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19 September 2017

Does self-control depletion affect risk attitudes?*

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Accepted for publication in the *European Economic Review*
DOI: 10.1016/j.euroecorev.2017.09.004

September 19, 2017

Abstract

A core prediction of recent "dual-self" models is that risk attitudes depend on self-control. While these models have received a lot of attention, empirical evidence regarding their predictions is lacking. We derive hypotheses from three prominent models for choices between risky monetary payoffs under regular and reduced self-control. We test the hypotheses in a lab experiment, using a well-established ego depletion task to reduce self-control, and measuring risk attitudes via finely graduated choice lists. Manipulation checks document the effectiveness of the depletion task. We find no systematic evidence in favor of the theoretical predictions. In particular, depletion does not increase risk aversion.

Keywords: Risk attitudes, Self-control, Ego depletion, Dual-self models, Experiment

JEL Codes: D03, D81, C91

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*We thank Carlos Alós-Ferrer, Simon Dato, Thomas Dohmen, Armin Falk, Drew Fudenberg, Wilhelm Hofmann, George Loewenstein, Sebastian Schaube, Martin Strobel, Matthias Sutter, and Joël van der Weele, as well as participants of the IMEBESS 2015 conference, the "Motivation, Self-Control, and Economic Behavior" seminar at the University of Cologne, and the M-BEES 2015 conference for helpful comments and discussion. We also thank four anonymous reviewers and the Associate Editor for valuable comments that helped to substantially improve the manuscript. Financial support by the German Research Foundation (Deutsche Forschungsgemeinschaft, DFG) through the Collaborative Research Center SFB/TR 15 and the Bonn Graduate School of Economics (BGSE) is gratefully acknowledged. We declare no competing financial interests.

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1. Introduction

A decision maker's attitude toward risk is a core component of her "economic personality". Risk preferences are an integral part of theoretical models in virtually all domains of economics, and empirical evidence documents that risk attitudes are an important predictor of both economic and health outcomes. For instance, a higher willingness to take risks is positively correlated with being self-employed, investing in stocks, and not having insurance, as well as being a smoker, drinking heavily, and being overweight (Anderson and Mellor, 2008; Barsky et al., 1997; Dohmen et al., 2011; Kimball et al., 2008).

Given the central role of risk attitudes in economic theory and their predictive power for individual behavior, a better understanding of factors that potentially influence risk attitudes is of great importance to economists. Inspired by the difficulty of expected-utility theory to explain empirical phenomena like the Allais paradox or small-stakes risk aversion, various recently developed models build on insights from psychology and posit that risk attitudes are shaped by the interaction of "dual systems" (a deliberative and an affective system, respectively; Loewenstein and O'Donoghue, 2005; Mukherjee, 2010) or of "dual selves" (a long-run and a short-run self; Fudenberg and Levine, 2006, 2011, 2012). In this framework, "self-control" amounts to the long-run self imposing restrictions on the short-run self. Consequently, a crucial determinant of a decision maker's risk attitude is her current level of self-control resources. In particular, the prominent Fudenberg–Levine model predicts that lower levels of self-control induce stronger risk aversion for stakes within a particular range.

In this paper, we derive three explicit hypotheses on the relationship between self-control and risk preferences, using the model by Fudenberg et al. (2014), a version of the Fudenberg–Levine model that is particularly well-suited to address decision making under risk in the case of pairwise lottery choice. The hypotheses refer to choices among pairs of two-outcome lotteries, choices among a safe payoff and two-outcome lotteries (all paid out immediately), and to choices among pairs of two-outcome lotteries that will only be paid out with a delay. We adopt a fourth hypothesis directly from Loewenstein and O'Donoghue (2005); their model predicts that self-control depletion leads to more pronounced probability weighting (p. 28). From the similar dual-self model by Mukherjee (2010) we derive a set of alternative predictions. We test these hypotheses in a laboratory experiment.

The purpose of the experiment is to provide causal evidence on the link between self-control and risk preferences. We exogenously manipulate the level of self-control between subjects using ego depletion, a concept from psychology (Baumeister et al., 1998). In doing so, we also provide sound empirical evidence regarding the effect of ego depletion on risk attitudes.

Our experiment uses a between-subject design with two conditions. At the beginning, subjects in the treatment group perform a so-called ego depletion task that is well-established in the literature and has been found to induce low self-control in numerous studies (see the meta-analysis by Hagger et al., 2010). Depletion tasks are based on the notion that the exertion of self-control in one activity consumes self-control resources, thereby increasing self-control costs in subsequent activities (Baumeister et al., 1998). The control group performs a similar, though nondepleting task, i.e., a task that does not reduce self-control resources.

Immediately following the respective task, we obtain precise measures of subjects' risk attitudes. Our measures are based on finely graduated choice lists, one for each of the four hypotheses derived from Fudenberg et al. (2014) and Loewenstein and O'Donoghue (2005); they also allow for testing the alternative predictions based on Mukherjee (2010). Each row of the choice lists consists of a choice between two two-outcome lotteries. Inspired by Eeckhoudt and Schlesinger (2006) and Ebert and Wiesen (2014), we chose one lottery to be a mean-preserving spread of the other, with a sure payoff (a risk premium) being added or subtracted. A noteworthy feature of this method is that it allows quantifying subjects' risk attitudes without assuming a specific utility function. This is particularly important in our case, since the Fudenberg–Levine model contains several functions of unknown parametric form as well as unobservable, difficult-to-estimate quantities.

Contrary to the predictions that we derive from the Fudenberg–Levine model, we do not find any evidence for increased risk aversion after ego depletion. For all of our four choice lists, subjects in the depletion group even exhibit a nonsignificant tendency toward less risk-averse choices, compared to the control group. Also evidence in favor of the fourth hypothesis (taken from Loewenstein and O'Donoghue, 2005) that reduced self-control leads to more pronounced probability weighting is limited at best. Neither do we find support for the alternative predictions derived from the model by Mukherjee (2010).

We do not observe that subjects behave in a more random manner under depletion. Depleted subjects also do not decide more quickly, as one would expect if they relied on heuristics to a stronger extent. Finally, self-control as a character trait (as opposed to the temporary level of self-control resources) does not explain heterogeneity of risk attitudes across individuals.

Overall, we deem our empirical results on the apparently weak link between self-control and risk attitudes informative for the future modeling of decision making under risk. In principle, we have no doubt that economics can benefit from incorporating psychological concepts in general and self-control in particular. Just as much, we acknowledge the potential of dual-self models to explain behavior in neighboring areas like intertemporal choice and economic theories of addiction. However, different levels of self-control do not seem to influence risk attitudes strongly—and if they do, the influence is primarily in the opposite direction of the prediction of the most prominent applicable model. This casts doubt on the "unified explanation" offered by Fudenberg and Levine (2006).

Taking a broader perspective, our paper adds to a recently emerging field of research that investigates whether aspects of the decision environment that go beyond incentives and constraints—such as self-control, cognitive load, emotions, or stress—influence decision making under risk.[1] A common feature of this line of research is that it challenges the standard assumption of stable preferences (which has shaped economics since Stigler and Becker, 1977). Our results provide evidence that self-control does not belong to the aspects of the decision environment that induce large variations in risk preferences; hence, the standard view of stable preferences may be adequate at least with regard to risk preferences and self-control.

Related literature

Traditionally, economics has modeled decision makers without any reference to psychological concepts like "self-control". However, in some cases, the standard models of economic choice—expected-utility theory and the discounted-utility model—have difficulties explaining observed behavior both in the field and in the laboratory. To remedy these problems, numerous theoretical models have been developed recently which capture the notion that some economic decisions may involve a competition between conflicting motives. Resolution of the conflict depends on the use of "self-control" (e.g., Gul and Pesendorfer, 2007; Dekel et al., 2009).

In particular, models involving "multiple selves" or "multiple systems" have become increasingly popular in economics. These "selves" or "systems" are either conceived of as diverging motives held by a decision maker at different points in time (e.g., Laibson, 1997; Diamond and Kőszegi, 2003; Heidhues and Kőszegi, 2009) or as conflicting motives that are present in a decision maker simultaneously (e.g., Loewenstein and O'Donoghue, 2005; Brocas and Carrillo, 2008; Fudenberg and Levine, 2006, 2011, 2012; Fudenberg et al., 2014). While the most common application of these models is temporal discounting, the dual-self model by Fudenberg and Levine (2006, 2011) as well as the "dual-system" models by Loewenstein and O'Donoghue (2005) and Mukherjee (2010) also explicitly address decision making under risk.

A particular strength of the model by Fudenberg and Levine is that it offers a "unified explanation" (Fudenberg and Levine, 2006, p. 1449) for several commonly observed discounting-related phenomena such as time inconsistency as well as risk-related phenomena such as the Rabin paradox[2] (Rabin, 2000) and the Allais paradox (Allais, 1953). More specifically, a core prediction of the Fudenberg–Levine model is that lower levels of self-control induce more risk-averse behavior for

[1] For instance, the results of Cohn et al. (2015), Guiso et al. (2014), Schulreich et al. (2014), and Schulreich et al. (2016) are based on emotional priming and suggest that sadness and fear induce stronger risk aversion. By contrast, the results of Conte et al. (2016) indicate that sadness, fear, anger, and joviality induce risk-seeking behavior. Benjamin et al. (2013), Deck and Jahedi (2015), and Gerhardt et al. (2016) find that cognitive load increases risk aversion. Concerning stress, Kandasamy et al. (2014) find that induced stress increases risk-averse behavior, while Buckert et al. (2014) observe stronger risk proclivity for gains, however only for a relatively small subgroup of participants.

[2] This paradox refers to the observation that the levels of small-stakes risk aversion observed in laboratory experiments are too high to be reconciled with behavior for higher stakes when assuming that decision makers care only about final wealth.

stakes within a specific range. However, empirical evidence on this particular relationship between self-control and risk attitudes is scarce. This paper aims at providing the first direct test of a central prediction of the Fudenberg–Levine model.

Fudenberg and Levine (2006, p. 1467), Fudenberg et al. (2014, p. 66), and especially Fudenberg and Levine (2012, p. 3) motivate characteristics of their dual-self model by referring to the so-called "strength model" of self-control. This model was introduced to the psychology literature by Baumeister et al. (1998). The strength model is based on the idea that exerting self-control consumes self-control resources that can be depleted. As a consequence, use of self-control in one task reduces the availability of self-control resources in a subsequent task. This process is referred to as "self-control depletion", "willpower depletion", or "ego depletion" (in analogy to the Freudian *ego* that controls the *id*). The strength model has also found its way into the economics literature. Not only does it serve as the basis of the models by Fudenberg and Levine (2012) and Ozdenoren et al. (2012); it is also part of the motivation of the analysis of resource allocation in the human brain by Alonso et al. (2014).

The foundations and implications of the strength model of self-control have been empirically investigated by both psychologists and economists numerous times (see Hagger et al., 2010, Carter and McCullough, 2014, Hagger et al., 2016, for extensive overviews and meta-analyses; see Bucciol et al., 2011, 2013, for economic applications). Yet, regarding the link between ego depletion and risk attitudes, the existing evidence is scarce and inconclusive. Moreover, none of the existing papers is tailored to testing the predictions of the Fudenberg–Levine dual-self model or the models by Loewenstein and O'Donoghue (2005) and Mukherjee (2010). Unger and Stahlberg (2011) find that depleted subjects make more risk-averse decisions, based on the results of a strongly framed investment experiment. Since Unger and Stahlberg instructed subjects to imagine that they were managers making a decision on behalf of their firm, subjects' decisions in this experiment do not necessarily reflect only their own individual risk preferences. Measuring risk attitudes via choice lists, but with a total sample size of only $N = 54$ in a between-subject design, Stojić et al. (2013) find that subjects tend to be more risk-averse under ego depletion—however, not significantly so. By contrast, Friehe and Schildberg-Hörisch (2017) find that depleted subjects tend to be less risk-averse than nondepleted subjects. Their measure of risk attitudes, however, only captures risk-averse up to risk-neutral behavior and does not cover the domain of risk proclivity.[3]

[3] There are some additional, less closely related studies. Combining prior losses and ego depletion in a single treatment, Kostek and Ashrafioun (2014) find a higher degree of risk aversion. In contrast, two psychological studies (Bruyneel et al., 2009; Freeman and Muraven, 2010) find increased "risk taking" under ego depletion. These use, however, either (unincentivized) vignettes or tasks with unknown probabilities, such that subjects decided under ambiguity instead of risk. De Haan and van Veldhuizen (2015) used, by contrast, incentivized, risky gambles. They also observe a reduction of risk aversion after depletion. However, the observed effect is not only small but also present in just one out of their three experiments, and de Haan and van Veldhuizen cannot rule out that it was caused by depleted subjects choosing more randomly (p. 59).

The experimental method perhaps most closely related to willpower depletion is putting subjects under concurrent cognitive load while they make decisions. Cognitive load usually takes on the form of working-memory load. So far, three studies have investigated the relationship between individual risk attitudes and cognitive load: in Benjamin et al. (2013) and in Deck and Jahedi (2015), the working-memory load manipulation was remembering a 7-digit number, while it was remembering a spatial arrangement of dots in Gerhardt et al. (2016). All three studies consistently find a significant increase in risk aversion due to cognitive load. At first glance, these findings seem to contradict the findings of our study, but at closer inspection they do not.

While closely related, willpower depletion and cognitive load are not identical. Baumeister and Vohs (2016b, p. 70) see the crucial difference in that ego depletion targets *self-regulation*, while cognitive load affects *attention*. This view is supported by the results of Maranges et al. (2017). A similar distinction is made by Kahneman (2011, p. 43): "Ego depletion is not the same mental state as cognitive busyness." He posits that "unlike cognitive load, ego depletion is at least in part a loss of motivation" (pp. 42/43). If one wanted to frame it in terms of dual-system thinking, ego depletion could be interpreted as shifting the balance of power between the affective "System 1" and the deliberative "System 2" in favor of "System 1", while cognitive load rather seems to influence the *contents* of "System 2". Hence, it is not clear that the two manipulations should have the same effect.

Moreover, as Gerhardt et al. (2016, p. 27) note, the stake sizes in their study are so low that the Fudenberg–Levine model is unlikely to predict any effect. Thus, it is unlikely that the particular channel envisioned by Fudenberg and Levine (2006, 2011) can account for the observed increase in risk attitudes caused by cognitive load. From the point of view of Gerhardt et al. (2016), cognitive load probably influences risk attitudes through a different channel.

Our study goes beyond the existing literature in that it tests the role of self-control guided by the theoretical frameworks of Fudenberg and Levine (2006), Loewenstein and O'Donoghue (2005), and Mukherjee (2010). Additionally, we provide particularly clean evidence regarding the effect of ego depletion on risk attitudes. For this purpose, several aspects of the design of our experiment are crucial. We use (*i*) incentivized choices, (*ii*) ego depletion is the only manipulation, and (*iii*) all probabilities associated with the payoffs are known to subjects. Our risk measure (*iv*) covers the entire domain of possible risk attitudes and (*v*) enables us to detect even small effect sizes. (*vi*) We take restrictions on the magnitude of the involved payoffs, as they follow from the Fudenberg–Levine model, into account.

Moreover, we use several survey and behavioral responses of our subjects to provide an independent manipulation check, showing that subjects in the treatment group were more depleted than subjects in the control group.

Finally, our sample size ($N = 308$) yields sufficient statistical power to document relevant effect sizes. The average effect size (Cohen's d) is $d = 0.62$ in the meta-analysis by Hagger et al. (2010) that is based on a total of 83 papers containing 198 independent studies. Carter and McCullough (2014) reevaluate the same ego depletion literature. They find evidence for small-study effects which, when controlled for, lead to lower estimates of the average effect size. In order not to fall prey to this issue, our study features a comparatively large sample size ($N = 308$; this exceeds the sample size of all but one of the 198 studies covered by Hagger et al., 2010). Given our large number of observations, a power analysis shows that, using a t-test and a significance level $\alpha = 0.05$, we are able to detect, for each choice list separately, an effect size as small as $d = 0.32$ at the conventional level of power of 80% or above.

The rest of the paper is structured as follows. Section 2 presents the Fudenberg–Levine model (2.1) and the hypotheses that we derive from the model regarding the impact of reduced self-control on risk attitudes (2.2) as well as the hypotheses based on Loewenstein and O'Donoghue (2005) (2.3) and Mukherjee (2010) (2.4). Section 3 describes the design and procedural details of our laboratory experiment. Section 4 presents the results. Section 5 discusses our findings and concludes.

2. Theory and hypotheses

In the psychology literature, it has been argued that depletion induces an increased propensity to engage in risk-seeking behavior (Freeman and Muraven, 2010). The dual-self model by Fudenberg and Levine (2006, 2011) makes the opposite prediction: we should typically observe more pronounced risk aversion under depletion. Fudenberg et al. (2014) explicitly model self-control as a determinant of choices between lotteries. Thus, their model allows us to derive precise hypotheses regarding the influence of ego depletion on pairwise lottery choice between two-outcome lotteries (as we use in our experiment).

2.1. Overview of the model by Fudenberg et al. (2014)

In all variants of the Fudenberg–Levine model, decision making is the outcome of the interaction of a short-run and a long-run self. One might think of the interaction between the two selves as that of a "planner" (the long-run self) and a "doer" (the short-run self), a terminology introduced by Thaler and Shefrin (1981). Both "selves" have the same per-period utility function, which is assumed to be monotonically increasing and concave. They differ, however, in the way they regard the future. The short-run self is completely myopic, i.e., it cares only about same-period consumption.[4] Consequently, it prefers to spend all available income immediately. Having a concave per-period utility function, the short-run self is risk-averse. The long-run self, in contrast, also derives utility from consumption in future periods and discounts them exponentially. Combined with its concave per-period utility function, this creates a preference for smoothing consumption over time. As a consequence of spreading consumption over a large number of pe-

[4] In Fudenberg and Levine (2012), the authors allow for an only partially myopic short-run self.

riods, the long-run self is (very close to) risk-neutral (for a formal derivation, see Fudenberg and Levine, 2011, p. 44).

