Empirical evidence on the determinants of economic decision-making

Inaugural-Dissertation

zur Erlangung des Grades eines Doktors
der Wirtschafts- und Gesellschaftswissenschaften

durch

die Rechts- und Staatswissenschaftliche Fakultät der
Rheinischen Friedrich-Wilhelms-Universität Bonn

vorgelegt von

Jana Willrodt

aus Henstedt-Ulzburg

2018

Tag der Promotion: 13.12.2018

Dekan:	Prof. Dr. Jürgen von Hagen
Erstreferent:	Prof. Dr. Armin Falk
Zweitreferent:	Prof. Dr. Thomas Dohmen
Tag der mündlichen Prüfung:	13.12.2018

Acknowledgments

"It is better to be a human being dissatisfied than a pig satisfied; better to be Socrates dissatisfied than a fool satisfied."

John Stuart Mill, Utilitarianism (1863)

Writing this thesis would not have been possible (and certainly much less satisfactory) without the support of various people.

First of all, I would like thank my thesis supervisors Armin Falk and Thomas Dohmen for their continuous help with the big steps along the way; for providing input and guidance; for offering perspective and making me see the big picture when I was lost in details. I learned an incredible amount from their experience.

Moreover, I would like to thank my fabulous co-authors. Hannah Schildberg-Hörisch gave me valuable advice and support at all the smaller but no less important steps of the way. Holger Gerhardt provided a tremendous amount of technical support, explained the importance of sans serif fonts and flexible programming. Simone Quercia was a calming influence, style guide and always ready to go for a cup of coffee.

Special thanks go to my cohort of fellow Ph.D. students. Discussions with them were challenging and fun at the same time. Sebastian Schaube offered helpful opinions on all kinds of things. In particular, I would like to mention Nina Bobkova, Yao Chen, and Ulrike Steins who helped me stay sane. At times they gave me much needed emotional support, but more frequently they were brilliant company for countless cups of coffees, lunches, and dinners. May our friendship of six years last another sixty.

Sebastian Kammer had the most important job. He reminded me of what matters.

Contents

List of Figures

List of Tables

1

Introduction

Every day of our lives, we make countless decisions; some are small and relatively easy to make, while others are important and complex. Often the difficulty in making these decisions arises because consequences are not deterministic, that is, an element of risk is involved. At the same time many decisions have consequences on others besides the decision-maker. The desire to understand the determinants of such decisions is at the heart of behavioral economics. For this purpose both theoretical models of decision-making as well as its controlled examination with laboratory experiments rely on two core concepts: preferences and beliefs.

Throughout this thesis we take the view that preferences are something akin to a character trait, that is, a relatively stable feature of a person's "economic personality" affecting decision-making. They describe how a person trades off a specific aspect of different options. For *risk preferences* this aspect is the riskiness of each option, while for *social preferences* it is the social implications.

Although we do not assume a specific functional form of either risk or social preferences, we assume that such preferences can be measured by observing decisions or through relevant survey measures. Depending on which measure is used, the inferred preference parameter can be quite different. However, this is not necessarily only due to noise. Rather specific decisions depend on factors beyond preferences, e.g., the decision environment, other character traits or psychological states.

The notion of beliefs that we employ here differs from that in a game-theoretical sense where in equilibrium beliefs are always equal to objective probabilities and adjust instantaneously if new information becomes available. In contrast, we consider a belief to be the subjective probability assessment a decision maker holds and which experimenters can measure independently from measuring behavior. Such an assessment exists even in the absence of information and does not necessarily adjust fully to new information.

The first three chapters of this thesis are dedicated to empirically examining factors beyond risk preferences that influence decision-making under risk: psychological states (Chapter 2), character traits (Chapter 3), and (stable) beliefs regarding the riskiness of situations (Chapter 4).

Chapter 2 deals with the impact different psychological states, in this case a state of low self-control, have on measured risk preferences. 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, ego depletion does not increase measured risk aversion.

Chapter 3 examines a channel through which a stable aspect of personality, optimism, affects measured risk preferences. We show that the disposition to focus on favorable or unfavorable outcomes of risky situations affects willingness to take risk as measured by the general risk question. We demonstrate that this disposition, which we call risk conception, is strongly associated with optimism, a stable facet of personality and that it predicts real-life risk taking. The general risk question captures this disposition alongside pure risk preference. This also explains why the general risk question is a better predictor of behavior under risk across different domains than measures of pure risk preference.

Next, we take a closer look at the interaction between preferences and the other building block of economic decision-making: beliefs. In standard economic theory, beliefs only depend on the available information and adjust immediately once new information becomes available. This means that under full rationality beliefs are the same for everyone when objective probabilities are explicitly stated.

Chapter 4 shows that this is not necessarily true. Extending the idea of Chapter 3, we show that even in situations where the decision environment is fully specified, i.e., in a lottery with objective probabilities, the perception of the riskiness of a situation differs considerably between people. We present evidence from a longitudinal experiment introducing a novel task asking subjects to assess the likelihood of winning a 50-50 lottery. We find substantial and systematic heterogeneity in answers. Moreover, there are robust correlations with several different risk preference measures such as lottery choices and the general risk question. Testing several channels for this relationship, we find little evidence that our findings are driven by differential perceptions of probabilities or probability weighting. We hence conclude that belief-related factors akin to personality traits play a role in decision making under risk.

Chapter 5 examines the relationship between measured preferences and beliefs in a setup where beliefs more traditionally are assumed to matter: social interactions. The assumption that beliefs only depend on available information also gives rise to the idea that the concepts of preferences and beliefs are distinct and unrelated in the sense that they don't influence on another. However, empirical evidence supports that own preferences influence beliefs about others. This phenomenon is known as the consensus effect. In this chapter we advance the hypothesis that the consensus effect may depend on the salience of own preferences when forming beliefs about others. We present two pieces of supportive evidence from a binary trust game experiment. First, the consensus effect is stronger when preferences elicitation precedes belief elicitation. Second, we observe a larger consensus effect when preferences and beliefs are elicited in the same session rather than two weeks apart.

Although each of the following four chapters addresses a distinct research question they are connected by one overarching question: What influences measured preferences and beliefs?

2

Does self-control depletion affect risk attitudes?

Joint with Holger Gerhardt and Hannah Schildberg-Hörisch

2.1 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 (Barsky et al., 1997; Anderson and Mellor, 2008; Kimball et al., 2008; Dohmen et al., 2011).

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.

[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.

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; Fudenberg and Levine, 2006; Brocas and Carrillo, 2008; Fudenberg and Levine, 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 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 sub-

[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.

sequent 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]

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

[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. Haan and 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 Haan and Veldhuizen cannot rule out that it was caused by depleted subjects choosing more randomly (p. 59).

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 2.2 presents the Fudenberg–Levine model (2.2.1) and the hypotheses that we derive from the model regarding the impact of reduced self-control on risk attitudes (2.2.2) as well as the hypotheses based on Loewenstein and O'Donoghue (2005) (2.2.3) and Mukherjee (2010) (2.2.4). Section 2.3 describes the design and procedural details of our laboratory experiment. Section 2.4 presents the results. Section 2.5 discusses our findings and concludes.

2.2 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).

2.2.1 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

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

over a large number of periods, 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 2.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.

2.2.2 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 2.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.

[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."

2.2.3 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 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.

2.2.4 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.

2.3 Experiment

2.3.1 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.

2.3.2 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).

[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.

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.

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 appendix 2.D.

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]

2.3.3 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]

[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.

- 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.

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 2.1 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 Figure 2.1.

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

Table 2.1. 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%

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]

[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

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.

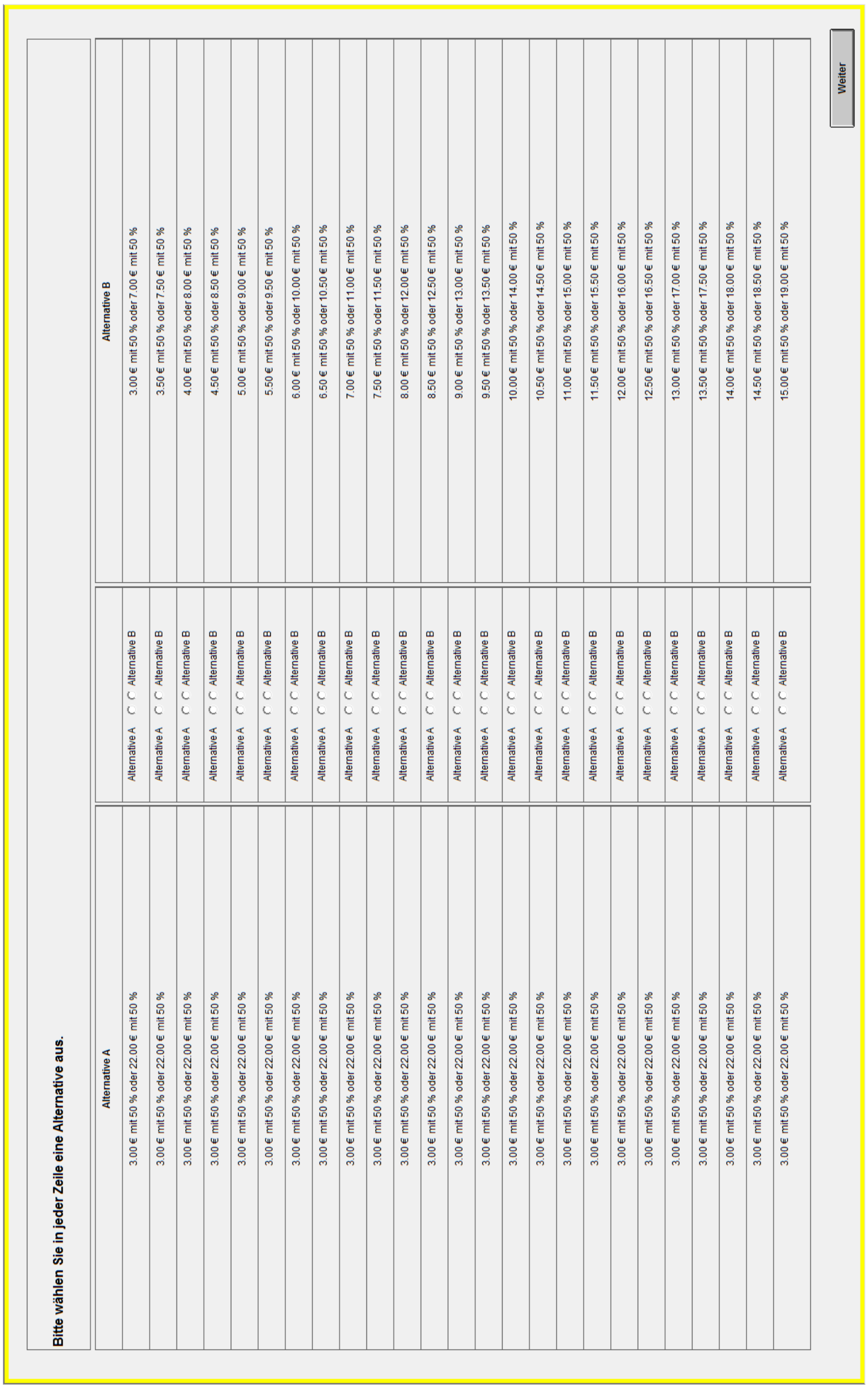
Obviously, there is a trade-off between the brevity of a choice list and the fineness and extent of measurement. Some experimenters solve this by using differently sized increments, i.e., smaller increments in intervals they expect to be most relevant. Since we were concerned that this might confound 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.

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.

---

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

Figure 2.1. 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%."

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.

2.3.4 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 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.

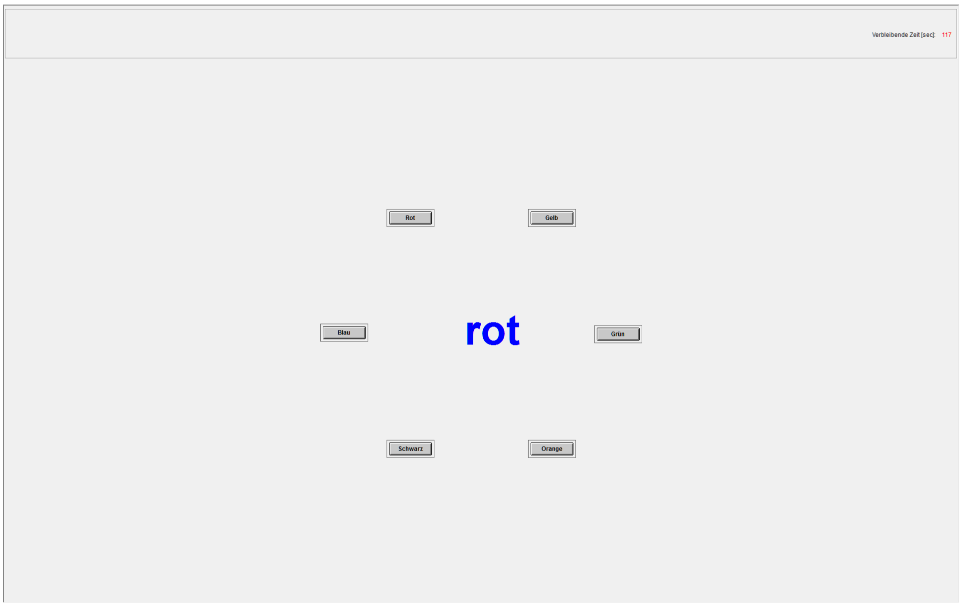


Figure 2.2. Screenshot of the computerized Stroop test.

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.

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 Figure 2.2. 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.

2.3.5 Procedural details and implementation

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

1. *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 Appendix 2.C.)
2. *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.
3. *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.
4. *Measurement of risk attitudes.* Subjects made lottery decisions in the four choice lists.
5. *Part 2 of the manipulation check.* Stroop test.
6. *Puzzle.* Subjects solved the puzzle with the chosen level of difficulty.
7. *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 2.A.4 in Appendix 2.A that the qualitative prediction *regarding the treatment effect* is unaffected. Hence, judged by the dual-self model, 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

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

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).

2.4 Results

2.4.1 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 2.3.4), 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, $p = 0.176$).

2.4.2 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-*

Table 2.2. 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: $p < 0.0001$.

seeking (negative 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 Appendix 2.B.

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 2.2). Hence, we are confident that our measures of risk attitudes pick up systematic variation in underlying individual risk attitudes.

2.4.3 Treatment effects

Figure 2.3 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.

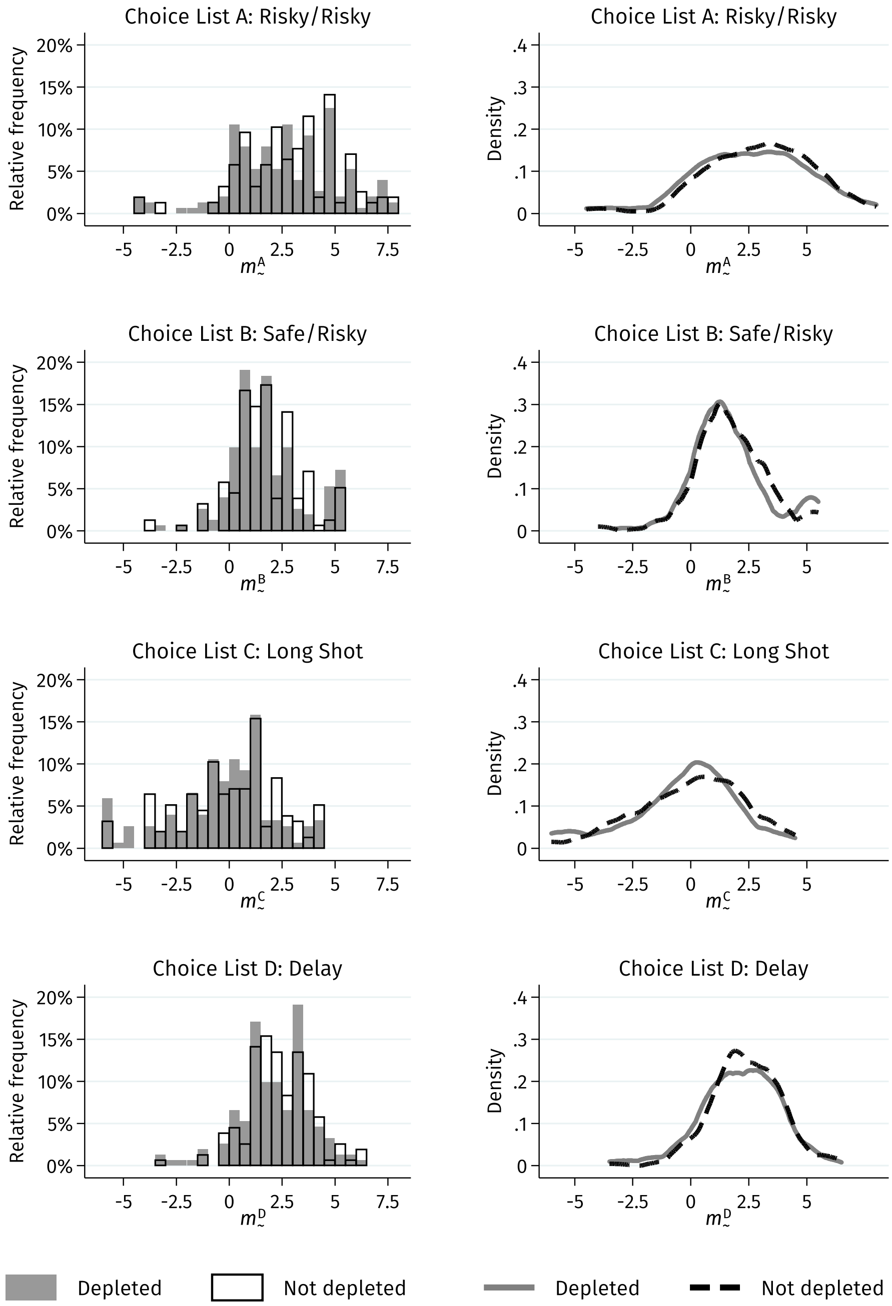


Figure 2.3. Treatment comparison of indifference-generating risk premia.

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).

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 ($\Delta_{\underset{\sim}{m}^{\mathrm{A}}} = 0.30$€ ; Cohen's $d = 0.13$) and Choice List B ($\Delta_{\underset{\sim}{m}^{\mathrm{B}}} = 0.10$€ ; 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 ($\Delta_{\underset{\sim}{m}^{\mathrm{D}}} = 0.22$€ ; 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 ($\Delta_{\underset{\sim}{m}^{\mathrm{C}}} = 0.28$€ , 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).

2.4.4 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 2.3. 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

[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 2.3. 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+	0.940	0.033	0.017	0.011	0.156
Female × Ego depletion = 0]	($p = 0.33$)	($p = 0.86$)	($p = 0.90$)	($p = 0.92$)	($p = 0.69$)

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.

of this coefficient and that on the interaction term (i.e., Ego depletion + Ego depletion × Female) represents the effect of depletion on women.

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 2.3. 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).

2.4.5 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.

2.5 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

[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.

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 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,

[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.

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 2.4.3, 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 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 2.4.4, 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.

References

Achtziger, Anja, Carlos Alós-Ferrer, and Alexander K. Wagner. 2015. "Money, Depletion, and Prosociality in the Dictator Game." *Journal of Neuroscience, Psychology, and Economics* 8 (1): pp. 1–14.

Achtziger, Anja, Carlos Alós-Ferrer, and Alexander K. Wagner. 2016. "The impact of self-control depletion on social preferences in the ultimatum game." *Journal of Economic Psychology* 53: pp. 1–16.

Allais, Maurice. 1953. "Le Comportement de l'Homme Rationnel devant le Risque: Critique des Postulats et Axiomes de l'École Américaine." *Econometrica* 21 (4): pp. 503–546.

Alonso, Ricardo, Isabelle Brocas, and Juan D. Carrillo. 2014. "Resource Allocation in the Brain." *Review of Economic Studies* 81 (2): pp. 501–534.

Anderson, Lisa R., and Jennifer M. Mellor. 2008. "Predicting health behaviors with an experimental measure of risk preference." *Journal of Health Economics* 27 (5): pp. 1260–1274.

Andersson, Ola, Håkan J. Holm, Jean-Robert Tyran, and Erik Wengström. 2016. "Risk Aversion Relates to Cognitive Ability: Preferences or Noise?" *Journal of the European Economic Association* 14 (5): pp. 1129–1154.

Azrieli, Yaron, Christopher P. Chambers, and Paul J. Healy. 2017. "Incentives in Experiments: A Theoretical Analysis." Working paper. Columbus, Ohio, USA: Ohio State University. Conditionally accepted for publication in the *Journal of Political Economy*.

Bamberg, Günther, and Franz Baur. 2001. *Statistik*. München, Germany: Oldenbourg Wissenschaftsverlag GmbH, p. 342.

Barsky, Robert B., F. Thomas Juster, Miles S. Kimball, and Matthew D. Shapiro. 1997. "Preference Parameters and Behavioral Heterogeneity: An Experimental Approach in the Health and Retirement Study." *Quarterly Journal of Economics* 112 (2): pp. 537–579.

Baumeister, Roy F., Ellen Bratslavsky, Mark Muraven, and Dianne M. Tice. 1998. "Ego Depletion: Is the Active Self a Limited Resource?" *Journal of Personality and Social Psychology* 74 (5): pp. 1–15.

Baumeister, Roy F., and Kathleen D. Vohs. 2016a. "Misguided Effort With Elusive Implications." *Perspectives on Psychological Science* 11 (4): pp. 574–575.

Baumeister, Roy F., and Kathleen D. Vohs. 2016b. "Strength Model of Self-Regulation as Limited Resource: Assessment, Controversies, Update." In *Advances in Experimental Social Psychology*. James M. Olson and Mark P. Zanna, ed. vol. 54, pp. 67–127.

Benjamin, Daniel J., Sebastian A. Brown, and Jesse M. Shapiro. 2013. "Who Is 'Behavioral'? Cognitive Ability and Anomalous Preferences." *Journal of the European Economic Association* 11 (6): pp. 1231–1255.

Bertrams, Alex, and Oliver Dickhäuser. 2009. "Messung dispositioneller Selbstkontroll-Kapazität." *Diagnostica* 55 (1): pp. 2–10.

Brocas, Isabelle, and Juan D. Carrillo. 2008. "The Brain as a Hierarchical Organization." *American Economic Review* 98 (4): pp. 1312–1346.

Bruyneel, Sabrina D., Siegfried Dewitte, Philip Hans Franses, and Marnik G. Dekimpe. 2009. "I Felt Low and My Purse Feels Light: Depleting Mood Regulation Attempts Affect Risk Decision Making." *Journal of Behavioral Decision Making* 22 (2): pp. 153–170.

Bucciol, Alessandro, Daniel Houser, and Marco Piovesan. 2011. “Temptation and productivity: A field experiment with children.” *Journal of Economic Behavior & Organization* 78 (1–2): pp. 126–136.

Bucciol, Alessandro, Daniel Houser, and Marco Piovesan. 2013. “Temptation at Work.” *PLoS ONE* 8 (1): Tom Denson, ed., Article e53713 (1–5).

Buckert, Magdalena, Christiane Schwieren, Brigitte M. Kudielka, and Christian J. Fiebach. 2014. “Acute stress affects risk taking but not ambiguity aversion.” *Frontiers in Neuroscience* 8: Article 82 (1–11).

Carter, Evan C., and Michael E. McCullough. 2014. “Publication bias and the limited strength model of self-control: has the evidence for ego depletion been overestimated?” *Frontiers in Psychology* 5: Article 823 (1–11).

Cohn, Alain, Jan Engelmann, Ernst Fehr, and Michel André Maréchal. 2015. “Evidence for Countercyclical Risk Aversion: An Experiment with Financial Professionals.” *American Economic Review* 105 (2): pp. 860–885.

Conte, Anna, M. Vittoria Levati, and Chiara Nardi. 2016. “Risk Preferences and the Role of Emotions.” *Economica*, (Early View):

Croson, Rachel, and Uri Gneezy. 2009. “Gender Differences in Preferences.” *Journal of Economic Literature* 47 (2): pp. 448–474.

Cubitt, Robin P., Chris Starmer, and Robert Sugden. 1998. “On the Validity of the Random Lottery Incentive System.” *Experimental Economics* 1 (2): pp. 115–131.

Deck, Cary, and Salar Jahedi. 2015. “The effect of cognitive load on economic decision making: A survey and new experiments.” *European Economic Review* 78: pp. 97–119.

Dekel, Eddie, Barton L. Lipman, and Aldo Rustichini. 2009. “Temptation-Driven Preferences.” *Review of Economic Studies* 76 (3): pp. 937–971.

Diamond, Peter, and Botond Kőszegi. 2003. “Quasi-hyperbolic discounting and retirement.” *Journal of Public Economics* 87 (9–10): pp. 1839–1872.

Dohmen, Thomas, Armin Falk, David Huffman, and Uwe Sunde. 2010. “Are Risk Aversion and Impatience Related to Cognitive Ability?” *American Economic Review* 100 (3): pp. 1238–1260.

Dohmen, Thomas, Armin Falk, David Huffman, Uwe Sunde, Jürgen Schupp, and Gert G. Wagner. 2011. “Individual Risk Attitudes: Measurement, Determinants, and Behavioral Consequences.” *Journal of the European Economic Association* 9 (3): pp. 522–550.

Ebert, Sebastian, and Daniel Wiesen. 2011. “Testing for Prudence and Skewness Seeking.” *Management Science* 57 (7): pp. 1334–1349.

Ebert, Sebastian, and Daniel Wiesen. 2014. “Joint measurement of risk aversion, prudence, and temperance.” *Journal of Risk and Uncertainty* 48 (3): pp. 231–252.

Eeckhoudt, Louis, and Harris Schlesinger. 2006. “Putting Risk in Its Proper Place.” *American Economic Review* 96 (1): pp. 280–289.

Falk, Armin, and Florian Zimmermann. 2017. “Consistency as a Signal of Skills.” *Management Science* 63 (7): pp. 2197–2210.

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

Freeman, Nicholas, and Mark Muraven. 2010. “Self-Control Depletion Leads to Increased Risk Taking.” *Social Psychological and Personality Science* 1 (2): pp. 175–181.

Friehe, Tim, and Hannah Schildberg-Hörisch. 2017. "Self-control and crime revisited: Disentangling the effect of self-control on risk taking and antisocial behavior." *International Review of Law and Economics* 49: pp. 23–32.

Fudenberg, Drew, and David K. Levine. 2006. "A Dual-Self Model of Impulse Control." *American Economic Review* 96 (5): pp. 1449–1476.

Fudenberg, Drew, and David K. Levine. 2011. "Risk, Delay, and Convex Self-Control Costs." *American Economic Journal: Microeconomics* 3 (3): pp. 34–68.

Fudenberg, Drew, and David K. Levine. 2012. "Timing and Self-Control." *Econometrica* 80 (1): pp. 1–42.

Fudenberg, Drew, David K. Levine, and Zacharias Maniadis. 2014. "An approximate dual-self model and paradoxes of choice under risk." *Journal of Economic Psychology* 41: pp. 55–67.

Gerhardt, Holger, Guido Biele, Harald Uhlig, and Hauke R. Heekeren. 2016. "Cognitive Load Increases Risk Aversion." SFB 649 Discussion Paper 2016-011. Bonn, Germany, et al.: University of Bonn et al.

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

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2014. "Time Varying Risk Aversion." Working paper. University of Chicago, USA, et al.

Gul, Faruk, and Wolfgang Pesendorfer. 2007. "Harmful Addiction." *Review of Economic Studies* 74 (1): pp. 147–172.

Haan, Thomas de, and Roel van Veldhuizen. 2015. "Willpower depletion and framing effects." *Journal of Economic Behavior & Organization* 117: pp. 47–61.

