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Does boredom affect economic risk preferences?

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Abstract

Previous literature and conventional wisdom have led researchers to believe that boredom increases economic risk taking, but the evidence in support of this conclusion is limited and has important shortcomings. In four experiments (including more than 1,300 subjects), we systematically studied the effects of boredom on economic risk preferences. Across different risk elicitation tasks, boredom inductions, incentive schemes, subject pools, and using both reduced form and structural analyses, we consistently failed to find an effect of boredom on risky decisions. Our results disprove that boredom leads to even small increments in risk taking in one-shot elicitation tasks, and small to medium increases in multiple-choice elicitations. These findings question an important established belief, contribute to better understand the consequences of boredom, and have substantive implications for experiments on economic decision making.

Keywords: risk preferences, boredom, emotions, experiments.

1 Introduction

In 2018, a Russian man stole an armored vehicle from a driving school, destroyed a car with it and crashed the vehicle into a liquor store. When questioned on his motives by local reporters, the man concluded that he was "just bored" (Kiryukhina & Coleman 2018).

The emotion of boredom can be defined as "the aversive experience of wanting, but being unable, to engage in satisfying activity" (Eastwood et al. 2012). The above anecdote captures a widespread intuition about boredom: it prompts the pursuit of stimulation and novelty. In

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line with this intuition, some research has found a significant relationship between boredom and stimulation-seeking behaviors such as alcohol consumption, impulsive eating, risky driving, or pathological gambling (Dahlen et al. 2005; Havermans et al. 2015; Biolcati et al. 2018; Mercer & Eastwood 2010). This research has focused mostly on the effects of trait boredom, the personal tendency to experience frequent and intense boredom; as opposed to state boredom, the actual experience of boredom in a particular moment.

Following this evidence and intuition about boredom, a number of researchers have assumed that this emotion, mostly characterized in terms of state boredom, translates into a more risk seeking behavior in economic decisions (see, e.g., Harrison et al. 2009; Crosetto & Filippin 2013; Ert & Haruvy 2017; Erkal et al. 2018; Hvide et al. 2019). This belief has been particularly prevalent in experiments on economic decision making, where the effects of boredom are a potential concern for the experimenters. However, the evidence on the influence of this emotion (either trait or state boredom) on economic risk taking is very limited. Moreover, most of the aforementioned behaviors are markedly different from economic decisions under risk, understood as decisions with clearly defined economic consequences and probabilities. Even pathological gambling, a behavior that resembles the domain of economic choices under risk, has a number of characteristics (including its pathological component) that separate it from typical economic choices.

In this paper, we present four experiments (including more than 1,300 subjects) in which we systematically studied the effects of state boredom on economic risk preferences. Across the experiments, we failed to find any significant effect of boredom on risky choices. We used different decision-making tasks, different boredom inductions, different incentive schemes, different subject pools, and different statistical techniques, but we consistently found risk preferences to be unaffected by boredom. We also present additional analyses of the relationship between trait boredom and risk taking that yield similar results.

In our first three experiments, we analyzed the effects of three previously validated boredom inductions on risk preferences as captured by Gneezy and Potters risky investment game (Gneezy & Potters 1997). Across these experiments, we varied the subject pool (using Amazon's Mechanical Turk and Prolific) and the incentives associated with risk taking (real vs. hypothetical decisions). In all three experiments, we found significant differences in self-reported levels of boredom, but no effect of boredom on risk taking. In our fourth experiment, a sample of undergraduate students was presented with either a boring or a neutral video before completing a risk preference elicitation task developed by Andersen et al. (2008) and based on multiple-price lists (Holt & Laury 2002). For this experiment, we analyzed the data by structurally estimating a random-parameter stochastic choice model, following Apesteguia and Ballester (2018). Again, we found significant and large differences in boredom across conditions, but we failed to find any effects of boredom on risk aversion.