The short-run self's preference for immediate consumption and the long-run self's consumption-smoothing motive generate a conflict of interest. By exerting self-control, the long-run self can restrict the short-run self to a consumption level below the latter's desired consumption level. Importantly, in the model by Fudenberg and Levine, the described conflict of interest only arises for *unanticipated* income. *Anticipated* income does not create a need to exert self-control: based on foreseeable income, the long-run self allocates a budget to the short-run self of each period, and the short-run selves spend exactly that budget.

Exertion of self-control when deciding over how to spend unanticipated income is assumed to be costly. This cost increases in the difference between the short-run self's utility derived from the consumption that the long-run self "permits" and the short-run self's preferred course of action, i.e., spending the entire period income immediately. To fit "the psychological evidence that self-control is a limited resource" as well as to explain the Allais paradox, the self-control cost function has to be convex, as Fudenberg and Levine (2006, p. 1467; 2011; 2012, pp. 3, 16) argue.

Fudenberg et al. (2014) develop a version of the Fudenberg–Levine model that improves the model's applicability to decision making under risk. Their main simplifying assumption is linearity of the long-run value function. This means that the marginal utility of saving is constant, such that the long-run self is completely risk-neutral (instead of being only very close to risk-neutral). In Section A, we use this version of the Fudenberg–Levine dual-self model to formally derive Hypotheses 1, 2, and 3. In the following, we present the hypotheses and explain the intuition behind them.

2.2. Hypotheses derived from Fudenberg et al. (2014)

Hypothesis 1. *Ego depletion leads to greater risk aversion for choices between lotteries if at least one of the lotteries contains a small payoff below and another larger payoff above a cutoff value $\hat{z}$.*

$\hat{z}$ denotes a threshold such that monetary lottery payoffs below $\hat{z}$ are spent completely, while any part of a payoff that exceeds $\hat{z}$ is saved for future consumption. The threshold $\hat{z}$ is endogenously determined by the interplay of the long-run self and the short-run self. It depends on the lottery under consideration, the menu of lotteries as well as the marginal cost of self-control. Therefore, ego depletion—which increases the marginal cost of self-control if the cost function is convex—shifts the balance of power in favor of the risk-averse short-run self, resulting in an increase in the degree of risk aversion expressed by the lottery choice. This is due to two effects: First, for a given $\hat{z}$ and a lottery with one payoff below and one payoff above $\hat{z}$, the relative contributions of the short-run self's and the long-run self's utility to the expected utility of this lottery change, with the effect that the combined preferences exhibit increased risk aversion (see Section A). Second, the threshold $\hat{z}$ increases. As a consequence, there are decisions which the short-run self is entirely in charge of under depletion even though the long-run self would have exerted self-control under nondepletion.

Hypothesis 2. *The effect of ego depletion (i.e., increased risk aversion) is stronger when one "lottery" is a sure payoff.*

When the per-period utility function is concave, a sure payoff leads to higher utility than a lottery with the same expected value. Consequently, self-control costs are higher in case the long-run self actually exerts control over the short-run self. Compared to a decision among two two-outcome lotteries, this amplifies the increase in risk aversion due to ego depletion (see also Fudenberg and Levine, 2011, pp. 35, 46, 66).

Hypothesis 3. *When payoffs are delayed, ego depletion has no effect.*

In case we observe the effects of ego depletion that we predict in Hypotheses 1 and 2, these need not necessarily be caused by a decrease in self-control resources. Other channels—for instance, a change in the propensity to rely on heuristics—could generate the same effects. Our third hypothesis thus serves to distinguish an influence of self-control from other possible explanations.

For this purpose, we exploit a particular feature of the dual-self model, namely that the short-run self cares only about the current period. Although Fudenberg and Levine (2006) do not specify the length of one period—i.e., the time horizon for one short-run self—it should not exceed a few days: "the horizon of the short-run self is on the order of a day to a week" (Fudenberg and Levine, 2011, p. 39). Thus, when both lotteries exclusively feature payoffs that occur in the future—i.e., beyond the short-run self's time horizon—self-control does not affect decisions. Therefore, self-control costs or an increase in self-control costs will not make a difference for risk attitudes over future payoffs.[5] If, however, ego depletion affected risk attitudes through the increased use of heuristics, this would also be the case for choices concerning the future. Thus, according to this alternative hypothesis, we would find the same change in risk aversion when payoffs are delayed as when they are immediate.

2.3. Hypothesis derived from Loewenstein and O'Donoghue (2005)

Hypothesis 4. *For a long shot, ego depletion leads to a lower degree of risk aversion.*

A long shot is a lottery that offers a low probability of obtaining a high payoff and a high probability of obtaining a low payoff. These lotteries are sometimes

[5] See also Fudenberg and Levine (2011, p. 48) for the implication that Allais-type paradoxes disappear "if the results of gambles are delayed long-enough that they fall outside the time horizon of the short-run self."

also referred to as "dollar bets". Hypothesis 4 is based on the idea that the decision maker overweights small probabilities and that this distortion becomes more pronounced under ego depletion. Overweighting the small probability of winning a large amount makes a long shot subjectively attractive despite its being relatively risky. A stronger distortion of the small probability in the direction of ½ under ego depletion should thus make risk-averse decision makers less risk-averse, and risk-seeking decision makers more risk-seeking.

Hypothesis 4 is a direct implication of Loewenstein and O'Donoghue (2005, p. 28). It deviates from the Fudenberg–Levine model in that Fudenberg and Levine assume the absence of probability weighting and a strictly risk-averse short-run self. The background of this hypothesis is empirical evidence that many subjects exhibit risk proclivity for long shots (Harbaugh et al., 2010). A common explanation for this phenomenon is probability weighting, in particular overweighting of small probabilities that are associated with large payoffs (as modeled by cumulative prospect theory, Tversky and Kahneman, 1992). Probability weighting is modeled explicitly as the outcome of the interaction of a deliberative and an affective system by Loewenstein and O'Donoghue (2005). In their model, the preferences of the deliberative system can be represented by expected-utility maximization, i.e., the deliberative system takes probabilities at face value. In contrast, the affective system assigns identical weight to all possible outcomes (i.e., ½ in the case of two-outcome lotteries) instead of using the true probabilities. The interplay of both systems then results in an inverse-S-shaped probability weighting function. Loewenstein and O'Donoghue (2005, p. 28) explicitly state that "if a person's willpower is depleted ..., then she should exhibit a more [inverse-]S-shaped probability weighting function". Thus, for long shots, we expect reduced risk aversion or increased risk proclivity, respectively, under depletion, because attaching a higher probability weight to the large payoff makes picking the long shot more attractive.

2.4. Hypotheses derived from Mukherjee (2010)

Hypothesis 1 b. (Alternative to Hypothesis 1.) *Ego depletion leads to increased risk aversion when choosing between 50%-50% lotteries.*

Hypothesis 2 b. (Alternative to Hypothesis 2.) *Ego depletion leads to increased risk aversion when choosing between a 50%-50% lottery and a sure payoff.*

Hypothesis 3 b. (Alternative to Hypothesis 3.) *Ego depletion also leads to increased risk aversion when choosing between two 50%-50% lotteries for which the payoffs are delayed.*

Assuming that the strength of the affective system relative to the deliberative system depends on self-control resources, we can also derive predictions for our experiment from the model by Mukherjee (2010). According to this model, the affective system replaces all original nonzero probabilities by a weight of $1/n$ (just like in the model by Loewenstein and O'Donoghue, 2005), and it combines these with a concave value function (e.g., $v_A(x) = \text{sgn}(m)\, x^m$ with $m < 1$). Note that the combination of both effects can generate risk-averse as well as risk-neutral or risk-seeking behavior. The deliberative system, by contrast, uses the correct probabilities and combines them with a linear value function, i.e., it exhibits risk neutrality. Total valuation is given by a weighted sum of the separate valuations by the affective and the deliberative system.

In combining the affective with the deliberative valuation, a parameter $\gamma \in [0,1]$ determines the relative strength of the affective system. As a consequence, a decrease in willpower—i.e., an increase in γ—affects risk attitudes through two simultaneous effects: a shift in probability weighting (in the direction of completely uniform $1/n$ weighting) and a shift in the evaluation of the outcomes (in the direction of more strongly concave valuation).

This entails that the prediction regarding a weakening of willpower is straightforward for equal-probability gambles: for a 50%-50% two-outcome gamble, the $1/n$ weighting is exactly correct, so that both systems use the same, correct weighting of probabilities. At the same time, the depletion-induced shift away from the linear toward the concave value function leads to *increased* risk aversion. Hence, the model by Mukherjee (2010) predicts the alternative Hypotheses 1 b, 2 b, and 3 b.

Hypothesis 4 b. (Alternative to Hypothesis 4.) *For a long shot, there is greater variance of observed risk attitudes in the depleted group than in the nondepleted group of subjects.*

The model's prediction for the "long shot" is a bit more involved. Since a decrease in willpower entails the two effects described above that potentially go in opposite directions, the total effect depends on two factors: the exact probabilities of the outcomes and the exact curvature of the affective system's value function (see Table 2 of Mukherjee, 2010, for an illustration). More precisely, depending on the probabilities and outcomes, there is a curvature parameter m^* such that for $m < m^*$, the affective system exhibits risk aversion, and an increase in γ leads to greater risk aversion of the total valuation. Conversely, for $m > m^*$, the affective system exhibits risk proclivity, and an increase in γ leads to greater risk proclivity of the total valuation. For $m = m^*$, both systems are risk-neutral, such that γ has no effect.

Hence, subjects who are risk-averse for a long shot in a nondepleted state should become more risk-averse when depleted, while subjects who are risk-loving when not depleted should become even more risk-loving when depleted. Finally, depletion should have no effect for risk-neutral subjects. Assuming that there is heterogeneity in subject's baseline risk aversion, we should thus observe a greater variance of risk attitudes in the depleted group. This leads to Hypothesis 4 b as an alternative to Hypothesis 4.

3. Experiment

3.1. General setup

Our objective is to test whether there is a causal effect of the current level of self-control on risk attitudes. We employ a between-subject design with two groups and exogenously vary the level of self-control using an ego depletion task.[6] More specifically, subjects in the depletion and in the control group work on different versions of a task that bring about different levels of self-control capacity. Subsequently, we measure subjects' risk attitudes via incentivized choices between lotteries.

3.2. Depletion task

In our experiment, the depletion task serves as the source of exogenous variation between subjects. Being such a vital part of the experiment, we required it to be both well established and as effective as possible in inducing low self-control. The task of our choice, the crossing-out letters task, meets both criteria and is also easily implementable in the lab. According to the meta-analysis of Hagger et al. (2010) the crossing-out letters task is the most effective of all ego depletion tasks. It has been used successfully to induce changes in outcomes like persistence in watching a boring movie, resistance to persuasion, advice on risk taking given to others in a vignette-style questionnaire, and offers made in a dictator or ultimatum game (Baumeister et al., 1998; Wheeler et al., 2007; Freeman and Muraven, 2010; Achtziger et al., 2015, 2016, respectively).

In the depletion group, the task works as follows. Subjects are first given a printed text spanning 22 lines and are asked to cross out all instances of the letter "e" (including the uppercase letter "E"). Subjects work on this task for three minutes. Immediately afterwards, subjects in the depletion group are given a different text spanning 44 lines. This time they are asked to cross out all instances of the letter "e" except when there is a vowel right after the "e" or two letters away (in either direction). Subjects work on this second part of the task for seven minutes. The rationale why this task depletes self-control is that it requires the constant cognitive suppression of an automatic impulse—the impulse to cross out the letter "e" that was built up in the first part of the task.

The task assigned to the control group also follows the standard of the literature. Subjects work on the same texts as the depletion group for the same duration but are only required to cross out all "e"s, without any additional rule, in both parts. Hence, there is no self-control–consuming impulse suppression in the control group.

[6] A within-subject design would have had the advantage of providing us with a baseline measure of risk attitudes at the individual instead of group level. However, we would have needed to present subjects the same lottery choices before and after the self-control manipulation. This would have been a severe drawback because subjects are likely to remember their earlier choices. Paired with a preference for consistency (Falk and Zimmermann, 2017), recalling previous choices might counteract any depletion effect.

We chose texts that we expected to be irrelevant and uninteresting to most subjects. The texts were based on the appendix of a statistics text book (Bamberg and Baur, 2001) and describe criteria for the choice of statistics software in a very general way. We provide the exact texts of the depletion task in Online Appendix C.

We deliberately chose not to pay subjects for this task because there is evidence that receiving payment for a task counteracts ego depletion (Muraven and Slessareva, 2003). In addition to announcing private feedback (to be provided at the end of the experiment), the instructions asked subjects to work on the task conscientiously. The data show that the vast majority of subjects did.[7]

3.3. Measure of risk attitudes

We used the following criteria to choose the method for quantifying subjects' risk attitudes:

- It does not require assuming a specific utility function or choice model.[8]
- Lotteries of various types, including long shots (lotteries with a low probability of winning a high prize) and safe choices (degenerate lotteries), need to be implementable.
- It has to allow for the measurement of risk aversion, risk neutrality, and risk proclivity—ideally in a single decision situation.
- It should provide a fine measure of risk attitudes to enable us to detect small effect sizes.

Following these criteria, we chose a measure using two-outcome lotteries and mean-preserving spreads of these lotteries. Our method was inspired by Ebert and Wiesen (2011, 2014) whose experimental measures are based on the model-independent concept of risk apportionment (Eeckhoudt and Schlesinger, 2006). Ebert and Wiesen (2014) classify an individual as risk-averse if she prefers a lottery $L = (c_{L,1}, p_{L,1}; c_{L,2}, p_{L,2}) = (x - r, 50\%; x - k, 50\%)$ over the lottery $M = (c_{M,1}, p_{M,1}; c_{M,2}, p_{M,2}) = (x - r - k, 50\%; x, 50\%)$, where x, r, and k are monetary payoffs. Note that this coincides with preferring a lottery to a mean-preserving spread of that lottery. In case the individual prefers M over L, she is classified as risk-seeking. In general—i.e., with arbitrary probabilities $p_{L,1}, p_{L,2}$—Lottery M is constructed by setting $c_{M,1} = x - (p_{L,2}/p_{L,1})\, r - k$. This is needed for constructing the mean-preserving spread of the long shot.

[7] For the first paragraph of the first part of the task, 85% of subjects reported the correct value or a value within the 10% interval around the correct value (typically below the correct value). This task was the same for both groups. For the first paragraph of the second task, performance was comparable for the control group (91% of subjects reported values inside the 10% interval around the correct value), but inferior for the treatment group who had a more difficult task to fulfill (only 56% of subjects stated values inside the 10% interval around the true value).

[8] We deliberately did not aim at designing an experiment that enables us to estimate the models' parameters. Structural estimation requires simultaneous estimation of several of the models' parameters. Any such estimation would need to rely on strong assumptions regarding functional forms and various unobservable quantities.

Table 1
Overview of the choice lists presented to subjects.

	Alternative A				Alternative B			
	$c_{A,1}$	$p_{A,1}$	$c_{A,2}$	$p_{A,2}$	$c_{B,1}$	$p_{B,1}$	$c_{B,2}$	$p_{B,2}$
Choice List A: risky/risky (x = €22.00, r = €7.50, k = €11.50; 25 rows)								
Top row	€3.00	50%	€22.00	50%	€3.00	50%	€7.00	50%
Center row	€3.00	50%	€22.00	50%	€9.00	50%	€13.00	50%
Row with $m = 0$	€3.00	50%	€22.00	50%	€10.50	50%	€14.50	50%
Bottom row	€3.00	50%	€22.00	50%	€15.00	50%	€19.00	50%
Choice List B: safe/risky (x = €16.00, r = €5.00, k = €5.00; 19 rows)								
Top row	€11.00	100%			€11.00	50%	€21.00	50%
Center row	€11.00	100%			€6.50	50%	€16.50	50%
Row with $m = 0$	€11.00	100%			€6.00	50%	€16.00	50%
Bottom row	€11.00	100%			€2.00	50%	€12.00	50%
Choice List C: "long shot" (x = €14.00, r = −€36.00, k = €7.00; 21 rows)								
Top row	€7.00	90%	€50.00	10%	€7.00	90%	€10.00	10%
Row with $m = 0$	€7.00	90%	€50.00	10%	€11.00	90%	€14.00	10%
Center row	€7.00	90%	€50.00	10%	€12.00	90%	€15.00	10%
Bottom row	€7.00	90%	€50.00	10%	€17.00	90%	€20.00	10%
Choice List D: delayed payoffs (x = €18.00, r = €6.00, k = €8.50, paid in one week; 20 rows)								
Top row	€9.50	50%	€12.00	50%	€9.50	50%	€24.00	50%
Above-center row	€9.50	50%	€12.00	50%	€5.00	50%	€19.50	50%
Below-center row	€9.50	50%	€12.00	50%	€4.50	50%	€19.00	50%
Row with $m = 0$	€9.50	50%	€12.00	50%	€3.50	50%	€18.00	50%
Bottom row	€9.50	50%	€12.00	50%	€0.00	50%	€14.50	50%

To measure the intensity of a subject's risk attitude, we determine the monetary amount m (compensation or "risk premium") that is needed to make her indifferent between the lotteries L and $M + m$. To this purpose, we use a choice list format, as introduced by Holt and Laury (2002). The switching row in the choice list delivers a proxy of the indifference-generating risk premium $m_{\sim}(L, M)$. If $m_{\sim}(L, M) > 0$, the decision maker exhibits risk aversion for that particular lottery pair; conversely, $m_{\sim}(L, M) < 0$ indicates risk proclivity.

