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

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One explanation for overpricing on asset markets is a lack of traders' self-control. We implement the first experiment to address the causal relationship between self-control and systematic overpricing on financial markets. Our setup detects some of the channels through which low individual self-control could transmit into irrational exuberance in markets. Our data indicate a large direct effect of reduced self-control on market overpricing. Low self-control traders report stronger emotions after the market. (*JEL* G41, G11, G12, D53, D84)

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> 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.
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> —*John Maynard Keynes*[1]

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[1] (Keynes, 1936, p. 136).

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Keynes famously saw "animal spirits" at the root of many (financial) decisions, potentially causing price exaggerations on the aggregate market level. As is often the case in Keynes' work, his terms, specifically the term "animal spirits," are not well delineated. "Animal spirits" alludes to an individual's optimism, instincts, urges, and emotions, among other things. In this paper we assess the notion that a *lack of self-control* may lead to price exaggerations on asset markets, and we analyze how the lack of self-control as related to animal spirits is associated with emotions and trading behavior. In psychology, self-control and willpower are defined as the capacity to override or inhibit undesired behavioral tendencies, such as impulses, and refrain from acting on them (Tangney et al. 2004). Self-control is necessary to guard one's self from undue optimism, emotional responses, and impulsivity. Furthermore, self-control is required to adhere to previously made plans.

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" (Stone 1999). Similarly, anecdotal evidence from rogue traders show that they completely lost their self-control at some stage. In a study by Lo et al. (2005) involving day traders from an online training program, participants stated attributes related to self-control as the most important determinants of trading success.[2] 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 (i.e., low or high self-control) on trading outcomes.[3] The major challenge to overcome is to exogenously vary the level of an individual's self-control, to obtain causal inference on the impact of self-control on behavior and market outcomes. A first step is to use the experimental laboratory and to affect the *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 that varies over time by person. Evidence for these two characteristics is abundant (e.g. Baumeister et al., 1998, Gailliot et al., 2012), although it also has been questioned lately

2 Attributes such as persistence, tenacity, perseverance, patience, discipline, planning, controlling emotions, and (lack of) impulsivity are crucial (Lo et al., 2005, table 3).

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

(Carter and McCullough 2013). Validated survey measures for *trait* self-control exist, but they provide correlational inference only.

In the spirit of Keynes, we use a laboratory experiment to first investigate aggregate market outcomes and then extend our analysis to individual behavior and performance. 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 the level of self-control exhibit by traders leads to mispricing and, in particular, overpricing. This experimental asset market is known for its basic tendency to exhibit overpricing; it features a dividend-bearing asset with decreasing fundamental value. This setup resembles a large class of assets traded on real asset markets: options, bonds, and depletable resources all exhibit a value that decreases over time.

To deplete an individual's self-control before the start of the market, we employ the Stroop task (Stroop 1935), one of the most commonly used tasks in psychology experiments for modulating self-control (Hagger et al. 2010). The test is easily administered; 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 though 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 situations in which trait self-control matters.

A drop in self-control may increase the extent of overpricing on a market through a variety of channels. One psychological transmission mechanism runs through an increased influence of the impulsive decision-making system. A consequence could be that trader behavior becomes more easily swayed by observing others' behaviors in the market (for instance, a more pronounced tendency to momentum trading). Another behavioral mechanism relates to a 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 in the market.

Our main finding is a significantly higher level of overpricing in markets in which traders' self-control has been depleted, compared to markets with traders whose self-control has not been depleted. If markets are populated by both depleted and nondepleted traders, the effect is similar in size and also highly significant. Apparently, having some self-control-depleted traders on a market suffices to create the additional overpricing effect. We also observe that low self-control traders do not make lower average profits than high self-control traders as the trading behaviors converge quickly. An initial tendency of low self-control traders to bid low and early can only be found in the first trading period. As traders do not earn different profits following the convergence, low self-control traders are not driven out of the market, delivering an explanation why such a bias may persist in some markets.

We then investigate potential channels through which reduced self-control affects outcomes. First, risk attitudes or cognitive abilities of traders are not affected and therefore cannot mediate the treatment effect. Second, self-control-depleted subjects report stronger emotions that are commonly associated with overpricing in asset markets. Third, we find that cognitive abilities lose their predictive power for earnings when subjects are low in self-control. Together, these findings suggest that self-control-depleted traders become more reliant on heuristics, are much more emotion driven, and rely less on their cognitive skills to find optimal trading strategies.

1. Related Literature

Our literature overview focuses on two aspects of the economics and psychology literature most relevant for our study: self-control and experimental asset markets. As already stated, self-control and willpower are defined as the capacity to override or inhibit undesired behavioral tendencies, such as impulses, and refrain from acting on impulses. Different theoretical approaches in psychology and in economics consider self-control and potential problems with self-control.

First, self-control can be related to dual-systems perspectives of decision-making in a straightforward way. 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.[4] 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 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)) 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

[4] Note that the division of system 1 as automatic (or unconscious decision-making) and system 2 as controlled (or conscious decision-making) describes tendencies; both automatic and controlled processes are involved in exerting self-control and in giving in to temptation, respectively (cf. Kotabe and Hofmann, 2015).

behavior (for instance, in connection with quasi-hyperbolic discounting) and for the fact that cognitive load makes temptations more difficult 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 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.[5] Self-control can involve cognitive control, affective control, or both (Hagger et al. 2010). Self-control regenerates through rest, can be trained, and can 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 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 reveal an increase in risk-taking following similar manipulations (Bruyneel et al. 2009; Freeman and Muraven 2010; Friehe and Schildberg-Hörisch 2017). Both Stojić et al. (2013) and Gerhardt et al. (2017) 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. Buckert et al. (2017) report that the Stroop task reduces prices in a Cournot game, and 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, researchers have looked at the effects of varying levels of self-control 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.[6]

[5] 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).

[6] Martinsson et al. (2014) indirectly analyze the relationship between self-control and prosociality, but their findings are also in line with the idea that prosocial behavior requires self-control. Kocher et al. (2017) provide a similar result.