Hagger, Martin S., Nikos L. D. Chatzisarantis, Hugo Alberts, Calvin Octavianus Anggono, Cédric Batailler, Angela R. Birt, Ralf Brand, Mark J. Brandt, Gene Brewer, Sabrina Bruyneel, Dustin P. Calvillo, W. Keith Campbell, Peter R. Cannon, Marianna Carlucci, Nicholas P. Carruth, Tracy Cheung, Adrienne Crowell, Denise T. D. De Ridder, Siegfried Dewitte, Malte Elson, Jacqueline R. Evans, Benjamin A. Fay, Bob M. Fennis, Anna Finley, Zoë Francis, Elke Heise, Henrik Hoemann, Michael Inzlicht, Sander L. Koole, Lina Koppel, Floor Kroese, Florian Lange, Kevin Lau, Bridget P. Lynch, Carolien Martijn, Harald Merckelbach, Nicole V. Mills, Alexej Michirev, Akira Miyake, Alexandra E. Mosser, Megan Muise, Dominique Muller, Milena Muzi, Dario Nalis, Ratri Nurwanti, Henry Otgaar, Michael C. Philipp, Pierpaolo Primoceri, Katrin Rentzsch, Lara Ringos, Caroline Schlinkert, Brandon J. Schmeichel, Sarah F. Schoch, Michel Schrama, Astrid Schütz, Angelos Stamos, Gustav Tinghög, Johannes Ullrich, Michelle VanDellen, Supra Wimbarti, Wanja Wolff, Cleoputri Yusainy, Oulmann Zerhouni, and Maria Zwienenberg. 2016. "A Multilab Preregistered Replication of the Ego-Depletion Effect." *Perspectives on Psychological Science* 11 (4): pp. 546–573.

Hagger, Martin S., Chantelle Wood, Chris Stiff, and Nikos L. D. Chatzisarantis. 2010. "Ego Depletion and the Strength Model of Self-Control: A Meta-Analysis." *Psychological Bulletin* 136 (4): pp. 495–525.

Harbaugh, William T., Kate Krause, and Lise Vesterlund. 2010. "The Fourfold Pattern of Risk Attitudes in Choice and Pricing Tasks." *Economic Journal* 120 (545): pp. 595–611.

Harrison, Glenn W, Morten I Lau, and E. Elisabet Rutström. 2007. "Estimating Risk Attitudes in Denmark: A Field Experiment." *Scandinavian Journal of Economics* 109 (2): pp. 341–368.

Hauge, Karen Evelyn, Kjell Arne Brekke, Lars-Olof Johansson, Olof Johansson-Stenman, and Henrik Svedsäter. 2014. "Keeping others in our mind or in our heart? Distribution games under cognitive load." Working Papers in Economics, No. 600. Göteborg, Sweden: University of Gothenburg.

Heidhues, Paul, and Botond Kőszegi. 2009. "Futile Attempts at Self-Control." *Journal of the European Economic Association* 7 (2/3): pp. 423–434.

Holt, Charles A., and Susan K. Laury. 2002. "Risk Aversion and Incentive Effects." *American Economic Review* 92 (5): pp. 1644–1655.

Kahneman, Daniel. 2011. *Thinking, Fast and Slow*. New York, NY, USA: Farrar, Straus, and Giroux, p. 512.

Kandasamy, Narayanan, Ben Hardy, Lionel Page, Markus Schaffner, Johann Graggaber, Andrew S. Powlson, Paul C. Fletcher, Mark Gurnell, and John Coates. 2014. "Cortisol shifts financial risk preferences." *Proceedings of the National Academy of Sciences USA* 111 (9): pp. 3608–3613.

Kimball, Miles S., Claudia R. Sahm, and Matthew D. Shapiro. 2008. "Imputing Risk Tolerance From Survey Responses." *Journal of the American Statistical Association* 103 (483): pp. 1028–1038.

Kostek, John, and Lisham Ashrafioun. 2014. "Tired Winners: The Effects of Cognitive Resources and Prior Winning on Risky Decision Making." *Journal of Gambling Studies* 30 (2): pp. 423–434.

Kouchaki, Maryam, and Isaac H. Smith. 2014. "The Morning Morality Effect: The Influence of Time of Day on Unethical Behavior." *Psychological Science* 25 (1): pp. 95–102.

Laibson, David. 1997. "Golden Eggs and Hyperbolic Discounting." *Quarterly Journal of Economics* 112 (2): pp. 443–477.

Loewenstein, George, and Ted O'Donoghue. 2005. "Animal Spirits: Affective and Deliberative Processes in Economic Behavior." Working paper. Carnegie Mellon University, and Cornell University.

MacLeod, Colin M. 1991. "Half a Century of Research on the Stroop Effect: An Integrative Review." *Psychological Bulletin* 109 (2): pp. 163–203.

Maranges, Heather M., Brandon J. Schmeichel, and Roy F. Baumeister. 2017. "Comparing cognitive load and self-regulatory depletion: Effects on emotions and cognitions." *Learning and Instruction* 51: pp. 74–84.

Mukherjee, Kanchan. 2010. "A Dual System Model of Preferences Under Risk." *Psychological Review* 117 (1): pp. 243–255.

Muraven, Mark, and Elisaveta Slessareva. 2003. "Mechanisms of Self-Control Failure: Motivation and Limited Resources." *Personality and Social Psychology Bulletin* 29 (7): pp. 894–906.

Ozdenoren, Emre, Stephen W. Salant, and Dan Silverman. 2012. "Willpower and the Optimal Control of Visceral Urges." *Journal of the European Economic Association* 10 (2): pp. 342–368.

Rabin, Matthew. 2000. "Risk Aversion and Expected-Utility Theory: A Calibration Theorem." *Econometrica* 68 (5): pp. 1281–1292.

Rubinstein, Ariel. 2007. "Instinctive and Cognitive Reasoning: A Study of Response Times." *Economic Journal* 117 (523): pp. 1243–1259.

Rubinstein, Ariel. 2016. "A Typology of Players: Between Instinctive and Contemplative." *Quarterly Journal of Economics* 131 (2): pp. 859–890.

Schulreich, Stefan, Holger Gerhardt, and Hauke R. Heekeren. 2016. "Incidental Fear Cues Increase Monetary Loss Aversion." *Emotion* 16 (3): pp. 402–412.

Schulreich, Stefan, Yana G. Heussen, Holger Gerhardt, Peter N. C. Mohr, Ferdinand C. Binkofski, Stefan Koelsch, and Hauke R. Heekeren. 2014. "Music-evoked incidental happiness modulates probability weighting during risky lottery choices." *Frontiers in Psychology* 4: Article 981 (1–17).

Segal, Uzi. 1990. "Two-Stage Lotteries without the Reduction Axiom." *Econometrica* 58 (2): pp. 349–377.

Stigler, George J., and Gary S. Becker. 1977. "De Gustibus Non Est Disputandum." *American Economic Review* 67 (2): pp. 76–90.

Stojić, Hrvoje, Michael R. Anreiter, and José A. Carrasco Martinez. 2013. "An Experimental Test of the Dual Self Model." Working paper. Barcelona, Spain: Universitat Pompeu Fabra.

Stroop, J. Ridley. 1935. "Studies of interference in serial verbal reactions." *Journal of Experimental Psychology* 18 (6): pp. 643–662.

Tangney, June P., Roy F. Baumeister, and Angie Luzio Boone. 2004. "High Self-Control Predicts Good Adjustment, Less Pathology, Better Grades, and Interpersonal Success." *Journal of Personality* 72 (2): pp. 271–324.

Thaler, Richard H., and Hersh M. Shefrin. 1981. "An Economic Theory of Self-Control." *Journal of Political Economy* 89 (2): pp. 392–406.

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

Unger, Alexander, and Dagmar Stahlberg. 2011. "Ego-Depletion and Risk Behavior." *Social Psychology* 42 (1): pp. 28–38.

Watson, David, and Lee Anna Clark. 1999. "The PANAS-X: Manual for the Positive and Negative Affect Schedule—Expanded Form." Online Manual. University of Iowa.

Wheeler, S. Christian, Pablo Briñol, and Anthony D. Hermann. 2007. "Resistance to persuasion as self-regulation: Ego-depletion and its effects on attitude change processes." *Journal of Experimental Social Psychology* 43 (1): pp. 150–156.

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

We briefly sketched the Fudenberg–Levine model in Section 2.2.1. 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.

2.A.1 The model in detail

2.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

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 $c > -x$.

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

2.A.1.2 Lotteries

In this setup, unexpectedly facing a set $\mathfrak{J}$ of income opportunities is a situation in which self-control becomes relevant. Lotteries are denoted as discrete random variables $Z \in \mathfrak{J}$ 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 $u^\star(\mathfrak{J}) \equiv \max_{Z \in \mathfrak{J}} \mathrm{E}u(Z)$.[16] The long-run self, in contrast, prefers to smooth consumption 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.

2.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 $\mathfrak{J}$.

2.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 $\mathfrak{J}$ 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.

---

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

[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).

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

For each lottery in the menu $\mathfrak{Z}$, the agent chooses a contingent consumption plan $\tilde{c}$ with outcomes $(c_1, \dots, c_n)$, where c_i is consumption in case the lottery outcome z_i realizes $(i = 1, \dots, n)$. 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

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

2.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 $c_i \le z_i$ 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:

$$\max_z U(u^\star, Z, z), \text{ where} \tag{2.1}$$
$$U(u^\star, Z, z) \equiv h[\mathrm{E}u(Z) - u^\star - \mathrm{E}\max\{u(Z) - u(z), 0\}] + u'(0)\,\mathrm{E}\max\{Z - z, 0\}.$$

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.

2.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 $z_1^A \le z_2^A$ and $z_1^B \le z_2^B$, 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 $\mathfrak{Z} = \{Z^A, Z^B\}$. (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

$$\frac{\mathrm{d}\Phi(\cdot)}{\mathrm{d}z_1^B} + \frac{\mathrm{d}\Phi(\cdot)}{\mathrm{d}z_2^B}\frac{\mathrm{d}z_2^B}{\mathrm{d}z_1^B} \;=\; 0 \tag{2.2}$$

$$\Longleftrightarrow \qquad \frac{\mathrm{d}z_2^B}{\mathrm{d}z_1^B} \;=\; -\frac{\mathrm{d}\Phi(\cdot)}{\mathrm{d}z_1^B}\bigg/\frac{\mathrm{d}\Phi(\cdot)}{\mathrm{d}z_2^B}$$

$$\Longrightarrow \qquad \frac{\mathrm{d}z_2^B}{\mathrm{d}z_1^B} \;=\; -\frac{\partial U(u^\star, Z^B, \hat{z}^B)}{\partial z_1^B}\bigg/\frac{\partial U(u^\star, Z^B, \hat{z}^B)}{\partial z_2^B}. \tag{2.3}$$

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

$$U(u^\star, Z^B, \hat{z}^B)$$
$$= h[\mathrm{E}u(Z^B) - u^\star - \mathrm{E}\max\{u(Z^B) - u(\hat{z}^B), 0\}] + u'(0)\,\mathrm{E}\max\{Z^B - \hat{z}^B, 0\} \tag{2.4}$$
$$\begin{aligned} &= h[p\,u(z_1^B) + (1-p)\,u(z_2^B) - u^\star - \\ &\qquad p\max\{u(z_1^B) - u(\hat{z}^B), 0\} - (1-p)\max\{u(z_2^B) - u(\hat{z}^B), 0\}] \\ &\quad + u'(0)[p\max\{z_1^B - \hat{z}^B, 0\} + (1-p)\max\{z_2^B - \hat{z}^B, 0\}]. \end{aligned} \tag{2.5}$$

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

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

and

$$\frac{\partial U(u^\star, Z^B, \hat{z}^B)}{\partial z_2^B} =$$

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

2.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 Figure 2.A.1. 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

$$\frac{\mathrm{d}z_2}{\mathrm{d}z_1} = -\frac{h'[\mathrm{E}u(Z) - u^\star]p\, u'(z_1)}{h'[\mathrm{E}u(Z) - u^\star](1-p)\, u'(z_2)} = -\frac{p\, u'(z_1)}{(1-p)\, u'(z_2)}. \quad (2.6)$$

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

$$\frac{\mathrm{d}z_2}{\mathrm{d}z_1} = -\frac{p\, u'(0)}{(1-p)\, u'(0)} = -\frac{p}{1-p}. \quad (2.7)$$

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 $\mathrm{E}(Z)$.

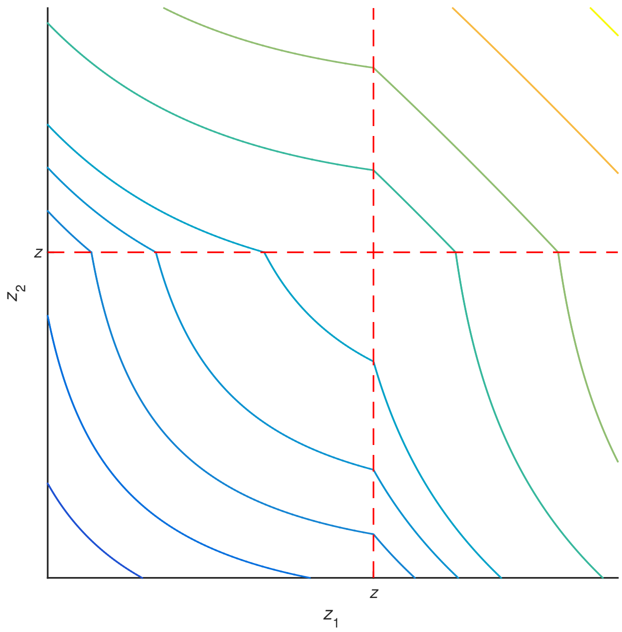


Figure 2.A.1. Illustration of the three cases.

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 (2.6), (2.7), and (2.8). 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 $h'[\cdot]$,

$$\frac{\mathrm{d}z_2}{\mathrm{d}z_1} = -\frac{h'[p\,u(z_1) - u^\star + (1-p)\,u(\hat{z})]\,p\,u'(z_1)}{(1-p)\,u'(0)}. \tag{2.8}$$

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.

2.A.3 Depletion: model predictions and hypotheses

2.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

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

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

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

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 $g^{\mathrm{ND}'}[\cdot] \leq g^{\mathrm{D}'}[\cdot]$. It follows that $h[\mathrm{E}u(\tilde{c}) - u^\star]$ is weakly concave and that

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

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 (2.8). When plugging in a higher value for $h'[\cdot]$ in (2.8), 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 (2.6) that were formerly evaluated by (2.8) and some lotteries will be evaluated by (2.8) that were formerly evaluated by (2.7). Both effects result in increased risk aversion.

2.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 (2.8) 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 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.

2.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 $\mathfrak{Z}$ of 25 compound lotteries.[19] Thus, Z is a multioutcome lottery, and the choice amounts to selecting one out of 25 elements in $\mathfrak{Z}$. 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 2.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 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 util-

[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.

ity 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. (2.5) 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. (2.6), (2.7), and (2.8), and thus cancels out. Only in determining where the marginal self-control gain function $h'[\cdot]$ is evaluated in Eq. (2.8) 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.

2.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).

2.B 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

[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.

in the first row. They are excluded from the further analysis, since dominated choices are incompatible with any deterministic model 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 2.B.1 displays the absolute and relative frequencies of choices in the choice lists.

Table 2.B.1. 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

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

2.C 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.

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 %

Figure 2.C.1. 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%."

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 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!

2.D 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.

3

Willingness to take risk: The role of risk conception and optimism

Joint work with Thomas Dohmen and Simone Quercia

3.1 Introduction

Most decisions in economic and social life are taken under risk or uncertainty. Expected utility theory posits that risk preference determines behavior in these situations; and non-expected utility theory allows for reference points and risk perception (e.g. probability weighting) to matter for risky choice. In this paper, we demonstrate that risk taking behavior is also determined by the disposition to focus on favorable or unfavorable outcomes of risky choice, an important factor beyond curvature of utility and departures from linearity in probabilities. This disposition, which we call risk conception, is akin to a trait; it is strongly related to optimism, an enduring facet of personality (Carver and Scheier, 2014). We show that individuals differ systematically in the way how they conceive risky situations, and that these differences map into heterogeneity in risk taking behavior.

When it comes to predicting risky behavior across contexts, it is advantageous to have measures of all stable characteristics that determine risky choice, including risk conception. We argue that instruments and methods designed to reveal risk preference capture risk conception to different degrees. Typically these risk preference measures are based on a risky choice R which is a function of the underlying latent risk preference parameter r and a vector of other relevant factors X, i.e., $R = f(r, X)$. Standard practice in economics is to create environments and elicitation mechanisms that control for X as much as possible in order to elicit r (see Charness et al., 2013, for a review). A prime example is an incentivized lottery choice in a controlled environment. While such measures may be suited to reveal parameter *r*, their predictive power for real life risk taking may be comparatively low precisely because of their tight control of other factors that systematically and persistently affect decision making under risk or uncertainty. In contrast, survey instruments that lack this control, e.g., with respect to stake size and probabilities, may capture these elements and have stronger predictive power for different risky behaviors *R* across situations.

We focus on one such instrument, the "general risk question", which asks subjects "Are you generally a person who is willing to take risks or do you try to avoid taking risks?" on an 11-point Likert scale ranging from "not at all willing to take risks" to "very willing to take risks". This question has been shown to be a good predictor of risk taking behavior across different domains (e.g., Bonin et al., 2007; Caliendo et al., 2009; Grund and Sliwka, 2010; Jaeger et al., 2010; Dohmen et al., 2011; Lönnqvist et al., 2015).

We hypothesize that part of the variation in answers to the general risk question depends on respondents' disposition to focus on positive or negative outcomes of risk, and that this disposition is stable and systematic.

Our experimental results support these hypotheses. We find that the degree to which respondents focus on the positive or negative outcomes of risk when answering the general risk question is a strong predictor of their responses. We further show that this disposition is systematically related to optimism, a stable character trait whose importance has been long recognized in personality psychology (e.g., Carver et al., 2010; Carver and Scheier, 2014).[1] Furthermore, we show that optimism affects responses to the general risk question but that it does so mostly through respondents' focus on the positive or negative outcomes of risk rather than directly.

In light of this result, we use optimism as a proxy for people's disposition to focus on favorable/unfavorable outcomes of risk taking, and in the second step of our analysis, we examine whether optimism relates to risk taking behavior. We do so using (i) an incentivized measure of risk taking contained in our experimental dataset and (ii) self-reported real life behaviors from the German Socio-Economic Panel (henceforth SOEP). For both datasets, we find a significant association between risk taking behavior and optimism. We conclude that, in addition to being a proxy for pure risk preferences, the general risk question captures important personality characteristics relevant for risk taking behavior, thereby providing a broader representation of the factors that should be taken into account when studying decision making under risk.

[1] In line with much of the personality psychology literature (Carver et al., 2010), we view optimism as a stable disposition (i.e., a personality trait) that affects beliefs in specific environments. There is initial evidence that this character trait also manifests itself in differential beliefs about uncertain events (see Felton et al., 2003, who show that in males optimism increases investment in stocks).

The remainder of the paper is structured as follows. Section 3.2 introduces the design of our experiment. Section 3.3 establishes the link between the way how people conceive risk, their responses to the general risk question, and optimism. Section 3.4 investigates the relationship between optimism, the general risk question and risk taking behavior. Section 3.5 discusses the results and concludes.

3.2 The experiment

The data we analyze in this paper were collected during a longitudinal experiment consisting of three one-hour sessions run in three consecutive weeks. The experiment was computerized using z-Tree (Fischbacher, 2007). Participants were invited from the BonnEconLab subject pool using hroot (Bock et al., 2014). Most of the 348 participants were students (95%) from various fields of study. 61% of subjects were female, and the average age was 22.4 years. In what follows, we describe the variables relevant to our research question.

General risk question. Our main variable of interest is the general risk question which was validated in Dohmen et al. (2011) (see also Section 3.1). We used the same wording as in the SOEP (see for example Wagner et al., 2007). The question was administered to subjects at the beginning of the session in the third week.[2]

Risk conception questions. After subjects had responded to the general risk question, we asked them what aspects of risk they focused on while answering. We use the following four questions (7-point Likert scale).[3]

- Did you rather think of the negative or positive sides of risk? [Risk - neg/pos; scale: "[1] only of the negative sides" to "[7] only of the positive sides"]

- Did you rather think of small everyday situations or large important ones? [Risk - stake size; scale: "[1] small everyday situations" to "[7] large important situations"]

- Did you rather think of situations in which there are small or large gains? [Risk - stake size (gains); scale: "[1] small gains" to "[7] large gains"]

- Did you rather think of situations in which there are small or large losses? [Risk - stake size (losses); scale: "[1] small losses" to "[7] large losses"]

2 It was also asked at the beginning of the other weeks' sessions. But since the "risk conception" questions were only asked in the third week to avoid interference with later risk-related tasks, we focus on week 3 here.

3 All questions are translated from German.

Before responding to these questions, subjects reported in free-form text what they thought of when answering the general risk question. To code the free-form text, we used a similar procedure as Brandts and Cooper (2007): two research assistants independently coded the free-form answers along the dimensions of positive/negative valence and stake size (see Section 3.A.2 in the appendix for details on the coding procedure). Spearman rank correlations between the resulting variables and the corresponding risk conception questions are $\rho = .39$ for "Free form - neg/pos"($p < .001$), $\rho = .42$ for "Free form - stake size" ($p < .001$), $\rho = .14$ for "Free form - stake size (gains)" ($p = .007$), and $\rho = .14$ for "Free form - stake size (losses)" ($p = .011$).[4]

Optimism measures. Our main optimism measure is the so-called SOP questionnaire (Kemper et al., 2015). It consists of two items eliciting self-reported degrees of optimism and pessimism (7-point Likert scale). The first item is: "Optimists are people who look to the future with confidence and who mostly expect good things to happen. How would you describe yourself? How optimistic are you in general?". The second item reads as "Pessimists are people who are full of doubt when they look to the future and who mostly expect bad things to happen. How would you describe yourself? How pessimistic are you in general?".

The SOP scale is based on the established Life Orientation Test (henceforth LOT; Scheier et al., 1994; Herzberg et al., 2006), which we also include in our questionnaire. Similar to Kemper et al. (2015), we find a convergent Spearman rank correlation between SOP and LOT of $\rho = .76$ ($p < .001$). In the main text of the paper, we restrict our analyses to the SOP measure but results are virtually the same if LOT is used (see Section 3.A.4 and Section 3.A.8 in the appendix for the LOT questionnaire and these results, respectively).

Optimism was elicited at the end of the session in the third week after subjects had completed several incentivized tasks without having received feedback. This makes spillover effects between the risk-related questions and the optimism measures unlikely. We also elicited SOP and LOT in the second week session of our longitudinal experiment. The Spearman rank correlation of measured optimism across weeks is $\rho = .81$ for SOP and $\rho = .84$ for LOT (Spearman, $p < .001$ for both). All the results presented in the paper are robust to using these previously elicited optimism measures (see Section 3.A.8 in the appendix).

Risk taking behavior. Our behavioral risk measure is based on the risk premia for three different lotteries. We elicited certainty equivalents of these lotteries in week 1 and week 3 using a multiple price list format. In both weeks, subjects

[4] Some free-form text answers were not classifiable according to our categories. This is especially prominent for the three variables referring to stake size where 50%, 56%, and 62%, respectively, of coded answers take the value 0, compared to 42% for "Free form - neg/pos" (see Table 3.A.2.) This suggests that it is rather the positive or negative sides of risk than stake sizes that subjects think about when answering the general risk question.

went through the same three choice lists (see Section 3.A.5 in the appendix). In all tables, subjects chose between a safe payment and a lottery paying 15 with probability p and 0 with probability $1-p$. The probability p was 0.25, 0.5, and 0.75 in tables 1, 2, and 3, respectively. The safe payment increased from 0€ to 15€ in steps of 0.50€. For each lottery, we average over the risk premia across weeks to reduce noise in our measure of risk taking. Furthermore, we construct a risk premium index aggregating the risk premia for the three lotteries for each subject.

Controls. We control for sociodemographics that were elicited in the first week of the experiment and a proxy for cognitive ability that was elicited in the third week. This proxy is based on ten Raven matrices (see Section 3.A.6 of the appendix for the distribution of responses). In addition, in some specifications we also use the Big Five personality characteristics that we elicited in every session using the 15 item questionnaire developed for the SOEP (Schupp and Gerlitz, 2008).

3.3 Conception of risk and the general risk question

There are two noteworthy patterns in our data. First, there is considerable heterogeneity in answers to risk conception questions, as is reflected by standard deviations in responses. Averages and standard deviations are 3.53 and 1.43, respectively, for “Risk - neg/pos”; 4.06 and 1.56 for “Risk - stake size”; 4.18 and 1.51 for “Risk - stake size (gains)”; as well as 4.49 and 1.58 for “Risk - stake size (losses)”. The correlational pattern between the different risk conception questions suggests that valence and stake size are orthogonal, as “Risk - neg/pos” and “Risk - stake size” are uncorrelated (Spearman’s $\rho = -.071$, $p = .185$), while all other risk conception questions are significantly correlated with one another (see Table 3.A.1 for details). Second, pairwise Spearman rank correlations between the general risk question and each of the conception questions are significant except for "Risk - stake size".[5]

Ordinary least squares regressions confirm that answers to the risk conception questions are systematically related to responses to the general risk question, even when controlling for gender and cognitive ability.[6] Column (1) of Table 3.1 indicates that subjects who focus on positive rather than negative sides of risk are significantly more willing to take risk. The effect sizes of all other risk conception questions are smaller. Thinking about higher gains is associated with

[5] The correlations are $\rho = 0.63$ and $p < .001$ for “Risk - neg/pos”, $\rho = -.04$ and $p = .488$ for “Risk - stake size”, $\rho = .27$ and $p < .001$ for “Risk - stake size (gains)”, $\rho = -.28$ and $p < .001$ for “Risk - stake size (losses)”.

[6] We do not control for age since there is very little variation in a student sample.

Table 3.1. Relationship between the general risk question and risk conception.

	General risk question					
	(1)	(2)	(3)	(4)	(5)	(6)
Risk - neg/pos	0.826***	0.951***				
	(0.070)	(0.064)				
Risk - stake size	0.118*		-0.019			
	(0.065)		(0.075)			
Risk - stake size (gains)	0.147**			0.373***		
	(0.065)			(0.075)		
Risk - stake size (losses)	-0.264***				-0.421***	
	(0.067)				(0.071)	
Female	-0.300	-0.314	-0.627**	-0.510**	-0.624***	-0.626***
	(0.187)	(0.191)	(0.243)	(0.236)	(0.231)	(0.242)
IQ (Raven)	-0.147***	-0.125***	-0.127**	-0.132**	-0.164***	-0.127**
	(0.045)	(0.045)	(0.058)	(0.056)	(0.056)	(0.058)
Constant	3.272***	2.644***	6.273***	4.589***	8.257***	6.193***
	(0.532)	(0.365)	(0.476)	(0.471)	(0.484)	(0.352)
R^2	0.44	0.41	0.03	0.09	0.12	0.03
N	348	348	348	348	348	348

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variables is the general risk question elicited on an 11-point scale. The independent variables “Risk - neg/pos” to “Risk - stake size (losses)” consist of the answers to questions eliciting what subjects thought of while answering the general risk question along the dimensions of valence and stake size.

a significantly higher willingness to take risk and thinking about higher losses with a significantly lower willingness to take risk.

