markets. The models show results based on an exclusion criterion for the CRT. We excluded subjects that indicated knowledge of CRT questions in the post experimental questionnaire. Our results are very similar without excluding those subjects. Hence, introducing measures for risk aversion and cognitive skills and their interactions with our treatments do not reduce the size or significance of the treatment coefficient. We conclude that neither changes in cognitive skills nor in risk preferences after self-control depletion can explain our main result of excess overpricing after self-control depletion.

Table 1.1: Determinants of Individual RD Based on Purchases

	(1)	(2)	(3)	(4)
		$IndRD_{purchases}$		
LOWSC	0.400**	0.390**	0.816***	0.843***
	(0.140)	(0.134)	(0.131)	(0.125)
CRT		-0.0708*	-0.0952	-0.0912
		(0.0392)	(0.0558)	(0.0547)
CE		-0.0188	0.0684	0.0719
		(0.0459)	(0.0441)	(0.0455)
CRT × *LOWSC*			0.0612	0.0628
			(0.0821)	(0.0831)
CE × *LOWSC*			-0.224***	-0.237***
			(0.0712)	(0.0709)
Female				0.0666
				(0.0690)
Constant	0.0933	0.194	0.0255	-0.0353
	(0.0971)	(0.120)	(0.0597)	(0.0682)
Observations	110	110	110	110
R^2	0.275	0.307	0.364	0.370

Note: OLS regression, dependent variable is Individual Relative Deviation (IndRD) for purchases, an individual equivalent to market level Relative Deviation (RD) restricted to purchases only. *LOWSC* is a dummy where 1 stands for *LOWSC* and 0 for *HIGHSC*. CE is an individual's certainty equivalent. CRT denotes the number of correct answers on the CRT. Subjects who indicated they knew one or more of the CRT questions before were excluded. Heteroskedasticity robust standard errors clustered at market level in parentheses, *** p<0.01, ** p<0.05, * p<0.1

1.5 Experiment II: Mixed Markets

1.5.1 Motivation and Design

The results reported in section 1.4 referred to markets, in which either all market participants underwent the tough Stroop task or none of them, i.e. either everyone's self-control resources had been reduced or no one's. In this section we report results from markets, in which only half of the participants' self-control resources were depleted. Each market consisted of five participants randomly assigned to the easy (placebo) Stroop version from the *HIGHSC* condition and five participants randomly assigned to the tough Stroop version from the *LOWSC* condition. We call this new condition *MIXED* and for simplicity refer to traders facing the tough version of the Stroop task as *MIXLO* traders and to those facing the easy version of the Stroop task as *MIXHI* traders. The motivation for this additional experiment is twofold. First, asset market experiments are zero sum games and behavior is highly path-dependent and endogenous to market prices, which makes it technically impossible to analyze differences in behavior resulting from reduced self-control in our homogeneous markets. Therefore, we wanted a condition in which traders under both conditions are active at the same time. It allows us to assess differences in trading behavior and performance between *MIXLO* traders and *MIXHI* traders. Second, since in real-world settings – either due to dispositional differences or due to differential previous demands on self-control resources – it is likely that individuals high and low in self-control interact, we want so see whether the effect of reduced self-control observed in *LOWSC* markets can be replicated with a smaller share of depleted traders in *MIXED* markets.

We conducted eight additional sessions with 16 markets in April 2014 and November 2015. In the last four sessions we added several questions to the experimental questionnaires dealing with participants' emotions. We were interested whether our variation of self-control had taken effect via changes in emotional states. In order to reduce experimenter demand effects and as is common in experiments analyzing emotions, we confronted subjects with several emotions of which some were not relevant at all to our question of interest. Apart from the assignment to the respective version of the Stroop task within a market and the additional questions in the questionnaires of the last four sessions, the experimental protocol remained exactly the same as in Experiment I. Experimental participants were not aware of the different versions of the Stroop task, i.e. they were unaware of the fact that half of the traders performed the tough version and half of the traders the easy version.

1.5.2 Aggregate Price Evolution

Figure 1.5 shows the evolution of average trading prices in all three treatments of Experiment I and II. Interestingly, the effect of reduced self-control on mispricing and overpricing does not seem to be changed if only part of the trader population is self-control depleted. Both *LOWSC* and *MIXED* on average display more overpricing than *HIGHSC*.

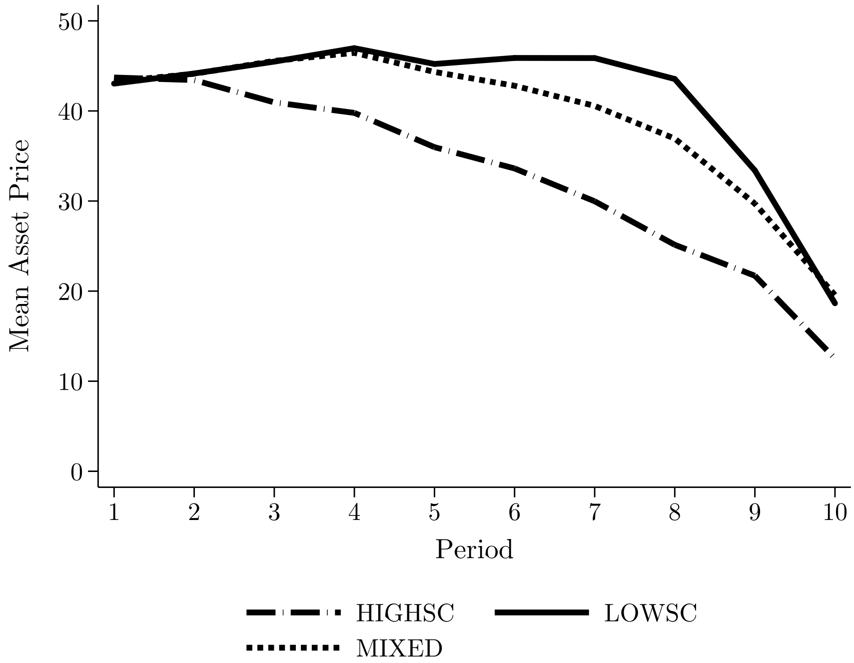


Figure 1.5: Trading Price Evolution Including *MIXED*

For *MIXED* we observe an average RAD of 0.551 and an average RD of 0.430. A Mann-Whitney test confirms that the mispricing measure RAD in *MIXED* is significantly different from *HIGHSC* ($p = 0.0500$) but cannot be statistically distinguished from *LOWSC* ($p = 0.8065$). This result also holds for our overpricing measure: RD in *MIXED* differs significantly from *HIGHSC* ($p = 0.0864$), but not from *LOWSC* ($p = 0.5006$).[22]

Figure 1.6 illustrates the evolution of mean trading prices for the 16 individual markets in the *MIXED* condition. Qualitatively, we get similar results as in *LOWSC*. That is, in some of these markets prices exhibit a hump-shaped development, initially increasing and peaking in some intermediate period. Thus already the presence of a moderate share of traders with depleted self-control abilities is sufficient to reproduce the excess overpricing we observed when all traders' self-control levels were depleted.

1.5.3 Differences in Trading Behavior and Outcomes

Trading Behavior

Differences in market outcomes in the *MIXED* condition compared to *HIGHSC* markets must result from different actions of *MIXLO* traders. However, when analyzing trading

[22] The results of these comparisons also hold when looking at quantity- or trade-adjusted mean prices.

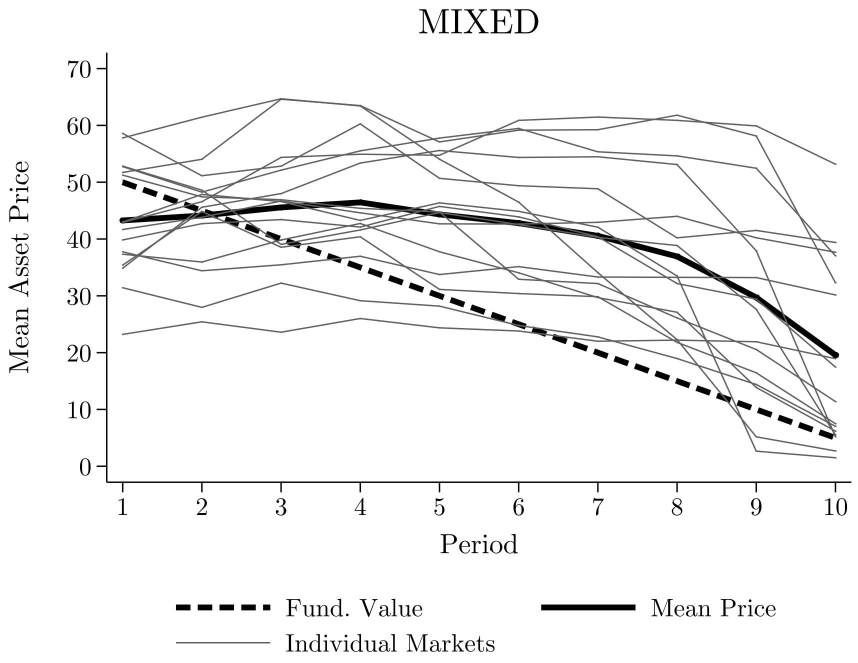


Figure 1.6: Price Evolution in Individual Markets in *MIXED*

behavior, distinguishing cause and effect is particularly difficult, as already mentioned earlier. A particular deviation in behavior by some traders in the early phases of a market might shift behavior of other (non-depleted) traders. We therefore start by focusing on the very first trading period, where dependencies are less relevant than in later periods. Table 1.2 compares several variables concerning trading activity between *MIXLO* and *MIXHI* traders. Remember that we conduct all statistical tests based on the most conservative definition of independence (the market level), and hence significant effects are usually associated with large absolute differences.

According to Wilcoxon signed-rank tests *MIXLO* traders make significantly lower bids initially ($p = 0.035$) and post these bids earlier than their non-depleted peers ($p = 0.017$). They are also quicker in posting their first bid at the beginning of the period ($p = 0.048$). While not significant, there also seems to be the tendency that *MIXLO* traders (while bidding low) ask for a higher price than the *MIXHI* traders ($p = 0.196$). After period one, these differences vanish, suggesting that non-depleted traders start imitating the behavior of self-control depleted traders.[23] The averages in Table 1.2 suggest an initially stronger activity of *MIXLO* traders, trying to buy lower and sell higher than *MIXHI* traders. From

[23]Results for period two are reported in table A.8 of the appendix indicating that these initial trading differences disappear, while *MIXLO* traders display significantly higher asking prices in period 2.

Table 1.2: First Period Differences in Trading Behavior

	Group Mean		
	MIXHI	*MIXLO*	p-value
$\overline{p_{bid}}$	36.377	28.487	0.035**
$\overline{p_{ask}}$	49.931	54.478	0.196
$\overline{q_{bid}}$	16.109	17.788	0.660
$\overline{q_{ask}}$	14.389	15.202	0.796
$\overline{time_{bid}}$	60.425	47.000	0.017**
$\overline{time_{ask}}$	50.230	50.383	0.796
$\overline{firsttime_{bid}}$	51.517	39.846	0.048**
$\overline{firsttime_{ask}}$	34.635	34.435	0.959

Note: Variables starting with a *p* denote prices, *q* quantities and time variables refer to the time passed in the current period, thus lower values indicate behavior earlier on. *bid* and *ask* refer to posted bids and asks, p-values from Wilcoxon signed-rank tests with data collapsed on market and treatment level, *** p<0.01, ** p<0.05, * p<0.1

trading period two on, however, their behavior has incited non-depleted traders to behave similarly and hence set many markets on an entirely different trajectory.

Table 1.3: Rank Correlations of First Period Behavior with Overpricing

	ρ	p-value
$\overline{p_{bid}}$	0.436	0.104
$\overline{p_{ask}}$	0.488	0.055*
$\overline{q_{bid}}$	0.486	0.066*
$\overline{q_{ask}}$	-0.229	0.393
$\overline{time_{bid}}$	-0.607	0.016**
$\overline{time_{ask}}$	0.262	0.327
$\overline{firsttime_{bid}}$	-0.421	0.118
$\overline{firsttime_{ask}}$	0.079	0.770

Note: Rank correlations of average first-period behavior over all market participants with average relative deviation over periods 2-10 for *MIXED* markets, *** p<0.01, ** p<0.05, * p<0.1

Table 1.3 presents evidence that the observed differences in first period behavior between our treated and non-treated traders are also those behaviors that are correlated with later overpricing. While Table 1.2 has shown that low-self control traders bid earlier in period one and also post their first bid significantly earlier, Table 1.3 shows that markets in which bidding occurs early in period one, are those that exhibit more overpricing over the course of the experiment.

Profits

On average, *MIXLO* traders earned € 8.16, and *MIXHI* traders earned € 7.84 on the experimental asset market – a difference that is not significant (Wilcoxon signed-rank test, $p = 0.9794$). We consider this as evidence that inhibited self-control abilities affect overpricing, but that depleted traders are not necessarily driven out of the market. Instead, as shown previously, they might goad non-depleted traders into speculative behavior, making everyone end up with similar profits. While this suggests that a lack of self-control abilities is not necessarily detrimental to trading performance, it shows how negative the effect can be for markets on which traders potentially imitate each other's behavior.

1.5.4 Increased Emotional Reactivity

In the experimental sessions that we conducted in November 2015, we asked participants a number of questions relating to their emotional experience during the asset market. In particular, we asked participants to rate how strongly they felt a number of emotions at the beginning of the first period and at the end of the last period, respectively. We asked participants at the end of the experiment, requiring them to recollect their emotions.[24]

Table 1.4 reports the results for those emotions that have previously been connected to overpricing in experimental asset markets (Hargreaves Heap and Zizzo, 2011; Andrade et al., 2015; Lahav and Meer, 2012; Breaban and Noussair, 2013). Note that we collapsed all the emotional measures on the treatment group level within each market and test for differences with Wilcoxon signed-rank tests. Strikingly, the intensity of every single measure of experienced emotions is higher in the *MIXLO* than in the *MIXHI* group, with many measures being statistically significant. At the beginning of period 1, *MIXLO* participants report to feel borderline significantly more surprise ($p = 0.103$) and significantly more joy ($p = 0.058$). Remember that Lahav and Meer (2012) found that inducing positive mood before trading leads to higher deviations from fundamental values and thus

[24]We also provided participants with a questionnaire regarding their trading behavior which we do not report here. The average responses to all the emotion-related questions and the test statistics can be found in Table A.4 of the appendix. Average values for changes in emotions over time can be found in Table A.5.

larger levels of overpricing and that correlational studies also suggest such a relationship (Breaban and Noussair, 2013; Hargreaves Heap and Zizzo, 2011). Furthermore, at the end of the final trading period, *MIXLO* traders report significantly higher levels of excitement, fear and surprise than *MIXHI* participants (all $p < 0.05$).

We also asked participants in the post-experimental questionnaire explicitly about how strongly they felt their behavior was driven by emotions and how much they had tried to suppress the influence of emotions on their trading behavior (see final panel of 1.4). Even though the difference in averages goes in the expected direction, given the responses to the questions on experienced emotions, they fail to reach significance on conventional levels. The results indicate that the behavior of the traders with depleted self-control abilities might have been driven by emotional factors to a larger degree than they were aware of themselves.

Table 1.4: Ex-post Reported Emotions of Traders in *MIXED*

	MIXHI	MIXLO	p-value
Beginning of the First Period			
Excitement	4.200	4.500	0.400
Fear	2.100	2.175	0.395
Surprise	3.600	4.050	0.103
Joy	3.625	4.375	0.058*
End of the Last Period			
Excitement	3.425	4.200	0.042**
Fear	1.900	2.575	0.014**
Surprise	2.450	3.400	0.030**
Joy	3.375	4.125	0.207
Self-Evaluation of Emotional Reactivity			
Emotion driven	2.475	2.725	0.362
Suppressed emotions	5.300	4.950	0.205

Note: Data collapsed on the treatment level per market; responses were on 7 point Likert scales; test results from Wilcoxon Signed Rank tests; *** p<0.01, ** p<0.05, * p<0.1

1.5.5 Reduced Cognitive Control

Experiment I did not show a direct effect of the Stroop task on incentivized CRT performance. Condition *MIXED* gives us the possibility to look at the issue again, in particular at the association between CRT, the treatment (*MIXLO* and *MIXHI*), and performance in terms of profits.

Previous research has shown that CRT scores correlate positively with individual participants' profits in similar experiments (Corgnet et al., 2014; Noussair et al., 2014). Toplak et al. (2011) find that CRT scores are correlated with measures of cognitive ability, thinking disposition and executive functioning. Thus, we can interpret the CRT score as a measure of cognitive control. In order to check whether the effect of CRT performance on profits is similar here, we ran additional regressions which we report in table 1.5. Note that we excluded participants who had indicated that they knew at least one of the CRT questions at the end of the experiment. The knowledge of CRT questions before the experiment might have driven up correct CRT responses and might thus obfuscate any interaction effects between treatment and CRT scores.[25]

In specification (1) we reproduce the finding that there is no statistically significant difference between the profits of traders in *MIXLO* and *MIXHI*. Specification (2) confirms findings from earlier studies that higher CRT scores are positively related to higher overall profits for both *MIXLO* and *MIXHI*. However, when we separate this effect by treatment by including an interaction of the *MIXLO* dummy with the CRT score, we obtain a larger effect of the CRT score on profits for *MIXHI* traders, while for *MIXLO* traders the effect of CRT scores on profits is significantly smaller ($p < 0.05$) and in fact cannot be distinguished from zero overall (post-estimation Wald test, $p = 0.43$).

Thus, *MIXLO* subjects' trading seems to be relying less on their underlying ability for cognitive control. Together with the results indicating higher emotional valence and reactivity, this suggests an interpretation of trading behavior of *MIXLO* participants as relatively more relying on impulsive system 1 processes than on reflective system 2 processes (Kahneman, 2011).

1.6 Discussion

We observe a strong main effect of self-control depletion on overpricing in both experiments. The difference in overpricing cannot be explained by a change in risk attitudes or a simple change in cognitive abilities. Experiment II gives us additional power to assess potential explanations for the excess overpricing after self-control depletion.

[25] 72 subjects in *MIXED* markets reported to know at least one of the CRT questions.

Table 1.5: Determinants of Profits in *MIXED*

	(1)	(2)	(3)	(4)
	Profit			
MIXLO	1.036	1.040	4.342*	4.301*
	(0.770)	(0.795)	(2.222)	(2.215)
CRT		1.084**	1.882***	1.757**
		(0.497)	(0.621)	(0.691)
CE		0.473	0.867	0.685
		(0.550)	(0.768)	(0.753)
CRT × MIXLO			-1.660**	-1.547**
			(0.642)	(0.690)
CE × MIXLO			-1.031	-1.051
			(1.125)	(1.098)
Female				-1.381
				(0.888)
Constant	7.035***	5.302***	3.936***	5.326***
	(0.441)	(1.097)	(1.323)	(1.638)
Observations	88	88	88	88
R^2	0.016	0.079	0.120	0.145

Note: Participants who indicated to know at least one of the CRT questions excluded; robust standard errors clustered on the market level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

First, there are differences in trading behavior. Self-control depleted traders trade slightly less on average, and their initial trading behavior shows potential patterns of speculative trading. For instance, the fact that self-control depleted traders post bids significantly earlier supports the notion that their behavior is driven by a higher degree of impulsivity than the behavior of non-depleted traders. In an environment in which early activity and speculation is potentially imitated by others on the market, not much is needed to set a market on an overpricing trajectory. Notably, trading behavior is strongly path-dependent in experimental asset markets, and the evolution of prices follow different forms and different timings on different markets. We could have presented additional empirical evidence for effects of self-control depletion on trading behavior and trading strategies, but such evidence requires assumptions that are somewhat arbitrary. Hence, we decided to present fewer analyses and only those in whose robustness we are confident.

Second, there are differences in the reported intensity of emotions and relevance of emotions. Due to existing findings, initial differences before the opening of the asset market (and after the Stroop task) are one channel via which depleted self-control could have affected overpricing. Apart from the pre-market emotional state, differential emotional reactions during the market could be driving our results. Emotion regulation has been shown to draw on self-control resources (Baumeister et al., 1998; Hagger et al., 2010). We have evidence that participants displayed more intense emotional states, in particular at the end of the asset market. We interpret our treatment effect as the result of an increased sensitivity towards emotions triggered by self-control depletion. Our effect is in line with the literature on self-control depletion. For example, Bruyneel et al. (2006) have shown that people whose self-control has been reduced rely more on affective and less on cognitive features in product choice. Similarly, in our setting traders with low self-control levels could rely more heavily on affective features of the asset, e.g. the thrill from its recent price increase or from speculation, than on cognitive features, e.g. the knowledge that the fundamental value of the stock is decreasing. Thus emotional responses could be responsible for more myopic decision making, a higher level of overconfidence/overoptimism (Michailova and Schmidt, 2016), and more speculative trading.

Third, cognitive abilities could be different after the two versions of the Stroop task. However, the issue is not as straightforward as we expected. For our sample, we cannot provide evidence on a direct impact of the treatment on CRT performance. This might be because the monetary incentives to do well in the CRT are relatively high, and it is well-known that people can temporarily overcome self-control problems if the motivation is sufficient (Muraven and Slessareva, 2003; Vohs et al., 2012). However, there is evidence in our data for an indirect effect of self-control depletion on cognitive abilities. We find that the CRT carries predictive power for traders' profits, but only if their self-control has not been depleted previously.

There are additional explanations that we cannot pin down fully and have to leave for verification in future research. Self-control depleted traders, for instance, report significantly higher levels of surprise after the last period of the market. This could be an indication for a reinforcement of myopic behavior when being self-control depleted. Another candidate explanation for our treatment effect is a problem to stop, i.e. to sell early enough and not to stick too long to the expectation of a future price rise. Self-control depletion could lead to a reluctance to sell an asset whose price is rising. Similarly, it could lead to undue overoptimism.

1.7 Conclusion

In this paper, we provide causal empirical evidence for the notion that a lack of self-control can fuel overpricing on asset markets. We consider experimental continuous double auction markets for which Smith et al. (1988) first reported a tendency for overpricing. We exogenously reduce market participants' ability to exert self-control using a tough version of the Stroop task, which has previously been shown to deplete people's ability to exert self-control in subsequent tasks (Baumeister et al., 1998). Comparing two market settings in which either everyone's or no one's self-control was reduced, we observe significantly more mispricing and overpricing as the result of a reduction in self-control abilities than without this reduction.

Self-control depletion affects trading behavior and the perception of the trades and market outcomes. We provide evidence that in markets populated by self-control depleted and non-depleted traders initial trading strategies of the former show more signs of speculative behavior than of the latter. However, the evidence is not entirely conclusive. Trading is path-dependent on experimental asset markets, and it is difficult to pin down the exact reasons for overpricing to emerge without making arbitrary assumptions. We do not observe a performance difference between traders with depleted self-control and traders with full self-control abilities, suggesting that low self-control traders might not be driven out of the market, but rather incite other traders to engage in speculative trading. In addition, we have evidence for an emotional channel that explains our main result. Self-control depleted traders show stronger emotions, in general, but in particular stronger emotions that have been linked to overpricing in previous studies that induce emotions or that measure emotions while trading. Finally, we find that our measure for cognitive skills loses predictive power for the profits of low self-control traders. This might indicate that even though cognitive skills seem unaffected by self-control depletion (as are risk attitudes), different cognitive processes play a role in traders with low self-control levels. These results are in line with a dual systems perspective of self-control: self-control depleted participants seem to have acted more on the basis of emotions and less on the basis of cognition, thus driving up prices.

Our findings have relevant implications: First, with differences in self-control levels, we add a potentially important explanation to the existing explanations for overpricing on asset markets. We have shown that already a moderate number of participants with low self-control levels are sufficient to more than double the extent of overpricing in terms of relative deviation from fundamental value. Second, our results can be regarded as indicative of the role of self-control in markets outside the laboratory – there, both temporary reductions in self-control as well as the personality trait self-control might play an important role in determining trading behavior and perception. Self-control might also be an important attribute on which individuals self-select into trading. However, low self-control traders might not be as easily exploitable by high self-control traders as one would think. In our case, they would not have been driven out of the market quickly. Several practical implications of our results for investing and trading activities come to mind. Given our findings, investment decisions should not be taken under limited self-control or willpower conditions. For instance, cognitive load, food or sleep deprivation, and self-control effort in unrelated domains have been shown to be correlated with limited self-control abilities. If such conditions are unavoidable, decision aides to sustain self-control such as commitment devices should prove useful to circumvent the potentially negative consequences. This might be particularly relevant in fast-paced markets.

Our experiment opens up interesting paths for future research: It would be interesting to see to what extent our results are robust to changes in alternative market mechanisms such as call markets and to changes in the fundamental value process such as a constant fundamental value process, which has been shown to reduce overpricing (Kirchler et al., 2012). Finally, the role of self-control for traders in markets outside the laboratory remains largely unexplored. One can imagine field experiments or using quasi-experimental variations of self-control abilities to study decisions of traders on real markets.

CHAPTER 2

The Impact of Self-Control on Investment Decisions

2.1 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

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

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.

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. (2005a) 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. (2016a), 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 2.2 takes a deeper look at the literature related to the current studies both from economics and psychology, section 2.3 considers experiment 1, while section 2.4 covers experiment 2. I discuss the findings from both experiments in section 2.5 and conclude in section 2.6.

2.2 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.

2.2.1 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 model of Gul and Pesendorfer (2001),

[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).

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.

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

2.2.2 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: 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 criticism 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.

[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.

2.3 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.

2.3.1 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 indirect evidence that (lack of) self-control plays an important role for the disposition effect.

2.3.2 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.

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.

[5]A translation of the instructions can be found in appendix B.3.

(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 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 2.1 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 2.1: Examples of Classifications in the Letter-E-Task

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

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

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

[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.

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.

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.

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.

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

[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 B.1 for a more extensive explanation of these measures and for the rationale behind including them.

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).

2.3.3 Results

Table B.3 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]

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

[9] The times of each session are summarized in table B.2 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.

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:

$$PGR = \frac{\text{\# of sales at gain}}{\text{\# of selling opportunities at gain}} \tag{2.1}$$

$$PLR = \frac{\text{\# of sales at loss}}{\text{\# of selling opportunities at loss}} \tag{2.2}$$

$$DE = PGR - PLR \tag{2.3}$$

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 2.1 shows the DE measures including 95% confidence intervals for each treatment. Table 2.2 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 2.2 compares the individual DE measures of the two participants that saw an identical

[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).

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 2.2 would lie to the right of the 45° line more frequently, which is not the case. Wilcoxon signed-rank (WSR) tests reported in table 2.3 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 B.5 of the appendix, as well as MWU tests for subgroups in table B.6 and table B.7 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.

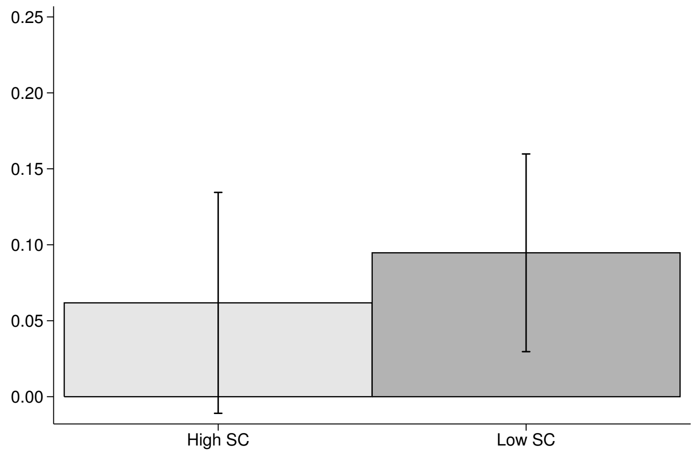


Figure 2.1: Disposition Effect Measure by Treatment

Table 2.2: 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 $H_0 : p(DE > 0) = 0.5$; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[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.