Existing studies also suggest a relationship between self-control 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 U.S. households. Ameriks et al. (2003) attribute differences in savings among households to differing "propensities to plan," that is, 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 and wealth accumulation for liquid assets, in particular. Gathergood (2012) conducts a similar study in the United Kingdom with a representative sample. He reports a positive association between lower levels of self-control and consumer overindebtedness.

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.[7] Trader inexperience and confusion have been considered as one of the aggravating factors of overpricing (Dufwenberg et al. 2005; Kirchler et al. 2012), and Bosch-Rosa et al. (2018), for example, show that grouping traders by cognitive skills leads to increased overpricing for groups with low cognitive sophistication. Nadler et al. (2017) provide evidence that giving testosterone to a group of male participants significantly increases prices, and Petersen and Spickers (2015) find that inducing stress decreases overpricing. In line with our findings, Dickinson et al. (2017) report that sleepy traders generate more overpricing. They interpret these findings as a result of an inhibition of cognitive functions and a reduced ability to anticipate others' actions, which is somewhat different from—though loosely related to—exerting self-control, that is, the capacity to override undesired behavioral tendencies.

Since emotion regulation is correlated with self-control (Tice and Bratslavsky 2000), the influence of emotions on prices in asset markets is also relevant to our research question: Andrade et al. (2016) 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 also has been evaluated using self-reported emotions on Likert scales (Hargreaves Heap and Zizzo 2011) and face reading software (Breaban and Noussair 2018), 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.

[7] See Noussair and Tucker (2013) and Palan (2013) for recent surveys.

2. Experimental Design

The experiment consisted of four independent main parts: (1) instructions and dry runs of the asset market without monetary consequences and without the possibility to build reputation for the parts to come; (2) the main treatment variation in self-control, the Stroop task (Stroop 1935) in two treatment versions; (3) elicitation of risk attitudes and cognitive abilities, both incentivized; and (4) a fully incentivized experimental asset market.

Our identification of the effects induced by a variation in self-control in market prices relies on the comparison of behavior in markets following two different versions of the Stroop task. A tough version lowered self-control, whereas a placebo version should have left self-control largely unaffected. We implemented a condition in which all market participants were subjected to the tough version of the Stroop task (henceforth *LOWSC* for low self-control), a condition in which all participants were subjected to the placebo version (henceforth *HIGHSC* for high self-control), and a condition in which half of the participants were randomly assigned to the tough and the placebo versions, respectively (henceforth *MIXED* to denote the mixed nature of the market). In the *MIXED* condition, we will refer to traders facing the tough version of the Stroop task as *MIXLO* and to those facing the placebo version of the Stroop task as *MIXHI*, to avoid confusion with the pure treatments. Except for this treatment variation in part (2), the three experimental conditions were identical in all other parts.

The Stroop task followed a simple protocol: participants were instructed to correctly solve as many problems as possible within 5 minutes. The left-hand side of Figure 1 displays an example of this problem. The task was to select the color of the font the word was printed in. A selection of six color buttons, always the same and in the same order, was given on the bottom right of the screen, and subjects were instructed to click on the correct one. As soon as they made a selection, the next word-color combination appeared. Consecutive word-color combinations always differed from each other. The difficulty of this task was that the words always described one of the six colors; the incongruence between the color of the word and the word itself caused a cognitive conflict, since reading the word was 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 had to be resolved, and resolution required self-control. Applying this effort depleted self-control, and left participants with lower levels of willpower and/or self-control after completing the 5-minute task.

The Stroop task is one of the most commonly applied methods to deplete self-control (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/MIXLO* and *HIGHSC/MIXHI* was the frequency with which a conflicting word-color

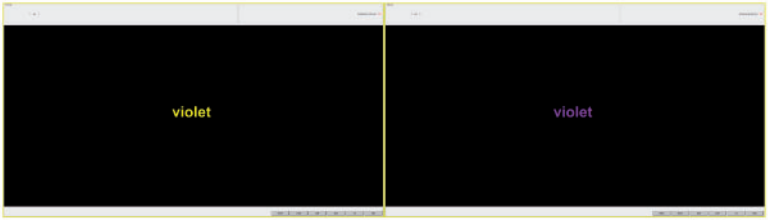

Figure 1
Treatment differences in the Stroop task. The left panel shows the word violet printed in yellow. The right panel shows the word violet printed in violet. (The color version of this figure is available online).

combination occurred.[8] All screens in *LOWSC/MIXLO* exhibited such a conflict, whereas, in *HIGHSC/MIXHI*, only every 70th screen did. Experimental participants did not receive any information on the frequency of such a conflict, and the instructions for the two versions of the task were identical. By having an occasional word-color incongruence in *HIGHSC/MIXHI* we were able to ensure that subjects took the task seriously. If anything, our setup reduced the potential treatment difference, because in *HIGHSC/MIXHI* some self-control depletion might still have taken 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 €3.00 for the Stroop task, to signal that we were interested in their performance. We did not use a piece rate or any other competitive payment scheme because it might have created different wealth levels after the treatment variation, and wealth differences might have been correlated with the treatment. Hence, treatment differences might have potentially been confounded by wealth effects.[9] After participants completed the 5-minute task, we asked them how difficult they perceived the task to be on a six-point Likert scale.

Self-control depletion can influence several relevant variables for the subsequent experimental asset market. We control for two mechanisms directly: cognitive ability and risk attitudes.[10] Eliciting control variables took place after the self-control manipulation but before the experimental asset market for two reasons: First, if these measures had followed the asset market, there might have been spillover effects due to experiences during the asset market, and second, the effect of our self-control manipulation might have worn off since the asset market part of the experiment lasted a considerable amount of time during which self-control could start to regenerate (Muraven and Baumeister 2000).

[8] The right-hand side of Figure 1 shows an example of congruence between font color and a word, as we use it in the placebo Stroop task in *HIGHSC/MIXHI*.