Table 1 provides an overview of all four choice lists (one per hypothesis) that we used, in the order in which they were presented to subjects: Choice List A is designed to address Hypothesis 1, while Choice List B relates to Hypothesis 2, Choice List C to Hypothesis 4, and Choice List D to Hypothesis 3.

We decided *not* to randomize the order of the choice lists since the most basic hypothesis, Hypothesis 1, is addressed by Choice List A. We had all subjects complete this choice list first to ensure that even if depletion effects fade out over a time as short as a minute, they should be present consistently when testing our most basic hypothesis.

In the instructions we referred to the choice lists as "tables". A sample screenshot displaying the exact representation that subjects saw is included in Fig. 1.

Each choice list starts from a first-order stochastically dominated choice and spans risk aversion, risk neutrality, and risk proclivity. To make the decisions easy for subjects to grasp, probabilities remain the same within a given choice list. Moreover, in all choice lists, the left lottery stays constant, while the right lottery's payoff changes in steps of €0.50 per row. Additionally, the expected value of "Alternative *A*" is similar (between €10.75 and €12.50) for all four choice lists.

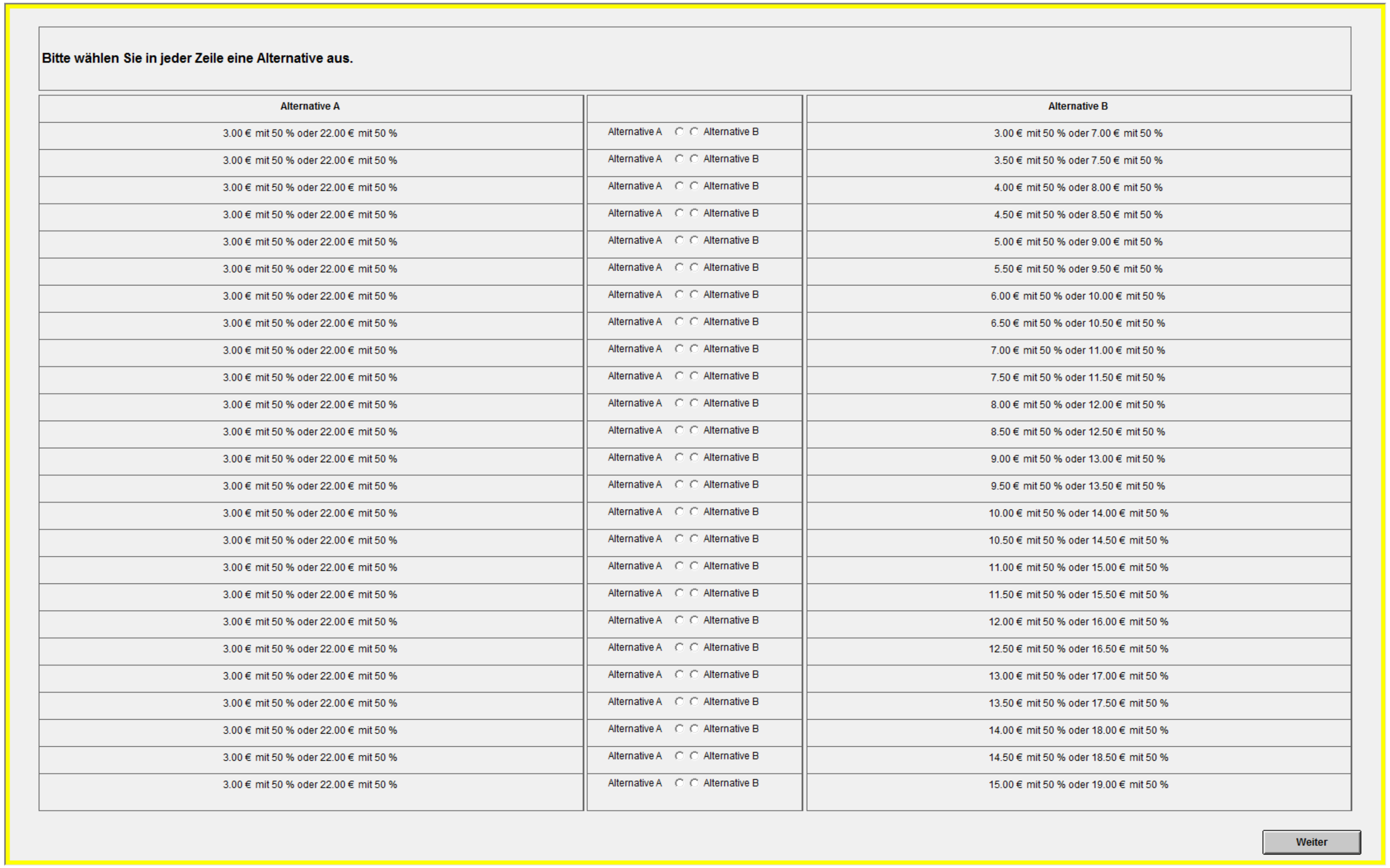
To address a recent criticism of choice-list–based measurement of risk attitudes by Andersson et al. (2016), we put the expected median switching row in the control condition to the center of each list.[9]

Moreover, we balanced the exposition of our choice lists: in two of our choice lists, the dominated choice in the first row is on the left, and in the other two, it is on the right.

Obviously, there is a trade-off between the brevity of a choice list and the fineness and extent of measurement. Some experimenters solve this by using differently sized increments, i.e., smaller increments in intervals they expect to be most relevant. Since we were concerned that this might confound subjects' choices by steering the switching row in a certain direction, we used constant increments (of €0.50) throughout all choice lists. To be able to pick up finer depletion-induced changes in risk attitudes, switching points in four additional "small" choice lists were elicited after subjects had made their choices in all four "large" choice lists. These "small" choice lists consisted of six rows covering the switching range in the respective "large" choice lists and had increments of €0.10. Importantly, one of the rows of the "small" choice lists was randomly chosen for payment if and only if the respective switching range of the associated "large" choice list had been selected for payment. This ensures that subjects have no incentive to misrepresent their preferences in the "large" choice lists in order to face lotteries with greater expected value in the "small" choice lists.

A particular feature of our computerized implementation of the choice lists is that, once a subject switches, all subsequent rows are automatically filled in. Subjects could still adjust their choices and had to press a "Continue" button to confirm their choices before moving on to the next choice list. This was done to let as little time as possible pass between the depletion task and the measurement of risk attitudes. While it is typically assumed that self-control resources replenish after some time, we are not aware of any evidence on how long depletion effects last. Furthermore, we did not want to exhaust or annoy subjects, and thus possibly impair the quality of our data, by forcing them to make 85 clicks.

[9] Andersson et al. (2016) show that when subjects make mistakes that lead to random choice and their "real" risk attitude does not imply a switching row at the center of a choice list, a systematic measurement error toward indifference at the center of the choice list occurs. Thus, we designed our choice lists in such a way that the switching row for the median risk attitude that we expected in the control condition—on the basis of degrees of risk attitudes commonly observed in experiments—was at the center of the respective choice list. It turns out that our expectations were rather accurate. The median switching row in the control group was close to the center (one to three rows above the center) for all choice lists.



Bitte wählen Sie in jeder Zeile eine Alternative aus.

Alternative A		Alternative B
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	3.00 € mit 50 % oder 7.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	3.50 € mit 50 % oder 7.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	4.00 € mit 50 % oder 8.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	4.50 € mit 50 % oder 8.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	5.00 € mit 50 % oder 9.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	5.50 € mit 50 % oder 9.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	6.00 € mit 50 % oder 10.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	6.50 € mit 50 % oder 10.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	7.00 € mit 50 % oder 11.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	7.50 € mit 50 % oder 11.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	8.00 € mit 50 % oder 12.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	8.50 € mit 50 % oder 12.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	9.00 € mit 50 % oder 13.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	9.50 € mit 50 % oder 13.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	10.00 € mit 50 % oder 14.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	10.50 € mit 50 % oder 14.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	11.00 € mit 50 % oder 15.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	11.50 € mit 50 % oder 15.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	12.00 € mit 50 % oder 16.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	12.50 € mit 50 % oder 16.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	13.00 € mit 50 % oder 17.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	13.50 € mit 50 % oder 17.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	14.00 € mit 50 % oder 18.00 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	14.50 € mit 50 % oder 18.50 € mit 50 %
3.00 € mit 50 % oder 22.00 € mit 50 %	Alternative A ◯ ◯ Alternative B	15.00 € mit 50 % oder 19.00 € mit 50 %

Weiter

Fig. 1. Choice List (“Table”) A: risky versus risky.

Translation: “Please choose one alternative in each row. Alternative A [first row:] €3.00 with 50% or €22.00 with 50%. Alternative B [first row:] €3.00 with 50% or €7.00 with 50%.”

A final aspect to consider is the value of $\hat{z}$ in the Fudenberg-Levine model, the theoretical threshold above which all additional income is saved. At least one of the lotteries needs to be such that one outcome is above and one below $\hat{z}$. To elicit a proxy for subjects' individual $\hat{z}$ values, we use two vignettes in the post-experiment questionnaire. The vignettes asked subjects to imagine two scenarios. The first scenario is going out with friends in the evening. The vignette asked subjects to indicate the minimal amount of money spent while going out such that they would consider the evening "expensive". The second scenario is casually discovering an item that one would like to buy in a store. It asks subjects to state the minimal price of that item that would induce them to deliberate about the expenditure instead of buying the item immediately.

Median values in the two vignettes are €15 and €20, respectively. Thus, taking these values as proxies for $\hat{z}$, our design of the choice lists ensures that, for most of the subjects, the vast majority of lottery choices under consideration should be affected by self-control depletion.

Although the "true" cutoff $\hat{z}$ is unobservable, one can argue that our choice lists likely fulfill the payoff requirements mentioned above even without taking the proxies provided by the vignettes into account. Strictly speaking, $\hat{z}$ varies between subjects, between lotteries, and between conditions, as well as over time, because all these factors might affect the marginal costs of self-control. However, as long as, for example, €3 $< \hat{z} <$ €22 for Choice List A, the list covers the payoff range that enables a test of the prediction of the Fudenberg–Levine model that lower self-control leads to greater risk aversion. The upper bound of €22 seems very reasonable given that the median daily disposable income, net of rent, in our sample is only €10 (surveyed through the post-experiment questionnaire). Regarding the lower bound, we argue that the value of $\hat{z}$ must be above the minimum payoff of our lotteries (€3) for the majority of subjects. If this was not the case, subjects would have to exhibit risk neutrality according to the Fudenberg–Levine model. Contrary to this, most of our subjects turn out to be risk-averse, as we report in the Results section. These arguments jointly suggest that the payoff ranges were appropriate for our purposes.

3.4. Manipulation checks

Most studies using ego depletion do not include *independent* manipulation checks but simply rely on the effectiveness of the implemented depletion task based on the results of previous studies. By contrast, we include a multifaceted manipulation check comprised of several parts in our experiment to be able to assess independently from possible treatment effects whether the depletion task did indeed induce variations in self-control. Ideally, one would assess subjects' state of self-control at the same time as measuring their risk attitudes. This is, of course, not feasible. One possibility would be to introduce all measures of the manipulation check in between the depletion task and the measurement of risk attitudes. Most

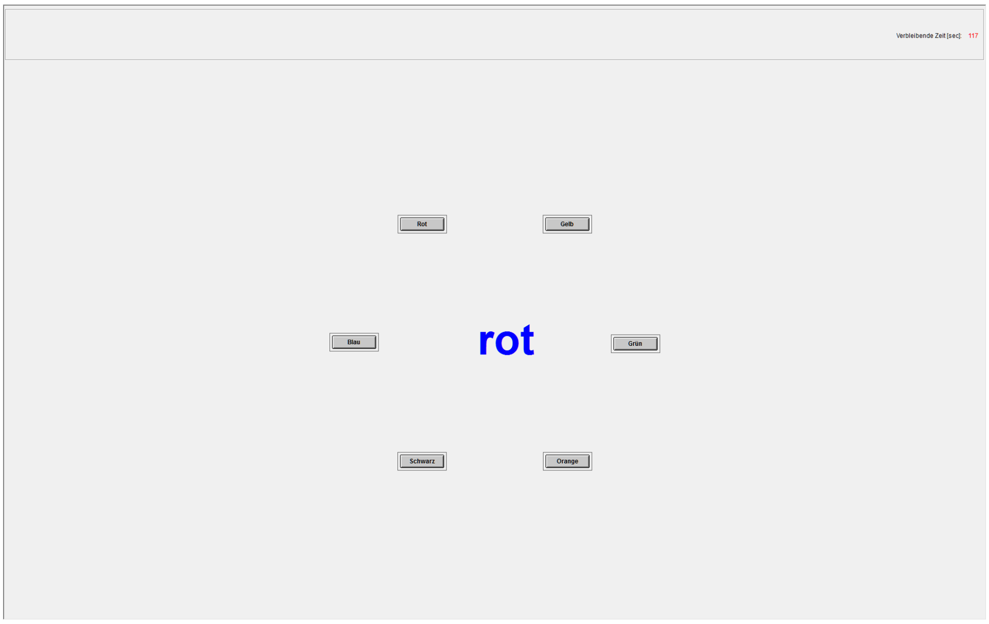


Fig. 2. Screenshot of the computerized Stroop test.

Notes: The screenshot depicts an incongruent trial, i.e., the meaning of the color word "rot" ("red") does not correspond to the color of the word (in this case, shown in blue on the screen). Subjects would have to press the "Blau" ("Blue") button.

candidates for manipulation checks (e.g., the Stroop test) are, however, likely to alter subjects' level of self-control themselves. We therefore include a short ad hoc measure that we do not consider depleting right after the depletion task and a more comprehensive, but possibly depleting part of the manipulation check right after measuring risk attitudes. Because self-control resources are generally thought to replenish over time, doing parts of the manipulation check only after the main part of the experiment may have the disadvantage that self-control resources could have already replenished partly or completely.

Our first short ad hoc measure consists of choosing the difficulty of a puzzle (on a scale from one to ten). Our conjecture was that depleted subjects would select an easier puzzle. Since the puzzle is solved only later, the mere choice of its difficulty level should not affect subjects' level of self-control resources.

The second part of the manipulation check, performed after risk attitudes have been measured, is a computerized version of the Stroop test (Stroop, 1935; MacLeod, 1991). The Stroop test is well-established both as a depletion task and as a dependent variable in depletion studies (see Hagger et al., 2010). In our computerized version, the name of a color appears in bold letters at the center of the screen. The letters themselves are also printed in color. In "congruent trials", this color corresponds to the word's meaning, while it differs from the word's meaning in "incongruent trials". Subjects' task is to indicate the color in which the letters are printed—and not the meaning of the color word. To this end, the screen shows six buttons that are labeled with color names and located on a circle around the bold color word. For a screenshot, see Fig. 2. Subjects have to click the button corresponding to the color in which the word is printed as fast as they can. Just as in the depletion task, in incongruent trials of the Stroop test, subjects have to exert self-control to suppress an automatic impulse, namely clicking the button corresponding to the meaning of the word. Immediately after each button click, a new word appears. Subjects receive no feedback. In our experiment, subjects work on this task for three minutes. Widely used measures to check for depletion effects are average response times per trial and the number of correct answers. We expect longer response times and a lower number of correct answers in the depletion compared to the control group.

As a third measure that is employed frequently (see Hagger et al., 2010), we asked subjects at the beginning of the final questionnaire how much they had to concentrate in each part of the depletion task and how exhausted they felt before the experiment and at the present moment. For both we calculate differences and compare them between treatment and control group.

Based on these five independent components of the overall manipulation check (choice of difficulty of a puzzle, response times and number of correct responses in the Stroop test, difference in self-reported need to concentrate during the two parts of the depletion task, and difference in self-reported fatigue at the beginning and end of the experiment) that all have their distinct strengths and weaknesses, we construct a joint index of depletion. We z-standardize each of the five measures, average over them, and again z-standardize the result. Averaging over different measures of the same construct is a common procedure to reduce measurement error. Thus, we believe that the aggregate depletion index is suited best to indicate the effectiveness of the depletion task.

3.5. Procedural details and implementation

The detailed sequence of events in each session was as follows:

(*i*) *Instructions.* Upon entering the lab, subjects drew a card containing a number and were asked to sit in the respective booth. They read the instructions, were encouraged to ask questions in private, and answered several control questions on the computer. (A translation of the instructions to English can be found in Online Appendix B.)

(*ii*) *Depletion task.* Subjects participated in the treatment-specific version of the crossing-out letters task that either induced low self-control or left self-control unchanged.

(*iii*) *Part 1 of the manipulation check.* Subjects chose the difficulty of a puzzle (on a scale from 1 to 10) that they solved at the end of the experiment.

(*iv*) *Measurement of risk attitudes.* Subjects made lottery decisions in the four choice lists.

(*v*) *Part 2 of the manipulation check.* Stroop test.

(*vi*) *Puzzle.* Subjects solved the puzzle with the chosen level of difficulty.

(*vii*) *Postexperiment questionnaire,* including *Part 3 of the manipulation check* (self-reported required concentration during each part of the depletion task and self-reported exhaustion before and after the experiment).

The experiment was conducted at the BonnEconLab, using the software z-Tree (Fischbacher, 2007). We used ORSEE (Greiner, 2015) for inviting subjects and for recording their participation. The experiment consisted of thirteen sessions in July and October 2014. Treatment and control were balanced with respect to day of the week and time of the day, since there is empirical work that suggests that both self-control and measured risk attitudes may exhibit a correlation with time of the day (Kouchaki and Smith, 2014).