Whether subjects focus on the positive or negative aspects of risk also has by far the highest explanatory power. This is evident from comparing the R^2 of the regressions in columns (2) to (5), in which we successively regress the general risk question on one of the risk conception questions and the set of control variables ($R^2 = 0.44$ and $R^2 = 0.41$ for models (1) and (2), respectively, and $R^2 = 0.03$, $R^2 = 0.09$ and $R^2 = 0.12$, respectively, for models (3) to (5)). In summary, this indicates that conception of risk is strongly related to self-assessed willingness to take risk.

Table 3.1 also reveals an interesting finding regarding the gender effect in willingness to take risk. Not controlling for risk conception, women report to be significantly less willing to take risk than men (model (6)). This is consistent with the gender difference in willingness to take risk reported in many previous studies using representative population samples of particular countries (e.g., Dohmen et al., 2011) and across the globe (Falk and Zimmermann, 2017) as

Table 3.2. Relationship between risk conception and optimism.

	Risk - neg/pos	Risk - stake size	Risk - stake size (gains)	Risk - stake size (losses)
	(1)	(2)	(3)	(4)
Optimism (SOP)	0.261***	-0.018	0.036	-0.177***
	(0.061)	(0.068)	(0.066)	(0.068)
Female	-0.317**	-0.075	-0.308*	-0.004
	(0.156)	(0.173)	(0.168)	(0.174)
IQ (Raven)	-0.006	-0.027	0.013	-0.084**
	(0.037)	(0.042)	(0.040)	(0.042)
Constant	3.506***	4.252***	4.271***	5.056***
	(0.232)	(0.259)	(0.250)	(0.259)
R^2	0.06	0.00	0.01	0.03
N	348	348	348	348

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variables consist of the answers to questions eliciting what subjects thought of while answering the general risk question along the dimensions of valence and stake size.

well as in various non-representative population studies (Vieider et al., 2015).[7] However, once we condition on whether respondents think about positive or negative aspects of risk when answering the general risk question, the gender difference becomes small and insignificant (models (1) and (2)). This indicates that the gender difference in self-assessed willingness to take risk is largely driven by gender differences in disposition to focus on positive or negative outcomes of risk taking, and not so much by gender differences in curvature of the utility function.

Our findings are corroborated when we measure risk conception in an alternative way, using the variables constructed from the free-form text question that was elicited before the risk conception questions (see Section 3.2 for details on variable construction).[8] When we replicate the regressions reported in Table 3.1 using variables derived from free-form text we find qualitatively very similar results (see Table 3.A.3 in the appendix).

As a next step, we investigate to what extent conception of risk is systematically related to stable individual characteristics. For this purpose, we regress answers to the four questions described in Section 3.2 on the optimism measure (SOP), our main proxy for personality characteristics, controlling for gender and cognitive ability. The results are shown in Table 3.2. The coefficient associated

[7] For reviews and meta-studies see Eckel and Grossman (2008), Croson and Gneezy (2009), Charness and Gneezy (2012), and Buser et al. (2014).

[8] The Spearman rank correlation between the general risk question and “Free form - neg/pos” is positive and significant ($\rho = .265$, $p < .001$), while this is not the case for “Free form - stake size”($\rho = -.024$, $p = .652$),“Free form - stake size (gains)”($\rho = -.003$, $p = .949$) and “Free form - stake size (losses)”($\rho = .043$, $p = .420$).

with optimism is significantly different from zero only for the regressions using "Risk - neg/pos" and "Risk - stake size (losses)", which were also the strongest predictors of answers to the general risk question. In line with the findings from Table 3.1, women exhibit a significantly lower propensity to think of the positive rather than the negative sides of risk, even when optimism is not controlled for (see Table 3.A.4 in the appendix). This supports the conjecture that gender differences in risk taking are partly due to systematic gender differences in risk conception.

The data enable us to perform a number of robustness checks on the relationship between conception of risk and optimism (see Table 3.A.7 to Table 3.A.10 in the appendix). A potential concern is that measurement error in optimism might be correlated with answers to the risk conception questions. For example, subjects' momentary psychological state might affect the optimism measure and answers to the risk conception questions, and hence introduce a spurious relationship between the measures, which does not reflect a relationship between the trait component of optimism and risk conception. We address this in several ways. First, we regress the answers to the risk conception questions on self-stated mood elicited at the beginning of the session (see model (5) in each of the aforementioned tables). Additionally, we regress the answers to the four risk conception questions on the optimism measures elicited one week prior to asking the risk conception questions (see model 2 in each of the aforementioned tables). Further, to correct for measurement error in the optimism measure we (i) aggregate the SOP measures elicited in week 2 and 3 and (ii) we instrument SOP elicited in week 3 with SOP elicited in week 2 using a two stage least squares estimation (see models (3) and (4) of each table). Finally, to validate the importance of optimism as a relevant personality characteristic in our context, we run the same specifications of models (3) and (4) adding the Big 5 personality traits also corrected for measurement error (see models (6) and (7) of each table).[9] Similar to the results in Table 3.2, the coefficient associated with optimism is significantly different from zero across all additional specifications when we use "Risk - neg/pos" and "Risk - stake size (losses)" as dependent variables, while it is not for the other two risk conception variables.

Since our hypothesis is that optimism is a reliable proxy (and might be causal) for people's disposition to focus on favorable/unfavorable outcomes of risk taking, which in turn affects responses to the general risk question, we next

[9] In personality psychology, optimism is viewed as a distinct trait that cannot be readily mapped into the Big Five inventory, even though there is a partial overlap between optimism and some dimensions of the Big Five (in particular agreeableness and extraversion; see Carver and Scheier (2014)). In our setup, optimism seems ex-ante an aspect of personality that can be used as a reliable proxy people's disposition to focus on favorable or unfavorable outcomes of risk taking. The models reported in Table 3.A.7 to Table 3.A.10 confirm this.

Table 3.3. Relationship between the general risk question and optimism controlling for risk conception.

	General risk question					
	(1)	(2)	(3)	(4)	(5)	(6)
Optimism (SOP)	0.406***	0.149**	0.166**	0.405***	0.392***	0.337***
	(0.093)	(0.075)	(0.076)	(0.093)	(0.090)	(0.090)
Risk - neg/pos		0.799***	0.919***			
		(0.071)	(0.065)			
Risk - stake size		0.112*		-0.014		
		(0.065)		(0.074)		
Risk - stake size (gains)		0.151**			0.363***	
		(0.065)			(0.074)	
Risk - stake size (losses)		-0.253***				-0.385***
		(0.067				(0.070)
Female	-0.609**	-0.302	-0.318*	-0.610**	-0.497**	-0.610***
	(0.236)	(0.186)	(0.190)	(0.237)	(0.230)	(0.227)
IQ (Raven)	-0.134**	-0.149***	-0.128***	-0.134**	-0.139**	-0.166***
	(0.057)	(0.045)	(0.045)	(0.057)	(0.055)	(0.055)
Constant	5.839***	3.195***	2.619***	5.900***	4.288***	7.784***
	(0.353)	(0.531)	(0.363)	(0.472)	(0.464)	(0.492)
R^2	0.08	0.45	0.42	0.08	0.14	0.15
N	348	348	348	348	348	348

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

study whether optimism has a direct effect on measured risk attitudes and how risk conception mediates this relationship.

In Table 3.3 we regress the general risk question on the SOP optimism measure. When we only include SOP and controls as explanatory variables (model (1)), the coefficient on optimism is sizable and significantly different from zero. However, once the question on whether subjects thought about the positive or negative sides of risk is added in the regression, the coefficient on optimism decreases considerably (model (2) and (3)). This pattern is weaker or non-existent for the other risk conception questions (models (4) to (6)).

The coefficient on “Risk - neg/pos” in models (2) and (3) is significantly different from zero and of the same order of magnitude as in Table 3.1, when optimism was not included. This suggests that it is not optimism itself but rather its influence on subjects’ conception of the general risk question, in terms of positive or negative outcomes of risk taking, that affects stated risk attitudes.

3.4 Optimism and risk taking behavior

So far, we have shown that responses to the general risk question are affected by aspects beyond parameters of a standard utility function. In fact, one crucial aspect is whether people have a disposition to focus on the positive or negative outcomes of risk taking. This disposition has persistence as it is related to optimism, an important and stable character trait. An intriguing question that extends beyond the relationship between risk conception and self-assessed willingness to take risk is whether actual risk taking behavior is also affected by risk conception. If this was not the case, answers to the general risk question would simply contain information irrelevant for risky behavior.[10] Below, we analyze data from our experiment and from a representative sample, and show that this disposition to focus on positive/negative outcomes of risk, proxied by optimism, is in fact related to risk taking behavior.

As a measure of risk taking behavior among our student sample, we use the risk premium index derived from three incentivized lottery choices (see Section 3.2). We regress this index on the SOP optimism measure, the general risk question, and basic control variables. Model (1) in Table 3.4 shows a significant association between risk taking behavior and optimism. Model (2) replicates findings from the previous literature and shows that the general risk question is a significant predictor of risk taking in lottery choice. When in model (3), we include both optimism and the general risk question in the regression, the coefficient on optimism is smaller and not statistically significant. This indicates that the general risk question captures the optimism component, thus making it a useful predictor for risk taking behavior. A similar pattern arises when using each risk premium separately rather than the risk premium index as a dependent variable (see Table 3.A.14 and Table 3.A.15 in the appendix).

Next, we investigate whether the association between optimism and risk taking behavior extends to real life behavior in a representative sample of the German population. For this purpose we use information on self-reported behaviors in the 2014 wave of the German Socio Economic Panel (SOEP). In particular, we focus on two domains that are relevant for economics and directly related to risk taking: portfolio choice and career choice. As a proxy for portfolio choice, we use information about household stock holdings. In particular, the variable "Stocks" takes value 1 if at least one household member holds stocks, shares, or stock options and zero otherwise. Since the question is only administered to the household head, the regressions involving this variable use the subsample of household heads. The variable “Self-employed” takes value 1 if an individual is self-employed and zero for individuals who are in other employment. As a proxy

[10] Such information unrelated to risk taking behavior would generate measurement error in responses to the general risk question lowering its predictive power (Beauchamp et al., 2017).

Table 3.4. Optimism and Risk Taking Behavior.

	Risk premium index		
	(1)	(2)	(3)
Optimism (SOP)	-0.096**		-0.055
	(0.041)		(0.041)
General risk question		-0.107***	-0.100***
		(0.022)	(0.023)
Female	0.431***	0.368***	0.370***
	(0.103)	(0.102)	(0.102)
IQ (Raven)	0.008	-0.008	-0.006
	(0.025)	(0.024)	(0.024)
Constant	-0.197	0.385*	0.389*
	(0.154)	(0.202)	(0.201)
R^2	0.064	0.108	0.113
N	348	348	348

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The variable "risk premium index" is created by standardizing the risk premia (aggregated over measurements in week 1 and 3), averaging, and then standardizing again.

for optimism we use the following question: "If you think about the future: Are you..."(translated from German). Respondents could answer on a scale from 1 to 4, where 1 = "optimistic", 2 = "rather optimistic than pessimistic", 3 = "rather pessimistic than optimistic", and 4 = "pessimistic". For ease of interpretation, we reverse the scale, such that, a higher scores means higher optimism. The general risk question has the exact same wording as in our experiment. We standardize both variables to ensure comparability. As expected from our experimental data, the correlation between the willingness to take risk as measured by the general risk question and optimism is positive and significant ($\rho = .165$, $p < .0001$).

To investigate whether optimism is also predictive of real life risk taking we run a series of linear probability models reported in Table 3.5 where we regress the aforementioned measures of risk taking on the optimism measure, the general risk question, and a set of control variables.[11] In line with the results from our experiment, models (1) and (4) show that optimism is a significant predic-

[11] We control for gender, age, and height which have been shown to predict risk taking in the previous literature (Dohmen et al., 2011). We also control for parents' education (*Abitur* mother and *Abitur* father) rather than own education to avoid the reverse causality problem that would occur with regard to self-employment. These variables are equal to 1 if a parent has "Abitur" or "Fachabitur", high school degrees that are awarded after 12 or 13 years of schooling and that grant access to (specific types of) university education. Further controls are logarithmic household wealth, logarithmic household debt, and logarithmic net household income. We also control for the number of adults (defined as older than 17) in the household in the stock-holding regression.

Table 3.5. Relationship between risk taking behavior and optimism.

	Risk taking: Stocks			Risk taking: Self-employed		
	(1)	(2)	(3)	(4)	(5)	(6)
Std. Optimism	0.012***		0.009**	0.012***		0.007**
	(0.004)		(0.004)	(0.003)		(0.003)
Std. General risk question		0.019***	0.017***		0.032***	0.031***
		(0.004)	(0.004)		(0.004)	(0.004)
Female	0.008	0.011	0.011	-0.021**	-0.012	-0.013
	(0.012)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)
Age	0.001**	0.001**	0.001***	0.004***	0.004***	0.004***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Height	0.002***	0.002**	0.002**	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0005)	(0.0005)
Abitur mother	-0.030*	-0.028	-0.031*	0.055***	0.051***	0.053***
	(0.018)	(0.018)	(0.018)	(0.013)	(0.013)	(0.013)
Abitur father	0.076***	0.076***	0.077***	0.037***	0.042***	0.040***
	(0.014)	(0.014)	(0.014)	(0.010)	(0.010)	(0.010)
Log househ. wealth	0.011***	0.012***	0.012***	0.003***	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log househ. debt	-0.006***	-0.006***	-0.006***	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log net househ. income	0.205***	0.203***	0.203***	0.001	0.002	0.001
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Number of adults in hh	-0.046***	-0.045***	-0.044***			
	(0.007)	(0.007)	(0.007)			
Constant	-1.675***	-1.663***	-1.648***	-0.189*	-0.212*	-0.199*
	(0.134)	(0.134)	(0.135)	(0.109)	(0.108)	(0.109)
R^2	0.138	0.138	0.139	0.032	0.041	0.041
N	9,324	9,325	9,267	8,593	8,573	8,537

Notes. OLS regressions. Standard errors in parentheses. $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The samples in columns 1 to 3 include only household heads. The dependent variable takes a value of 1 if the household holds stocks and 0 otherwise. The dependent variable in columns 4 to 6 takes a value of 1 if respondent is self-employed and 0 otherwise. Here, we limit the sample to individuals under 66 years who are part of the labor force.

tor of both holding stocks and being self-employed. In particular, an increase by one standard deviation in the response to the optimism question raises the probability of holding stocks (being self-employed) by 1.2 (1.2) percentage points.

When we use the general risk question (models (2) and (5)) as a predictor of holding stocks or being self-employed, we find that an increase by one standard deviation in willingness to take risk raises the probability of holding stocks (being self-employed) by 1.9 (3.2) percentage points. These results are consistent with Dohmen et al. (2011), who find similar effects for the 2004 wave of SOEP.

Finally, when we include both optimism and the general risk question (models (3) and (6)), the optimism coefficients are reduced, similar to the regressions reported in Table 3.4, indicating that the general risk question is also partly capturing the optimism component.

3.5 Conclusion

In this paper, we have provided evidence that responses to the general risk question (Dohmen et al., 2011) are influenced by factors that extend beyond parameters of a standard utility function. The way how people conceive risk and in particular whether they have a tendency to focus on favorable or unfavorable outcomes of risk taking is a crucial determinant of their responses. We have shown that heterogeneity in this disposition is systematic as it is related to optimism, a stable character trait. While optimists tend to focus on the positive outcomes associated with risk, pessimists tend to focus on the potential negative outcomes of risky decisions, leading to divergent responses.

Our data strongly suggest that the disposition to focus on positive or negative aspects of risks affects actual risk taking behavior. In our student sample and in a representative sample, we find that optimism, which predicts this disposition, is related to risk taking behavior. In the student sample it predicts lottery choices and in the representative sample investing in the stock market or being self-employed. The fact that the general risk question captures the disposition to focus on favorable or unfavorable outcomes of risky alternatives and that this factor is relevant for risk taking behavior, may explain why the general risk question is a better predictor of risk taking behavior across contexts than other measures of risk preferences that control more tightly risk conception, stakes and probabilities.

References

Beauchamp, Jonathan P, David Cesarini, and Magnus Johannesson. 2017. “The psychometric and empirical properties of measures of risk preferences.” *Journal of Risk and Uncertainty* 54 (3): pp. 203–237.

Bock, Olaf, Ingmar Baetge, and Andreas Nicklisch. 2014. “hroot: Hamburg registration and organization online tool.” *European Economic Review* 71: pp. 117–120.

Bonin, Holger, Thomas Dohmen, Armin Falk, David Huffman, and Uwe Sunde. 2007. “Cross-sectional earnings risk and occupational sorting: The role of risk attitudes.” *Labour Economics* 14 (6): pp. 926–937.

Brandts, Jordi, and David J Cooper. 2007. “It’s what you say, not what you pay: an experimental study of manager-employee relationships in overcoming coordination failure.” *Journal of the European Economic Association* 5 (6): pp. 1223–1268.

Buser, Thomas, Muriel Niederle, and Hessel Oosterbeek. 2014. “Gender, competitiveness, and career choices.” *The Quarterly Journal of Economics* 129 (3): pp. 1409–1447.

Caliendo, Marco, Frank M Fossen, and Alexander S Kritikos. 2009. "Risk attitudes of nascent entrepreneurs–new evidence from an experimentally validated survey." *Small Business Economics* 32 (2): pp. 153–167.

Carver, Charles S., and Michael F Scheier. 2014. "Dispositional optimism." *Trends in cognitive sciences* 18 (6): pp. 293–299.

Carver, Charles S., Michael F. Scheier, and Suzanne C. Segerstrom. 2010. "Optimism." *Clinical Psychology Review* 30 (7): pp. 879–889.

Charness, Gary, and Uri Gneezy. 2012. "Strong Evidence for Gender Differences in Risk Taking." *Journal of Economic Behavior & Organization* 83 (1): pp. 50–58.

Charness, Gary, Uri Gneezy, and Alex Imas. 2013. "Experimental methods: Eliciting risk preferences." *Journal of Economic Behavior & Organization* 87: pp. 43–51.

Croson, Rachel, and Uri Gneezy. 2009. "Gender Differences in Preferences." *Journal of Economic Literature* 47 (2): pp. 448–474.

Dohmen, Thomas, Armin Falk, David Huffman, Uwe Sunde, Jürgen Schupp, and Gert G. Wagner. 2011. "Individual Risk Attitudes: Measurement, Determinants, and Behavioral Consequences." *Journal of the European Economic Association* 9 (3): pp. 522–550.

Eckel, Catherine C., and Philip J. Grossman. 2008. "Forecasting risk attitudes: An experimental study using actual and forecast gamble choices." *Journal of Economic Behavior & Organization* 68 (1): pp. 1–17.

Falk, Armin, and Florian Zimmermann. 2017. "Consistency as a Signal of Skills." *Management Science* 63 (7): pp. 2197–2210.

Felton, James, Bryan Gibson, and David M. Sanbonmatsu. 2003. "Preference for Risk in Investing as a Function of Trait Optimism and Gender." *Journal of Behavioral Finance* 4 (1): pp. 33–40.

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

Grund, Christian, and Dirk Sliwka. 2010. "Evidence on performance pay and risk aversion." *Economics Letters* 106 (1): pp. 8–11.

Herzberg, Philipp Yorck, Heide Glaesmer, and Jürgen Hoyer. 2006. "Separating optimism and pessimism: a robust psychometric analysis of the revised Life Orientation Test (LOT-R)." *Psychological Assessment* 18 (4): p. 433.

Jaeger, David A, Thomas Dohmen, Armin Falk, David Huffman, Uwe Sunde, and Holger Bonin. 2010. "Direct evidence on risk attitudes and migration." *The Review of Economics and Statistics* 92 (3): pp. 684–689.

Kemper, Christoph J, Maria Wassermann, Annekatrin Hoppe, Constanze Beierlein, and Beatrice Rammstedt. 2015. "Measuring Dispositional Optimism in Large-Scale Studies." *European Journal of Psychological Assessment* 33: pp. 403–408.

Lönnqvist, Jan-Erik, Markku Verkasalo, Gari Walkowitz, and Philipp C. Wichardt. 2015. "Measuring individual risk attitudes in the lab: Task or ask? An empirical comparison." *Journal of Economic Behavior & Organization* 119: pp. 254–266.

Scheier, Michael F, Charles S Carver, and Michael W Bridges. 1994. "Distinguishing optimism from neuroticism (and trait anxiety, self-mastery, and self-esteem): a reevaluation of the Life Orientation Test." *Journal of Personality and Social Psychology* 67 (6): p. 1063.

Schupp, Jürgen, and Jean-Yves Gerlitz. 2008. "BFI-S: Big Five Inventory-SOEP." In *Zusammenstellung sozialwissenschaftlicher Items und Skalen. ZIS Version*. vol. 12,

Vieider, Ferdinand M, Mathieu Lefebvre, Ranoua Bouchouicha, Thorsten Chmura, Rustamdjan Hakimov, Michal Krawczyk, and Peter Martinsson. 2015. "Common components of risk and uncertainty attitudes across contexts and domains: Evidence from 30 countries." *Journal of the European Economic Association* 13 (3): pp. 421–452.

Wagner, Gert G, Joachim R Frick, and Jürgen Schupp. 2007. "The German Socio-Economic Panel Study (SOEP) - evolution, scope and enhancements." *SOEPpapers on Multidisciplinary Panel Data Research*, No. 1.

3.A Appendix to Chapter 3

3.A.1 Correlations between responses to risk conception questions

Table 3.A.1. Spearman rank correlations between responses to risk conception questions.

	Risk - neg/pos	Risk - stake size	Risk - stake size (gains)
Risk - stake size	-0.071		
	(0.185)		
Risk - stake size (gains)	0.278	0.205	
	(<0.001)	(<0.001)	
Risk - stake size (losses)	-0.288	0.449	0.133
	(<0.001)	(<0.001)	(0.013)

Notes. N= 348. p-values in parentheses

3.A.2 Coding of free-form responses

Before answering the four risk conception questions described in the main text, subjects were asked to report in free-form text what they thought about when answering the general risk question. Answers varied substantially, with some subjects stating financial risk, others considering the risk of being the victims of crime, or risk taking in sports. We coded the answers employing a strategy similar to that used by Brandts and Cooper (2007). Two research assistants unfamiliar with the research question and the rest of the dataset coded the answers independently such that coding errors would be uncorrelated. They created four categorical variables for each answer, one referring to the positive/negative valence and three referring to the stake size in general, stake size in the gains dimension, and stake size in the loss dimension respectively . "Free-form - neg/pos" could be either positive (1) or negative (-1), while "Free-form - stake size", "Free-form - stake size (gains)" and "Free-form - stake size (losses)" could be large (1) or small (-1). Furthermore, each variable took the value 0, if answers were mixed or not classifiable.[12] We found significant cross-coder Spearman rank correlations of $\rho = .49$, $\rho = .71$, $\rho = .61$, and $\rho = .38$ ($p < .001$ for all four) for valence, stake size, stake size (gains), and stake size (losses), respectively. For the analysis reported in the paper, following Brandts and Cooper (2007), we average the values across coders. Average responses to the risk conception questions split by coded free-form question response are reported in Table 3.A.2 below.

[12] Mixed answers can occur in situations where subjects state more than one risky situation.

Table 3.A.2. Responses to selected risk conception questions (by coded answer to Free-form question).

	Free-form - neg/pos			Free-form - stake size		
Value	Frequency	Mean	SD	Frequency	Mean	SD
-1	44	2.682	1.137	74	3	1.365
-0.5	43	2.767	1.231	42	3.571	1.548
0	146	3.479	1.266	175	4.325	1.391
0.5	93	4.097	1.533	36	5.028	1.464
1	22	4.545	1.405	21	4.905	1.411
	Free-form - stake s. (gains)			Free-form - stake s. (losses)		
Value	Frequency	Mean	SD	Frequency	Mean	SD
-1	40	3.675	1.269	30	3.333	1.583
-0.5	42	4.095	1.559	54	4.5	1.587
0	194	4.175	1.472	217	4.631	1.498
0.5	48	4.708	1.557	44	4.545	1.745
1	24	4.125	1.801	3	4.333	1.154

3.A.3 Regression on coded free-form variables

Table 3.A.3. Robustness check to Table 3.1.

	General risk question				
	(1)	(2)	(3)	(4)	(5)
Free-form - neg/pos	1.345***	1.090***			
	(0.237)	(0.211)			
Free-form - stake size	-0.023		-0.074		
	(0.277)		(0.213)		
Free-form - stake size (gains)	0.931***			0.381	
	(0.322)			(0.235)	
Free-form - stake size (losses)	-0.223				0.466
	(0.356)				(0.296)
Female	-0.602***	-0.628***	-0.622**	-0.614**	-0.620**
	(0.232)	(0.234)	(0.243)	(0.242)	(0.242)
IQ (Raven)	-0.131**	-0.127**	-0.127**	-0.127**	-0.119**
	(0.0557)	(0.056)	(0.058)	(0.058)	(0.058)
Constant	6.195***	6.184***	6.178***	6.201***	6.194***
	(0.339)	(0.340)	(0.355)	(0.351)	(0.351)
N	348	348	348	348	348
R^2	0.130	0.099	0.029	0.036	0.036

Notes. OLS regressions. Standard errors in parentheses * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable is the general risk question elicited on an 11-point scale. The independent variables are generated by coding the answer to the Free-form question "What kind of risk did you think of while answering the general risk question?"

3.A.4 LOT-R questionnaire

For the validation of the German version we used refer to Herzberg et al. (2006).

English version by Scheier et al. (1994): Please state to what extent your opinion agrees with the following statements (7 point Likert Scale from “does not apply to me at all” to “applies to me exactly”).

1. In uncertain times, I usually expect the best.
2. It’s easy for me to relax.
3. If something can go wrong for me, it will. (R)
4. I’m always optimistic about my future.
5. I enjoy my friends a lot.
6. It’s important for me to keep busy.
7. I hardly ever expect things to go my way. (R)
8. I don’t get upset too easily.
9. I rarely count on good things happening to me. (R)
10. Overall, I expect more good things to happen to me than bad.

Items marked with *(R)* are reverse-scaled, while items 2, 5, 6 and 8 are fillers.

3.A.5 Risk behavior measure - lottery choice lists

TABELLE 1 - Bitte wählen Sie in jeder Zeile eine Alternative aus.

Alternative A		Alternative B
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	0.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	0.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	1.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	1.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	2.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	2.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	3.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	3.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	4.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	4.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	5.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	5.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	6.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	6.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	7.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	7.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	8.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	8.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	9.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	9.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	10.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	10.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	11.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	11.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	12.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	12.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	13.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	13.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	14.00 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	14.50 € mit 100 %
15.00 € mit 25 % und 0.00 € mit 75 %	Alternative A ○ ○ Alternative B	15.00 € mit 100 %

Weiter

Figure 3.A.1. Exemplary Choice list: Certainty equivalent of lottery “15 € with 25% and 0 € with 75%”
Translation from German: "TABLE 1 - Please choose an alternative in each row.".

3.A.6 Measurement of cognitive ability

The appropriateness of the level of difficulty for a student population is confirmed by the roughly normal distribution of the number of correctly solved matrices displayed in Figure 3.A.2.

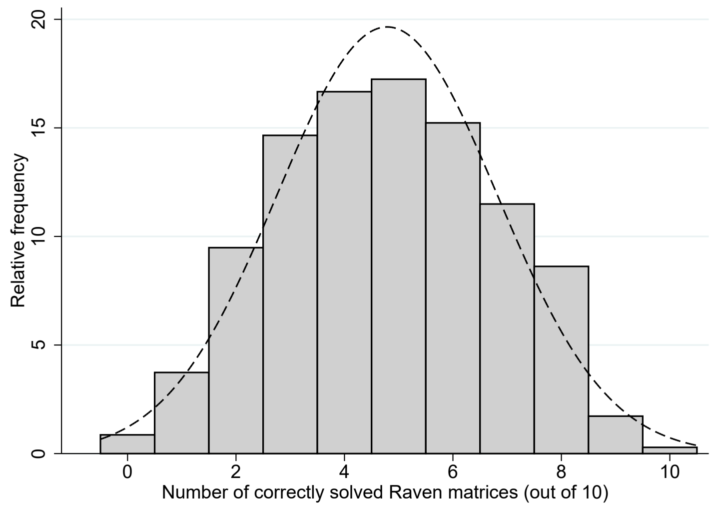


Figure 3.A.2. Distribution of proxy for cognitive ability.