[9] 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; no one answered fewer than 110 items. Most of our subjects answered many more. For details, see the Online Appendix Section A.3.

[10] For evidence of potential effects of self-control depletion on complex thinking, see Schmeichel et al. (2003).

To avoid having the self-control variation wear off before the asset market interaction started, we required the control variables measurement not to take much time. Two tasks that fitted this requirement were 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 answered the three questions of the standard CRT. It is well known that CRT responses are correlated with more time-consuming measures of cognitive ability and risk and time preferences (Frederick 2005) and with decisions in a wide array of experimental tasks, such as entries in p-beauty contests (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. (2016) find that the CRT is a good predictor of individual trader's profits in asset market experiments.[11] Subjects were paid €0.5 for every correct answer but did not learn their CRT results—and thus earnings—until the end of the experiment.

Second, we elicited 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 in the market, thus sparking 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 had to choose 10 times between a lottery that paid either €0.20 or €4.20 with equal probability and increasing certain amounts of money that were equally spaced between the two outcomes of the lottery. Subjects could switch at most once from the lottery to the certain amounts. At the end of the experiment, the computer randomly picked one of the 10 decisions of each individual as payoff relevant and implemented the preferred option, potentially simulating the lottery outcome.

Immediately after risk elicitation the main part of the experiment, the asset market, opened. The asset market featured a dividend-bearing asset with decreasing fundamental value over 10 trading periods (lasting 120 seconds each) in a continuous double-auction market design with 10 traders and with open order books, following Kirchler et al. (2012).[12] This is a simplified version of the markets in Smith et al. (1988). Before the first trading period, five subjects in a given market received 1000 experimental points in cash and 60 assets, and the other five received 3000 points in cash and 20 assets as their initial endowment. Assignment to the two initial asset allocations was random.

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[11] The CRT is a measure of cognitive ability and thinking disposition (Toplak et al. 2011). Therefore, we strictly regard the CRT as a measure of cognitive skills.

[12] Online Appendix Section A.7.1 provides the experimental instructions, including a screenshot and a description of the trading screen.

During each trading period, traders could post bids and asks as well as accept open bids and asks. Partially executed bids and asks continued 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 paid a dividend of either 10 or 0 experimental points with equal probability. The dividend payment was added to each trader's cash holdings. Assets had no remaining value after the last dividend payment; that is, they displayed a declining (expected) fundamental value. This design feature was explicitly stated and highlighted in the instructions. To make things clear, the instructions provided a detailed table with the sum of remaining expected dividend payments per unit of the asset at any point in time. Assets and cash were carried from period to period. Short selling and borrowing experimental points were not allowed. After every period, the average trading price as well as the realizations of the current and all past dividends were displayed on a separate feedback screen. At the end of the 10 periods, experimental points were converted into euros, using an initially announced exchange rate of 500 points = €1.00.

In 8 of the 12 *MIXED* markets, 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. To reduce experimenter demand effects—and as is common in experiments analyzing emotions—we presented subjects with several emotions, of which some were not relevant at all to our question of interest. In four of the eight sessions, we asked subjects about their emotions not only after the 10 trading periods but also immediately after the first trading period.

At the end of the experiment, subjects learned about their payoffs from all parts of the experiment. We asked them to complete a short questionnaire concerning demographics and background data. We also asked participants how tired they felt after the experiment and how difficult they have perceived decisions to be over the course of the entire experiment on a 6-point Likert scale. Then all earnings were paid out in private, and the subjects were dismissed from the laboratory.

The sessions for the treatment *HIGHSC* and *LOWSC* were conducted in October 2013. Participants (160) took part in 10 experimental sessions: four with one market and six with two markets. Hence, we obtained 16 independent observations, 8 for each condition. We conducted a total of 12 sessions with 24 markets and 240 subjects of the *MIXED* treatment in April 2014, November 2015, and October 2017 (four sessions each). In all, 400 subjects participated in 40 markets. The experiment was programmed using z-Tree (Fischbacher 2007), and recruitment was performed with the help of ORSEE (Greiner 2015). Experimental sessions lasted for about 90 minutes, and participants earned €18.27 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.[13] 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.

3. Experimental Results

3.1 Manipulation check

The data suggest that our treatment manipulation was successful: First, during the Stroop task, participants attempted fewer problems, achieved fewer correctly solved problems, and made more mistakes in the *LOWSC/MIXLO* condition than in the *HIGHSC/MIXHI* condition (all Mann-Whitney tests $p < .01$; $N = 400$).[14] Participants perceived the Stroop task as significantly more demanding in the *LOWSC/MIXLO* condition than in the *HIGHSC/MIXHI* condition (Mann-Whitney test $p < .01$; $N = 400$). Finally, we do not find any differences in background characteristics such as field ($p = .695$; $N = 400$) and year of study ($p = .358$; $N = 400$), age ($p = .573$; $N = 400$) and gender ($p = .679$; $N = 400$) 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.

3.2 Definitions and measures

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.

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)) and *relative deviation* (RD) (in Equation (2)) as measures for general mispricing and overpricing, respectively.

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

[13] Of our 400 subjects, five suffer from some form of dyschromatopsia, a vision impairment that makes seeing colors difficult. We queried participants about the disorder in the post-experimental questionnaire, to make sure that it is not a common phenomenon.

[14] Online Appendix Section A.3 provides detailed distributions for these variables. All tests reported in this paper are two-sided unless stated otherwise.