All written instructions and all recorded data (except, of course, personal data such as information on subjects' bank accounts) as well as the source code used for running the experiment and used in the data analysis are available as supplementary material on the journal website.

308 subjects participated, each in only one group (152 in the depletion and 156 in the control group). 150 subjects were male (74 in the depletion and 76 in the control group), 158 were female (78 in the depletion and 80 in the control group). Most subjects (92%) were students and majored in various subjects. Age varied between 17 and 55 years[10] (median age, 24 years; 93% in the range 19–30 years) and did not differ significantly between groups. No particular exclusion criteria applied, except for color blindness.

In total, the experiment lasted about 75 minutes (including payment). Subjects received on average €12.25 from the outcome of one randomly drawn lottery decision (random lottery incentive mechanism; RLIM) plus an additional €1 for filling out the questionnaire. Given that we are testing theories that deviate from expected-utility theory, a comment regarding incentive compatibility of the payment mechanism is in order. Our instructions tell subjects that it is in their best interest to treat each choice as if it were the only one because only one lottery decision will be randomly selected for payment. Nevertheless, in case subjects do not "isolate" decisions but integrate them at least partially into a compound lottery, experiments with only one choice may yield different results than experiments with repeated decisions. This can even occur when the RLIM is used, unless subjects obey "statewise monotonicity" (Azrieli et al., 2017), a condition equivalent to "compound independence" (Segal, 1990). In fact, the Fudenberg–Levine model *can* predict that different choices are made in a setup with a single choice than in a setup with multiple choices and remuneration via the RLIM. Importantly, however, we show in Section A.4 in Appendix A that the qualitative prediction *regarding the treatment effect* is unaffected. Hence, judged by the dual-self model,

[10] Underage subjects must provide written consent by their parents in order to participate in experiments at the BonnEconLab.

the RLIM is appropriate for detecting treatment differences resulting from reduced self-control.

Payments were made in a separate room to ensure privacy. Those subjects for whom the delayed lottery was drawn did not receive the lottery's payoff until a week later. They could choose between a dated bank transfer and collecting the amount in cash in person.

The postexperiment questionnaire measured socioeconomic and demographic characteristics (age, gender, high school GPA, latest math grade at school, student status and field of study, experience with experiments) and assessed subjects' general attitudes toward risk and time, using questions from the SOEP questionnaire (German Socio-Economic Panel). Additionally, via ten questions adopted from Hauge et al. (2014), we aimed at measuring whether subjects primarily used the deliberative or the affective system while making their lottery choices. Subjects also answered a questionnaire on character trait self-control (Tangney et al., 2004; Bertrams and Dickhäuser, 2009) as well as a questionnaire on positive and negative affect at the present moment (Watson and Clark, 1999).

4. Results

4.1. Manipulation checks

A manipulation check based on the aggregate depletion index indicates that subjects in the treatment group were significantly more depleted than their counterparts in the control group (two-sided t-test, $p < 0.001$). Translating the aggregate depletion index into a standardized effect size, we find Cohen's $d = 0.74$. Moreover, for each of the five separate parts that comprise the depletion index (see Section 3.4), we observe differences between depleted and nondepleted subjects in the expected direction. That is, subjects in the depletion treatment chose slightly lower levels of difficulty of a puzzle (two-sided Wilcoxon rank-sum test, $p = 0.391$), had slightly longer response times and a slightly lower number of correct answers in the Stroop test (two-sided Wilcoxon rank-sum test: $p = 0.286$ and $p = 0.222$, respectively), a stronger increase in concentration required for the second part of the depletion task (two-sided Wilcoxon rank-sum test, $p < 0.001$), and a stronger increase in exhaustion compared to their control-group counterparts (two-sided Wilcoxon rank-sum test, $p = 0.176$).

4.2. Descriptive statistics

Recall that the switching row in each of the four choice lists measures an individual's risk attitude. More precisely, differences in expected values of the less risky lottery and its mean-preserving spread at the switching row measure an individual's "risk premium" $m_{\sim}$ that has to be added to the riskier lottery to make that subject indifferent between the two lotteries. Based on these indifference-generating risk premia, we classify subjects' behavior into four categories: *risk-seeking* (negative

Table 2
Pairwise Pearson's correlation coefficients of the individual indifference-generating risk premia in the four choice lists.

	$m_{\sim}^{A}$	$m_{\sim}^{B}$	$m_{\sim}^{C}$	$m_{\sim}^{D}$
$m_{\sim}^{A}$	1			
$m_{\sim}^{B}$	0.3830	1		
$m_{\sim}^{C}$	0.3819	0.2654	1	
$m_{\sim}^{D}$	0.4173	0.5660	0.3481	1

Note. For *all* correlations shown above: $p < 0.0001$.

risk premium), *risk-neutral* (risk premium of zero), *risk-averse* (positive risk premium), and *dominated choices.*

For Choice Lists A, B, and D, 80% to 89% of subjects made risk-averse choices. This is in line with the widely observed empirical result that a vast majority of individuals is risk-averse (e.g., Harrison et al., 2007; Dohmen et al., 2011).

For Choice List C, where one of the alternatives is a long shot, i.e., offers a low probability of winning a large prize, only 46% of subjects are classified as risk-averse. This shift in expressed risk attitudes due to the presence of a long shot is expected, based on the commonly observed fourfold pattern of risk attitudes (Harbaugh et al., 2010) which is usually attributed to an overweighting of the small probability associated with the large payoff.

A more detailed description of how the risk premium is calculated and how the categories are formed as well as a table displaying the absolute and relative frequencies of choices in the four choice lists can be found in Online Appendix A.

It is noteworthy that the size of the indifference-generating risk premia reacts to differences between the choice lists in a plausible manner: On average, subjects exhibit the highest risk premium ($m_{\sim}^{A}$ = €2.69) for Choice List A (risky vs. risky lottery), i.e., the choice list with the largest difference between the spreads of the two lotteries. For the long shot, the average risk premium is $m_{\sim}^{C}$ = −€0.36, indicating that on average subjects behave in a slightly risk-seeking manner.

The risk premia measured in the different choice lists exhibit significant and positive pairwise correlation coefficients (see Table 2). Hence, we are confident that our measures of risk attitudes pick up systematic variation in underlying individual risk attitudes.

4.3. Treatment effects

Fig. 3 displays subjects' choices in detail and serves as a graphical representation of our main results. The variable on the horizontal axis is the indifference-generating "risk premium" $m_{\sim}$, i.e., the difference in expected values between the lotteries in the switching row in euros. Thus, subjects to the right of zero are classified as risk-averse, while those to the left are classified as risk-seeking.

Result 1. (Hypotheses 1, 2 and 1 b, 2 b.) *Ego depletion does not increase risk aversion.*

We do not observe an increase in risk aversion of the treatment relative to the control group for any of the choice lists. For Choice Lists A and B, Wilcoxon rank-sum tests do not reject the null hypothesis of no treatment difference in risk premia (two-sided $p = 0.245$ and $p = 0.253$, respectively). For both choice lists, depleted subjects even show a nonsignificant tendency toward less risk aversion, with depleted subjects exhibiting lower indifference-generating risk premia in both Choice List A ($\Delta_{\underset{\sim}{m}^{\text{A}}} = €0.30$; Cohen's $d = 0.13$) and Choice List B ($\Delta_{\underset{\sim}{m}^{\text{B}}} = €0.10$; Cohen's $d = 0.07$).

Result 2. (Hypotheses 3 and 3 b.) *When payoffs are delayed, ego depletion does not affect risk attitudes.*

Also in Choice List D, where all payoffs are delayed by one week, depleted subjects are slightly less risk-averse than subjects in the control group ($\Delta_{\underset{\sim}{m}^{\text{D}}} = €0.22$; Cohen's $d = 0.15$). Using two one-sided mean equivalence tests, we reject the null hypothesis of a difference of half a standard deviation or more ($p = 0.001$). This is in line with our initial hypothesis. However, this result would only be evidence in support of the Fudenberg–Levine model, had Hypotheses 1 and 2 been confirmed by the data.

Result 3. (Hypothesis 4.) *For long shots, there is no evidence for a difference in risk attitudes under ego depletion.*

For Choice List C, where one of the lotteries yields an outcome of €50 with 10% probability, we hypothesized (based on Loewenstein and O'Donoghue, 2005, p. 28) that ego depletion induces less risk-averse choices through an increased overweighting of the small probability associated with the large payoff. On average, subjects are mildly risk-seeking in both the treatment and control group for Choice List C. We again find that depleted subjects made slightly less risk-averse choices; however, the difference between the two groups is not statistically significant ($\Delta_{\underset{\sim}{m}^{\text{C}}} = €0.28$, Cohen's $d = 0.13$, Wilcoxon rank-sum test, two-sided $p = 0.335$). Thus, for the sample as a whole, we find no evidence in support of the prediction by Loewenstein and O'Donoghue.

Result 4. (Hypothesis 4 b.) *For long shots, there is no evidence for greater variance of risk attitudes under ego depletion.*

When testing for a difference in variances between treatment and control group for Choice List C, we find no evidence in favor of this hypothesis (Levene's robust test statistic, W_0, for the equality of variances, $p = 0.999$). Neither is there an indication for a difference in variances for any of the other choice lists (Levene's W_0; $p = 0.310$ for Choice List A, $p = 0.756$ for B, and $p = 0.069$ for D).

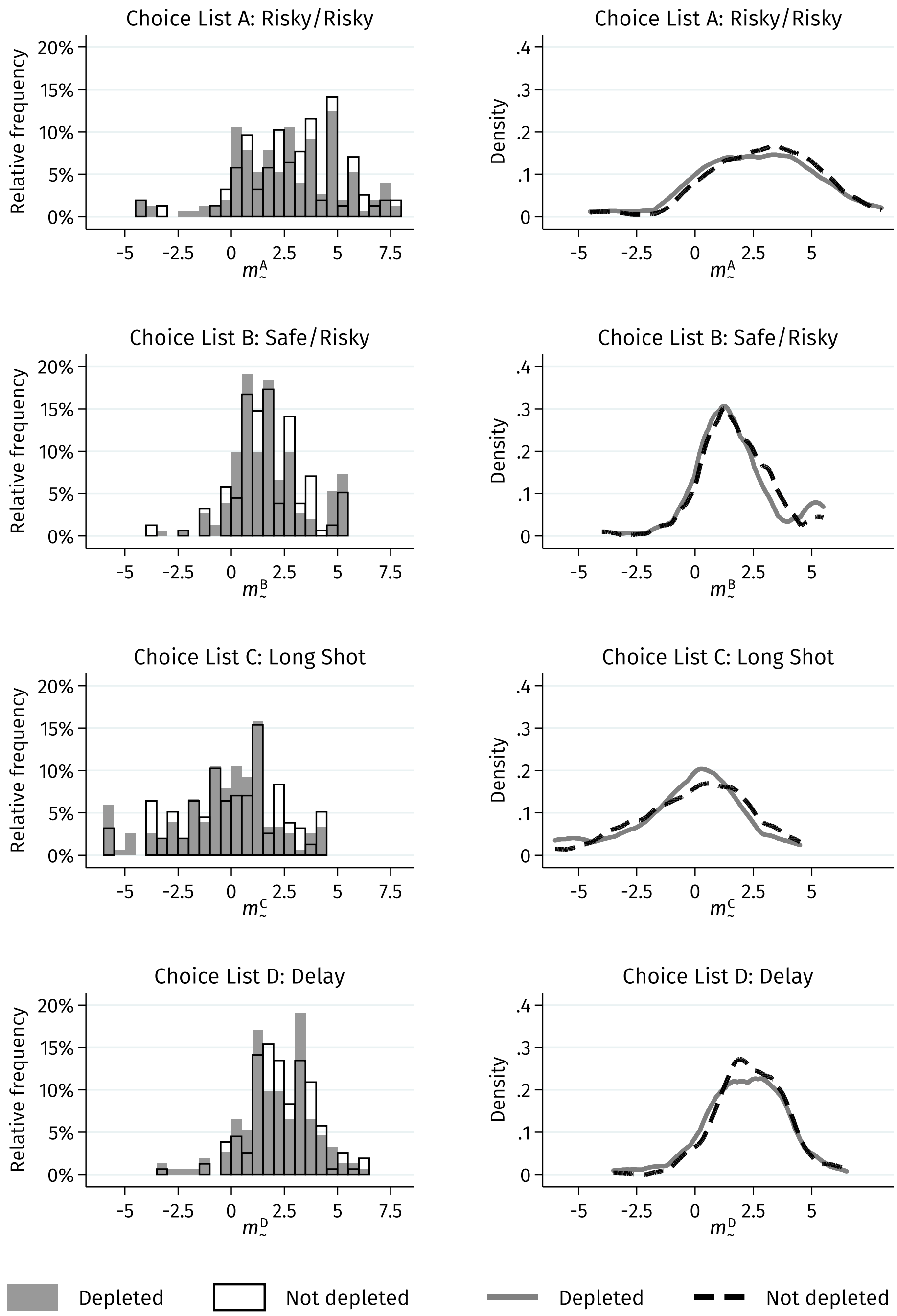


Fig. 3. Treatment comparison of indifference-generating risk premia.

Notes. The horizontal axis displays the indifference-generating risk premia $m_{\sim}$, i.e., the difference in the expected values of the more risky and the less risky lottery at the switching row (in euros). Left column: Histograms of observed risk premia. Right column: Estimated kernel densities (Epanechnikov kernel functions, optimal-bandwidth routine by Stata).

4.4. Disaggregating the data: results from linear regressions

Even though we obtain an overall null result regarding the effect of ego depletion on risk attitudes, it is possible that there is heterogeneity in the effect of ego depletion depending on subject characteristics. Thus, we investigate whether choices and the effect of ego depletion in our experiment vary with observable characteristics. We do so by regressing choices on those observables which are most likely to determine risk attitudes. The results are presented in Table 3. In addition, the table contains (in its top part) the coefficients of a simple linear regression of risk attitude on the treatment dummy for convenient comparison.

In the specification with controls, we include gender as an explanatory variable because women are typically found to be more risk-averse than men (see Croson and Gneezy, 2009, for a general survey on gender differences in risk taking). Furthermore, we regress our measure of risk attitudes on the final grade at high school (self-reported by subjects; reverse scoring compared to the American GPA, i.e., higher grades are worse) as a proxy for IQ, since cognitive ability has been found to covary with risk attitudes (Dohmen et al., 2010).[11] Since there may be individual differences in both subjects' baseline self-control ability and in the treatment effect on self-control, we include trait self-control (questionnaire measure; Tangney et al., 2004) and the standardized aggregate score of the depletion index in the regression.

Columns 1 through 4 display the regression of choices in each choice list separately, while column 5 uses the data from all choice lists. The dependent variable is the indifference-generating risk premium $m_{\sim}$ for the respective choice list indicated in the column header. Note that the larger $m_{\sim}$, the greater is risk aversion. The unit of this measure is €.

In line with the literature, baseline risk aversion is higher for women than for men across all choice lists but only significantly so in Choice List A ($p = 0.010$). The coefficients of the IQ proxy and of trait self-control turn out not to be significant, neither for individual choice lists nor for all choice lists jointly. Moreover, we find no evidence that risk attitudes vary by the extent of depletion. The coefficient of the aggregate depletion index is never significantly different from zero. Only for Choice List A, it is marginally significant ($p = 0.075$).

Including the interaction of Female and Ego depletion allows us to split up the effect of ego depletion by gender. The coefficient on the regressor Ego depletion represents the influence of self-control depletion on men, and the sum of this coefficient and that on the interaction term (i.e., Ego depletion + Ego depletion × Female) represents the effect of depletion on women.

[11] Despite evidence that age is a determinant of risk attitudes, we do not include it due to limited variation in age in our sample. For our sample, we find that the coefficient on age is heavily influenced by two outliers (above 50 years). Similarly, we do not control for student status because 92% of our sample are students. When using math grade instead of high school GPA as a proxy for IQ, the coefficients and their significance levels are virtually unchanged.

Table 3
Linear regressions of the measure of risk aversion on individual characteristics.

Dependent variable: indifference-generating risk premium $m_{\sim}$ [in €]					
Model 1: Simple regression	(1) Choice List A	(2) Choice List B	(3) Choice List C	(4) Choice List D	(5) Combined
Ego depletion	–0.301	–0.100	–0.284	–0.224	–0.233
	(0.271)	(0.166)	(0.265)	(0.177)	(0.161)
Constant	2.842***	1.542***	–0.221	2.317***	1.636***
	(0.183)	(0.117)	(0.182)	(0.114)	(0.114)
Observations	303	289	295	304	1191
R^2	0.004	0.001	0.004	0.005	0.003
Model 2: Including controls	(6) Choice List A	(7) Choice List B	(8) Choice List C	(9) Choice List D	(10) Combined
Ego depletion	–0.563	–0.299	–0.839**	–0.425*	–0.540***
	(0.362)	(0.216)	(0.379)	(0.241)	(0.208)
Manipulation check	0.251*	0.111	0.153	–0.036	0.128
	(0.141)	(0.094)	(0.134)	(0.104)	(0.086)
Female	0.900***	0.041	0.166	0.313	0.362*
	(0.347)	(0.228)	(0.368)	(0.222)	(0.216)
Female × Ego depletion	0.173	0.247	0.891*	0.453	0.440
	(0.511)	(0.326)	(0.533)	(0.341)	(0.306)
Final grade at high school	–0.176	–0.019	0.034	0.128	0.002
(GPA equivalent, inversely coded)	(0.200)	(0.149)	(0.228)	(0.147)	(0.135)
Trait self-control	–0.017	0.001	–0.017	0.000	–0.008
	(0.016)	(0.010)	(0.015)	(0.010)	(0.009)
Constant	2.467***	1.559***	–0.253	2.142***	1.493***
	(0.247)	(0.150)	(0.263)	(0.163)	(0.152)
Observations	303	289	295	304	1191
R^2	0.066	0.013	0.042	0.043	0.026
Effect of self-control depletion on women					
Ego depletion + Female × Ego depletion	–0.390	–0.052	0.051	0.028	–0.101
F[Ego depletion+ Female × Ego depletion = 0]	0.940 (p = 0.33)	0.033 (p = 0.86)	0.017 (p = 0.90)	0.011 (p = 0.92)	0.156 (p = 0.69)

Notes. Dependent variable: indifference-generating risk premium $m_{\sim}$ for the choice list indicated in the column header. The larger $m_{\sim}$, the greater risk aversion. Robust standard errors (cluster-corrected at the subject level for columns 5 and 10) in parentheses. Asterisks indicate significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Missing observations ($N < 308$) are due to exclusion of trials in which subjects chose a dominated option. The regressors Depletion Check, Final grade at high school and Trait self-control are mean-centered (so that the constant represents the indifference-generating risk premium at the sample mean of these variables). Also note that in the German grading system, 1 is the best grade, and 4 is the worst among the passing grades.