3.A.7 Gender differences in risk conception

Table 3.A.4. Relationship between gender and risk conception.

	Risk - neg/pos	Risk - stake size	Risk - stake size (gains)	Risk - stake size (losses)
	(1)	(2)	(3)	(4)
Female	-0.328**	-0.074	-0.310*	0.003
	(0.159)	(0.173)	(0.167)	(0.175)
IQ (Raven)	-0.002	-0.027	0.014	-0.087**
	(0.038)	(0.042)	(0.040)	(0.042)
Constant	3.733***	4.236***	4.303***	4.902***
	(0.231)	(0.252)	(0.243)	(0.254)
R^2	0.05	0.00	0.00	0.02
N	348	348	348	348

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$ The dependent variables consist of the answers to questions eliciting what subjects thought of while answering the general risk question along the dimensions of valence and stake size.

3.A.8 Robustness of results to use of different specifications

Table 3.A.5. Robustness check to Table 3.2.

	Risk - neg/pos	Risk - stake size	Risk - stake size (gains)	Risk - stake size (losses)
	(1)	(2)	(3)	(4)
Optimism (SOP) - week 3	0.263***	-0.019	0.040	-0.182***
	(0.061)	(0.068)	(0.066)	(0.069)
Constant	3.278***	4.078***	4.141***	4.655***
	(0.095)	(0.105)	(0.102)	(0.106)
R^2	0.05	0.00	0.00	0.02
N	348	348	348	348

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variables consist of the answers to questions eliciting what subjects thought of while answering the general risk question along the dimensions of valence and stake size.

Table 3.A.6. Robustness check to Table 3.2.

Specification 1	Risk - neg/pos	Risk - stake size	Risk - stake size (gains)	Risk - stake size (losses)
	(1)	(2)	(3)	(4)
Optimism (LOT) - week 3	0.283***	-0.003	0.080	-0.239***
	(0.074)	(0.084)	(0.081)	(0.084)
Female	-0.327**	-0.052	-0.360**	0.058
	(0.158)	(0.180)	(0.172)	(0.179)
IQ (Raven)	-0.007	-0.022	-0.010	-0.082*
	(0.038)	(0.043)	(0.042)	(0.043)
Constant	2.355***	4.214***	4.056***	6.002***
	(0.409)	(0.466)	(0.446)	(0.462)
R^2	0.06	0.00	0.02	0.04
N	326	326	326	326
Specification 2	**Risk - neg/pos**	**Risk - stake size**	**Risk - stake size (gains)**	**Risk - stake size (losses)**
	(1)	(2)	(3)	(4)
Optimism (SOP) - week 2	0.293***	0.004	0.025	-0.266***
	(0.060)	(0.068)	(0.065)	(0.067)
Female	-0.340**	-0.023	-0.316*	0.077
	(0.157)	(0.178)	(0.171)	(0.175)
IQ (Raven)	-0.003	-0.023	0.004	-0.076*
	(0.038)	(0.042)	(0.041)	(0.042)
Constant	3.451***	4.190***	4.305***	5.049***
	(0.235)	(0.265)	(0.255)	(0.261)
R^2	0.08	0.00	0.01	0.06
N	335	335	335	335
Specification 3	**Risk - neg/pos**	**Risk - stake size**	**Risk - stake size (gains)**	**Risk - stake size (losses)**
	(1)	(2)	(3)	(4)
Optimism (LOT) - week 2	0.327***	0.026	0.033	-0.243***
	(0.073)	(0.082)	(0.079)	(0.082)
Female	-0.343**	-0.021	-0.316*	0.082
	(0.158)	(0.178)	(0.171)	(0.177)
IQ (Raven)	-0.017	-0.024	0.002	-0.067
	(0.038)	(0.043)	(0.041)	(0.043)
Constant	2.258***	4.078***	4.179***	5.894***
	(0.398)	(0.446)	(0.429)	(0.444)
R^2	0.07	0.00	0.01	0.04
N	335	335	335	335

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variables consist of the answers to questions eliciting what subjects thought of while answering the general risk question along the dimensions of valence and stake size. The optimism measure varies by specification. LOT-R is the Life Orientation Test. SOP is a two-item measure assessing subjects self-stated optimism and pessimism. Both were elicited in weeks 2 and 3.

Table 3.A.7. Robustness check: Relationship between optimism and risk conception questions.

	Risk - neg/pos						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	OLS	OLS	2SLS
Optimism (SOP - week 3)	0.261***			0.366***			0.231**
	(0.061)			(0.076)			(0.095)
Optimism (SOP - week 2)		0.293***					
		(0.060)					
Optimism (SOP - agg)			0.305***		0.304***	0.167**	
			(0.064)		(0.066)	(0.095)	
Female	-0.317**	-0.340**	-0.340**	-0.337**	-0.340**	-0.113	-0.136
	(0.156)	(0.157)	(0.158)	(0.160)	(0.158)	(0.169)	(0.173)
IQ (Raven)	-0.006	-0.003	-0.004	-0.006	-0.004	0.024	0.018
	(0.037)	(0.038)	(0.038)	(0.038)	(0.038)	(0.037)	(0.038)
Mood (week 3)					0.0004		
					(0.037)		
Conscientiousness (agg)						-0.168**	
						(0.076)	
Extraversion (agg)						0.225***	
						(0.068)	
Openness(agg)						0.0152	
						(0.061)	
Agreeableness (agg)						-0.174**	
						(0.085)	
Neuroticism (agg)						-0.200***	
						(0.069)	
Conscientiousness (week 3)							-0.101
							(0.084)
Extraversion (week 3)							0.194**
							(0.076)
Openness (week 3)							0.0390
							(0.066)
Agreeableness (week 3)							-0.192**
							(0.096)
Neuroticism (week 3)							-0.164**
							(0.078)
Constant	3.506***	3.451***	3.447***	3.401***	3.445***	4.754***	4.378***
	(0.232)	(0.235)	(0.236)	(0.240)	(0.321)	(0.749)	(0.810)
R^2	0.06	0.08	0.08	0.06	0.08	0.16	0.15
N	348	335	335	335	335	335	335

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. While “Risk - neg/pos” was elicited in week 3, models 1 and 2 use the SOP measures from weeks 3 and 2, respectively. Models 3,5 and 6 use the SOP measure aggregated over these two weeks, with model 5 including mood (beginning of session in week 3) and model 6 including the Big Five (aggregated across weeks 2 and 3) as controls. Models 4 and 7 are two-stage least squares estimations using the variables for SOP and the Big Five from week 2 as instruments for those from week 3.

Table 3.A.8. Robustness check: Relationship between optimism and risk conception questions.

	Risk - stake size						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	OLS	OLS	2SLS
Optimism (SOP - week 3)	-0.018			0.005			-0.055
	(0.068)			(0.084)			(0.111)
Optimism (SOP - week 2)		0.004					
		(0.068)					
Optimism (SOP - agg)			-0.005		-0.001	-0.053	
			(0.072)		(0.075)	(0.086)	
Female	-0.075	-0.022	-0.022	-0.022	-0.022	0.0062	0.036
	(0.173)	(0.178)	(0.178)	(0.178)	(0.178)	(0.197)	(0.201)
IQ (Raven)	-0.027	-0.023	-0.022	-0.023	-0.022	-0.030	-0.030
	(0.042)	(0.042)	(0.042)	(0.042)	(0.043)	(0.044)	(0.044)
Mood (week 3)					-0.007		
					(0.042)		
Conscientiousness (agg)						0.072	
						(0.089)	
Extraversion (agg)						-0.061	
						(0.080)	
Openness (agg)						0.143**	
						(0.071)	
Agreeableness (agg)						0.054	
						(0.099)	
Neuroticism (agg)						-0.101	
							(0.090)
Conscientiousness (week 3)							0.060
							(0.098)
Extraversion (week 3)							-0.061
							(0.089)
Openness (week 3)							0.126*
							(0.076)
Agreeableness (week 3)							0.043
							(0.112)
Neuroticism (week 3)							-0.101
							(0.090)
Constant	4.252***	4.190***	4.198***	4.189***	4.241***	3.554***	3.824***
	(0.259)	(0.265)	(0.265)	(0.268)	(0.362)	(0.876)	(0.940)
R^2	0.002	0.001	0.001	0.001	0.012	0.019	0.021
N	348	335	335	335	335	335	335

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. While "Risk - stake size" was elicited in week 3, models 1 and 2 use the SOP measures from weeks 3 and 2, respectively. Models 3,5 and 6 use the SOP measure aggregated over these two weeks, with model 5 including mood (beginning of session in week 3) and model 6 including the Big Five (aggregated across weeks 2 and 3) as controls. Models 4 and 7 are two-stage least squares estimations using the variables for SOP and the Big Five from week 2 as instruments for those from week 3.

Table 3.A.9. Robustness check: Relationship between optimism and risk conception questions.

	Risk - stake size (gains)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	OLS	OLS	2SLS
Optimism (SOP - week 3)	0.036			0.031		-0.060	
	(0.066)			(0.081)		(0.106)	
Optimism (SOP - week 2)		0.025					
		(0.065)					
Optimism (SOP - agg)			0.034		0.023	-0.033	
			(0.069)		(0.072)	(0.083)	
Female	-0.308*	-0.316*	-0.316*	-0.316*	-0.316*	-0.273	-0.257
	(0.168)	(0.171)	(0.171)	(0.171)	(0.171)	(0.189)	(0.193)
IQ (Raven)	0.013	0.004	0.004	0.004	0.002	0.023	0.025
	(0.040)	(0.041)	(0.041)	(0.041)	(0.041)	(0.042)	(0.042)
Mood (week 3)					0.021		
					(0.040)		
Conscientiousness (agg)						-0.009	
						(0.086)	
Extraversion (agg)						0.147*	
						(0.076)	
Openness (agg)						0.015	
						(0.068)	
Agreeableness (agg)						-0.146	
						(0.095)	
Neuroticism (agg)						-0.047	
						(0.078)	
Conscientiousness (week 3)							-0.036
							(0.094)
Extraversion (week 3)							0.159*
							(0.085)
Openness (week 3)							0.018
							(0.073)
Agreeableness (week 3)							-0.113
							(0.107)
Neuroticism (week 3)							-0.048
							(0.087)
Constant	4.271***	4.305***	4.298***	4.301***	4.171***	4.479***	4.375***
	(0.250)	(0.255)	(0.255)	(0.258)	(0.348)	(0.841)	(0.904)
R^2	0.012	0.011	0.011	0.012	0.012	0.033	0.033
N	348	335	335	335	335	335	335

Notes. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. While “Risk - stake size (gains)” was elicited in week 3, models 1 and 2 use the SOP measures from weeks 3 and 2, respectively. Models 3,5 and 6 use the SOP measure aggregated over these two weeks, with model 5 including mood (beginning of session in week 3) and model 6 including the Big Five (aggregated across weeks 2 and 3) as controls. Models 4 and 7 are two-stage least squares estimations using the variables for SOP and the Big Five from week 2 as instruments for those from week 3.

Table 3.A.10. Robustness check: Relationship between optimism and risk conception questions.

	Risk - stake size (losses)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	2SLS	OLS	OLS	2SLS
Optimism (SOP - week 3)	-0.177***			-0.331***			-0.375***
	(0.068)			(0.085)		(0.113)	
Optimism (SOP - week 2)		-0.266***					
		(0.066)					
Optimism (SOP - agg)			-0.248***		-0.217***	-0.264***	
			(0.071)		(0.074)	(0.086)	
Female	-0.004	0.078	0.078	0.075	0.079	0.022	0.051
	(0.174)	(0.175)	(0.176)	(0.179)	(0.176)	(0.197)	(0.205)
IQ (Raven)	-0.084**	-0.076*	-0.075*	-0.072*	-0.072*	-0.073*	-0.067
	(0.042)	(0.042)	(0.042)	(0.043)	(0.042)	(0.044)	(0.045)
Mood (week 3)					-0.061		
					(0.041)		
Conscientiousness (agg)						0.030	
						(0.089)	
Extraversion (agg)						0.002	
						(0.080)	
Openness (agg)						0.060	
						(0.071)	
Agreeableness (agg)						0.060	
						(0.099)	
Neuroticism (agg)						0.0311	
						(0.081)	
Conscientiousness (week 3)							-0.028
							(0.100)
Extraversion (week 3)							0.041
							(0.090)
Openness (week 3)							0.035
							(0.078)
Agreeableness (week 3)							0.079
							(0.114)
Neuroticism (week 3)							0.004
							(0.092)
Constant	5.056***	5.049***	5.028***	5.094***	5.392***	4.171***	4.467***
	(0.259)	(0.261)	(0.263)	(0.269)	(0.358)	(0.875)	(0.957)
R^2	0.032	0.058	0.047	0.020	0.054	0.051	0.017
N	348	335	335	335	335	335	335

Notes. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. While "Risk - stake size (losses)" was elicited in week 3, models 1 and 2 use the SOP measures from weeks 3 and 2, respectively. Models 3,5 and 6 use the SOP measure aggregated over these two weeks, with model 5 including mood (beginning of session in week 3) and model 6 including the Big Five (aggregated across weeks 2 and 3) as controls. Models 4 and 7 are two-stage least squares estimations using the variables for SOP and the Big Five from week 2 as instruments for those from week 3.

Table 3.A.11. Robustness check to Table 3.3: Alternative specifications showing the relationship between the general risk question and optimism controlling for the risk conception questions.

Specification 1	General risk question					
	(1)	(2)	(3)	(4)	(5)	(6)
Optimism (LOT - week 3)	0.512***	0.212**	0.252***	0.512***	0.483***	0.421***
	(0.114)	(0.093)	(0.094)	(0.114)	(0.111)	(0.111)
Risk - neg/pos		0.796***	0.919***			
		(0.075)	(0.069)			
Risk - stake size		0.112*		-0.009		
		(0.067)		(0.076)		
Risk - stake size (gains)		0.155**			0.363***	
		(0.068)			(0.076)	
Risk - stake size (losses)		-0.264***				-0.379***
		(0.070)				(0.073)
Female	-0.602**	-0.265	-0.301	-0.602**	-0.471**	-0.580**
	(0.244)	(0.194)	(0.198)	(0.244)	(0.238)	(0.235)
IQ (Raven)	-0.144**	-0.156***	-0.138***	-0.145**	-0.141**	-0.175***
	(0.059)	(0.047)	(0.047)	(0.059)	(0.057)	(0.057)
Constant	3.778***	2.382***	1.613***	3.816***	2.304***	6.055***
	(0.632)	(0.682)	(0.534)	(0.708)	(0.685)	(0.750)
R^2	0.08	0.41	0.08	0.14	0.15	0.44
N	326	326	326	326	326	326

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variables consist of the answers to the general risk question elicited on an 11-point scale. The optimism measure LOT is the Life Orientation Test elicited in week 3.

Table 3.A.12. Robustness check to Table 3.3 - continued: Alternative specifications showing the relationship between the general risk question and optimism controlling for the risk conception questions.

Specification 2			General risk question			
	(1)	(2)	(3)	(4)	(5)	(6)
Optimism (SOP - week 2)	0.442***	0.130*	0.171**	0.442***	0.433***	0.345***
	-0.092	-0.076	-0.076	-0.092	-0.089	-0.091
Risk - neg/pos		0.812***	0.925***			
		-0.073	-0.068			
Risk - stake size		0.130*		-0.006		
		-0.067		-0.075		
Risk - stake size (gains)		0.149**			0.376***	
		-0.067			-0.075	
Risk - stake size (losses)		-0.263***				-0.365***
		-0.069				-0.073
Female	-0.652***	-0.306	-0.338*	-0.652***	-0.533**	-0.624***
	(0.242)	(0.191)	(0.195)	(0.242)	(0.235)	(0.234)
IQ (Raven)	-0.125**	-0.140***	-0.123***	-0.125**	-0.127**	-0.153***
	-0.058	-0.045	-0.046	-0.058	-0.056	-0.056
Constant	5.759***	3.094***	2.568***	5.785***	4.142***	7.604***
	(0.361)	(0.537)	(0.371)	(0.478)	(0.475)	(0.508)
R^2	0.093	0.452	0.422	0.093	0.157	0.156
N	335	335	335	335	335	335

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variables consist of the answers to the general risk question elicited on an 11-point scale. The optimism measure SOP was elicited in week 2.

Table 3.A.13. Robustness check to Table 3.3 - continued: Alternative specifications showing the relationship between the general risk question and optimism controlling for the risk conception questions.

Specification 3	General risk question					
	(1)	(2)	(3)	(4)	(5)	(6)
Optimism (LOT - week 2)	0.543***	0.207**	0.241***	0.543***	0.530***	0.451***
	(0.111)	(0.0904)	(0.0916)	(0.112)	(0.108)	(0.109)
Risk - neg/pos		0.804***	0.922***			
		(0.073)	(0.067)			
Risk - stake size		0.128*		-0.0114		
		(0.067)		(0.075)		
Risk - stake size (gains)		0.151**			0.375***	
		(0.066)			(0.075)	
Risk - stake size (losses)		-0.264***				-0.377***
		(0.069)				(0.072)
Female	-0.654***	-0.307	-0.338*	-0.655***	-0.536**	-0.624***
	(0.242)	(0.190)	(0.194)	(0.242)	(0.234)	(0.233)
IQ (Raven)	-0.149**	-0.150***	-0.133***	-0.149**	-0.150***	-0.174***
	(0.058)	(0.046)	(0.046)	(0.058)	(0.056)	(0.056)
Constant	3.740***	2.326***	1.658***	3.786***	2.174***	5.959***
	(0.606)	(0.651)	(0.507)	(0.680)	(0.664)	(0.722)
R^2	0.094	0.456	0.425	0.094	0.158	0.163
N	335	335	335	335	335	335

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variables consist of the answers to the general risk question elicited on an 11-point scale. The optimism measure LOT is the Life Orientation Test elicited in week 2.

Table 3.A.14. Robustness check to Table 3.4:
Optimism and Risk taking behavior using each risk premium separately.

	Risk premium choice list 1		
	(1)	(2)	(3)
Optimism (SOP)	-0.104**		-0.073*
	(0.040)		(0.041)
General risk question		-0.087***	-0.077***
		(0.023)	(0.023)
Female	0.105	0.055	0.058
	(0.103)	(0.102)	(0.102)
IQ (Raven)	0.080***	0.067***	0.070***
	(0.025)	(0.025)	(0.025)
Constant	-0.314**	0.131	0.136
	(0.153)	(0.203)	(0.202)
R^2	0.047	0.069	0.078
N	348	348	348

	Risk premium choice list 2		
	(1)	(2)	(3)
Optimism (SOP)	-0.067		-0.023
	(0.041)		(0.041)
General risk question		-0.111***	-0.108***
		(0.023)	(0.023)
Female	0.354***	0.288***	0.289***
	(0.105)	(0.102)	(0.102)
IQ (Raven)	-0.026	-0.042*	-0.049*
	(0.025)	(0.025)	(0.025)
Constant	-0.020	0.609***	0.611***
	(0.156)	(0.203)	(0.203)
R^2	0.046	0.102	0.103
N	348	348	348

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$;*** $p < 0.01$. Choice list 1 elicits the risk premium for a lottery with 25% chance of receiving 15€ and 75% chance of receiving nothing, while choice list 2 elicits the risk premium for a lottery with 50% chance of receiving 15€. The dependent variables are aggregates over measurements in weeks 1 and 3.

Table 3.A.15. Robustness check to Table 3.4 - continued:
Optimism and Risk taking behavior using each risk premium separately.

	Risk premium choice list 3		
	(1)	(2)	(3)
Optimism (SOP)	-0.073*		-0.045
	(0.040)		(0.040)
General risk question		-0.077***	-0.070***
		(0.022)	(0.023)
Female	0.640***	0.596***	0.597***
	(0.101)	(0.101)	(0.101)
IQ (Raven)	-0.034	-0.045*	-0.043*
	(0.024)	(0.024)	(0.024)
Constant	-0.168	0.242	0.245
	(0.151)	(0.199)	(0.199)
R^2	0.125	0.146	0.149
N	348	348	348

Notes. OLS regressions. Standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Choice list 3 elicits the risk premium for a lottery with 75% chance of receiving 15€ and 25% chance of receiving nothing. The dependent variable is an aggregate over measurements in week 1 and 3.

4

Will you win? Optimism, risk assessment and risk preferences

Joint work with Armin Falk, Thomas Dohmen and Simone Quercia

4.1 Introduction

Risk is a key feature of most economic decisions, be they investment decisions, choosing a career, or buying a house. This ubiquity is reflected in the amount of research concerning decision-making under risk. Regardless of whether decision-making under risk is itself the object of study or whether another research subject just contains a risk element, a way of eliciting risk preferences is usually needed. The measures available range from lottery choices to self-reported willingness to rake risk.

However, convergent correlations between different measures are often low (Frey et al., 2017). A possible explanation is that these measures differ in the degree to which the underlying situation probabilities and outcomes are spelled out. But even when different representations of risk contain the same "hard facts", can they evoke different responses. For example, Slovic (1987) show that a frequentist representation (e.g., 1 in 10) may be perceived differently than a probabilistic one (e.g., 10%). Similarly, framing may affect measured risk preferences (Tversky and Kahneman, 1981).

In this paper, we do not view such behavior as erratic but will argue that heterogeneity in responses to different risk preference measures is explained not only by differences in risk preferences in different scenarios but rather by heterogeneity in another factor that affects the way people interpret risky situations.

To structure our line of argument we think of risk preference measurement in terms of the following framework. Let risk preference measure R be the outcome of a function $f(r,X)$ where r is a risk preference parameter that describes a person's taste for riskiness and X is a vector of other factors, part of which may be systematic and part of which may be noise. We stay agnostic about the exact content of X but will argue that amongst the elements of that vector there is one that is captured by a novel measure that we introduce.

For this purpose we present evidence from a longitudinal laboratory experiment with a total of 370 subjects. It introduces the "Will you win?" task showing that even when a lottery is completely described and objective probabilities explicitly stated do people's assessments of whether or not they will win the lottery vary. The measure consists of subjects' self-stated assessment of whether they will win a 50-50 lottery on a scale from "absolutely sure to lose" to "absolutely sure to win". We find that there is substantial heterogeneity in the responses to that question. Moreover, that heterogeneity is systematic in the sense that it is related to a stable character trait that but not to the transitory state of mood.

Responses to the "Will you win?" question exhibit strong correlations with the standard risk preference measures we elicited. These range from rather cognitive lists of lottery choices with a fully described and abstract choice environment, to the more affect-driven bomb risk elicitation task where probabilities are known but lotteries not explicitly stated, to the general risk question that is open to subjective interpretations of the task. Correlations between "Will you win?" and risk preference measures are robust to including a rich set of control variables, e.g., sociodemographics, cognitive ability, numeracy and optimism. Several control tasks enable us to test the following possible channels that could be driving these results.

A possible interpretation of our results would be that subjects exhibit heterogeneity in the way the process probabilities. The same objective probability could always be perceived as more likely by some people compared to others, i.e., some subjects could have a systematic bias in one or the other direction in the way they process probabilities. We test this channel using a control task where subjects estimate the number of balls in an urn (see Section 4.4.1). The measure generated from this task shows no systematic relationship with the "Will you win?" task or measured risk preferences.

Another interpretation that our data allows us to explore is that the response to the "Will you win?" question is related to the extent to which subjects violate the axioms of expected utility theory and hence engage in probability weighting. If this were the case, those who engage in probability weighting (e.g., exhibit the common ratio effect) would also be more likely to exhibit a correlation between the "Will you win?" task and measured risk preferences. Again, there is no compelling evidence that this is the case.

An alternative interpretation of the heterogeneity in subjects' responses to the "Will you win?" question would be that for risky situations that regard themselves, like the 50-50 lottery in that task, subjects hold beliefs that differ from objective probabilities, and that these beliefs differ systematically between subjects. If this was the case, heterogeneity in beliefs may translate into heterogeneity in risk preference measures. We test whether subjects actually hold different beliefs for events that concern themselves compared to general events in one of our control tasks (see Section 4.4.3). Although this seems to be the case, we do not find any systematic relationship between the measures elicited in the control task and either the “Will you win?” task or measured risk preferences.

Since according to our data, none of the professed interpretations explains our empirical findings regarding the “Will you win?” task, we propose another explanation in Section 4.5; namely that responding that oneself is more likely to win a 50-50 lottery is expressive of focusing on positive outcomes when confronted with decision-making under risk rather than having a belief that differs from 50% probability. This would be in line with the findings of a companion paper (Dohmen et al., 2018b).

The remainder of this paper is structured as follows. Section 4.2 contains the design of our experiment. Section 4.3 presents its main results, while Section 4.4 presents evidence on specific channels that may be driving them and Section 4.5 concludes.

4.2 Design of the experiment

The following measures were elicited as part of a longitudinal experiment, taking place over the course of three consecutive weeks. The experiment is part of a larger project on preferences and beliefs in the risk but also in the social domain. In what follows we will focus on the risk domain and on those variables that are of interest to the research question of this paper. For a complete overview of all tasks refer to Table 4.A.1 in the appendix. The fact that the experiment runs over several weeks and contains tasks from different domains that may serve as filler tasks offers the advantage that potentially related measures can be elicited with minimal influence on one another. For example, the “Will you win?” measure is elicited in week 2, while lottery choice measures eliciting risk preferences are elicited in weeks 1 and 3. Furthermore, some essential measures are repeated to reduce measurement error.

4.2.1 Main measure: “Will you win?”

A main contribution of the paper is that we introduce a novel non-choice measure designed to capture subjects’ assessment of a risky situation.

In this measure, subjects are exposed to a risky situation; without having a choice to opt in or out. They take part in a lottery with a 50% chance of winning 15 € and a 50% probability of getting nothing. Objective probabilities in this setup are explicitly announced to subjects. Their task is to state whether they will win the lottery and receive 15 € or lose the lottery and receive nothing on an 11-point Likert scale ranging from “absolutely sure to lose” to “absolutely sure to win”. The aim of this task is to measure to what extent subject’s assessment of the likelihood of winning the lottery deviates from the middle option that we interpret as the objective probability.

We elicit this in a verbal fashion rather than asking for a specific number. This is done to evoke a subjective perception and to avoid subjects feeling the need to “get it right”. Moreover, the task is unincentivized to avoid making risk preferences a confounding factor by creating strong hedging motives for risk-averse subjects. At the same time, monetary incentives might crowd out the affective response we would like.

This measure is taken in the beginning of the second week such that it remains uninfluenced by other risk belief measures.

4.2.2 Risk preference measures

We employ three different measures of risk preferences; risk premia of lotteries, the general risk question, and the Bomb Risk Elicitation Task (BRET). These measures differ along several dimensions, most importantly the degree of cognition involved in answering the task and how explicitly the choice environment (probabilities and outcomes) is described.

4.2.2.1 General risk question

The first measure we employ is the general risk question (Dohmen et al., 2011) as used in the German Socio-economic Panel (see for example Wagner et al., 2007). It asks subjects to answer the following question on an 11-point Likert scale: "Are you generally a person who is willing to take risks or do you try to avoid taking risks?".

This measure offers scope for different interpretations of the question. For example, there may be heterogeneity in what subjects think about concerning stake size, or concerning gains and losses of risk taking (see Dohmen et al., 2018b). Thus, the answer to this question may be influenced most strongly by the way subjects tend to assess risky situations. To reduce measurement error and to learn about test-retest stability, this measure was elicited in all three weeks of the experiment.