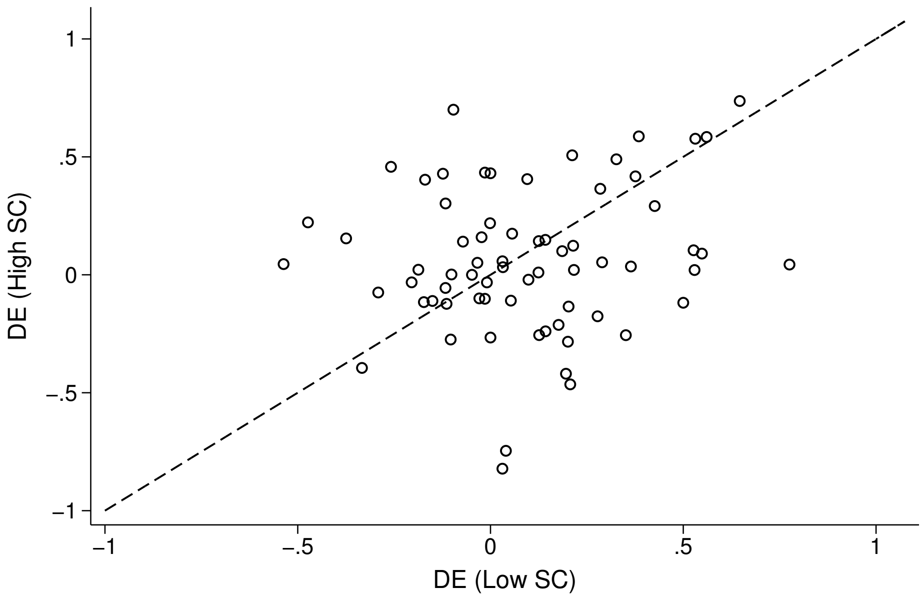


Figure 2.2: Paired Disposition Effect Measures across Treatments

Table 2.3: 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

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 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., 2016a) 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:

$$TC = 1 - \frac{\#\text{ of trading periods}}{\#\text{ of distinct trades}} \tag{2.4}$$

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 2.3 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 2.4 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

Table 2.5 displays results from MWU tests, where participants have been split into three groups, according to their CRT responses, following the classification suggested in Cueva

[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 B.1. 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.

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 2.6 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 2.7. 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

[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.

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.

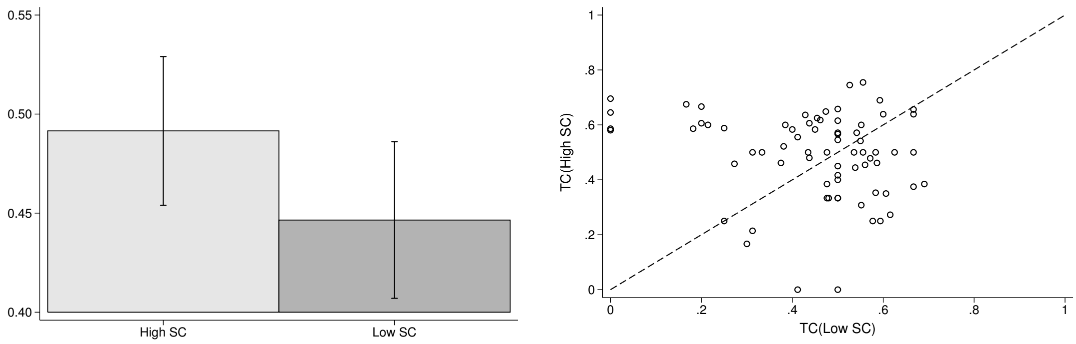


Figure 2.3: Trade Clustering Measures across Treatments

Table 2.4: 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 2.5: 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 2.6: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Effect on Additional Measures

Finally, in line with the results in Kocher et al. (2016a) 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]

[17] Appendix B.1 analyzes the impact of the self-control manipulation on these additional measures in more depth.

Table 2.7: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

2.4 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) 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.

2.4.1 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 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

[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).

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.

2.4.2 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]

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

[20] A translated version of the instructions can be found in appendix B.3.

[21] See appendix B.2 for the wording and correct answers of these questions.

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.

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; Stanford et al., 2009) and a number of socio-economic background measures. Please refer to appendix B.2 for detailed descriptions of these tasks and the rationale for including them.

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).

[22]Refer to table B.9 of the appendix for the timing of each session.

2.4.3 Results

The manipulation checks – reported in table B.10 of the appendix – yield very similar results as in experiment 1. Furthermore, appendix B.2 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, $p = 0.103$).

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 2.4 displays the investment levels for the four treatments including 95% confidence intervals and table 2.8 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* and *Low SC* within each investment frame (MWU, $p = 0.425$ and $p = 0.557$ for *Narrow* and *Broad* respectively).

Heterogeneity:

In appendix B.2, 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 B.2, 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]

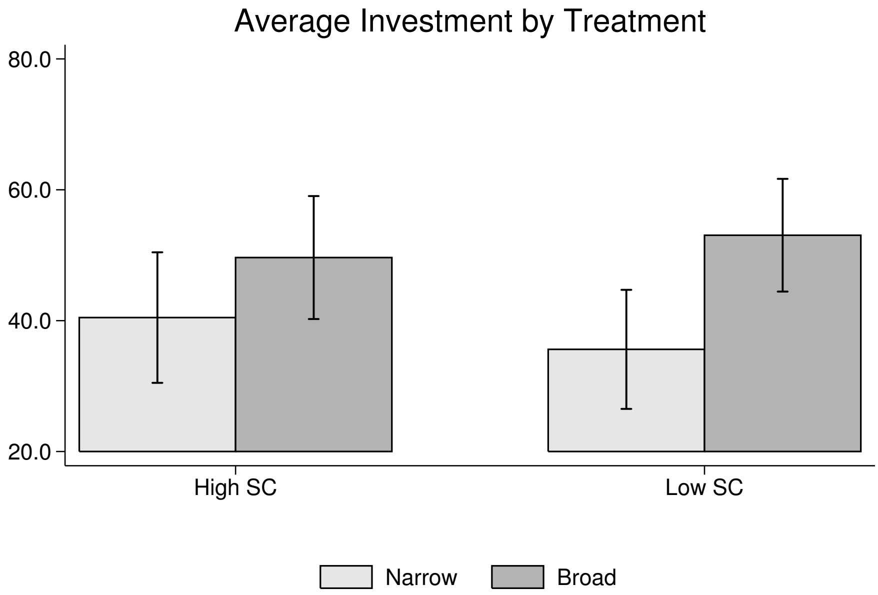


Figure 2.4: Average Investment by Treatment Condition

Table 2.8: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Dynamics of Investments

Figure 2.5 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* $\times$ *Narrow* treatment were the only ones to reduce their investment levels over the course of the experiment.

[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.

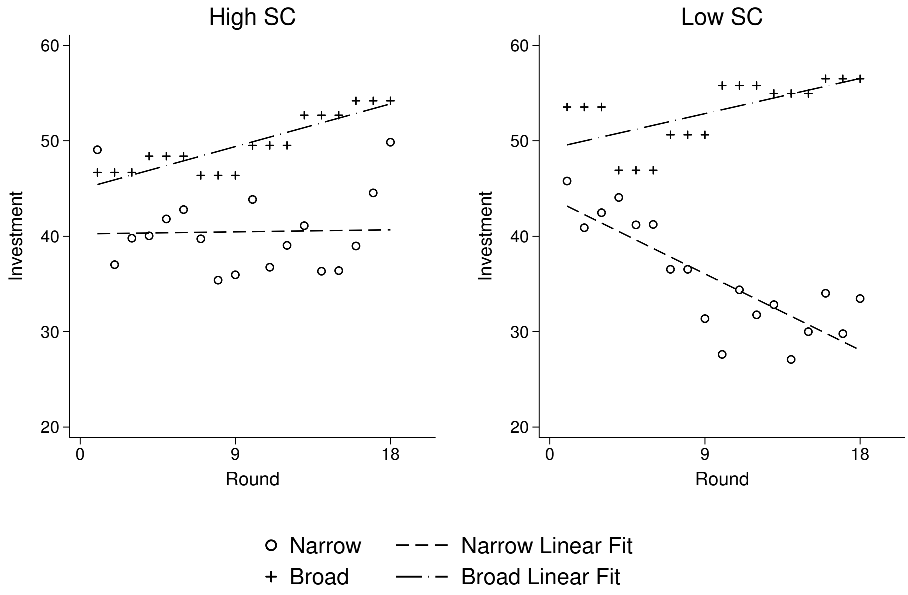


Figure 2.5: Average Investment per Round by Treatment Condition

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 2.9. 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.

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

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] I report the same set of regressions separately for each investment frame in appendix B.2. In line with figure 2.5, 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.

2.5 Discussion

2.5.1 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

[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).

[26]Figure B.1 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 2.9: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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.

2.5.2 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. (2016a). 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 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 ($p = 0.103$).

2.5.3 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 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. (2016a). 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

[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.

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 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. (2016a). 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.

2.6 Conclusion

Even though the effects on individual investment decisions seem to be relatively small in the present paper, the results in Kocher et al. (2016a) 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. (2016a) 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.

CHAPTER 3

Peer Effects in Risk Preferences among Adolescents[1]

3.1 Introduction

Taking risks is an important part of teenagers' everyday life. There is a concern that teenagers take excessive risks and also that their risk-taking is exacerbated by peer interaction.[2] This paper develops an experimental strategy to assess the importance of peer effects in risk- and loss-taking among adolescents.

A fundamental challenge to the identification of peer effects is unobserved endogenous selection into social relationships. Laboratory experiments on social interaction address this concern by randomly allocating subjects to peer groups. One criticism of this approach is that "(...) the groups whose interactions are observed are formed artificially for the sake of the experiment. This raises obvious questions about the credibility of extrapolating findings from experimental settings to populations of interest." (Manski, 2000) Carrell et al. (2013) present empirical evidence supporting this view. They show that social interaction patterns can vary substantially in spite of a random assignment process due to endogenous friendship formation after the assignment. In this paper, we

[1]This chapter is based on joint work with Melanie Lührmann and Joachim Winter.

[2]Strong peer influences among adolescents and youth have been found in several domains. Sacerdote (2001) emphasizes their influence on university students' educational performance, Card and Giuliano (2013) highlight social interaction effects in risky behaviours such as sexual initiation, smoking, marijuana use, and truancy (for similar results, see also Arnett, 1992; Brown et al., 1986; McPhee, 1996)). Gardner and Steinberg (2005) argue that peer pressure in risk-taking is larger in adolescence than in adulthood. Psychologists suggest that "risk taking [in adolescence] is the product of a competition between the socio-emotional and cognitive-control networks, and adolescence is a period in which the former abruptly becomes more assertive (i.e., at puberty) while the latter gains strength only gradually, over a longer period of time" (Steinberg, 2007). Hence, increased susceptibility to peer influences may arise from the heightened role of socio-emotional networks in the adolescent brain (cf. Galvan et al., 2006, 2007; Steinberg, 2008; Van Leijenhorst et al., 2010; Reyna et al., 2011). Additionally, adolescence is a period of increasingly independent decision-making, in which the influence of (more risk-averse) parents on choices weakens and the influence of same-age peers becomes stronger.

randomly match individuals with *naturally occurring* peers; these are either randomly selected classmates or friends. In previous experimental studies, social interaction usually took place between random, anonymous peers (e.g. in Lahno and Serra-Garcia, 2015), or between participants with no common history (e.g. in Falk and Ichino, 2006).

Non-experimental approaches often rely on data on endogenous peer relations alone to account for selective peer interaction. These studies face the challenge that "true social interaction effects are difficult to distinguish from unobserved background factors that are correlated across friends." (Card and Giuliano (2013); a similar point is made by Manski (1993) and Moffitt (2001)). This also applies to the natural peers in our experiment who have been previously assigned to classrooms and have already formed friendships (or not). While subjects are randomly exposed to peer interaction in our experiment, unobserved background factors may have led to assortative matching of friends and classmates. We thus randomise individuals into controlled social interaction conditions – among randomly assigned natural peers –, and collect additional data on peer relations in the control group. First, this enables us to analyse whether subjects match assortatively on risk preferences which may lead to a strong positive correlation in choices.[3] Second, it allows us to distinguish between peer similarity that arises through assortative matching and the impact of social interaction in the choice situation.

We conduct an experiment among adolescents in the age range of 13 to 15 years that attend the lowest tier of the German high school system. These are in an age group close to graduation, where they are about to face many important (financial) decisions involving risk. In this crucial period, they decide whether to continue education, engage in vocational training or directly start their working career.

We randomly allocated 12 classes from 5 schools to one of three treatments: In control classes, teenagers make choices in the absence of social interaction. In the two treatments conditions, they communicate with a natural peer and then make individual choices regarding risk and loss. The two treatment conditions vary the degree of peer closeness in the stable environment of the classroom experimentally; teenagers are either randomly paired up with a classmate (in the first treatment arm) or with a friend (in the second treatment arm). Risky choices are taken individually in all conditions, using lists of lot-

[3]While assortative matching on socio-demographic characteristics has been widely documented for many peer relationships, matching on preferences has received less attention, mostly because preferences are difficult to observe for the researcher (and potentially for the peer).

teries with pure and mixed prospects as in Tanaka et al. (2010). They are designed to be perfectly correlated across individuals to avoid scope for strategic interaction. Our design thus creates random variation in *whether* and *with whom* subjects can interact in a choice situation involving risk and potential loss.

Our experiment has the following additional features. We use a between-subjects design with simultaneous peer choices. The between-subjects design avoids experimenter demand effects that appear when choices are repeated under different interaction conditions (see Zizzo (2010) for a detailed discussion of such demand effects). We use a simultaneous choice setting where peers make individual decisions while communicating with each other, as opposed to the more frequently used sequential designs with a first and a second mover, effectively closing down feedback between peers (see, e.g. Bursztyn et al. (2014) and Harbaugh et al. (2002)). The latter design allows to identify who influences whom in a peer group and thus to address the reflection problem (Manski, 1993). However, the second mover then faces a simple binary choice – to follow the first mover or not. In a simultaneous setting, peers interact dynamically (and communicate naturally), so that we can study (near continuous) changes in the *degree* of risk (and loss) taken in a social interaction situation.

The following two key findings emerge. First, we find no evidence of assortative matching on (or convergence in) risk preferences, neither on risk nor loss aversion. As expected, however, friends (and classmates) match on observable characteristics such as age and gender (as well as on family background characteristics). Overall, there is large variation in risk preferences among natural peers (i.e. friends and classmates) in the absence of social interaction.

Our second finding is that peers influence each other's risky decisions strongly when they are allowed to interact dynamically. Peer communication increases choice similarity markedly in both treatment arms – i.e. regardless of whether they interact with friends or random classmates – and in both types of lotteries – those with positive and mixed payoffs. To demonstrate these effects, we use prospect theory to infer risk and loss aversion parameters *as if* decisions were taken independently. Communication reduces the difference in risk and in loss aversion within pairs by about 45%. Increased similarity mainly arises from perfect alignment of lottery choices. 59% of those paired with random classmates choose the same switchpoints in lotteries with positive payoffs, and 67% make identical choices in mixed lotteries. Qualitatively similar effects are found when *friends* are allowed to discuss their choices before making decisions – communication reduces differences in risk aversion by 57%, and differences in loss aversion decrease even more strongly, by 85%. Again, this is mainly due to the increase in identical choices.

Our study contributes to a body of literature that documents how the typical patterns of risky choices develop during childhood and adolescence (Reyna and Ellis, 1994; Harbaugh et al., 2002; Levin and Hart, 2003; Levin et al., 2007a,b). Most studies find that risk aversion increases with age after adolescence in both experimental and self-reported survey measures (see, e.g. Gardner and Steinberg (2005); Dohmen et al. (2011)). We find that teenagers in our sample share one common feature: a large majority are risk *and* loss averse, i.e. display $\sigma < 1$ and $\lambda > 1$ (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992). 59.4% of adolescents in the control group are risk averse, and the large majority of 79.7% are loss averse. We compare the risk choices of our sample of teenagers from relatively low SES backgrounds with those of undergraduate students at a top-ranked university in Germany using the same choice task. Teenagers are significantly less risk- and loss-averse than undergraduate students. Our results are consistent with previous results regarding risk aversion among teenagers, and present new results on their reaction to downside risks. A notable exception is Glätzle-Rützler et al. (2015) who found no evidence of *myopic* loss aversion in individual decision of youth between age 11 and 18. Yet, even among teenagers, only a small fraction is risk-loving or risk-neutral, so that the source of their risk-taking is still open to debate. Tymula et al. (2012) suggest that lower levels of ambiguity aversion in adolescents lead to higher risk taking.[4]

We explore the impact of peer interaction on adolescents' risky choices in a school-based field experiment. Our design has several features that offer new insights: we introduce controlled experimental variation in social interaction, and randomly pair individuals with partners whose relation is observed through additionally collected data. We thus bridge the gap between experimental studies that often rely on artificial peers, and non-experimental approaches who face the challenge of endogenous natural peer groups.

A few experimental studies also use naturally occurring peers. For example Bursztyn et al. (2014) consider adult peer effects among friends and family members. These studies

[4]In parallel, recent evidence from neuroscience shows that brain maturity is reached later than previously thought, i.e. in early adulthood up to age 25, so risk-taking in ambiguous situations may provide important learning opportunities and thus foster neurodevelopment (Steinberg, 2007).

abstract from the endogenous nature of these links that were formed prior to the experiment. Our experimental design allows the separate identification of social interaction effects. Additionally, we add to the scarce evidence on assortative matching based on risk preferences. Attanasio et al. (2012) find evidence of assortative matching on risk preferences, but in a group lending context where sorting into groups by risk preferences is economically beneficial (for risk-pooling arrangements).[5] In a recent study, Ahern et al. (2014) showed in a sample of MBA students that peer effects lead to a convergence of risk attitudes over the course of one academic year. We find different results.[6] In our study, individuals in the control group do not interact during the risk task, so we use their choices to obtain an estimate of assortative matching on (or realised convergence of) risk preferences. We use a dyadic approach similar to Fafchamps and Gubert (2007) with symmetric undirected matches to model matches among friends or classmates. We find no evidence of assortative matching on risk preferences – neither on risk nor loss aversion. Unsurprisingly, friends and classmates do match on observable socio-demographic characteristics such as age and gender (as well as on family background characteristics).

The social interaction effect towards increased similarity in risk aversion (but not in loss aversion) has been found in previous studies among adults, and a few mechanisms have been suggested. Lahno and Serra-Garcia (2015) provide evidence consistent with conformity as a reason for increased choice similarity. In other words, peer effects arise from anchoring effects of peers' choices to which individuals conform due to relative payoff concerns (Festinger, 1954). In contrast, Bursztyn et al. (2014) investigate social learning and social preference mechanisms in an experiment that allows to test for these mechanisms explicitly. They find both channels to be empirically important for investment decisions. Social utility, in the form of a "keeping up with the Joneses" effect, arises when learning about the peer's actual (rather than intended) investment.[7] Relatedly, Fafchamps et al. (2015) find that in a repeated game subjects try to keep up with the winners.[8] Social learning effects in the Bursztyn et al. (2014) study are greatest when the first (second) investor is financially sophisticated (financially unsophisticated). Dahl et al. (2014) find

[5]Lahno et al. (2015) find in a sample of villagers from Uganda that differences in individual risk attitudes are larger among peers in interpersonal conflict, in particular among kin.

[6]One reason for the differences in the results may be that in Ahern et al. (2014), peer influences may interact with learning on the course, as financial risk is one of the major components of the MBA curriculum.

[7]First movers make investment choices but receipt of the chosen investment product is randomised.

[8]See Trautmann and Vieider (2012) for a discussion of social utility aspects regarding behaviour under uncertainty.

that the dynamics of social learning increase participation in paid paternity leave schemes over time more than would otherwise be expected. We investigate social learning effects in our simultaneous setting, and use a private and a public signal of financial competency. We find that randomly paired classmates with a high private signal seem less prone to peer influence, i.e. less likely to coordinate their choices perfectly. We find that differences in peer quality predict the likelihood of coordination only if they are publicly visible, while we find no such effect for the private signal. These differential effects are suggestive of social learning effects between peers.

Previous studies have also documented increased choice similarity (among adults) between the first and second mover, but they have been silent about the impact of peer interaction on the nature (or level) of risk-taking.[9] In our simultaneous design with multiple choices, we can examine whether adolescents make more or less risk- and loss-averse choices under social interaction. We find mixed results that depend on the type of peer: communication with a less close peer leads to a *higher* degree of coordination, and results in *more* loss averse (and weakly more risk-averse) choices. The percentage of loss averse choices among randomly paired, interacting classmates is with 92.6% significantly higher than when teenagers decide alone (79.7%). In contrast, we do not find evidence of increasingly loss averse choices in social interaction with friends (76.2% loss averse choices).

This leads to an important conclusion on the nature of peer influences: our results suggest that there are strong adolescent peer effects in risk-taking, that arise from social interaction (not from assortative matching). We re-examine the hypothesis of Carrell et al. (2013) that the nature of the peer relation matters, and find evidence in its favour. We experimentally vary the degree of peer closeness between randomly paired classmates and self-selected friends into discussion groups of two. We find that while social learning appears to be an important driver of peer effects among classmates, there is no evidence of social learning among paired friends – leading to mixed results regarding the level of peer-induced risk- and loss-taking.

[9] For the analysis of the mechanisms through which peer effects arise, sequential designs which allow the experimenter to control the feedback mechanism, and – as done in Bursztyn et al. (2014) – the peer signals that the second-mover receives, are important. They are also useful to understand who influences whom as they address the reflection problem. However, to understand the nature of individual risky choices that result from peer interaction, a simultaneous design seems more appropriate, as it replicates the natural interaction between peers, and allows for dynamic feedback. Dahl et al. (2014), for example, emphasize the multiplicator effect of social learning among peers by documenting snowballing spillovers across individuals in the context of a paid paternity leave scheme.

The remaining chapter is organized as follows: In section 3.2, we explain and motivate our experimental design. Section 3.3 presents the results from our experiment and section 3.3 discusses our findings. Section 3.4 concludes.

3.2 Experiment

We conducted the experiment in 7th and 8th grade classes in lower tier schools around Berlin in June 2014.[10] First, students were given an incentivized experimental task involving lottery choices. Second, they completed individual surveys on cognitive ability, and socio-demographic characteristics.[11]

Before the visits, we randomly allocated each class to the control group or one of the two treatment arms. In control classes (short: *CONTROL*), students completed the task alone, without any communication among students. In the treatments, *RANDOM* and *FRIENDS*, students were allowed to discuss their choices with one partner who was seated next to them. They then made individual choices. We used this between-subjects design to avoid experimenter demand effects that may arise in a within-subjects design from repeating the same lottery choices under different conditions.

Peers within treatment classes are allocated in two ways: In classes in the *RANDOM* condition, students drew a random number at the beginning of the session. Classmates with the same number were seated together. In the *FRIENDS* condition, we asked students to self-select a friend.[12]

[10]These schools belong to the lower track of the German high school system whose students have on average lower socio-economic status (Dustmann, 2004). Graduation from this track is typically followed by vocational training.

[11]All participating schools were part of a larger survey on financial literacy among high school students that was conducted in June and July 2014 throughout Germany. Therefore, the survey also contains information on financial knowledge and teenagers' finances that are not used in this paper. A subsample of 4 classes in 3 schools received three sessions of a financial literacy training several months prior to our visit. All results are robust to including dummies for the financial literacy training as an additional variable in the regressions.

[12]We framed this as pairing up with a friend by stating: "Choose a friend with whom you would like to discuss your choices and sit together in pairs. You are allowed to discuss your decisions with your partner quietly, but you decide only for yourself!" Following common practice for partner tasks in German high schools, students formed groups simultaneously. 18 students ended up with no partner (due to uneven class size or lack of parental consent of either partner) and were excluded from the analysis.

3.2.1 Task Design

The experimental task consisted of 18 lottery choices on two separate decision sheets (A and B) which we adapted from Tanaka et al. (2010). Each of the eleven choices on sheet A was between two lotteries with positive payoffs, while the seven lotteries on sheet B involved gains and losses. This design enables us to assign parameters of prospect theory utility (cf. Tversky and Kahneman, 1992). Due to timing[13] and complexity constraints, we abstracted from probability weighting[14]. All lottery choices involved 50:50 lotteries. To identify loss and risk aversion, we hence required the two decision sheets (A and B) which can be found in appendix C.6. The order of the sheets was randomized within each class. In the treatment groups, we randomized across pairs such that interacting students received identically ordered decision sheets.

We determine students' individual utility parameters assuming 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. We insert each payoff x into the value function $v(x)$, and weight it with the objective probability of 0.5. The 11 choices on sheet A involve only gains. The first lottery option on the left hand side of the sheet (L) is the same across choices on sheet A, while we increase the high payoff in the second lottery (R) across choices. An individual's switching point in these 11 choices on sheet A determines a range for her risk aversion parameter σ. E.g. when person i switched in row 4 from option L to option R, this can be explained by $\sigma_i \in [0.8, 1]$. We assign the midpoint of the range as σ_i, e.g. in the previous example $\sigma_i = 0.9$.[15] Later switches to the second lottery imply lower σ, i.e. higher risk aversion.

[13]Sessions usually had to be conducted within the 45-minutes duration of one lesson.

[14]Typically, probability weighting functions are inversely S shaped and intersect unweighted probability around $p = \frac{1}{3}$ (Wakker, 2010). For several estimates of probability weighting functions (e.g. Tversky and Kahneman, 1992; Camerer and Ho, 1994; Wu and Gonzalez, 1996) deviations of $p = 0.5$ from its decision weight are relatively small. Identifying probability weighting would have required participants to fill out at least one more decision sheet (Tanaka et al., 2010) and a considerably more complicated and time consuming mechanism for determining lottery realizations. Thus, we decided against obtaining such a measure in this study.

[15]If a participant always chose option L or option R, there is no bound for σ from below and above. In this case we assign a“midpoint” σ, calculated using the same interval width as in the neighboring

Then, we assign λ given a participant's choices on lists A and B. The 7 choices on sheet B are mixed lotteries, with the first lottery's (L) expected value deteriorating from row to row (choices 1, 2, 3, 5), or the second lottery (R) improving from row to row (choices 4, 6, 7). E.g. if a participant switched in row 4 on sheet A and was assigned $\sigma_i = 0.9$, and switched to option R in row 4 on sheet B, she is assigned the midpoint $\lambda_i = 2.03$ of the respective range of $\lambda \in [1.70, 2.35]$. As a consequence there are $11 \times 7 = 77$ possible combinations of (σ_i, λ_i) in our dataset.[16] Later switches to option R on sheet B imply higher loss aversion λ.

3.2.2 Procedures

In this section, we describe the procedures used to implement lottery choices, such as satisfying the participation constraint and ensuring trust, and the procedures we followed to match peers and identify friends in this anonymized study. All procedures were explained in the instructions before the incentivized task was started.

Instructions and experimenters: In each session, the instructions for the lottery task were read out aloud in front of the class – each time by the same experimenter. Sessions lasted one lesson (45-60 minutes), of which the instructions and the lottery task usually took roughly 15 minutes each. All experiments were conducted by one of the authors and an aide. The instructions can be found in appendix C.5.

Transaction costs and participation constraint: Students received an initial endowment of €3.10 to ensure an overall positive payoff regardless of their choices in order to satisfy the participation condition and to ensure trust. The endowment was placed in front of each student on her desk at the beginning of the session. Trust was further enhanced by teachers being present in the class during the experiment and witnessing payment announcements and payouts.

Task comprehension: Before participants proceeded with the incentivized task, they had to answer four test questions which can be found at the beginning of appendix C.6. The experimenters checked each participant's responses and allowed only students with correct answers, i.e. those who understood the task, to proceed to making their choices.

interval. I.e. for a person i who always chose option L, we assign $\sigma_i = 0.05$, and for person j who always chose option R, we assign $\sigma_j = 1.625$.

[16] Appendix C.4 lists all choices and their associated σ parameters, and an exemplary table for assigning λ parameters.

If a participant had made a mistake, the experimenter would explain the calculation of payoffs again. The lottery task was completed using pen and paper.

Payment method and incentivization: Upon task completion, the experimenters collected students' responses. Once the task was completed by all, one participant was selected to draw one random card, thereby determining which of the 18 choices would be paid out. Another participant was selected to draw one of 10 colored chips (5 red and 5 blue) from a bag to determine whether the high or the low payoff had been selected for payout.[17] High and low payoffs were colour-coded in blue and red on the sheets to connect students' lottery choices to the draw, and probabilities were visualized as 5 red and 5 blue chips at the top of each sheet. Both chance draws were applied to all students in the class, so that risks were perfectly correlated across students to minimize the scope for mutual insurance. All payments were made in class at the end of the session. Participants earned on average €4.10 with a standard deviation of €1.81.

Inconsistent choices: We encouraged consistent choices by explaining to participants why it only makes sense to switch once from option L to option R, and asked for a summary of choices at the bottom of each sheet (cf. appendix C.6). We allowed participants to disregard the choice summary and determined payouts from the binary choices at the top. Participants often left out (parts of the) summary at the bottom of the decision sheets: of the 235 original observations, 50 participants left out items of the summary on sheet A and 41 left out items of the summary on sheet B. Therefore, we only analyse the individual choices.

