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Active and Passive Risk-Taking

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Active and Passive Risk-Taking

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Abstract

Risk-taking may depend on whether risks result from an action (active risk-taking) or from not taking action (passive risk-taking). We develop a new experimental risk-elicitation procedure, the Dynamic Lottery Adjustment Task, and employ it across two separate experiments to study the size and direction of potential mode-of-choice effects (i.e. differences in risk-taking between active and passive decision modes). While our tightly controlled lab study provides little evidence for such effects, we find substantial evidence for mode-of-choice effects when decisions are spread out over 10 days and attention costs are a key feature of the online choice environment we use.

JEL: D81, D91, C91

Keywords: risk-taking, mode-of-choice, passive decision making, attention costs

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1 Introduction

Risk is a fundamental feature of economic decision making and there is broad interest in understanding how individuals make decisions about risk (e.g. Rieger et al., 2015; Armantier and Treich, 2016; Holzmeister et al. 2020). Standard theories of decision making under risk draw no distinction between active (i.e., risk resulting from an action) and passive risk-taking (i.e., risk resulting from not taking an action) as they are mainly concerned with outcomes and their probabilities (von Neuman and Morgenstern, 1944; Savage, 1954; Gollier, 2001).

However, in many settings of economic relevance taking or avoiding risks is considered to be an active decision: Chief executives decide to invest in moon-shot research and development projects, portfolio managers pick assets destined to outperform, and laypeople buy total permanent disability insurance. Accordingly, most empirical and experimental research on risk preferences and risk-taking builds on variations of the seminal lottery choice paradigm which generally involves an active choice between prospects with different expected values and variances (Binswanger, 1981; Gneezy and Potters, 1997; Holt and Laury, 2002; Lejuez et al., 2002; Eckel and Grossman, 2002, 2008; Andersen et al., 2006; Banks et al., 2019;). There is, however, also a wide range of situations of economic relevance in which a change in risk-exposure is not a consequence of an *active* decision, but rather the result of inaction. Not making the necessary investments to stay competitive, not rebalancing portfolios, and not buying insurance clearly affect the risk exposure of companies and individuals, yet are the result of abstaining from taking an action. Future outcomes from these decisions become the result of *passive* risk-taking.

As yet, there exists little empirical evidence whether decision makers take more risks actively or passively all else equal. There is survey evidence that individuals see active and passive risk-taking as distinct concepts as well as self-report differences in the propensity to take risks actively or passively. Keinan and Bereby-Meyer (2012, 2017) were the first to characterize active and passive-risk taking as distinct concepts and provide evidence for differences in self-reported propensities to take risks actively and passively. As yet, there exists little empirical evidence whether these self-reported differences also carry over to actual risk-taking behavior i.e. whether decision makers take more risks actively or passively all else equal.¹ Understanding whether the mode of choice (active vs. passive) matters would not only

¹ Arend et al. (2020) provide evidence for such behavioural differences in the specific context of cyber security risks. The paucity of empirical evidence in other areas of risk taking may reflect that for many naturally occurring situations active and passive risk taking occurs across different domains.

be important to accurately characterize and predict risk taking in different types of economic decisions but would also have important methodological implications for thinking about appropriate risk elicitation methods. Reviewing the state of risk preference measurement research, Bran and Vaidis (2020) conclude that most of the methods currently in use ignore the fact that in many domains risks are taken passively.

We answer the authors' call for the development of incentivized methods for the assessment of passive risk-taking by designing the Dynamic Lottery Adjustment Task (DLAT), an incentivized risk elicitation procedure that extends the popular lottery-choice paradigm (details in section 2; cf. Binswanger, 1981; Eckel and Grossman, 2002, 2008). In this paper, we report on two studies that use the DLAT to gain a better understanding of how the different modes of choice (active vs. passive) influence risk-taking behavior in monetarily incentivized decisions.

Previous empirical research on several well-known inaction phenomena relate to the main question of our paper. In particular, vignette studies on omission bias (Ritov and Baron, 1990, 1992) posit that DMs differ in their assessment of risky outcomes, depending on whether these outcomes result from an action or from remaining passive. Individuals displaying omission bias will select riskier options when these are implemented by remaining inactive as they would judge adverse outcomes resulting from passivity as less regrettable than adverse consequences resulting from an active choice.² While these phenomena have been well described in the literature, there is little systematic research how pure mode of choice effects (passive vs. active) affect risk taking in context free settings that allow to control many of the features that could lead to omission bias in the settings being studied.³ Attention costs provide a second reason for suspecting that the mode-of-choice could affect risk-taking: Not acting typically requires less (cognitive) effort and less attention than taking an action.⁴ For instance, recent experimental evidence demonstrates that disturbing attention by a background task almost doubles the likelihood that decision makers remain passive instead of taking an action (Altmann et al., 2019). Thus, a decision maker who is (rationally) economizing on her attention and cognitive

² In naturally occurring situations, omission bias is often difficult to disentangle from status quo bias, i.e. a general tendency to do nothing or a preference for maintaining a previous decision (Schweitzer, 1994). In line with Samuelson and Zeckhauser (1988), we refer to status quo bias as the tendency of adopting options more frequently when they are (perceived as) the status quo.

³ Much of the early evidence on omission bias comes from vignette studies that are set in the context of vaccination decisions. While this is an important context in which passive risk taking occurs it does not allow for clean comparison to active risk taking within the same domain.

⁴ Note that various cognitive costs, such as effort and attention costs, are difficult to disentangle. For our purpose, we collectively refer to these as attention costs and assume that they affect decisions in the same direction.

resources may be less prone to take action. Attention cost, therefore, imply that DMs will take more risks when inaction leads to risk-taking than when inaction leads to risk avoidance and vice versa.

In the first experiment, which we describe in section 3, we elicit risk preferences through the DLAT in a highly controlled laboratory environment using a two-by-two experimental design. We vary the mode of choice (active/passive) and the initial lottery endowment (risky/safe) and keep attention costs minimal. Between-subjects comparisons allow us to identify potential mode-of-choice effects on the measurement of risk preferences while controlling for starting-point effects resulting from the initial lottery endowment. In the second experiment (section 4), we design an online choice environment that more closely resembles risk-taking outside of the lab. In particular, eliciting choices online allows us to conduct the experiment over a period of ten days, thereby increasing the time between any two decisions. Stretching choices over this extended period considerably increases the attention costs of implementing an active choice. Hence, Experiment 2 allows us to test if there is a mode-of-choice effect when active and passive modes differ by the additional factor of attention costs.

Experiment 1 does not provide evidence for a pure mode-of-choice effect. Both when participants are endowed with a safe lottery, and when they are endowed with a risky lottery, there are no significant differences in risk-taking that can be attributed to the mode of choice. Independent of the mode of choice, we observe that the initial lottery endowment has a strong effect on risk-taking: Participants initially endowed with a safe lottery take less risk than participants initially endowed with a risky lottery. Mode-of-choice effects are substantial in Experiment 2. When remaining passive leads to greater risk, participants take more risks compared to situations in which they take risks by an active decision. In situations where inaction leads to smaller risks, participants take less risk compared to the active choice treatment. As in Experiment 1, the initial lottery endowment affects risk-taking.

We attribute our main finding that mode-of-choice effects are absent in Experiment 1 but present in Experiment 2 to the fact that attention costs are significantly higher in Experiment 2, where decisions are spread over ten days. This observation suggests that laboratory experiments that elicit risk attitudes through active choices may provide suitable measurements for passive risk-taking in naturally occurring situations only if attention costs are not a significant feature in these environments. Our results also shed some light on the theoretical arguments that underlie inaction phenomena such as omission bias. The fact that mode-of-choice effects are absent in Experiment 1 suggests that decision-makers do not generally treat risky outcomes

differently, depending on whether these outcomes result from taking an action or from remaining passive. Conversely, arguments based on attention costs are consistent with the pattern of risk-taking observed in Experiment 2. We further explore the practical implications of mode-of-choice effects in the concluding section 5, where we relate our findings to a literature on reminders and attention costs (e.g., Altmann et al., 2020).

2 Dynamic Lottery Adjustment Task

We construct the DLAT as a new risk elicitation procedure in which participants change their exposure to risk through either taking action or through remaining passive. For this purpose, we modify the *Ordered Lottery Selection* (OrdLS) procedure popularized by Binswanger (1980, 1981) and Eckel and Grossman (2002, 2008) and adapt both the presentation of lotteries and the mechanism of lottery selection. The OrdLS task asks subjects to select one lottery from a set. Each lottery in the set has two equally likely outcomes. The expected payoffs of the lotteries range from safe (both outcomes yield the same payoff) to very risky (one outcome yields no payoff). Expected payoff and variance increase when moving from the safe to the very risky lottery. While the OrdLS procedure typically presents all lotteries within a choice set simultaneously and asks participants to actively select their preferred lottery, we only present one lottery at a time. Participants then decide whether they would like to retain their current lottery or whether they wish to adjust its payoffs, thereby creating a new lottery. In the latter case, payoffs adjust and participants can then decide if they wish to make further adjustments or retain the newly created lottery. This process continues until participants retain the current lottery, or the maximum number of adjustment steps have occurred.⁵

On participants' decision screens, lotteries are displayed as bar charts. Each bar represents the payoff associated with one of the outcomes, which are labelled as *Green* and *Yellow*. The number below each bar indicates the change in payoffs, which would result if subjects choose to adjust their current lottery. In the example shown in Figure 1, the next adjustment step would result in a new lottery yielding EUR 5.40 (-0.60) if the outcome is *Green* and EUR 6.90 (+0.90) if the outcome is *Yellow* (see Figure B2 in Appendix B). Participants can cycle through up to ten adjustment steps, with each step leading to the same change in payoffs for the *Green* and *Yellow* outcomes.

⁵ For more information on the construction of our lotteries and the actual lotteries we used in the experiment, refer to Appendix A.

Figure 1: Example decision screen



Note: This Figure represents lottery F1.5-Medium at step $s = 0$. All values are in Euros.

The task format allows for variations in two treatment dimensions. First, to test for mode-of-choice effects, lottery adjustment steps can be implemented through action or inaction. In the ACTIVE condition, adjustments require the participant to click a button within a fixed time period. If they click the button, lottery payoffs adjust after the time period has passed. If they do not click the button within the time period, the current lottery is selected and the round ends. In the PASSIVE condition, adjustments to the payoffs of the current lottery occur automatically at the end of each adjustment period. Participants have the option to stop the adjustment process at any time by clicking a button. Once subjects click the button, the current lottery is selected and the round ends. If participants do not click the button at all, lottery payoffs continuously adjust up to ten times. Importantly, all features of the decision process, other than the mode by which decisions are implemented, are held constant across the two decision modes. In particular, we ensure that the adjustment process happens in the same time-intervals in both the ACTIVE and the PASSIVE conditions. That is, in the ACTIVE condition adjustments do not occur immediately, but only after the same time has passed as in the PASSIVE condition.⁶

Second, to understand if the presence of mode-of-choice effects depends on whether an adjustment leads to more or less risk-taking, the initial lottery is either SAFE or RISKY. In the SAFE condition, participants are initially endowed with a safe lottery that pays the same amount for each outcome. Any adjustment step decreases the payoff associated with the *Green* outcome

⁶ Instead, if choices in the ACTIVE condition were implemented immediately, conditions would be less comparable because participants in the ACTIVE condition could cycle through multiple adjustments more quickly.

by some amount and increases the payoff associated with the *Yellow* outcome by a larger amount. In other words, each adjustment increases the expected payoffs and their variance. In the RISKY condition, participants are initially endowed with the riskiest lottery, where only the *Yellow* outcome yields a positive payoff. Any adjustment step increases the payoff of the *Green* outcome by some amount and reduces the payoff associated with the *Yellow* outcome by a larger amount. Thus, payoff variance and expected value decrease with each adjustment step.