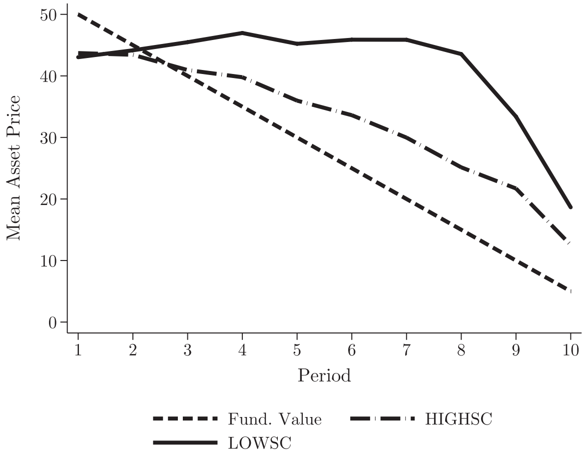

Figure 2
Mean (volume-adjusted) trading prices in the two treatments

$$\mathrm{RD} = \frac{1}{T}\sum_{t=1}^{T}\frac{P_t - FV_t}{\bar{FV}} \tag{2}$$

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 have straightforward interpretations: a RAD of 0.1 means that prices are on average 10% *off* the fundamental value, while a RD of 0.1 indicates that prices are on average 10% *above* the fundamental value. Both measures are independent of the number of periods and the fundamental value.

3.3 Aggregate price development

Figure 2 shows how average market prices in *LOWSC* and *HIGHSC* evolve over the 10 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 sharply drop, but not below the fundamental value again.

The most conservative comparisons between these two treatments are based on market averages over all traders and over all 10 periods. This is the approach we apply for all nonparametric tests regarding aggregate market outcomes. These averages are statistically independent in the strict sense, and test statistics are based on eight observations for each of the two treatments. Thereby, we also eliminate all temporal correlation to make sure that our standard errors are not biased by autoregressive properties of the data. A Wilcoxon signed-rank test confirms the impression from eyeballing, that is, that market prices in both conditions are significantly different from the fundamental value (*HIGHSC*: $p = .0929$, *LOWSC*: $p = .0173$; $N = 8$).[15] Figure 2 suggests more pronounced overpricing in the *LOWSC* condition than in *HIGHSC*, which is confirmed by a Mann-Whitney test (*HIGHSC*: $R\bar{D} = 0.1885$, *LOWSC*: $R\bar{D} = 0.4990$; $p = .0742$; $N = 16$).[16] 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: $p = .0460$; $N = 16$). 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 3 displays the price evolution of single markets in the two conditions. Price evolution in the markets exhibits a high degree of path dependence and endogeneity 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, whereas 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.[17]

15 We use $p < .1$ as the lowest significance level. The choice of this significance level is common practice in economics and finance and only matters for our most conservative comparisons with very few observations. In all regression outputs, relevant coefficients always will be significant at the $p < .05$ level or better.

16 Both measures are significantly different from zero for both conditions.

17 Online Appendix Section A.1 shows a comparison of overpricing measures across treatments for each period separately. Overpricing in *LOWSC* significantly exceeds overpricing in *HIGHSC* in periods 6–9.

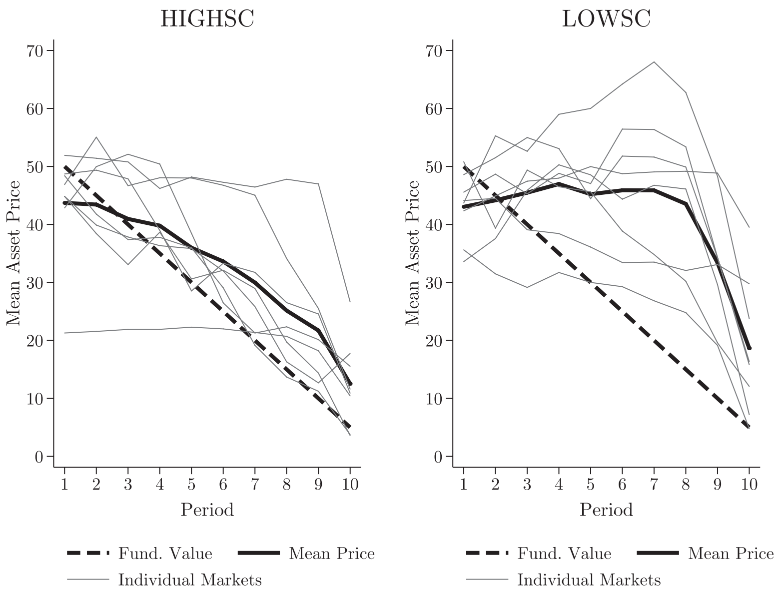

Figure 3
Evolution of individual market prices in *HIGHSC* and *LOWSC*

Figure 4 shows the evolution of average trading prices in all three treatments. 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*. For *MIXED* we observe an average RAD of 0.529 and an average RD of 0.398. A Mann-Whitney test confirms that the mispricing measure RAD in *MIXED* is significantly different from *HIGHSC* ($p = .056$; $N = 32$) but cannot be statistically distinguished from *LOWSC* ($p = .5716$; $N = 32$). This result also holds for our overpricing measure: RD in *MIXED* differs significantly from *HIGHSC* ($p = .0982$; $N = 32$), but not from *LOWSC* ($p = .4334$; $N = 32$).[18]

Figure 5 illustrates the evolution of mean trading prices for the 24 individual markets in the *MIXED* condition. Qualitatively, we get similar results as in *LOWSC*. That is, in some of these markets, price development is hump shaped: it initially increases and peaks in some intermediate period. Thus, already the presence of a moderate share of traders with depleted self-control is sufficient to reproduce the excess overpricing we observed when all traders' self-control levels were depleted. This seems relevant as in reality we do not expect all traders, but only a moderate share, to be low in self-control.