Identification of peers: All survey questionnaires had a unique running ID number. A red sticker with the same ID number as the questionnaire was attached to the bottom of the survey on which participants recorded their choices in the incentivized task. In the treatment groups *RANDOM* and *FRIENDS*, pairs were formed before the task began, and documented through the exchange of these stickers. *CONTROL* classes exchanged stickers at the end of the survey identifying their closest friend in the classroom.[18] The latter procedure enables us to identify similarities due to assortative matching in the absence of task interaction in the control group.

[17] For example, if B1 and a red chip were drawn, and the participant had chosen lottery L, the payoff was -0.40 €.

[18] Like the pairing in the friends treatment, students had to mutually agree to exchange.

3.2.3 Sample

Our sample consists of 235 teenagers from 12 classes in 5 schools. All participants included in the sample provided parental consent to participate in the study. 17 teenagers made inconsistent choices[19] and were therefore excluded from the analysis. 5 participants made incomplete choices. 18 participants could not be matched with a partner.[20] This leaves us with 198 observations for the main part of our analysis.

Individual characteristics are balanced across the two treatment groups and the control group, as shown by the χ^2 tests presented in Table 3.1[21], supporting the validity of our randomization. They include the school grade (7th or 8th), age (in months), gender, and numeracy (measured as the last math grade relative to the average class grade). We also include fluid intelligence or cognition which are measured by four of Raven's progressive matrices (Raven, 1989), chosen to reflect the variance in cognitive ability among German teenagers following Heller et al. (1998). Finally, we capture socio-demographics and family background variables such as the log of household size, a dummy capturing migrant background, and the number of books at home[22]. Since we randomized at the class level, we cluster standard errors accordingly (Moulton, 1986).

At the end of the survey, all pairs (friends, randomly matched classmates and participants who exchanged stickers in the control group) were asked about their degree of closeness, proxied by the frequency of seeing each other outside of class.[23] As intended in our design, the degree of contact is higher among those who were asked to identify a friend as peer (in *CONTROL* and *FRIENDS*) than those in *RANDOM* who stated their degree of contact with a randomly allocated classmate. We find that the friends pairs in *CONTROL* and *FRIENDS* report a weakly significant higher contact intensity than the classmate pairs in *RANDOM* (MWU test; $p = 0.0696$).

[19] 12 on decision sheet A, 7 on decision sheet B

[20] Either because no partner was available due to uneven class size, or because the partner did not fill out (all) choices, behaved inconsistently or failed to hand in a consent form, which we allowed students to do late (in these cases, earnings remained with the teachers until the consent form was handed in).

[21] A similar table for the whole sample can be found in appendix C.1.

[22] This variable is also used in PISA surveys as a proxy of important family inputs into education. See also Hanushek and Woessmann (2011)

[23] This question can be found in appendix C.7.

Table 3.1: Descriptive Statistics Analyzed Sample

	TOTAL	*CONTROL*	*RANDOM*	*FRIENDS*	p-value
grade	7.768	7.750	7.741	7.800	0.670
age	14.105	13.935	14.114	14.238	0.151
male	0.595	0.594	0.558	0.620	0.775
relmath	0.024	-0.031	0.116	0.009	0.747
stanmath	0.022	-0.048	0.123	0.013	0.626
cogn1 4	0.788	0.672	0.759	0.900	0.400
cognlow	0.823	0.859	0.870	0.762	0.180
risk	3.156	3.190	3.122	3.147	0.863
risklow	0.167	0.172	0.148	0.175	0.911
singleparent	0.301	0.379	0.319	0.231	0.166
hhsize	3.931	3.855	4.061	3.909	0.288
migrantbackground	0.628	0.694	0.542	0.629	0.263
books	2.505	2.705	2.688	2.227	0.207
bookslow	0.449	0.391	0.389	0.537	0.122
contact	2.505	2.828	2.229	2.408	0.012
contacthigh	0.414	0.578	0.296	0.362	0.004
students	198	64	54	80	
classes	12	4	3	5	
schools	5	3	3	4	

Note: age measured in months; cogn1 4 corresponds to number of correct Raven matrices out of 4; language2 is dummy taking value 1 if student checked box “other language spoken at home”; contacthigh is a dummy taking the value 1 if the respondent replied to have contact with the partner at least once per week; p-values are from χ^2 tests.

3.3 Experimental Results

We discuss the results of our study in four sections. First, we determine the degree of risk and loss aversion based on prospect theory from teenagers’ choices, to shed new light on risk preferences among adolescents (Section 3.3.1). Second, we test whether friends and classmates (as opposed to random matches among adolescents in our sample) have matched assortatively on their risk preferences (Section 3.3.2). Third, we separate assortative matching (that influenced peer group formation, i.e. class and friend matching, before our experiment) from direct interaction between peers during the risk choice task to identify the causal effect of peer communication on risky choices. We discuss outcomes in terms of similarity of lottery choices (see Section 3.3.3), and riskiness of choice (see Section 3.3.4). We also comment on how similarity among different *types* of peers affects teenagers’ choices.

3.3.1 Adolescents' Risk and Loss Aversion

In our discussion of risk preferences among teenagers, we first focus on the control group in which adolescents take individual and independent decisions without peer interaction. We apply the method derived from prospect theory described in Section 3.2.1. The risk preferences determined from adolescents' choices[24] show that adolescents are, on average, risk and loss averse, i.e. display $\sigma < 1$ and $\lambda > 1$ (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992). We find a mean risk aversion parameter $\bar{\sigma}$ of 0.828, and a mean loss aversion parameter $\bar{\lambda}$ of 2.579 (see upper panel of Table 3.2). One-sided t-tests confirm the hypotheses of risk and loss aversion of adolescents (see last column of Table 3.2).[25] Looking across the distribution of parameters, we find that 59.4% of teenagers in the control group are risk averse, and the large majority of 79.7% are loss averse.

Table 3.2: Descriptive Statistics for Individual Parameters: Adolescents and Young Adults

	mean	median	5th	25th	75th	95th	% risk- (loss-) averse	p-value[a]
Adolescents' risk preference parameters (CONTROL)								
σi	0.828	0.650	0.150	0.500	1.125	1.625	0.594	0.004
λi	2.579	1.985	0.458	1.548	2.499	7.490	0.797	0.000
Young adult university students' risk preference[b]								
σi	0.613	0.550	0.050	0.350	0.750	1.625	0.775	0.000
λi	2.949	2.023	0.671	1.540	3.141	10.019	0.775	0.000

Note: [a] p-values from t-tests of H_0: $\sigma \geq 1$ or $\lambda \geq 1$ respectively. [b] Risk preferences are elicited using the same lottery choices (rounded to one decimal).

The prevalence of risk aversion among adults and adolescents has been established in previous studies (Glätzle-Rützler et al., 2015; Tanaka et al., 2010; Tymula et al., 2012; von Gaudecker et al., 2011). Tanaka et al. (2010), for example, report $\bar{\sigma}_{south} = 0.59$ ($\bar{\sigma}_{north} = 0.63$) for farmers in south (north) Vietnam, while Glätzle-Rützler et al. (2015) estimate mean risk aversion to be 0.57 among children and adolescents aged between 10 and 18. Our results show a similar degree of risk aversion at the median, but a slightly lower mean degree of risk aversion compared to these studies.

[24]For a summary of teenagers' choices, see Table C.2 in appendix C.2.

[25]We find adolescents to be risk and loss averse also in the full sample. Average parameter values are $\bar{\sigma} = 0.77$ and $\bar{\lambda} = 2.86$. We reject the hypothesis that individuals are not risk and loss-averse at $p < 0.01$ (one-sided tests).

To cross-validate our lottery instrument, we replicate the individual risk choice task (that we posed to the control group in our experiment), using the same payoff structure, with university students[26]. In a sample of 64 students at LMU Munich, we find risk aversion among university students at the mean ($\bar{\sigma} = 0.613$; see lower panel of Table 3.2) that is similar to the parameters found in the previous literature. We test for differences in risk aversion between teenagers (of lower socio-economic status (SES)) and university students (who tend to be of high SES), and find teenagers to be statistically significantly less risk-averse (Mann-Whitney-U test, $p = 0.004$).[27]

Fewer studies provide information on the existence and degree of loss aversion, and all of these apply to adults. These point to (a large degree of) loss aversion (Abdellaoui et al., 2008; Etchart-Vincent and L'Haridon, 2011). To our knowledge, we are the first to measure the degree of loss aversion among adolescents. Relatedly, Glätzle-Rützler et al. (2015) study myopic loss aversion, i.e. a combination of loss aversion and mental accounting, in an adolescent sample. They find, in contrast to adults, no evidence for such myopia among teenagers, but their design does not allow the separate identification of λ.

We find evidence for the existence of loss aversion among nearly 80% of adolescents. The average degree of loss aversion among adolescents ($\bar{\lambda} = 2.579$) is similar to previous estimates for adults. Tanaka et al. (2010) finds $\bar{\lambda} = 2.63$, and Tversky and Kahneman (1992) provide an estimate of 2.25. Comparing our parameter values to those of adults in Tanaka et al. (2010) using t-tests, we find no statistically significant differences in loss aversion λ. However, we do find statistically significant differences between adolescents and university students, using the same choice task. University students are more risk-averse (Mann-Whitney-U test, $p = 0.004$)[28], and more loss-averse ($\lambda = 2.949$) than the

[26] These students completed a computerised lab task using the same lottery choices (with payoffs rounded to one decimal). Students were paid according to an individual chip and choice draw, compared to a joint in-class draw in our experiment.

[27] Correspondingly, we find that teenagers switch to the more risky lottery B earlier (average switching point: 5.359) than university students (switching point: 6.845, Mann-Whitney-U test, $p = 0.003$), and that a larger percentage of young adults is risk-averse (χ^2 test, $p = 0.023$). This result is consistent with the findings of Tymula et al. (2012) who report increasing risk aversion between adolescence and adulthood. Contrary evidence is provided in Glätzle-Rützler et al. (2015) who found the degree of risk aversion to be stable across age between childhood and adolescence. However, since the two study populations differ in their socio-economic status, we cannot interpret our findings solely in terms of age.

[28] Additionally, a statistically significantly higher fraction of students – 77.5 relative to 59.4% of adolescents – is risk-averse (χ^2 test, $p = 0.023$)

adolescents in this study. However, the latter difference is not statistically significant (Mann-Whitney-U test, $p = 0.383$).

In summary, adolescents have – consistent with previous findings – strongly risk-averse preferences. We present new evidence that adolescents are loss-averse. In a parallel task with university students, we find that both risk and loss aversion are heterogeneous across and within population groups, e.g. by age and socio-economic status.[29]

Result 1: Risk preferences among adolescents

Adolescents are on average risk and (strongly) loss averse, i.e. $\sigma < 1$ and $\lambda > 1$.

3.3.2 Assortative Matching on Risk Preferences among Peers

Preference heterogeneity may be reduced among peers. In our experiment, we collect information about adolescents' peer networks in all groups which allows us to test whether individual and independent lottery choices (i.e. those made by the control group) display similarity with their peers' risk preferences (in the absence of social interaction during the task). Two mechanisms may lead to similarity in risk preferences: first, adolescents likely exhibit assortative matching in their choice of friends. Similarly, there may be selection in the sorting of adolescents into classes (due to geographic proximity, family background, and so forth), either by parental or school influence on class assignment (see, inter alia, Jackson and Rogers (2007) for evidence on social sorting processes). Secondly, attending the same class or friendship may result in convergence in risk preferences over time (Ahern et al., 2014). The scope of this paper is not to differentiate between friends assortative matching (or selective sorting into classes) and subsequent convergence in preferences as relations deepen. Rather, our aim is to separate the effect of direct social interaction in the choice situation from the peer sorting processes which took place before our experiment. In the remainder of the paper, we hence use the term "assortative matching" to characterise the cumulative effect of the matching, sorting and convergence between peers that take place before the experiment.

We elicit peer information at two levels: First, we identify each teenagers' classmates (and know their choices). Second, we follow the same procedure of identifying (mutual)

[29] Recent studies by (Alan et al., 2012) and (Zumbühl et al., 2013) emphasize the importance of inter-generational transmission of risk preferences as the source of heterogeneity in risk preferences

friends in the control group (via the exchange of stickers with their ID number) as in the *FRIENDS* treatment. Using this information, we investigate similarity of choices in the control group across three dimensions: we compare individual i's risk preferences by calculating absolute parameter differences with i) the average risk preferences of all other individuals in the control group (i.e. across classes and schools, denoted as Δ_{ij}^{S})[30], ii) of i's classmates (Δ_{ij}^{C}), and iii) with i's endogenously chosen friend j (Δ_{ij}^{F}). None of these teenagers were allowed to discuss their simultaneously made lottery choices, so all similarity in choices is due to *ex ante* similarity.

Under positive assortative matching, risk preferences of friends would be more similar than those of classmates, and more similar than those among teenagers in the control group. Based on previous evidence for young adults, we expect preferences among peers to have converged over the course of their joint school time together. Ahern et al. (2014) find convergence in risk aversion among MBA students which overall increases average risk aversion over the course of one study year. McPhee (1996) showed that adolescents are more likely to exert peer pressure on friends than on acquaintances, hence we expect stronger similarity in risk preferences among friends. Additionally, friends may match assortatively, potentially including matching on risk preferences.

Table 3.3 compares the similarity in risk and loss aversion parameters across the three dimensions described above. Friends' risk aversion and loss aversion parameters are less similar to each other than those of adolescents in our control group overall. Adolescents do not exhibit the expected ranking in similarity by peer closeness. For both – loss and risk aversion – friends exhibit a significantly larger variance in preferences. In consequence, the hypothesis of increased similarity among friends is not confirmed. Among classmates, we obtain mixed results regarding assortative matching. Adolescents in the same class exhibit larger similarity in risk aversion, but less similarity in loss aversion than adolescents in the control group (across classes and schools). We reject the Null hypothesis of equal similarity in a Wilcoxon-Signrank test ($p = 0.037$ and $p = 0.001$ for σ and λ respectively, see Table 3.3). Hence, while we do find some evidence of increased similarity in risk preferences among classmates, we do not find evidence of assortative matching among friends on risk aversion or loss aversion.

[30]Similarity in risk aversion within the control group is e.g. computed as $\Delta_{ij}^{S}(\sigma) = |\sigma_i - \sigma_{Ros}| = |\sigma_i - \frac{1}{S-1}\sum_{j \neq i \in S} \sigma_j|$. $\Delta_{ij}^{S}(\lambda)$ is constructed analogously. Similarity measures within classes are constructed analogously, but comparing to the mean within the class ($j \in C$) only. Finally, similarity in risk preferences among friends is, e.g.: $\Delta_{ij}^{F}(\sigma) = |\sigma_i - \sigma_j|$ where j denotes the friend.

Table 3.3: Comparison of Similarity by Peer Relation within *CONTROL*

	σ		λ	
	mean	var	mean	var
Δ^F	0.382	0.113	2.094	7.061
Δ^C	0.354	0.086	1.614	3.584
Δ^S	0.411	0.071	1.479	3.283
	p-values			
$\Delta^F = \Delta^C$	0.579		0.920	
$\Delta^F = \Delta^S$	0.482		0.339	
$\Delta^C = \Delta^S$	0.037**		0.001***	

Note: p-values from Wilcoxon-Signrank Tests;
*** $p < 0.01$, **$p < 0.05$, * $p < 0.1$

Individual and family background characteristics of adolescents are likely determinants of school choice, class assignment and the formation of friendships. To test for assortative matching on risk preferences (rather than on individual characteristics), we additionally control for the latter. We follow the dyadic approach outlined in Fafchamps and Gubert (2007) (see also Arcand and Fafchamps (2011) and Attanasio et al. (2012)). We treat each possible pair of individuals as one observation. We first consider selection into friendships and create these dyads within a class, thus creating 335 combinations[31]. This allows us to study whether friends' socio-economic characteristics as well as their (individually elicited) risk preferences are correlated more strongly (than those who do not consider each other as friends). Since friendship pairs are formed jointly by exchanging stickers, they are by construction undirected, so the matches m_{ij} and m_{ji} are symmetric. Let $m_{ij} = 1$ if two classmates declare their friendship, and zero otherwise. Since $m_{ij} = m_{ji}$, we collapse identical pairs and adapt a symmetric dyadic approach (see Arcand and Fafchamps (2011); Attanasio et al. (2012); Fafchamps and Gubert (2007)). We specify the following model:

$$
\begin{aligned}
m_{ij} = \beta_0 &+ \beta_1|\sigma_i - \sigma_j| + \delta_1|\lambda_i - \lambda_j| + \eta_1|z_i - z_j| + \zeta_1|y_i - y_j| \\
&+ \beta_2|\sigma_i + \sigma_j| + \delta_2|\lambda_i + \lambda_j| + \eta_2|z_i + z_j| + \zeta_2|y_i + y_j| + \mu_i \qquad (3.1)
\end{aligned}
$$

where σ and λ denote elicited risk preferences, $\mathbf{z}$ is a vector of individual characteristics, $\mathbf{y}$ are family background characteristics, and μ_i is a classroom fixed effect. Note that the specification includes absolute values of sums and differences of each explanatory variable

[31]The number of possible pairs depends on the class size as friends and classmates can only be matched within, not across classes.

within the pair. We use absolute values as the match is undirected. Sums model the likelihood that individuals with particular characteristics are more likely to have a friend in the class. Difference terms allow us to test for assortative matching on time-invariant characteristics (as most of the variables that we include in $\mathbf{x}$ and $\mathbf{y}$ are, e.g gender). To be precise, differences reflect (as)sorting *and* ex ante convergence in preferences (or characteristics) through previous contact when characteristics are possibly time-variant (as adolescents' risk preferences σ and λ may be). As discussed at the beginning of this section, we do not distinguish between these and test joint hypotheses summarising these mechanisms. Negative (positive) parameter estimates β_1 or δ_1 would indicate (negative) assorting in risk preferences. Parameter estimates for η_1 and ζ_1 indicate ex ante matching on individual and family background characteristics.

Since all teenagers enter a pair by experimental design (unless class size is uneven or an adolescent in a pair did not give consent, in which case one teenager remains unmatched), there is little source for endogenous participation choice. We estimate the dyadic model using a logit estimator and cluster standard errors at the class level.

We find no evidence for the joint hypothesis that friendships form based on or converge in their risk preferences over time (see column 1 of Table 3.4). Controlling for individual and family background characteristics, such as age, gender and numeracy (measured by the math grade relative to average class performance), we find, consistent with the matching literature, strong evidence for positive assortative matching. It is based on age and gender, but not on numeracy (see column 2). As expected, we find none of the terms involving sums of characteristics to affect the probability of a friend matching, confirming the viability of our design and rejecting the hypothesis of endogenous participation choice.

In column 3, we expand the set of characteristics, using family background characteristics such as household size, whether they live with a single parent, have migrant background, or have low socio-economic status. Socio-economic status is proxied by a dummy that takes the value of 1 if there are no more than 25 books in a teenagers' home. The number of books is a standard question in PISA that captures important family inputs into a teenager's education, or lack thereof (Hanushek and Woessmann, 2011). We find very similar parameter estimates for the matching variables age and gender. Additionally, adolescents are more likely to match on single parent and socio-economic status.[32]

[32]In a robustness check, we re-estimate specifications 2 and 3 on a pooled control (*CONTROL*) and friends matched treatment (*FRIENDS*) to check whether both groups have a similar matching on ob-

Table 3.4: Logit Regressions of Probability of Being Friends on Dyad Level explanatory Variables within *CONTROL*

	(1) $P(friends = 1)$	(2) $P(friends = 1)$	(3) $P(friends = 1)$
DIFFERENCES			
σ	0.0647	0.288	0.416
	(0.212)	(0.550)	(0.890)
λ	0.00834	-0.0542	-0.0642
	(0.115)	(0.183)	(0.161)
rel. math grade		0.130	0.157
		(0.291)	(0.261)
age		-0.613**	-0.761**
		(0.263)	(0.304)
male		-2.517***	-2.526**
		(0.972)	(1.110)
household size			-0.158
			(0.175)
single parent			-1.751***
			(0.601)
migrant			-0.0728
			(0.286)
low SES			-0.357***
			(0.0887)
SUMS			
σ	0.216	0.151	0.212
	(0.280)	(0.300)	(0.426)
λ	0.0186	0.0748	0.0982
	(0.0637)	(0.129)	(0.123)
rel. math grade		0.0629	-0.00710
		(0.163)	(0.221)
age		0.0353	0.0804
		(0.0658)	(0.133)
male		-0.334*	-0.348
		(0.203)	(0.285)
household size			0.0889*
			(0.0463)
single parent			0.460*
			(0.251)
migrant			-0.0466
			(0.360)
low SES			-0.216
			(0.328)
Constant	-3.213***	-2.956	-4.128
	(0.600)	(2.963)	(5.332)
Observations	335	335	335

Note: Dependent variable is *classmates* which is a dummy variable taking the value 1 for pairs of participants who are classmates; rel. math grade refers to the math grade of each individual subtracted by the class average; robust standard errors clustered at the class level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Logit Regressions of Being Classmates on Dyad Level Explanatory Variables within *CONTROL*

	(1) $P(classmates = 1)$	(2) $P(classmates = 1)$	(3) $P(classmates = 1)$
DIFFERENCES			
σ	-0.637	-0.699	-0.688
	(0.742)	(0.583)	(0.498)
λ	0.0106	0.00426	0.0146
	(0.0454)	(0.0715)	(0.0429)
rel. math grade		-0.108	-0.107
		(0.140)	(0.111)
age		-0.612***	-0.615***
		(0.156)	(0.166)
male		0.163***	0.163***
		(0.0557)	(0.0560)
household size			-0.0117
			(0.0420)
single parent			0.0859
			(0.0725)
migrant			0.0385
			(0.0835)
low SES			-0.0466
			(0.130)
SUMS			
σ	-0.0485	0.00422	-0.00239
	(0.454)	(0.495)	(0.453)
λ	-0.0214	-0.0121	-0.0214
	(0.0350)	(0.0444)	(0.0700)
rel. math grade		0.0223	-0.00352
		(0.0793)	(0.143)
age		0.189	0.207
		(0.335)	(0.384)
male		-0.101	-0.110
		(0.120)	(0.151)
household size			0.0371
			(0.112)
single parent			-0.0760
			(0.216)
migrant			-0.00896
			(0.250)
low SES			-0.0797
			(0.263)
Constant	-0.667	-5.273	-5.888
	(1.551)	(7.967)	(9.997)
Observations	1,378	1,378	1,378

Note: Dependent variable is *classmates* which is a dummy variable taking the value 1 for pairs of participants who are classmates; rel. math grade refers to the math grade of each individual subtracted by the class average; robust standard errors clustered at the class level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Finally, we use the dyadic analysis to investigate matching among classmates (Table 3.5). We form all possible pairs across classes and redefine the dependent variable to take the value of 1 if two adolescents who are randomly paired in the dyadic analysis are classmates, and otherwise estimate the same specifications as before. Again, we find no evidence of pairing based on risk preferences but we do find matching on age and gender which can be easily explained: positive assortative matching based on age arises because we have classes from two different cohorts in our sample, thus age differences between classes are more pronounced than within classes. Negative assortative matching based on gender can be explained using conditional probabilities: if you consider the gender of one observation within one classroom, this observation will be more likely to be matched with a classmate of the opposite gender within the classroom (since she cannot be matched with herself) than across different classrooms, since the distribution of gender across *CONTROL* classes is relatively unaffected by removing one observation from the treatment. Apart from these mechanical results, we find no evidence of any sorting or systematic matching effects.

Result 2: Similarity of friends in risk preferences in the absence of social interaction

a Similarity in risk or loss aversion is not larger among friends than among random peers.

b Adolescents do not assortatively match on loss or risk aversion.

d Friends assortatively match on age and gender, and on living with a single parent and family socio-economic status.

c Classmates are, however, matched by observable characteristics, i.e. age and gender.

In summary, we do not find evidence of assortative matching on risk preferences, suggesting that friendships are neither formed nor more likely sustained based on similar risk preferences. We also do not find matching on risk preferences among classmates.

servables. We find very similar parameter estimates as for the control group alone, so friends match on the same characteristics in both groups (see Table C.5 in appendix C.3).

3.3.3 Social Interaction in Risk Choices: Choice Similarity

Peer effects may not only arise from assortative matching but also from direct social interaction, i.e. communication between peers, in a choice situation. We thus introduce controlled variation in social interaction in our experiment. In the two treatment conditions, we pair each teenager with a random classmate (*RANDOM*) or a friend (*FRIENDS*) and allow them to communicate. During or after the discussion, each subject makes individual lottery choices. We acknowledge the dynamic nature of peer influences by allowing for bilateral interaction and simultaneous decision-making under random exposure to naturally occurring peers. This comes at a cost: we are not able to identify the direction of peer influences, as is possible in a sequential design. However, our design does preserve the innate uncertainty about the actions of one's peer present in many choice situations, and allows for dynamic interaction. The latter is ruled out in sequential designs.

Our ability to condition on assortative matching helps us better understand the mechanisms of peer influences, as we can separate it from (dynamic) social interaction in the choice situation. Since we did find assortative matching on socio-economic characteristics among classmates and friends, we identify the impact of peer communication on adolescents' risk choices by comparing the similarity in risk preferences among paired teenagers in the treatment groups with similarly matched pairs in the control group.

In the *RANDOM* treatment, our counterfactual is the similarity between an individual's choice and the average of the choices of the remainder of her class[33]. We find that communication decreases the difference in risk and in loss aversion within pairs by about 45% (for both $p \approx 0$, see Table 3.6). The increased in similarity mainly stems from perfect alignment of lottery choices. 59% of pairs in the treatment group choose the same switchpoint in the lotteries with positive payoffs ($p \approx 0$) relative to 11% in the control group, and 67% make identical choices in the mixed lotteries ($p = 0.002$). 56% of subjects in *RANDOM* make identical choices in both decision sheets, while only 3% do so in the control group.

The social interaction effects are similar when friends (rather than classmates) are allowed to discuss their choices (see Table 3.7). Here, the counterfactual is the similarity of friends pairs in the control group, identified through a mutual exchange of ID stickers,

[33] I.e. excluding the individual

Table 3.6: Similarity of Risk Parameters across Matches for *RANDOM* vs. *CONTROL*

	CONTROL (individual relative to RoC)		*RANDOM*		
	Mean	SD	Mean	SD	p-value
$\Delta_{ij}(\sigma)$	0.354	0.293	0.194	0.372	0.000
$\Delta_{ij}(\lambda)$	1.614	1.893	0.907	2.103	0.000
$\%(\Delta_{ij}(switch_A) = 0)$	0.109		0.593		0.000
$\%(\Delta_{ij}(switch_B) = 0)$	0.406		0.667		0.023
$\%(ident.choice)$	0.031		0.556		0.000

Note: i and j refer to matched participants. RoC denotes the rest of class, i.e. excluding individual i; $\Delta_{ij}(\sigma)$ and $\Delta_{ij}(\lambda)$ refer to the absolute difference in σ respectively λ between two matched participants; to construct $\%(\Delta_{ij}(switch_A) = 0)$ and $\%(\Delta_{ij}(switch_B) = 0)$ in *CONTROL* the average switching point rounded to the next integer was used; p-values for $\Delta_{ij}(\sigma) = 0$ ($\Delta_{ij}(\lambda) = 0$) from Mann-Whitney-U tests and for $\%(\Delta_{ij}(switch_A) = 0)$, ($\%(\Delta_{ij}(switch_B) = 0)$) and $\%(ident.choice)$ from χ^2 tests.

Table 3.7: Similarity of Risk Parameters across Matches for *FRIENDS* vs. *CONTROL*

	CONTROL (friends pairs)		*FRIENDS*		
	Mean	SD	Mean	SD	p-value
$\Delta_{ij}(\sigma)$	0.382	0.336	0.166	0.242	0.002
$\Delta_{ij}(\lambda)$	2.094	2.657	0.290	0.549	0.000
$\%(\Delta_{ij}(switch_A) = 0)$	0.156		0.450		0.008
$\%(\Delta_{ij}(switch_B) = 0)$	0.281		0.750		0.000
$\%(ident.choice)$	0.031		0.425		0.000

Note: i and j refer to matched participants. $\Delta_{ij}(\sigma)$ and $\Delta_{ij}(\lambda)$ refer to the absolute difference in σ respectively λ between two matched participants; p-values for $\Delta_{ij}(\sigma) = 0$ ($\Delta_{ij}(\lambda) = 0$) from Mann-Whitney-U tests and for $\%(\Delta_{ij}(switch_A) = 0)$, ($\%(\Delta_{ij}(switch_B) = 0)$) and $\%(ident.choice)$ from χ^2 tests.

the same method that we used in the *FRIENDS* treatment. Differences in the risk aversion parameter reduce by more than more than half (57%, $p = 0.002$), and differences in the loss aversion parameter decrease by 85% ($p \approx 0$). Again, this is mainly due to the increase in identical choices within peer pairs. The fraction of pairs who coordinate their choices perfectly in the lotteries with positive payoffs increases from 16 to 45%, and from 28 to 75% in the mixed lotteries. 43% of individuals make perfectly aligned choices in both lottery types, as opposed to 3% in the control group. All increases in coordination are statistically significant at the 1% level.