There are several advantages in basing our design on the *Ordered Lottery Selection* (OrdLS) procedure (Binswanger, 1980, 1981) rather than other existing risk elicitation procedures.⁷ First, the DLAT is an abstract lottery selection task and as such void of contextual clues or a supporting narrative. We avoid a context-rich environment which may interact with the different modes of choice or distract from the main risk-reward trade-off (e.g. by activating fear of losses through contextual clues). Our task is therefore especially suitable to study general mechanisms of risk taking behaviour, rather than risk-taking in specific contexts. Second, we aim for a comprehensive design that allows to study mode-of-choice effects both in a setting where adjustment steps can lead to more and less risk taking. In the abstract setting of the DLAT, it is possible to endow participants with either a RISKY or a SAFE starting lottery, without having to change the instructions or any other aspects of the task. Third, the DLAT is linear in probabilities and it is easy to visualize that both expected payoffs and their variance increase (decrease) with every adjustment step. In this regard, we follow the simple task structure of Eckel and Grossman (2002, 2008). More complex tasks, which are often non-linear in probabilities, are harder to understand for participants and tend to generate more noise and generate more inconsistent choices (cf. Charness et al. 2013; Crosetto and Filippin, 2013).

3 Experiment 1

3.1 Design

We implement the DLAT in a two-by-two between subject design, varying both the mode of choice (ACTIVE / PASSIVE) and the initial lottery endowment (SAFE / RISKY). Experiment

⁷ In particular, *dynamic* and *click* variants of the Bomb Risk Elicitation Task (BRET) by Crosetto and Filippin (2013, 2016) could be used to study active and passive risk taking. In the BRET, a bomb is hidden in one of 100 boxes that explodes after the end of the task. Participants can collect as many boxes as they wish with each box collected increasing their potential earnings from the task, as long as the bomb is not in one of the boxes collected. There are, however, some disadvantages of adapting this task for our purposes. First, the task is non-linear in probabilities which can lead participants to change their reasoning throughout the task. The specific setting also makes it difficult to implement a treatment in which participants start with a risky lottery and can subsequently reduce their exposure without substantially changing the narrative of the task.

1 consists of ten distinct decision rounds of the DLAT. Participants remain in the same treatment for all rounds. Rounds differ in lottery payoffs and adjustment step sizes. Varying lottery payoffs across rounds allows us to test if mode-of-choice effects depend on two additional features: The size of stakes and adjustment steps. Over the ten rounds, the expected payoff from the riskiest lottery varies from EUR 2 to EUR 18. We separate the ten rounds in two blocks of five rounds each. In the first block, adjustments to the yellow outcome are 1.5 times larger than that of the green outcome. We refer to these as *Factor 1.5* lotteries. In the second block, the adjustment factor is 3 (*Factor 3* lotteries). We counterbalance the order in which both blocks occur during the experiment to control for systematic order effects; i.e. some participants first encounter the Factor 1.5 lotteries while others first encounter the Factor 3 lotteries.⁸ We set the time between individual payoff adjustments to 5 seconds.⁹ Participants do not receive any feedback on lottery outcomes until the end of the experiment.

3.1 Procedures

After admitting participants to the lab, they are placed at a random computer terminal and read the instructions on screen.¹⁰ Participants start with a short trial task (see Figure B1 in Appendix B), aimed at familiarizing them with the adjustment mechanism and lottery display. After that, they could begin with the main part of the experiment. Immediately after the main tasks, we elicit additional information regarding the content of the experiment, participants' demographics, and stated risk preferences. The final questionnaire also contained an incentivized, choice set version of the Factor 1.5 Medium lottery of the DLAT (Table A1 in Appendix A) in a pie chart presentation format also used by Deck et al. (2013; our Figure B3 in Appendix B). We use this in a robustness check that compares the different presentation formats (sequential versus simultaneous). One of the ten rounds or the choice set version was randomly selected with an equal chance to be payoff relevant.

⁸ Within a block, lotteries occur in a non-monotonic, but fixed, order. This prevents participants from anticipating the payoffs of subsequent rounds. Tables A1 and A2 in Appendix A contain information on all lotteries and associated payoffs.

⁹ We pretested different time intervals with a group of colleagues prior to settling at 5 seconds. Longer time periods were perceived as increasing decision fatigue by participants, while shorter time periods were seen as creating mild time pressure. Kirchler et al. (2017) and Kocher et al. (2013, 2019) do not find evidence of time pressure affecting risky decisions compared to unconstrained decisions in the pure gain domain. We nevertheless take measures to mitigate potential time pressure effects. First, we added a delay of 15 seconds to the start of each adjustment period (i.e. for each new lottery). This delay allows participants to familiarize themselves with the decision screen and clearly identify the starting values of the lottery. Second, we introduced an unincentivized trial period before the actual decision rounds. In the trial period, participants could try out the mechanics of adjusting a bar chart on the screen and get a feeling for the length of a five second interval (see also Figure B1 in Appendix B).

¹⁰ All materials for replication of the experiment will be made available in a data repository after publication.

The experiment was conducted at the AWI Lab at Heidelberg University in October and November 2017. We used hroot to invite participants from the standard pool of student subjects (Bock et al. 2014), and oTree to program and run the experiment (Chen et al. 2016). In total, 199 participants took part in the experiment (SAFE/ACTIVE: 52, SAFE/PASSIVE: 52, RISKY/ACTIVE: 48, RISKY/PASSIVE: 47) and each session took approximately 45 minutes. In our sample, there are 80% native German speakers, 57% females, and 28% students of economics. At the time of the experiment, our participants were on average 22.9 years old. Average earnings amounted to EUR 9.87, including a show-up fee of EUR 3.00.

3.2 Results

We use the coefficient of constant relative risk aversion (r) implied by participants' lottery choices as our main measure of risk preferences. Assuming a CRRA utility function ($u(x) = \frac{x^{1-r}}{1-r}$), participants' lottery choices translate into a range of possible coefficients r that are consistent with these choices.¹¹ Table 1 summarizes, for the different lotteries used in the experiment, the implied risk aversion coefficient ranges and their midpoints we use for our analysis.

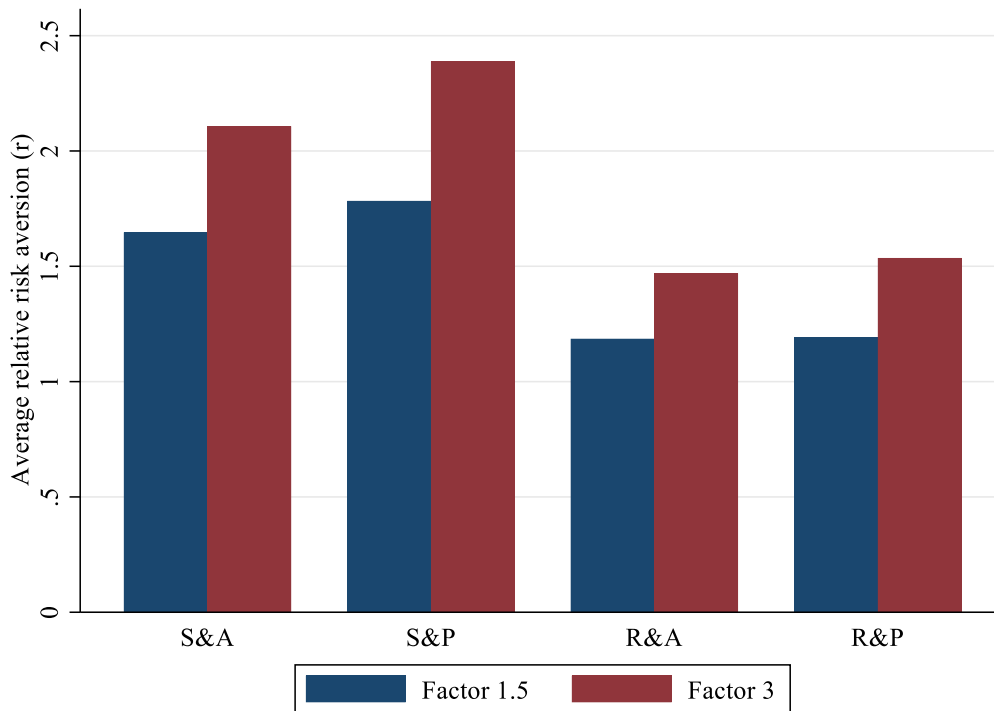
¹¹ In line with the existing literature, we take the midpoints of these ranges as the implied risk aversion of our participants. For participants who select the first or the last lottery, only the upper or lower bound of the interval can be derived from lottery choices and we use these as the measure of implied risk aversion.

Table 1: Implied risk aversion coefficients

Step	Factor 1.5 lotteries		Factor 3 lotteries	
	Range	Midpoint	Range	Midpoint
0	$r \leq 0.11$	(0.11)	$r \leq 0.24$	(0.24)
1	$0.11 < r \leq 0.16$	0.135	$0.24 < r \leq 0.35$	0.295
2	$0.16 < r \leq 0.2$	0.18	$0.35 < r \leq 0.44$	0.395
3	$0.2 < r \leq 0.24$	0.22	$0.44 < r \leq 0.52$	0.48
4	$0.24 < r \leq 0.3$	0.27	$0.52 < r \leq 0.63$	0.575
5	$0.3 < r \leq 0.37$	0.335	$0.63 < r \leq 0.77$	0.7
6	$0.37 < r \leq 0.49$	0.43	$0.77 < r \leq 0.97$	0.87
7	$0.49 < r \leq 0.68$	0.585	$0.97 < r \leq 1.31$	1.14
8	$0.68 < r \leq 1.12$	0.9	$1.31 < r \leq 2.08$	1.695
9	$1.12 < r \leq 3.33$	2.225	$2.08 < r \leq 6.34$	4.21
10	$r > 3.33$	(3.33)	$r > 6.34$	(6.34)

Note: Column Step shows the number of adjustment steps from the safe lottery towards the riskiest lottery. The range columns show the implied coefficient of relative risk aversion ranges of choosing the present lottery instead of choosing one of the neighboring lotteries. Midpoint columns show the values we use as our main measure of relative risk aversion.

Figure 2: Risk aversion by treatment



Note: The figure shows relative risk aversion coefficients averaged over all lottery decisions by treatment condition, differentiated by Factor 1.5 and Factor 3 lotteries.

Figure 2 shows participants' average CRRA coefficient for each of the four treatment conditions and separated by step size factor. Mode-of-choice effects are clearly absent: Independent from initial lottery endowments (SAFE vs. RISKY), participants choose similar final lotteries when further adjustments require taking action or when they result from allowing lotteries to change automatically. The levels of average risk aversion are slightly higher in the passive adjustment treatments, but the differences are not statistically significant at conventional levels (Factor 1.5: RISKY: 1.19 vs. 1.19, $p = 0.66$; SAFE: 1.65 vs. 1.78, $p = 0.48$; Factor 3: RISKY: 1.47 vs. 1.54, $p = 0.77$; SAFE: 2.11 vs. 2.39, $p = 0.34$; Mann–Whitney U tests).¹² Our conclusions regarding the average level of risk aversion are further corroborated by a multivariate regression analysis that controls for order effects and potential differences in sample composition (Table C1 in Appendix C). Importantly, this analysis demonstrates that our conclusions also hold for a simple measure of risk aversion, namely the number of adjustment steps made by participants.