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

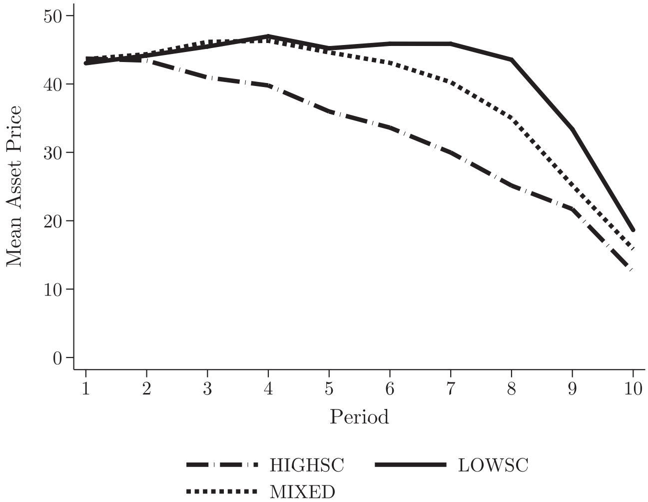

Figure 4
Trading price evolution including *MIXED*

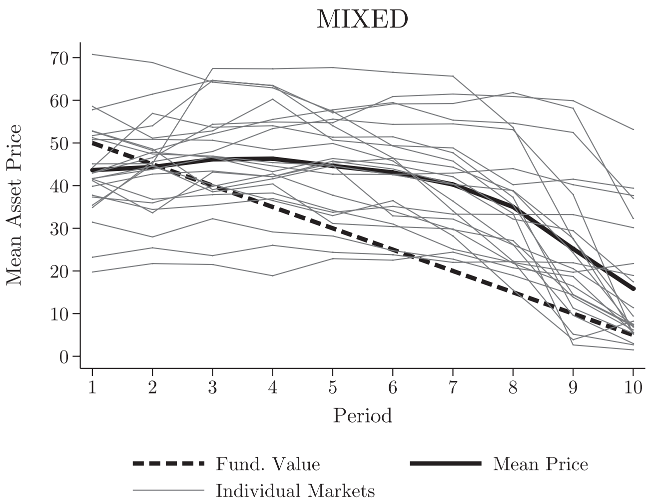

Figure 5
Price evolution in individual markets in *MIXED*

3.4 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 affected market outcomes. Detailed descriptive results on the variables considered in this section can be found in the Online Appendix Section A.4ff.

3.4.1 Cognitive abilities and risk attitudes. Self-control-depleted participants might not be willing to expend as much energy 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 scores between the two conditions is not significant according to a Mann-Whitney test ($p = .7223$; $N = 160$). We conclude that the Stroop task did not affect our incentivized version of the CRT.[19] Furthermore, risk attitudes might be affected by self-control. 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., 2017), we also find no significant effect (Mann-Whitney test, $p = .4083$; $N = 160$) of our treatment variation on risk attitudes as measured by the multiple price list certainty equivalent elicitation.[20]

Although our control variables seem unaffected by our treatment, they could still possess explanatory power for the difference in overpricing that we observe. Therefore, we run regressions and include controls as independent variables. To avoid endogeneity problems across trading periods and between subjects, respectively, we aggregate overpricing measures over all 10 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 definition of the RD measure, 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 at the market level as before. We report results for $IndRD_{purchases}$ as the dependent variable in the regressions in Table 1. In Tables A.2 and A.3 in the Online Appendix Section 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 is reduced. Performance on the CRT has the expected effect of reducing the

[19] When we include the observations from the *MIXED* conditions, the CRT scores of the two groups become 1.08 and 1.18, respectively, with $p = .3911$ and $N = 400$ from a Mann-Whitney test.

[20] Including observations from the *MIXED* conditions does not provide significant differences between the two groups either ($p = .9134$; $N = 400$).

Table 1
Determinants of individual RD based on purchases

	(1)	(2)	(3)	(4)
		$IndRD_{purchases}$		
LOWSC	0.369**	0.390**	0.816***	0.843***
	(0.136)	(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	0.084	0.194	0.0255	−0.0353
	(0.082)	(0.120)	(0.0597)	(0.0682)
Observations	160	110	110	110
R^2	.227	.307	.364	.370

This table provides results from an ordinary least squares (OLS) regression, where the 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 that equals 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 are excluded from Columns 2 through 4. Heteroscedasticity-robust standard errors are clustered at the market level in parentheses. *** p<.01; ** p<.05; * p<.1.

tendency of buying at prices above fundamental value in all specifications where it is included, although not always statistically significant, and its effect does not significantly differ between participants in *LOWSC* and *HIGHSC* markets.[21] 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 can explain our main result of excess overpricing after self-control depletion.

3.4.2 Trading activity. An additional channel through which our results could be explained is changes in trading activity, that is, 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). But "passiveness" may not necessarily equal lower trading frequencies. The authors of that paper define "passive" as following usual action patterns without deliberation, while they define "active" as the result of cognitive effort to determine the best action to be chosen. As an example, they describe a married couple deciding whether to go to sleep. While the decision to go to bed could be considered "active" in

[21] From all regressions including this variable, we exclude subjects who were familiar with the CRT, because such knowledge might have inflated correct CRT responses and thus obfuscate any effects of CRT scores. The regression results are qualitatively very similar when including these subjects.

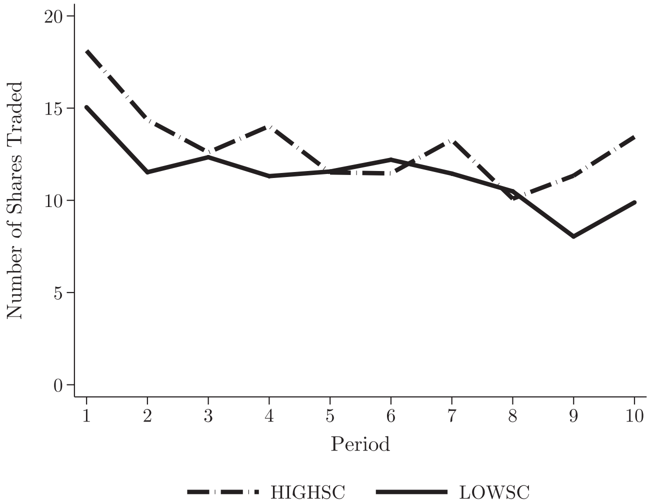

Figure 6
Evolution of average shares traded per trader by condition

terms of movement, it also may be "passive" if the couple does so as part of a routine.