Result 3: Similarity of choices in the communication treatments

a Similarity in risk preferences among peers who are allowed to communicate increases (among friends and classmates).

b Social interaction increases the fraction of paired teenagers who make identical decisions (in both treatments).

In a related study among close peers, Bursztyn et al. (2014) study peer effects in an investment choice using a design in which information exchange is unidirectional. In their experiment, second mover adults who receive information about their (first mover) friends' investment choice are significantly more likely to buy the same asset. Investment take-up increases from 42% to 71% (respectively 93%), yielding effect sizes that are similar to those found in our study: close peers choose similar lotteries, and a large fraction makes identical choices. While they do not consider the role of assortative matching among their close peers, the authors chose a design which allows them to test two social interaction mechanisms: social learning and "keeping up with the Joneses" motives. Social learning describes a situation where the individual interprets her peer's choice as a quality signal. "Keeping up with the Joneses" type preferences arise when one's utility from making a risky choice depends directly on another individual's risk choices. Bursztyn et al. (2014) find that both channels affect investment choices.

We offer new evidence that social learning plays a role in peers' choice coordination – also among teenagers. We exploit that peers' willingness to coordinate choices should then depend on signals of financial competency. We have two measures of financial competency at our disposal: our study contains 12 questions on financial literacy (see appendix C.7 for further details)[34] and a numeracy measure (the last math grade). Note that as in Bursztyn et al. (2014), the evidence we present is suggestive, as financial literacy and numeracy are not randomly assigned. While both could in principle serve as signals of peer quality, they differ in an important aspect: while math grades (and performance) are publicly observable in the classroom, our test of financial literacy was conducted during the experiment and adolescents received no feedback on their own or each others'

[34]Our measure of financial literacy replicates the interest compounding and risk diversification measure of Van Rooij et al. (2011), but provides a more comprehensive measurement of financial literacy using ten questions in which adolescents are asked to choose between differently risky assets, trade off between risk and liquidity, and so forth.

test results. Therefore, numeracy is a publicly observed signal of peer quality, while the financial literacy score remains a private signal. Since peer interaction is bilateral in our simultaneous setting – unlike in Bursztyn et al. (2014) –, coordination can be rejected unilaterally but has to be agreed bilaterally. If social learning matters, individuals who receive a negative peer signal should decide not to coordinate. Individuals with high financial competency, in contrast, may be more likely to reject coordination, as high financial literacy may reduce the need for social learning and indicate a stronger cognitive-control system, thus muting the scope for peer influence. We use perfect choice coordination in all lottery choices as a conservative measure of peer influence, and examine this hypothesis.

In Table 3.8, we investigate the determinants of perfect coordination of choices across both sheets in each treatment condition. We include measures of (own) financial literacy and numeracy and peer differences in these. We find evidence suggestive of social learning: while there is no association between peer differences in (privately observed) financial literacy and the probability of coordination, we find that peer differences in (publicly observed) numeracy reduce the probability of coordination, in line with social learning effects in a simultaneous choice setting. We also find that high own financial literacy is associated with a decrease in the likelihood of coordinating, as hypothesized above. This is again showing that the results for adults in Bursztyn et al. (2014) carry through to teenagers, and suggests that own competence makes teenagers less prone to peer influences.

However, Table 3.8 also shows that these effects are statistically significant (and economically important) only in the *RANDOM* treatment, so whether social learning takes place appears sensitive to the type of peer. We find no correlation between numeracy or financial sophistication (or peer differences in these) in the *FRIENDS* treatment, although interacting friends also display strong increases in coordination. The sensitivity of peer influence to peer closeness has also been highlighted by Carrell et al. (2013). Our results would be consistent with the hypothesis that social learning and information exchange as the mechanisms behind peer interaction may play a stronger role between less close peers. Given that we did not create exogenous variation in financial competency and since our sample size shrinks when we split the data by treatment condition and degree of coordination, the robustness of the differential results between these two peer groups should

Table 3.8: Determinants of Perfect Choice Coordination

	RANDOM	*FRIENDS*
Financial literacy	-0.239 (1.99)**	0.068 (0.91)
Δ(Financial literacy)	-0.076 (0.34)	-0.110 (1.51)
Math	0.236 (0.88)	-0.060 (0.34)
$\Delta(Math)$	-0.706 (2.11)**	0.269 (1.51)
Male	-0.260 (0.55)	-0.590 (1.78)*
$\Delta(Male)$	0.525 (1.01)	-0.564 (1.25)
Constant	2.512 (2.28)**	-0.317 (0.45)
Observations	46	74

Note: Probit regressions; estimates are clustered at the class level. t-statistics in parentheses; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

be tested in further research. A suitable design for such a test could be to introduce experimental variation in private and public signals between rounds in repeated lotteries with mixed and positive prospects.

Result 4: Peer influence and social learning

a Coordination increases in the similarity in publicly observed financial competency, consistent with social learning effects.

b Coordination among friends is not related to either measure of financial competency.

3.3.4 Social Interaction in Risk Choices: Level of Risk and Loss Taking

While sequential peer experimental designs pose a take-it-or-leave-it offer to the second mover, our simultaneous setting involves several choices between differently risky lotteries. Thus, we examine the impact of social interaction on the *level* of risk-taking, using the prospect theory parameters to give an indication of scale. Under peer influence, choices are, however, no longer independent and thus do not identify individual preference parameters. We therefore also report choices directly, i.e. the switching points in the lotteries with positive (A) and mixed payoffs (B).

Gardner and Steinberg (2005) found that subjects under peer influence took more risks, focused more on benefits than costs of risky behavior and made riskier decisions than

Table 3.9: Risk Parameters per Treatment

				p-values		
	CON	RAN	FRI	RAN vs CON	FRI vs CON	RAN vs FRI
$\bar{\sigma}$	0.828	0.761	0.727	0.245	0.350	0.681
$\bar{\lambda}$	2.579	3.407	2.724	0.039**	0.906	0.021**
risk-averse	0.594	0.630	0.700	0.692	0.185	0.397
loss-averse	0.797	0.926	0.762	0.048**	0.623	0.014**

Note: Full sample included; p-values are from Mann-Whitney-U tests; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

when they did not communicate.[35] Similarly, we expected participants in the two peer discussion treatments to take more risk, both in the domain of pure lotteries as well as in the domain of mixed lotteries. We additionally expected that these effects would be more pronounced for closer peers, in line with the social comparison theory by Festinger (1954). He postulated that the tendency to compare oneself with some other specific person decreases in the differences with that person. Friends (who have assortatively matched) would be more alike, so we expected stronger effects of discussions towards risk taking and loss acceptance in the *FRIENDS* treatment.

We find none of these predictions from previous studies confirmed. Table 3.9 shows that teenagers make less risky choices in both treatments compared to control, but the difference is not statistically significantly different from zero. The degree of loss aversion *increases* in both treatments, among (endogenously) matched friends and randomly allocated classmates. In the *FRIENDS* group, this increase is small and not statistically significant compared to *CONTROL*, but we detect a large and strongly statistically significant increase in loss averse choices when classmates are matched randomly (*RANDOM* vs. *FRIENDS*; Mann-Whitney-U test, $p = 0.021$). The loss aversion parameter rises throughout the distribution in *RANDOM* compared to *CONTROL* (see Table 3.10, Kolmogorov-Smirnov test, $p = 0.084$), and leads to an increase in the percentage of loss averse adolescents by 12.9 percentage points, or 16.2%, to over 90%. While the loss aversion parameter among classmate pairs who make different choices, is very similar to mean loss aversion in the control group, the loss aversion parameter is much higher among those who coordinate perfectly, i.e. who are more prone to peer influence ($\Delta(\lambda) = 1.59$, one-sided t-test, $p = 0.0271$). All these results hold when we look at the raw choices: randomly paired interacting classmates switch later in lottery choices with mixed prospects,

[35]The study used a non-incentivised "chicken game" to elicit risk-taking.

mainly driven by those who coordinate their choices.

Table 3.10: Descriptive Statistics for Estimated Individual Parameters

	mean	median	5th	25th	75th	95th	p-value
$\bar{\sigma} i$							
CON	0.828	0.650	0.150	0.500	1.125	1.625	0.004
RAN	0.761	0.600	0.150	0.350	1.125	1.625	0.001
FRI	0.727	0.650	0.100	0.550	0.900	1.625	0.000
$\bar{\lambda} i$							
CON	2.579	1.985	0.458	1.548	2.499	7.490	0.000
RAN	3.407	2.094	0.879	1.875	3.592	10.517	0.000
FRI	2.724	1.981	0.791	1.541	2.975	10.371	0.000

Note: Full sample included; p-values from t-tests of H_0: $\sigma = 1$ or $\lambda = 1$ respectively

Result 5: Risk preferences in the communication treatments

Communication with a random classmate increases loss aversion among adolescents, especially among coordinating peers.

To summarise, peer interaction increases choice similarity in both gains and mixed lotteries, and regardless of the type of peer. In addition, the fraction of peers who make identical choices increases substantially. Coordination with a less close peer follows "rational" signal extraction criteria, meaning that individuals with higher own financial literacy are less likely to coordinate their choice with a peer, while peer similarity in publicly observed numeracy increases the likelihood of coordination (although neither classmates nor friends match assortatively on numeracy). In contrast, we find that coordination of choices between friends varies only by gender – with female pairs being more likely to coordinate. Finally, interaction with a random classmate leads to more loss averse choices. While lottery choices of classmate pairs who make non-coordinated choices are similar to those in the control group, statistically significantly more loss averse choices are made among classmates who coordinate. We speculate that this may suggest a social insurance motive among random classmates, who make very loss averse choices for fear of ending up loosing more of their initial endowment relative to their peer. This motive may be stronger among classmates than friends due to differences in the level of trust. However, more research is needed to shed light on the mechanisms behind these peer-specific effects.

3.4 Conclusion

In this paper, we studied peer effects in risk and loss-taking among adolescents. We developed an experimental strategy that uses naturally occurring peers – in our case classmates and friends – to study choices in gains and mixed lotteries. We randomised subjects into experimental conditions with and without social interaction, and explored differences between social interaction with randomly allocated classmates and mutually chosen friends. We recognised the influence of pre-existing relations among natural peers, and bridged the gap between experimental and non-experimental approaches by collecting additional information about such relationships. We used this information to analyse assortative matching on risk preferences (in the absence of social interaction).

Although friends assortatively match on socio-economic characteristics like age and gender, and on family background characteristics, we found no evidence that adolescents match on their preferences regarding risk or loss. In contrast, we found strong peer effects on incentivised risky choices that work through social interaction in the choice situation. Both treatment groups – randomly paired classmates and mutual friends – display increased choice similarity in lotteries with positive as well as with mixed prospects when they are allowed to communicate in pairs. In fact, a large fraction coordinates their choices perfectly.

One potential driver of increased choice similarity among pairs is social learning. We explore this mechanism using a private and a public signal of financial competency. We find that high own financial competency appears to make teenagers less prone to peer influence, i.e. less likely to coordinate choices. Differences in the public signal of financial competency, reduce the likelihood of choice coordination among peers. However, evidence in favour of the social learning hypothesis is found for randomly paired classmates, but not for friends. This may reflect channels of peer effects depending on the nature of the relationship.

We also find mixed results regarding the direction in which these strong effects of peer interaction affect teenagers' choices. Teenagers make more loss averse choices under peer influence, but do not seem to systematically change their risk-taking. Like the social learning result, increasingly loss averse choices are only found among communicating classmates, i.e. they may depend on the nature of the peer relation.

Finally, we find that more than half of adolescents are risk averse and over two-thirds are loss averse. As a reference sample, university students displayed more risk aversion than adolescents in the same choice task. This confirms existing evidence on how risk aversion varies with age. Additionally, we also show weakly declining loss aversion by age. To our knowledge we are the first to explore loss aversion among adolescents.

Overall, we draw important conclusions on the nature of peer influences: our results suggest that adolescents are on average risk and loss averse. Their risky choices are strongly influenced by their peers. Peer effects do not arise from assortative matching but from social interaction, so that social learning or social utility considerations may be main drivers. We re-examine the hypothesis of Carrell et al. (2013) that the nature of the peer relation matters, and find evidence in its favour (in our two treatment conditions).

We suggest the following avenues for further research: First, with the exception of Gardner and Steinberg (2005), drivers of risky decision making among adolescents remain relatively unexplored. This study provides new evidence. Peer closeness may shape the nature of peer effects, especially in adolescence. For example, social learning may be more relevant in interaction with less close peers. We developed an experimental design for the study of such effects. Experimental variation in private and public signals of task competency could be used to investigate social learning effects in more detail.

Appendices

APPENDIX A

Unleashing Animal Spirits – Self-Control and Overpricing in Experimental Asset Markets

A.1 Period-specific Price Comparisons

Looking at single periods, it is possible to get a more precise picture of when the price differences between conditions arise. Table A.1 reports the per-period differences of volume-adjusted mean prices, trade-adjusted mean prices, RAD and RD between *LOWSC* and *HIGHSC*. The z-values from Mann-Whitney tests testing the equality of the respective measures across the two conditions are displayed in parentheses with significance levels indicated by asterisks. While in the first periods we see almost no price differences, starting from period five, markets in *LOWSC* exhibit significantly higher mean prices, mispricing, and overpricing, with the peak in period 8. There are no significant differences between the two conditions in the ultimate period. By definition, this implies a more pronounced bubble and burst pattern in *LOWSC* markets than in *HIGHSC* markets.

Table A.1: Period-specific Effects

Period	Δvolume-adjusted mean price	Δtrade-adjusted mean price	ΔRAD	ΔRD
1	-0.67	-0.85	0.0143	-0.0245
	(0.84)	(0.735)	(-0.63)	(0.84)
2	0.73	2.87	-0.0749	0.0266
	(0.105)	(-0.21)	(0.21)	(0.105)
3	4.53	3.38	0.0006	0.1646
	(-0.84)	(-0.525)	(-0.105)	(-0.84)
4	7.18	7.64*	0.1720	0.2612
	(-1.47)	(-1.89)	(-1.26)	(-1.47)
5	9.24*	9.03*	0.2523	0.3359*
	(-1.785)	(-1.785)	(-1.47)	(-1.785)
6	12.27**	12.01**	0.4186**	0.4461**
	(-2.205)	(-2.31)	(-2.205)	(-2.205)
7	15.90**	15.84**	0.5703**	0.5781**
	(-2.521)	(-2.415)	(-2.521)	(-2.521)
8	18.40**	19.00**	0.6573**	0.6693**
	(-2.521)	(-2.521)	(-2.521)	(-2.521)
9	11.69**	11.78**	0.4249**	0.4249**
	(-2.1)	(-1.995)	(-2.1)	(-2.1)
10	6.13	6.48	0.2007	0.2228
	(-1.26)	(-1.26)	(-1.05)	(-1.26)

Note: Differences between *LOWSC* and *HIGHSC* and z-values (in parentheses) for Mann-Whitney tests. Volume-adjusted mean prices denote the average price per asset, while trade-adjusted mean prices denote average price per trade.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.2 Additional Regression Results

Table A.2: Determinants of Trading Activity

	(1)	(2)	(3)	(4)
	Average quantity traded			
LOWSC	-0.120	-0.0503	-2.915	-3.038
	(2.079)	(2.032)	(4.235)	(4.562)
CRT		-0.0287	-1.310*	-1.328*
		(0.842)	(0.700)	(0.716)
CE		0.685	0.558	0.542
		(0.802)	(0.820)	(0.868)
CRT × *LOWSC*			2.881*	2.874*
			(1.525)	(1.537)
CE × *LOWSC*			0.278	0.335
			(1.700)	(1.922)
Female				-0.295
				(1.942)
Constant	12.52***	11.06***	12.42***	12.69***
	(1.025)	(2.173)	(2.280)	(3.313)
Observations	110	110	110	110
R^2	0.000	0.006	0.035	0.036

Note: OLS regression, dependent variable is individual average number of trades. LOWSC is a dummy where 1 stands for *LOWSC* and 0 for *HIGHSC*. CE is an individual's certainty equivalent. CRT denotes the number of correct answers on the CRT. Subjects who indicated they knew one or more of the CRT questions before were excluded. Heteroskedasticity robust standard errors clustered at market level in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Determinants of Trading Activity (*MIXED*)

	(1)	(2)	(3)	(4)
	Average quantity traded			
MIXLO	-0.424	-0.828	-0.573	-0.586
	(2.252)	(2.157)	(5.110)	(5.143)
CRT		-1.309	0.579	0.541
		(0.807)	(1.429)	(1.462)
CE		1.653	1.217	1.161
		(0.973)	(1.191)	(1.289)
CRT × *MIXLO*			-3.984*	-3.950*
			(1.903)	(1.885)
CE × *MIXLO*			1.211	1.205
			(2.265)	(2.258)
Female				-0.422
				(1.602)
Constant	12.09***	9.805***	9.266***	9.691***
	(1.155)	(1.517)	(2.189)	(3.124)
Observations	88	88	88	88
R^2	0.001	0.040	0.084	0.085

Note: OLS regression, dependent variable is individual average number of trades. *MIXLO* is a dummy where 1 stands for *MIXLO* and 0 for *MIXHI*. CE is an individual's certainty equivalent. CRT denotes the number of correct answers on the CRT. Subjects who indicated they knew one or more of the CRT questions before were excluded. Heteroskedasticity robust standard errors clustered at market level in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Ratings of Emotions in *MIXED* Markets

	MIXHI	*MIXLO*	p-value
Excitement1	4.200	4.500	0.400
Fear1	2.100	2.175	0.395
Surprise1	3.600	4.050	0.103
Anger1	1.800	2.025	0.440
Relief1	2.825	3.250	0.161
Sadness1	1.525	1.725	0.324
Joy1	3.625	4.375	0.058*
Excitement2	3.425	4.200	0.042**
Fear2	1.900	2.575	0.014**
Surprise2	2.450	3.400	0.030**
Anger2	2.025	2.000	0.723
Relief2	3.275	4.150	0.233
Sadness2	1.950	1.725	0.622
Joy2	3.375	4.125	0.207
Emotion intensity	2.720	3.163	0.025**
Emotion valence	1.464	1.969	0.208
Emotion intensity1	2.811	3.157	0.123
Emotion valence1	1.754	2.069	0.123
Emotion intensity2	2.629	3.168	0.025**
Emotion valence2	1.604	1.944	0.400

Note: p-values from Wilcoxon-Signed Rank tests collapsing data on the market level by *MIXLO* and *MIXHI* respectively; emotion intensity is the average score over all emotion questions, emotion valence is the average score over all positive emotions minus the score over all negative emotions; variables ending in 1 or 2 relate to questions at the beginning (1) or the end (2) of the asset market, respectively; *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Changes of ex-post Emotion Ratings in *MIXED* Markets

	MIXHI	*MIXLO*	p-value
Diff excitement	-0.775	-0.300	0.232
Diff fear	-0.200	0.400	0.029**
Diff surprise	-1.150	-0.650	0.288
Diff anger	0.225	-0.025	0.575
Diff relief	0.450	0.900	0.441
Diff sadness	0.425	0.000	0.290
Diff joy	-0.250	-0.250	1.000

Note: p-values from Wilcoxon-Signed Rank tests collapsing data on the market level by *MIXLO* and *MIXHI* respectively; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.6: Determinants of Individual RD Based on Sales

	(1)	(2)	(3)	(4)
		$IndRD_{sales}$		
LOWSC	0.355**	0.350**	0.605**	0.648**
	(0.146)	(0.143)	(0.243)	(0.221)
CRT		-0.0488	-0.0774	-0.0712
		(0.0395)	(0.0613)	(0.0608)
CE		0.00173	0.0584	0.0639
		(0.0551)	(0.0617)	(0.0634)
CRT × *LOWSC*			0.0684	0.0709
			(0.0800)	(0.0782)
CE × *LOWSC*			-0.146	-0.167
			(0.109)	(0.106)
Female				0.103
				(0.0655)
Constant	0.172	0.210	0.111	0.0164
	(0.106)	(0.147)	(0.104)	(0.110)
Observations	110	110	110	110
R^2	0.227	0.241	0.269	0.283

Note: OLS regression, dependent variable is Individual Relative Deviation (IndRD) for sales, an individual equivalent to market level Relative Deviation (RD) restricted to sales only. *LOWSC* is a dummy where 1 stands for *LOWSC* and 0 for *HIGHSC*. CE is an individual's certainty equivalent. CRT denotes the number of correct answers on the CRT. Subjects who indicated they knew one or more of the CRT questions before were excluded. Heteroskedasticity robust standard errors clustered at market level in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Determinants of Individual Miscpricing

	(1)	(2)	(3)	(4)
		IndRD		
LOWSC	0.383**	0.375**	0.723***	0.760***
	(0.138)	(0.134)	(0.160)	(0.147)
CRT		-0.0593	-0.0775	-0.0722
		(0.0349)	(0.0574)	(0.0568)
CE		-0.0155	0.0553	0.0600
		(0.0457)	(0.0461)	(0.0475)
CRT × *LOWSC*			0.0461	0.0483
			(0.0722)	(0.0728)
CE × *LOWSC*			-0.182**	-0.200**
			(0.0784)	(0.0774)
Female				0.0884
				(0.0584)
Constant	0.119	0.203	0.0648	-0.0159
	(0.0979)	(0.125)	(0.0713)	(0.0774)
Observations	110	110	110	110
R^2	0.299	0.326	0.370	0.382

Note: OLS regression, dependent variable is Individual Relative Deviation (IndRD), an individual equivalent to market level Relative Deviation (RD). *LOWSC* is a dummy where 1 stands for *LOWSC* and 0 for *HIGHSC*. CE is an individual's certainty equivalent. CRT denotes the number of correct answers on the CRT. Subjects who indicated they knew one or more of the CRT questions before were excluded. Heteroskedasticity robust standard errors clustered at market level in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table A.8: Second-Period Differences in Trading Behavior

	Group Mean		
	MIXHI	*MIXLO*	p-value
$\overline{p_{bid}}$	29.400	31.803	0.510
$\overline{p_{ask}}$	50.383	55.751	0.039**
$\overline{q_{bid}}$	15.441	15.407	0.659
$\overline{q_{ask}}$	13.291	11.687	0.796
$\overline{time_{bid}}$	54.392	49.446	0.470
$\overline{time_{ask}}$	48.067	45.576	0.587
$\overline{firsttime_{bid}}$	42.484	40.912	0.683
$\overline{firsttime_{ask}}$	28.344	28.591	0.717

Note: Variables starting with a p denote prices, q quantities and time variables refer to the time passed in the current period, thus lower values indicate behavior earlier on. *bid* and *ask* refer to posted bids and asks, p-values from Wilcoxon signed-rank tests with data collapsed on market and treatment level, *** p<0.01, ** p<0.05, * p<0.1

Table A.9: Determinants of Trading Profits

	(1)	(2)	(3)	(4)
	Trading Profits			
MIXLO	1.036	1.040	4.342*	4.301*
	(0.770)	(0.795)	(2.222)	(2.215)
CRT		1.084**	1.882***	1.757**
		(0.497)	(0.621)	(0.691)
CE		0.473	0.867	0.685
		(0.550)	(0.768)	(0.753)
CRT × *MIXLO*			-1.660**	-1.547**
			(0.642)	(0.690)
CE × *MIXLO*			-1.031	-1.051
			(1.125)	(1.098)
Female				-1.381
				(0.888)
Constant	7.035***	5.302***	3.936***	5.326***
	(0.441)	(1.097)	(1.323)	(1.638)
Observations	88	88	88	88
R^2	0.016	0.079	0.120	0.145

Note: OLS regression, dependent variable is average trading profit from asset market in €˙ CRT denotes the number of correct answers in the CRT. Subjects who indicated they knew one or more of the CRT questions before were excluded. CE is the individual certainty equivalent, robust standard errors clustered on market level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

A.3 Distribution of Answers in the Stroop Task

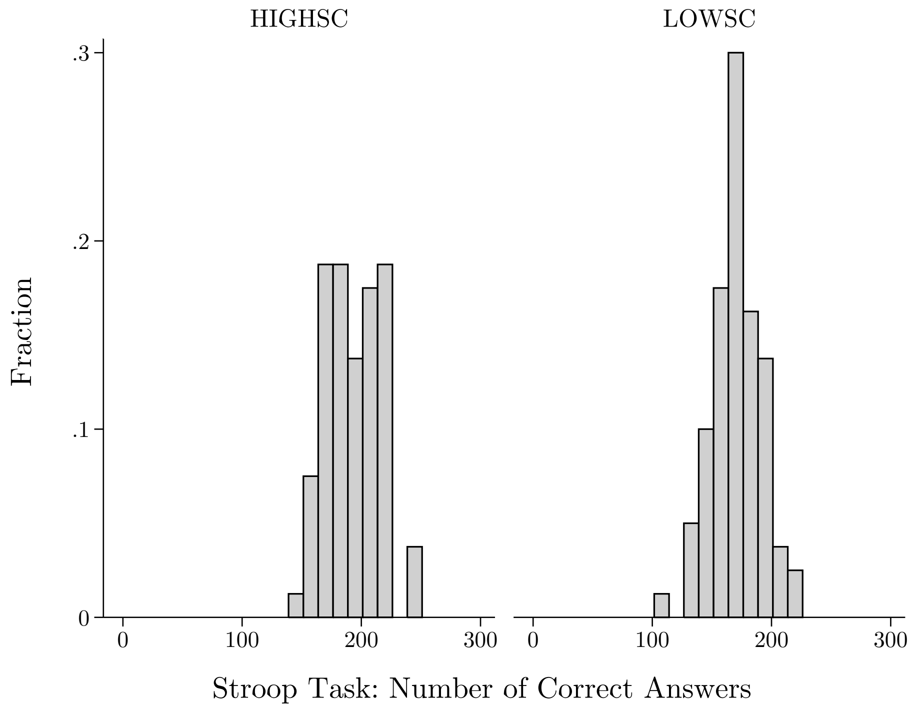


Figure A.1: Correct Stroop responses in *HIGHSC* vs. *LOWSC*

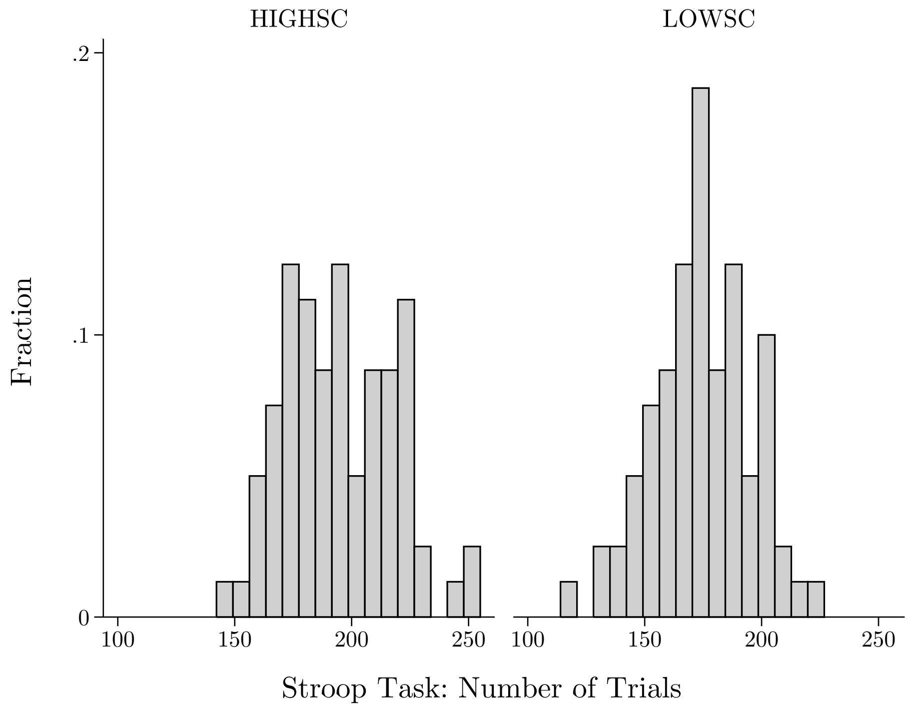


Figure A.2: Stroop Trials in *HIGHSC* vs. *LOWSC*

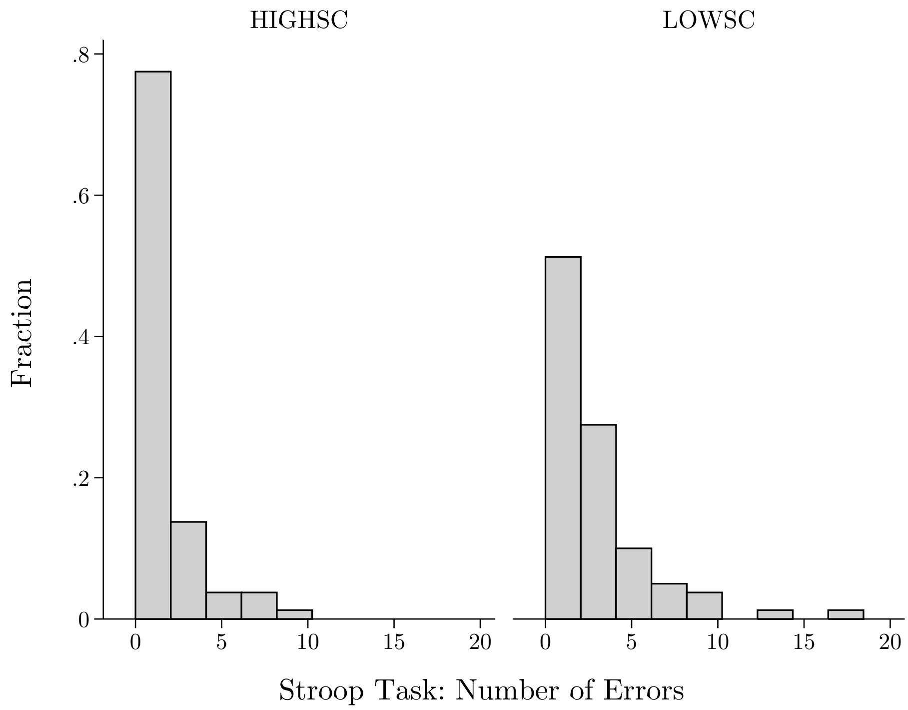


Figure A.3: Errors in the Stroop Task in *HIGHSC* vs. *LOWSC*

Table A.10: Distribution of Answers in the Stroop Task

HIGHSC	Mean	Standard deviation
Correct Answers	192.65	22.61
Trials	194.55	23.56
Errors	1.9	1.88
LOWSC	Mean	Standard deviation
Correct Answers	171.3125	20.68
Trials	174.45	20.97
Errors	3.14	2.97