In all choice lists, men tend to become *less* risk-averse under depletion, for Choice Lists C and D even (marginally) significantly so ($p = 0.028$, and $p = 0.079$, respectively). Aggregating over all choice lists, the average man is indifferent at a 54 cent lower risk premium in the depletion than in the control group ($p = 0.010$), which goes against our hypotheses.[12] Interestingly, self-control depletion has basically no effect on women, as can be seen from the bottom three rows of Table 3. This finding is in line with Friehe and Schildberg-Hörisch (2017).

Recall that in Choice List C, one of the lotteries is a "long shot", i.e., it features a small probability of winning a large prize. Here, the tendency that depleted men are on average indifferent at a 84 cents lower risk premium than nondepleted men ($p = 0.028$) conforms with Hypothesis 4. This could be interpreted as evidence in favor of increased overweighting of the small probability associated with the large payoff, as Loewenstein and O'Donoghue (2005) predict for reduced self-control. However, we find a decrease in risk aversion, albeit less pronounced and mostly nonsignificant, also across the remaining choice lists—for which their model does not predict an influence of self-control depletion. Moreover, the effect is absent for women. Hence, in sum, there is only limited evidence for the channel proposed by Loewenstein and O'Donoghue (2005).

4.5. Summary

Although our manipulation check suggests that the manipulation effectively depleted the self-control resources of subjects in the treatment group, we do not find any significant difference in risk attitudes between subjects in the treatment and control group. However, we observe the same nonsignificant tendency for all four choice lists. Under depletion, subjects behave in a slightly *less* risk-averse manner, in contrast to the prediction of the Fudenberg–Levine model.

5. Discussion and conclusion

Our goal in this paper is to investigate the causal influence of self-control on risk attitudes. Self-control, a concept from psychology, has been conceptualized and formalized in economics through dual-self models (in particular, Fudenberg and Levine, 2006, 2011, 2012) and dual-system models (in particular, Loewenstein and O'Donoghue, 2005; Mukherjee, 2010). These models posit that a central determinant of risk attitudes is an individual's current level of self-control.

Building on the Fudenberg–Levine dual-self model, we derive hypotheses for choices between risky monetary payoffs in a state of low self-control, compared to regular self-control. We show that the model predicts that lower levels of self-control induce stronger risk aversion for stakes within a particular range. We then test the hypotheses in a lab experiment with a large number of subjects by exogenously lowering self-control resources in half of our subjects via so-called ego depletion. We do not find any evidence for increased risk aversion after self-control

[12] The regression analysis adds significance tests for several coefficients. Given the large share of nonsignificant results, the significant effect of ego depletion on men's risk aversion should be judged with due care.

depletion. Contrary to the theoretical predictions of the Fudenberg–Levine model, our results document a consistent but nonsignificant tendency of depleted subjects to become *less* risk-averse. Only for male subjects, this tendency is significant when considering their decisions across all four choice lists jointly.

Before discussing the implications of our findings for the modeling of decision making under risk, we exclude several alternative explanations of our data.

A possible concern might be that some of the payoffs of our choice lists were not chosen optimally, i.e., the respective choices did not reflect the case in which, at least in one of the two lotteries, one payoff was below and another above the theoretical cutoff $\hat{z}$. In these cases, the model of Fudenberg et al. (2014) does not predict any effect of ego depletion on risk attitudes. Using the values that we measured in the vignettes as proxies for $\hat{z}$ suggests that this might be the case for about 33% of the total number of choices (85 per subject), while our payoff choices imply that ego depletion should affect risk attitudes in the remaining 67% of choices.[13] We find that our results are robust to excluding those choices for which we predict no effect of ego depletion on risk attitudes based on the individual $\hat{z}$. In particular, we still do not find any significant difference in risk attitudes between treatment and control group for any of the choice lists (Wilcoxon rank-sum tests, $p = 0.487$ for Choice List A, $p = 0.915$ for Choice List B, $p = 0.346$ for Choice List C, and $p = 0.435$ for Choice List D).[14]

Another possible concern may be that all subjects were depleted to begin with and thus could not have been affected by the treatment difference. We do not find evidence in support of that view. 65% of subjects report an initial exhaustion level of 5 or less on a Likert scale ranging from 0 to 10. Moreover, when we restrict our analysis to those subjects who report initial exhaustion below the median level of 3, there is no significant increase in risk aversion either (t-tests, $p = 0.244$ for Choice List A, $p = 0.912$ for B, $p = 0.089$ for C, and $p = 0.362$ for D, with subjects in the depletion condition being even less risk-averse for Choice Lists B through D).

A further hypothesis is that self-control depletion has a different effect on risk attitudes than is suggested by the Fudenberg–Levine model. Rather than causing a shift in the distribution of risk preferences, it may make subjects more likely to make mistakes, leading to a higher variance in decisions under depletion. Based on the tests reported in Section 4.3, we do not find evidence in favor of this hypothesis.

Alternatively, self-control as a stable character trait might explain heterogeneity in risk attitudes across individuals, even if temporary changes in self-control as induced by depletion tasks do not have a significant impact on risk preferences. In this vein, Fudenberg and Levine (2011, p. 57) state, "One possible next step would be to try to more explicitly account for the evident heterogeneity of the population, and estimate distributions of self-control parameters … ." We therefore measure trait self-control in the questionnaire, using the German version of the scale by

[13] We assign each subject the average $\hat{z}$ from her answers to both vignettes.

[14] Subjects are included if at their individual switching row, the condition that for at least one of the two lotteries one payoff is below and the other is above their individual average $\hat{z}$ is fulfilled.

Tangney et al. (2004). While trait self-control is, for example, a significant predictor of the final grade at high school in our data, it does not explain risk attitudes in any of our choice lists.

Moreover, decision making between groups might differ in systematic ways which do not manifest themselves in choices. For example, subjects may rely on heuristics to a larger extent in the depletion than in the control group (e.g., Loewenstein and O'Donoghue, 2005). It is likely that decision times using heuristics are shorter (Rubinstein, 2007, 2016). According to this measure, we do not find any evidence for increased reliance on heuristics by depleted subjects. In fact, decision times of subjects in the depletion group are slightly longer for all choice lists, albeit insignificantly so (t-tests; 87 s vs. 81 s, $p = 0.149$ for Choice List A; 49 s vs. 46 s, $p = 0.219$ for B; 72 s vs. 68 s, $p = 0.474$ for C; 81 s vs. 77 s, $p = 0.275$ for D; and 72 s vs. 68 s, $p = 0.192$, cluster-corrected, for all choice lists jointly).

Being null results, our findings add to the recent skepticism against all "strength of self-control"-type of models, including Fudenberg and Levine's. Most prominently, Carter and McCullough (2014) reanalyze the data of the meta-analysis of Hagger et al. (2010). They find indications of small-sample effects, in particular publication bias. When correcting for publication bias, estimated effect sizes are smaller, between 0.42 and 0, depending on which statistical method is used. In reaction to Carter and McCullough (2014), Hagger et al. (2016) conducted a large-scale, preregistered replication study that failed to reproduce an effect of a crossing-out letters task on a subsequent self-control task. Our crossing-out letters task, however, differs from the one in their study in that it is neither computerized nor does it leave out the stage of establishing the impulse to cross out the letter "e". These are exactly the aspects that Baumeister and Vohs (2016a) regard as the main reasons for manipulation failure in Hagger et al. (2016). Thus, our setup gives self-control failure due to ego depletion the best possible chance to produce an effect.

Nevertheless, one could argue that the effect of ego depletion might not be strong enough to induce an increase in self-control costs. However, using an ego depletion task is a way to operationalize self-control that Fudenberg et al. (2014) themselves suggest in order to make their model testable. Moreover, our aggregate manipulation check implies that subjects in the treatment group were indeed significantly more depleted than those in the control group.

Furthermore, our sample size of $N = 308$ exceeds the sample size of all except one of the 198 studies on the effects of ego depletion that are covered by the meta-analyses by Hagger et al. (2010) and Carter and McCullough (2014) (only 10 of the 198 studies have a sample size that exceeds 100), and power analyses show that it yields sufficient power to document relevant effect sizes.

Still, we do not find any evidence for increased risk aversion after ego depletion as predicted by the model of Fudenberg et al. (2014) with convex self-control costs. On average, depleted subjects even tend to be less risk-averse, albeit not significantly so. As we argue in Section 4.4, for men there is some but limited evidence that this decrease in risk aversion is generated by increased probability weighting, as Loewenstein and O'Donoghue (2005) predict. Our findings that men are significantly less risk-averse when considering all choice lists are also in line with those by Friehe and Schildberg-Hörisch (2017).

Traditionally, economics has modeled decision makers without any reference to psychological concepts like "self-control". Due to the inability of the standard models of economic choice—expected-utility theory and discounted utility—to explain particular phenomena in intertemporal decision making and decision making under risk, concepts from psychology have been integrated into new models to increase their explanatory power.

We have no doubt that economics can benefit from incorporating psychological concepts in general and self-control in particular. For instance, we consider it plausible that self-control plays an important role in savings decisions, addiction, and health-related behavior such as food choice. However, its influence in decision making under risk seems limited: in our data different levels of self-control only carry over to different risk attitudes in a negligible extent. In particular, given that we observe a nonsignificant tendency toward *decreased* rather than increased risk aversion following ego depletion, our findings cast doubt on the "unified explanation" offered by Fudenberg and Levine (2006): risk attitudes and intertemporal choice seem to be less interrelated—or related in different ways—than their model suggests.

Appendix A. Derivation of hypotheses from the Fudenberg–Levine model

We briefly sketched the Fudenberg–Levine model in Section 2.1. In the following, we describe the interaction between the long-run and the short-run self in greater detail. In particular, we examine pairwise choice between two-outcome lotteries in the approximate model developed by Fudenberg et al. (2014). Finally, we explicitly incorporate self-control depletion in the model so that we can derive hypotheses concerning its effect on choices between two-outcome lotteries.

A.1. The model in detail

A.1.1. Mental accounting

Just like in Fudenberg and Levine (2006, 2011), agents in the approximate model (Fudenberg et al., 2014) use mental accounting—the mental assignment of expenditures to different accounts—as a means to avoid costly self-control.[15] An agent in this model lives for several periods. Each of these periods can be thought of as being mentally divided into two subperiods, a "banking period" for planning and a "night-club period" for spending money. During the banking subperiod, there is no possibility for consumption. Instead, the long-run self plans how much "pocket cash" x to take to the night club and how much to save for future periods. In other words, it chooses an expenditure level for the second subperiod. During the night-club subperiod, the short-run self spends all "pocket cash", and no self-control costs arise.

There can be unanticipated income ("windfall profits") during the night-club period. This income can be stochastic, and it can present itself in the form of multiple income opportunities between which the agent can choose, such that the realized income depends on the agent's choice (e.g., accepting or declining the offer to substitute for a coworker who has called in sick on short notice). Following the notation in Fudenberg et al. (2014), let consumption c refer to consumption on top of the planned consumption level x. In such a situation, once planned consumption x has been determined, the short-run self's choice between unanticipated income opportunities depends on c only. Hence, we suppress x in our notation and can denote the short-run self's consumption utility as a function $u(c)$. It is assumed that $u'(c) > 0$ and $u''(c) < 0$. Note that, unlike "standard" consumption levels, c can be negative, as long as $c > -x$.

A.1.2. Lotteries

In this setup, unexpectedly facing a set $\Im$ of income opportunities is a situation in which self-control becomes relevant. Lotteries are denoted as discrete random variables $Z \in \Im$ that can take on values $z_1, \ldots, z_n$, the lotteries' outcomes. Since the short-run self only cares about immediate consumption, its preferred plan of action is to spend all lottery gains immediately and, thus, to choose the lottery with the highest expected short-run utility $\mathrm{E}u(Z)$. The utility derived from this is called "temptation" and denoted $u^\star(\Im) \equiv \max_{Z\in\Im} \mathrm{E}u(Z)$.[16] The long-run self, in contrast, prefers to smooth consumption

[15] What is referred to as mental accounting here is only one component of mental accounting as described in Thaler and Shefrin (1981).

[16] For notational convenience, we suppress the dependence of temptation $u^\star$ on the menu $\Im$ in the following.

over time. Its value function is therefore close to risk-neutral. Through use of self-control it enforces an action that balances the short-run self's want for immediate consumption and its own preference for consumption smoothing.

A.1.3. Self-control

This act of self-control is assumed to be costly, with the cost depending on the temptation $u^\star$ as well as the actual consumption plan $\tilde{c}$ that the long-run self enforces. This cost enters the overall objective function through a self-control cost function $g[u^\star - \mathrm{E}u(\tilde{c})]$. The function $g[\cdot]$ is assumed to be smooth, nondecreasing, and weakly convex.[17] Its argument, $u^\star - \mathrm{E}u(\tilde{c})$, can be interpreted as foregone utility: (expected) utility that the short-run self was not allowed to realize due to being restricted by the long-run self. If $\mathrm{E}u(\tilde{c}) = u^\star$, no self-control is exerted and, consequently, no costs arise, $g[0] = 0$. Whenever the long-run self enforces an (expected) level of utility that is lower than the one desired by the short-run self, i.e., whenever $\mathrm{E}u(\tilde{c}) < u^\star$, self-control costs are nonnegative: $g[u^\star - \mathrm{E}u(\tilde{c})] \geq 0$. It is important to note that this makes preferences over lotteries menu-dependent, because these preferences depend on self-control costs which depend on temptation $u^\star$ which, in turn, depends on the menu of lotteries $\Im$.

A.2. Optimization

We will now consider preferences over menus of unanticipated lotteries, which match the situation in the lab. We address the decision problem that an agent faces when picking a lottery Z from menu $\Im$ in two steps. We first calculate optimal consumption for an arbitrary lottery. Then we derive how lotteries are ranked for two-outcome lotteries—the case that we employ in our experiment.

A.2.1. Optimal consumption plan in the presence of self-control costs

For each lottery in the menu $\Im$, the agent chooses a contingent consumption plan $\tilde{c}$ with outcomes $(c_1, \ldots, c_n)$, where c_i is consumption in case the lottery outcome z_i realizes $(i = 1, \ldots, n)$. Note that choosing the optimal consumption plan is equivalent to choosing an optimal level of self-control for each of the n lottery outcomes. It is determined by equating the marginal cost from exerting self-control and the marginal gain from saving for future periods.

The first-period utility for each lottery is $\mathrm{E}u(\tilde{c}) - g[u^\star - \mathrm{E}u(\tilde{c})]$. Representing all future utility using a value function v, the discounted present value of all future consumption is $\delta\,\mathrm{E}v(w_2 + Z - \tilde{c})$. Here, w_2 denotes total wealth at the beginning of the next period, δ is the discount factor, and $Z - \tilde{c}$ is the random savings plan implied by consumption plan $\tilde{c}$.

Thus, we get an overall objective function of

$$V(\tilde{c}, u^\star, Z, w_2) = \underbrace{\mathrm{E}u(\tilde{c}) - g[u^\star - \mathrm{E}u(\tilde{c})]}_{1^{\text{st}}\text{-period utility}} + \underbrace{\delta\,\mathrm{E}v(w_2 + Z - \tilde{c})}_{\text{future utitlity}}.$$

[17] In order to model potential effects of varying levels of self-control, a convex self-control cost function is the relevant—and realistic—case to consider (see Fudenberg and Levine, 2006, Section V).

A.2.2. Approximate model

Fudenberg et al. (2014) derive an approximate objective function from this as follows. First, the authors define a "self-control gain function"

$$h[\mathrm{E}u(\tilde{c}) - u^\star] \equiv \mathrm{E}u(\tilde{c}) - u^\star - g[u^\star - \mathrm{E}u(\tilde{c})]$$

which is substituted into the objective function. It captures the effect of exerting self-control on first-period utility. At $\mathrm{E}u(\tilde{c}) = u^\star$, no self-control is exerted, and neither a cost nor a benefit arises. Exerting an additional unit of self-control both increases the cost of self-control, $g[u^\star - \mathrm{E}u(\tilde{c})]$, and lowers $\mathrm{E}u(\tilde{c}) - u^\star$, the expected utility for consumption plan $\tilde{c}$ compared to succumbing to temptation completely, i.e., receiving $u^\star$. The function $h[\cdot]$ is nonpositive, smooth, strictly increasing, and weakly concave, while its argument is nonpositive by definition. Furthermore, it holds that $h'(0) \geq 1$.