We think that "passiveness" in our experiment can in principle be understood in the same way. If being passive simply means to act less deliberately and follow more common heuristics, it could be that traders low in self-control trade more and earlier, as they might rely on a preconception of the notion of a trader as frequently buying and selling stocks. Then higher trading activity might translate into more liquid markets (Brennan and Subrahmanyam 1995; Chordia et al. 2001; Stoll 1978), which, in turn, can drive overpricing in experimental markets (Kirchler et al. 2012). Alternatively, if "passiveness" meant lower trading activity, under certain circumstances we might also expect a positive effect of trading activity on prices as trading at excessive prices by only a small set of traders might be enough to drive aggregate price evolution.

Thus, we compare the number of shares traded in the two conditions. Figure 6 illustrates the evolution of average shares traded per period. Traders in *HIGHSC* traded slightly more overall: whereas 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, the amounts traded between the two conditions are not significantly different ($p = 0.3446$; $N = 16$).[22]

Therefore, we turn to our *MIXED* condition. We investigate if we can find differences in trading behavior when both treated and untreated traders interact

22 Additional regression analysis in Table A.4 in the Online Appendix Section A.2 reinforces this conclusion.

Table 2
First period differences in trading behavior

	Group Mean		
	MIXHI	*MIXLO*	*p*-value
$\overline{p_{bid}}$	35.927	28.539	.044**
$\overline{p_{ask}}$	59.303	53.097	.753
$\overline{q_{bid}}$	14.511	17.076	.119
$\overline{q_{ask}}$	12.940	13.813	.627
$\overline{time_{bid}}$	60.732	47.318	.044**
$\overline{time_{ask}}$	51.410	51.194	.954
$\overline{firsttime_{bid}}$	52.978	41.458	.108
$\overline{firsttime_{ask}}$	34.176	36.358	.690

Variables starting with a p denote prices and variables starting with q denote quantities. Time variables refer to the time passed in the current period, and, thus, lower values indicate quicker actions. bid and ask refer to posted bids and asks. p-values are from a Wilcoxon signed-rank test with data collapsed on market and treatment level. *** p<.01; ** p<.05; * p<.1.

in the same market. However, when analyzing trading behavior, distinguishing cause and effect is particularly difficult, as most of the trading behavior may be endogenous due to interactions across traders. A particular deviation in behavior by some traders in the early phases of a market might shift behavior of other (nondepleted) traders. Therefore, we start by focusing on the first trading period, where dependencies are less relevant than in later periods. Table 2 compares several variables concerning trading activity between *MIXLO* and *MIXHI* traders.

According to Wilcoxon signed-rank tests, *MIXLO* traders make significantly lower bids initially (p=.044; N=22) and post these bids earlier than their nondepleted peers (p=.044; N=22). They are also quicker in posting their first bid at the beginning of the period, but this test misses conventional significance levels (p=.108; N=22). After period one, these differences vanish, suggesting that nondepleted traders start imitating the behavior of self-control-depleted traders.[23] Imitation seems to arise as markets subsequently follow the trajectory observed in *LOWSC* markets, rather than that of *HIGHSC* markets, suggesting that all traders eventually behave like those in our *LOWSC* treatment. There exist several possibilities why *MIXHI* traders may start imitating the behavior of *MIXLO* traders. Remember that traders do not know that different levels of self-control are present in traders in the market. For example, they might expect those who bid early to have a better understanding of the optimal strategies, or they could simply enjoy the faster pace of the market and therefore adjust. In our setting, it is impossible to pinpoint the motivation and the beliefs of traders, but we think that this would be an interesting starting point for future research.

We conclude our analysis of trading behavior by noting that while overall trading activity is not increased, we still observe initial differences in posting bids. This behavior seems in line with Baumeister et al.'s (1998) definition of

23 Results for period two, which are reported in the Online Appendix Table A.8, indicate that these initial trading differences disappear.

"passiveness," that is, an automatism, without much active, effortful, and time-consuming deliberation. In contrast, thinking more deeply about the underlying dividend process and strategies to act optimally in the market against other traders would require more cognitive effort and thus potentially more time. Therefore, posting bids early can be seen as a manifestation of less deliberate choices.

3.5 Profits

On average, *MIXLO* traders earned €8.23, and *MIXHI* traders earned €7.77 in the experimental asset market, a difference that is not significant (Wilcoxon signed-rank test, $p=.2531$; $N=24$).[24] We consider this as evidence that inhibited self-control affects overpricing, but that depleted traders are not necessarily driven out of the market. While it initially may seem as a puzzle that *MIXLO* traders do not suffer consequences, it is very well in line with our findings from the previous section on trading activity. Because trading behavior differs significantly only during the first period and converges afterwards, traders eventually become indistinguishable in their behavior. With similar behavior and profits, low self-control traders have shifted markets onto different price trajectories. This finding may carry potential implications for real markets, as markets attracting many inexperienced traders may lead these traders to imitate other, possibly self-control-depleted traders. The resultant lack of differences in profits may thus be a reason behavioral biases persist in real markets (Barberis and Thaler 2003).

Previous research has furthermore shown that CRT scores correlate positively with individual participants' profits in similar experiments (Corgnet et al. 2014; Noussair et al. 2016). 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. To check whether the effect of CRT performance on profits is similar here, we ran additional regressions which we report in Table 3. Note that we excluded participants who had indicated at the end of the experiment that they had already known at least one of the CRT questions. The knowledge of CRT questions before the experiment might have inflated correct CRT responses and 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 in showing that higher CRT scores are positively related to higher overall profits for both *MIXLO* and

[24] Testing profits at the market level constitutes the most conservative test. Results are similar when we test on the subject level instead ($p=.3520$; $N=240$).

[25] Seventy-two subjects in *MIXED* markets reported having known at least one of the CRT questions. Including these subjects renders the coefficient of the interaction term CRT × MIXLO insignificant in the specifications parallel to (3) and (4), as these subjects dilute the effect.