Result 1 (lab, mode-of-choice): *There is no evidence for a significant mode-of-choice effect. Independent from initial lottery endowments, average lottery choices are statistically indistinguishable between the ACTIVE and the PASSIVE conditions.*

The initial lottery endowment (SAFE vs. RISKY) affects risk-taking independently from the mode-of-choice (ACTIVE vs. PASSIVE). Participants who are initially endowed with a safe lottery display significantly higher levels of risk aversion than participants who are initially endowed with the riskiest lottery (Factor 1.5: ACTIVE: 1.65 vs. 1.19, $p < 0.01$; PASSIVE: 1.78 vs. 1.19, $p < 0.001$; Factor 3: ACTIVE: 2.11 vs. 1.47, $p < 0.05$; PASSIVE: 2.39 vs. 1.54, $p < 0.01$; Mann–Whitney U test).¹³

Result 2 (lab, initial lottery): *The initial lottery endowment affects the average level of risk aversion independent from the mode-of-choice. Participants initially endowed with a safe lottery choose less risky lotteries than participants initially endowed with the riskiest lottery.*

¹² Full Sample without differentiation by adjustment size factor: 1.77 vs. 1.63, p -value = 0.34; RISKY: 1.38 vs. 1.34, p -value = 0.73; SAFE: 2.12 vs. 1.90, p -value = 0.31; Mann–Whitney U test. Insignificant findings are not due to a low statistical power of the test. Ex-post power analysis reveals that our sample size is sufficient to detect small to medium effect sizes (Cohen's $d = 0.4$).

¹³ Full Sample without differentiation by adjustment size factor: 2.01 vs. 1.36, p -value < 0.001; PASSIVE: 2.11 vs. 1.38, p -value < 0.001; ACTIVE: 1.90 vs. 1.38, p -value < 0.001; Mann–Whitney U tests.

3.3 Robustness

3.3.1 Individual Lotteries

Looking at each of the lotteries individually in separate OLS regressions instead of investigating the average level of risk aversion, we still do not find systematic evidence for mode-of-choice effects. Only in one out of nine lotteries (F3-Lowest),¹⁴ participants in the ACTIVE condition display significantly lower levels of risk aversion than participants in the PASSIVE condition ($p = 0.03$). In the remaining eight lottery decisions, mode-of-choice effects are small and statistically indistinguishable from zero (all $p > 0.3$). Using a Bonferroni correction to account for multiple comparisons, none of the coefficients for the active decision mode reaches the adjusted significance level. In contrast, being initially endowed with a safe lottery significantly increases risk aversion (all $p < 0.05$) in all nine rounds (Figure C1 in Appendix C). Finally, employing a set of panel regressions, we demonstrate that accounting for specific payoff characteristics of single lotteries and the full sequence in which single lottery choices appeared on participants' decision screens does not alter any of these conclusions (Figure C1 and Table C2 in Appendix C). Jointly, these observations provide additional support for the robustness of results 1 and 2.

3.3.2 Retention rate of original lottery

The mode of choice does not affect how often participants retain the original lottery. Moreover, focusing only on participants who make at least one adjustment step to their initial lottery leaves conclusions about the absence of mode-of-choice effects unchanged.

Across all nine rounds, no changes to the initial lottery occur in 19–25% of all cases. Only a small minority of participants (<2%) retains their initial lottery in all nine decisions, whereas 33% of participants choose to adapt their lottery payoffs at least once in all decisions. For each participant we compute a variable that shows the number of rounds in which the participant selected the initial lottery. Panel A of Figure 3 shows that this variable does not vary substantially across the different treatment conditions ($p > 0.1$ for all possible pairwise comparisons, Mann–Whitney U tests).

Importantly, the mode of choice does not affect the frequency of retaining the initial lottery. Independent from the initial lottery type, we find that it does not matter whether changing

¹⁴ Due to a coding error, Lottery 1.5-Low was not shown to participants, leaving us with 9 different lotteries per round only.

lottery payoffs requires participants to take action (such that retaining the initial lottery is the default option) or adaption steps are implemented by waiting (such that retaining the default requires an action) (Full Sample: 0.21 vs. 0.23 p-value = 0.18; RISKY: 0.24 vs. 0.22, p-value = 0.42; SAFE: 0.19 vs. 0.24, p-value = 0.66; Mann–Whitney U test).

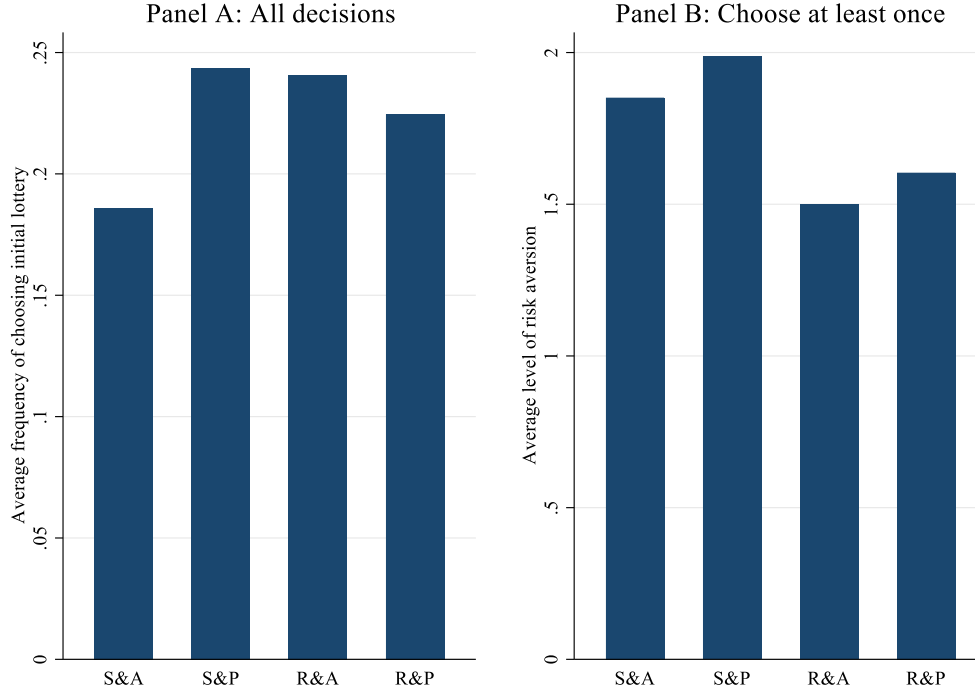
Similarly, the initial lottery endowment (SAFE vs. RISKY) does neither affect the propensity to retain the initial lottery in the ACTIVE (0.19 vs. 0.24, p-value = 0.52; Mann–Whitney U test) nor in the PASSIVE (0.22 vs. 0.24, p-value = 0.13; Mann–Whitney U test) treatments. When aggregating across different modes of choice, we similarly do not find evidence that the propensity to retain the initial lottery differs significantly between treatments with a SAFE or a RISKY initial lottery endowment (0.23 vs. 0.21, p-value = 0.51; Mann–Whitney U test).

These observations are robust to controlling for additional individual characteristics and the order in which choices were presented to participants, as shown in regression Table C3 of Appendix C. Finally, an analysis of retention rates across single lottery decisions (i.e. at the lowest level of aggregation) produces similar results (Figure C2 in Appendix C). Only for F3-low, participants in the ACTIVE condition are significantly less likely to retain their initial lottery than participants in the PASSIVE condition ($p < 0.05$). This indicates that the overall mode-of-choice effect found for this lottery results, at least in part, from differences in the rates at which participants retain their initial lottery endowment. For all remaining lottery decisions, there are no differences in retention rates across different modes of choice.

In Figure 3, Panel B we further probe the robustness of result 1 (i.e., the absence of mode-of-choice effects) by displaying the average level of relative risk aversion (r) across treatment conditions for a subsample of participants who made at least one change to their initial lottery payoffs. In this restricted sample, there is still no evidence that lottery choices arrived at through waiting (PASSIVE) differ in their implied level of risk aversion from choices arrived at through an action (ACTIVE). Thus, the observed absence of mode-of-choice effects in the full sample is not due to different retention rates of the initial lottery.

Concerning the effects of the initial lottery endowment summarized in result 2, excluding participants who choose the initial lottery changes our earlier finding on differences between the SAFE and the RISKY condition. Participants in the SAFE condition who decide against retaining their initial lottery show a lower level of average risk aversion than participants who decide against retaining their initial lottery in the RISKY condition.

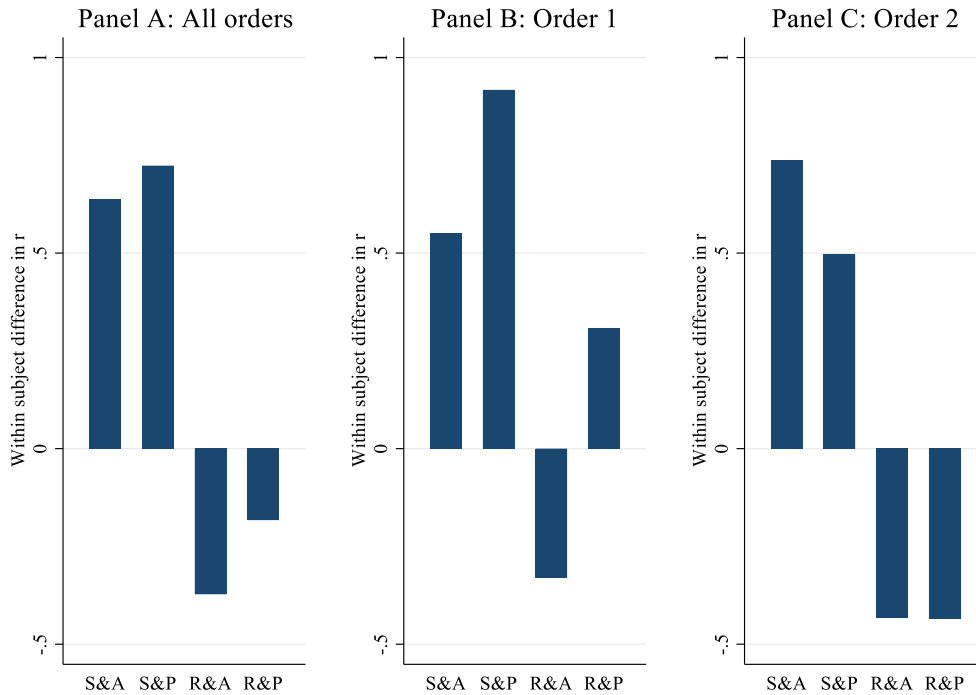
Figure 3: Retention of initial lottery



Note: Panel A shows the average frequency of participants' choosing the initial lottery by treatment. Panel B shows average risk aversion coefficients by treatment for those that take at least one adjustment step.

Finally, we investigate the differences in elicited risk preferences between the DLAT and a standard OrdLS risk elicitation task employing the graphical presentation format of Deck et al. (2013). Lottery payoffs and associated probabilities are the same across both tasks, such that any observed differences in behavior would be purely attributable to differences in presentation format. In particular, this within-participants analysis compares the payoffs selected for F1.5-medium in the adjustment task to the lottery choice made when the same lottery was presented again in the Eckel-Grossman choice set format at the end of the experiment. Figure 4 depicts the within-subjects differences in elicited risk aversion (the coefficient of relative risk aversion in the Eckel-Grossman format is subtracted from the coefficient of relative risk aversion calculated from the choice in lottery F1.5: in the DLAT: $r_{F1.5} - r_{EG}$). Panel A contains the result for all participants independent of the order condition. In order 1, F1.5-medium occurred earlier in the progression of the experiment than in order 2. Thus, depending on order, the two choices are either elicited with a larger (Panel B) or smaller (Panel C) temporal gap. In Panels B and C, we account for the possibility that the choice order may influence individual choice consistency.

Figure 4: Within-subject differences in elicited coefficients of risk aversion



Note: Panel A shows the differences in elicited coefficients of relative risk aversion between the main adjustment task and the standard choice set presentation of lottery F1.5-Medium by treatment. Panels B and C further distinguish between the orders of the lotteries in the experiment, specifically whether lottery F1.5-Medium was presented 6 or 3 rounds before the standard choice set presentation of the same lottery.