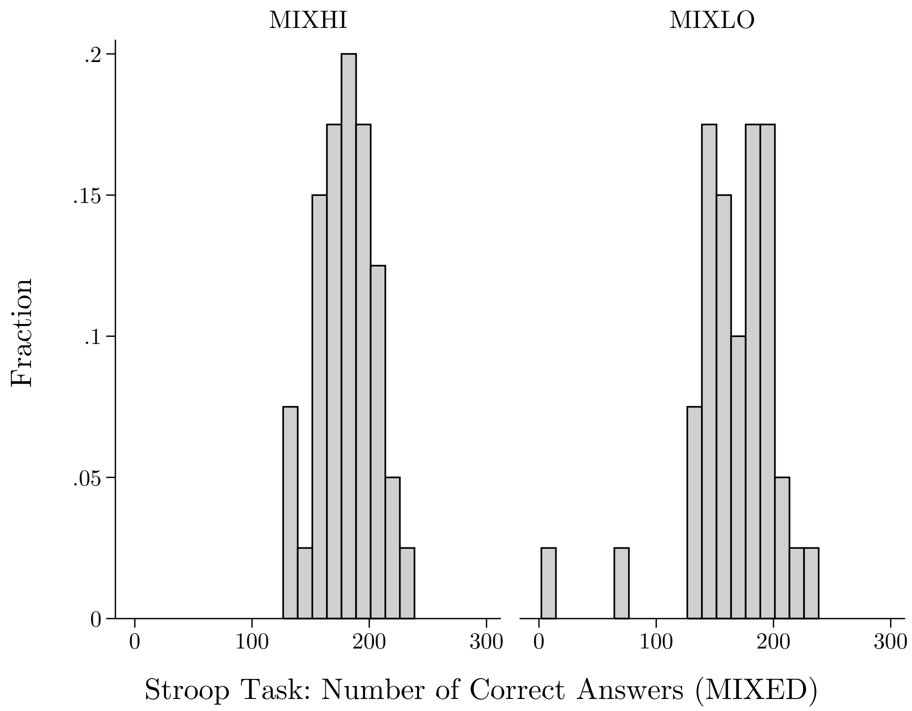


Figure A.4: Correct Stroop Responses in *MIXED* by Condition

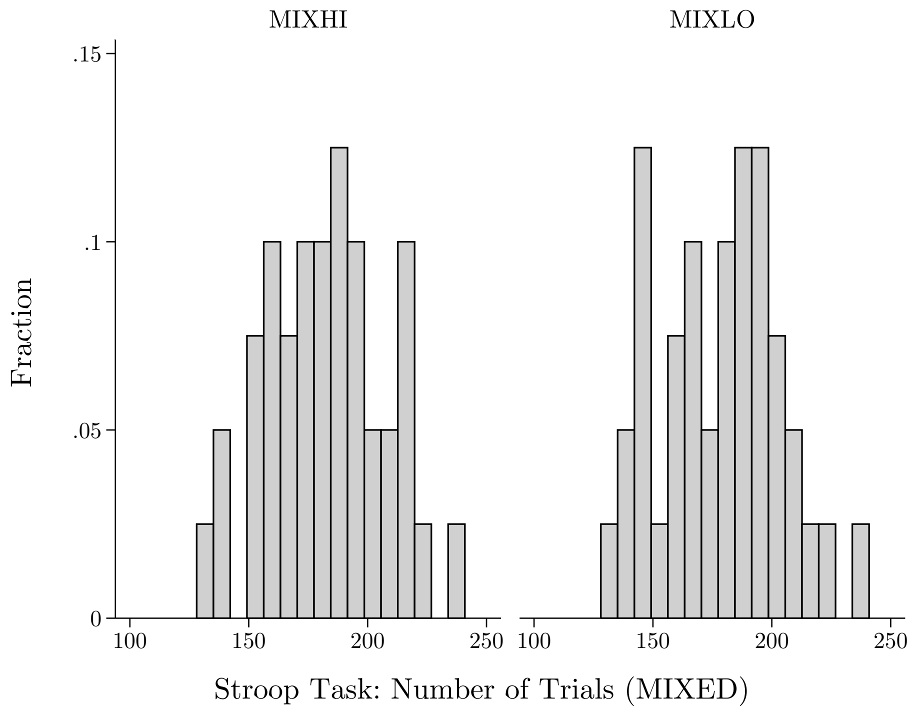


Figure A.5: Stroop Trials in *MIXED* by Condition

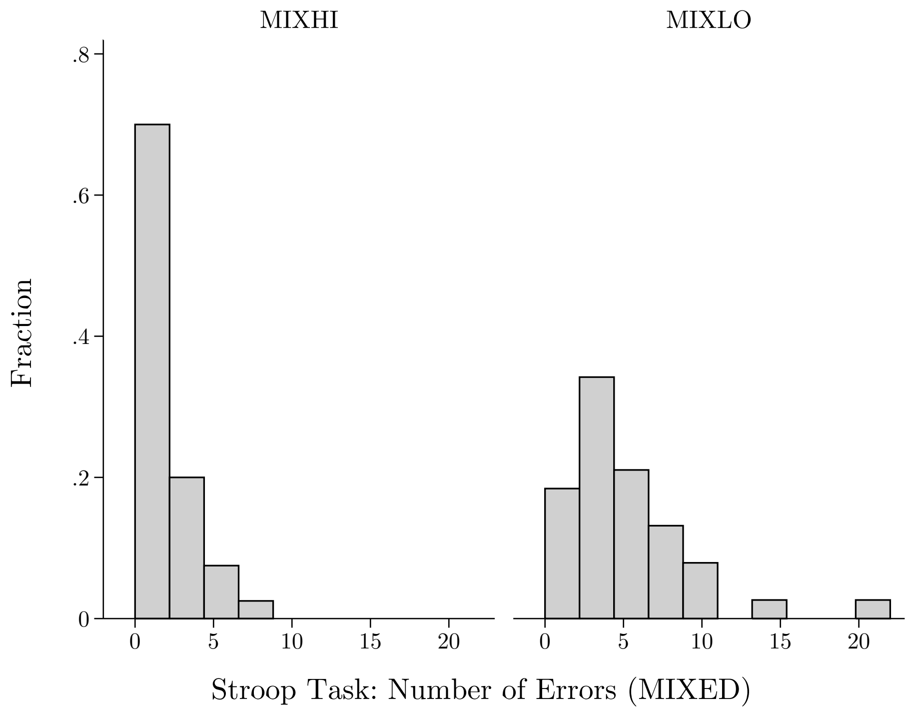


Figure A.6: Errors in the Stroop Task in *MIXED* by Condition[1]

Table A.11: Distribution of Answers in the Stroop Task (*MIXED*)

MIXHI	Mean	Standard deviation
Correct Answers	179.23	24.11
Trials	182.65	24.60
Errors	2.425	1.45
MIXLO	Mean	Standard deviation
Correct Answers	164.05	39.94
Trials	178.3	25.48
Errors	13.25	36.44

[1]Two outliers were dropped from this display in the *MIXLO* group, both of whom apparently did not fully understand the task. One had 123 errors and the other had 205 errors.

A.4 Distribution of Subjective Measures

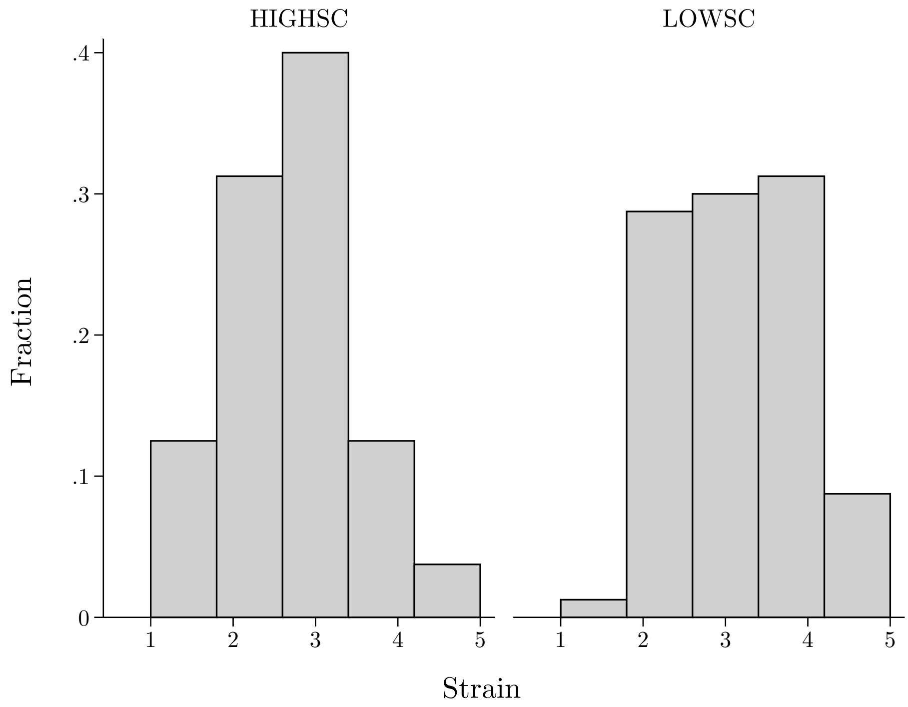


Figure A.7: Strain in *HIGHSC* vs. *LOWSC*

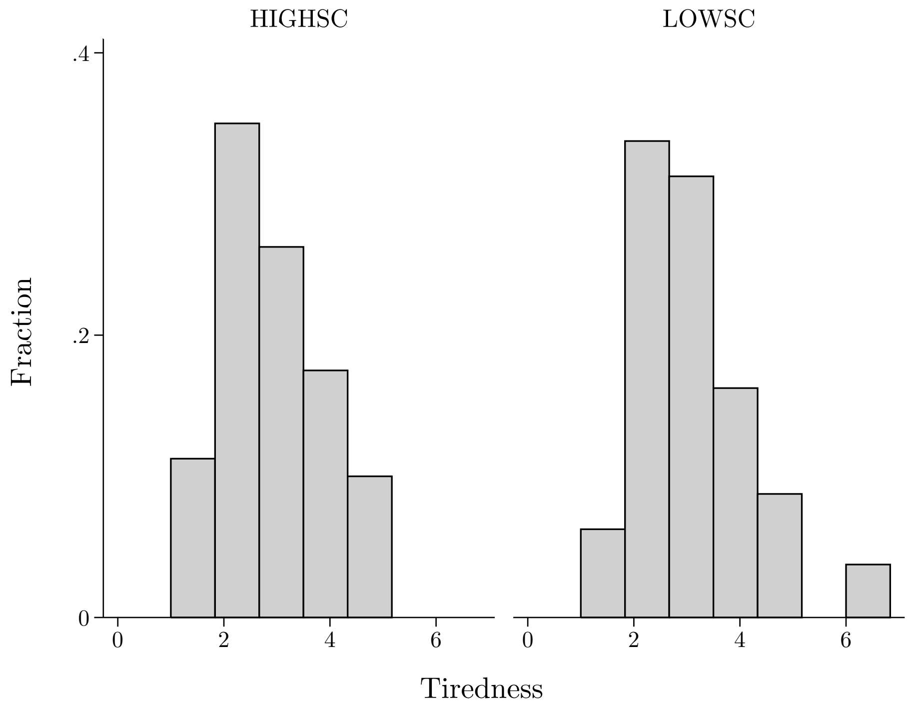


Figure A.8: Tiredness in *HIGHSC* vs. *LOWSC*

Table A.12: Distribution of Subjective Measures

HIGHSC	Mean	Standard deviation
Strain	2.64	0.98
Tiredness	2.80	1.16
LOWSC	Mean	Standard deviation
Strain	3.18	0.99
Tiredness	2.99	1.21

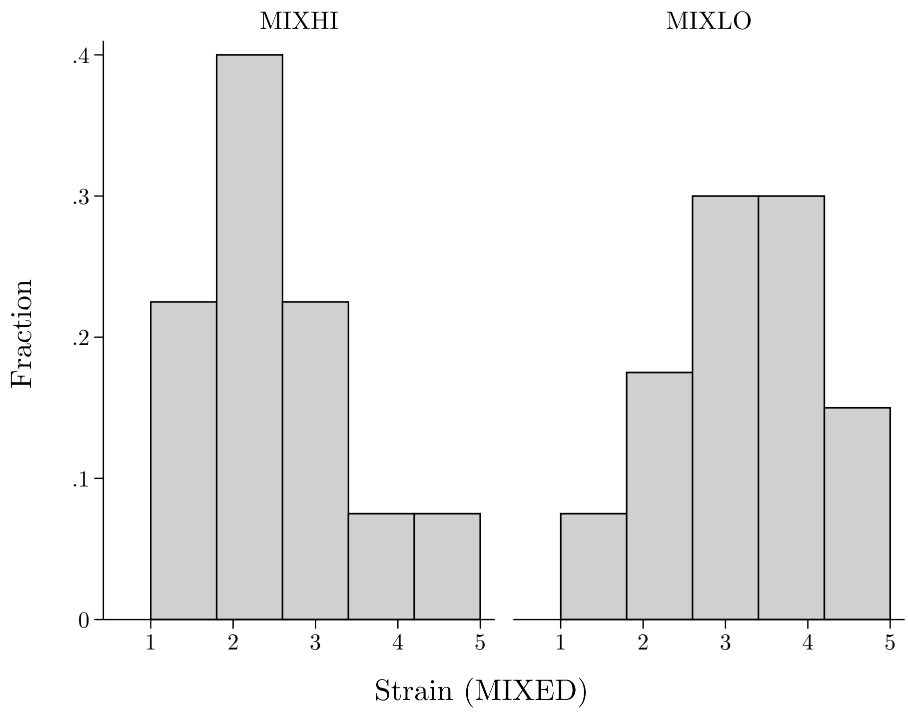


Figure A.9: Strain in *MIXED* by Condition

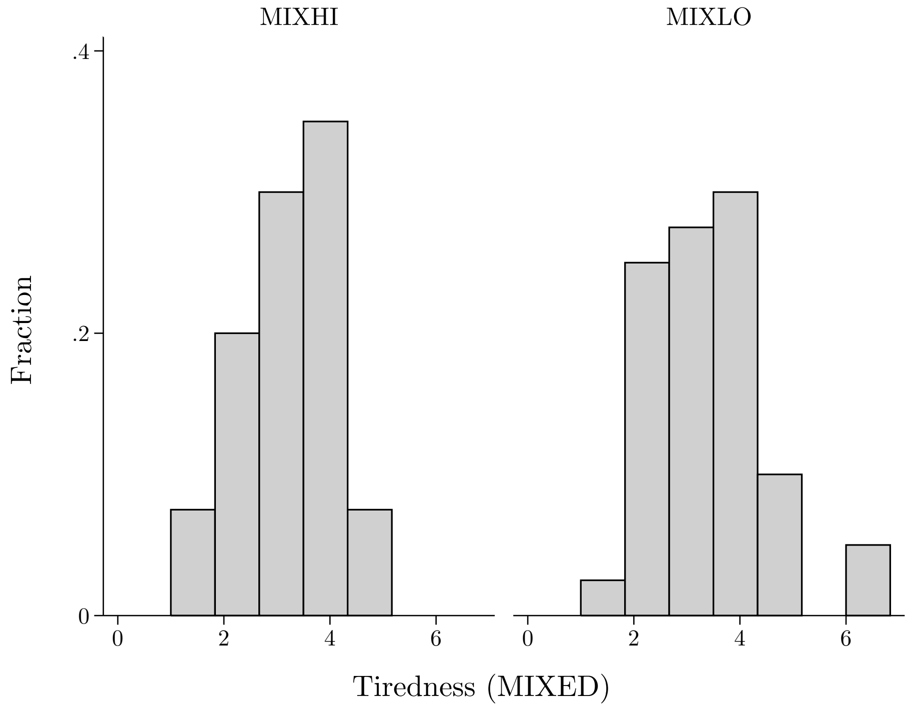


Figure A.10: Tiredness in *MIXED* by Condition

Table A.13: Distribution of Subjective Measures (*MIXED*)

MIXHI	Mean	Standard deviation
Strain	2.375	1.15
Tiredness	3.15	1.08
MIXLO	Mean	Standard deviation
Strain	3.275	1.15
Tiredness	3.35	1.19

A.5 Distribution of Answers in the Cognitive Reflection Test

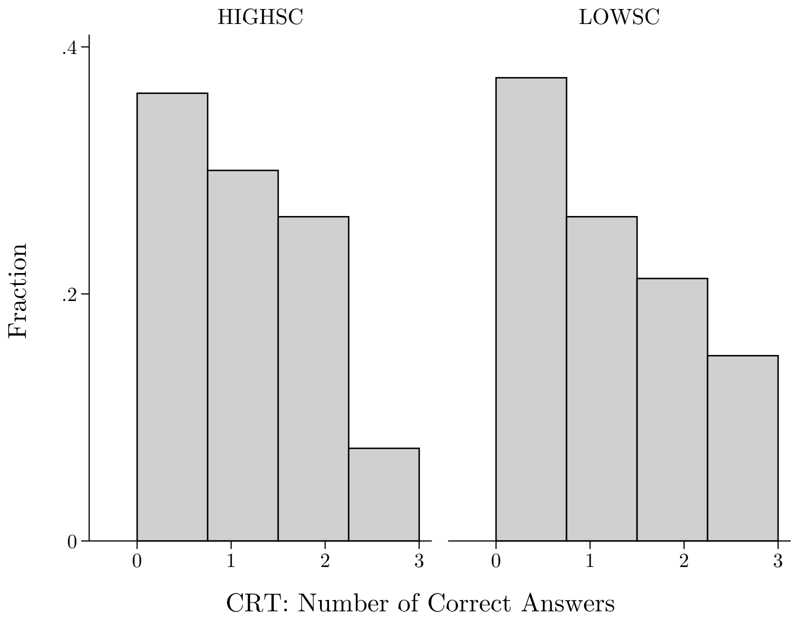


Figure A.11: Correct CRT Answers in *HIGHSC* vs. *LOWSC*

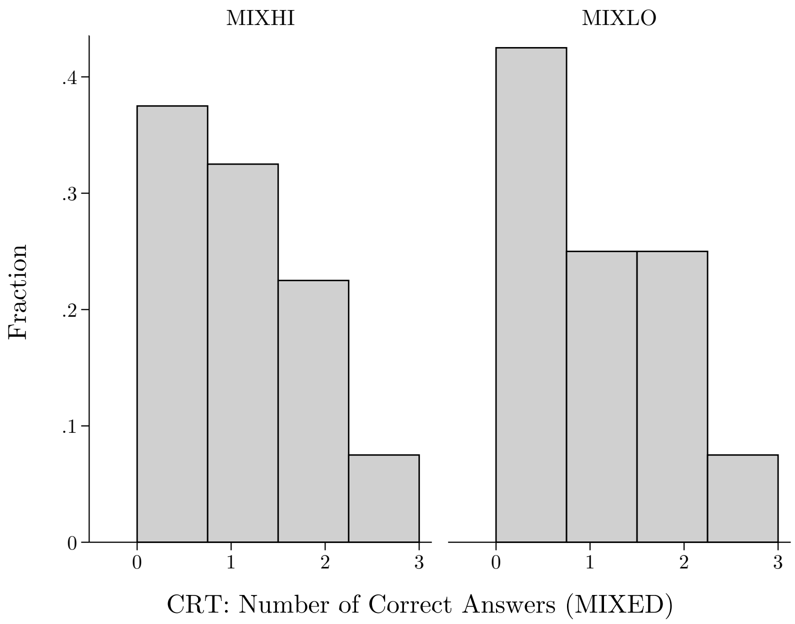


Figure A.12: Correct CRT Answers in *MIXED* by Condition

Table A.14: Distribution of Answers in the Cognitive Reflection Test

	Mean	Standard deviation
HIGHSC	1.05	.97
LOWSC	1.14	1.09
MIXED	Mean	Standard deviation
MIXHI	1.00	.96
MIXLO	.98	1.00

A.6 Distribution of Certainty Equivalents

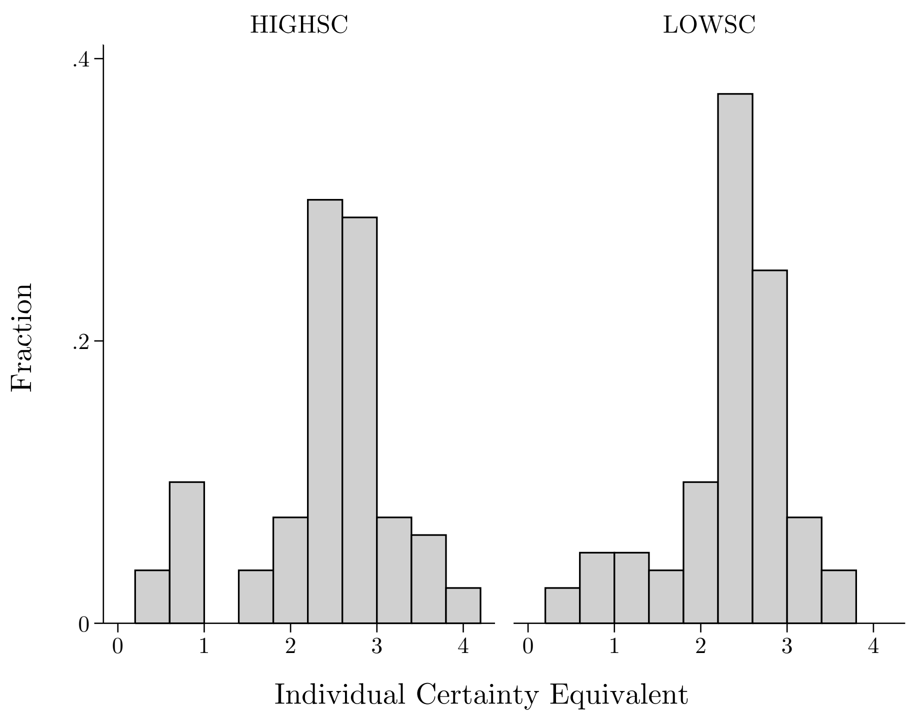


Figure A.13: Individual Certainty Equivalents in *HIGHSC* vs *LOWSC*

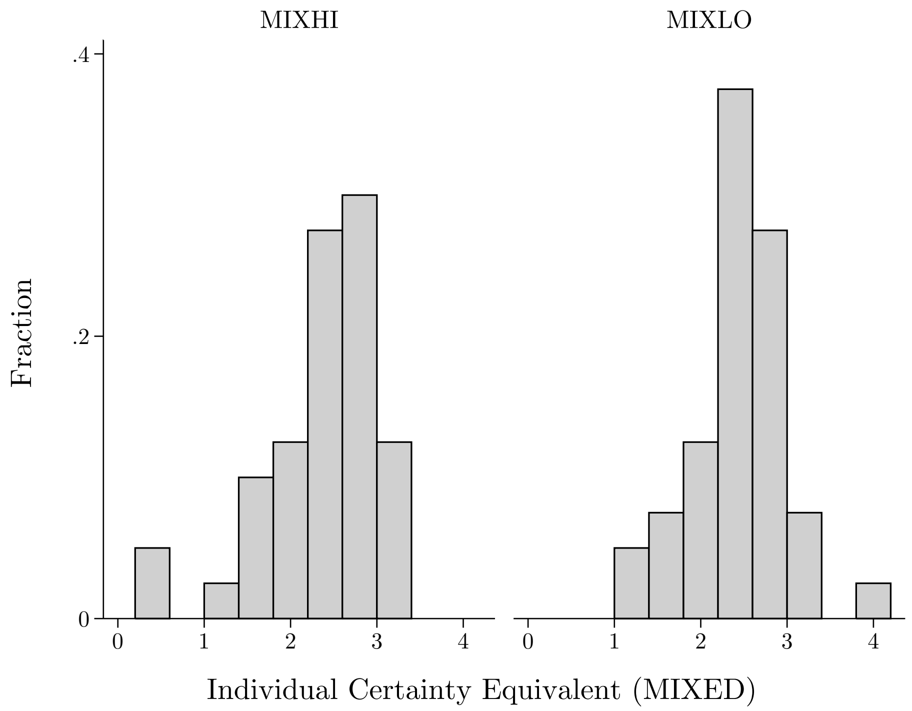


Figure A.14: Individual Certainty Equivalents in *MIXED* by Condition

Table A.15: Distribution of Individual Certainty Equivalents

	Mean	Standard deviation
HIGHSC	2.20	.85
LOWSC	2.15	.70
MIXED	Mean	Standard deviation
MIXHI	2.16	.68
MIXLO	2.24	.55

A.7 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

The purpose of this experiment is to investigate economic decision making. You can earn money during the experiment, which will be paid to you individually and in cash after the experiment has ended.

The whole experiment takes about 1.5 hours and consists of 3 parts. At the beginning you will receive detailed instructions for all parts of the experiment. If you have any questions after reading the instructions or at any time during the experiment please raise your hand. One of the experimenters will then come to you and answer your question in private.

During the experiment, you and the other participants will be asked to make decisions. In some parts, you will interact with other participants. Thus both your own decisions and the decisions of other participants can determine your payoffs. Your payoffs are determined according to the rules which are explained in the following. As long as you can make your decisions, a countdown will be displayed in the upper right corner of the screen which is intended to give you an orientation for how much time you should use to make your choices. In most parts you can exceed the time limit if needed; in some parts, however, you can only act within the time limit (You will be informed about this beforehand). Information screens not requiring any decisions will disappear after the time-out.

Payment

In some parts of the experiment we will not refer points instead of Euros. Points will be converted to Euros at the end of the experiment. You will be informed about the exchange rate at the beginning of the respective part.

For your timely arrival you will receive 4 € additionally to the income earned during the experiment.

Anonymity

We evaluate the data from the experiment only in aggregate and never connect personal information to data from the experiment. At the end of the experiment you have to sign a receipt, which we need for our sponsor. The sponsor does not receive any further data from the experiment.

Aid

On your desk you will find a pen. Please leave it on there after the experiment.

Part I

Task

The first part of the experiment consists of a task that will last 5 minutes. You will see a black screen on which words in different colors will appear. Here you can see an example:

You will be asked to click one of the buttons at the bottom of the screen. You will be asked to choose the button corresponding to the color the word is written in (**not** the word itself). In the example you should click on "yellow".