Table 3
Determinants of profits in *MIXED*

	(1)	(2)	(3)	(4)
		Profit		
MIXLO	0.468	0.924	3.803**	3.855**
	(0.377)	(0.591)	(1.776)	(1.797)
CRT		0.960**	1.747***	1.6776***
		(0.356)	(0.469)	(0.508)
CE		0.325	0.607	0.443
		(0.426)	(0.580)	(0.575)
CRT × MIXLO			−1.485***	−1.419**
			(0.507)	(0.531)
CE × MIXLO			−0.839	−0.853
			(0.843)	(0.834)
Female				−1.045
				(0.634)
Constant	7.766***	5.723***	4.617***	5.689***
	(0.189)	(0.906)	(1.097)	(1.288)
Observations	240	137	137	137
R^2	.004	.081	.121	.138

Participants who indicated that they knew at least one of the CRT questions are excluded in Columns 2–4. Robust standard errors are clustered at the market level in parentheses. *** p<.01; ** p<.05; * p<.1.

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 ($p < .01$) and in fact cannot be distinguished from zero overall (post-estimation Wald test; $p = .56$).

Thus, *MIXLO* subjects cannot turn advantages in cognitive skills into higher trading profits. These findings reinforce our earlier results on trading activity that suggested less deliberate decision-making by depleted subjects. 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).[26] As Section 3.4.2 demonstrated that all initial differences in behavior vanish after the first period, it therefore seems possible that *MIXHI* subjects, by imitating *MIXLO* subjects, might have adapted the use of heuristics over careful deliberations as the basis to form their trading decisions.

3.6 Increased emotional reactivity

If depleted subjects' trading patterns were more strongly driven by impulsive actions or heuristics rather than deliberative processes, it may be that the treatment also affected their propensity to act based on emotions. In the experimental sessions that we conducted in November 2015 and October 2017, we asked participants a number of questions relating to their emotional

[26] Hefti et al. (2016) argue that good performance in an asset market requires two dimensions of cognitive capabilities: (1) mentalizing and (2) cognitive abilities. Self-control depletion could, in principle, affect both dimensions and lead subjects to act impulsively. We deem this an interesting question for future research.

Table 4
Ex post reported emotions of traders in *MIXED*

	MIXHI	MIXLO	*p*-value	*N*
A. Beginning of the first period				
Excitement	4.088	4.325	.300	16
Fear	2.113	2.200	.717	16
Surprise	3.475	3.813	.168	16
Joy	3.475	3.888	.097*	16
B. End of the last period				
Excitement	3.538	4.175	.030**	16
Fear	2.163	2.513	.107	16
Surprise	2.788	3.350	.066*	16
Joy	3.188	3.813	.055*	16
C. Asked after the first period				
Excitement	3.925	4.200	.483	8
Fear	2.325	2.475	.833	8
Surprise	2.900	3.350	.056*	8
Joy	2.800	3.275	.140	8
D. Self-evaluation of emotional reactivity				
Emotion driven	2.600	2.925	.066*	16
Suppressed emotions	4.913	4.713	.468	16

Data were collapsed on the treatment level per market. Responses are based on a seven-point Likert scale. Test results are from a Wilcoxon signed-rank test. *** p<.01; ** p<.05; * p<.1.

experience during trading in 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.[27] In the sessions conducted in October 2017, we additionally asked participants for their emotions directly after the first trading period, to assess whether their recollection is accurate.

Table 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. 2016; Lahav and Meer 2012; Breaban and Noussair 2018). Note that we collapse all the emotional measures on the treatment group level within each market and test for differences with Wilcoxon signed-rank tests. Strikingly, except for one measure, the intensity of all experienced emotions is higher in the *MIXLO* than in the *MIXHI* group. At the beginning of period 1, *MIXLO* participants report feeling significantly more joy ($p = .097$). Remember that Lahav and Meer (2012) found that inducing positive mood before trading leads to higher deviations from fundamental values and thus larger levels of overpricing and that correlational studies also suggest such a positive relationship (Breaban and Noussair 2018; Hargreaves Heap and Zizzo 2011). Furthermore, at the end of the final trading period, *MIXLO* traders report significantly higher levels of excitement, joy and surprise than *MIXHI*

27 We also provided participants with a questionnaire about their trading behavior, but we do not report those results here. Online Appendix Table A.6 provides the average responses to all emotion-related questions and the test statistics. Online Appendix Table A.7 reports average values for changes in emotions over time.

participants (all $p < .1$; excitement $p < .05$; fear $p = .107$). When asked for their emotions directly after the first period, subjects also report stronger emotions when self-control depleted, although only the level of surprise is significantly higher. The lack of significant differences may be a consequence of the small sample size for these comparisons ($N = 8$). The general picture, however, suggests that the emotional experience of the asset market was different for *MIXLO* and *MIXHI* traders.

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 Table 4). *MIXLO* participants report to have acted more emotion driven ($p = .066$). They also report lower levels of emotion suppression, but the differences fail to reach significance at conventional levels. The results indicate that the behavior of the traders with depleted self-control might have been driven by emotional factors to a larger degree than they were themselves aware of.

The substantially higher level of emotions displayed by *MIXLO* participants, combined with their inability to convert cognitive capacities into higher earnings reported earlier, shows the strong adverse effects of a reduction in self-control on deliberative decision-making. In line with our earlier findings on trading activity, depleted subjects seem to be less able to spend time and resources on thinking about optimal strategies, but rather relied on quickly available heuristics and emotional responses. Because the behaviors of depleted and nondepleted subjects converge quickly, they do not suffer negative consequences from their actions. Instead, they seem to shift markets onto different price trajectories.

4. Welfare Implications

We establish a strong effect of reduced self-control on prices, but an important question remains: is welfare affected? After all, real-life asset markets exist because trade can be welfare improving.[28] In the following, we discuss how our setting can be informative for markets outside the laboratory.

First, we think that even a setup using a zero-sum environment can be informative. Note that under the restrictive assumption of risk-neutrality, we should not observe any trades. With heterogeneity in risk preferences (which we observe), however, trade can be welfare improving if the risk (of the stochastic asset) is transferred from risk-averse to risk-loving traders. This is exactly one kind of welfare improvement that real asset markets generate. Technically, trading an asset is only a zero-sum exchange of money for assets, while the valuations (e.g., because of different risk profiles) of investors may differ.