Additionally, the authors perform a first-order Taylor approximation of the unknown value function v. It is by virtue of this approximation that the long-run self in the approximate model is completely risk-neutral—instead of only being very close to risk-neutral, as in the original model.

Note that, since the level of pocket cash was chosen optimally in the absence of self-control problems, we know that at $c = 0$ (no incremental consumption), it must hold that $u'(0) = \delta\, v'(w_2)$. This is a useful observation since the unknown expression $\delta\, v'(w_2)$ can be replaced by $u'(0)$.

These two steps lead to the following approximate objective function:

$$\max_{\tilde{c}} U^c(\tilde{c}, u^\star, Z) \ = \ \max_{\tilde{c}} \{h[\mathrm{E}u(\tilde{c}) - u^\star] + u'(0)\,(\mathrm{E}Z - \mathrm{E}\tilde{c})\}.$$

This optimization problem over $\tilde{c}$ is constrained by $c_i \leq z_i$ for $i = 1, 2, \ldots, n$.

Fudenberg et al. (2014)'s main theorem (p. 57) states that this optimization problem over the optimal consumption plan (a vector of dimension n) is equal to an optimization problem where the choice variable is a single threshold, denoted z. All lottery earnings are spent in full for realizations below z, while above z, self-control is exerted, and all earnings beyond z are saved:

$$\begin{aligned} &\max\nolimits_z U(u^\star, Z, z), \text{ where} \\ U(u^\star, Z, z) \ \equiv \ &h[\mathrm{E}u(Z) - u^\star - \mathrm{E}\max\{u(Z) - u(z), 0\}] + u'(0)\,\mathrm{E}\max\{Z - z, 0\}. \end{aligned} \tag{A.1}$$

The optimal z that solves this problem is denoted

$$\hat{z} \ \equiv \ \arg\max_z U(u^\star, Z, z).$$

Note that this value is specific to each lottery and menu, as it depends on both Z and the menu-dependent $u^\star$. Refer to the main theorem in Fudenberg et al. (2014) for proof.

A.2.3. Ranking two-outcome lotteries

The final step in the agent's optimization problem is choosing between lotteries, taking into account the lottery-specific optimal consumption plans as they were derived above. That is, the agent ranks lotteries Z according to $U(u^\star, Z, \hat{z})$. While the preceding derivation was general, the following will be specific to the case that we use in our experiment: pairwise choice between two-outcome lotteries. Let us denote these lotteries as discrete random variables Z^A (with possible realizations z_1^A and z_2^A) and Z^B (with possible realizations z_1^B and z_2^B). Assume $z_1^A \leq z_2^A$ and $z_1^B \leq z_2^B$, without loss of generality.

In our experiment, we test whether subjects' choices, i.e., their pairwise lottery rankings, change in response to an increase in self-control costs due to ego depletion. Formally, such preference reversals come about when the slope of an agent's indifference curve, $\mathrm{d}z_2/\mathrm{d}z_1|_{U=\text{const}}$ (i.e., her willingness to accept a reduction in one payoff of the lottery in exchange for an increase in the second payoff, holding expected utility constant), changes. Thus, to derive predictions about how an increase in self-control costs affects agents' lottery choices, we need to consider the effect of increased self-control costs on the slope of their indifference curves.

Let us denote by $\hat{z}^A$ the optimal cutoff value associated with Lottery Z^A and by $\hat{z}^B$ the optimal cutoff value associated with Lottery Z^B, given that the menu is $\Im = \{Z^A, Z^B\}$. (Remember that each cutoff value, and thus the ranking of the lotteries, is menu-dependent through $u^\star = \max\{\mathrm{E}u(Z^A), \mathrm{E}u(Z^B)\}$.)

The indifference set for a "reference lottery" Z^A, $\mathbb{I}(Z^A)$, is the set of all lotteries Z^B for which the agent is indifferent when given the choice between Z^A and Z^B, i.e., $\mathbb{I}(Z^A) \equiv \{Z^B \mid U(u^\star, Z^A, \hat{z}^A) = U(u^\star, Z^B, \hat{z}^B)\}$. It is implicitly defined by

$$\underbrace{U(u^\star, Z^A, \hat{z}^A) - U(u^\star, Z^B, \hat{z}^B)}_{\equiv \Phi(Z^A, Z^B, u^\star, \hat{z}^A, \hat{z}^B)} = 0.$$

Note that $\Phi(\cdot)$ is a function of z_1^B and z_2^B and the associated probabilities p and $1-p$, respectively—as well as Z^A, $u^\star$, $\hat{z}^A$, and $\hat{z}^B$. To be able to determine the slope of the indifference curve, i.e., $\mathrm{d}z_2^B/\mathrm{d}z_1^B$, we use the implicit function theorem. One of its prerequisites is continuous differentiability of the function $\Phi(\cdot)$ with respect to z_1^B and z_2^B, at least in some neighborhood of the point $Z^A = Z^B$, which is where we calculate the slope. It can be shown that at this point, $\mathrm{d}\Phi/\mathrm{d}u^\star = 0$. In addition, it holds for any Z^A, Z^B that $\mathrm{d}\Phi/\mathrm{d}\hat{z}^A = \mathrm{d}\Phi/\mathrm{d}\hat{z}^B = 0$. This is because $\hat{z}^A$ and $\hat{z}^B$ maximize $U(u^\star, Z^A, z^A)$ and $U(u^\star, Z^B, z^B)$, respectively. Therefore, we only need to consider the dependence of $\Phi(\cdot)$ on z_1^B and z_2^B through the *direct* dependence of $U(u^\star, Z^B, \hat{z}^B)$ on these values, i.e., the partial derivatives $\partial U(u^\star, Z^B, \hat{z}^B)/\partial z_i^B$ with $i = 1, 2$.

Via the implicit function theorem, it holds that

$$\frac{\mathrm{d}\Phi(\cdot)}{\mathrm{d}z_1^B} + \frac{\mathrm{d}\Phi(\cdot)}{\mathrm{d}z_2^B}\frac{\mathrm{d}z_2^B}{\mathrm{d}z_1^B} = 0 \tag{A.2}$$

$$\iff \quad \frac{\mathrm{d}z_2^B}{\mathrm{d}z_1^B} = -\frac{\mathrm{d}\Phi(\cdot)}{\mathrm{d}z_1^B} \Big/ \frac{\mathrm{d}\Phi(\cdot)}{\mathrm{d}z_2^B}$$

$$\implies \quad \frac{\mathrm{d}z_2^B}{\mathrm{d}z_1^B} = -\frac{\partial U(u^\star, Z^B, \hat{z}^B)}{\partial z_1^B} \Big/ \frac{\partial U(u^\star, Z^B, \hat{z}^B)}{\partial z_2^B}. \tag{A.3}$$

Recall that the probability of payoff z_1^B is p and that of z_2^B is $1-p$. Then

$$\begin{aligned}
&U(u^\star, Z^B, \hat{z}^B) \\
&= h\big[\mathrm{E}u(Z^B) - u^\star - \mathrm{E}\max\{u(Z^B) - u(\hat{z}^B), 0\}\big] + u'(0)\,\mathrm{E}\max\{Z^B - \hat{z}^B, 0\} &\text{(A.4)}\\
&= h\big[p\,u(z_1^B) + (1-p)\,u(z_2^B) - u^\star - &\text{(A.5)}\\
&\qquad p\max\{u(z_1^B) - u(\hat{z}^B), 0\} - (1-p)\max\{u(z_2^B) - u(\hat{z}^B), 0\}\big] \\
&\quad + u'(0)\big[p\max\{z_1^B - \hat{z}^B, 0\} + (1-p)\max\{z_2^B - \hat{z}^B, 0\}\big].
\end{aligned}$$

For the derivatives with respect to z_1^B and z_2^B, we get

$$\frac{\partial U(u^\star, Z^B, \hat{z}^B)}{\partial z_1^B} =$$
$$\begin{cases} h'\left[\mathrm{E}u(Z^B) - u^\star - (1-p)\max\{u(z_2^B) - u(\hat{z}^B), 0\}\right] p\, u'(z_1^B) & \text{if } z_1^B < \hat{z}^B \\ u'(0)\, p & \text{if } z_1^B > \hat{z}^B \end{cases}$$

and

$$\frac{\partial U(u^\star, Z^B, \hat{z}^B)}{\partial z_2^B} =$$
$$\begin{cases} h'\left[\mathrm{E}u(Z^B) - u^\star - p\max\{u(z_1^B) - u(\hat{z}^B), 0\}\right] (1-p)\, u'(z_2^B) & \text{if } z_2^B < \hat{z}^B \\ u'(0)\,(1-p) & \text{if } z_2^B > \hat{z}^B \end{cases}.$$

A.2.4. Case distinctions

Since the following reasoning applies to arbitrary two-outcome lotteries Z, we now drop the superscript B. The dependence of utility on the cutoff $\hat{z}$ implies that when calculating the slopes of the indifference curves that describe preferences over two-outcome lotteries, we need to distinguish three cases. The three cases are also illustrated graphically in Fig. A1. Remember that temptation $u^\star$ is menu-dependent but identical for both lotteries, while the threshold $\hat{z}$ is menu-dependent (through $u^\star$) and at the same time lottery-specific. Hence, whenever it holds that $z_1 < \hat{z} < z_2$ (the 3rd case below) for at least one of the two lotteries, self-control affects the curvature of the indifference curves and, thus, the agent's risk attitudes when she chooses among two lotteries.

1st case: $\max\{z_1, z_2\} \leq \hat{z}$. In this case, the short-run self spends all additional income. Hence, the slope of the indifference curve is

$$\frac{\mathrm{d}z_2}{\mathrm{d}z_1} = -\frac{h'\left[\mathrm{E}u(Z) - u^\star\right] p\, u'(z_1)}{h'\left[\mathrm{E}u(Z) - u^\star\right](1-p)\, u'(z_2)} = -\frac{p\, u'(z_1)}{(1-p)\, u'(z_2)}. \tag{A.6}$$

Thus, in the 1st case, the combined preferences of the two selves correspond to those of the short-run self, i.e., risk aversion. If all lotteries in the menu fall into this category, they are ranked according to $\mathrm{E}u(Z)$, expected utility with the short-run self's degree of risk aversion.

2nd case: $\hat{z} \leq \min\{z_1, z_2\}$. In this case, the amount that the short-run self is permitted by the long-run self to spend in addition to its initial allowance is smaller than all of the lottery outcomes. Thus, the short-run self derives the same utility from all outcomes, and the agent's combined preferences over lotteries correspond to those of the long-run self. Consequently, the slope of the indifference curve is

$$\frac{\mathrm{d}z_2}{\mathrm{d}z_1} = -\frac{p\, u'(0)}{(1-p)\, u'(0)} = -\frac{p}{1-p}. \tag{A.7}$$

Thus, if all lotteries in the menu fall into this category, the agent behaves in a risk-neutral manner, and lotteries are ranked according to their expected value $\mathrm{E}(Z)$.

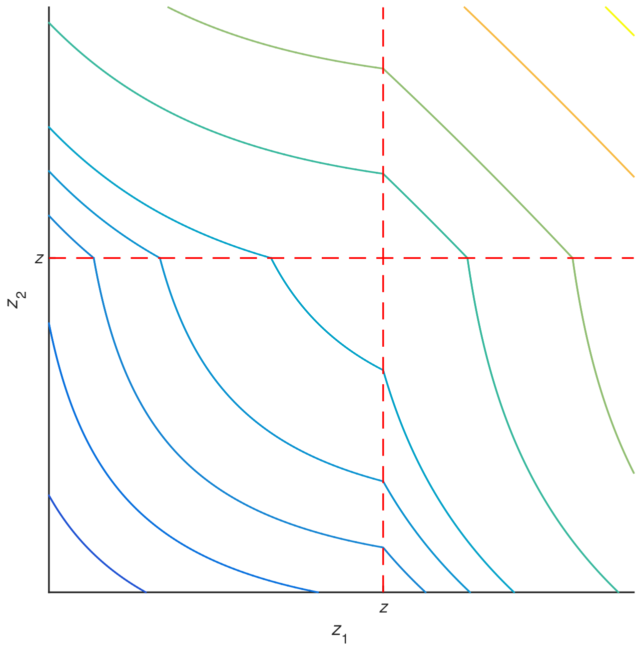


Fig. A1. Illustration of the three cases.

Notes: Displayed are indifference curves over two-outcome lotteries with payoffs z_1 and z_2, associated probabilities $p_1 = p_2 = 0.5$, and cutoff z. The agent's indifference curves are linear for $z_1 > z$ and $z_2 > z$, while they are concave elsewhere. For $z_1 < z$ and $z_2 < z$, the curvature is strongest. Note that this graph only serves to illustrate the rationale of (A.6), (A.7), and (A.8). The optimal cutoff $\hat{z}$ is lottery-dependent, and z will thus differ depending on which lottery is considered.

3rd case: $z_1 < \hat{z} < z_2$. In this case, one outcome is below and the other is above the cutoff $\hat{z}$, so that the slope of the indifference curves depends on $h'[\cdot]$,

$$\frac{\mathrm{d}z_2}{\mathrm{d}z_1} = -\frac{h'\left[p\,u(z_1) - u^\star + (1-p)\,u(\hat{z})\right]\,p\,u'(z_1)}{(1-p)\,u'(0)}. \tag{A.8}$$

Only in this 3rd case does the slope of the self-control gain function enter the slope of the indifference curves.

Consequently, only in the 3rd case does the slope of the indifference curves change under self-control depletion, such that depletion can lead to changes in lottery choices, i.e., measured risk attitudes.

A.3. Depletion: model predictions and hypotheses

A.3.1. Incorporating different levels of self-control

We now apply the model's predictions for two-outcome lotteries to derive specific hypotheses concerning the effects of self-control depletion. We incorporate depletion and the resulting increase in marginal self-control costs into the model by defining different self-control cost functions $g^{\mathrm{ND}}[\cdot]$ and $g^{\mathrm{D}}[\cdot]$ for the nondepleted and the depleted state, respectively.

Recall that a greater $u^\star - \mathrm{E}u(\tilde{c})$ denotes a greater amount of exerted self-control. Also recall that $u^\star - \mathrm{E}u(\tilde{c}) \geq 0$, $g[u^\star - \mathrm{E}u(\tilde{c})] \geq 0$, and $g[0] = 0$. We assume that

$$g^{\mathrm{ND}}[u^\star - \mathrm{E}u(\tilde{c})] \;\leq\; g^{\mathrm{D}}[u^\star - \mathrm{E}u(\tilde{c})] \;\text{ for all }\; u^\star - \mathrm{E}u(\tilde{c}).$$[18]

The "self-control gain function" was defined as $h[\mathrm{E}u(\tilde{c}) - u^\star] \equiv \mathrm{E}u(\tilde{c}) - u^\star - g[u^\star - \mathrm{E}u(\tilde{c})]$; hence, $h[\mathrm{E}u(\tilde{c}) - u^\star] \leq 0$ and $h[0] = 0$. Thus, with $h^{\mathrm{ND}}[\mathrm{E}u(\tilde{c}) - u^\star] \equiv \mathrm{E}u(\tilde{c}) - u^\star - g^{\mathrm{ND}}[u^\star - \mathrm{E}u(\tilde{c})]$ and $h^{\mathrm{D}}[\mathrm{E}u(\tilde{c}) - u^\star] \equiv \mathrm{E}u(\tilde{c}) - u^\star - g^{\mathrm{D}}[u^\star - \mathrm{E}u(\tilde{c})]$, we have

$$h^{\mathrm{ND}}[\mathrm{E}u(\tilde{c}) - u^\star] \;\geq\; h^{\mathrm{D}}[\mathrm{E}u(\tilde{c}) - u^\star] \;\text{ for all }\; \mathrm{E}u(\tilde{c}) - u^\star.$$

The function $g[u^\star - \mathrm{E}u(\tilde{c})]$ was assumed to be weakly convex. Therefore, $g^{\mathrm{ND}}[\cdot] \leq g^{\mathrm{D}}[\cdot]$ implies $g^{\mathrm{ND}\prime}[\cdot] \leq g^{\mathrm{D}\prime}[\cdot]$. It follows that $h[\mathrm{E}u(\tilde{c}) - u^\star]$ is weakly concave and that

$$h^{\mathrm{ND}\prime}[\mathrm{E}u(\tilde{c}) - u^\star] \;\leq\; h^{\mathrm{D}\prime}[\mathrm{E}u(\tilde{c}) - u^\star] \;\text{ for all }\; \mathrm{E}u(\tilde{c}) - u^\star.$$

Intuitively, an increase in the marginal cost of self-control affects optimal choice by increasing the relative importance of the self-control costs in the current period compared to the benefit of saving for future periods. In other words, the short-run self's interest to consume right now becomes more important. This has two effects. The first is immediately apparent from (A.8). When plugging in a higher value for $h'[\cdot]$ in (A.8), the slope of the indifference curve becomes steeper. Thus, the agent's combined risk attitudes exhibit more risk aversion. The second effect of an increase in marginal self-control costs is that $\hat{z}$ increases for each lottery. A higher $\hat{z}$ implies that some lotteries will be evaluated by (A.6) that were formerly evaluated by (A.8) and some lotteries will be evaluated by (A.8) that were formerly evaluated by (A.7). Both effects result in increased risk aversion.