Distinguishing by initial lottery endowment and pooling across orders (Panel A), we find that within-subjects differences in the PASSIVE condition are significant and positive when the initial lottery endowment is SAFE ($p < 0.001$; Sign-Rank Test) but not when it is RISKY ($p = 0.24$; Sign-Rank Test). Hence, subjects take less risk in the DLAT than in the standard presentation format when their initial lottery endowment is SAFE and lottery adjustments are made by waiting. Similarly, for the ACTIVE condition, within-subjects differences between the DLAT and the standard OrdLS task format are significant when the initial lottery endowment is SAFE ($p < 0.05$; Sign-Rank Test) but not when it is RISKY ($p = 0.17$; Sign-Rank Test).¹⁵ It appears that risk preferences elicited via the DLAT can result in different levels of risk aversion than risk preferences elicited via the OrdLS method. However, these individual-level differences mostly reflect differences in the starting lottery, i.e. differences across the SAFE and RISKY conditions. Different modes of risk-taking (in the ACTIVE and PASSIVE),

¹⁵ These differences persist when analysing data for the two order conditions separately (Panel B and Panel C). They are significant in Order 1 and not significant in Order 2.

do not influence individual-level differences in elicited risk preference beyond the effect of the initial lottery.

3.4 Discussion

In sum, we find no evidence for mode-of-choice effects in Experiment 1. A comprehensive set of robustness checks further corroborates this core result. Therefore, theories about omission bias, which posit that different modes of choice could result in different assessments of lottery outcomes, appear to have little traction in a tightly controlled and incentivized laboratory experiment. One possible explanation for this divergence to earlier studies providing substantial empirical support for omission bias is that these earlier studies are mainly based on hypothetical vignette studies and are placed in particular contextual settings such as vaccination or investment decisions (e.g. Ritov and Baron, 1990, 1992; Schweitzer, 1994).

Studying mode-of-choice effects in a highly controlled and distraction-free laboratory setting deliberately eliminates attention costs as an important factor that may differentiate active from passive decision making. In many naturally occurring situations, including those that were the focus of previous investigations (e.g. Ritov and Baron, 1990, 1992; Schweitzer, 1994), DMs have to pay sufficient attention to remember and execute an active decision. In contrast, passive decisions that occur automatically do not require constant attention. Instead, decision makers may have a stopping rule that they need to remember to act upon at the right time. For instance, many private investors tend to rebalance their portfolio at the end of a year while not acting at other times (Ritter & Chopra, 1989). In naturally occurring situations, decisions about risk also compete for attention with other daily activities. These parallel demands on cognitive resources make it harder to pay attention to one specific decision and differentiate naturally occurring situations from stylized laboratory tasks where participants already have committed their time and effort to a single activity by coming to the lab. The absence of mode-of-choice effects in experiment 1 may hence reflect the absence of attention cost that are a defining feature of risk taking outside of the experimental laboratory. To let the experimental risk elicitation procedure compete for attention with other daily activities, we thus leave the laboratory environment in Experiment 2 and considerably increase the time between adjustment steps.

4 Experiment 2

The fundamental difference between Experiment 1 in the lab and Experiment 2 online is the time between adjustment steps. In the lab, independent of the treatment condition, lottery adjustments occur after 5 seconds and the resulting maximal period during which participants

have to pay attention in a round of the lab implementation of the DLAT is 50 seconds (to implement ten adjustment steps). This unified time frame for decisions in both the ACTIVE and PASSIVE conditions keeps attention and effort costs close to zero and both conditions highly comparable. While this serves the purpose of creating a clean experimental setting, it is not very realistic and arguably fails to capture a defining component of active and passive risk taking outside of the laboratory environment. This defining component are effort, attention, and more generally opportunity costs, which are minimized in the lab, but may have substantial influence in real-life decision making. To introduce this component in the experimental setting, online Experiment 2 requires participants make decisions over the course of ten days. Adjustment steps now take place approximately 24 hours apart, allowing attention and effort costs to enter the optimization problem.¹⁶

4.1 Design

In Experiment 2, we elicit risk preferences through one single round in the DLAT, but stretch out adjustment decisions over ten days, during which participants can implement one adjustment step per day. On each day, adjustment decisions are either made by an active click of a button (ACTIVE) or by refraining from taking any action (PASSIVE). We apply the same two-by-two design as in Experiment – i.e. adjustment steps result in either more risk-taking (SAFE) or in less risk-taking (RISKY).

To elicit choices during a longer decision horizon, we created a website through which participants could make choices without having to be physically present in the lab. Adjustment decisions are approximately equally spaced in time by mandating decisions to be made within a four-hour time window on any given day. Participants could individually choose their preferred time window before the actual experiment began. That is, participants were able to select a time window which fit their individual day to day schedules. This design feature makes us confident that failures to make decisions on time are not the result of exogeneous, pre-existing scheduling commitments, but are indeed individually attributable. The selected time-window remained the same for all days of the experiment. Fixing a time windows in this way has the additional advantage that the time difference between decisions is held constant across participants. When logging on to the website outside of this time window, participants were reminded of the time window in which they can make a decision, but could not see the currently

¹⁶ Note that when we speak of attention and effort costs, we refer to the attention and effort it takes to remember to participate in the experiment, not the attention required to compare one set of lottery payoffs to the next between adjustment steps.

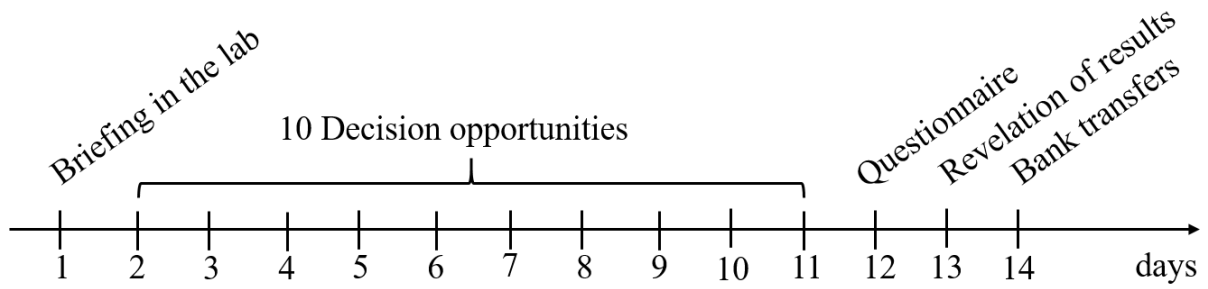
selected lottery or make any choices. Just as in Experiment 1, decisions to stop adjustments (PASSIVE) or not to trigger another adjustment (ACTIVE) are final. That is, participants in the PASSIVE condition could not restart the adjustment procedure after stopping it. Participants in the ACTIVE condition could not “skip” a day and continue adjusting lottery payoffs on the next day.

At the beginning of Experiment 2, participants learn that the adjustment step sizes of the lottery payoffs vary within each round and that they only learn about each step size once it has occurred.¹⁷ As in Experiment 1, the instructions do mention the payoff combination of the initial lottery and the final lottery, which correspond to those of lottery Factor 1.5-Medium in Experiment 1 (Table A1 in Appendix A). They also mention the smallest and the largest possible adjustment step size (0.50€ and 1.05€).

In contrast to Experiment 1, the decision screen does not contain any information about how the next adjustment step affects payoffs. We chose to vary the adjustment step sizes to ensure that subjects could *not* calculate the exact number of adjustment steps required to reach their preferred lottery payoffs. This is important, because otherwise participants could approach the two mode-of-choice treatments differently. If the step sizes were known exactly ex-ante, participants could plan ahead and calculate the days (steps) necessary to implement their preferred level of risk. Participants in the PASSIVE conditions would then be able to determine the day they wish to log-in to select their preferred lottery and achieve this by setting themselves a reminder or make a calendar entry.¹⁸ This would counter the idea of creating attention cost. Participants in the ACTIVE conditions on the other hand would not be able to rely on a self-set reminder, because they would have to log in each day to implement a desired adjustment step.

¹⁷ Specifically, we take lottery F1.5-Medium of Experiment 1, which had ten adjustments of equal size. With each step, one outcome was adjusted by 0.6, the other by 0.9, which we denote by writing (0.6, 0.9). For the second experiment, we modify this lottery by creating a fixed sequence of ten individual adjustment steps [(0.5, 0.75), (0.52, 0.78), (0.54, 0.81), (0.56, 0.84), (0.58, 0.87), (0.62, 0.93), (0.64, 0.96), (0.66, 0.99), (0.68, 1.02), (0.70, 1.05)]. Note that we only vary the adjustment steps, but not the starting payoffs or final payoffs after all adjustment steps have been applied compared to F1.5-Medium used in Experiment 1. Furthermore, the order of adjustment steps does not matter for reaching the same final lottery payoff combination. All participants get the same lottery adjustment steps, but their order is randomized on the individual level.

Figure 5: Timeline of Experiment 2



4.2 Procedures

Overall, the procedures of Experiment 2 span three phases. Figure 5 shows the timeline of the experiment. As part of the sign-up procedure, participants were invited to take part in a briefing session in the lab for which they received a EUR 3.00 show-up fee (day 1). The purpose of the briefing session was to walk participants through the instructions of the experiment, allow them to ask clarifying questions, collect basic demographic information, create a unique participation code, determine their preferred four-hour time window for decision making, and inform them about the specific payment procedures. To prevent treatment spill-over, participants in the same briefing session were all allocated to the same mode-of-choice treatment (all ACTIVE or all PASSIVE).

Starting from day 2, participants could make the actual adjustment decisions online over ten days. They used the unique participation code, generated in the briefing session, for signing into the website.¹⁹ The participation code allows us to merge choices made in the experiment with the demographic information collected in the briefing session and was also used for payment purposes.

After the main decision phase (day 12), all participants were asked to complete a short online questionnaire in exchange for an additional EUR 2.00. The questionnaire serves as a test for sample attrition, as it was announced in the briefing session and could be completed by all participants, independent of their lottery choices for extra monetary compensation. The questionnaire elicited further demographics, included a survey measure of risk preferences, and asked questions regarding the content of the experiment. Only from day 12 onwards,

¹⁹ Note that participants could log in multiple times within their time window to see the currently selected lottery. Naturally, they could only make a single decision in each window. For our analysis, logging in and not making a decision is the same as not logging in at all. However, based on login data, we can differentiate these cases.

participants were informed about the lottery outcome. Everybody received their lottery earnings and other payments via bank transfer on the following bank business day.

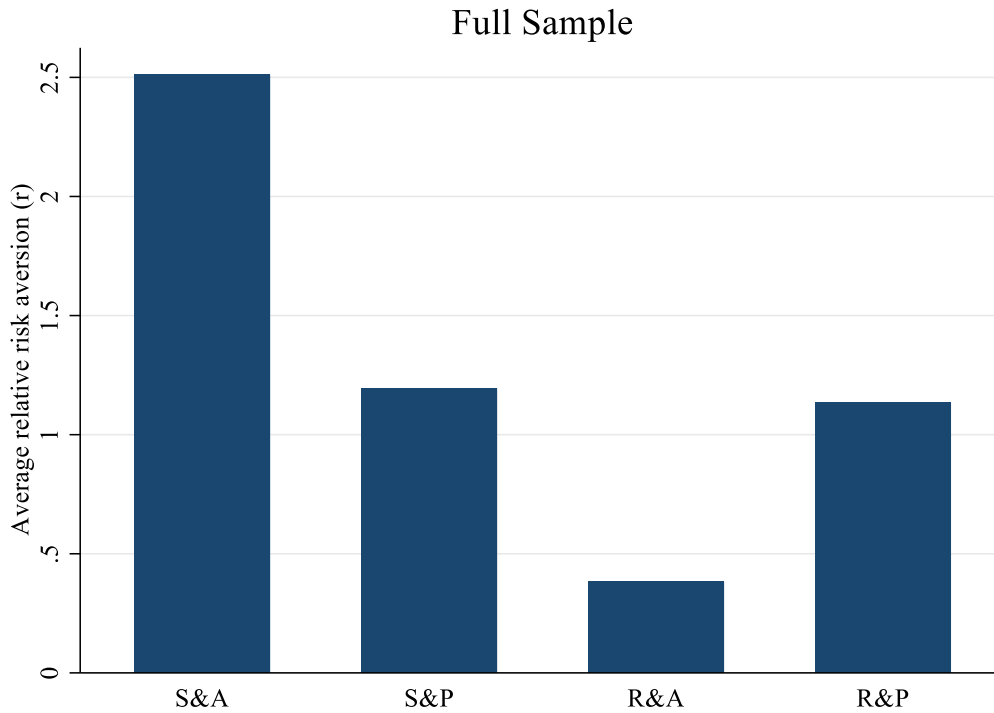
We developed our own website for the experiment and used hroot (Bock et al. 2014) to invite participants from the standard participant pool at the AWI lab in Heidelberg, excluding participants of Experiment 1. The invitation letter informed them that they would agree to take part in a multi-day online study after the initial briefing session in the lab and that they would receive their earnings from the online task via bank transfer.