After clicked a button, the screen disappears and **another word in another color** appears. Please try to solve **as many word/color combinations** as possible within 5 minutes.

After 5 minutes the first part ends automatically and the second part begins.

Payment

You receive 3 € for part I.

Part II

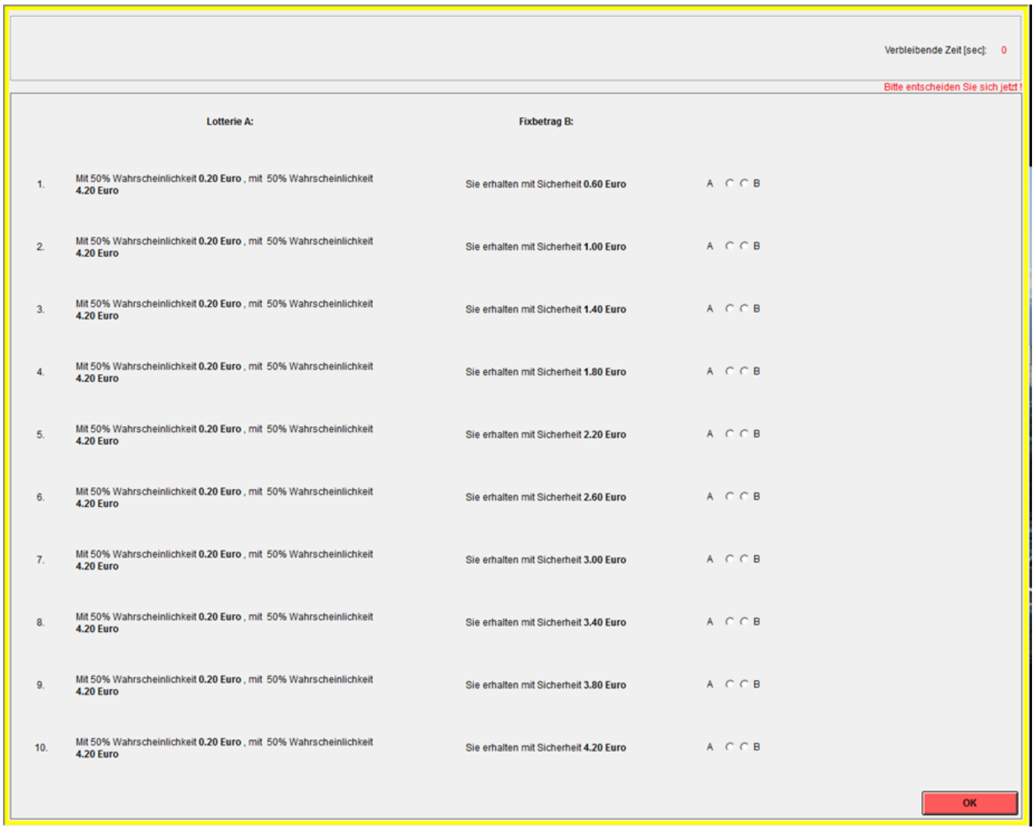
Task

In the second part you first have to answer three questions. For each question answered correctly you receive 0.5 € = 50 Cents.

Afterwards, you will be shown **10 decision problems**. In each of these problems you can choose between **a lottery and a safe amount of money**. The lottery remains unchanged within a period, whereas the safe amount of money increases with every additional decision problem. As the safe amount of money is strictly increasing from row to row, you should stay with the safe amount of money after you have switched to it once.

Your decision is only valid after you have made a choice for each problem and then confirmed it by clicking the OK-button on the bottom right of the screen. Take enough time for your decisions, as your choice – as described in the following – will determine your payoff from this part.

Here you can see what your screen will look like:



Your profit will be determined according to the following rules: First, **the computer chooses randomly and with equal probability one of the ten decision problems**

for payment. If you selected the lottery in the relevant problem, the computer will simulate the outcome and you will receive it as payment. If you selected the safe amount in the relevant problem, you will receive it for sure.

For example: Assume the computer randomly chooses the first decision problem and you chose the lottery. Then the computer will simulate the outcomes of this lottery and you either receive 0.2 € (50% probability) or 4.2 € (50% probability).

Payment

The sum of your payoffs from the questions answered correctly at the beginning and your payoff from the decision problem chosen by the computer are your payment for part II of the experiment.

Please note: The computer will directly calculate the result. However, you will only learn about this at the end of the experiments, i.e. how many questions you answered correctly and which decision problem with which outcome the computer selected for you. That information will be presented to you on a separate screen at the end of the experiment.

After the end of part II, part III begins automatically.

Part III

Payment

In the third part of the experiment we refer to points rather than Euros. Points are converted to Euros at the end of the experiment according to the following exchange rate

500 points = 1 Euro (1 point = 0.002 Euros = 0.2 Cents)

Short Description

The third part of the experiment consists of a simulated stock market. The stock market lasts for 10 consecutive periods. Within these periods you can buy or sell shares of a single firm.

At the end of each period for every share that you own you receive either a dividend of 10 points (probability 50%) or 0 points (probability 50%).

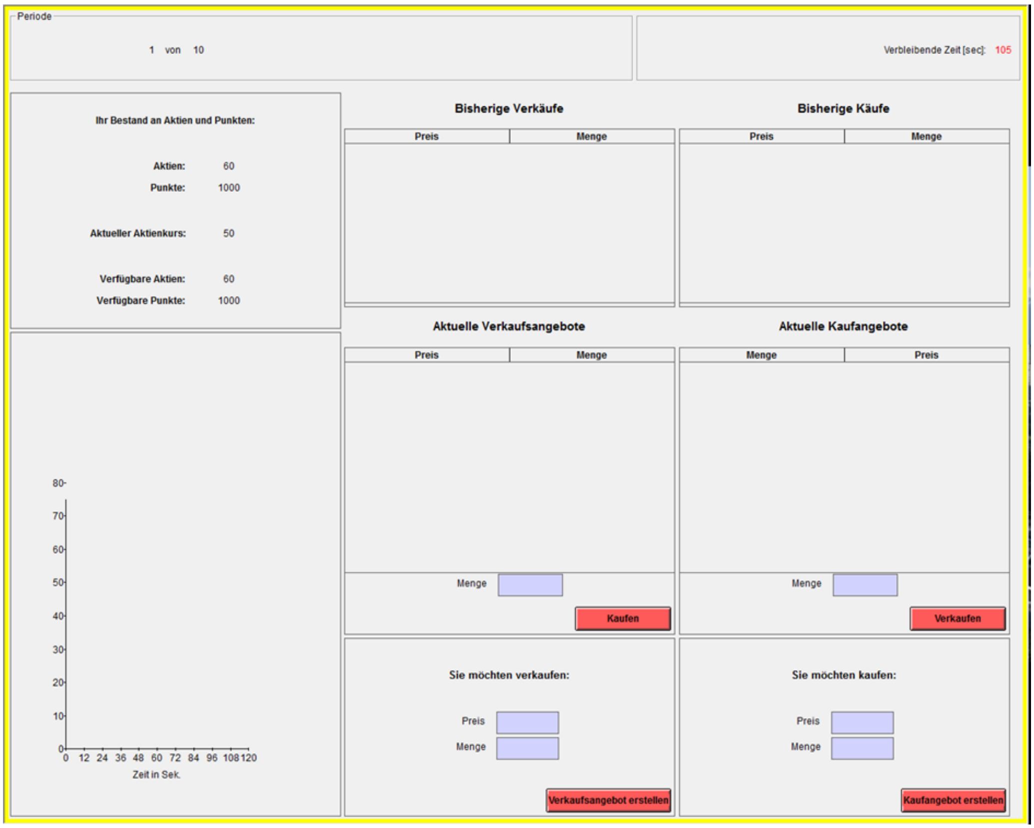
During the 2 minutes trading period you can either offer to sell or buy shares or accept existing buying or selling offers by other participants.

Detailed description: Trading Period

At the beginning of the first trading period you will receive an endowment of shares and points. Every participant receives either 20 shares and 3000 points or 60 shares and 1000 points. The distribution of endowments is random with a 50% probability of receiving each endowment.

Each period lasts exactly 120 seconds (= 2 minutes) and all screens disappear after the time out. You cannot make any trades or offers until he next trading period starts. During a trading period neither your amount of shares nor your amount of points can fall below zero.

During a trading period your screen will look like the following.



In the upper box you see the current period and how much time you have left in the current period. Below it to the left the box displays how many shares you currently own and how large your current wealth is expressed in points. Additionally the current share price and the amount of available shares and points are displayed.

Available shares are those of your shares that you have not offered for sale yet. If you offer to sell shares, you still own them, but they will be subtracted from your account as soon as someone else accepts your offer. Hence, you can only make sale offers that do not exceed your current amount of available shares.

Available points are those of your points that you have not used for buying offers yet. If you make an offer to buy shares, you still own the points, but they will be subtracted from your account as soon as someone else accepts your offer. Hence, you can only make buying offers that do not exceed your current amount of available points.

On the bottom left you can see a graph that shows the evolution of share prices in the current period. On the horizontal axis (the x-axis) you can see the time in seconds at which a trade was made. On the vertical axis (the y-axis) you can see the corresponding price.

In the upper part of the screen you see two lists that have the headlines “Previous Sales” and “Previous Purchases”. Here, every trade that you made is listed. For each trade where you bought shares, price and quantity will be listed in “Previous Purchases”. For each trade where you sold shares, price and quantity will be listed in “Previous Sales”.

Below you find two lists with the headlines “Current Selling Offers” and “Current Buying Offers”.

Accepting Selling Offers

In the list “Current Selling Offers” you find price and quantity of each offer, in which a participant offers to sell shares. Your own selling offers will also appear in this list. You can accept every offer in this list (except for your own offers) by marking the corresponding entry in the list, entering the quantity you want to buy into the field “quantity”, and then confirming by clicking on the button “Buy”. If you accept a selling offer, you will receive the number of shares that you have entered from the seller and the seller receives the corresponding price for each share he sold to you.

Please note: You can also buy less than the number of shares stated in the offer. In that case the offer of the seller will remain on display in the list after the trade, but the number of shares on offer will be reduced by your purchase. Example: A seller makes an offer to sell 10 shares at the price of 60 points each. A buyer buys 6 of those shares. Then an offer to buy 4 shares at the price of 60 points each will continue to be available to all other participants.

Please note that the computer automatically marks the best selling offer (i.e. the one with the lowest price) with a blue bar. You can recognize your own offers, as they are not displayed in black but in blue font.

Accepting offers to buy

In the list “Current Buying Offers” you find price and quantity of each offer, in which a participant offers to buy shares. Your own buying offers will also appear in this list. You can accept every offer in this list (except for you own offers) by marking the corresponding entry in the list, entering the quantity you want to sell into the field “quantity”, and then confirming by clicking on the button “Sell”. If you accept a buying offer, the other participant will receive the number of shares that you entered and you receive the corresponding price for each share you sold.

Please note: You can also sell less than the number of shares the buyer offers to buy. In that case the offer of the buyer will remain on display in the list after the trade, but the number of shares demanded will be reduced by your sale.

Please note that the computer automatically marks the best buying offer (i.e. the one with the highest price) with a blue bar. You can recognize your own offers according to their blue font.

Creating Selling or Buying Offers

In the bottom part of the screen you have the possibility to create your own selling or buying offers. If you want to create an offer to sell, enter the quantity of shares that you want to sell and the price per share which you demand for each unit in the field below "You Want to Sell" . After clicking the button "Create Selling Offer", your selling offer will show up in the list "Current offers to sell". Example: You want to sell 10 shares at a price of 55 points per share. Then you enter 10 into the field "Quantity" and 55 into the field "Price".

If you want to create a buying offer, enter the quantity that you want to buy in the field below "You Want to Buy" and the price per share for which you are willing to buy that quantity. After clicking the button "Make Buying Offer" your offer will show up in the list "Current Buying Offers". Example: You want to buy 20 shares at a price of 45 points per share. Then you enter 20 into the field "amount" and 45 into the field "price".

Please note: An offer to buy or to sell that has been made cannot be cancelled. Only if no one accepts an offer during the course of a trading period, it will not be displayed in the next period of trade.

Dividends

After the end of a trading period the following screen displays a summary of the previous period showing you how many shares and points you own, whether a dividend has been paid and if so, how large your overall dividend payments were.

In each period the dividend per share either amount to 10 points (with a probability of 50%) or to 0 points (with a probability of 50%) and is the same for all shares. After the end of period 10, all shares are worthless. All participants learn the realization of the dividend simultaneously on a separate screen at the end of the corresponding period.

The following table displays the value pattern of a share, i.e. the expected value of the remaining dividends. The first column indicates the current period, in the second column you find the number of remaining dividend payments. The third column shows the average expected dividend per share and period. The last column shows the average of remaining dividends per share in the corresponding period.

Current period	Remaining dividend payments	x	Average dividend value per period (0 or 10 with equal probability)	=	Average remaining dividends per share that you own
1	10		5		50
2	9		5		45
3	8		5		40
4	7		5		35
5	6		5		30
6	5		5		25
7	4		5		20
8	3		5		15
9	2		5		10
10	1		5		5

Assume for example that four trading periods remain. As the dividend per share is either 0 or 10 points with a probability of 50% each, this yields an expected dividend of 5 points per share and period. Assume you only own one single share which you intend to hold until the market closes. Then you can expect a total dividend payment for the four remaining periods of '4 remaining periods' x '5 points' = '20 points'.

Payoff

At the end of part III the shares no remaining value. Only your amount of points will be converted to Euros according to the exchange rate stated above of 1 point = 0.002 Euros = 0.2 Cents.

Afterwards, you will see a screen displaying your payoffs from the second part.

In the following, we will ask you to completely and honestly answer some questions concerning your person. On leaving the laboratory, we will pay you your profit privately and in cash. Please remain seated until we call you up in a random order. Please leave the instructions and the pen at your desk and take your numbered seat card with you.

Practice Period

Before you start today's experiment with part I, you will first play a practice period of part III to become familiar with the stock market. The payoff from this practice period will not influence your final payoff. Please note that the realization of the dividend and your endowment are not necessarily identical to the first period of part III as the realization is random and endowments will be randomly assigned.

After completion of the practicing period part I of the experiment begins.

APPENDIX B

The Impact of Self-Control on Investment Decisions

B.1 Appendix for Experiment 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., 2016a). 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. (2016a) 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.[1] 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)[2] 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 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).

[1] 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 B.1.

[2] The exact wording of the questions can be found in appendix B.1

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.

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.

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 B.1 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. (2016a) who also found no direct effect of their self-control manipulation on the CRT score. Note however, that unlike in Kocher et al. (2016a), 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. (2016a).

Secondly, again in line with Kocher et al. (2016a) 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 B.1: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Additional Tables and Figures

Table B.2: 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 B.3: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

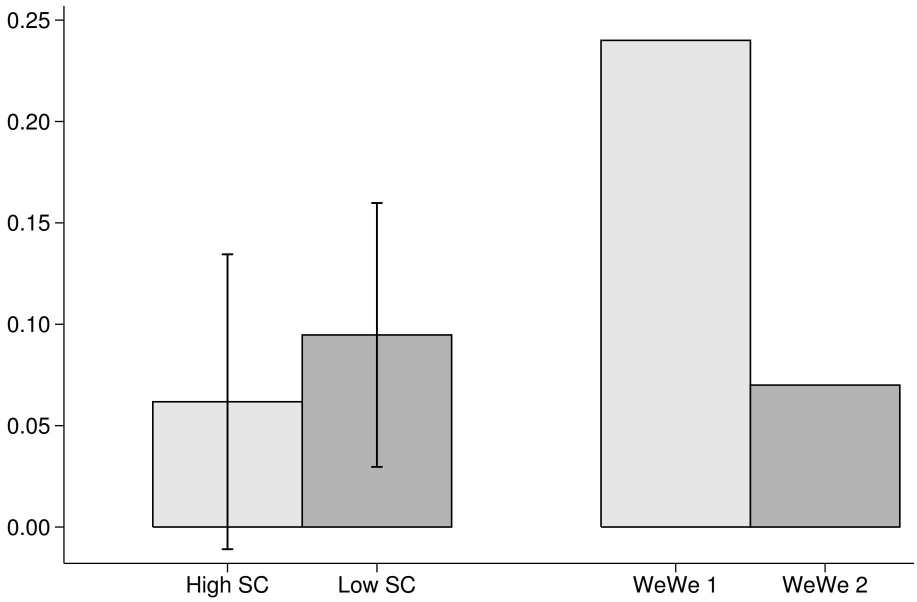


Figure B.1: Disposition Effect Measure by Treatment and Comparison with Weber and Welfens (2007)

Table B.4: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.5: 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 B.6: 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 B.7: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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:

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

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

$$|\Delta TC| = |TC - TC^{*}| \tag{B.2}$$

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.

B.2 Appendix for Experiment 2

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.[3] 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

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

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.

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)

Effect on Additional Measures

Table B.8 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. (2016a) that the self-control reducing treatment does not impact cognitive abilities significantly.

Table B.8: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Additional Tables and Figures

Table B.9: 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 B.10: 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 B.11: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Heterogeneity – BIS

Table B.12: 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 B.13: 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

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

Heterogeneity – CRT

Table B.14: 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 B.15: 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 B.16: 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

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

Tobit Regressions by Frame

Table B.17: 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 × 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 × 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 × Low SC						10.20**
						(4.401)
wins last 3						-7.185***
						(2.432)
wins last 3 × Low SC						-7.156**
						(3.564)
wealth						-0.00931
						(0.00923)
wealth × 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 B.18: 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

B.3 Instructions

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.

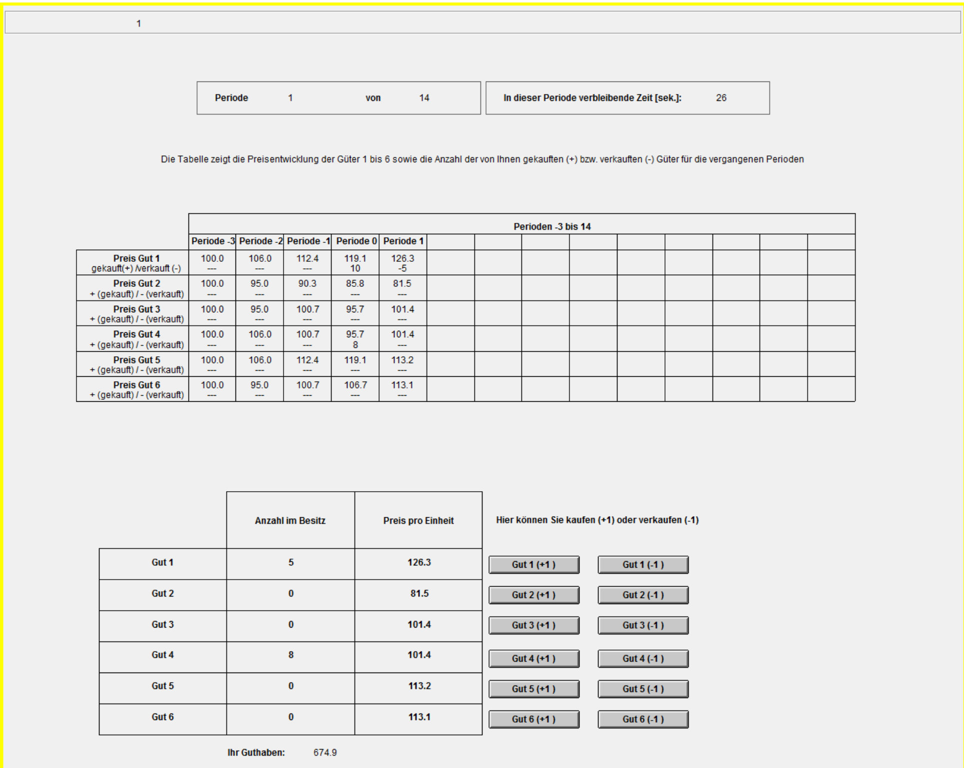
Price changes in one period are independent from price changes in other periods, i.e. the probability of a price increase of a certain good stays constant within all periods. In addition the price changes between two different goods are independent. Furthermore neither you nor other participants can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



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:

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!

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.

APPENDIX C

Peer Effects in Risk Preferences among Adolescents

C.1 Additional Summary Statistics

Table C.1 reports descriptive statistics of the whole sample and separately for each treatment. The last column reports p-values from χ^2 tests.

Table C.1: Descriptive Statistics Full Sample

	TOTAL	CONTROL	RANDOM	FRIENDS	p-value
students	235	77	59	99	
classes	14	5	4	5	
schools	7	4	4	4	
contact	2.536	2.750	2.250	2.516	0.004
training	0.371	0.295	0.383	0.424	0.204
grade	7.785	7.795	7.717	7.818	0.367
age	14.117	13.937	14.077	14.281	0.138
male	0.586	0.636	0.544	0.571	0.520
relmath	0.000	0.000	0.000	-0.000	0.000
sum m correct	0.797	0.705	0.750	0.899	0.246
Risk	3.144	3.143	3.152	3.140	0.770
singleparent	0.273	0.328	0.327	0.200	0.122
hh size	3.072	2.973	3.111	3.128	0.116
language2	0.366	0.293	0.434	0.386	0.232
books	2.532	2.699	2.679	2.319	0.250

Note: p-values are from χ^2 tests

C.2 Switching Points and Estimated Preference Parameters

Table C.2 shows the distribution of switching points in the three treatment conditions. We find that while individuals switch on average later on sheets A and B in both treatments, most of the effect comes from late switchers delaying their switch as the median remains similar. The last column reports p-values from t-tests comparing the mean switching point on the first (second) choice list to the average switching point under risk (loss) neutrality for each treatment. Switching points on list A (B) are significantly different from risk (loss) neutrality according to these tests.

Table C.2: Distribution of Switching Points by Treatment

	mean	median	5th	25th	75th	95th	p-value
CON	5.359	6.0	1.0	3.0	7.5	11.0	0.000
RAN	6.037	6.5	1.0	3.0	9.0	11.0	0.000
FRI	5.888	6.0	1.0	4.0	7.0	11.5	0.000
switch B							
CON	3.859	4.0	1.0	3.0	4.0	7.0	0.000
RAN	4.463	4.0	2.0	3.0	5.0	8.0	0.000
FRI	3.950	4.0	2.0	3.0	5.0	8.0	0.000

Note: Full sample included; p-values from t-tests of H_0: switch A = 3.5 or switch B = 2 respectively

Using prospect theory, we estimate preference parameters from the choices in the lotteries with positive and mixed payoffs. The estimates represent estimates of loss and risk-aversion parameters for teenagers in the control group as these decide independently of their peers. As we show in this paper, the assumption of independent choices is violated in the social interaction treatments. Therefore, our estimates of σ and λ do not represent preference parameters but rather *as if* estimates under the independence assumption. Furthermore, these parameters are identified from the two decision sheets as combinations (σ, λ), hence they are identified conditional on each other. For this reason, we present our results on the nature of choices in terms of switching points as well as changes in the estimated σ and λ parameters. Our results are robust to both outcome dimensions.

C.3 Differences in Parameters between Matched Participants

Table C.3 displays the absolute parameter differences between matched participants, i.e. in the *RANDOM* and *FRIENDS* treatments absolute parameter differences between discussion partners. In the *CONTROL* treatment, we report two alternative possibilities for ‘matched’ partners: in the ‘vs. RoC’ column we ‘match’ participants with the rest of their class, i.e. with everyone else but themselves and in the 'vs. Fri' column we match participants with the person with whom they exchanged stickers. For the comparison of these parameter differences between *FRIENDS* and *CONTROL*, we use the 'vs. Fri' difference, as pairs of non-interacting friends are the suitable comparison group to *FRIENDS*. For the comparison between *RANDOM* and *CONTROL*, we use the 'vs ROC' difference, as pairs of non-interacting random classmates are the suitable comparison group to *RANDOM*.

For both of these comparisons we find that interacting pairs make significantly more similar choices both in the pure as well as in the mixed lotteries, while when we compare *RANDOM* and *FRIENDS* (i.e. pairs of interacting friends and interacting random pairs), we find no significant differences in similarity in either dimension.

Table C.3: Differences in Parameters between matched Participants per Treatment

	CONTROL				p-values		
	vs. ROC	vs. Fri	Mean RAN	Mean FRI	RAN vs. CON	FRI vs. CON	RAN vs. FRI
$\Delta_{ij}(\sigma)$	0.274	0.382	0.165	0.146	0.198	0.000	0.518
$\Delta_{ij}(\lambda)$	1.760	2.180	1.008	0.281	0.428	0.000	0.854

Note: Data collapsed on the dyad level; p-values from two-sided Mann-Whitney U tests; CONTROL vs. ROC differences as comparison for RAN vs. CON tests, for other tests differences to matched participant; CON stands for *CONTROL*, RAN for *RANDOM* and FRI for *FRIENDS*

In Table C.4 we report robustness checks to the dyadic regressions based on Fafchamps and Gubert (2007) displayed in Table 3.4. Here we include the observations from our *FRIENDS* treatment in columns (2) and (4). Naturally, in interacting pairs of friendsthe preference parameters are endogenous to the interaction therefore we cannot include them as explanatory variables. The results indicate that the correlation between age, gender and the family characteristics and the probability of being friends is robust to including the observations from the *FRIENDS* treatment.

In Table C.5 we report robustness checks to the dyadic regressions displayed in Table 3.5. Here we again include the observations from our *FRIENDS* treatment in columns (2) and (4). Naturally, in interacting pairs of friends the preference parameters are endogenous to the treatment, as some classmates in *FRIENDS* were interacting with each other, therefore we cannot include them as explanatory variables. The results indicate that the correlation between age and the probability of being classmates is robust to including the observations from the *FRIENDS* treatment. However, the correlation between the gender and being classmates disappears.

Table C.4: Logit Regressions of Being Friends on Dyad Level Explanatory Variables within *CONTROL* and *FRIENDS*

	(1) $P(friends = 1)$	(2) $P(friends = 1)$	(3) $P(friends = 1)$	(4) $P(friends = 1)$
DIFFERENCES				
σ	0.288		0.416	
	(0.550)		(0.890)	
λ	-0.0542		-0.0642	
	(0.183)		(0.161)	
rel. math grade	0.130	-0.0400	0.157	0.0169
	(0.291)	(0.148)	(0.261)	(0.147)
age	-0.613**	-0.449**	-0.761**	-0.445**
	(0.263)	(0.205)	(0.304)	(0.223)
male	-2.517***	-2.181***	-2.526**	-2.197***
	(0.972)	(0.479)	(1.110)	(0.525)
household size			-0.158	-0.126*
			(0.175)	(0.0755)
single parent			-1.751***	-0.960**
			(0.601)	(0.387)
migrant			-0.0728	-0.385
			(0.286)	(0.276)
low SES			-0.357***	-0.340*
			(0.0887)	(0.199)
SUMS				
σ	0.151		0.212	
	(0.300)		(0.426)	
λ	0.0748		0.0982	
	(0.129)		(0.123)	
rel. math grade	0.0629	0.0269	-0.00710	0.0252
	(0.163)	(0.0547)	(0.221)	(0.0599)
age	0.0353	-0.0568	0.0804	-0.0775**
	(0.0658)	(0.0347)	(0.133)	(0.0352)
male	-0.334*	-0.352***	-0.348	-0.321***
	(0.203)	(0.0832)	(0.285)	(0.0871)
household size			0.0889*	0.0564
			(0.0463)	(0.0466)
single parent			0.460*	0.507***
			(0.251)	(0.177)
migrant			-0.0466	-0.126
			(0.360)	(0.0846)
low SES			-0.216	0.0165
			(0.328)	(0.0747)
Constant	-2.956	0.262	-4.128	0.965
	(2.963)	(0.870)	(5.332)	(0.968)
Observations	335	705	335	705

Note: Dependent variable is exchanged which is a dummy variable taking the value 1 for pairs of participants who are classmates; columns 3 and 5 contain observations from FRIENDS treatment; Robust standard errors clustered at the class level in parentheses; relmath refers to the math grade of each individual subtracted by the class average. *** p<0.01, ** p<0.05, * p<0.1

Table C.5: Logit Regressions of Being Classmates on Dyad Level Explanatory Variables within *CONTROL* and *FRIENDS*

	(1) $P(classmates = 1)$	(2) $P(classmates = 1)$	(3) $P(classmates = 1)$	(4) $P(classmates = 1)$
DIFFERENCES				
σ	-0.699		-0.688	
	(0.583)		(0.498)	
λ	0.00426		0.0146	
	(0.0715)		(0.0429)	
rel. math grade	-0.108	-0.118	-0.107	-0.111
	(0.140)	(0.0748)	(0.111)	(0.0808)
age	-0.612***	-0.382***	-0.615***	-0.385***
	(0.156)	(0.143)	(0.166)	(0.128)
male	0.163***	0.0117	0.163***	0.0133
	(0.0557)	(0.0900)	(0.0560)	(0.0864)
household size			-0.0117	-0.0461
			(0.0420)	(0.0345)
single parent			0.0859	-0.155
			(0.0725)	(0.267)
migrant			0.0385	-0.313
			(0.0835)	(0.324)
low SES			-0.0466	-0.0226
			(0.130)	(0.0740)
SUMS				
σ	0.00422		-0.00239	
	(0.495)		(0.453)	
λ	-0.0121		-0.0214	
	(0.0444)		(0.0700)	
rel. math grade	0.0223	0.00450	-0.00352	-0.00639
	(0.0793)	(0.0255)	(0.143)	(0.0416)
age	0.189	0.0617	0.207	0.0714
	(0.335)	(0.206)	(0.384)	(0.224)
male	-0.101	0.0121	-0.110	0.00191
	(0.120)	(0.119)	(0.151)	(0.102)
household size			0.0371	0.0280
			(0.112)	(0.0546)
single parent			-0.0760	0.0389
			(0.216)	(0.143)
migrant			-0.00896	-0.0506
			(0.250)	(0.152)
low SES			-0.0797	-0.0613
			(0.263)	(0.213)
Constant	-5.273	-2.601	-5.888	-2.689
	(7.967)	(5.813)	(9.997)	(6.470)
Observations	1,378	3,269	1,378	3,269

Note: Dependent variable is exchanged which is a dummy variable taking the value 1 for pairs of participants who are classmates; columns 3 and 5 contain observations from FRIENDS treatment; Robust standard errors clustered at the class level in parentheses; relmath refers to the math grade of each individual subtracted by the class average. *** p<0.01, ** p<0.05, * p<0.1

C.4 Assigning Parameter Values

Table C.6 and C.7 display the lotteries on the two , including their payoffs and the differences in expected payoffs. Additionally, Table C.6 demonstrates how σ is assigned according to the row of the switch to the right option. Table C.7 demonstrates with an example how λ is assigned based on a σ of 0.65 (switch on sheet A to the right option in the 6th row).