[28] We are very grateful to Wei Jiang and two anonymous referees for raising this question.

Therefore, that our depleted traders trade at higher prices without measurable changes in risk preferences must be, on average, welfare decreasing if the choices at higher levels of self-control were optimal. This, we argue, constitutes a reasonable assumption, given that our findings have shown that depleted subjects respond more emotionally and therefore less deliberatively.

Second, one could understand the particular experimental asset market as a partial equilibrium depiction of a real asset market. Assuming that some investors with market power have information about the value of the asset while others without market power have not, prices may then be used as signals by uninformed investors (Fama 1970; Grossman 1976; Radner 1979; Wolinsky 1983). Higher prices generated due to a lack of self-control of informed traders could entice other traders to wrongly invest in inferior assets, thereby harming welfare as more lucrative options are passed on. This might even happen, as our *MIXED* treatment indicates, when only a moderate share of traders is low in self-control.

To show that uninformed traders actually make wrong investment choices, we report results from another experiment, conducted in January 2018. We invited 96 subjects in four sessions to the laboratory and made them choose repeatedly between an asset with known value (KVA), and an asset with unknown value (UVA) for which they only could observe the trading price. In 20 decisions, appearing in random order for each subject, the UVA's trading price was simply the average trading price from each of the 10 periods of our *LOWSC* and our *HIGHSC* treatments. The KVA was always superior to the value underlying the UVA. In each decision, subjects received 100 points to distribute among the KVA and the UVA. Subjects were made aware that the UVA's price was determined by other human traders who were informed about the asset's value.[29]

We observe that subjects invest on average 58.60 points in the inferior asset when prices were generated by the traders in the *LOWSC* treatment, but only 43.91 points when prices were generated by the traders in the *HIGHSC* treatment. This difference is highly significant according to a Mann-Whitney test ($p < .01$; $N = 96$). Optimal choices would have required an investment of 0 points in the UVA; that is, low self-control traders raise the existing inefficiency by about 33%, which translates into a welfare reduction. We conclude that excessively high prices may harm welfare substantially if uninformed traders are present and follow the strategy that our experimental participants followed.

5. 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

[29] Online Appendix Section A.7.2 provides instructions, and decisions were incentivized.

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). When comparing the three market settings in which we reduce the self-control of either (1) all, (2) half, or (3) none of the traders, we observe significantly more mispricing and overpricing when at least some traders are self-control depleted.

We provide evidence that in markets populated by both, self-control-depleted and nondepleted traders, initial bidding behavior is different in the two groups. Depleted subjects bid earlier, suggesting they may not deliberate as much as nondepleted subjects and seem to rely more on quickly available heuristics instead. 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. Furthermore, we find that our measure for cognitive skills loses predictive power for the profits of low self-control traders. These results are all 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 heuristics and less on the basis of cognition, thus driving up prices. As profits, however, do not differ between depleted and nondepleted subjects due to quick convergence in behavior, these biases may persist in markets also outside the experimental laboratory. We discuss that higher prices may be an issue from a welfare perspective, as they distort traders' decisions and may serve as misleading signals for uninformed buyers.

Our findings have relevant practical 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 is sufficient to more than double the extent of overpricing in our experiment. If the finding carries over to asset markets outside the laboratory, this may be problematic in cases in which large parts of the population engage in stock market trading (e.g., Chinese markets). If some of these investors suffer from self-control problems, for whatever reasons, they might be susceptible to using less deliberative choices and might exacerbate stock market bubbles, even in the presence of more rational traders.

Second, outside the experimental laboratory, both temporary reductions in self-control and the personality trait self-control are supposed to play an important role in determining trading behavior and perception of market developments. Self-control also might be a relevant attribute on which individuals self-select into trading. An important lesson from our results is that low self-control traders might not be as easily exploitable by high self-control traders as one would think. In our case, low self-control traders were not driven out of the market quickly, as they did not suffer negative consequences from their behavior over our trading periods. Our experiment was not designed to provide evidence on the exact mechanism that leads to the convergence of trading strategies of ego-depleted and nondepleted traders. Unveiling this mechanism would be important, but the endogeneity in the market environment makes this a complicated problem. In any case, as our results suggest that nondepleted traders behave suboptimally in the presence of depleted traders, it is in the interest of market organizers to keep low the share of low self-control traders. Mechanisms designed to filter out traders low in self-control and limiting their market access could lower aggregate inefficiencies.

Certainly, our findings and policy implications arise in a highly stylized environment, which is needed to isolate the causal effect of low self-control on trading behavior. While real markets may have distinguishing features that could influence the effect of self-control in both directions—suppression and exacerbation—they also often bear features common to our setting, such as the double auction market mechanism, or the decreasing fundamental asset value, which is also a feature of a large class of assets, such as options, bonds, and depletable resources. Thus, we think that the effects of low self-control are relevant in many (but certainly not all) real-world markets unless other mitigating factors are present.

Our experiment opens up relevant 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). This could generate insights into the robustness of our discoveries in other settings that are also relevant in practice. Furthermore, our setting cannot explain why nondepleted subjects adjust their behavior and are soon indistinguishable statistically from nondepleted subjects. As already noted, it would be interesting to know if our results carried through in settings in which this is not the case and under which circumstances or prerequisites imitation of strategies occurs. Moreover, we discuss welfare implications of our setting, but our experimental market does not explicitly allow for welfare variation (in the sense of being a positive-sum game). We did not implement experiments that incorporate such a feature, because doing so would have deviated from the relevant experimental literature on price bubbles and would have made comparisons much more difficult. However, it would be interesting to study the effects of self-control depletion on behavior in positive-sum settings. Finally, the self-control of traders outside the laboratory remains largely unexplored. Future research could develop field experiments or use quasi-experimental variations of self-control to study decisions of traders in real markets.

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