4.2.2.2 Risk premia of lotteries

Eliciting the risk premium of a lottery is an established way to measure preferences over risk (Andreoni and Sprenger, 2011; Dohmen et al., 2011). An advantage of the method compared to many other lottery choice-based measures, most prominently the one by Holt and Laury (2002), is that there is no need to make assumptions about the shape of the utility function. Instead it relies on the classic definition of risk aversion based on mean-preserving spreads as introduced by Stiglitz and Rothschild (1970).

To cover different ranges of probabilities we elicit the certainty equivalent of three different lotteries: receiving a prize of 15 € with 25%, 50%, and 75% probability, respectively, and receiving nothing otherwise. We do so using a choice list format where subjects choose between the lottery and a certain payoff. In addition, we elicit the difference in risk premia when both options are lotteries.

There is full information about the decision environment, i.e., objective probabilities as well as outcomes for each lottery are explicitly stated. One row of one choice list is randomly selected for payment if this part of the experiment is paid. Refer to Section 4.A.2 in the appendix for a detailed description of the choice lists.

To reduce measurement error, this measure was elicited in the first and third week of the experiment. It was omitted, however, in the second week where the "Will you win" measure was elicited to avoid the two influencing one another.

4.2.2.3 Bomb risk elicitation task

Our third measure of risk preferences is the dynamic version of the bomb risk elicitation task (BRET; Crosetto and Filippin, 2013). This task aims at providing a more affect-driven risk measure than lottery choices. Subjects express their willingness to take risk in the following way. On the screen they see a 10-by-10 grid of boxes, one of which is automatically collected each second. Subjects receive 0.30 € per collected box. But one randomly placed box out of the 100 boxes contains a hidden "bomb" which reduces payment to 0 if collected. By clicking on a stop button, subjects decide how many boxes they want to collect. To avoid truncation of decisions, subjects are only informed at the end of the session which box contained the bomb and whether they collected it. As each collected box increases the potential payoff by 0.30 € as well as the probability of receiving nothing by 1 percentage point, they essentially choose between 100 lotteries of the type $\langle 0.30€ * x, (1-x)\%;\ 0€, x\% \rangle$ where $x \in \{1, 2, ..., 100\}$ denotes the number of collected boxes. Thus, a risk-neutral agent should choose $x = 50$, which yields an expected value of 15 €, while a risk-averse agent should choose $1 \leq x < 50$, and a risk loving one $50 < x < 100$. Detailed instructions and control questions made sure that subjects understood the trade-off between the increasing potential payoff and the increasing likelihood of collecting the bomb.

Although, same as in the choice list measures, there is full information about all outcomes and their assigned probability, this is less obviously presented to subjects. Thus, the task may provide larger scope for heterogeneity in assessment of the risky situation than the choice list measures.

4.2.3 Questionnaire

The questionnaire parts of the experiment include several variables that we will use as control variables in the regression analyses. The character trait optimism will be of particular importance. We elicit it using two different measures: the established Life Orientation Test (henceforth LOT; Scheier et al., 1994; Herzberg et al., 2006) and a very short two-item optimism measure (Kemper et al., 2015). For the exact wording of both refer to Section 4.A.3 in the appendix. Similar to Kemper et al. (2015), we find a convergent correlation between SOP and LOT of $\rho = .77$ (Spearman correlation coefficient, $p < .001$, optimism measures aggregated across weeks). In the main text of the paper, we restrict our analyses to the SOP measure but results are virtually the same if LOT is used (see Section 4.A.8).

Other relevant questionnaire measures include a mood measure both at the beginning and end of each week's experiment, a 15-item Big Five personality inventory (Schupp and Gerlitz, 2008) as well as sociodemographic information. Furthermore, we perform a detailed test of cognitive ability at the end of the third week. Subjects' numeracy regarding probabilities is tested using the Berlin numeracy test (Cokely et al., 2012); an adaptive test containing three to four math problems in text form.

4.2.4 Procedures

A total of 370 subjects participated in our experiment in the summer and fall of 2016. Participants were recruited from the BonnEconLab subject pool using the software hroot (Bock et al., 2014). The vast majority of subjects were students (95%), with mean age 22.3 years. The experiment was computerized using z-Tree (Fischbacher, 2007) and took place over the course of three consecutive weeks. Each week's experiment lasted about one hour, during which subjects completed several parts of the experiment that were clearly labeled as distinct. Subjects were also informed that one randomly selected part per week would be paid out. We chose this payment scheme to preclude hedging and to keep the stakes within one part sizable enough for subjects to exert effort. Attrition was kept at a low rate of 9% by rewarding subjects a bonus of 10 € if they participated in all three weeks. On average, subjects received a total payoff of 31.99 € (about 35 US-$ at the time).

4.3 Results

4.3.1 "Will you win?" measure

The implicit assumption of standard models of decision making under risk is that subjects take objective probabilities at face value. This would mean that in the ”Will you win?” measure, where subjects were asked to state how likely they thought it was that they would win a 50-50 lottery on an 11-point Likert Scale, everyone chooses the middle option. Interestingly, only about half the subjects (52%) do. About 30% express that they are more likely to win than to lose a 50-50 lottery, while 18% state the opposite (see Table 4.1). For the full distribution refer to Figure 4.A.1 in the appendix.

Result 1. *About half the subjects deviate from the middle option when asked to assess their chances of winning a 50-50 lottery.*

Table 4.1. ”Will you win?” measure: Answers on an 11-point Likert scale ranging from "absolutely sure to lose" (-5) to "absolutely sure to win" (+5).

	Losing more likely (-5 to -1)	Middle option (0)	Winning more likely (+1 to +5)
Absolute frequency	64	182	103
Relative frequency	18%	52%	30%

The regression in Table 4.2 is constructed to understand whether the “Will you win?” measure actually expresses a relatively stable trait-like tendency to assess risk or whether it is rather influenced by subjects’ current state of mind. For this purpose we regress “Will you win?” on two factors that may influence beliefs: a stable character trait, optimism, and a more transitory state, mood.[1] Specifications 1 and 2 regress “Will you win?” on optimism (without and with controls). Specifications 3 and 4 do the same for mood. Although the coefficients on both optimism and mood are significantly different from zero in the respective regressions, the explanatory power of optimism exceeds that of mood in the simple regression ($R^2_{opt} = .119$ and $R^2_{mood} = .019$) as well as in the regression containing control variables ($R^2_{opt} = .140$ and $R^2_{mood} = .029$). More importantly, once both are included in the regression (specifications 5 and 6), only the coefficient on optimism is sizable and significantly different from zero. This

[1] In line with much of the personality psychology literature (Carver et al., 2010; Carver and Scheier, 2014), we view optimism as a stable disposition akin to a personality trait affecting beliefs in specific environments. One such environment may be decision-making under risk. The finding of Felton et al. (2003) that optimism in males increases investment in stocks is in line with that argument.

Table 4.2. Explanatory variables of the ”Will you win?” measure

	“Will you win?”					
	(1)	(2)	(3)	(4)	(5)	(6)
Optimism (SOP)	0.464***	0.504***			0.492***	0.419***
	(0.072)	(0.074)			(0.078)	(0.091)
Mood (beginning week 2)			0.116**	0.115**	0.021	0.020
			(0.045)	(0.045)	(0.045)	(0.046)
Female		-0.232		-0.238	-0.234	-0.108
		(0.198)		(0.210)	(0.199)	(0.218)
Budget (in 100 €)		0.016		0.041	0.016	0.013
		(0.043)		(0.045)	(0.043)	(0.043)
Political Orientation		0.029		-0.007	0.028	0.038
		(0.071)		(0.075)	(0.071)	(0.073)
Cognitive ability (Raven)		-0.004		0.002	-0.006	-0.007
		(0.046)		(0.049)	(0.047)	(0.048)
Numeracy		0.024		0.024	0.023	0.021
		(0.094)		(0.099)	(0.094)	(0.094)
Conscientiousness						-0.017
						(0.096)
Extraversion						-0.007
						(0.083)
Agreeableness						0.026
						(0.107)
Neuroticism						-0.159*
						(0.086)
Openness						0.093
						(0.078)
Constant	-2.085***	-2.355***	-0.533*	-0.599	-2.423***	-1.944*
	(0.373)	(0.567)	(0.311)	(0.538)	(0.586)	(1.073)
N	349	335	349	335	335	335
R^2	0.106	0.132	0.019	0.029	0.133	0.146

Notes. OLS regressions. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Answers to “Will you win?” measure (dependent variable) are on an 11-point Likert scale ranging from "absolutely sure to lose" (-5) to "absolutely sure to win" (+5). The optimism measure consists of the two-item SOP measure (7-point Likert scale). It is aggregated over measurements of weeks 2 and 3. Current mood is elicited in the beginning of the experiment in week 2 on an 11-point Likert scale. The Big Five measures were elicited in all three weeks. They are aggregated over measurements of all three weeks. The control variables Female, Budget, and Political Orientation were elicited in week 1. Throughout the paper we do not include age as a control variable since its variation is limited in a student sample and estimates would hence be strongly influenced by outliers.

pattern is consistent with mood containing a stable component related to optimism, but the transitory part of mood that remains once optimism is controlled for not influencing the answers to the “Will you win?” question. The stable character trait optimism, however, seems to be systematically related to the "Will you win?" measure. We interpret this as evidence for the measure expressing a stable characteristic.

Result 2. *Subjects' assessment of their chances of winning a 50-50 lottery are systematically related to a stable character trait.*

4.3.2 Quantitative analysis: "Will you win?" measure and risk preferences

4.3.2.1 Descriptive statistics - risk preference measures

General risk question. The first measure of risk preferences is the general risk question. It was asked each week at the beginning of the experiment (with only the mood measure preceding it). In Table 4.A.2 in the appendix we report subjects' answers in each week as well as an aggregate measure created by averaging over all weeks. The answers in different weeks correlate significantly with one another (Pearson correlation coefficient: $\rho_{12} = .78$, $\rho_{23} = .80$, $\rho_{13} = .74$, $p < .001$ for all) indicating good test-retest reliability (see also Section 4.A.4.1 in the appendix). On average, subjects report a value of 5.195 on a Likert scale ranging from 0 (not at all willing to take risks) to 10 (very willing to take risks). This is consistent with values found in the German Socio-economic Panel for a similar age group.[2]

Risk Premia. Our second measure of risk preferences consists of the risk premia of lotteries. For a detailed account of how these were calculated refer to Section 4.A.2 in the appendix.

The means and standard deviations for subjects' risk premia are reported in Table 4.3 as aggregates over weeks 1 and 3. Pearson correlation coefficient between the two measurements vary between $\rho = .37$ and $\rho = .48$ for the lottery with 25% and that with 75% probability of winning, respectively ($p < .001$ for all). Note that a positive risk premium indicates risk aversion, while a negative risk premium indicates risk seekingness. In line with frequently observed patterns of risk attitudes (Harbaugh et al., 2010), subjects *on average* engage in risk seeking behavior when chances of winning are low (25%), while exhibiting risk aversion for lotteries with 50% and 75% probability of winning 15 €. When both options are lotteries with 50% probability attached to each outcome, subjects are also risk averse. Refer to Figure 4.A.2 in the appendix for the exact distributions.

Bomb Risk Elicitation Task (BRET). In this task subjects had to decide how many out of 100 boxes to collect, with 50 boxes indicating risk neutrality.

[2] The average answer to the general risk question reported by people born before 1993 and after 1983 in the 2013 survey is 5.168.

Table 4.3. Aggregate Risk Premia Elicited Through Choice Lists.

	Risk Premium 25% of 15 €	Risk Premium 50% of 15 €	Risk Premium 75% of 15 €	Risk Premium Both Lotteries
Mean	-1.74	0.11	1.33	1.86
SD	(2.063)	(2.108)	(2.281)	(1.843)
N	368	368	368	368

According to BRET, 62% of subjects are classified as risk averse, while 10% are classified as risk-neutral, and 28% as risk-loving (see also Figure 4.A.4 in the appendix).

4.3.2.2 Risk assessment and risk preferences

As a next step we examine the relationship between responses to the "'Will you win?" question and risk preference measures. If risk preference measures indeed contained a component related to the assessment of the riskiness of a situation, we would find that subjects expressing more optimistic views on their chances of winning in the "Will you win?" question would also exhibit lower degrees of risk aversion as measured by standard risk preference measures than those who state the realistic middle option. Similarly, those expressing pessimistic views on their chances of winning would exhibit the higher degrees of risk aversion. The data reported in Table 4.4 shows just the expected pattern. Subjects who state that they are more likely to lose the lottery in the "Will you win" question have an average response of 4.315 on the scale from 0 to 10 for the general risk question, while the response of subjects who think they are more likely to win is around three-quarters of a standard deviation higher at 5.833 (significant difference; Wilcoxon rank-sum test: $p < .0001$). Similarly, subjects stating they are more likely to lose the lottery exhibit significantly higher risk premia (i.e. more risk aversion) than those who state they are more likely to win it for all three lotteries (Wilcoxon rank-sum tests: $p^{25} = .0069$, $p^{50} = .0295$, and $p^{75} = .0018$.) The same tendency is visible for the Bomb Risk Elicitation task (Wilcoxon rank-sum test: $p = .0671$). For the choice list where both options are lotteries, however, measured risk premia do not differ significantly between those subjects expressing they are more likely to lose and those expressing they are more likely to win the 50-50 lottery (Wilcoxon rank-sum test: $p = .4197$.)

The same relationship can be expressed in terms of a simple regression of risk preference measures on "Will you win?" (see Table 4.5, Panel 1). The coefficients on "Will you win?" are significantly different from zero for the general risk question, risk premia elicited from choice lists and the Bomb Risk Elicitation Task.

Table 4.4. Measured risk preferences grouped by response to “Will you win?”.

"Will you win?"		General risk question	Risk Premium 25% of 15 €	Risk Premium 50% of 15 €	Risk Premium 75% of 15 €	Risk Premium Both Lotteries	BRET
Losing more likely	Mean	4.315	-1.383	.558	2.184	2.238	-10.806
(-5 to -1)	SD	(1.993)	(1.942)	(2.021)	(2.386)	(1.933)	(13.809)
	N	64	64	64	64	64	62
Middle option	Mean	5.071	-1.594	.076	1.190	1.633	-6.511
(0)	SD	(2.035)	(1.987)	(2.020)	(2.259)	(1.763)	(-6.511)
	N	182	181	181	181	181	176
Winning more likely	Mean	5.833	-2.154	-.063	1.102	2.016	-6.236
(+1 to +5)	SD	(1.927)	(2.181)	(2.257)	(2.173)	(1.880)	(15.813)
	N	124	123	123	123	123	110

Notes. Risk preference measures are aggregated over multiple measurements when applicable, i.e., weeks 1 and 3 for choice lists, and weeks 1, 2 and 3 for the general risk question.

Table 4.5. Linear Regressions of the Risk Preference Measures.

Panel 1: Simple Regression	General risk question (1)	Risk Premium 25% of 15 € (2)	Risk Premium 50% of 15 € (3)	Risk Premium 75% of 15 € (4)	Risk Premium Both Lotteries (5)	BRET (6)
”Will you win?”	0.349***	-0.150**	-0.194***	-0.261***	-0.041	1.083**
	(0.059)	(0.061)	(0.063)	(0.068)	(0.055)	(0.471)
Constant	5.091***	-1.638***	0.124	1.385***	1.878***	-7.354***
	(0.106)	(0.108)	(0.113)	(0.120)	(0.099)	(0.827)
N	349	347	347	347	347	335
R^2	0.091	0.017	0.027	0.041	0.002	0.016

Panel 2: Including controls	General risk question (1)	Risk Premium 25% of 15 € (2)	Risk Premium 50% of 15 € (3)	Risk Premium 75% of 15 € (4)	Risk Premium Both Lotteries (5)	BRET (6)
“Will you win?”	0.244***	-0.104	-0.147**	-0.212***	-0.034	0.912*
	(0.063)	(0.063)	(0.066)	(0.068)	(0.058)	(0.503)
Female	-0.570**	0.307	0.783***	1.315***	0.734***	-4.621**
	(0.227)	(0.228)	(0.236)	(0.245)	(0.208)	(1.808)
Budget (in 100 €)	0.085*	0.030	-0.008	-0.044	0.022	0.431
	(0.049)	(0.049)	(0.051)	(0.053)	(0.045)	(0.389)
Political orientation	0.119	-0.014	-0.148*	-0.130	0.002	0.269
	(0.082)	(0.082)	(0.085)	(0.088)	(0.075)	(0.649)
Cognitive ability (Raven)	-0.102*	0.134**	-0.055	-0.068	-0.054	0.155
	(0.053)	(0.053)	(0.055)	(0.057)	(0.049)	(0.422)
Numeracy	-0.050	0.139	0.098	-0.027	-0.071	-1.686**
	(0.107)	(0.108)	(0.111)	(0.115)	(0.098)	(0.852)
Optimism (SOP)	0.343***	-0.175*	-0.086	-0.083	0.067	0.295
	(0.090)	(0.091)	(0.094)	(0.097)	(0.083)	(0.718)
Constant	3.706***	-1.974***	0.583	1.923***	1.463**	-4.602
	(0.667)	(0.669)	(0.692)	(0.717)	(0.610)	(5.298)
N	335	335	335	335	335	335
R^2	0.167	0.057	0.077	0.161	0.060	0.047

Notes. OLS regressions. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Risk preference measures are aggregated over multiple measurements when applicable, i.e., weeks 1 and 3 for choice lists, weeks 1, 2 and 3 for the general risk question. “Will you win?” is elicited on an 11-point scale, while optimism is elicited on a 7-point scale. Cognitive ability ranges from 0 to 10 correct answers, and numeracy from 0 to 4 points.

Panel 2 shows the regression including various sociodemographic control variables as well as measures of numeracy, fluid intelligence and optimism. The purpose of this exercise is to examine whether the inclusion of control variables lowers the explanatory power of responses to the "Will you win?" question for risk preferences measures. As can be seen when comparing the coefficients on "Will you win?" between Panel 1 and Panel 2, this is rarely the case. For the risk premium measures as well as for BRET (columns 2, 3, 4, 6), they are not significantly smaller when the aforementioned control variables are included (χ^2-test with $H_0: \beta_1^{\text{Panel 1}} = \beta_1^{\text{Panel 2}}; p^{25\%} = .3580, p^{50\%} = .2461, p^{75\%} = .2088$, $p^{\text{BRET}} = 0.371$.

When considering the general risk question (column 1), however, the coefficient on "Will you win?" when including control variables is significantly smaller ((χ^2-test with $H_0: \beta_1^{\text{Panel 1}} = \beta_1^{\text{Panel 2}}; p = .0015$) with optimism apparently explaining some of the variance in responses to the general risk question. Nevertheless, the coefficients on "Will you win?" are sizable for both specifications. For example in Panel 2, an increase of 2 points on the 11-point Likert scale for "Will you win?" would be comparable in size to the coefficient on gender.

These observations along with Result 2 are consistent with a framework as we describe in the introduction where standard risk preference measures do indeed contain a component beyond standard risk preference parameters.

Result 3. *Measured risk preferences are correlated with responses to the "Will you win?" measure.*

4.4 Testing possible channels

In this section we will present additional measures that we elicited during our experiment. Each is designed to test one of three related explanations for the observed relationship between the "Will you win" measure and risk preference measures. What these explanations have in common is that they relate to the probabilities assigned to positive and negative outcomes of a lottery rather than the outcomes themselves. The first explanation is that there is systematic heterogeneity in the way that subjects process probabilities. We examine this using a choice-free measure of probability perception. The second measure is based on lottery choices and elicits heterogeneity in how subjects weight probability. This is done to understand whether the response to the "Will you win" question is a manifestation of probability weighting. The third measure explores whether subjects have systematically different beliefs about the riskiness of a situation when that situation regards themselves compared to when it does not.

4.4.1 Perception of probabilities - urns task

In this task we examine whether people differ systematically in the way they process probabilities. Furthermore we want to learn whether such heterogeneity translates into heterogeneity in both the “Will you win?” measure and risk preference measures. For example, someone who systematically perceived their chances of winning as higher than they actually are would overstate their chances of winning in the “Will you win?” task. Such a person would also overestimate their chances of winning in a lottery and as a result demand a higher risk premium than someone who perceived probabilities realistically. Hence, the correlation between reported chances of winning in the “Will you win?” task and risk preference measures as we observe in the data.

The control task we report here aims to measure if and to what extent such a systematic heterogeneity in the way that subjects process signals relating to their chances of winning actually exists. In this task, subjects participate in a series of lotteries, for which objective probabilities exist but are not communicated explicitly. Instead subjects are provided with a strong visual signal: the image of an urn that they see for 1.5s. The urn contains 100 balls, each of which can be either red or black (see Figure 4.1). Subjects receive 15€ if a red ball is drawn, and nothing otherwise. Subjects' task is to state their estimated chance of winning the lottery. The idea of the task is that those with a disposition towards perceiving their chances in lotteries more favorably, perceive signals of probabilities more positively and, hence, state seeing a higher number of red balls. If the urns task is selected for payment, either belief elicitation is paid or one of the lotteries is played out (see Section 4.A.5 in the appendix for details).

The task consists of 29 trials organized in two blocks with different urns, containing either a small, medium-sized, or large number of red balls, i.e., chance of winning. The sequence, in which the urns appear, as well as the position of each colored ball are pre-randomized and the same for all subjects. For an overview of the number of correct balls in each urn refer to Table 4.A.3 in the appendix. To be able to control for possible distortions during the visual perception of each particular urn, there was an additional stage before the main task, in which subjects estimated the number of red balls in each urn without any connotation of lotteries or winning and losing. A description of how the data are aggregated as well as detailed descriptive statistics can be found in Section 4.A.6 in the appendix.

Our first observation is that subjects on average overstate the number of balls both in stage 2 when they represent probabilities and in stage 1 when they do not (see Table 4.6). However, on average they to do less so in stage 2 (significant difference; Wilcoxon signed-rank test: $p < .0001$). This could either indicate that subjects estimate the number of red balls more realistically when they represent probabilities or simply reflect learning over time.

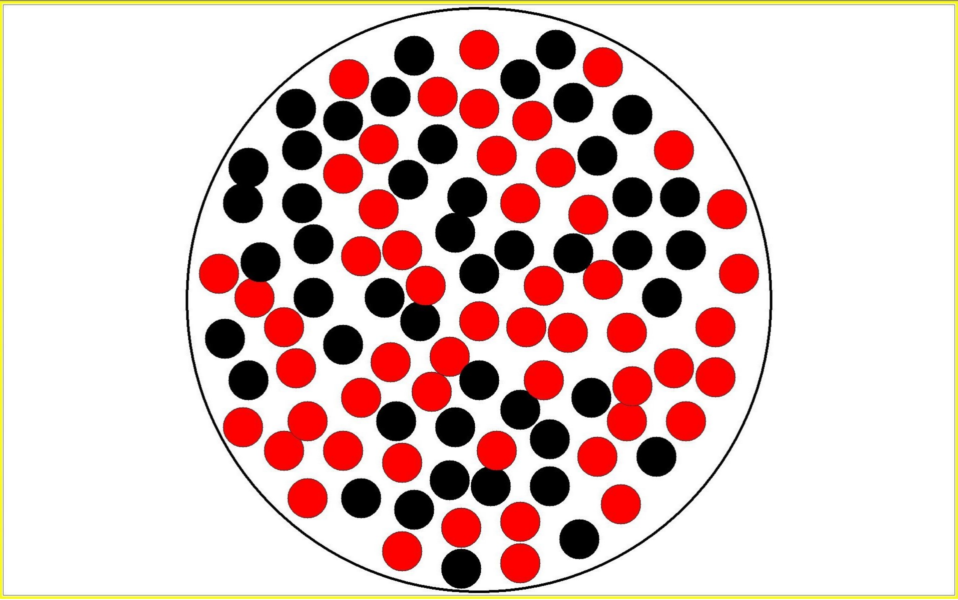


Figure 4.1. Exemplary urn. Each urn contains 100 balls and was shown for 1.5s.

Table 4.6. Urns task: Avg deviations from the correct answer

	Stage 1 (number of red balls)	Stage 2 (chances of winning)	Difference (Stage 2 - Stage 1)
Mean	3.432	2.680	-0.728
SD	2.775	2.518	2.466

Notes. N = 349. Stage 1 and Stage 2 averages are significantly different from 0 (t-tests: $p < .0001$ for both). Stage 1 and Stage 2 averages differ significantly (Wilcoxon signed-rank test: $p < .0001$).

If systematic misperception of probabilities manifested itself in stage 2 but not in stage 1, the difference between them would be the appropriate measure to capture that misperception. However, if it was captured by responses in both stages, an aggregate measure of both would be preferable. We remain agnostic about this issue and correlate both the difference and aggregate measures with risk preference measures as well as the response to "'Will you win?" (see Table 4.7). For any of the two, we would expect a lower degree of risk aversion (lower risk premia, higher response to the general risk question, more bombs collected in BRET) and a more optimistic response to "Will you win?"(i.e., a higher value), the more positively probabilities are perceived. Such a pattern is neither visible for the difference measure nor for the aggregate measure. Correlations are either not significantly different from 0 or do not go in the expected direction. Hence, the urns task does not provide evidence that the correlation

Table 4.7. Urns task: Correlations with average deviations from the true number

	"Will you win?"	General risk question	Risk Premium 25% of 15€	Risk Premium 50% of 15€	Risk Premium 75% of 15€	Risk Premium MPS	BRET
Stage 1 (number of red balls)	-0.085 (0.123)	0.006 (0.912)	0.053 (0.338)	0.047 (0.396)	0.168*** (0.002)	0.068 (0.215)	0.092* (0.092)
Stage 2 (chances of winning)	-0.103* (0.060)	-0.033 (0.542)	0.082 (0.134)	0.057 (0.300)	0.113** (0.038)	0.053 (0.329)	0.020 (0.711)
Measure 1: Differences (Stage 2 - Stage 1)	-0.028 (0.605)	-0.047 (0.388)	0.002 (0.971)	-0.025 (0.654)	-0.093* (0.089)	-0.045 (0.417)	-0.097* (0.076)
Measure 2: Aggregate (Avg (Stage 2, Stage 1))	-0.115** (0.036)	-0.027 (0.627)	0.057 (0.300)	0.039 (0.477)	0.150*** (0.006)	0.055 (0.316)	0.071 (0.192)

Notes. Spearman rank-order correlation coefficients. p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. N = 335. Risk preference measures are aggregated over multiple measurements when possible, i.e., weeks 1 and 3 for choice lists, and weeks 1, 2 and 3 for the general risk question. All non-trivial trials are included in the average.

between "Will you win?" and risk preference measures is caused by systematic misperception of probabilities.

4.4.2 Decision weights - the common ratio effect

The second control task tests whether subjects display a common ratio effect Kahneman and Tversky (1979). It serves to understand whether there is a relationship between our findings with respect to the "Will you win?" question and probability weighting.

This control task relies on a hypothetical-choice example in (Kahneman and Tversky, 1979) but uses lower stakes that are actually paid out if this part is chosen for payment. Subjects make two binary lottery choices in random order:[3]

- 16 € with 80% probability (A) versus 12 € for sure (B)
- 16 € with 20% probability (C) versus 12 € with 25% probability (D)

According to the substitution axiom that is part of expected utility theory, a subject who prefers B over A (as 71% of our subject do), should also prefer D over C because $D = \frac{1}{4} * B$ and $C = \frac{1}{4} * A$. Violations of this axiom by choosing B over A but C over D are known as the common ratio effect. This occurs for about 45% of subjects in our experiment, whereas 8% display the opposite pattern and 46% make consistent choices (see Table 4.8). These fractions are almost exactly the same as in prior research (see, e.g., Carlin, 1992).