In total, 212 participants took part in the initial briefing session (SAFE/ACTIVE: 47, SAFE/PASSIVE: 59, RISKYACTIVE: 54, RISKY/PASSIVE: 52) and received instructions and login information for the experiment. The briefing took at most 20 minutes and took place either Mondays or Wednesdays to counter potential weekend effects in the progression of the experiment. Our sample consists of 86% native German speakers, 57% females, and 26% study economics. At the time of the experiment, our participants were on average 23.6 years old. Including the show-up fee of EUR 3.00, they earned EUR 10.50 on average.

4.3 Results

Just as in Experiment 1, the main dependent variable for our analysis of risk-taking in Experiment 2 is the coefficient of constant relative risk aversion (r), which we derive for each participant from their lottery choice and its associated payoffs. In a first step, we consider data from all of our 212 participants, without excluding any observations.

Figure 6: Risk aversion by treatments



Note: Average relative risk aversion coefficients for the full sample, by treatment conditions.

Figure 6 illustrates how the risk aversion coefficient varies across the four treatment conditions of Experiment 2. It provides strong evidence for a mode-of-choice effect: participants, who are initially endowed with a SAFE lottery, such that each change of lottery payoffs results in more risk, take significantly higher risks in the PASSIVE than in the ACTIVE condition. ($p < 0.0001$; Mann–Whitney U test). When participants are initially endowed with a RISKY lottery, and each adjustment step decreases risk, they take significantly less risk in the PASSIVE compared to the ACTIVE condition ($p < 0.0001$; Mann–Whitney U test).²⁰ Thus, in contrast to Experiment 1, we find strong evidence for mode-of-choice effects. Participants in Experiment 2 were more likely to avoid risks when avoidance results from passivity. They were also more likely to take risks when risk-taking results from passivity. This pattern is consistent with a mechanism based on attention costs, but inconsistent with a mechanism based on regret avoidance. These results find further support in a set of regression models that control for additional individual characteristics (see Table D1 in Appendix D). Importantly, these

²⁰ Overall, these two mode-of-choice effects are similar in absolute size such that they cancel out when aggregating over the two endowment conditions (pooled: $p = 0.707$; Mann–Whitney U test).

regressions also show that our results also hold for a simpler measure of risk taking i.e. the number of adjustment steps taken in each condition.

Result 3 (online, mode of choice): *In the presence of attention and effort costs, there is systematic evidence for a significant mode-of-choice effect, independent from initial lottery endowments.*

As in Experiment 1, we find that initial lottery endowments have a strong effect on lottery choices when an action is required to implement changes ($p < 0.0001$; Mann–Whitney U test). In contrast to Experiment 1, this effect disappears when adjustment steps occur automatically (PASSIVE) such that the level of risk aversion that participants display across the SAFE and RISKY condition is similar ($p = 0.532$; Mann–Whitney U test). In other words, the overall significant effect of the initial endowment is almost fully attributable to the ACTIVE condition (pooled: $p < 0.001$; Mann–Whitney U test). Note that the simple explanation of participants in the PASSIVE conditions always waiting for a similar number of days before logging in and stopping the adjustment of lottery payoffs independent of initial lottery endowment cannot explain the absence of an effect of initial endowments: Participants in RISKY and SAFE take approximately the same level of risk. Implementing this level requires 1-2 passive adjustment steps on average in RISKY. In contrast, it requires 8-9 passive adjustment steps on average in SAFE.

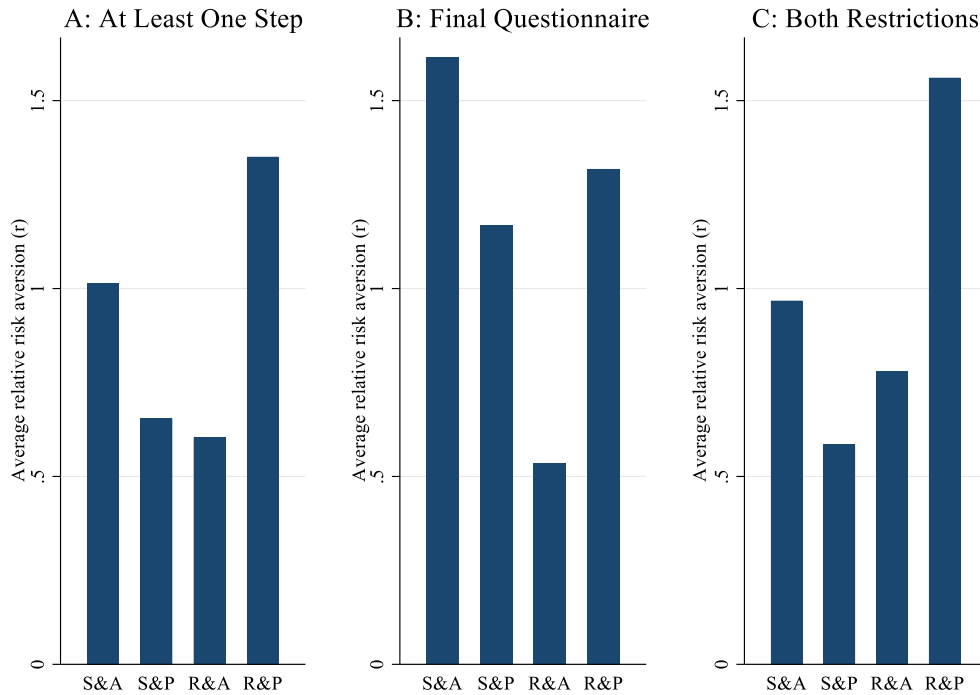
Result 4 (online, initial lottery): *In the presence of attention and effort costs, the initial lottery endowment affects the average level of risk aversion. Participants initially endowed with a safe lottery choose safer lotteries than participants initially endowed with the riskiest lottery. This effect is almost fully attributable to the ACTIVE condition, where an action is required to adjust lottery payoffs.*

4.4 Robustness

4.4.1 Subsample analysis

Based on Figure 7, we probe the robustness of mode-of-choice effects by repeating the analysis for several subsamples. Each subsample looks at participants who display a minimum level of attentiveness by making at least one change to the lottery payoffs or by filling the final questionnaire. In this selected sample of more attentive participants, we continue to find evidence for mode-of-choice effects.

Figure 7: Risk aversion by subsamples and treatments



Note: Average relative risk aversion coefficients by treatments conditions. Panel A restricts the sample to those who took at least one adjustment step. Panel B restricts the sample to those who completed the final online questionnaire. Panel C combines both sample restrictions.

Panel A shows results for 138 participants who implement at least one adjustment step.²¹ For participants in the RISKY condition, we find a mode-of-choice effect that is similar in size and direction as in the full sample ($p < 0.001$; Mann–Whitney U test). For participants in the SAFE condition, there is still some evidence for a mode-of-choice effect that is, however, smaller in size and statistically insignificant ($p = 0.168$; Mann–Whitney U test). Panel B restricts the sample to those 115 participants who log-in after 12 days to fill the final questionnaire. Again, for participants in the RISKY condition, we find strong evidence of a mode-of-choice effect ($p < 0.001$; Mann–Whitney U test). In the SAFE condition, the observed mode-of-choice effect is of smaller size and not statistically significant ($p = 0.251$; Mann–Whitney U test). Combining both previous sample restrictions and looking at the choices of 86 participants in Panel C yields very similar results. Participants in the RISKY condition display a statistically significant mode-of-choice effect ($p < 0.01$; Mann–Whitney U test), while

²¹ Across all conditions the rate of subjects who make at least one adjustment choice is 65% which is smaller than the 75%-81% observed in experiment 1. This could be seen as an indication that we have successfully manipulated attentions costs in Experiment 2.

participants in the SAFE condition display a mode-of-choice effect that is statistically insignificant ($p = 0.251$; Mann–Whitney U test).

Overall, this robustness check shows that mode-of-choice effects persist and are particularly strong for participants in the RISKY condition, i.e. when adjustment steps reduce the exposure to risk. For risk avoidance, participants are more likely to display risk-averse behavior when adjustments towards safer lottery payoffs emerge automatically than when they need to be implemented through taking an action. For the SAFE condition, in which adjustment steps lead to more risk-taking, mode-of-choice effects do not reach statistical significance. Notably, the theoretical arguments behind omission bias, which state that decision makers experience less regret from passive decisions, is not consistent with the pattern we observe in the SAFE condition: If suffering from omission bias, participants should take significantly less risk by action than by inaction. In the RISKY conditions, where both action and inaction lead to a reduction of risk exposure, this asymmetry in regret should not be present. Here, omission bias would not predict any significant differences between action and inaction, yet this is the pattern we observe.

The results of this subsample analysis also demonstrate that the differences between active and passive risk taking are not driven by a general lack of engagement in the PASSIVE conditions. Even participants, who stay engaged with the experiment over the course of 12 days display significant behavioral differences.

4.4.2 Lottery retention and participation in the final questionnaire

We next assess how treatments and individual characteristics affect participants' propensity to retain the original lottery and their propensity to complete the final questionnaire. While the former can be a direct effect of the experimental treatments, the latter would be an indication of treatment specific attrition (i.e., whether the treatments affect the propensity to drop out before completing the final questionnaire). We do so with the help of a set of Probit regressions and present their results in Table 2. Model (1) takes the propensity that participants choose to implement at least one adjustment step to their initial lottery endowment as the dependent variable and regresses it on the full set of treatment indicators.

Table 2: Regression results

	(1) Probability of at least one adjustment step	(2) Probability of filling the final questionnaire	(3) Probability of at least one adjustment step
Passive (1=Yes)	0.802** (0.268)	-0.088 (0.247)	0.838* (0.386)
Safe (1=Yes)	-0.437 (0.253)	-0.470 (0.253)	0.355 (0.423)
Passive#Safe (1=Yes)	0.325 (0.375)	0.382 (0.349)	-0.617 (0.592)
Constant	0.140 (0.172)	0.282 (0.173)	-1.453 (1.115)
Individual Controls	No	No	Yes
Obs.	212	212	115
Pseudo R ²	0.109	0.013	0.144

Note: Probit Regression. Standard errors are in parenthesis. *** / ** / * denote statistical significance at 0.1% / 1% / 5%. Control variables in model (3): Age (years), Male (1/0), Economics Major (1/0), Smoking (1/0), SOEP-Risk Scale (0-10), Final Grade Math (0-15), Regular Dental Check-Up (1/0), Disposable Income (Euros).