A.3.2. Hypotheses

For one or both of these mechanisms to affect choices, at least one of the two lotteries needs to be such that (A.8) applies in at least one of the states (depletion or nondepletion). This leads us to our first hypothesis:

Hypothesis 1. *Ego depletion leads to greater risk aversion for choices between lotteries if at least one of the lotteries contains a small payoff below and another larger payoff above a cutoff value $\hat{z}$.*

Our second hypothesis refers to the case in which one of the lotteries is a sure payoff:

Hypothesis 2. *The effect of ego depletion (i.e., increased risk aversion) is stronger when one "lottery" is a sure payoff.*

If the per-period utility function is concave, a sure payoff leads to higher short-run utility than a lottery with the same expected value. A sure payoff is, thus, more tempting. (See also Fudenberg and Levine, 2011, pp. 35, 46, 66.) With a more tempting reference lottery, the function $g[u^\star - \mathrm{E}u(\tilde{c})]$ is evaluated at a higher level $u^\star - \mathrm{E}u(\tilde{c})$ than for a less tempting reference lottery. Consequently, $h[\mathrm{E}u(\tilde{c}) - u^\star]$ and $h'[\mathrm{E}u(\tilde{c}) - u^\star]$ are evaluated for more negative $\mathrm{E}u(\tilde{c}) - u^\star$. As $h[\mathrm{E}u(\tilde{c}) - u^\star]$ is weakly concave, the difference in the slopes of the

[18] This is conceptually very similar to the way in which Fudenberg and Levine (2006, Section V) incorporate cognitive load in their original model, by adding an amount d to $u^\star - \mathrm{E}u(\tilde{c})$.

indifference curves under depletion and nondepletion will be larger with a riskless reference lottery than with a risky reference lottery. Again, this holds only if for at least one of the two lotteries in the menu, one lottery outcome is below and the other is above the cutoff $\hat{z}$.

Our third hypothesis serves to differentiate the dual-self model from other models that potentially make similar predictions as Hypotheses 1 and 2. It does so by outlining a situation in which changes in self-control should show *no* effect according to the model, while, for instance, increased reliance on heuristics would generate an effect.

Hypothesis 3. *When payoffs are delayed, ego depletion has no effect.*

The intuition for Hypothesis 3 is provided in the main text.

A.4. Choosing between multiple two-outcome lotteries

The derivation above assumes that subjects face a single lottery choice only—or that they consider each choice among repeated choices in isolation. In our case, this would mean that subjects conceive of each choice in each row of each choice list as a separate choice, thus making 85 separate choices between two two-outcome lotteries. We call such behavior "*isolating*" (borrowing the terminology of Cubitt et al., 1998).

In our experiment, we used repeated choices in combination with the random lottery incentive mechanism (RLIM; also called "random problem selection" or "RPS" mechanism) to remunerate subjects. This means that each offered lottery is in fact part of a compound lottery, with the superordinate lottery given by the RLIM. Subjects might thus perceive the two lotteries offered in each row just as that: as the second stages of a compound lottery. In that case, choice is not repeated pairwise choice between two-outcome lotteries anymore but instead amounts to selecting one out of the available multioutcome compound lotteries.

To give an example, consider a subject who conceives of determining the switching row in our Choice List A (which featured 25 rows) as one single choice. In the terminology of Fudenberg et al. (2014), this subject selects one particular compound lottery Z out of a choice set $\Im$ of 25 compound lotteries.[19] Thus, Z is a multioutcome lottery, and the choice amounts to selecting one out of 25 elements in $\Im$. Henceforth, we will call this behavior "*integrating*".

The question arises whether the predictions regarding the treatment effect still hold when subjects do not isolate choices but integrate them into a compound lottery. As Azrieli et al. (2017, p. 10) state, it "is well known that the RPS mechanism is incentive compatible when all admissible extensions satisfy the expected utility axioms." However, since the Fudenberg–Levine model was developed to explain, among other phenomena, deviations from expected utility, we need to analyze the model's predictions regarding repeated pairwise lottery choice for integrating decision makers.

Essentially, the same logic as outlined in Section A.2.3—where z_1^B and z_2^B were the only payoffs in a two-outcome lottery—applies to any pair of payoffs included in a higher-dimensional compound lottery. A subject who integrates pairwise choices into a compound lottery still has to trade off the involved outcomes. Crucially, an integrating subject has to do this for each pairwise choice between the two-outcome lotteries that ultimately comprise the high-dimensional compound lotteries. This is due to the structure of

[19] The 26th option of always choosing the lottery on the right-hand side of the choice list contains a dominated compound lottery, so that choosing it is not consistent with the Fudenberg et al. (2014) model.

our choice lists, which all feature first-order stochastically dominating alternatives from row to row. Since the dual-self model respects stochastic dominance (see Fudenberg et al., 2014, Proposition 4), for our choice lists, the preference order over the compound lotteries is unambiguously determined by the dual-self's risk attitudes over pairwise choices. Hence, also an integrating subject has to decide which of the two two-outcome lotteries s/he prefers in every row—just like an isolating subject.

Therefore, all arguments concerning two-outcome lotteries and binary choices (as isolating subjects would perceive the choice lists) are also valid for a multialternative choice with multioutcome lotteries (as integrating subjects would view the decision problem).

While this shows that integrating and isolating subjects' decisions rely on the same basic logic, it does not imply that they make identical choices. Recall that both temptation utility $u^\star$ and the cutoff $\hat{z}$ are menu-dependent. Hence, an integrating subject potentially has different values for $u^\star$ and $\hat{z}$ than an isolating subject. Nevertheless, they will be quantitatively similar, as we argue in the following. For an integrating subject, the short-run self's temptation utility $u^\star$ is the highest achievable expected utility of the compound lottery. This expected utility is given by a weighted sum of the highest achievable expected utility in each row—i.e., it is a weighted sum of the various temptation utilities of an isolating subject. Hence, $u^\star$ and, consequently, $\hat{z}$ will have similar values independent of whether subjects view a choice list as one decision or as several decisions. Thus, while the predictions of the model, while not identical, will be quantitatively similar for both types of subjects.

Another difference between integrating and isolating subjects may arise from the fact that probabilities of the two-outcome lotteries are downweighted by integrating subjects, because integrators take into account that each row is only chosen with a probability of $1/n^{\text{int}}$ (e.g., $n^{\text{int}} = 85$ for the entire experiment or $n^{\text{int}} = 25$ for Choice List A). For integrating subjects, the associated probabilities of the outcomes in Eq. (A.5) are $(1/n^{\text{int}})\,p$ and $(1/n^{\text{int}})\,(1-p)$, respectively. As argued above, the rest of the calculus remains the same because the choice of the compound lottery reduces to pairwise choice per row. The factor $1/n^{\text{int}}$ appears in both the numerator and the denominator in Eqs. (A.6), (A.7), and (A.8), and thus cancels out. Only in determining where the marginal self-control gain function $h'[\cdot]$ is evaluated in Eq. (A.8) does it play a role.

Summing up, this means that although isolators and integrators may behave in *quantitatively* different ways, the prediction regarding the *qualitative* effect of decreased self-control stays the same. This is because integrating subjects ultimately face a pairwise choice per row of the choice lists. Hence, while the dual-self model suggests that repeated choices under the RLIM lead to different expressed preferences than a single choice, the predicted *treatment effect* for our experiment is qualitatively the same, regardless of whether subjects isolate decisions or integrate them into a compound lottery.

A.5. Operationalization of the model

As a final aspect, any empirical investigation of the Fudenberg–Levine model requires making an assumption about timing that the model is silent about. In the model, the decision maker's choice of an option with stochastic outcomes, the realization of the outcome, and the subsequent consumption decision (determined by the interplay of the short-run and the long-run self) all happen instantaneously. This entails that self-control when making the lottery choice and when making the consumption decision are identical.

In reality, the simultaneity of the lottery choice and the consumption decision is unavoidably violated: consumption will always occur later than the lottery choice—in an experiment, subjects usually have to postpone it until after leaving the lab. Hence, the question becomes important whether self-control capacity during the lottery choice or during the consumption decision should be manipulated.

If one advocates that depletion should be applied at the time of the consumption decision, one has to assume that decision makers forecast their self-control capacity for the consumption decision and make their lottery choice based on the anticipated self-control capacity (an assumption which is not part of the original model). In contrast to this, Fudenberg and Levine (2011) and Fudenberg et al. (2014) consider self-control at the time of the lottery choice the relevant factor. Fudenberg and Levine (2011, pp. 65/66) refer to experimental evidence by Benjamin et al. (2013) regarding the influence of *concurrent* cognitive load on lottery choices, and Fudenberg et al. (2014, p. 66) explicitly advocate the use of ego depletion and/or cognitive load to test their theory: "This means that the theory implies that reversals … can be induced by increasing cognitive load". Please note that cognitive load is an even more short-term manipulation than ego depletion (see Baumeister and Vohs, 2016b, pp. 70/71). A possible justification for why self-control at the time of lottery choices matters would be that even though actual consumption occurs only later, subjects make their consumption plan already at the time of the lottery choice (e.g., "If I receive the €x payoff, I will stop by the cafeteria after leaving the lab and buy myself a piece of cake").

We follow Fudenberg and Levine (2011) and Fudenberg et al. (2014) in their assessment of the relevant point in time of self-control when deriving our hypotheses from the simplified model by Fudenberg et al. (2014).

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Online Appendix A. Categorization of behavior

The switching row in each of the four choice lists measures an individual's risk attitude. More precisely, differences in expected values of the less risky lottery and its mean-preserving spread at the switching row measure an individual's "risk premium" $m_{\sim}$ that has to be added to the riskier lottery to make that subject indifferent between the two lotteries. We calculate this risk premium as the average difference of the expected values in the two rows around the switching point; i.e., a subject who chooses the more risky lottery when the difference in expected values is €2.60 but switches to the less risky lottery when it is €2.50 is assigned a risk premium $m_{\sim} = €2.55$.

Based on these indifference-generating risk premia, we classify subjects' behavior into four categories: *risk-seeking*, *risk-neutral*, *risk-averse*, and *dominated choices*. The behavior of subjects whose risk premium is positive is classified as risk-averse, while a risk premium of zero implies risk neutrality, and a negative risk premium risk proclivity.[20] 3% of choices are in favor of the dominated lottery in the first row. They are excluded from the further analysis, since dominated choices are incompatible with any deterministic model

Table A1
Categorization of behavior.

	Depletion		Control		Combined	
	Frequency	%	Frequency	%	Frequency	%
Choice List A: Risky/Risky						
Risk-seeking	11	7.2	8	5.1	19	6.2
Risk-neutral	15	9.9	7	4.5	22	7.1
Risk-averse	124	81.6	138	88.5	262	85.1
Dominated choices	2	1.3	3	1.9	5	1.6
Choice List B: Safe/Risky						
Risk-seeking	10	6.6	11	7.1	21	6.8
Risk-neutral	12	7.9	10	6.4	22	7.1
Risk-averse	119	78.3	127	81.4	246	79.9
Dominated choices	11	7.2	8	5.1	19	6.2
Choice List C: "Long Shot"						
Risk-seeking	70	46.1	72	46.2	142	46.1
Risk-neutral	9	5.9	1	0.4	10	3.3
Risk-averse	68	44.7	75	48.1	143	46.4
Dominated choices	5	3.3	8	5.1	13	4.2
Choice List D: Delayed Payoffs						
Risk-seeking	9	5.9	4	2.6	13	4.2
Risk-neutral	9	5.9	8	5.1	17	5.5
Risk-averse	133	87.5	141	90.4	274	89.0
Dominated choices	1	0.7	3	1.9	4	1.3

[20] As we observe switching points instead of points of indifference, we cannot technically observe risk neutrality. We, thus, classify subjects as risk-neutral who switch at or immediately after the risk-neutral row ($m_{\sim} = €0.05$ or $m_{\sim} = -€0.05$). Subjects with $m_{\sim} > €0.05$ are classified as risk-averse and subjects with $m_{\sim} < -€0.05$ as risk-seeking.

of decision making under risk.[21] We decided to abstract from possible stochastic components in decision-making since the already rather complex "dual-self" models that we test abstract from them as well.

Table A1 displays the absolute and relative frequencies of choices in the choice lists.

Online Appendix B. Translated instructions for the depletion [control] group

General explanations

Welcome to this economic experiment.

In the course of this experiment you can earn a nonnegligible amount of money. The exact amount strongly depends on your decisions. So please read the following instructions carefully! If you have any questions, please raise your hand and we will come to your seat.

During the whole experiment it is not allowed to talk to other participants, to use cell phones, or to launch any other programs on the computer. Disregarding any of these rules will lead to your exclusion from the experiment and from all payments.

In principle, the earnings resulting from your decisions will be paid out to you in cash at the end the experiment. Only in an exceptional case, you will receive your money later, either in cash or via a bank transfer according to which you choose. (More on that will be announced in a moment.)

On the following pages, we will describe the exact experimental procedure.

The experiment: your decisions

In this experiment you will make 85 different decisions, each between two alternatives: A and B. Each of these two alternatives is a lottery. Here is an example of such a lottery: With a probability of 50% you win €9 and with a probability of 50% you win €12. Winning probabilities and the amounts in euro that you can win will vary between decisions.

The 85 decisions are summarized in four large tables, with about 20 rows each. Each row represents one decision. The four tables will be shown to you on four subsequent decision screens.

This is a decision screen with one such table. This table only serves as an example and is, therefore, shortened to five rows.

Please choose whether you prefer Alternative A or B for every row by checking the respective option with your mouse. Alternative B becomes either more or less attractive when moving from the top to the bottom, depending on the table. Therefore, the respective rows are filled out automatically, as soon as you have switched from Alternative A to B, or from Alternative B to A, for the first time.

As long as you have not hit the "continue" button, you can still change your decisions. Once you have made all decisions in one of the large tables, please click the "continue" button in

[21] We do not exclude these subjects altogether but just their choices for specific lotteries. Our results are robust to excluding those subjects altogether.

Bitte wählen Sie in jeder Zeile eine Alternative aus.

Alternative A		Alternative B
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	7.00 € mit 50 % oder 23.00 € mit 50 %
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	6.50 € mit 50 % oder 22.50 € mit 50 %
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	6.00 € mit 50 % oder 22.00 € mit 50 %
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	5.50 € mit 50 % oder 21.50 € mit 50 %
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	5.00 € mit 50 % oder 21.00 € mit 50 %

Fig. B1. Screenshot of an example choice list ("table") that subjects were given as part of the written instructions.

Translation of the depicted table: "Please choose one alternative in each row. Alternative A [first row:] €4.00 with 50% or €19.00 with 50%. Alternative B [first row:] €7.00 with 50% or €23.00 with 50%."

the lower right corner of the screen. You will then see the next decision screen containing another large table.

Your payment from the experiment is determined in a two-step process: In Step 1, **one of your 85 decisions** (i.e., one row from one of the four tables) will be drawn randomly. This is the only decision that will affect your payment. That means you should make your decision in every single row as if it were your only decision. All decisions are drawn with the same probability (1/85).

For the drawn decision, it is determined whether you selected alternative A or B. In Step 2 the lottery you have chosen is played, determining your payment. An example: Assume decision 4 from the table shown above is drawn in Step 1. Alternative B was chosen in decision 4. In Step 2 it is—according to the lottery—randomly determined whether you receive €5.50 or €21.50. In this example, the payoffs of €5.50 and €21.50 are equally likely (both have a probability of 50%).

Following those four large tables we will show you additional, smaller tables. The purpose of those smaller tables is to learn about your decisions in more detail. You will receive a more detailed description and explanation regarding these tables on your screen during the course of the experiment.

Further tasks

Before you make your decision (as described above), there are two additional tasks to be completed. It is very important for the experiment that you make an effort to complete the tasks diligently and correctly. For each task, you will be handed out a sheet of paper containing text that you should work on. We will collect both sheets of paper at the end of the experiment. Moreover, you will receive private feedback about your performance in the two tasks on screen at the end of the experiment.

First task

You will receive a first sheet of paper containing text. **Please cross out each instance of the letter "e" (including "E") in the text.** Start the task with working on the first paragraph and continue paragraph by paragraph.

You have 3 minutes to work on this task. Rather work conscientiously on few paragraphs than try to work on many paragraphs. The time remaining for the task is shown in the upper right corner of your screen.

Second task

After having finished the first task, you will receive a second sheet of paper containing text. **Now you have to cross out each instance of the letter "e" according to the following set of rules:**

Generally, you cross out the letter "e"; there are, however, the following exceptions:

(a) there is a vowel in the text after the letter "e", or
(b) there is a vowel in the text two letters after the letter "e", or
(c) there is a vowel in the text two letters in front of the letter "e".

If there is a vowel directly in front of the "e" (as, for instance, in case of "circa **e**lf"), the "e" is to be crossed out.

In counting letters, disregard full stops, commas, hyphens, or spaces. Vowels comprise: "a", "ä", "e", "i", "o", "ö", "u", "ü".

The following schematic representation summarizes the rules:

_ _ **e** _ _
1 2   3 4

Cross out all instances of "e" in principle. Exceptions: Do not cross out the "e" if there is a vowel at position 1, 3, or 4.

As in the first task, please start with the first paragraph and continue paragraph by paragraph.

Second task [control group]

After having finished the first task, you will receive a second sheet of paper containing text. ***Please cross out each instance of the letter "e" (including "E") in the text again.*** *This is the same instruction as in the first task. As in the first task, please start with the first paragraph and continue paragraph by paragraph.*

You have 7 minutes to work on this task. Rather work conscientiously on few paragraphs than try to work on many paragraphs. The time remaining for the task is shown in the upper right corner of your screen.

Following these two tasks, you will make the 85 decision described previously.

Training and comprehension questions

Before you start working on both tasks, we ask you to answer a few training questions regarding the decisions. Answering those questions will make it easier to acquaint yourself with the decision situation.

At the end of today's experiment—following your decisions—there are a few screens with questions and the like, before the money you earned is paid out.

In case you have any questions—now or while working on the training tasks—please raise your hand. We will come to your seat to answer your questions.