We now examine whether the correlation between "Will you win?" and risk preference measures is caused by those subjects whose behavior violates the substitution axiom of expected utility theory. Average responses to the "Will you win?" question are slightly higher for subjects who violate the substitution

[3] All lotteries were presented as "x_1 euros with p% probability and x_2 euros with $1-p$% probability".

Table 4.8. Absolute number of subject displaying each choice pattern.

	A (16€, p=.8)	B (12€, p=1)	Total
C (16€, p=.2)	74	157	231
D (12€, p=.25)	28	89	117
Total	102	246	348

Table 4.9. Correlations between “Will you win?” measure and risk preferences by type.

	General risk question	Risk Premium 25% of 15€	Risk Premium 50% of 15€	Risk Premium 75% of 15€	Risk Premium MPS	BRET
Violate substitution axiom	0.295***	-0.258***	-0.220***	-0.177**	0.006	0.088
	(0.0001)	(0.001)	(0.003)	(0.018)	(0.941)	(0.244)
N	179	179	179	179	179	179
Consistent choices	0.289***	-0.026	-0.086	-0.217***	-0.039	0.122
	(0.0001)	(0.735)	(0.267)	(0.005)	(0.620)	(0.130)
N	170	168	168	168	168	156
All	0.289***	-0.150***	-0.151***	-0.187***	-0.009	0.106*
	(0.0000)	(0.005)	(0.005)	(0.0005)	(0.870)	(0.052)
N	349	347	347	347	347	335

Notes. Spearman correlation coefficients. p-values in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Risk preference measures are aggregated over multiple measurements when possible, i.e., weeks 1 and 3 for choice lists, weeks 1, 2 and 3 for the general risk question.

axiom than for those who make consistent choices (0.352 rather than 0.112), but not significantly so (two-sided t-test: $p = 0.205$).[4] However, for some risk preference measures the correlations between “Will you win?” and the respective measure differ between those who violate the substitution axiom and those who don’t. For lottery choices with a low and moderate chance of winning 15 € ($p = 25\%$ and $p = 50\%$, respectively) the correlation with the “Will you win?” measure seems to be driven mostly by those who violate the substitution axiom (see Table 4.9), being much lower and insignificant for those who make consistent choices. Correlations between the “Will you win?” measure and the general risk question as well as the risk premium of the lottery with a 75% chance of winning are similar for both groups of subjects.

4.4.3 Beliefs about personal risk - risk scenarios

The third explanation for the relationship between the “Will you win?” task and risk preference measures is related to the idea that subjects may have substantially different beliefs about the probability of an event occurring when the

[4] The results are quantitatively very similar if instead we classify subjects depending on whether or not they display the common ratio effect.

event concerns themselves compared to when it does not. If this were the case, someone who systematically overestimates probabilities attached to positive outcomes for themselves, would also overestimate both their chances of winning in "Will you win?" as well as in tasks measuring risk preferences, leading to the observed correlation between "Will you win?" and risk preference measures. For this reasoning to apply there needs to be scope for beliefs to vary even when objective probabilities are given (as in the "Will you win?" question and certainty equivalent elicitation). The heterogeneity in responses to the "Will you win?" question (where probabilities are also stated explicitly) indicate that this may be the true.

The following task is designed to test whether subjects indeed attach different probabilities to events that concern themselves compared to those that do not. Subjects are presented with descriptions of eight different real-world events. For each scenario they sequentially state two probabilities: i) the probability of the event happening to an average person (henceforth: *general probabilities*) and ii) the probability of it happening to themselves (henceforth: *personal probabilities*). For all scenarios objective probabilities exist (derived either from theory or official statistics) but are unknown to subjects. Our measure of interest is the difference between personal and general probabilities.

Some of the events presented in this task are completely exogenous in the sense that outcomes cannot be influenced (e.g., "What are the chances of winning any money at all in the lottery for the average person [if you played yourself]?"). These scenarios provide the cleanest measure for how much subjects think that personal probabilities differ from objective probabilities because completely rational subjects would state the same personal and general probabilities. Other scenarios, for which it is reasonable that personal and general probabilities differ due to heterogeneity in personal characteristics and behavior, were included to provide more scope for heterogeneity in responses as these scenarios make it easier for subjects to rationalize why probabilities for themselves may be different. While we cannot incentivize questions about the personal probability, we employ the same two-step procedure as described in Section 4.A.5 to elicit beliefs between 0 and 100%. The risk scenarios are included at the end of the first week to avoid interaction with other measures.

We will now present an overview of the descriptive statistics relating to this task. For the full distribution of answers to each set of questions refer to Table 4.A.5 in the appendix.

Subjects' estimations of the general probabilities of the described event differ substantially from the objective probabilities given by official statistics or theoretical calculations. But since estimations also differ substantially between the eight scenarios, it is reasonable to assume that subjects exerted effort completing the task despite its difficulty.

On average, subjects over- rather than underestimate the general probability for all events except one.[5] This is the case for both positive and negative events and is in line with the common observation that the likelihood of an event is overestimated once it is brought to someone's attention.

In line with the reasoning that people hold different beliefs about risky situation when events concern themselves, subjects report significantly different personal probabilities compared to the associated general probability for all but one scenario (Wilcoxon signed-rank tests: $p_1 < .001, p_2 < .001, p_3 < .001, p_4 < .001, p_5 = .008, p_6 = .530, p_7 < .001, p_8 < .001$). Moreover, differences between personal and general probabilities correlate significantly with one another for all but two items (see Table 4.A.6 in the appendix), thus indicating that responses are systematic.

For further analysis we aggregate the data by reverse-coding negative events and averaging over the ranks of the 8 items. This way we create three variables "Avg. rank - general probability", "Avg. rank - personal probability", and "Avg. rank - difference" for each subject, with the latter being our main measure.[6] A higher value indicates that higher probabilities are assigned to good events and lower probabilities to bad events. The measure for the personal probability created this way correlates significantly with both measures of optimism (Pearson correlation coefficients $\rho_{pers,LOT} = .144$, $\rho_{pers,SOP} = .146$, p-values .006 and .005, respectively).

Next, we examine whether a heterogeneity in the extent to which subjects over- or underestimate personal compared to general probabilities translates into heterogeneity in responses to the "Will you win?" question and risk preference measures. In Table 4.10 we report correlations between the measures elicited in the risk scenarios task and "Will you win?" as well as risk preferences. We do not find any significant correlations that would serve as evidence for the proposed channel.

4.5 Discussion and conclusion

In this paper we have introduced a novel non-choice measure designed to capture the assessment of a risky situation: the "Will you win?" task. Subjects participate in a lottery with a 50% chance of winning a prize and 50% chance of receiving nothing. Their task is to state whether they will win or lose the lottery.

[5] Subjects underestimate the probability of trains being on time by on average 67 percentage points. This constitutes by far the largest average deviation from the true probability and may have been caused by difficulties in understanding the wording of the question.

[6] We use ranks instead of absolute values because the latter will differ by scenario depending on the objective probability. Thus, using absolute values would give items with a higher variance in answers more weight.

Table 4.10. Correlations between stated probabilities, “Will you win?” and risk preference measures.

	Will you win?	General risk question	Risk Premium 25% of 15€	Risk Premium 50% of 15€	Risk Premium 75% of 15€	Difference RP Both Lotteries	BRET
“Risk scenarios: difference”	-0.042 (0.431)	0.033 (0.525)	0.008 (0.881)	0.027 (0.601)	0.078 (0.136)	-0.015 (0.769)	-0.004 (0.946)
“Risk scenarios: general prob.”	0.011 (0.838)	-0.010 (0.856)	0.003 (0.951)	-0.022 (0.671)	0.033 (0.525)	0.006 (0.912)	-0.011 (0.837)
“Risk scenarios: personal prob.”	0.046 (0.390)	-0.079 (0.131)	0.011 (0.835)	-0.066 (0.210)	-0.041 (0.431)	0.046 (0.378)	-0.036 (0.499)

Notes. Spearman rank-order correlation coefficients. N = 347 for column 1. N = 368 for columns 2 to 5. N = 348 for column 6. p-values in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Risk preference measures are aggregated over multiple measurements when possible, i.e., weeks 1 and 3 for choice lists, weeks 1, 2 and 3 for the general risk question. “Risk scenarios - general” refers to the aggregate rank of answers to the assessment of the general probability or average person. “Risk scenarios - personal” refers to the aggregate rank of answers to the assessment of the probability for oneself. “Risk scenarios - difference” refers to the aggregate rank of the difference between the two.

About half (48%) of our subjects state either that losing is more likely or that winning is more likely. We consider these responses to be systematic since they are related to a stable character trait, optimism, but not to the more transitory state of mood.

The “Will you win?” measure correlates with responses to standard measures of risk preferences like the risk premia of lotteries. This is the case even when control variables such as gender, cognitive ability and optimism are included in the regression. As the measure may invoke the need to state the middle option in the “Will you win?” question, what we find is most likely a lower bound of the influence of assessment of risk on risk preferences.

Several explanations for the close relationship between the “Will you win?” measure and measured risk preferences are possible. For example, people could process probabilities in systematically different ways or have different beliefs about the riskiness of a situation when it regards themselves compared to when it does not, or probability weighting could matter. We use different control tasks to test these three different channels but do not find evidence that any of the three plays a decisive role.

While the control tasks all target explanations relating to probabilities, it is conceivable that channels relating to outcomes explain our finding. In a companion paper based on the same data set (Dohmen et al., 2018b) we argue that there is systematic heterogeneity in whether people focus on the positive or negatives aspects of risk taking and that this affects decision-making under risk as well as responses to the general risk question. If such a tendency also manifested itself in responses to the “Will you win?” question, i.e., subjects who tend to focus on the positive aspects of risk stated they were more likely to win than to lose a 50-50 lottery, it may explain the correlation between the “Will you win?” and risk preference measures.

References

Andreoni, James, and Charles Sprenger. 2011. “Uncertainty equivalents: Testing the limits of the independence axiom.” *NBER Working Paper, No. w17342*,

Bock, Olaf, Ingmar Baetge, and Andreas Nicklisch. 2014. “hroot: Hamburg registration and organization online tool.” *European Economic Review* 71: pp. 117–120.

Carlin, Paul S. 1992. “Violations of the reduction and independence axioms in Allais-type and common-ratio effect experiments.” *Journal of Economic Behavior & Organization* 19 (2): pp. 213–235.

Carver, Charles S., and Michael F Scheier. 2014. “Dispositional optimism.” *Trends in cognitive sciences* 18 (6): pp. 293–299.

Carver, Charles S., Michael F. Scheier, and Suzanne C. Segerstrom. 2010. “Optimism.” *Clinical Psychology Review* 30 (7): pp. 879–889.

Cokely, Edward T, Mirta Galesic, Eric Schulz, Saima Ghazal, and Rocio Garcia-Retamero. 2012. “Measuring risk literacy: The Berlin numeracy test.” *Judgment and Decision Making* 7 (1): p. 25.

Crosetto, Paolo, and Antonio Filippin. 2013. “The “bomb” risk elicitation task.” *Journal of Risk and Uncertainty* 47 (1): pp. 31–65.

Dohmen, Thomas, Armin Falk, David Huffman, Uwe Sunde, Jürgen Schupp, and Gert G. Wagner. 2011. “Individual Risk Attitudes: Measurement, Determinants, and Behavioral Consequences.” *Journal of the European Economic Association* 9 (3): pp. 522–550.

Dohmen, Thomas, Simone Quercia, and Jana Willrodt. 2018a. “Knowing who you are: Salience of own preferences and the consensus effect.” (mimeo).

Dohmen, Thomas, Simone Quercia, and Jana Willrodt. 2018b. “Willingness to take risk: The role of risk conception and optimism.” CRC TR 224 Discussion Paper, No. 23.

Felton, James, Bryan Gibson, and David M. Sanbonmatsu. 2003. “Preference for Risk in Investing as a Function of Trait Optimism and Gender.” *Journal of Behavioral Finance* 4 (1): pp. 33–40.

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

Frey, Renato, Andreas Pedroni, Rui Mata, Jörg Rieskamp, and Ralph Hertwig. 2017. “Risk preference shares the psychometric structure of major psychological traits.” *Science advances* 3 (10):

Harbaugh, William T., Kate Krause, and Lise Vesterlund. 2010. “The Fourfold Pattern of Risk Attitudes in Choice and Pricing Tasks.” *Economic Journal* 120 (545): pp. 595–611.

Herzberg, Philipp Yorck, Heide Glaesmer, and Jürgen Hoyer. 2006. “Separating optimism and pessimism: a robust psychometric analysis of the revised Life Orientation Test (LOT-R).” *Psychological Assessment* 18 (4): p. 433.

Holt, Charles A., and Susan K. Laury. 2002. “Risk Aversion and Incentive Effects.” *American Economic Review* 92 (5): pp. 1644–1655.

Kahneman, Daniel, and Amos Tversky. 1979. “Prospect Theory: An Analysis of Decision under Risk.” *Econometrica* 47 (2): pp. 263–292.

Kemper, Christoph J, Maria Wassermann, Annekatrin Hoppe, Constanze Beierlein, and Beatrice Rammstedt. 2015. "Measuring Dispositional Optimism in Large-Scale Studies." *European Journal of Psychological Assessment* 33: pp. 403–408.

Scheier, Michael F, Charles S Carver, and Michael W Bridges. 1994. "Distinguishing optimism from neuroticism (and trait anxiety, self-mastery, and self-esteem): a reevaluation of the Life Orientation Test." *Journal of Personality and Social Psychology* 67 (6): p. 1063.

Schupp, Jürgen, and Jean-Yves Gerlitz. 2008. "BFI-S: Big Five Inventory-SOEP." In *Zusammenstellung sozialwissenschaftlicher Items und Skalen. ZIS Version*. vol. 12,

Slovic, Paul. 1987. "Perception of risk." *Science* 236 (4799): pp. 280–285.

Stiglitz, Joseph E, and M Rothschild. 1970. "Increasing risk: I. A definition." *Journal of Economic Theory* 2 (3): pp. 225–243.

Tversky, Amos, and Daniel Kahneman. 1981. "The framing of decisions and the psychology of choice." *Science* 211 (4481): pp. 453–458.

Wagner, Gert G, Joachim R Frick, and Jürgen Schupp. 2007. "The German Socio-Economic Panel Study (SOEP) - evolution, scope and enhancements." *SOEPpapers on Multidisciplinary Panel Data Research*, No. 1.

4.A Appendix to Chapter 4

4.A.1 Full study

Table 4.A.1. Overview of all task participants completed.

Week 1	Week 2	Week 3
Mood Question **General Risk Question** Big Five	Mood Question **General Risk Question** Big Five Trust Question	Mood Question **General Risk Question** Big Five Locus of Control
Binary Trust Game: Treatments		Binary Trust Game: Treatments
Risk Premia (Choice Lists) **Risk Scenarios**	**“Will you win?” task** **Urns Task** Bet: Heads or Tails? Ambiguity preferences and Beliefs	**Risk Premia (Choice Lists)** **Common Ratio Effect** **BRET**
Sociodemographics	Optimism: LOT and SOP	Optimism: LOT and SOP IQ
Mood Question	Mood Question	Mood Question

Notes. For detailed information on the tasks not described in this paper refer to Dohmen et al. (2018a) and Dohmen et al. (2018b)

4.A.2 Details on risk preference elicitation using choice lists

In this task subjects are presented with four lists containing several choices between a fixed lottery and either a certain amount (tables 1 to 3) or another lottery (tables 4). The left-hand side of each table stays constant across rows. The right-hand side of each table contains a dominated option in the first row so that subjects should always choose the left-hand side if they have preferences that are increasing in money. The payoffs on the right-hand side then increase by 0.50€ per row, making it gradually more attractive so that subjects will switch at some point. Since there are four tables of around 20 rows each, clicking each row would be quite tedious. Thus, once a subject switches, the remaining rows are automatically filled out. Subjects are aware, however, that they can revise their choices as long as they have not moved on to the next screen.

For each choice list and subject we observe a switching row. To generate a measure of risk preferences that is comparable across tables, we calculate the risk premium RP from this switching row ($RP = EV - (CE_{switch} - 0.25€)$, where EV is the expected value of the lottery on the left hand side of the table, and CE_{switch} is the lowest safe option the subject prefers to that lottery). The expected value on the right hand side increases by 0.50€ in each row. Due to the discreteness of the measure ”switching row”, we cannot technically observe indifference. Thus, we assume that the point of indifference between the lottery and the safe option is right between the last row subjects choose the lottery and the first row they choose the safe option. This means we have to subtract 0.25€ from CE_{switch}. The procedure for the fourth choice list where both options are lotteries is analogous, just that CE_{switch} is substituted by the expected value of the lottery on the right hand side of the table.

Tables 1 to 3 elicit the certainty equivalents of lotteries paying 15€ with 25%, 50%, and 75% probability, respectively and nothing otherwise. Table 4 elicits the difference in risk premia demanded for a lottery paying 3€ or 22€ with 50% probability each and a lottery paying 3€ or 7€ with 50% probability each.

4.A.3 Optimism measures

4.A.3.1 LOT-R

For validation of the German version we used refer to Herzberg et al. (2006).

English version by Scheier et al. (1994): Please state to what extent your opinion agrees with the following statements (7 point Likert Scale from “does not apply to me at all” to “applies to me exactly”).

1. In uncertain times, I usually expect the best.
2. It’s easy for me to relax.
3. If something can go wrong for me, it will. (R)
4. I’m always optimistic about my future.
5. I enjoy my friends a lot.
6. It’s important for me to keep busy.
7. I hardly ever expect things to go my way. (R)
8. I don’t get upset too easily.
9. I rarely count on good things happening to me. (R)
10. Overall, I expect more good things to happen to me than bad.

Items marked with *(R)* are reverse-scaled, while items 2, 5, 6 and 8 are fillers.

4.A.3.2 SOP

The SOP questionnaire (Kemper et al., 2015) consists of two items eliciting self-reported degrees of optimism and pessimism on a 7-point Likert scale.

1. Optimists are people who look to the future with confidence and who mostly expect good things to happen. How would you describe yourself? How optimistic are you in general?

2. Pessimists are people who are full of doubt when they look to the future and who mostly expect bad things to happen. How would you describe yourself? How pessimistic are you in general?

4.A.4 Additional descriptive statistics

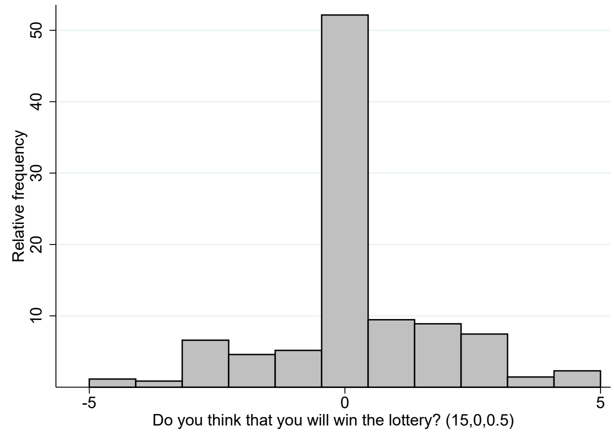


Figure 4.A.1. Answers to the "Will you win?" question.

4.A.4.1 Risk preference measures

Table 4.A.2. General Risk Question.

	Aggregate	Week 1	Week 2	Week 3
Mean	5.195	5.185	5.135	5.201
SD	(2.055)	(2.222)	(2.186)	(2.208)
25th Percentile	3.333	3	3	3
Median	5.333	5	5	5
75th Percentile	6.667	7	7	7
N	370	368	349	348

Notes. The general risk question asks subjects "Are you generally a person who is willing to take risks or do you try to avoid taking risks?" on a Likert scale ranging from 0 (not at all willing to take risk) to 10 (very willing to take risk).

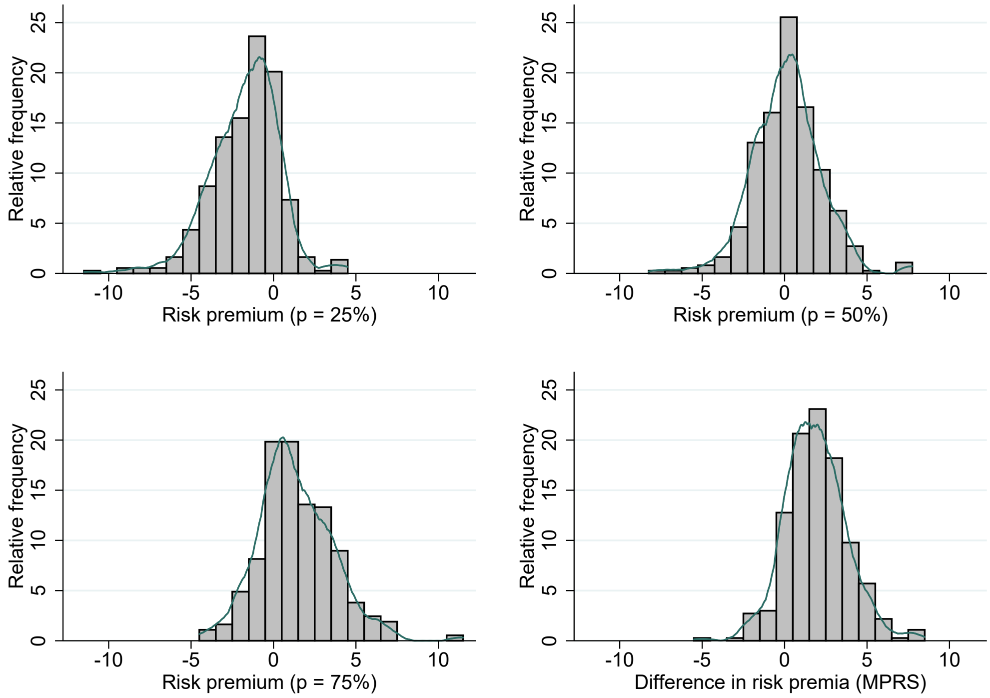


Figure 4.A.2. Distributions of risk premia including a kernel density estimation.

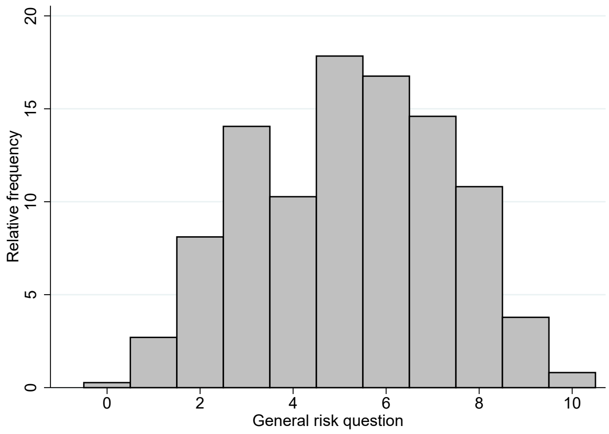


Figure 4.A.3. Answers to the general risk question (aggregated over measurement in three subsequent weeks).

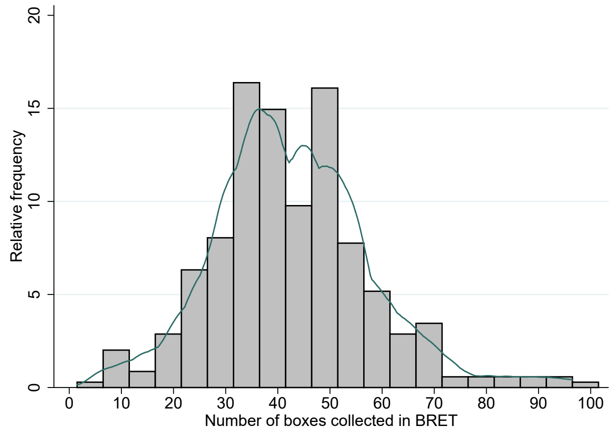


Figure 4.A.4. Answers to the Bomb Risk Elicitation Task (BRET).

4.A.5 Belief elicitation procedure (urns task and risk scenarios)

In both tasks, we employ the same two-step procedure to elicit beliefs about the respective probability between 0 and 100%, i.e, winning the lottery, the event in a specific risk scenario, respectively (or the number of red balls between 0 and 100 in the first stag of the urns task). The procedure is designed to encourage subjects to exert effort while still eliciting a fine enough measure. In the first step subjects are asked to choose between 10 intervals (0-9; 10-19; etc.). If the true value lies in that interval, they receive 1€. In the second step the interval is "zoomed into". For example, if a subject chose "10-19", she is asked to choose between the values in that interval (10, 11, etc.). If she chooses correctly, she receives another 1€. Such a procedure ensures incentive-compatibility as long as we assume beliefs to be symmetrically distributed around their modal value.

4.A.6 Details on urns task

The complete list of the number of red balls, i.e., the probability of winning the lottery in stage 2, is presented in Table 4.A.3. There were five urns that contained very few balls of one color (5 or less) such that stating the right number of red balls is trivial for any subject paying attention to the task. These were included as an indicator of whether subjects took the task seriously. The share of answers within an interval of 1 below or above the correct answer is between 92.8% and 98.6% for these urns in stage 1 and between 96.0% and 99.4% in stage 2. We will not use those trials in further analyses since i) there is very little variation in answers and ii) with the correct answers being close to the boundaries possible answers are effectively censored.

Table 4.A.3. Urns task - Correct solutions for each urn.

Number of red balls					
Urn 1	81	Urn 6	25	Urn 11	50
Urn 2	78	Urn 7	81	Urn 12	64
Urn 3	97	Urn 8	22	Urn 13	81
Urn 4	25	Urn 9	19	Urn 14	36
Urn 5	3	Urn 10	5	Urn 15	1
Urn 16	53	Urn 21	25	Urn 26	50
Urn 17	64	Urn 22	61	Urn 27	84
Urn 18	64	Urn 23	50	Urn 28	3
Urn 19	30	Urn 24	75	Urn 29	28
Urn 20	47	Urn 25	67		

It seems that some subjects inverted the colors when answering, e.g. stated 95 when they saw 5 red balls, thus generating large outliers. To reduce some of the noise generated this way, we exclude answers for which the stated number differs by more than 50 from the correct number in the following analyses. This amounts to 3.3% and 0.08% of answers for stage 1 and stage 2, respectively.

We classify all remaining observations by the number of red balls into "low probability" (19 - 30 red balls), "medium probability" (36 - 67 red balls), and "high probability" (75 - 84 red balls) and report descriptive statistics by category in Table 4.A.4.

When there is a medium or high share of red balls, subjects on average overstate the number of balls both in stage 2 when they represent probabilities and in stage 1 when they do not (see Table 4.A.4). However, they tend to do less so in stage 2. Average answers between stage 2 and stage 1 differ significantly for the low and medium probability category as well as when aggregating over all trials, but not for the high probability category (Wilcoxon signed-rank test: $p_{low} < .0001$, $p_{med} = .0004$, $p_{high} = .674$,and $p_{all} < .0001$, respectively).

Table 4.A.4. Urns task: Avg deviations from the correct answer by category.

		Stage 1 (number of red balls)	Stage 2 (chances of winning)	Difference (Stage 2 - Stage 1)
Low Probability	Mean	0.932	-0.503	-1.517
	SD	5.977	5.331	6.147
	Min	-12.3	-11.0	-25.0
	Max	29.7	23.6	22.3
Medium Probability	Mean	5.803	5.064	-0.735
	SD	4.191	4.437	3.842
	Min	-6.4	-6.5	-12.9
	Max	20.6	20.0	14.3
High Probability	Mean	2.261	2.417	0.118
	SD	3.491	3.592	3.353
	Min	-20.0	-28.2	-15.7
	Max	9.5	9.3	12.7
All (except "Trivial")	Mean	3.432	2.680	-0.728
	SD	2.775	2.518	2.466
	Min	-5.5	-5.3	-8.8
	Max	15.9	9.4	6.6

Notes. N = 349. All stage 1 averages are significantly different from 0 (t-tests: $p_{low} = .004$, $p_{med} < .0001$, $p_{high} < .0001$, and $p_{all} < .0001$, respectively). All stage 2 averages except for category "low probability are significantly different from 0 (t-tests: $p_{low} = .079$, $p_{med} < .0001$, $p_{high} < .0001$, and $p_{all} < .0001$, respectively).