Participants in the PASSIVE condition are significantly more likely to adjust their initial lottery at least once compared to participants in the ACTIVE condition. There is no significant evidence that participants in the SAFE condition are less likely to make an initial lottery adjustment than their RISKY counterparts. There is also no significant interaction between the two main treatment effects. This observation indicates that the overall significant mode-of-choice effects can be partly attributed to the initial decision to change lottery payoffs. Model (3) repeats the analysis of model (1) controlling for a richer set of individual characteristics and focusing only on subjects who filled the final questionnaire. The conclusions regarding the PASSIVE treatment remain qualitatively unaffected by including these additional sets of control variables.

In model (2), we ask if the propensity to fill the final questionnaire varies with the different treatment conditions. Participants may decide early on that they wish to drop out of the experiment, i.e. there might be some treatment specific attrition, which would be problematic for the interpretation of our results. The estimated regression coefficients of model (2), however, do not indicate statistically significant effects of our treatments on the propensity to fill the final questionnaire. Thus, treatment-specific attrition is not detectable.

4.5 Discussion

In the presence of larger attention costs, we observe strong and robust differences between taking risks actively or passively. This raises two questions: First, can we rule out alternative explanations that would differ from a pure mode-of-choice effect? Second, are our results specific to risk preferences or are we observing a more general effect of attention costs?

In both experiments, initial lottery endowments affect participants' choices directly, much in the spirit of a status-quo bias (Samuelson and Zeckhauser, 1988). Initial lottery endowments also influence how the different modes of choice affect risk-taking. Participants display less (more) risk aversion in the passive compared to the active choice situation when starting from a safe (risky) lottery. The fact that the direction of mode-of-choice effects we observe in Experiment 2 is contingent on initial lottery endowments provides strong evidence against theories which argue that DMs judge outcomes differently if they result from an omission or a commission. These theories would suggest *more* risk-taking by omission, not *less* risk-taking by omission.

Active risk-taking differs from passive risk taking, yet the manifestation of such differences in our experiments exhibits substantial context-dependence: Mode-of-choice effects appear to be mainly driven by attention costs. When such costs are eliminated (as in Experiment 1) we observe no differences in risk taking. In Experiment 2, where such costs become actionable, the mode-of-choice matters greatly. The observation that mode-of-choice effects change in direction with initial lottery endowments is fully consistent with theoretical arguments based on attention costs.

It is conceivable that the effects of attention costs on behavior are not limited to situations of risk-taking alone, but may play a more general role for economic behavior. In its simplest form, an argument based on time-dependent attention costs may posit that taking an action becomes increasingly less likely as time progresses. Notably, choices in our experiment show an asymmetry between treatments that cannot be reconciled with this argument. In particular, we observe that participants in the SAFE/PASSIVE and RISKY/PASSIVE conditions take approximately the same level of risk, despite this requiring them to allow a different number of adjustment steps. In other words, if the decision to stop and implement a choice after a specific number of days was driven by attention cost alone (or a general loss of engagement with the experiment) we would see the same number of adjustment steps and, as a consequence, a difference in risk taking. This demonstrates, that in the context of risk, the effects of attention costs are more complex and interact with other features of the decision environment such as the

initial lottery endowment. Naturally, Experiment 2 can only directly speak to the differences attention costs make for risk-taking. We see a fruitful path for future research to identify situations in which attention and effort costs affect other revealed preferences and characteristics (e.g. ambiguity attitudes, attitudes towards losses, time preferences, or preferences for the outcomes of others.).

We arrive at similar results as Keinan and Bereby-Meyer (2012), who report marked differences between active and passive risk-taking in self-reported behavior, despite employing a different methodology. Taken together, our research reveals that attention costs – while being a defining feature across many economic decisions of interest - have the potential to increase the incidence of mode-of-choice effects in decisions about risk.

5 Conclusion

We explore whether different modes of choice, i.e. making choices actively or passively, have systematic effects on risk-taking. Extending established methods of eliciting risk preferences in economic experiments, we developed the DLAT as a stepwise adjustment task, which allows us to study active and passive risk-taking while accounting for starting-point effects. The DLAT and its implementation facilitate different parametrizations of the risky prospects and allow for easy adaptations of the time span decisions can be taken in.

We have conducted two experiments to systematically test for mode-of-choice effects with and without attention costs. While Experiment 1 is a standard laboratory experiment, in which participants face hardly any costs of paying attention to changes in risk, Experiment 2 is an online-experiment, which introduces significant attention and effort costs by extending the decision period to 10 days. Thereby, Experiment 2 takes a big step away from the tightly-controlled laboratory environment and moves our investigation into the direction of more realistic and naturally occurring decision situations.

In both experiments, the initial lottery endowment has a pull on participants' subsequent choices. This is in line with prior findings on status-quo bias (e.g., Samuelson and Zeckhauser, 1988). However, our main results with respect to the mode of choice hinge on the presence of attention and effort costs. While we do not find statistically significant mode-of-choice effects in the laboratory study (Experiment 1), they are very pronounced in the longer online experiment (Experiment 2). These core findings survive several robustness checks, among them changes in lottery parameters, controls for individual characteristics, and subdividing our

sample in more and less attentive participants. The patterns we observe in Experiment 2 can also not be explained by a lack of engagement with the experiment.

The fact that mode-of-choice effects only appear in the presence of attention costs hints at one potential explanation of why abstract experimental procedures that are not taking account of important contextual features can have low predictive power towards real-world behavior (e.g. Szrek et al., 2012, Hellerstein et al., 2013, Menapace et al., 2016, Massin et al., 2018). In many experimental settings, effort, attention, and opportunity costs are neglectable, because participants have already made an effort to show up and have blocked the time required for taking part in the experiment. Outside of tightly controlled laboratory experiments, each of these factors is a feature of the decision environment and, as we demonstrate, can have strong effects on risk-taking behavior, depending on the mode of choice. Our results provide some hints for the efforts of finding a risk-elicitation method that has high predictive power beyond the lab. In particular, such methods would not only need to consider contextual features but would also need to pay attention to the decision mode and their interaction with attention costs.

Interestingly, in Experiment 2 we observe that the mode-of-choice effects appear to mitigate and even completely offset status quo effects in decisions under risk. While initial lottery endowments affect final lottery choices, this occurs only in the active condition while there is little effect in the passive condition. If robust, this pattern suggests that manipulating the likelihood of people making their decisions in either mode of choice may have considerable effects on outcomes. In particular, it appears plausible that choice architectures can be adjusted to promote choices desired by the choice architect (cf. nudging techniques; Thaler and Sunstein, 2003). Reminders could be used to prevent passive decision-making by refocusing attention and bringing issues back to the top of the mental agenda. This can be desirable if the choice architect deems the expected outcome of active, rather than passive, choice more beneficial to the decision maker. In the context of medical check-ups, general practitioners or insurance companies may deliberately increase the frequency with which they remind patients to book an appointment. Increasing the frequency of reminders may reduce passive risk-taking that results from inattention and thereby increase the number of medical check-ups and the early detection of dangerous medical conditions. While reminders can backfire and lead to avoidance behavior in the context of charitable donations (Damgaard and Gravert, 2018), they have already proven to be effective in increasing the frequency of medical check-ups (Altman and Traxler, 2014). Similarly, adjusting the frequency of reminders could potentially be used to increase engagement in early financial planning for retirement.

In some other instances, it may depend on the perspective of a decision maker whether he perceives a given decision environment as one where he takes risks actively or passively. For instance, some decision makers may treat participation in the stock market as a succession of active (trading) decisions while other decision makers may perceive it as a rather passive exercise in benefitting from average long-term growth. Situations in which the perception of risks as passive or active may depend on the perspective of the decision maker, allow choice architects to shift the perspective of the decision maker. For example, a choice architect might want to keep people from making frequent, active, but potentially harmful investment decisions when it comes to decisions about retirement saving (cf. myopic loss aversion: Benartzi and Thaler, 1995; Gneezy and Potters, 1997). It may be possible to nudge participants towards perceiving an investment situation as passive risk-taking, by limiting the frequency with which brokers send automatic portfolio balance reports to their clients. This would ultimately allow clients to “forget” about their asset holdings and promote a “hands-off approach”. Relatedly, not being informed frequently may prevent individual investors from overtrading, which has been linked to poorer financial performance (e.g., Barber et al., 2009). Combined with the observation that initial risk exposure seems to distort the implementation of actual risk preferences less in passive decision contexts than in active ones, these measures may result in a better fit between people’s risk preferences and investment outcomes.

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Appendix A: Lottery construction

The lotteries are constructed similarly to those used by Binswanger (1980, 1981) and Eckel and Grossman (2002) but are presented differently. Each lottery has two possible outcomes $O \in \{L, H\}$, which can occur with equal probability: $P(L) = P(H) = \frac{1}{2}$. We denote payoffs associated with the outcome as $\pi_O(s)$, where $s \in \{0, 1, 2, \dots, 10\}$ denotes how risky the lottery is in terms of discrete adjustment steps from the safest ($s = 0$) to the riskiest ($s = 10$) lottery. The safest lotteries always yield the same, sure payoff π_S for both outcomes: $\pi_L(0) = \pi_H(0) = \pi_S$. The riskiest lotteries always yield zero for outcome L , i.e. $\pi_L(10) = 0$. Intermediate lotteries for $s \geq 1$ are constructed by subtracting $s * a$ from π_S and adding $s * f * a$ to π_S , where $f \in \{1.5, 3\}$ is a constant step factor and $a > 0$ is the amount the payoff of outcome L is reduced by in each adjustment step. We have:

$$\pi_L(s) = \pi_S - sa$$

$$\pi_H(s) = \pi_S + sfa$$

Setting $\pi_L(10) = 0$ then also implies $\pi_S = 10a$ which allows us to write these as:

$$\pi_L(s) = (10 - s)a$$

$$\pi_H(s) = (10 + sf)a$$

In each step, reducing $\pi_L(s)$ by a , but increasing $\pi_H(s)$ by $f * a$ ensures that more risky lotteries have both higher variance as well as a higher expected value. The lotteries used by Eckel and Grossman (2002) introduce this pattern and, using our notation, are characterized by a step factor of $f = 2$.

Defining the lotteries in this way, fixing the number of adjustment steps s to 10, and setting $\pi_L(10) = 0$ for all lotteries allows us to characterize each set of lotteries the participant can choose from by the step factor f and the step-wise adjustment value a . In total, participants take part in ten adjustment rounds. In each round, they adjust their starting lottery in discrete steps to choose their preferred level of risk. Five of these rounds are characterized by $f = 1.5$ and $a \in \{0.2, 0.4, 0.6, 0.8, 1\}$, while the other five are characterized by $f = 3$ and $a \in \{0.1, 0.3, 0.5, 0.7, 0.9\}$. We refer to these two blocks of rounds by F1.5 and F3, respectively. All lotteries are shown in Tables A1 and A2.

We hold the step factor f constant within each block of five rounds to keep the choices comparable and be able to average the level of risk taken over these rounds. Note that choices

of lotteries from rounds characterized by the same step factor f imply the same coefficient ranges of Constant Relative Risk Aversion (CRRA), because they are unaffected by the choice of a .

Table A1: Lotteries with factor 1.5

Designation	F1.5-Lowest		F1.5-Low		F1.5-Medium		F1.5-Higher		F1.5-Highest	
Order in block	3		5		1		4		2	
Step size a	0.2		0.4		0.6		0.8		1.0	
Outcomes O	L	H	L	H	L	H	L	H	L	H
Step s										
0	2	2	4	4	6	6	8	8	10	10
1	1.8	2.3	3.6	4.6	5.4	6.9	7.2	9.2	9	11.5
2	1.6	2.6	3.2	5.2	4.8	7.8	6.4	10.4	8	13
3	1.4	2.9	2.8	5.8	4.2	8.7	5.6	11.6	7	14.5
4	1.2	3.2	2.4	6.4	3.6	9.6	4.8	12.8	6	16
5	1	3.5	2	7	3	10.5	4	14	5	17.5
6	0.8	3.8	1.6	7.6	2.4	11.4	3.2	15.2	4	19
7	0.6	4.1	1.2	8.2	1.8	12.3	2.4	16.4	3	20.5
8	0.4	4.4	0.8	8.8	1.2	13.2	1.6	17.6	2	22
9	0.2	4.7	0.4	9.4	0.6	14.1	0.8	18.8	1	23.5
10	0	5	0	10	0	15	0	20	0	25

Note: The table shows the five lotteries with factor 1.5. We refer to them by their stake sizes implied by the choice of the adjustment step size a . For each lottery, we present the Euro payoffs associated with each outcome for each of the 10 adjustments steps. The table also shows the fixed order of the five lotteries within the block.