Table C.6: Assigning σ Parameters

	Option L		Option R		$\Delta_{LR}(EV)$	σ range		Assigned σ
Switch	Low	High	Low	High		Min	Max	
1	1.00	2.00	0.20	2.41	0.20	1.50	n/a	1.63
2	1.00	2.00	0.20	2.56	0.12	1.25	1.50	1.38
3	1.00	2.00	0.20	2.80	0.00	1.00	1.25	1.13
4	1.00	2.00	0.20	3.09	-0.14	0.80	1.00	0.90
5	1.00	2.00	0.20	3.29	-0.24	0.70	0.80	0.75
6	1.00	2.00	0.20	3.54	-0.37	0.60	0.70	0.65
7	1.00	2.00	0.20	3.87	-0.53	0.50	0.60	0.55
8	1.00	2.00	0.20	4.31	-0.76	0.40	0.50	0.45
9	1.00	2.00	0.20	4.93	-1.07	0.30	0.40	0.35
10	1.00	2.00	0.20	5.85	-1.53	0.20	0.30	0.25
11	1.00	2.00	0.20	7.33	-2.27	0.10	0.20	0.15
Never						n/a	0.1	0.05

Note: Switch in first rows corresponds to choosign option R in all rows; σ for switch in row 1 and for never assigend by extrapolating distance from neighboring category's midpoint to respective category

Table C.7: Assigning λ Parameters ($\sigma = 0.65$)

	Option L		Option R		$\Delta_{LR}(EV)$	λ range		Assigned λ
Switch	Low	High	Low	High		Min	Max	
1	-0.40	2.50	-2.10	3.00	0.60	0.21	n/a	0.06
2	-0.40	0.40	-2.10	3.00	-0.45	1.40	0.21	0.80
3	-0.40	0.10	-2.10	3.00	-0.60	1.70	1.40	1.55
4	-0.40	0.10	-1.60	3.00	-0.85	2.26	1.70	1.98
5	-0.80	0.10	-1.60	3.00	-1.05	3.69	2.26	2.97
6	-0.80	0.10	-1.40	3.00	-1.15	4.79	3.69	4.24
7	-0.80	0.10	-1.10	3.00	-1.30	9.14	4.79	6.97
Never						n/a	9.14	9.69

Note: Switch in first rows corresponds to choosign option R in all rows; λ for switch in row 1 and for never assigend by extrapolating distance from the two neighboring category's midpoints to respective category

Table C.8: Determinants of Switching Points

	$Switch_A$			$Switch_B$		
	RAN & FRI	RAN	FRI	RAN & FRI	RAN	FRI
fin. lit.	-0.025	0.038	-0.046	-0.057	-0.120	-0.031
	(1.21)	(1.00)	(1.85)*	(2.33)**	(2.49)**	(1.02)
$\Delta(fin.lit.) < 0$	-0.149	-0.164	-0.145	-0.193	-0.246	-0.179
	(1.59)	(1.06)	(1.21)	(1.72)*	(1.33)	(1.23)
math	0.156	0.218	0.148	0.046	0.061	0.009
	(3.37)***	(2.14)**	(2.68)***	(0.82)	(0.52)	(0.13)
$\Delta(math) < 0$	0.174	0.279	0.130	0.060	-0.113	0.122
	(1.84)*	(1.49)	(1.11)	(0.53)	(0.51)	(0.86)
male	0.058	0.274	0.007	-0.107	-0.270	-0.001
	(0.61)	(1.13)	(0.07)	(0.95)	(0.98)	(0.01)
$\Delta(male)$	0.023	-0.145	0.144	0.100	0.122	0.028
	(0.26)	(0.94)	(0.94)	(0.94)	(0.69)	(0.15)
single parent	-0.383	-0.494	-0.470	-0.319	-0.444	-0.250
	(3.08)***	(2.09)**	(2.78)***	(2.13)**	(1.59)	(1.27)
$\Delta(singlep.)$	0.158	0.211	0.213	0.164	0.189	0.175
	(1.69)*	(1.41)	(1.58)	(1.46)	(1.07)	(1.09)
cons	2.057	1.538	2.234	2.068	2.834	1.709
	(9.98)***	(4.10)***	(8.74)***	(8.54)***	(6.22)***	(5.53)***
N	114	40	74	114	40	74

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table C.8 reports the results from OLS regressions of the switching point on sheet A and sheet B on several explanatory variables for *RANDOM* and *FRIENDS* pooled and individually.

C.5 Instructions (translated from German)

Part 1 – Risk Game

Welcome to today's survey. Our **survey** consists of **two parts**. Please raise your hand if you did not hand in a parents' consent!

[***CONTROL***

In part 1 it is very important that you make your decisions on your own. There are no right or wrong decisions. We would only like to know what you prefer. As soon as we have finished the explanation, we ask you to be **absolutely quiet for 5 minutes**.]

[***RANDOM***

In part 1 you can discuss your decisions with another person in the classroom. We prepared a bag with numbers. Each number exists twice. Please draw a number and, as soon as everyone has drawn, look for the person that has drawn the same number and sit together in pairs. Please take a pen with you! You are allowed to discuss your decisions with your partner quietly, but you decide only for yourself! If no one else has drawn your number, you can fill in the first part alone! In the lower part of your questionnaires you find red stickers with your questionnaire number. Please remove the sticker from your own questionnaire and put it on your partner's questionnaire, so we know who you worked with!]

[***FRIENDS***

In part 1 you can discuss your decisions with another person in the classroom. Please stand up and take a pen with you! Choose a friend with whom you would like to discuss your choices and sit together in pairs. You are allowed to discuss your decisions with your partner quietly, but you decide only for yourself! [with odd number of participants: the person that does not find a partner can fill in the first part alone!] In the lower part of your questionnaires you find red stickers with your questionnaire number. Please remove the sticker from your own questionnaire and put it on your partner's questionnaire, so we know who you worked with!]

We will now go through the **first part of the survey**. We will have breaks in between, so you can ask questions. Please raise your hand in case you have a question.

Part 1

In the first part you can earn money. The money will be paid out to you at the end of this lesson. For your participation you receive 3.10 Euros right now.

The amount of money you will have in the end depends on your decisions and on chance. **There are no right or wrong answers!** It is important that you fully understand the rules. In the first part you have to make 18 decisions. We will randomly select one of the decisions and actually pay it to you at the end of the lesson. You will not know which decision this will be before you have finished the part 1! Therefore, you have to think carefully about all your decisions, because every decision might be drawn.

One of you will draw a chip from this bag later on. In this bag there are 5 blue and 5 red chips. You have to decide how much money you receive if a blue chip or a red chip is drawn. Part 1 consists of two sheets with several decisions. You always have to decide between two options – option L (for left) and option R (for right).

Let's start with the first sheet! Here is an example. In the upper part of the sheet you can see how many chips of each color are in the bag. We take a look at the first decision you have to make.

	Option L				**Option R**
A1	**€ 2.00** for 🔵	☐	or	☐	**€ 2.41** for 🔵
	€ 1.00 for 🔴				**€ 0.20** for 🔴

If you choose option L, you receive 2.00 Euros if a blue chip is drawn and 1.00 Euro if a red chip is drawn. If you choose option R, you receive 2.41 Euros for a blue chip, but only 0.20 Euros for a red chip. If you prefer option L in A1, tick the left box. If you prefer option R, tick the right box. **Please do only tick one box per line!**

The other decision on the first sheet are similar. You have to make a decision for each line.

In the second line you again decide between L and R. In this line, however, you can win either 2.56 Euros or 0.20 Euros in option R. As you can see here, the amount of money you can win if a blue chip is drawn increases for option R. Option L always stays the same in this sheet.

After you have made all your decisions in a sheet, you have to summarize your decisions in the bottom part of the sheet.

Assuming that you want to coose option L for all decisions, you should tick the left box in each line. Then, you state that you choose option L for lines A1 to A11 in the bottom part of the sheet and cross out the line for option R.

It works the same way if you only want choose option R. You cross out the line for option L to indicate that you never want to choose L.

Assume that you choose option L for lines A1 to A4 and then option R from line A5 on. Then you write A4 into the empty box in the upper line and A5 in the lower line.

Please pay close attention now!

Most people only switch from option L to option R **once** at some point between the first line and the last line. As soon as you have ticked a box on the right side, you should stay with option R for all following lines.

Why is this the case?

Let's look at the second and third line. If you have chosen right in a previous line, it does not make sense to switch back to the left side in the following lines. Option L is equal in both lines. In option R the amount for a red chip is the same in both lines, but in the third line you receive 2.80 Euros for a blue chip. This is more than in the second line, where you only receive 2.56 Euros for a blue chip. Therefore, option R is better in the third line than in the seond line. Thus, if you alreday choose right in the second line, you should all the more choose option R is the third line! It is always the same if you compare two consecutive lines – the further you go down, the better option R gets. Therefore, you should tick option R if you have already chosen option R earlier!

Is this clear to everyone?

Sheet 2 is similar to sheet 1, only the numbers are different. If a red chip is drawn, you loose money in both options. In case this happens, you have to return money to us. Additonally, sometimes also the left option changes from line to line.

Let's look at the second sheet!

	Option L				Option R
B1	**€ 2.50** for 🔵 **€ -0.40** for 🔴	☐	or	☐	**€ 3.00** for 🔵 **€ -2.10** for 🔴

The decisions work the same way as in the first sheet. You decide how you want to receive or pay money for the chips. If you choose option L, you receive 2.50 Euros if a blue chip is drawn, but you have to return 0.40 Euros to us if a red chip is drawn. It works the same way with option R: for a blue chip you receive 3.00 Euros. If a red chip is drawn, you have to return 2.10 Euros to us.

As in sheet 1, we ask you to make a decision for each line. As before, you have to transfer your decisions into the summary in the end. As in sheet 1, cross out the lower line if you only want to shoose option L. Cross out the upper line if you only want to choose R.

If you choose option L to line B3, for example, and option R from line B4 onwards, then please indicate it in the bottom part of the sheet.

It works the same as in the first sheet!

Most people switch from left to right only once. As soon as you have ticked a box on the right side, you should stay with option R in all following lines.

Why is it like that?

Let's look at the third and the fourth line. It does not make sense to switch back to the left side in the lower lines if you have already chosen right before. Option L is equal in both lines. In option R the payment for a blue chip is equal in both lines. In line 3, however, you loose 2.10 euros for a red chip, which is more than in the fourth line, where you only loose 1.60 Euros. Thus, option R is definitely better in the fourth line than in the third line. If you have already chosen option R over option L in the third line, then you should all the more tick option R in the fourth line. It is always like that if you compare two consecutive lines. Thus, if you have chosen option R once, you should also choose option R in all following lines.

Is this clear to everyone?

Now we have to explain to you how you get your money. In the end of part 1 you have made 18 decisions, 11 in the first sheet and 7 in the second sheet.

When all of you have finished part 1, one of you can draw a card from these 18 cards. There is one card for each of your decisions – A1 to A11 and B1 to B7. The card that will be drawn counts for all of you. If you draw A1, the decision A1 is actually paid out **to all of you**.

Then one of you draws a chip from the bag. This chip determines the payment for all of you. If you draw A1 and blue, the ones who have chosen option L in A1 get 2.00 Euros and the ones who have chosen option R get 2.41 Euros. If you draw red, some get 1.00 Euro and others get 0.20 Euros.

As each of your 18 decisions can be drawn, you should think carefully for each line about whether you want to choose option L or option R!

Before you are allowed to start with part 1, you have to fill in the comprehension check on the first page. Please work on your own! We will go through the rows and check whether you have filled in the correct answers. Please do not start before we have given you [*DISCUSSION*: and your partner] the start signal. Remember that we have already given you 3.10 Euros!

[*TREATMENT*: You are allowed to discuss your decisions with your partner, but you choose for yourself and the money is only for you. Therefore, you do not have to make the same decision.]

[*CONTROL*: It is very important that you make the decisions on your own. This takes only a few minutes in part 1. Please be absolutely quiet now.]

Please raise your hand as soon as you have finished part 1, so we can collect it.

Part 2

Those of you who are done can start with part 2. Part 2 is a **questionnaire about yourself**. We are very interested in your own opinion, so please answer all questions on your own – without talking to your neighbours. Please try to answer every question as good as you can.

You have time for this until [5 minutes to the end of the lesson]. Do not take too long for a single question – at most one minute.

[Towards the end of part 2 in *CONTROL*: In the bottom of your sheets you find red stickers with your questionnaire number. We would like to look at your decsions and the decisions of one of your friends later. Please choose a friend and exchange your stickers!]

[5 minutes to end] You have three minutes left to answer the remaining questions. Please answer the questions on the sticker in any case, as we are specifically interested in these questions!

Thank you for your participation!

C.6 Decision Sheets

Part I
Your decisions around money

Prof. Dr. Joachim Winter

Ludwig-Maximilians-University, Munich

Comprehension Check

Here we ask you to answer four questions to check whether you have understood the rules. Imagine that the following decision has benn drawn and that you have decided as ticked here:

Z6	**€ 1.50** for 🔵 **€ 0.50** for 🔴	☐	or	☒	**€ 2.50** for 🔵 **€ 0.10** for 🔴

1. How much money do you **receive additionally** if 🔵 is drawn? . . . Euros
2. How much money do you **receive additionally** if 🔴 is drawn? . . . Euros
3. Inculding our thank-you, how much money do you have **in total in the end** if 🔵 is drawn? . . . Euros
4. Inculding our thank-you, how much money do you have **in total in the end** if 🔴 is drawn? . . . Euros

Which option do you choose?

Your payment for both options is determined by the draw of the chip from the bag, which contains 5 red and 5 blue chips:

🔴🔴🔴🔴🔴 🔵🔵🔵🔵🔵

	Option L				Option R
A1	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**2.41 Euros** for 🔵 **0.20 Euros** for 🔴
A2	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**2.56 Euros** for 🔵 **0.20 Euros** for 🔴
A3	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**2.80 Euros** for 🔵 **0.20 Euros** for 🔴
A4	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**3.09 Euros** for 🔵 **0.20 Euros** for 🔴
A5	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**3.29 Euros** for 🔵 **0.20 Euros** for 🔴
A6	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**3.54 Euros** for 🔵 **0.20 Euros** for 🔴
A7	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**3.87 Euros** for 🔵 **0.20 Euros** for 🔴
A8	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**4.31 Euros** for 🔵 **0.20 Euros** for 🔴
A9	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**4.93 Euros** for 🔵 **0.20 Euros** for 🔴
A10	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**5.85 Euros** for 🔵 **0.20 Euros** for 🔴
A11	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**7.33 Euros** for 🔵 **0.20 Euros** for 🔴

Your choices

I choose option L for questions	A1	to	
I choose option R for questions		to	A11

Which option do you choose?

Your payment for both options is determined by the draw of the chip from the bag, which contains 5 red and 5 blue chips:

🔴🔴🔴🔴🔴 🔵🔵🔵🔵🔵

	Option L				Option R
B1	**2.50 Euros** for 🔵 **-0.40 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-2.10 Euros** for 🔴
B2	**0.40 Euros** for 🔵 **-0.40 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-2.10 Euros** for 🔴
B3	**0.10 Euros** for 🔵 **-0.40 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-2.10 Euros** for 🔴
B4	**0.10 Euros** for 🔵 **-0.40 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-1.60 Euros** for 🔴
B5	**0.10 Euros** for 🔵 **-0.80 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-1.60 Euros** for 🔴
B6	**0.10 Euros** for 🔵 **-0.80 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-1.40 Euros** for 🔴
B7	**0.10 Euros** for 🔵 **-0.80 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-1.10 Euros** for 🔴

Your choices

I choose option L for questions	B1	to	
I choose option R for questions		to	B7

C.7 Additional Measurements

The following is a translation of the financial literacy questions that were asked in part 2 of the study. The financial literacy score is constructed by adding up the number of correct responses. A maximum of 13 points can be reached. For the verbal responses in situation 1, we had two research assistants independently judge the correctness of the response. Contradictory judgements were then settled by one of the authors.

Situation 1

Martin can buy tomatoes individually or in crates. 1 kilo of tomatoes individually cost 2.75 Euros. One crate with 10 kilos costs 22 Euros. Martin thinks: “The crate with tomatoes offers a better cost-benefit-ratio than the individual tomatoes.”

Give one reason for this:

____

Buying one crate of tomatoes (10 kilos) could be a bad idea for some people. What could be the reason for this?

____

Situation 2

Imagine your grandmoter would like to save money. Potentially she needs to use the money on short notice, if her car breaks down and she needs to pay the repair. Which of the following investments would you recommend to her? (Please only mark one answer)

☐ Real estate (e.g. a house or apartment)

☐ Stocks

☐ Savings book

Situation 3

Chrstina wants to invest her savings and to take as little risk as possible. Which of the following investments would you recommend to her? (Please only mark one answer)

- ☐ Real estate (e.g. a house or apartment)
- ☐ Stocks
- ☐ Savings book

Situation 4

Anna wants to take up a new mobile plan. She can choose from two plans. Each month, she never sends more than 100 text messages, talks for a maximum of 100 minutes on the phone and surfs at most 100 minutes online. She has to pay the phone costs from her own money. Which alternative would you recommend to her?

- ☐ **Alternative A** (flatrate contract): For 40 Euros a month Anna can phone, text and surf online without limit.
- ☐ **Alternative B** (prepaid card to top-up): Anna gets a SIM card for free. With this plan, she can talk for 10 cents a minute, surf for 10 cents a minute and send texts for 10 cents per text.

Situation 5

Which of the following statements is correct? If you buy a stock of company B...

- ☐ ... you own a par of company B.
- ☐ ... you have lent money to company B.
- ☐ ... you are liable for the debt of company B.
- ☐ I don't know.

Situation 6

Marie has 100 Euros in her savings account. She gets 2% interest a year. Marie asks you, how much money she will have in her account after five years, if she leaves the full amount in the account for the whole time. What do you think?

- ☐ More than 102 Euros
- ☐ Exactly 102 Euros

- ☐ Less than 102 Euros
- ☐ I don't know.

Situation 7

If you buy 5 stocks from a single company, you get a safer return (profit) than if you invest the same amount of money into 5 different stocks (e.g. into an investment fund). Right or wrong?

- ☐ Right
- ☐ Wrong
- ☐ I don't know.

Situation 8

(Please mark the possibility that seems the most important to you) Buying something on credit...

- ☐ ... allows me to afford everything today that I want.
- ☐ ... gives me the freedom to decide myself what I can afford when.
- ☐ ... means getting into debt now in order to buy something now that I cannot afford right now.

Situation 9

Which ones are one-off costs and which are recurring costs in sports?

Tennis racket	☐one-off	☐recurring	costs
Monthly fee at the sports club	☐one-off	☐recurring	costs
10er ticketbook at the swimming pool	☐one-off	☐recurring	costs

Situation 10

Why do companies advertise?

- ☐ To sell their products better.
- ☐ Because the employees like to see their company on TV, in magazines etc..

- ☐ They produce funny advertisements in order to thank their customers.

- ☐ I don’t know.

Furthremore, participants were asked about the degree of contact they have with the person they exchanged stickers with using the following question:

Degree of Contact

You and your best friend in your class (your current discussion partner), how often do you see each other outside of school?

- ☐ Daily

- ☐ Several times a week

- ☐ Once a week

- ☐ Less than once a week

- ☐ Never

Bibliography

Abdellaoui, M., Bleichrodt, H., and L'Haridon, O. (2008). A tractable method to measure utility and loss aversion under prospect theory. *Journal of Risk and Uncertainty*, 36(3):245–266.

Achtziger, A., Alós-Ferrer, C., and Wagner, A. K. (2011). Social preferences and self-control. Working Paper.

Achtziger, A., Alós-Ferrer, C., and Wagner, A. K. (2015). Money, depletion, and prosociality in the dictator game. *Journal of Neuroscience, Psychology, and Economics*, 8(1):1–14.

Achtziger, A., Alós-Ferrer, C., and Wagner, A. K. (2016). The impact of self-control depletion on social preferences in the ultimatum game. *Journal of Economic Psychology*, 53:1–16.

Ahern, K. R., Duchin, R., and Shumway, T. (2014). Peer effects in risk aversion and trust. *Review of Financial Studies*, 27(11):3213–3240.

Alan, S., Boneva, T., Baydar, N., Crossley, T., and Ertac, S. (2012). Parental socialization effort and the intergenerational transmission of risk preference. Working Paper.

Ali, S. N. (2011). Learning self-control. *Quarterly Journal of Economics*, 126(2):857–893.

Ameriks, J., Caplin, A., and Leahy, J. (2003). Wealth accumulation and the propensity to plan. *Quarterly Journal of Economics*, 118(3):1007–1047.

Ameriks, J., Caplin, A., Leahy, J., and Tyler, T. (2007). Measuring self-control problems. *American Economic Review*, 97(3):966–972.

Andrade, E. B., Odean, T., and Lin, S. (2015). Bubbling with excitement: an experiment. *Review of Finance*, 19(6):1–20.

Arcand, J.-L. and Fafchamps, M. (2011). Matching in community-based organizations. *Journal of Development Economics*, 98(2):203–219.

Arnett, J. (1992). Reckless behavior in adolescence: A developmental perspective. *Developmental Review*, 12(4):339–373.

Attanasio, O., Barr, A., Cardenas, J., Genicot, G., and Meghir, C. (2012). Risk pooling, risk preferences and social networks. *American Economic Journal: Applied Economics*, 4(2):134–167.

Barberis, N., Huang, M., and Santos, T. (2001). Prospect theory and asset prices. *Quarterly Journal of Economics*, 116(1):1–53.

Barberis, N. and Thaler, R. (2003). A survey of behavioral finance. *Handbook of the Economics of Finance*, 1:1053–1128.

Barberis, N. and Xiong, W. (2012). Realization utility. *Journal of Financial Economics*, 104(2):251–271.

Baumeister, R. F., Bratslavsky, E., Muraven, M., and Tice, D. M. (1998). Ego depletion: Is the active self a limited resource? *Journal of Personality and Social Psychology*, 74(5):1252–1265.

Bellemare, C., Krause, M., Kröger, S., and Zhang, C. (2005). Myopic loss aversion: Information feedback vs. investment flexibility. *Economics Letters*, 87(3):319–324.

Benartzi, S. and Thaler, R. H. (1995). Myopic loss aversion and the equity premium puzzle. *Quarterly Journal of Economics*, 110(1):73–92.

Benhabib, J. and Bisin, A. (2005). Modeling internal commitment mechanisms and self-control: A neuroeconomics approach to consumption–saving decisions. *Games and Economic Behavior*, 52(2):460–492.

Benjamin, D. J., Brown, S. A., and Shapiro, J. M. (2013). Who is 'behavioral'? Cognitive ability and anomalous preferences. *Journal of the European Economic Association*, 11(6):1231–1255.

Bernheim, B. D. and Rangel, A. (2004). Addiction and cue-triggered decision processes. *American Economic Review*, 94(5):1558–1590.

Beshears, J., Choi, J. J., Laibson, D., Madrian, B. C., and Milkman, K. L. (2015). The effect of providing peer information on retirement savings decisions. *Journal of Finance*, 70(3):1161–1201.

Bolton, G. E. and Ockenfels, A. (2000). ERC: A theory of equity, reciprocity, and competition. *American Economic Review*, 90(1):166–193.

Bosch-Rosa, C., Meissner, T., and Bosch-Domènech, A. (2015). Cognitive bubbles. Working Paper.

Brañas-Garza, P., García-Muñoz, T., and González, R. H. (2012). Cognitive effort in the beauty contest game. *Journal of Economic Behavior & Organization*, 83(2):254–260.

Breaban, A. and Noussair, C. N. (2013). Emotional state and market behavior. Working Paper.

Brown, B. B., Clasen, D. R., and Eicher, S. A. (1986). Perceptions of peer pressure, peer conformity dispositions, and self-reported behavior among adolescents. *Developmental Psychology*, 22(4):521–530.

Brown, J. R., Ivković, Z., Smith, P. A., and Weisbenner, S. (2008). Neighbors matter: Causal community effects and stock market participation. *Journal of Finance*, 63(3):1509–1531.

Bruyneel, S., Dewitte, S., Vohs, K. D., and Warlop, L. (2006). Repeated choosing increases susceptibility to affective product features. *International Journal of Research in Marketing*, 23(2):215–225.

Bruyneel, S. D., Dewitte, S., Franses, P. H., and Dekimpe, M. G. (2009). I felt low and my purse feels light: Depleting mood regulation attempts affect risk decision making. *Journal of Behavioral Decision Making*, 22(2):153–170.

Bucciol, A., Houser, D., and Piovesan, M. (2010). Willpower in children and adults: a survey of results and economic implications. *International Review of Economics*, 57(3):259–267.

Bucciol, A., Houser, D., and Piovesan, M. (2011). Temptation and productivity: A field experiment with children. *Journal of Economic Behavior & Organization*, 78(1):126–136.

Bucciol, A., Houser, D., and Piovesan, M. (2013). Temptation at work. *PloS one*, 8(1):1–5.

Burger, N., Charness, G., and Lynham, J. (2011). Field and online experiments on self-control. *Journal of Economic Behavior & Organization*, 77(3):393–404.

Bursztyn, L., Ederer, F., Ferman, B., and Yuchtman, N. (2014). Understanding mechanisms underlying peer effects: Evidence from a field experiment on financial decisions. *Econometrica*, 82(4):1273–1301.

Cai, J., de Janvry, A., and Sadoulet, E. (2015). Social networks and the decision to insure. *American Economic Journal: Applied Economics*, 7(2):81–108.

Camerer, C. F. and Ho, T.-H. (1994). Violations of the betweenness axiom and nonlinearity in probability. *Journal of Risk and Uncertainty*, 8(2):167–196.

Card, D. and Giuliano, L. (2013). Peer effects and multiple equilibria in the risky behavior of friends. *Review of Economics and Statistics*, 95(4):1130–1149.

Carrell, S. E., Sacerdote, B. I., and West, J. E. (2013). From natural variation to optimal policy? The importance of endogenous peer group formation. *Econometrica*, 81(3):855–882.

Carter, E. C. and McCullough, M. E. (2013). Is ego depletion too incredible? Evidence for the overestimation of the depletion effect. *Behavioral and Brain Sciences*, 36(6):683–684.

Carter, E. C. and McCullough, M. E. (2014). Publication bias and the limited strength model of self-control: Has the evidence for ego depletion been overestimated? *Frontiers in Psychology*, 5:1–11.

Charness, G., Gneezy, U., and Kuhn, M. A. (2013). Experimental methods: Extra-laboratory experiments – extending the reach of experimental economics. *Journal of Economic Behavior & Organization*, 91:93–100.