4.A.7 Details on risk scenarios

Find below the wording of the Risk Scenarios (translated from German). The question for the probability in general (or for the average person) is listed as (a), with the correct answer from official statistics or theory rounded to the nearest integer in parentheses. The question for the personal probability is listed as (b).

Question 1 (a) For an average person, what is the probability in percent, that they suffer from an accident within the course of one year? Accidents are defined sudden events that cause an injury or another health impairment (e.g., a concussion due to falling). The data refers to Germany in 2013.(1%)

(b) How do you rate the probability to suffer from an accident yourself (for the next year)?

Question 2 (a) What is the probability in percent that an average marriage is divorced within 25 years? The data refer to divorce in Germany in 2013. (36%)

(b) How do you rate the probability of divorce within 25 years of marriage for yourself?

Question 3 (a) What is the probability in percent that an average person that is registered as unemployed and has tertiary education starts a new job withing one month? The data refer to Germany in 2011. (10%)

(b) How do you rate this probability for yourself, should you become unemployed?

Question 4 (a) What is the probability that a long-distance train in Germany reaches a train station on time? The value refers to passenger transportation of Deutsche Bahn in the first half of 2015 and a delay of at most 5 mins and 59 s. (79%)

(b) How do you rate the probability that a long-distance train in Germany is on time if you take the train yourself?

Question 5 (a) What is the probability in percent that an average suffers from appendicitis within their life-time? (8%)

(b) How do you rate the probability that you suffer from appendicitis yourself within your life-time?

Question 6 (a) What is the probability in percent that there is no rain in Siebengebirge near Bonn on an arbitrary day? The value refers to days with 0mm precipitation between March and May 2015.(18%)

(b) How do you rate the probability that there is no rain in Spring (March to May) on an arbitrary day, if you visit this area yourself?

Question 7 (a) What is the probability of winning anything (regardless of the amount) when participating in the lottery (6 out of 49) once?(3%)

(b) How do you rate the probability of winning anything (regardless of the amount) when participating in the lottery (6 out of 49) if you played the lottery once?

Question 8 (a) Given someone in Bonn becomes the victim of a damage to property and reports this crime to the police, what is the probability that the case is solved? The data refer to 2015. (18%)

(b) How do you rate the probability that the case is solved, if you yourself reported a damage of property to the police?

Table 4.A.5. Descriptive statistics for risk scenarios.

		Mean	Std Dev	Percentiles 10th	25th	50th	75th	90th
Q1	True	1						
	General probability	41.6	24.1	12	22	37	63	76
	Personal probability	52.0	17.1	30	40	51.5	65.5	76
	Difference	10.4	27.6	-29	-8	13	29.5	43
Q2	True	36						
	General probability	50.8	22.9	18	32	53.5	69	78
	Personal probability	59.0	21.2	28	43	62.5	75	87
	Difference	8.1	32.0	-36	-13	9	32	48
Q3	True	10						
	General probability	35.3	20.0	10	22	32.5	49	65
	Personal probability	47.3	22.2	20	31.5	45	64	77
	Difference	12.0	30.2	-29	-9	10	36	50
Q4	True	79						
	General probability	12.0	16.4	0	1	7	14	34
	Personal probability	32.3	21.7	10	15	27	44	66
	Difference	20.3	25.7	-6	7	18	33	55
Q5	True	8						
	General probability	32.7	23.6	8	13	26	48.5	68
	Personal probability	28.1	23.1	2	10	22	45	60
	Difference	-4.6	31.0	-44	-23	-3	14	34
Q6	True	18						
	General probability	54.9	27.3	15	32	56	78	89
	Personal probability	53.6	24.0	20	35	55	74	86
	Difference	-1.3	35.4	-50	-23.5	-3.5	24.5	45
Q7	True	3						
	General probability	35.3	27.6	7	15	27	50	82
	Personal probability	50.9	25.1	20	31.5	50	69	85
	Difference	15.6	38.2	-35	-9	18	45	62
Q8	True	18						
	General probability	11.0	15.6	0	1	5	13	30
	Personal probability	30.1	21.9	5	12	25	43	65
	Difference	19.2	25.7	-9	5	18	33	53

Notes. $N = 368$. "Difference = Personal probability - general probability" for each subject.

Table 4.A.6. Inter-item correlations for “Risk scenarios - difference”.

	Q1	Q2	Q3	Q4	Q5	Q6	Q7
Q2	0.211						
	(0.000)						
Q3	0.208	0.086					
	(0.000)	(0.102)					
Q4	-0.016	-0.108	-0.030				
	(0.761)	(0.039)	(0.565)				
Q5	0.520	0.170	0.188	0.057			
	(0.000)	(0.001)	(0.000)	(0.278)			
Q6	0.172	0.642	0.047	-0.001	0.184		
	(0.001)	(0.000)	(0.376)	(0.990)	(0.000)		
Q7	0.194	0.055	0.706	-0.001	0.201	0.086	
	(0.000)	(0.294)	(0.000)	(0.990)	(0.000)	(0.103)	
Q8	-0.019	-0.164	-0.072	0.856	0.007	-0.041	-0.009
	(0.721)	(0.002)	(0.173)	(0.000)	(0.893)	(0.440)	(0.865)

Notes. Spearman (rank-order) correlation coefficients. p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. $N = 368$.“Difference = Personal probability - general probability” for each subject.

4.A.8 Robustness of the analyses to using another optimism measure

Table 4.A.7. Robustness Check to Table 4.2: Explanatories of the ”Will you win?” measure.

	"Will you win?"					
	(1)	(2)	(3)	(4)	(5)	(6)
Optimism (LOT)	0.600***	0.628***			0.615***	0.544***
	(0.088)	(0.089)			(0.094)	(0.108)
Mood (beginning week 2)			0.116**	0.115**	0.019	0.020
			(0.045)	(0.045)	(0.045)	(0.045)
Female		-0.234		-0.238	-0.236	-0.096
		(0.197)		(0.210)	(0.198)	(0.217)
Budget (in 100€)		0.003		0.041	0.003	0.004
		(0.043)		(0.045)	(0.043)	(0.043)
Political Orientation		0.020		-0.007	0.020	0.033
		(0.071)		(0.075)	(0.071)	(0.073)
Cognitive ability (Raven)		-0.027		0.002	-0.028	-0.021
		(0.046)		(0.049)	(0.047)	(0.047)
Numeracy		0.038		0.024	0.037	0.034
		(0.093)		(0.099)	(0.093)	(0.094)
Conscientiousness						-0.089
						(0.096)
Extraversion						0.031
						(0.080)
Agreeableness						-0.0001
						(0.107)
Neuroticism						-0.132
						(0.087)
Openness						0.115
						(0.077)
Constant	-2.612***	-2.669***	-0.533*	-0.599	-2.725***	-2.258**
	(0.425)	(0.587)	(0.311)	(0.538)	(0.603)	(1.075)
N	349	335	349	335	335	335
R^2	0.119	0.140	0.019	0.029	0.140	0.156

Notes. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Answers to "Will you win?" measure (dependent variable) are on an 11-point Likert scale ranging from "absolutely sure to lose" (-5) to "absolutely sure to win" (+5). The optimism measure consists of the Life Orientation test (LOT-R) on a 7-point Likert scale. It is aggregated over measurements of weeks 2 and 3. Current mood is elicited in the beginning of the experiment in week 2 on an 11-point Likert scale. The Big Five measures were elicited in all three weeks. They are aggregated over measurements of all three weeks. The control variables Female, Budget, and Political Orientation were elicited in week 1. Throughout the paper we do not include age as a control variable since its variation is limited in a student sample and estimates would hence be strongly influenced by outliers.

Table 4.A.8. Robustness Check to Table 4.5: Linear Regressions of the Risk Preference Measures.

Panel 1: Simple Regression	General risk question (1)	Risk Premium 25% of 15€ (2)	Risk Premium 50% of 15€ (3)	Risk Premium 75% of 15€ (4)	Difference RP Both Lotteries (5)	BRET (6)
”Will you win?”	0.349***	-0.150**	-0.194***	-0.261***	-0.041	1.083**
	(0.059)	(0.061)	(0.063)	(0.068)	(0.055)	(0.471)
Constant	5.091***	-1.638***	0.124	1.385***	1.878***	-7.354***
	(0.106)	(0.108)	(0.113)	(0.120)	(0.099)	(0.827)
N	349	347	347	347	347	335
R^2	0.091	0.017	0.027	0.041	0.002	0.016

Panel 2: Including controls	General risk question (1)	Risk Premium 25% of 15€ (2)	Risk Premium 50% of 15€ (3)	Risk Premium 75% of 15€ (4)	Difference RP Both Lotteries (5)	BRET (6)
“Will you win?”	0.249***	-0.122*	-0.139**	-0.215***	-0.031	0.906*
	(0.064)	(0.064)	(0.066)	(0.068)	(0.058)	(0.505)
Female	-0.570**	0.303	0.786***	1.315***	0.734***	-4.624**
	(0.228)	(0.229)	(0.236)	(0.245)	(0.208)	(1.808)
Budget (in 100 €)	0.079	0.030	-0.004	-0.043	0.022	0.423
	(0.049)	(0.050)	(0.051)	(0.053)	(0.045)	(0.391)
Political orientation	0.111	-0.007	-0.148*	-0.128	0.000	0.265
	(0.082)	(0.082)	(0.085)	(0.088)	(0.075)	(0.648)
Cognitive ability (Raven)	-0.115**	0.136**	-0.049	-0.066	-0.056	0.141
	(0.054)	(0.054)	(0.055)	(0.057)	(0.049)	(0.424)
Numeracy	-0.041	0.136	0.095	-0.029	-0.069	-1.677*
	(0.108)	(0.108)	(0.111)	(0.115)	(0.098)	(0.853)
Optimism (LOT)	0.379***	-0.123	-0.136	-0.083	0.065	0.377
	(0.110)	(0.111)	(0.114)	(0.118)	(0.101)	(0.873)
Constant	3.701***	-2.283***	0.761	1.887**	1.499**	-4.828
	(0.699)	(0.701)	(0.722)	(0.749)	(0.637)	(5.534)
N	335	335	335	335	335	335
R^2	0.160	0.050	0.078	0.160	0.059	0.047

Notes. OLS regressions. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Risk preference measures are aggregated over multiple measurements when applicable, i.e., weeks 1 and 3 for choice lists, weeks 1, 2 and 3 for the general risk question. “Will you win?” is elicited on an 11-point scale, while optimism is elicited on a 7-point scale. Cognitive ability ranges from 0 to 10 correct answers, and numeracy from 0 to 4 points.

5

Knowing who you are: Salience of own preferences and the consensus effect

Joint work with Thomas Dohmen and Simone Quercia

5.1 Introduction

The term *consensus effect* refers to an egocentric tendency in assessing and predicting others' actions or preferences. This phenomenon is typically identified empirically as a correlation between personal characteristics and beliefs about the same characteristics in others. This correlation has been documented in several domains in social psychology (see, e.g., Mullen et al., 1985) and in economics (see, e.g., Blanco et al., 2014). Early literature in social psychology interpreted this as a cognitive bias and referred to it as a *false consensus effect*. However, it has been noted that it is rational for an individual to take information about themselves into account when making inferences about a population if the individual is part of that population (Dawes, 1989).

One important question which we tackle in this paper is *under which conditions* people take such information about themselves into account when making inferences about other people. From a rational point of view, individuals should know their preferences and holding everything else constant should always give the same weight to their own "type". However, there is evidence that with respect to information about others people are very sensitive to the way information is presented (Engelmann and Strobel, 2012). If information about others is particularly prominent or salient, they oversample that information and undersample information about themselves. But if a little cognitive effort is required to retrieve the same information about others, the opposite is true, i.e., people undersample that information. This suggests that salience of available information is crucial in determining how beliefs are formed.[1]

In this paper we advance the hypothesis that people are also sensitive to the salience of information about *their own type*. This is particularly important when agents need to form a belief in environments where little or no information about others is available. Salience of their own type is not usually reflected in economics models of decision-making where instead agents know their own preferences with certainty. A more realistic assumption may be that people are not always completely aware of their preferences. This awareness may depend on the extent to which the decision environment favors the emergence of salient information about own preferences. Take social media as an example decision environment: Users are continuously invited to share information about themselves, thus making their own type very prominent. If our conjecture is correct, this extreme focus on themselves will cause users to use their own "type" extensively when making inferences about others. This may intensify polarization in social media.[2]

To examine this question we conduct a longitudinal laboratory experiment, in which we exogenously vary the salience of individual preferences in a binary trust game. In this game, a first mover decides whether to trust and expose themselves to a socially risky situation or not to trust and get a safe payoff. If they trust, a second mover can decide to reciprocate trust splitting the efficiency gains from trust or to betray and leave the first mover with less than their safe outcome from not trusting. In our experiment, subjects play both roles in the trust game and state their beliefs about strategies of other participants playing as second movers. As second-mover actions do not involve strategic uncertainty we interpret them as a pure measure of preferences. To influence the degree of salience of own preferences we use two main manipulations: first we vary the order in which beliefs and preferences are elicited and second we vary the time in which they are elicited measuring them either in the same session or two weeks apart.

---

[1] Note that the notion of beliefs that we employ here differs from that in a game-theoretical sense where in equilibrium beliefs are always equal to objective probabilities and adjust instantaneously if new information becomes available. Such a definition leaves no scope for a consensus effect. In contrast, we consider a belief to be the subjective probability assessment a decision maker holds and which experimenters can measure independently from measuring behavior. Such an assessment exists even in the absence of information and can be sticky, i.e., does not adjust fully and instantaneously once additional information is revealed. A similar notion of beliefs is postulated in Costa-Gomes and Weizsäcker (2008) and employed by Blanco et al. (2014).

[2] Another common explanation for polarization in social media is the existence of echo chambers, that is, the fact that people can self-select into groups of like-minded people and neglect that this is an endogenously selected sample (Enke and Zimmermann, 2018).

We find that both order and time of measurement substantially affect the strength of a consensus effect. A large and significant correlation between preferences and beliefs is found when beliefs are elicited directly following preference elicitation (the setup that has been used most frequently in prior studies). If, however, the order is switched or measurement takes place in different sessions, the correlation is much smaller. This is in line with the interpretation that the consensus effect is largely driven by salience of individuals' own preferences. Therefore, institutional arrangements that foster egocentricity, for example repeatedly sharing information or focusing on about oneself, may increase the extent to which people believe others to be like themselves.

Our paper contributes to an extensive literature on the consensus effect (see Mullen et al. (1985) who provide an influential meta-study of 115 studies and Bazinger and Kühberger (2012) a more recent overview).[3] Although a consensus effect has been found across different domains (Mullen et al., 1985), we focus our attention on the domain of strategic interaction. This setting is particularly interesting for several reasons. First, in such a setting forming a correct belief has a specific monetary value beyond the psychological benefits that may come from agreeing with other people. Examples include business interactions like negotiations or auctions. Second, the setup allows us to elicit revealed preferences via incentivizing choices using the strategy method (Selten, 1965). This would not be possible in less stylized settings such as eliciting political or religious preferences. Finally, a large portion of economic studies on the consensus effect have used social dilemmas and this allows us to compare our results with theirs.

The first of these studies to explicitly examine the consensus effect in economics was the one by Offerman et al. (1996) who provide evidence for a consensus effect in public goods games.[4] Engelmann and Strobel (2000) use a wide variety of different choices and tastes. They explicitly distinguish between a consensus effect and a truly false consensus effect where information about the self is *overweighed* compared to information about others. Their study attests the presence of a consensus effect but reject the presence of a *false* consensus effect. Further evidence for the existence of a consensus effect has been provided for the trust game (Altmann et al., 2008), the sequential prisoner's dilemma (Blanco et al., 2011, 2014; Miettinen et al., 2017) and the leader-follower game (Gächter et al., 2012). The most telling evidence, comes from Blanco et al. (2014) who are the first to explicitly elicit beliefs about second-mover actions and show that these are influenced by subjects' own second-mover actions. However, all men-

[3] Another term for this phenomenon that highlights the mechanism rather than its effect is social projection (see, e.g., Bazinger and Kühberger, 2012).

[4] Correlations between preferences and beliefs in social dilemmas have been documented first in papers whose main objective was not to investigate the consensus effect (Jacobsen and Sadrieh, 1996; Selten and Ockenfels, 1998; Charness and Grosskopf, 2001).

tioned economics studies rely on elicitation of preferences and beliefs in the same session, with beliefs being elicited *after* preference elicitation, a setup where subjects' own types are extremely salient. It is unclear whether similar effects would have been found, had subjects' own types been less salient. In this paper we address this question explicitly.

The remainder of the paper is structured as follows. Section 5.2 describes the design and procedural details of our experiment. Section 5.3 derives testable hypotheses from different sets of assumptions on the consensus effect. The results of the experiment are presented and contrasted with the hypotheses in Section 5.4, while Section 5.5 discusses implications and alternative explanations and concludes.

5.2 Design and procedures of the experiment

The data presented were gathered as part of a longitudinal experiment in which participants were invited to take part in three sessions over three consecutive weeks. The whole experiment included measures on the relation between preferences and beliefs in different domains. A complete overview of all tasks and the order in which they were administered is provided in Table 5.A.5 of the appendix. In the main text of the paper we describe the relevant measures for the research question of this paper. In particular, we present four between-subject treatments constituting a 2×2 design. Importantly, these were the only treatment variations in the longitudinal experiment and all elicitations of additional measures were constant across the four treatments.

5.2.1 The binary trust game

As a workhorse to examine our research question, we use the binary trust game shown in Figure 5.1. In this game, a first mover chooses between actions "OUT" and "IN". If they choose "OUT", the payoff for both players is 10 €, regardless of the second mover's action. If they choose "IN", there is an efficiency gain and players' payoffs depend on the second mover's choice who can decide whether to distribute the payoffs equally ("Option B" yielding 15 € for each player), or to keep more for themselves ("Option A" yielding payoffs of 8 € to the first mover and 22 € for the second mover).

The subgame-perfect Nash equilibrium for self-interested players is ("OUT", "Option A"). However, joint payoff is maximized if the first mover chooses "IN".

5.2.1.1 Measures and interpretation

In our experiment, subjects play the trust game in both roles. For each participant, the main measures we elicit are: first-mover actions, second-mover actions,

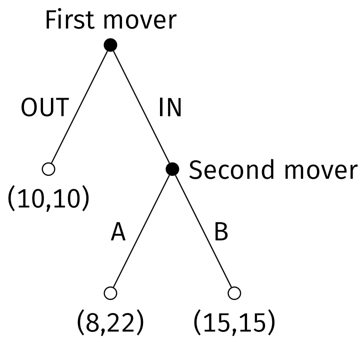


Figure 5.1. Game tree of the binary trust game.

and beliefs about other second-movers' behavior. At the end of the experiment, one of these decisions is randomly selected for payment to exclude hedging possibilities (Blanco et al., 2010).

First-mover actions. First movers' actions are elicited by asking players to make a decision between "IN" and "OUT". Assuming purely self-interested preferences, these decisions reflect only beliefs about second-movers behavior. In particular, if the first mover ranks outcomes $(8,22) \prec (10,10) \prec (15,15)$ and they have purely self-interested preferences, they will choose "IN" if and only if their belief about the probability that the second mover chooses "Option B" exceeds some positive threshold. Such a belief is rational in case it is commonly known that some second movers have social preferences leading them to choose "Option B". However, choosing "IN" may also be related to social preferences, preferences for efficiency or other motives such as risk preferences, betrayal aversion or altruism (see, e.g., Bohnet and Zeckhauser, 2004; Cox, 2004). Due to these potential confounds, in our design we also measure beliefs directly rather than inferring them from first-mover choices.

Second-mover actions. Second-mover actions were elicited using the strategy method (Selten, 1965). Participants were asked whether they would choose "Option A" or "Option B" in case the first mover chose "IN". The sequentiality of players' moves ensures the absence of a strategic component in the second-mover choice. Thus, it can be interpreted as a pure preference measure. Moreover, this measure of (social) preferences is not confounded with efficiency concerns since "Option A" and "Option B" lead to the same sum of payoffs.[5] Choosing 'Option

[5] In a sequential prisoner's dilemma preferences for efficiency may cause a correlation between first- and second-mover actions. In this game as it is usually parameterized $(2 * \pi_i(C,C) > \pi_i(C,D) + \pi_j(C,D) > 2 * \pi_i(D,D))$, players who care only about efficiency, i.e., seek to maximize total payoff, should always choose C as first and second movers, thus leading to a perfect correlation of actions even in the absence of a consensus effect. This relationship is less pronounced, but present for players who care about their own payoff as well as efficiency.

B" can be consistent with several models of social preferences such as inequity aversion (Fehr and Schmidt, 1999), reciprocity (Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006), and guilt aversion (Battigalli and Dufwenberg, 2007). As the main objective of the paper is not related to the main motivation behind these choices, it is sufficient to assume that individual have a psychological cost high enough to make them choose "Option B" instead of "Option A".

Beliefs. Our third measure is the belief that a participant has about other second-movers' actions. We ask subjects to state how many out of 20 students playing as "Player 2" (i.e., the second mover) in another session they think will choose "Option B". They answer by choosing between 7 equally-sized intervals from "0 − 2" to "18 − 20". Correct guesses are rewarded with 8 €, while there is no payoff for incorrect guesses. This incentive scheme ensures incentive-compatible elicitation of subjects' beliefs in a way that is not confounded with risk preferences.[6] Giving subjects a choice between 7 intervals rather than all 21 possibilities makes the measurement coarser but at the same time increases subjects' chances of actually guessing correctly, thus increasing the perceived importance of their decision. Throughout the paper we will report beliefs as relative frequencies converted from subjects' answers by taking the mid-point of the chosen interval and dividing by 20.

Since first movers' choices may not (only) reflect beliefs about second movers' behaviour (see above), we focus our attention on second-mover actions and beliefs about others' second-movers actions to identify the consensus effect.

5.2.2 Treatments

We employ four main between-subjects treatments, in which we manipulate salience of own preferences. To do this, across treatments we vary the order and time of elicitation of the above mentioned measures. An overview of the main treatments is displayed in Table 5.1.

5.2.2.1 High salience

To ensure comparability with prior research on the consensus effect we conduct a baseline treatment where in week 1 we elicit our measures in the following order: second-mover actions first, then beliefs about other second movers' actions and finally first-mover actions. We label this order of elicitation Order 1. The same elicitation is repeated in week 3 to learn about stability of elicited preferences and beliefs. As subjects did not know in week 1 the tasks of week 3, the

[6] Similar to the literature, we treat beliefs as if they were point beliefs. If, instead, one assumes subjects to have a distribution of beliefs, the method described here elicits the interval, in which its mode lies.

Table 5.1. Main treatments.

	Same session	Different sessions
Order 1	**High salience** 2^{nd} mover (W1) Belief (W1) 1^{st} mover (W1)	**Low salience - time** 2^{nd} mover (W1) Belief (W3) 1^{st} mover (W3)
N	34	54
Order 2	**Low salience - order** Belief (W1) 1^{st} mover (W1) 2^{nd} mover (W1)	**Low salience - time & order** Belief (W1) 1^{st} mover (W1) 2^{nd} mover (W3)
N	44	34

Notes. The terms (W1) and (W3) following a measure indicate that this measure was elicited in week 1 and week 3, respectively.

elicitation in week 1 serves as our main measure. To our knowledge, all prior economics experiments on the consensus effect have relied on this particular order, i.e., have second-mover actions directly precede belief elicitation (Jacobsen and Sadrieh, 1996; Selten and Ockenfels, 1998; Charness and Grosskopf, 2001; Blanco et al., 2014).

5.2.2.2 Low salience - order

The first method we use to manipulate the salience of subjects' own preferences during belief elicitation is to vary the order, in which preferences and beliefs are elicited. If having to express one's preference by making a decision increases its salience when forming beliefs, eliciting preferences after belief elicitation should reduce their salience. Hence, in our *low salience - order treatment* we modify the order of elicitation from *second mover – belief – first mover* to *belief – first mover – second mover*.

5.2.2.3 Low salience - time

The second way we exogenously vary salience exploits the longitudinal nature of our experiment. In treatment *low salience – time*, the measurement of preferences and beliefs takes place two weeks apart. Subjects play the trust game in the role of second movers in week 1, but beliefs (and first-mover actions) are not elicited until week 3. The assumption behind this manipulation is that subjects' own preferences are less salient if beliefs are elicited two weeks later.

5.2.2.4 Low salience - time & order

A fourth treatment *low salience – time & order (time & order)* combines our two manipulations of salience. That is, beliefs (and first-mover actions) are elicited in week 1, while second-mover actions are elicited in week 3. We speculate that salience should be at least as low as in treatments *order* and *time*, but possibly lower.[7]

5.2.3 Procedures

A total of 346 subjects (95% students, 62% female, mean age 22.4 years) participated in all sessions, 174 of which took part in the main four treatments and 170 in four robustness treatments.[8] Each session lasted about one hour and contained several distinct parts. The experiment took place in the summer and fall of 2016. It was computerized using z-Tree (Fischbacher, 2007) and participants were recruited from the BonnEconLab subject pool using the software h-root (Bock et al., 2014). We only invited subjects who had played not the trust game in the BonnEconLab before.

One part per week was randomly selected for payoff, with each part being equally likely to be selected. This was clearly communicated to subjects. This payment scheme precludes hedging, while keeping the stakes within one part sizable enough for subjects to exert effort. On average, subjects received a total payoff of 31.99 € (about 35 US-$ at the time). This includes a bonus of 10 € that we used to incentivize participation in all three weeks, hence achieving a low attrition rate of around 5% and leaving us with a sample of 327 subjects in total (166 in the main treatments, 159 in the robustness treatments) who completed at least weeks 1 and 3 of the experiment. To ensure comparability of all statistics as well as for simplicity of exposition we will restrict our analysis to these subjects.

5.3 Framework and predictions

In this section we will state testable predictions from different sets of assumptions. Since our main research question relates to a direct test of the consensus effect, we focus on predictions regarding the relationship between second-mover actions and beliefs.

---

[7] As robustness checks, we conducted four treatments that control for the influence of playing the game in both roles and measuring beliefs in the same session (see Section 5.A.2 in the appendix). Results are in line with our findings presented in the main body of the paper.

[8] There were an additional 2 subjects who participated only in week 2 and a session of 22 subjects that had to be dropped from the sample because of a programming error.

Throughout we assume that each agent is of one of two types $t \in \{0,1\}$, a *selfish* ($t = 0$) or a *trustworthy* ($t = 1$) type. As second movers, selfish types always choose "Option A", while trustworthy types always choose "Option B". First-mover actions depend on beliefs b that the second mover is a trustworthy type; they choose "IN" as soon as this belief exceeds a specific positive threshold. This is, of course, an oversimplification of the vast literature on social preferences and not meant to generalize beyond the given setup. However, it serves our purpose of leading to a positive chance of second movers choosing the equal payoff distribution and hence justifies beliefs higher than zero. Moreover, it does generate a direct relationship between between first- and second-mover actions and does not predict a correlation between them in the absence of a consensus effect.[9]

The consensus effect enters the framework through an influence of types on beliefs. We assume that subject i's own type t_i, and the weight σ thereof, influences beliefs b_i in the following way: $b_i(t_i, \sigma) = b_i^0 + \sigma(t_i - b_i^0) = (1-\sigma)b_i^0 + \sigma(t_i)$, with $\sigma \in [0,1]$, and b_i^0 the realization of a random variable. We assume that initial belief b^0 follows a normal distribution truncated at 0 and 1 with mean equal to the share of trustworthy subjects in the population, i.e., the correct belief.[10] Such a framework encompasses the special cases of both the absence of a consensus effect ($\sigma = 0$) where subjects disregard their own type during belief formation as well as full egocentricity ($\sigma = 1$) where they disregard the initial belief and believe all others to be exactly like themselves.