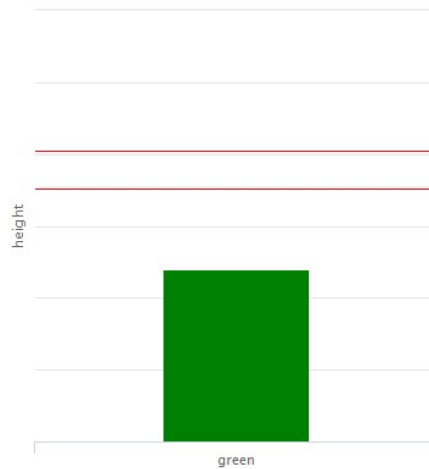
Table A2: Lotteries with factor 3

Designation	F3-Lowest		F3-Lower		F3-Medium		F3-Higher		F3-Highest	
Order in block	3		5		1		4		2	
Step size a	0.1		0.3		0.5		0.7		0.9	
Outcomes O	L	H	L	H	L	H	L	H	L	H
Step s										
0	1	1	3	3	5	5	7	7	9	9
1	0.9	1.3	2.7	3.9	4.5	6.5	6.3	9.1	8.1	11.7
2	0.8	1.6	2.4	4.8	4	8	5.6	11.2	7.2	14.4
3	0.7	1.9	2.1	5.7	3.5	9.5	4.9	13.3	6.3	17.1
4	0.6	2.2	1.8	6.6	3	11	4.2	15.4	5.4	19.8
5	0.5	2.5	1.5	7.5	2.5	12.5	3.5	17.5	4.5	22.5
6	0.4	2.8	1.2	8.4	2	14	2.8	19.6	3.6	25.2
7	0.3	3.1	0.9	9.3	1.5	15.5	2.1	21.7	2.7	27.9
8	0.2	3.4	0.6	10.2	1	17	1.4	23.8	1.8	30.6
9	0.1	3.7	0.3	11.1	0.5	18.5	0.7	25.9	0.9	33.3
10	0	4	0	12	0	20	0	28	0	36

Note: The table shows the five lotteries with factor 3. We refer to them by their stake sizes implied by the choice of the adjustment step size a . For each lottery, we present the Euro payoffs associated with each outcome for each of the 10 adjustments steps. The table also shows the fixed order of the five lotteries within the block.

Appendix B: Task Presentation

Figure B1: Trial Task



Note: This figure shows the trial chart used to familiarize participants with the user interface. The starting position for the SAFE treatments is shown. Red lines mark the target height range for the green bar.

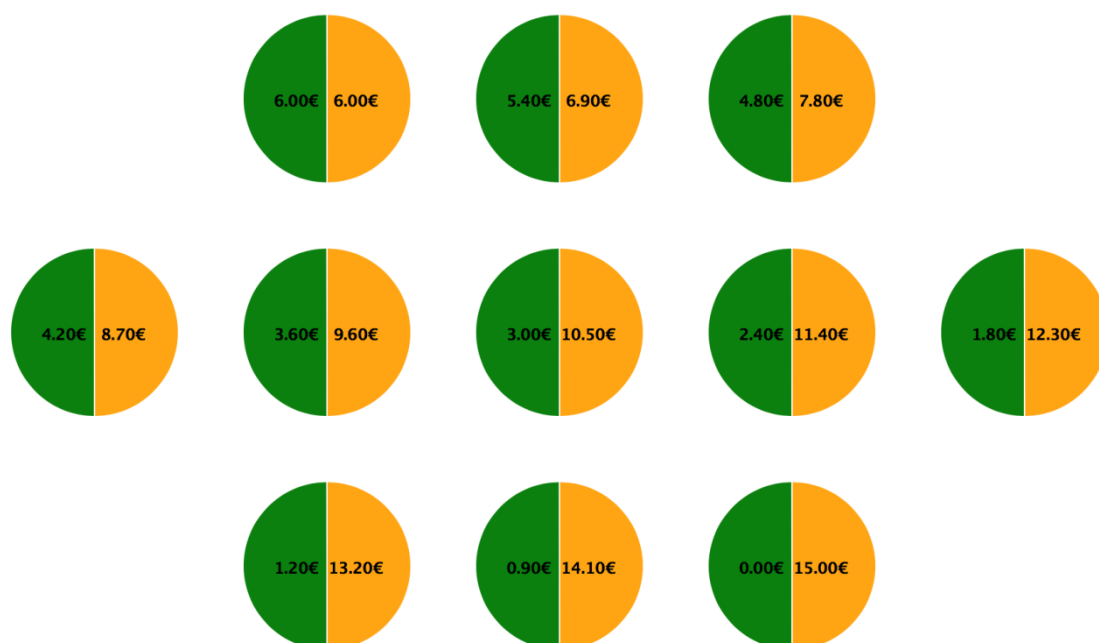
On the trial screen, a single bar chart with a single, green bar was shown (Figure B1). The y-axis was labeled “height”, but did not show any numbers. Depending on the treatment conditions, the green bar would initially either reach from the bottom to the middle of the chart (SAFE) or to the very top (RISKY). Participants were asked to operate the button to adjust the height of the bar to end within a predetermined area marked by two horizontal red lines. Adjustments to the height of the bar were either carried out by repeatedly, actively clicking a button (ACTIVE) or by remaining inactive for several steps before stopping the automatic height adjustment process (PASSIVE). As in the main experiment adjustment steps were implemented every 5 seconds. In order to advance beyond the trial screen, participants had to complete this task successfully two times. The second trial showed a different target area. Target areas were identical for all participants of the same treatment condition.

Figure B2: Effect of a single adjustment



Note: The left panel shows an example lottery from the SAFE condition. Making one adjustment step (through action or inaction, depending on treatment), would result in the lottery shown in the right panel. The payoff associated with the green outcome is decreased by 0.60€, while the payoff for the yellow outcome is increased by 0.90€. Expected payoffs and variance are higher for the right resulting lottery, compared to the initial lottery.

Figure B3: Ordered Lottery Selection Task



Note: The second representation of lottery F1.5-Medium presents all possible payoff combinations on the same screen. Participants select their preferred payoff combination by clicking on the pie chart. The presentation format is similar to the one used by Deck et al. (2013).

Appendix C: Supplementary Analyses for Experiment 1

Supporting Evidence Results 1 and 2

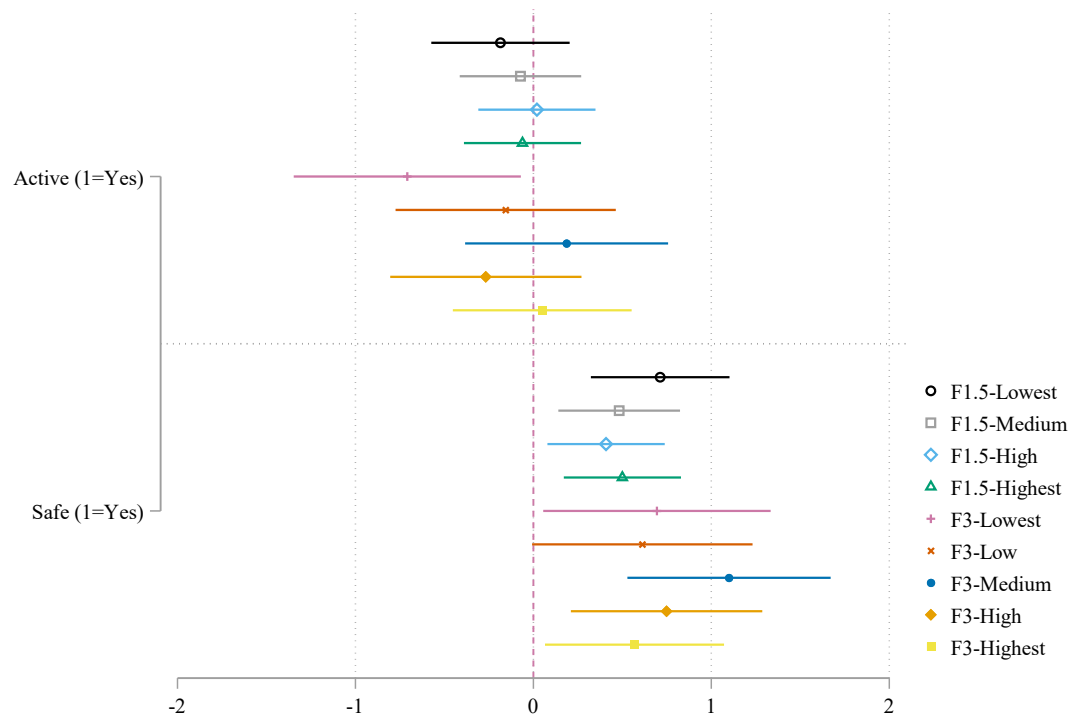
Table C1: Tobit Regression

	(1) Risk (r)	(2) Risk (Step)	(3) Risk (r)	(4) Risk (Step)
Active (1 = Yes)	0.046 (0.229)	0.386 (0.510)	0.095 (0.197)	0.557 (0.396)
Safe (1 = Yes)	0.555* (0.220)	1.670** (0.491)	0.758** (0.192)	2.237** (0.386)
Active#Safe (1 = Yes)	0.172 (0.314)	0.199 (0.700)	0.077 (0.272)	-0.151 (0.546)
Order	-0.050 (0.159)	-0.241 (0.353)	0.063 (0.140)	0.006 (0.281)
Female (1 = Female)			0.233 (0.156)	0.927** (0.312)
Math Grade (0-15)			-0.034 (0.024)	-0.071 (0.047)
EG – Risk Measure (0-10)			0.130** (0.022)	0.358** (0.044)
Risk Soep (0-10)			-0.046 (0.035)	-0.043 (0.069)
Risk Dentist (0-10)			0.006 (0.025)	0.074 (0.051)
Risk Smoking (0-10)			0.057 (0.034)	0.158* (0.068)
Economics Major (1=Yes)			-0.139 (0.160)	0.040 (0.321)
Disposable Income (Euro)			0.000 (0.000)	-0.000 (0.001)
Constant	1.420** (0.275)	5.233** (0.613)	0.553 (0.429)	1.883* (0.862)
Obs.	199	199	199	199
Pseudo R ²	0.028	0.029	0.126	0.137

Note: Standard errors are in parenthesis. *** / ** / * denote statistical significance at 0.1% / 1% / 5%.

Table C1 contains a set of Tobit regressions estimating the average treatment effect of our four treatment conditions on average levels of risk aversion measured by r (columns 1 and 3) or by the selected lottery (columns 2 and 4). The parameter estimates for the main treatment effects corroborate the results shown in the main text. Risk taking is affected by the initial lottery endowment, but not by the mode by which adjustments are made to this initial lottery. This also holds, when we control for a rich set of individual characteristics in columns (3) and (4). Importantly, our results are robust to controlling for the order in which lotteries were presented on the decision screen.

Figure C1: ATE by Lottery



Note: Coefficient plot for main treatment effects for each lottery decision. Outcome variable: Coefficient of relative risk aversion (r). Explanatory variables are treatment indicators for participants being randomized to the Active and Safe conditions. OLS regression.