Please do not ask any questions aloud!

Online Appendix C. German original of the instructions and text of the depletion task

EXPERIMENT AM 28. OKTOBER 2014

Allgemeine Erklärungen

Wir begrüßen Sie herzlich zu diesem wirtschaftswissenschaftlichen Experiment.

Im Rahmen dieses Experiments können Sie eine nicht unerhebliche Summe Geld verdienen. Wie viel Geld Sie verdienen, hängt dabei maßgeblich von Ihren Entscheidungen ab. Lesen Sie die folgenden Erklärungen daher bitte gründlich durch! Wenn Sie Fragen haben, strecken Sie bitte Ihre Hand aus der Kabine – wir kommen dann zu Ihrem Platz.

Während des Experiments ist es nicht erlaubt, mit den anderen Experimentteilnehmern zu sprechen, Mobiltelefone zu benutzen oder andere Programme auf dem Computer zu starten. Die Nichtbeachtung dieser Regeln führt zum Ausschluss aus dem Experiment und von allen Zahlungen.

Grundsätzlich bekommen Sie das Einkommen aus Ihren Entscheidungen am Ende dieses Experiments bar ausbezahlt. Nur im Ausnahmefall erfolgt die Auszahlung später, und zwar wahlweise in bar oder per Banküberweisung. (Näheres dazu erfahren Sie gleich.)

Auf den nächsten Seiten beschreiben wir den genauen Ablauf des Experiments.

Das Experiment: Ihre Entscheidungen

Sie treffen im Rahmen dieses Experiments 85 verschiedene Entscheidungen zwischen jeweils zwei Alternativen: A und B. Jede der zwei Alternativen ist eine Lotterie. Ein Beispiel für eine solche Lotterie ist: Mit einer Wahrscheinlichkeit von 50 % gewinnen Sie 9 Euro, und mit einer Wahrscheinlichkeit von 50 % gewinnen Sie 12 Euro. Die Gewinnwahrscheinlichkeiten und die Euro-Beträge, die Sie gewinnen können, variieren zwischen den verschiedenen Entscheidungen.

Die 85 Entscheidungen sind in vier großen Tabellen mit jeweils ungefähr 20 Zeilen zusammengefasst. Jede Zeile einer Tabelle entspricht dabei einer Entscheidung. Die vier Tabellen werden Ihnen nacheinander auf vier verschiedenen Entscheidungsbildschirmen angezeigt.

1

Hier sehen Sie einen Entscheidungsbildschirm mit einer solchen Tabelle. Diese Tabelle soll nur als Beispiel dienen und ist daher auf fünf Zeilen verkürzt:

Bitte wählen Sie in jeder Zeile eine Alternative aus.

Alternative A		Alternative B
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	7.00 € mit 50 % oder 23.00 € mit 50 %
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	6.50 € mit 50 % oder 22.50 € mit 50 %
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	6.00 € mit 50 % oder 22.00 € mit 50 %
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	5.50 € mit 50 % oder 21.50 € mit 50 %
4.00 € mit 50 % oder 19.00 € mit 50 %	Alternative A ○ ◉ Alternative B	5.00 € mit 50 % oder 21.00 € mit 50 %

Für jede Zeile klicken Sie bitte mit der Maus an, ob Sie Alternative A oder B bevorzugen. Alternative B wird – je nach Tabelle – von oben nach unten immer attraktiver oder weniger attraktiv. Daher füllen sich die entsprechenden Zeilen einer Tabelle automatisch aus, sobald Sie einmal von Alternative A zu Alternative B oder einmal von Alternative B zu Alternative A gewechselt haben.

Solange Sie nicht den „Weiter“-Knopf drücken, können Sie Ihre Entscheidungen noch ändern. Sobald Sie alle in einer großen Tabelle zusammengefassten Entscheidungen gefällt haben, drücken Sie bitte den „Weiter“-Knopf unten rechts auf dem Bildschirm. Es wird Ihnen dann der nächste Entscheidungsbildschirm mit einer weiteren großen Tabelle angezeigt.

Ihre Auszahlung aus dem Experiment wird in einem zweischrittigen Prozess bestimmt: Im 1. Schritt wird **eine Ihrer 85 Entscheidungen** (d. h. eine einzige Zeile aus einer der vier Tabellen) zufällig ausgelost. Nur diese eine Entscheidung bestimmt Ihre Auszahlung. Dies bedeutet, dass Sie Ihre Entscheidung in jeder Zeile so treffen sollten, als wäre es Ihre einzige Entscheidung. Alle Entscheidungen werden mit derselben Wahrscheinlichkeit (also 1/85) ausgelost.

Für die ausgeloste Entscheidung wird festgestellt, ob Sie Alternative A oder B angeklickt haben. Im 2. Schritt wird nun für die von Ihnen angeklickte Alternative die entsprechende Lotterie ausgespielt und dadurch Ihre Auszahlung bestimmt.

Ein Beispiel: Nehmen Sie an, dass in der oben abgebildeten Tabelle in Schritt 1 Entscheidung 4 ausgelost wurde. In Entscheidung 4 wurde Alternative B angeklickt. In Schritt 2 wird nun ausgelost, ob Sie 5,50 Euro oder 21,50 Euro ausgezahlt bekommen. In diesem Beispiel sind die Auszahlungen 5,50 Euro und 21,50 Euro gleich wahrscheinlich (beide haben eine Wahrscheinlichkeit von 50 %).

Nach den vier großen Tabellen werden wir Ihnen noch weitere, kleinere Tabellen vorlegen. Der Zweck dieser kleineren Tabellen ist, dass wir Ihre Entscheidung noch genauer erfragen möchten. Die Details hierzu bekommen Sie im Laufe des Experiments auf dem Computerbildschirm angezeigt und erklärt.

Ihre weiteren Aufgaben

Bevor Sie Ihre Entscheidungen – wie oben beschrieben – treffen, haben Sie noch zwei Aufgaben zu erledigen. Für das Experiment ist es sehr wichtig, dass Sie die Aufgaben gründlich und korrekt bearbeiten.

Für jede Aufgabe teilen wir ein Blatt mit Text aus, das Sie bearbeiten sollen. Am Ende des Experiments sammeln wir beide von Ihnen bearbeiteten Blätter ein. Außerdem erhalten Sie am Ende des Experiments auf dem Computerbildschirm ein privates Feedback, wie gut Sie beide Aufgaben bearbeitet haben.

Erste Aufgabe

Sie bekommen von uns das erste Blatt mit Text ausgehändigt. **Ihre Aufgabe besteht darin, in diesem Text sämtliche Buchstaben ,,e'' (gilt auch für ,,E'') durchzustreichen**. Beginnen Sie bitte mit dem ersten Absatz und gehen Sie danach absatzweise vor.

Insgesamt haben Sie für diese erste Aufgabe 3 Minuten Zeit. Arbeiten Sie lieber gründlich an weniger Absätzen, als mit der Bearbeitung vieler Absätze anzufangen. Oben rechts auf dem Computerbildschirm wird Ihnen die verbleibende Arbeitszeit angezeigt.

Zweite Aufgabe

Nachdem Sie die erste Aufgabe erledigt haben, teilen wir ein zweites Blatt mit Text aus. **Jetzt müssen Sie den Buchstaben ,,e'' nach folgenden neuen Regeln durchstreichen:**

Prinzipiell streichen Sie das „e“ durch, allerdings gibt es folgende Ausnahmen:

a) im Text folgt nach dem „e“ ein Vokal oder
b) im Text folgt ein Vokal im Abstand von zwei Buchstaben nach dem „e“ oder
c) im Text steht ein Vokal im Abstand von zwei Buchstaben vor dem „e“.

Wenn ein Vokal direkt vor dem „e“ steht (wie zum Beispiel im Fall von „zirka **e**lf“), ist das „e“ durchzustreichen.

Bei der Abzählung sind Satzzeichen wie Punkt und Komma sowie Leerzeichen und Bindestriche nicht zu beachten. Vokale sind: „a“, „ä“, „e“, „i“, „o“, „ö“, „u“, „ü“.

Die folgende schematische Darstellung fasst die Regeln zusammen:

```
_ _ e _ _
1 2   3 4
```

Prinzipiell streichen Sie alle ,,e'' durch. Ausnahmen: Sie streichen ein ,,e'' nicht durch, wenn auf der Position 1, 3 oder 4 ein Vokal steht.

Beginnen Sie bitte wieder – wie in der ersten Aufgabe – mit dem ersten Absatz und gehen Sie danach absatzweise vor.

3

Description of the “second task” in the control group:

Zweite Aufgabe

Nachdem Sie die erste Aufgabe erledigt haben, teilen wir ein zweites Blatt mit Text aus. **Bitte streichen Sie in diesem Text wieder alle Buchstaben ,,e'' durch.** Dies entspricht der Aufgabenstellung aus der ersten Aufgabe. Beginnen Sie bitte wieder mit dem ersten Absatz und gehen Sie danach absatzweise vor.

Insgesamt haben Sie für diese zweite Aufgabe 7 Minuten Zeit. Arbeiten Sie auch hier lieber gründlich an weniger Absätzen, als mit der Bearbeitung vieler Absätze anzufangen. Oben rechts auf dem Computerbildschirm wird Ihnen die verbleibende Arbeitszeit angezeigt.

Im Anschluss an die beiden Aufgaben treffen Sie Ihre anfangs beschriebenen 85 Entscheidungen.

Übungsaufgaben und Verständnisfragen

Bevor Sie mit der Bearbeitung der beiden Aufgaben beginnen, bitten wir Sie, am Computer einige Übungsfragen zu den Entscheidungen zu beantworten. Das Beantworten dieser Fragen soll es Ihnen erleichtern, sich mit der Entscheidungssituation vertraut zu machen.

Gegen Ende des heutigen Experiments – im Anschluss an Ihre Entscheidungen – folgen dann noch einige Bildschirme mit Fragen u. Ä., bevor wir zur Auszahlung des von Ihnen verdienten Geldes kommen.

Falls Sie jetzt Fragen haben oder während der Beantwortung der Übungsfragen Ihrerseits Fragen entstehen, halten Sie bitte die Hand aus der Kabine. Ein Leiter des Experiments wird dann an Ihren Platz kommen, um Ihre Fragen zu beantworten. **Stellen Sie Fragen keinesfalls laut!**

28.10.2014   12:00 Uhr   Kabine: _________

Aufgabe 1

Bitte streichen Sie alle Buchstaben „e“ (auch Großbuchstaben „E“) durch.
Bitte arbeiten Sie 3 Minuten an dieser Aufgabe.

Die Anwendung des statistischen Methodenspektrums auf reale Problemstellungen aus Wissenschaft und Praxis bedeutet in der Regel die Bearbeitung einer großen Fülle von Primärdaten, die wiederholte Behandlung eines Datensatzes mit verschiedenen Methoden oder die mehrmalige Anwendung einer Methode auf unterschiedliche Datensätze. Der damit verbundene Rechenaufwand ist insbesondere bei den ökonometrischen Verfahren und bei der Zeitreihenanalyse sehr

umfangreich und zwingt zum Einsatz elektronischer Datenverarbeitungsanlagen. Um Forschern und Praktikern den Einsatz statistischer Verfahren zu erleichtern, wurden leistungsfähige Software-Produkte entwickelt, die einem permanenten Wandel unterliegen. Waren früher die Entwicklungen auf Großrechnern dominierend, so gewannen inzwischen die Software-Pakete auf Workstations zunehmend an Bedeutung. Zum einen wurden mittlerweile fast alle wichtigeren Statistik-Pakete aus

der Großrechnerwelt, wie etwa BMDP, SAS und SPSS für Workstations portiert. Zum anderen wurde auch eine große Zahl von neuen Produkten speziell für Geräte dieser Klasse entwickelt. Heute existieren für Workstations mehrere hundert kommerziell vertriebene Statistik-Pakete und unzählige Produkte aus dem Share- und Freewarebereich. Dennoch bleiben nach wie vor bestimmte statistische Anwendungen Großrechnern vorbehalten. Man kann Statistik-Software grob in Pakete

mit allgemeinem Methodenspektrum und solche für spezielle Methodengebiete unterscheiden. Statistik-Pakete mit allgemeinem Methodenspektrum verfügen zumeist über univariate deskriptive Statistiken, Kreuztabellen, Regressionsrechnung, nichtparametrische Tests, Varianzanalyse und Grafiken. Vom Anwender verlangen sie häufig keine besonderen Methodenkenntnisse; es genügt im Wesentlichen, sich eine leicht erlernbare Steuersprache bzw. die Bedienung einer vorgegebenen

Benutzeroberfläche anzueignen. Diese prinzipiell zu begrüßende einfache Handhabbarkeit statistischer Software birgt aber auch Gefahren, die nicht übersehen werden dürfen.

28.10.2014   12:00 Uhr   Kabine: _________

Aufgabe 2

Bitte streichen Sie nun nach den <u>neuen Regeln</u> den Buchstaben „e“ (auch Großbuchstaben „E“) durch.
Zusammenfassung der neuen Regeln: Prinzipiell streichen Sie alle „e“ durch.
Ausnahmen: Sie streichen ein „e“ **nicht** durch, wenn auf der Position 1, 3 oder 4 ein Vokal steht:

_ _ e _ _
1 2   3 4

Bitte arbeiten Sie 7 Minuten an dieser Aufgabe.

Gerade weil viele Programme so einfach zu bedienen sind, können sie auch von Benutzern mit teils nur rudimentären Statistikkenntnissen eingesetzt werden. Über die Voraussetzungen und Implikationen der verwendeten Verfahren sowie die angemessene Interpretation der Ergebnisse sind sich unerfahrene Anwender oftmals nicht im Klaren. Es steht zu befürchten, daß die Verfügbarkeit einfach zu bedienender Statistikprogramme unbedachte Auswertungen mit inadäquaten Methoden

und fragwürdigen Ergebnissen provoziert. Der andere drohende Extremfall ist, daß das vom ausgewählten Statistik-Paket abgedeckte Methodenspektrum die durchzuführende Untersuchung in dem Sinne vorstrukturiert, daß der Anwender nur die von der Software angebotenen Verfahren in Betracht zieht. Methoden, die im Hinblick auf das vorliegende Datenmaterial und die zu untersuchende Fragestellung unter Umständen geeigneter wären, aber im Programm nicht

vorgesehen sind, werden dann a priori ignoriert. Im nachfolgenden Abschnitt werden einige gemeinhin als wichtig erachtete Eigenschaften von Statistik-Paketen erläutert. Aufgrund der Heterogenität des Angebotes wie auch der Erfordernisse der Anwender kann ein solcher Anforderungskatalog keinen normativen Charakter besitzen. Er ist vielmehr als Entscheidungshilfe zu betrachten, die bei der Auswahl eines Statistik-Paketes Hinweise auf Qualitätskriterien geben kann.

Oftmals wenig beachtet wird die statistische und numerische Korrektheit der von den Programmen generierten Ergebnisse. Bei den Produkten mit allgemeinem Methodenspektrum wurde der Schwerpunkt auf Programmpakete für Workstations gelegt. Da die angebotenen Produkte und ihre Eigenschaften einem ständigen Wandel unterliegen, kann es sich hierbei nur um eine Momentaufnahme handeln. Ebenso wenig ist es möglich, sämtliche auf dem Markt angebotenen

Produkte zu würdigen. Ein Anspruch auf Vollständigkeit wird dementsprechend nicht erhoben. Noch viel mehr gilt dies für die erwähnten und auf bestimmte Anwendungsgebiete spezialisierten Pakete. Aus der Fülle des Angebotes werden nur einige wenige Produkte expressis verbis genannt. Die Nennung eines Produktes ist also keineswegs mit einer Wertung verbunden. Aufgrund der Unmöglichkeit einer umfassenden Würdigung aller verfügbaren Programme ist der nach seinem

Statistik-Paket Suchende auf weiterführende Informationen angewiesen. Hinweise zur Beschaffung dieser Informationen, mit besonderer Berücksichtigung des Internets, können in diesem Abschnitt gefunden werden. Der im Folgenden diskutierte Katalog wünschenswerter Eigenschaften von Statistikprogrammen fasst die in der einschlägigen Literatur anzutreffenden Forderungen zusammen. In dieser Hinsicht ist er als Minimalkonsens anzusehen, der um etliche Kriterien, die für bestimmte

Benutzer oder Anwendungen wichtig sind, ergänzt werden könnte. Auf eine Gewichtung der genannten Aspekte wurde bewusst verzichtet. Sie hängt entscheidend vom geplanten Einsatzgebiet und den individuellen Präferenzen ab und sollte daher von jedem selbst vorgenommen werden. Im Wesentlichen können, neben den diskutierten Korrektheitsproblemen, vier Kategorien von Kriterien identifiziert werden die bei der Auswahl eines Statistik-Paketes berücksichtigt werden

sollten: Systemvoraussetzungen, Bedienung, Systemschnittstellen und Vielfalt der statistischen Verfahren. In die erste Gruppe fallen technische Eigenschaften wie unterstützte Hardware in die zweite dagegen Aspekte wie Art und Weise der Bedienung oder die Qualität der Handbücher und Onlinehilfen. Die dritte Kategorie deckt die Möglichkeiten des Datenaustausches sowie der Integration in andere Programmsysteme ab. Und die Kernfunktionalität eines Statistikprogrammes,

nämlich der Umfang der angebotenen Verfahren und grafischen Darstellungen, wird schließlich im vierten Punkt angesprochen. Die Systemvoraussetzungen spezifizieren die Plattform, auf der die betreffende Statistik-Software funktionsfähig ist. Unter einer Plattform wird hierbei eine konkrete Kombination aus Hardware und Betriebssystem verstanden.