Corgnet, B., Hernán-González, R., Kujal, P., and Porter, D. (2014). The effect of earned versus house money on price bubble formation in experimental asset markets. *Review of Finance*, 19(4):1–34.

Croson, R. and Gneezy, U. (2009). Gender differences in preferences. *Journal of Economic Literature*, 47(2):448–474.

Cueva, C., Iturbe-Ormaetxe, I., Mata-Pérez, E., Ponti, G., Sartarelli, M., Yu, H., and Zhukova, V. (2016). Cognitive (ir) reflection: New experimental evidence. *Journal of Behavioral and Experimental Economics*. Forthcoming.

Dahl, G. B., Løken, K. V., and Mogstad, M. (2014). Peer effects in program participation. *American Economic Review*, 104(7):2049–2074.

De Haan, T. and Van Veldhuizen, R. (2015). Willpower depletion and framing effects. *Journal of Economic Behavior & Organization*, 117:47–61.

De Langhe, B., Sweldens, S., Van Osselaer, S. M., and Tuk, M. A. (2008). The emotional information processing system is risk averse: Ego-depletion and investment behavior. Working Paper.

de Ridder, D. T., Lensvelt-Mulders, G., Finkenauer, C., Stok, F. M., and Baumeister, R. F. (2012). Taking stock of self-control – A meta-analysis of how trait self-control relates to a wide range of behaviors. *Personality and Social Psychology Review*, 16(1):76–99.

Dhar, R. and Zhu, N. (2006). Up close and personal: Investor sophistication and the disposition effect. *Management Science*, 52(5):726–740.

Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J., and Wagner, G. G. (2011). Individual risk attitudes: Measurement, determinants, and behavioral consequences. *Journal of the European Economic Association*, 9(3):522–550.

Duflo, E. and Saez, E. (2002). Participation and investment decisions in a retirement plan: The influence of colleagues' choices. *Journal of Public Economics*, 85(1):121–148.

Duflo, E. and Saez, E. (2003). The role of information and social interactions in retirement plan decisions: Evidence from a randomized experiment. *Quarterly Journal of Economics*, 118(3):815–842.

Dufwenberg, M., Lindqvist, T., and Moore, E. (2005). Bubbles and experience: An experiment. *American Economic Review*, 95(5):1731–1737.

Dustmann, C. (2004). Parental background, secondary school track choice, and wages. *Oxford Economic Papers*, 56(2):209–230.

Eckel, C. C. and Füllbrunn, S. C. (2015). Thar "SHE" blows? Gender, competition, and bubbles in experimental asset markets. *American Economic Review*, 105(2):906–920.

Eriksen, K. W. and Kvaløy, O. (2010). Do financial advisors exhibit myopic loss aversion? *Financial Markets and Portfolio Management*, 24(2):159–170.

Etchart-Vincent, N. and L'Haridon, O. (2011). Monetary incentives in the loss domain and behaviour toward risk: An experimental comparison of three reward schemes including real losses. *Journal of Risk and Uncertainty*, 42(1):61–83.

Fafchamps, M. and Gubert, F. (2007). The formation of risk sharing networks. *Journal of Development Economics*, 83(2):326–350.

Fafchamps, M., Kebede, B., and Zizzo, D. J. (2015). Keep up with the winners: Experimental evidence on risk taking, asset integration and peer effects. *European Economic Review*, 79:59–79.

Falk, A. and Ichino, A. (2006). Clean evidence on peer effects. *Journal of Labor Economics*, 24(1):39–57.

Fama, E. F. (1970). Efficient capital markets: A review of theory and empirical work. *Journal of Finance*, 25(2):383–417.

Fehr, E. and Goette, L. (2007). Do workers work more if wages are high? Evidence from a randomized field experiment. *American Economic Review*, 97(1):298–317.

Fehr, E. and Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. *Quarterly Journal of Economics*, 114(3):817–868.

Fellner, G. and Maciejovsky, B. (2007). Risk attitude and market behavior: Evidence from experimental asset markets. *Journal of Economic Psychology*, 28(3):338–350.

Fellner, G. and Sutter, M. (2009). Causes, consequences, and cures of myopic loss aversion – an experimental investigation. *Economic Journal*, 119(537):900–916.

Feng, L. and Seasholes, M. S. (2005). Do investor sophistication and trading experience eliminate behavioral biases in financial markets? *Review of Finance*, 9(3):305–351.

Fenton-O'Creevy, M., Soane, E., Nicholson, N., and Willman, P. (2011). Thinking, feeling and deciding: The influence of emotions on the decision making and performance of traders. *Journal of Organizational Behavior*, 32(8):1044–1061.

Festinger, L. (1954). A theory of social comparison processes. *Human Relations*, 7(2):117–140.

Fischbacher, U. (2007). z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, 10(2):171–178.

Fischbacher, U., Hoffmann, G., and Schudy, S. (2015). The causal effect of stop-loss and take-gain orders on the disposition effect. Working Paper.

Frederick, S. (2005). Cognitive reflection and decision making. *Journal of Economic Perspectives*, 19(4):25–42.

Freeman, N. and Muraven, M. (2010). Self-control depletion leads to increased risk taking. *Social Psychological and Personality Science*, 1(2):175–181.

Friehe, T. and Schildberg-Hörisch, H. (2014). Crime and self-control revisited: Disentangling the effect of self-control on risk and social preferences. Working Paper.

Frydman, C. and Rangel, A. (2014). Debiasing the disposition effect by reducing the saliency of information about a stock's purchase price. *Journal of Economic Behavior & Organization*, 107:541–552.

Fudenberg, D. and Levine, D. K. (2006). A dual-self model of impulse control. *American Economic Review*, 96(5):1449–1476.

Fudenberg, D. and Levine, D. K. (2012). Timing and self-control. *Econometrica*, 80(1):1–42.

Gailliot, M. T., Gitter, S. A., Baker, M. D., Baumeister, R. F., et al. (2012). Breaking the rules: Low trait or state self-control increases social norm violations. *Psychology*, 3(12):1074–1083.

Galvan, A., Hare, T., Voss, H., Glover, G., and Casey, B. (2007). Risk-taking and the adolescent brain: who is at risk? *Developmental Science*, 10(2):F8–F14.

Galvan, A., Hare, T. A., Parra, C. E., Penn, J., Voss, H., Glover, G., and Casey, B. (2006). Earlier development of the accumbens relative to orbitofrontal cortex might underlie risk-taking behavior in adolescents. *Journal of Neuroscience*, 26(25):6885–6892.

Gardner, M. and Steinberg, L. (2005). Peer influence on risk taking, risk preference, and risky decision making in adolescence and adulthood: an experimental study. *Developmental Psychology*, 41(4):625.

Gathergood, J. (2012). Self-control, financial literacy and consumer over-indebtedness. *Journal of Economic Psychology*, 33(3):590–602.

Gerhardt, H., Schildberg-Hörisch, H., and Willrodt, J. (2015). Does self-control depletion affect risk attitudes? Working Paper.

Glätzle-Rützler, D., Sutter, M., and Zeileis, A. (2015). No myopic loss aversion in adolescents? – An experimental note. *Journal of Economic Behavior & Organization*.

Gneezy, U. and Potters, J. (1997). An experiment on risk taking and evaluation periods. *Quarterly Journal of Economics*, 112(2):631–645.

Greiner, B. (2015). Subject pool recruitment procedures: Organizing experiments with ORSEE. *Journal of the Economic Science Association*, 1(1):114–125.

Grinblatt, M. and Keloharju, M. (2001). What makes investors trade? *Journal of Finance*, 56(2):589–616.

Gul, F. and Pesendorfer, W. (2001). Temptation and self-control. *Econometrica*, 69(6):1403–1435.

Hagger, M. S., Wood, C., Stiff, C., and Chatzisarantis, N. L. (2010). Ego depletion and the strength model of self-control: a meta-analysis. *Psychological Bulletin*, 136(4):495–525.

Haigh, M. S. and List, J. A. (2005). Do professional traders exhibit myopic loss aversion? An experimental analysis. *Journal of Finance*, 60(1):523–534.

Hanushek, E. A. and Woessmann, L. (2011). *Handbook of the Economics of Education*, volume 3, chapter "The economics of international differences in educational achievement.", pages 89–200. North-Holland, Amsterdam.

Harbaugh, W. T., Krause, K., and Vesterlund, L. (2002). Risk attitudes of children and adults: Choices over small and large probability gains and losses. *Experimental Economics*, 5(1):53–84.

Hargreaves Heap, S. and Zizzo, D. (2011). Emotions and chat in a financial markets experiment. Working Paper.

Heidhues, P. and Koszegi, B. (2010). Exploiting naïvete about self-control in the credit market. *American Economic Review*, 100(5):2279–2303.

Heller, K. A., Kratzmeier, H., and Lengfelder, A. (1998). *Ein Handbuch zu den Standard Progressive Matrices von J. C. Raven.* Beltz-Testgesellschaft, Weinheim.

Hilgers, D. and Wibral, M. (2014). How malleable are choice brackets? The case of myopic loss aversion. Working Paper.

Hirshleifer, D. and Shumway, T. (2003). Good day sunshine: Stock returns and the weather. *Journal of Finance*, 58(3):1009–1032.

Hofmann, W., Friese, M., and Strack, F. (2009). Impulse and self-control from a dual-systems perspective. *Perspectives on Psychological Science*, 4(2):162–176.

Holt, C. A. and Laury, S. K. (2002). Risk aversion and incentive effects. *American Economic Review*, 92(5):1644–1655.

Huber, J. and Kirchler, M. (2012). The impact of instructions and procedure on reducing confusion and bubbles in experimental asset markets. *Experimental Economics*, 15(1):89–105.

Ingersoll, J. E. and Jin, L. J. (2013). Realization utility with reference-dependent preferences. *Review of Financial Studies*, 26(3):723–767.

Inzlicht, M. and Schmeichel, B. J. (2012). What is ego depletion? Toward a mechanistic revision of the resource model of self-control. *Perspectives on Psychological Science*, 7(5):450–463.

Ivković, Z. and Weisbenner, S. (2007). Information diffusion effects in individual investors' common stock purchases: Covet thy neighbors' investment choices. *Review of Financial Studies*, 20(4):1327–1357.

Jackson, M. O. and Rogers, B. W. (2007). Meeting strangers and friends of friends: How random are social networks? *American Economic Review*, 97(3):890–915.

Job, V., Dweck, C. S., and Walton, G. M. (2010). Ego depletion – is it all in your head? Implicit theories about willpower affect self-regulation. *Psychological Science*, 21(11):1686–1693.

Kahneman, D. (2011). *Thinking, fast and slow.* Penguin Books, London.

Kahneman, D. and Tversky, A. (1979). Prospect theory: An analysis of decision under risk. *Econometrica*, 47(2):263–291.

Keynes, J. M. (1936). *The General Theory of Interest, Employment and Money.* Macmillan, London.

Kim, J.-Y. (2006). Hyperbolic discounting and the repeated self-control problem. *Journal of Economic Psychology*, 27(3):344–359.

Kirchler, M., Huber, J., and Stöckl, T. (2012). Thar she bursts: Reducing confusion reduces bubbles. *American Economic Review*, 102(2):865–883.

Kocher, M. G., Lucks, K. E., and Schindler, D. (2016a). Unleashing animal spirits – self-control and overpricing in experimental asset markets. Working Paper.

Kocher, M. G., Myrseth, K. O. R., Martinsson, P., and Wollbrant, C. E. (2016b). Strong, bold, and kind: Self-control and cooperation in social dilemmas. *Experimental Economics*, Forthcoming.

Kostek, J. and Ashrafioun, L. (2014). Tired winners: The effects of cognitive resources and prior winning on risky decision making. *Journal of Gambling Studies*, 30(2):423–434.

Kotabe, H. P. and Hofmann, W. (2015). On integrating the components of self-control. *Perspectives on Psychological Science*, 10(5):618–638.

Kuhn, M., Kuhn, P., and Villeval, M. C. (2014). Self control and intertemporal choice: Evidence from glucose and depletion interventions. Working Paper.

Kumar, A. and Lim, S. S. (2008). How do decision frames influence the stock investment choices of individual investors? *Management Science*, 54(6):1052–1064.

Lahav, Y. and Meer, S. (2012). The effect of induced mood on prices in asset markets – experimental evidence. Working Paper.

Lahno, A. M. (2014). *Experiments on choice under uncertainty.* PhD thesis, Ludwig-Maximilians-Universität München.

Lahno, A. M. and Serra-Garcia, M. (2015). Peer effects in risk taking: Envy or conformity? *Journal of Risk and Uncertainty*, 50(1):73–95.

Lahno, A. M., Serra-Garcia, M., D'Exelle, B., and Verschoor, A. (2015). Conflicting risk attitudes. *Journal of Economic Behavior & Organization*, 118:136–149.

Laibson, D. (1997). Golden eggs and hyperbolic discounting. *Quarterly Journal of Economics*, 112(2):443–478.

Langer, T. and Weber, M. (2008). Does commitment or feedback influence myopic loss aversion? An experimental analysis. *Journal of Economic Behavior & Organization*, 67(3):810–819.

Levin, I. P. and Hart, S. S. (2003). Risk preferences in young children: Early evidence of individual differences in reaction to potential gains and losses. *Journal of Behavioral Decision Making*, 16(5):397–413.

Levin, I. P., Hart, S. S., Weller, J. A., and Harshman, L. A. (2007a). Stability of choices in a risky decision-making task: a 3-year longitudinal study with children and adults. *Journal of Behavioral Decision Making*, 20(3):241–252.

Levin, I. P., Weller, J. A., Pederson, A. A., and Harshman, L. A. (2007b). Age-related differences in adaptive decision making: Sensitivity to expected value in risky choice. *Judgment and Decision Making*, 2(4):225–233.

Lo, A. W., Repin, D. V., and Steenbarger, B. N. (2005a). Fear and greed in financial markets: A clinical study of day-traders. *American Economic Review*, 95(2):352–359.

Lo, A. W., Repin, D. V., and Steenbarger, B. N. (2005b). Fear and greed in financial markets: A clinical study of day-traders. Working Paper.

MacLeod, C. M. (1991). Half a century of research on the stroop effect: an integrative review. *Psychological Bulletin*, 109(2):163.

Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *Review of Economic Studies*, 60(3):531–542.

Manski, C. F. (2000). Economic analysis of social interactions. *Journal of Economic Perspectives*, 14(3):115–136.

Martinsson, P., Myrseth, K. O. R., and Wollbrant, C. (2014). Social dilemmas: When self-control benefits cooperation. *Journal of Economic Psychology*, 45:213–236.

Masatlioglu, Y., Nakajima, D., and Ozdenoren, E. (2011). Revealed willpower. mimeo.

Masicampo, E. J. and Baumeister, R. F. (2008). Toward a physiology of dual-process reasoning and judgment: Lemonade, willpower, and expensive rule-based analysis. *Psychological Science*, 19(3):255–260.

McPhee, J. H. (1996). Influence strategies in young adolescent dyads. *Dissertation Abstracts International*, 57(2):1468B.

Mehra, R. and Prescott, E. C. (1985). The equity premium: A puzzle. *Journal of Monetary Economics*, 15(2):145–161.

Michailova, J. and Schmidt, U. (2016). Overconfidence and bubbles in experimental asset markets. *Journal of Behavioral Finance*, forthcoming.

Miller, R. M. (2002). Can markets learn to avoid bubbles? *Journal of Psychology and Financial Markets*, 3(1):44–52.

Mischel, W. and Ebbesen, E. B. (1970). Attention in delay of gratification. *Journal of Personality and Social Psychology*, 16(2):329.

Mischel, W., Shoda, Y., and Rodriguez, M. I. (1989). Delay of gratification in children. *Science*, 244(4907):933–938.

Moffitt, R. (2001). Policy interventions, low-level equilibria, and social interactions. In Durlauf, S. and Young, P., editors, *Social Dynamics*, pages 45–82. MIT Press, Cambridge MA.

Molden, D. C., Hui, C. M., Scholer, A. A., Meier, B. P., Noreen, E. E., D'Agostino, P. R., and Martin, V. (2012). Motivational versus metabolic effects of carbohydrates on self-control. *Psychological Science*, 23(10):1137–1144.

Moulton, B. R. (1986). Random group effects and the precision of regression estimates. *Journal of Econometrics*, 32(3):385–397.

Muraven, M. (2010). Building self-control strength: Practicing self-control leads to improved self-control performance. *Journal of Experimental Social Psychology*, 46(2):465–468.

Muraven, M. and Baumeister, R. F. (2000). Self-regulation and depletion of limited resources: Does self-control resemble a muscle? *Psychological Bulletin*, 126(2):247.

Muraven, M., Baumeister, R. F., and Tice, D. M. (1999). Longitudinal improvement of self-regulation through practice: Building self-control strength through repeated exercise. *Journal of Social Psychology*, 139(4):446–457.

Muraven, M. and Slessareva, E. (2003). Mechanisms of self-control failure: Motivation and limited resources. *Personality and Social Psychology Bulletin*, 29(7):894–906.

Nadler, A., Jiao, P., Alexander, V., Johnson, C., and Zak, P. (2015). Testosterone and trading: A biological driver of asset mispricing. mimeo.

Noussair, C. N. and Tucker, S. (2013). Experimental research on asset pricing. *Journal of Economic Surveys*, 27(3):554 – 569.

Noussair, C. N., Tucker, S. J., and Xu, Y. (2014). A futures market reduces bubbles but allows greater profit for more sophisticated traders. Working Paper.

Odean, T. (1998). Are investors reluctant to realize their losses? *Journal of Finance*, 53(5):1775–1798.

O'Donoghue, T. and Rabin, M. (1999). Doing it now or later. *American Economic Review*, 89(1):103–124.

Ozdenoren, E., Salant, S. W., and Silverman, D. (2012). Willpower and the optimal control of visceral urges. *Journal of the European Economic Association*, 10(2):342–368.

Palan, S. (2013). A review of bubbles and crashes in experimental asset markets. *Journal of Economic Surveys*, 27(3):570–588.

Patton, J. H., Stanford, M. S., et al. (1995). Factor structure of the barratt impulsiveness scale. *Journal of Clinical Psychology*, 51(6):768–774.

Petersen, G.-K., Spickers, T., Glaser, M., and Brodbeck, F. C. (2015). How private investors' stress influences investor behavior and financial markets. Presentation at Experimental Finance Conference.

Porter, D. P. and Smith, V. L. (1995). Futures contracting and dividend uncertainty in experimental asset markets. *Journal of Business*, 68(4):509–541.

Rabin, M. (2000). Risk aversion and expected-utility theory: A calibration theorem. *Econometrica*, 68(5):1281–1292.

Rabin, M. and Weizsäcker, G. (2009). Narrow bracketing and dominated choices. *American Economic Review*, 99(4):1508–1543.

Raven, J. (1989). The raven progressive matrices: A review of national norming studies and ethnic and socioeconomic variation within the United States. *Journal of Educational Measurement*, 26(1):1–16.

Read, D., Loewenstein, G., and Rabin, M. (1999). Choice bracketing. *Journal of Risk and Uncertainty*, 19(1):171–197.

Reyna, V. F. and Ellis, S. C. (1994). Fuzzy-trace theory and framing effects in children's risky decision making. *Psychological Science*, 5(5):275–279.

Reyna, V. F., Estrada, S. M., DeMarinis, J. A., Myers, R. M., Stanisz, J. M., and Mills, B. A. (2011). Neurobiological and memory models of risky decision making in adolescents versus young adults. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 37(5):1125.

Sacerdote, B. (2001). Peer effects with random assignment: Results for Dartmouth roommates. *Quarterly Journal of Economics*, 116(2):681–704.

Samuelson, P. A. (1937). A note on measurement of utility. *Review of Economic Studies*, 4(2):155–161.

Schmeichel, B. J., Vohs, K. D., and Baumeister, R. F. (2003). Intellectual performance and ego depletion: role of the self in logical reasoning and other information processing. *Journal of Personality and Social Psychology*, 85(1):33–46.

Schmeichel, B. J. and Zell, A. (2007). Trait self-control predicts performance on behavioral tests of self-control. *Journal of Personality*, 75(4):743–756.

Seru, A., Shumway, T., and Stoffman, N. (2010). Learning by trading. *Review of Financial Studies*, 23(2):705–739.

Shapira, Z. and Venezia, I. (2001). Patterns of behavior of professionally managed and independent investors. *Journal of Banking & Finance*, 25(8):1573–1587.

Shefrin, H. and Statman, M. (1985). The disposition to sell winners too early and ride losers too long: Theory and evidence. *Journal of Finance*, 40(3):777–790.

Shiller, R. J. (1999). Human behavior and the efficiency of the financial system. *Handbook of Macroeconomics*, 1:1305–1340.

Shiller, R. J., Fischer, S., and Friedman, B. M. (1984). Stock prices and social dynamics. *Brookings Papers on Economic Activity*, 1984(2):457–510.

Smith, A., Lohrenz, T., King, J., Montague, P. R., and Camerer, C. F. (2014). Irrational exuberance and neural crash warning signals during endogenous experimental market bubbles. *Proceedings of the National Academy of Sciences*, 111(29):10503–10508.

Smith, V. L., Suchanek, G. L., and Williams, A. W. (1988). Bubbles, crashes, and endogenous expectations in experimental spot asset markets. *Econometrica*, 56(5):1119–1151.

Spinella, M. (2007). Normative data and a short form of the barratt impulsiveness scale. *International Journal of Neuroscience*, 117(3):359–368.

Sripada, C., Kessler, D., and Jonides, J. (2014). Methylphenidate blocks effort-induced depletion of regulatory control in healthy volunteers. *Psychological Science*, 25(6):1227–1234.

Stanford, M. S., Mathias, C. W., Dougherty, D. M., Lake, S. L., Anderson, N. E., and Patton, J. H. (2009). Fifty years of the barratt impulsiveness scale: An update and review. *Personality and Individual Differences*, 47(5):385–395.

Steinberg, L. (2007). Risk taking in adolescence. *Current Directions in Psychological Science*, 16(2):55–59.

Steinberg, L. (2008). A social neuroscience perspective on adolescent risk-taking. *Developmental Review*, 28(1):78–106.

Stojić, H., Anreiter, M. R., and Martinez, J. A. C. (2013). An experimental test of the dual self model. Working Paper.

Stroop, J. R. (1935). Studies of interference in serial verbal reactions. *Journal of Experimental Psychology*, 18(6):643.

Stöckl, T., Huber, J., and Kirchler, M. (2010). Bubble measures in experimental asset markets. *Experimental Economics*, 13(3):284–298.

Tanaka, T., Camerer, C. F., and Nguyen, Q. (2010). Risk and time preferences: Linking experimental and household survey data from Vietnam. *American Economic Review*, 100(1):557–571.

Tangney, J. P., Baumeister, R. F., and Boone, A. L. (2004). High self-control predicts good adjustment, less pathology, better grades, and interpersonal success. *Journal of Personality*, 72(2):271–324.

Thaler, R. H. (2015). *Misbehaving: The making of behavioral economics*. WW Norton & Company, New York.

Thaler, R. H. and Shefrin, H. M. (1981). An economic theory of self-control. *Journal of Political Economy*, 89(2):392–406.

Thaler, R. H., Tversky, A., Kahneman, D., and Schwartz, A. (1997). The effect of myopia and loss aversion on risk taking: An experimental test. *Quarterly Journal of Economics*, 112(2):647–661.

Thoma, V., White, E., Panigrahi, A., Strowger, V., and Anderson, I. (2015). Good thinking or gut feeling? Cognitive reflection and intuition in traders, bankers and financial non-experts. *PloS one*, 10(4).

Tice, D. M., Baumeister, R. F., Shmueli, D., and Muraven, M. (2007). Restoring the self: Positive affect helps improve self-regulation following ego depletion. *Journal of Experimental Social Psychology*, 43(3):379–384.

Tice, D. M. and Bratslavsky, E. (2000). Giving in to feel good: The place of emotion regulation in the context of general self-control. *Psychological Inquiry*, 11(3):149–159.

Toplak, M. E., West, R. F., and Stanovich, K. E. (2011). The cognitive reflection test as a predictor of performance on heuristics-and-biases tasks. *Memory & Cognition*, 39(7):1275–1289.

Toplak, M. E., West, R. F., and Stanovich, K. E. (2014). Assessing miserly information processing: An expansion of the cognitive reflection test. *Thinking & Reasoning*, 20(2):147–168.

Trautmann, S. T. and Vieider, F. M. (2012). Social influences on risk attitudes: Applications in economics. In *Handbook of Risk Theory*, pages 575–600. Springer, Berlin.

Trautmann, S. T. and Vlahu, R. (2013). Strategic loan defaults and coordination: An experimental analysis. *Journal of Banking & Finance*, 37(3):747–760.

Tversky, A. and Kahneman, D. (1981). The framing of decisions and the psychology of choice. *Science*, 211(4481):453–458.

Tversky, A. and Kahneman, D. (1992). Advances in prospect theory: Cumulative representation of uncertainty. *Journal of Risk and Uncertainty*, 5(4):297–323.

Tyler, J. M. and Burns, K. C. (2008). After depletion: The replenishment of the self's regulatory resources. *Self and Identity*, 7(3):305–321.

Tymula, A., Belmaker, L. A. R., Roy, A. K., Ruderman, L., Manson, K., Glimcher, P. W., and Levy, I. (2012). Adolescents' risk-taking behavior is driven by tolerance to ambiguity. *Proceedings of the National Academy of Sciences*, 109(42):17135–17140.

Unger, A. and Stahlberg, D. (2011). Ego-depletion and risk behavior: Too exhausted to take a risk. *Social Psychology*, 42(1):28–38.

Van der Heijden, E., Klein, T. J., Müller, W., and Potters, J. (2012). Framing effects and impatience: Evidence from a large scale experiment. *Journal of Economic Behavior & Organization*, 84(2):701–711.

Van Leijenhorst, L., Moor, B. G., de Macks, Z. A. O., Rombouts, S. A., Westenberg, P. M., and Crone, E. A. (2010). Adolescent risky decision-making: neurocognitive development of reward and control regions. *Neuroimage*, 51(1):345–355.

Van Rooij, M., Lusardi, A., and Alessie, R. (2011). Financial literacy and stock market participation. *Journal of Financial Economics*, 101(2):449–472.

Vohs, K. D., Baumeister, R. F., and Schmeichel, B. J. (2012). Motivation, personal beliefs, and limited resources all contribute to self-control. *Journal of Experimental Social Psychology*, 48(4):943–947.

von Gaudecker, H.-M., van Soest, A., and Wengstrom, E. (2011). Heterogeneity in risky choice behavior in a broad population. *American Economic Review*, 101(2):664–94.

Von Neumann, J. and Morgenstern, O. (2007). *Theory of Games and Economic Behavior*. Princeton University Press, Princeton.

Wakker, P. P. (2010). *Prospect theory: For risk and ambiguity*. Cambridge university press.

Weber, M. and Camerer, C. F. (1998). The disposition effect in securities trading: An experimental analysis. *Journal of Economic Behavior & Organization*, 33(2):167–184.

Weber, M. and Welfens, F. (2007). An individual level analysis of the disposition effect: Empirical and experimental evidence. Working Paper.

Wu, G. and Gonzalez, R. (1996). Curvature of the probability weighting function. *Management science*, 42(12):1676–1690.

Xu, H., Bègue, L., and Bushman, B. J. (2012). Too fatigued to care: ego depletion, guilt, and prosocial behavior. *Journal of Experimental Social Psychology*, 48(5):1183–1186.

Xu, X., Demos, K. E., Leahey, T. M., Hart, C. N., Trautvetter, J., Coward, P., Middleton, K. R., and Wing, R. R. (2014). Failure to replicate depletion of self-control. *PLoS ONE*, 9(10).

Zizzo, D. J. (2010). Experimenter demand effects in economic experiments. *Experimental Economics*, 13(1):75–98.

Zumbühl, M., Dohmen, T., and Pfann, G. A. (2013). Parental investment and the intergenerational transmission of economic preferences and attitudes. Working Paper.

Eidesstattliche Versicherung

Ich versichere hiermit eidesstattlich, dass ich die vorliegende Arbeit selbstständig und ohne fremde Hilfe verfasst habe. Die aus fremden Quellen direkt oder indirekt übernommenen Gedanken sowie mir gegebene Anregungen sind als solche kenntlich gemacht.
Die Arbeit wurde bisher keiner anderen Prüfungsbehörde vorgelegt und auch noch nicht veröffentlicht. Sofern ein Teil der Arbeit aus bereits veröffentlichten Papers besteht, habe ich dies ausdrücklich angegeben.

Datum: 05.07.2016

Unterschrift: Konstantin Lucks