Table C2: Panel Regression

	(1) Risk (r) Small	(2) Risk (r) Large	(3) Risk (r) All	(4) Risk (Step) Small	(5) Risk (Step) Large	(6) Risk (Step) All
Passive (1=Yes)	0.079 (0.196)	0.176 (0.415)	0.113 (0.218)	0.478 (0.673)	0.518 (0.700)	0.567 (0.602)
Safe (1=Yes)	0.717** (0.190)	1.259** (0.404)	0.834** (0.212)	2.543** (0.659)	2.878** (0.683)	2.809** (0.588)
Safe#Passive (1=Yes)	0.051 (0.269)	0.364 (0.571)	0.106 (0.301)	0.089 (0.931)	0.417 (0.965)	0.193 (0.831)
Order	0.021 (0.139)	0.103 (0.294)	0.046 (0.155)	-0.068 (0.479)	0.210 (0.497)	0.034 (0.428)
Step Size (Euro)	-0.234** (0.085)	-0.026 (0.091)	0.071 (0.052)	0.052 (0.272)	0.486** (0.138)	-0.178 (0.126)
Female (1 = Female)	0.178 (0.154)	0.659* (0.325)	0.271 (0.172)	0.825 (0.534)	1.599** (0.550)	1.268** (0.475)
Math Grade (0-15)	-0.016 (0.023)	-0.093 (0.049)	-0.038 (0.026)	-0.038 (0.080)	-0.165* (0.083)	-0.106 (0.071)
EG-Risk Measure (0-10)	0.136** (0.022)	0.316** (0.047)	0.154** (0.024)	0.497** (0.076)	0.612** (0.079)	0.554** (0.067)
Risk Soep (0-10)	0.009 (0.034)	-0.100 (0.074)	-0.044 (0.038)	0.052 (0.119)	-0.128 (0.124)	-0.053 (0.106)
Risk Dentist (0-10)	0.020 (0.025)	0.023 (0.053)	0.010 (0.028)	0.111 (0.087)	0.077 (0.090)	0.100 (0.078)
Risk Smoking (0-10)	0.039 (0.034)	0.121 (0.071)	0.059 (0.037)	0.162 (0.116)	0.249* (0.119)	0.196 (0.103)
Economics Major (1=Yes)	0.018 (0.158)	-0.415 (0.336)	-0.142 (0.177)	0.248 (0.546)	-0.419 (0.568)	-0.062 (0.488)
Disposable Income (Euros)	0.000 (0.000)	-0.000 (0.001)	0.000 (0.000)	0.000 (0.001)	-0.001 (0.001)	-0.000 (0.001)
Constant	0.030 (0.435)	-0.859 (0.927)	0.225 (0.479)	1.059 (1.500)	-1.496 (1.557)	0.334 (1.327)
Obs.	796	995	1791	796	995	1791

Note: Tobit panel regression accounting for potential censoring from below or above. Dependent variable is the coefficient of relative risk aversion in models (1)-(3) and the number of adjustment steps in models (4)-(6). Models 1 (4) and 2 (5), show estimation results for lotteries 1-5 (Factor 1.5) and 6-10 (Factor 3) only. Models 3 and 6 present results based on all lotteries. Standard errors are in parenthesis. *** / ** / * denote statistical significance at 0.1% / 1% / 5%.

Supporting Evidence Lottery Retention Rates

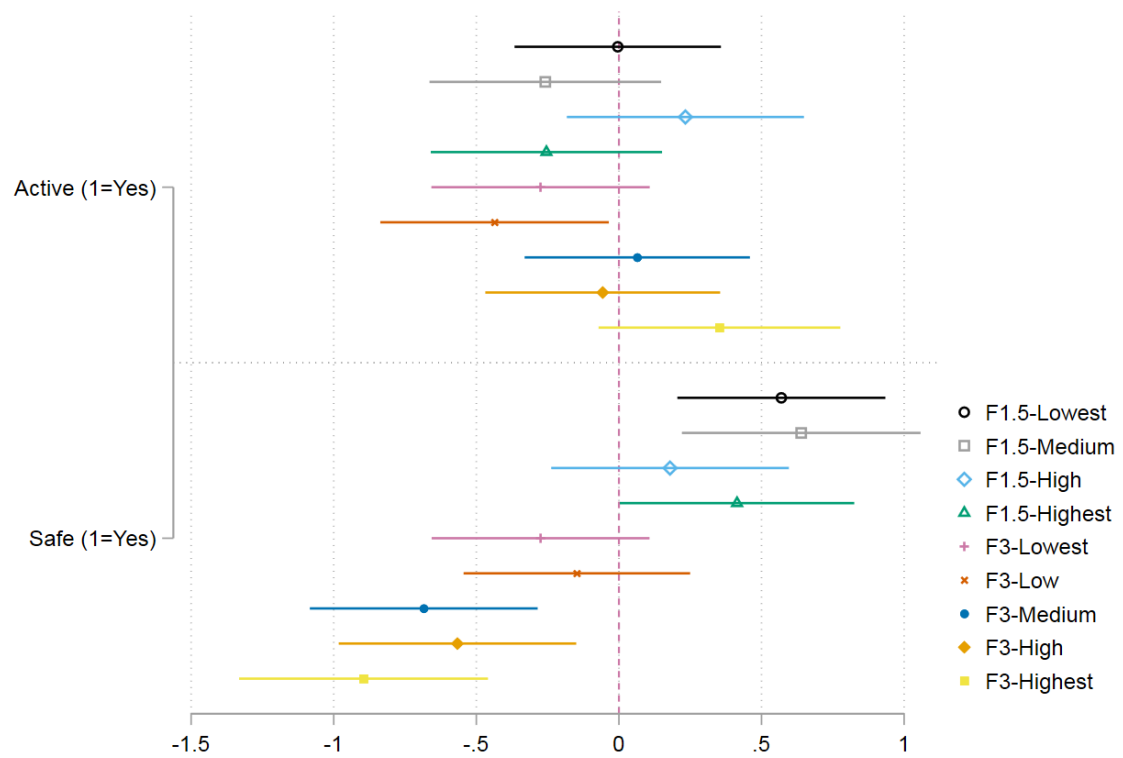
Table C3: OLS Regression

	(1) Average of initial (All)	(2) Average of initial (Small)	(3) Average of initial (Large)
Passive (1=Yes)	-0.022 (0.051)	-0.039 (0.054)	-0.010 (0.059)
Safe (1=Yes)	-0.077 (0.050)	0.085 (0.059)	-0.207** (0.055)
Safe#Passive (1=Yes)	0.093 (0.070)	0.101 (0.081)	0.087 (0.077)
Order	-0.005 (0.035)	0.067 (0.040)	-0.063 (0.039)
Female (1 = Female)	-0.040 (0.041)	-0.011 (0.049)	-0.063 (0.044)
Math Grade (0-15)	-0.001 (0.005)	-0.000 (0.006)	-0.001 (0.006)
EG-Risk Measure (0-10)	-0.014* (0.007)	-0.007 (0.008)	-0.019** (0.007)
Risk Soep (0-10)	0.001 (0.009)	0.006 (0.012)	-0.002 (0.010)
Risk Dentist (0-10)	-0.004 (0.006)	-0.003 (0.008)	-0.004 (0.007)
Risk Smoking (0-10)	-0.002 (0.009)	-0.009 (0.011)	0.003 (0.009)
Economics Major (1=Yes)	-0.009 (0.041)	0.001 (0.046)	-0.016 (0.044)
Disposable Income (Euros)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Constant	0.350** (0.133)	0.089 (0.153)	0.559*** (0.139)
Obs.	199	199	199
R-squared	0.077	0.102	0.167

Notes: Standard errors are in parenthesis. *** / ** / * denote statistical significance at 0.1% / 1% / 5%.

Table C3 contains a set of OLS regressions estimating the average treatment effect of our four treatment conditions on the average rate at which participants retain their initial lottery. Column 1 looks at all choices, column 2 at choices in lotteries with a small adjustment factor and column 3 at choices with a large adjustment factor. The parameter estimates for the main treatment effects corroborate the results shown in the main text. Risk taking is affected by the initial lottery endowment, but not by the mode by which adjustments are made to this initial lottery. This continues to hold after controlling for a rich set of individual characteristics and the order in which lotteries were presented on the decision screen.

Figure C2: Propensity to retain the initial lottery



Note: Coefficient plot for main treatment effects for each lottery decision in a Probit model. Outcome variable is the propensity to retain the initial lottery. Explanatory variables are treatment indicators for participants being randomized to the Active and Safe conditions.

Appendix D: Supplementary Analyses for Experiment 2

Table D1: Regression results

	(1) Risk (Step)	(2) Risk (r)	(3) Risk (Step)	(4) Risk (r)	(5) Risk (Step)	(6) Risk (r)
Passive (1 = Yes)	5.124** (1.097)	0.891** (0.252)	4.469** (1.348)	0.799* (0.314)	3.542** (1.091)	0.973** (0.283)
Safe (1 = Yes)	10.947** (1.250)	2.383** (0.259)	5.777** (1.563)	1.242** (0.360)	1.862 (1.271)	0.593 (0.337)
Passive#Safe	-10.284** (1.621)	-2.315** (0.355)	-6.697** (2.070)	-1.446** (0.477)	-5.799** (1.679)	-1.742** (0.439)
Age (Years)			0.033 (0.084)	0.000 (0.020)	0.023 (0.062)	0.014 (0.016)
Male (1 = Male)			-1.880 (1.057)	-0.511* (0.250)	-1.764* (0.820)	-0.446* (0.215)
Economics Major (1=Yes)			-0.588 (1.336)	0.036 (0.315)	0.175 (1.053)	0.259 (0.274)
Risk Smoking (0-10)			-0.308 (0.242)	-0.061 (0.059)	0.001 (0.206)	0.021 (0.054)
Risk Soep (0-10)			-0.643* (0.254)	-0.122* (0.058)	-0.337 (0.214)	-0.126* (0.056)
Math Grade (0-15)			0.021 (0.158)	0.003 (0.037)	0.022 (0.126)	-0.005 (0.033)
Risk Dentist (0-10)			0.198 (0.213)	0.048 (0.050)	-0.050 (0.174)	-0.066 (0.045)
Disposable Income (Euros)			-0.001 (0.003)	0.000 (0.001)	0.001 (0.002)	0.000 (0.001)
Constant	0.475 (0.793)	0.182 (0.179)	4.072 (2.777)	0.837 (0.658)	6.091** (2.177)	1.292* (0.571)
Obs.	212	212	115	115	86	86
Pseudo R ²	0.084	0.100	0.071	0.078	0.056	0.106

Notes: Standard errors are in parenthesis. *** / ** / * denote statistical significance at 0.1% / 1% / 5%.

Table D1 contains a set of Tobit regressions estimating the average treatment effect of our four treatment conditions on average levels of risk aversion measured by r (columns 2, 4, 6) or by the selected lottery (columns 1, 3, and 5). Columns 3-6 test the robustness of our results to controlling for several individual characteristics. As these are only available for those participants who fill the final questionnaire, columns 3 and 4 correspond to the evidence shown in Panel B of and columns 5 and 6 correspond to the evidence shown in Panel C of Figure 7.

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Christian König-Kersting, Johannes Lohse, Anna Louisa Merkel

Active and Passive Risk-Taking

Abstract

Risk-taking may depend on whether risks result from an action (active risk-taking) or from not taking action (passive risk-taking). We develop a new experimental risk-elicitation procedure, the Dynamic Lottery Adjustment Task, and employ it across two separate experiments to study the size and direction of potential mode-of-choice effects (i.e. differences in risk-taking between active and passive decision modes). While our tightly controlled lab study provides little evidence for such effects, we find substantial evidence for mode-of-choice effects when decisions are spread out over 10 days and attention costs are a key feature of the online choice environment we use.

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