We interpret the weight σ that is placed on an agent's type as the salience level, a feature of the decision environment and therefore common to all agents in the same situation.[11] We will now state different sets of predictions depending on the nature of σ.

Consensus effect independent of salience. Contrary to the standard model we could assume that a consensus effect exists but is the consequence of the relationship of agents' innate personality traits. That is, preferences and beliefs follow a joint distribution, for example, because of intergenerational transmission, differences in experiences that people of different types make, or heterogeneity in

[9] For a thorough discussion of which social preference models predict positive and which predict negative correlations between first- and second-mover actions in the absence of a consensus effect refer to Blanco et al. (2014) (pp. 126-128). If one wanted to model a direct interaction between first- and second-mover behavior (independent of a consensus effect) in our framework, the belief threshold for first movers to choose "IN" could be type-dependent, with prosocial types requiring a lower belief than selfish types.

[10] Of course, other assumptions such as biased initial beliefs or other distributional forms are possible.

[11] In addition this weight could capture heterogeneity in agents' egocentricity, i.e., the specific extent to which they think everyone is the same as themselves. Since we can neither measure such heterogeneity nor do we know whether it exists, we focus on treatment differences in salience.

the updating process between types. In this case, the weight σ determines the influence of an agent's own belief would not depend on how salient the type is during decision-making. Thus, in our experiment we would observe a correlation between second-mover actions but no treatment differences.

Prediction set 1. *a) Second-mover behavior and beliefs are positively correlated in high salience.*

b) The timing of measurement is irrelevant, i.e., the same positive correlation between second-mover behavior and beliefs as in high salience is observed in low salience – time.

c) The order of measurement is irrelevant, i.e., the same positive correlation between second-mover behavior and beliefs as in high salience is observed in low salience – time and low salience – time & order.

Consensus effect depends on salience. Our second set of predictions is derived from the assumption that the level of the consensus effect depends on the salience level of an agent's type. Possible explanations for agents weighting their own type more strongly when it is more salient includes the availability heuristic (Tversky and Kahneman, 1973) or anchoring on one's own type (Tversky and Kahneman, 1974). In case subjects have already played the game as second-movers, the wish to make consistent decisions to signal skills as professed in Falk and Zimmermann (2016) could also play a role. In our framework, σ would differ between treatments, taking its highest value in *high salience*, where subjects have just played the game as second movers when asked to state their belief. We would thus expect the following pattern in our data.

Prediction set 2. *a) Second-mover behavior and beliefs are positively correlated in high salience.*

b) The timing of measurement matters. If belief elicitation takes place two weeks after subjects have played as second movers (low salience – time), the positive correlation between second-mover behavior and beliefs is smaller than in high salience.

c) The order of measurement matters. If beliefs are measured before subjects play as second movers (in low salience – time and low salience – time & order), the positive correlation between second-mover behavior and beliefs is smaller than in high salience.

5.4 Results

In this section we will first report descriptive statistics on subjects' behavior in the experiment. We will then consider relationships between our preference measure (second-mover actions) and beliefs to test the differential predictions made in Section 5.3.

Table 5.2. Averages of actions and beliefs in trust game by treatment.

	High salience	Low salience - time
2^{nd} mover	41.2%	46.3%
Belief	42.1%	43.3%
1^{st} mover	41.2%	59.3%
N	34	54
	Low salience - order	**Low salience - time & order**
2^{nd} mover	52.3%	41.2%
Belief	44.5%	49.6%
1^{st} mover	63.6%	61.8%
N	44	34

Notes. "2nd mover" displays the share of participants who chose the equal distribution (15,15) as second movers in the binary trust game. "Belief" describes the average belief subjects hold about the share of second movers in another session choosing (15,15). "First mover" describes the share of participants who chose "IN" as first movers in the binary trust game.

5.4.1 Descriptive statistics

Considering all four main treatments jointly, 45.7% of our subjects choose "Option B" when playing as second mover and 57.2% choose "IN" when playing as first movers. Neither first- nor second-mover actions differ significantly between treatments (see Table 5.2): χ^2-tests for homogeneity do not reject the null hypothesis of the four treatments being drawn from the same distribution (first-mover action: $p = .196$; second-mover action:$p = .721$).

Subjects' beliefs about the second-mover action of other subjects were quite accurate. Averaging over the four main treatments they predicted that 44.6% would reciprocate as second movers. Also with respect to beliefs, we do not find a significant difference between treatments (see Table 5.2; χ^2-test for homogeneity: $p = .454$).

5.4.2 Consensus effect

Next, we move to our main research question by analyzing the relationship between beliefs and second movers' strategies. Figure 5.2 displays the distribution of beliefs conditional on second mover strategies in each of the four main treatments. The figure shows that the two distributions partially overlap in all treatments but are more clearly distinct in the *high salience* treatment. Comparing

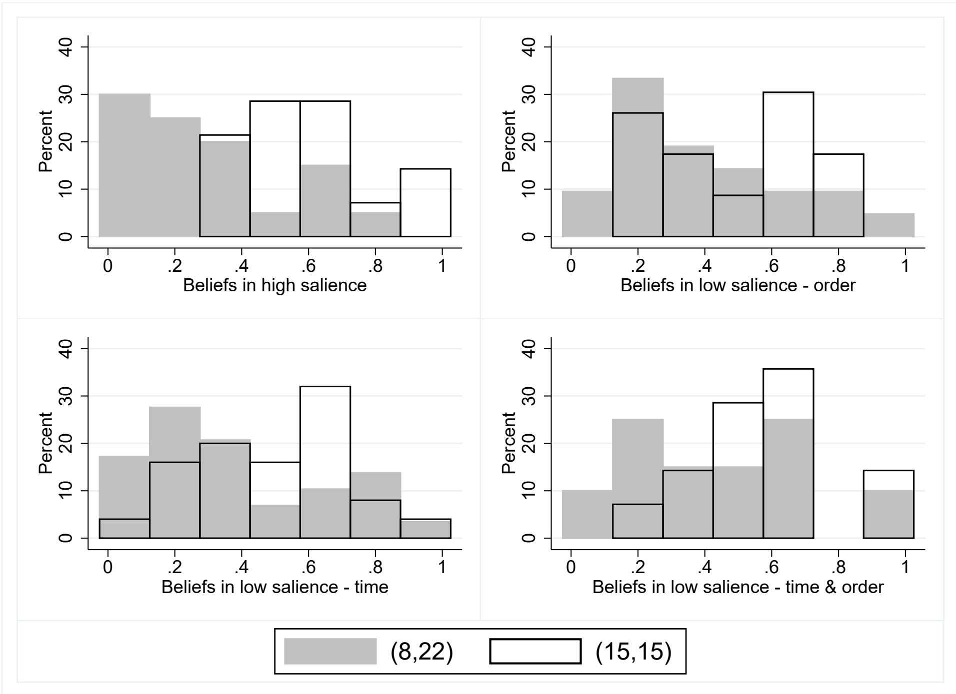


Figure 5.2. Beliefs about the share of second movers in another session choosing (15,15) by second mover type.

the belief distributions using Kolmogorov-Smirnov tests reveals a significant difference by type only for the *high salience* treatment (two-sided test: $p = .008$) but not for the other treatments (*low salience - order* $p = .284$, *low salience - time* $p = .473$, and *low salience - time & order* $p = .425$, respectively).

Following the previous literature (Mullen et al., 1985; Blanco et al., 2014), we attest the presence of a consensus effect whenever there is a significant positive correlation between second-mover actions and beliefs. Because the data are ordinal for actions in the trust game and cardinal for beliefs, we report the Spearman rank-order correlation coefficient. We report these correlations in Table 5.3.

In the *high salience* treatment, second movers choosing the payoff-equalizing option on average believe that 59.6% of other second movers would do the same, while those who choose the higher payoff for themselves express significantly lower beliefs (average belief: 29.8%; Wilcoxon rank-sum test: $p = .001$). This leads to a large and significant correlation between second-mover actions and beliefs ($\rho = .559, p = .001$; see Table 5.3). Note that the *high salience* treatment employs the order of elicitation typically used in previous studies and hence replicates previous findings (see, e.g., Blanco et al. (2014)).

Table 5.3. Consensus effect by treatment.

	High salience	Low salience - time
$\rho_{2^{nd},belief}$	.559*** (.001)	.234* (.089)
N	34	54

	Low salience - order	Low salience - time & order
$\rho_{2^{nd},belief}$	.218 (.155)	.268 (.126)
N	44	34

Notes. ρ: Spearman's correlation coefficient (between second-mover actions and beliefs). p-values in parentheses.

Result 1. *A large and significant consensus effect (correlation between beliefs and second-mover actions) can be documented when both measures are elicited in the same session and belief elicitation follows second-mover actions (treatment high salience).*

When preferences and beliefs are elicited in the same session but belief elicitation comes first (*low salience - order*), the difference in beliefs between trustworthy and untrustworthy subjects is positive (average beliefs: 49.4% and 39.3%, respectively) but smaller than in the high salience treatment. The correlation coefficient between second movers' strategies and beliefs is $\rho = .218$ ($p = .155$) and it is not statistically significant at conventional levels. Moreover, this correlation is significantly smaller than the correlation found for treatment *high* (one-sided z-test: $p = .043$).[12]

Result 2. *When both measures are elicited in the same session, a consensus effect can only be documented when second-mover decisions precede belief elicitation (treatment high salience) but not when the beliefs are elicited first (treatment low salience - order).*

[12] To test whether correlations coefficients are significantly different from one another we use the following procedure. We apply the approximate Fisher's z transformation (Fisher, 1915) to transform the distribution of the relevant correlation coefficients: $z' = \frac{1}{2}(\ln(1+\rho) - \ln(1-r))$. This generates variables distributed with an approximate normal distribution with standard error $\sigma_z = \frac{1}{\sqrt{N-3}}$ on which a z-test can be performed. Although this procedure is aimed at Pearson correlation coefficients, Myers and Sirois (2006) find it to be the most efficient for Spearman correlation coefficients as well. It was implemented using the CORTESTI package (Caci, 2000) in Stata.

Next, we analyze the *low salience - time* treatment where beliefs were elicited two weeks after second-mover actions. As before, the average belief of a trustworthy type (49.4%) is significantly higher than that of an untrustworthy (38.1%, Wilcoxon rank-sum test $p = .089$). However, similar to the *low salience (order)* treatment, the relevant correlation ($\rho = .234, p = .089$) is significantly smaller than in the *high salience* treatment (one-sided z-test $p = .042$).

Result 3. *The consensus effect is significantly smaller when beliefs are elicited two weeks after second-mover actions (treatment low salience - time) than when measurement takes place in the same session (treatment high salience).*

Finally, we look at the *low salience - time and order* treatment in which both the time and order of elicitation should induce lower salience of own preferences. We find a non-significant correlation coefficient $\rho = .268$ ($p = .126$). This correlation is not significantly different from those in treatments *low salience - time* and *low salience - order* (two-sided z-tests $p = .872$ and $p = .823$, respectively).

5.4.3 Robustness check: Preference instability

An obvious concern with the interpretation of the *low salience - time* treatment is that lower correlations between preferences and beliefs measured at different points in time may be caused by fluctuations in our preference measure over time rather than an exogenous reduction of salience of own preferences. Instability of the preference measure may be due either to true underlying preference instability or to measurement error. If our measure for preferences is highly unstable this could constitute a confound in the identification of our salience channel.

To tackle this concern, we employ the following procedure. We consider our *high salience* treatment where we have data on individuals' preferences for week 1 as well as week 3. A share of 85% of second movers make the same decision in both weeks. Using this information we construct a simulated sample "mock *low salience - time*" by changing our elicited preference measure in week 1 for a randomly selected 15% of the subjects. This simulates the same degree of preference instability found in the *high salience* treatment. We then correlate the obtained distribution of preferences with the beliefs elicited in week 3 of our experiment. This process is repeated 10 000 times. The obtained distribution of correlation coefficients can be found in Figure 5.3. By comparing this distribution of correlation coefficients with the one obtained in our *low salience - time* treatment we can assess whether the result we obtain is a likely outcome of preference instability or at least part of it can be imputed to salience. The average correlation between beliefs and simulated preferences is $\rho = .394$ and exceeds the correlation that is found in the real treatment *low salience - time* in over 95% of cases. This means that even though preference instability may play some role in lowering the observed correlation between second-mover actions and beliefs

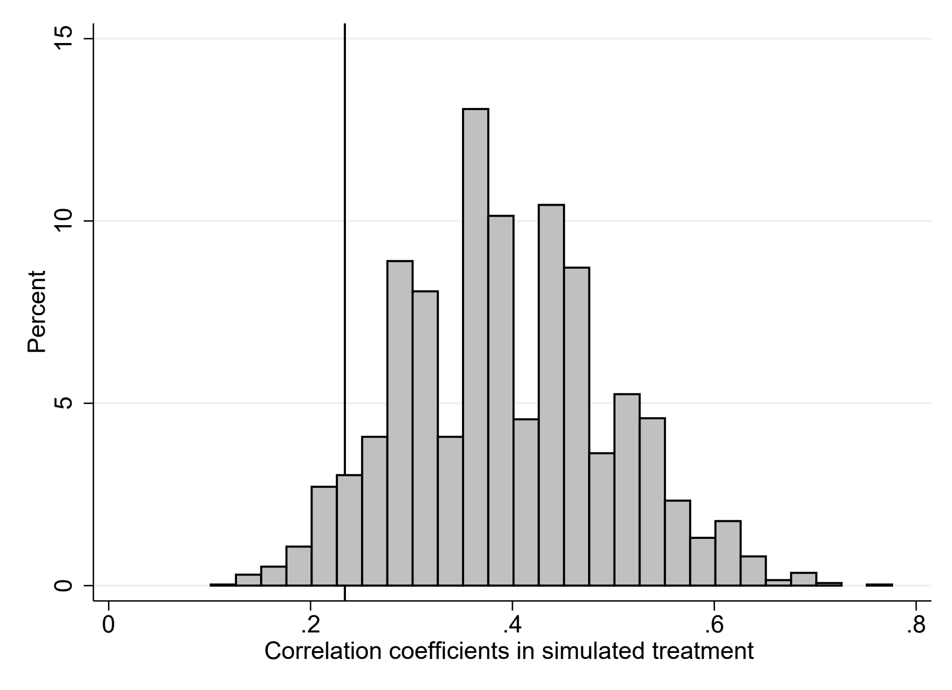


Figure 5.3. Correlation between simulated second-mover actions and beliefs in "mock treatment low salience - time". The vertical line marks the correlation of $rho = .234$ in the real low salience time.

when they are elicited two weeks apart (compare with *high salience* treatment), this cannot fully account for the strong reduction in the correlation that we find in our real treatment which is at least partially caused by a reduction in salience of own preferences.

5.5 Discussion and conclusion

In this paper, we have tested whether the extent to which subjects make inferences from themselves to other people depends on the salience of own preference "type" during belief elicitation. Overall, we can clearly document the existence of a consensus effect in our data. However, the strength of the consensus effect depends heavily on the salience of own preferences. While it is large and significant when beliefs are elicited right after preferences, it is considerably smaller as soon as the order or timing of measurement (or both) is changed. This is in line with our Prediction Set 2 that was derived from the assumption that salience of one's own preferences during belief elicitation is crucial.

Throughout the paper we interpret a correlation between preferences and beliefs as preferences influencing beliefs. However, it is also conceivable that belief elicitation influences preferences. This is not usually considered in the literature. A noteworthy exception is the work by Denolf et al. (2017) who establish a quantum-like model of the measurement of preferences and beliefs in a social dilemmas which they fit to the data elicited by Blanco et al. (2014). Although our design is not ideally suited to understand the impact of belief elicitation on second-mover actions since there is no treatment where second-mover decisions directly follow belief elicitation with no first-mover decision in between, the evidence that our data can provide does not favor such a channel. The fact that the correlation between second-mover actions and belief is not significantly different from zero in treatment *low salience - order* where second-mover actions are elicited following beliefs and first-mover actions as well as our finding in Result 2 suggest that the influence of preference elicitation on belief elicitation is stronger than in the opposite direction. Comparing the treatments *low salience - order* and *low salience - order & time* that both elicit beliefs first but where second-mover actions are taken either in the same session or two weeks later is particularly telling. The fact that correlations do not differ between them could be interpreted as salience of *beliefs* not mattering during *preference elicitation*. So in our experiment there is no evidence of reverse causality for the consensus effect.

In summary, we find that the correlation between preferences and beliefs is by far strongest when belief elicitation directly follows preference elicitation. In all other cases, the relationship is much smaller. Hence, the large consensus effects previously reported in the literature are partially caused by the mode of measurement. We thus conclude that salience of one's own type plays a crucial role in determining the strength of the consensus effect.

References

Altmann, Steffen, Thomas Dohmen, and Matthias Wibral. 2008. "Do the reciprocal trust less?" *Economics Letters* 99 (3): pp. 454–457.

Battigalli, Pierpaolo, and Martin Dufwenberg. 2007. "Guilt in Games." *American Economic Review* 97 (2): pp. 170–176.

Bazinger, Claudia, and Anton Kühberger. 2012. "Is social projection based on simulation or theory? Why new methods are needed for differentiating." *New Ideas in Psychology* 30 (3): pp. 328–335.

Blanco, Mariana, Dirk Engelmann, Alexander K Koch, and Hans-Theo Normann. 2010. "Belief elicitation in experiments: is there a hedging problem?" *Experimental Economics* 13 (4): pp. 412–438.

Blanco, Mariana, Dirk Engelmann, Alexander K Koch, and Hans-Theo Normann. 2014. "Preferences and beliefs in a sequential social dilemma: a within-subjects analysis." *Games and Economic Behavior* 87: pp. 122–135.

Blanco, Mariana, Dirk Engelmann, and Hans Theo Normann. 2011. "A within-subject analysis of other-regarding preferences." *Games and Economic Behavior* 72 (2): pp. 321–338.

Bock, Olaf, Ingmar Baetge, and Andreas Nicklisch. 2014. "hroot: Hamburg registration and organization online tool." *European Economic Review* 71: pp. 117–120.

Bohnet, Iris, and Richard Zeckhauser. 2004. "Trust, risk and betrayal." *Journal of Economic Behavior & Organization* 55 (4): pp. 467–484.

Caci, Herve M. 2000. "CORTESTI: Stata module to test equality of two correlation coefficients."

Charness, Gary, and Brit Grosskopf. 2001. "Relative payoffs and happiness: an experimental study." *Journal of Economic Behavior & Organization* 45 (3): pp. 301–328.

Costa-Gomes, Miguel A, and Georg Weizsäcker. 2008. "Stated beliefs and play in normal-form games." *The Review of Economic Studies* 75 (3): pp. 729–762.

Cox, James C. 2004. "How to identify trust and reciprocity." *Games and economic behavior* 46 (2): pp. 260–281.

Dawes, Robyn M. 1989. "Statistical criteria for establishing a truly false consensus effect." *Journal of Experimental Social Psychology* 25 (1): pp. 1–17.

Denolf, Jacob, Ismael Martínez-Martínez, Haeike Josephy, and Albert Barque-Duran. 2017. "A quantum-like model for complementarity of preferences and beliefs in dilemma games." *Journal of Mathematical Psychology* 78: pp. 96–106. Quantum Probability and Contextuality in Psychology and Economics.

Dohmen, Thomas, Armin Falk, Simone Quercia, and Jana Willrodt. 2018a. "Will you win? Optimism, risk assessment and risk preferences." (mimeo).

Dohmen, Thomas, Simone Quercia, and Jana Willrodt. 2018b. "Willingness to take risk: The role of risk conception and optimism." CRC TR 224 Discussion Paper, No. 23.

Dufwenberg, Martin, and Georg Kirchsteiger. 2004. "A theory of sequential reciprocity." *Games and Economic Behavior* 47 (2): pp. 268–298.

Engelmann, Dirk, and Martin Strobel. 2000. "The False Consensus Effect Disappears if Representative Information and Monetary Incentives Are Given." *Experimental Economics* 3: pp. 241–260.

Engelmann, Dirk, and Martin Strobel. 2012. "Deconstruction and reconstruction of an anomaly." *Games and Economic Behavior* 76 (2): pp. 678–689.

Enke, Benjamin, and Florian Zimmermann. 2018. "Correlation Neglect in Belief Formation." *The Review of Economic Studies*,

Falk, Armin, and Urs Fischbacher. 2006. "A theory of reciprocity." *Games and Economic Behavior* 54 (2): pp. 293–315.

Falk, Armin, and Florian Zimmermann. 2016. "Consistency as a Signal of Skills." *Management Science* 63 (7): pp. 2197–2210.

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

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

Fisher, Ronald A. 1915. "Frequency distribution of the values of the correlation coefficient in samples from an indefinitely large population." *Biometrika* 10 (4): pp. 507–521.

Gächter, Simon, Daniele Nosenzo, Elke Renner, and Martin Sefton. 2012. "Who makes a good leader? Cooperativeness, optimism, and leading-by-example." *Economic Inquiry* 50 (4): pp. 953–967.

Jacobsen, Eva, and Abdolkarim Sadrieh. 1996. "Experimental proof for the motivational importance of reciprocity." University of Bonn, Germany.

Miettinen, Topi, Michael Kosfeld, Ernst Fehr, and Jorgen W Weibull. 2017. "Revealed Preferences in a Sequential Prisoners' Dilemma: A Horse-Race Between Five Utility Functions."

Mullen, Brian, Jennifer L Atkins, Debbie S Champion, Cecelia Edwards, Dana Hardy, John E Story, and Mary Vanderklok. 1985. "The false consensus effect: A meta-analysis of 115 hypothesis tests." *Journal of Experimental Social Psychology* 21 (3): pp. 262–283.

Myers, Leann, and Maria J. Sirois. 2006. "Differences between Spearman Correlation Coefficients." In. *Encyclopedia of Statistical Sciences*. American Cancer Society.

Offerman, Theo, Joep Sonnemans, and Arthur Schram. 1996. "Value orientations, expectations and voluntary contributions in public goods." *The Economic Journal*, pp. 817–845.

Selten, Reinhard. 1965. "Die Strategiemethode zur Erforschung des eingeschränkt rationalen Verhaltens im Rahmen eines Oligopolexperimentes." In. *Beiträge zur experimentellen Wirtschaftsforschung*. Heinz Sauermann, ed. Tübingen: J.C.B. Mohr (Paul Siebeck), pp. 135–168.

Selten, Reinhard, and Axel Ockenfels. 1998. "An experimental solidarity game." *Journal of Economic Behavior & Organization* 34 (4): pp. 517–539.

Tversky, Amos, and Daniel Kahneman. 1973. "Availability: A heuristic for judging frequency and probability." *Cognitive Psychology* 5 (2): pp. 207–232.

Tversky, Amos, and Daniel Kahneman. 1974. "Judgment under uncertainty: Heuristics and biases." *Science* 185 (4157): pp. 1124–1131.

5.A Appendix to Chapter 5

5.A.1 Full descriptive statistics and correlations

Table 5.A.1. Main treatments: Average actions and beliefs in the trust game.

	High salience	Low salience - time
1^{st} mover	41.2%	59.3%
2^{nd} mover	41.2%	46.3%
Belief	42.1%	43.3%
N	34	54

	Low salience - order	Low salience - time & order
1^{st} mover	63.6%	61.8%
2^{nd} mover	52.3%	41.2%
Belief	44.5%	49.6%
N	44	34

Notes. Values for baseline from week 1. Belief on second-mover action of 20 participants in another session.

5.A.2 Robustness checks

In addition to the treatments reported in the main body of the paper, we conducted four robustness treatments. Their aim is to further examine the influence of the mode of measurement. For this purpose only one action and the belief were elicited. That means subjects play the game either as second movers or as first movers in one week, while beliefs are elicited in the other week (see Table 5.A.3).

In treatments *robustness: second mover* and *robustness: second mover - order* second-mover actions and beliefs are elicited in different weeks. The only difference treatments *low salience - time* and *low salience - time & order*, respectively, is that there subjects are never asked to play in the role of first movers. In the former treatments, we do not find significant correlations between second-mover actions and beliefs (see Table 5.A.4, left column). This is in line with our interpretation of the findings in treatments *low salience - time* and *low salience - time & order* that the consensus effect is small or non-existent unless measurement beliefs are measured immediately following preferences.

In treatments *robustness: first mover* and *robustness: first mover - order* first-mover actions and beliefs are elicited two weeks apart. This procedure is unlike any of the main treatments and aims at understanding the effect of simulta-

Table 5.A.2. Main treatments: Correlations between first-mover actions, second-mover actions and beliefs.

	High salience	**Low salience - time**
$\rho_{1st,2nd}$	.150 (.397)	.165 (.233)
$\rho_{2nd,belief}$	.559*** (.001)	.234* (.089)
$\rho_{1st,belief}$	.371** (.031)	.237* (.084)
N	34	54

	Low salience - order	**Low salience - time & order**
$\rho_{1st,2nd}$	.413*** (.005)	-.080 (.655)
$\rho_{2nd,belief}$	.218 (.155)	.268 (.126)
$\rho_{1st,belief}$	.036 (.816)	.158 (.373)
N	44	34

Notes. ρ: Spearman's correlation coefficient. p-values in parentheses.

neous elicitation of first-mover actions and beliefs and the role of timing. The correlations between first-mover actions and beliefs are virtually identical in the robustness treatments.

Table 5.A.3. Treatments: Robustness checks.

Same session Role	No 2^{nd} mover	No 1^{st} mover
	Robustness: Second mover	**Robustness: First mover**
Order 1	2^{nd} mover (W1) Belief (W3)	1^{st} mover (W1) Belief (W3)
N	45	34
	Robustness: Second mover - order	**Robustness: First mover - order**
Order 2	Belief (W1) 2^{nd} mover (W3)	Belief (W1) 1^{st} mover (W3)
N	41	41

Notes. The terms (W1) and (W3) following a measure indicate that this measure was elicited in week 1 and week 3, respectively.

Table 5.A.4. Robustness treatments: Correlations between actions and beliefs.

	Robustness: Second mover	**Robustness: First mover**
$\rho_{2nd,belief}$	.167 (.272)	
$\rho_{1st,belief}$		.304* (.081)
N	45	34
	Robustness: Second mover - order	**Robustness: First mover - order**
$\rho_{2nd,belief}$	.119 (.458)	
$\rho_{1st,belief}$		.281* (.075)
N	41	41

Notes. ρ: Spearman's correlation coefficient. p-values in parentheses.

5.A.3 Full study

Table 5.A.5. Overview of all tasks participants completed.

Week 1	**Week 2**	**Week 3**
Mood Question General Risk Question Big Five	Mood Question General Risk Question Big Five Trust Question	Mood Question General Risk Question Big Five Locus of Control
Binary Trust Game: Treatments		**Binary Trust Game: Treatments**
Risk Premia (Choice Lists) Risk Scenarios	"Will you win?" task Urns Task Bet: Heads or Tails? Ambiguity preferences and Beliefs	Risk Premia (Choice Lists) Common Ratio Effect BRET
Sociodemographics	Optimism: LOT and SOP	Optimism: LOT and SOP IQ
Mood Question	Mood Question	Mood Question

Notes. For detailed information on the tasks not described in this paper refer to Dohmen et al. (2018a) and Dohmen et al. (2018b)