Across our studies, we mainly tested the presence of boredom-induced changes in risk taking using two-tailed t-tests and a null hypothesis of a zero effect of boredom on risk preferences. However, for completeness, we also considered so-called inferiority tests (Lakens et al. 2018). That is, using one-tailed t-tests, we also considered whether our results allow us to reject the presence of small (or small to medium) increases in risk taking following our boredom inductions. Using inferiority tests, we can reject the presence of even small (i.e., Cohen's d = 0.2) boredom-induced increases in risk taking when measured through a one-shot elicitation task (Studies 1 to 3), and small to moderate (i.e., Cohen's d = 0.35) increases in risk taking when measured through a multiple-choice elicitation (Study 4).

Apart from our main analyses of state boredom, in our first two experiments we included a measurement of trait boredom, showing that boredom proneness is also unrelated to risky economic decisions.

Our results contrast with a body of work in psychology that stresses the relationship between boredom and novelty or stimulation seeking in non-economic domains. A number of studies have considered this relationship at the trait level, finding that individuals prone to experiencing boredom are also more likely to engage in activities such as alcohol consumption and risky driving (Dahlen et al. 2005; Biolcati et al. 2018). Aside from this correlational evidence at the trait level, boredom as an emotional state has also been shown to cause an increased demand for stimulation. For instance, Havermans et al. (2015) showed an increased consumption of candy when individuals were exposed to a boring video. Moreover, consumers seem to be aware of this increased demand for stimulation, as they increase their willingness to pay for entertainment when expecting to get bored (Mas & Wittmann 2017). The stimulation seeking component of boredom has even been shown to lead to the pursuit of negative forms of stimulation (Bench & Lench 2019). For example, as much as 67% of males and 25% of females participating in a study preferred administering electric shocks to themselves than doing nothing for 15 minutes (Wilson et al. 2014). Further research shows that the use of electric shocks as stimulation is a unique feature of boredom, as it does not occur when other negative emotions are induced (Nederkoorn et al. 2016).

There are, however, several important factors that separate these results from the domain of economic decision making under risk. Although stimulation seeking is certainly an aspect of some risky economic decisions, it is clearly not the only one. There is a notoriously large number of economic theories of decision making under risk (see Camerer 1995; Starmer 2000; Rieskamp et al. 2006; Birnbaum 2008, for reviews), which propose a plethora of factors that drive risky decisions, including the marginal utility of wealth, probability weighting, reference points, anticipated regret and disappointment, attention, or preference imprecision, among others. It is difficult to know how boredom will interact with all these factors in shaping risk preferences. Overall, we see no unequivocal reason why boredom would lead to more (and not less) risky economic decisions.

Furthermore, the idea that boredom increases risk taking through a drive for stimulation assumes that the risk taking involved is stimulating. While some forms of risk are clearly arousing, economic decisions under risk have often been characterized as relatively affect poor (Rottenstreich & Hsee 2001; Pachur et al. 2014; Suter et al. 2015). Especially in experimental contexts, where risky choices typically involve low monetary outcomes, known numerical probabilities, and repetition of very similar scenarios, the stimulating power of risky options is very limited.

Finally, while the literature on boredom and economic decisions is almost non-existent, there are four recent papers that analyze the relationship between boredom and economic risk preferences (Bench et al. 2021; Kılıç et al. 2020; Miao et al. 2020; Yakobi & Danckert 2021). In line with our main results, in an extensive analysis of the relationship between trait boredom and risk taking, Yakobi and Danckert (2021) show that trait boredom does not predict economic risk taking. These authors present 4 studies (amounting to over 1,300 subjects) in which trait boredom fails to predict behavior in the Balloon Analogue Risk Task (BART), in the Iowa Gambling Task (IGT), and in a binary-choice decision making paradigm. Importantly, while the authors do not manipulate boredom, they collect a measure of state boredom at the end of each of these risk elicitation tasks (in Study 1 a pre-task measure of state boredom is also collected). As in our main results, these measures of state boredom display a non-significant relationship with economic risk taking. Despite the lack of a significant association between trait or state boredom and risk taking, Yakobi and Danckert (2021) find that boredom-prone individuals are more likely to randomize across alternatives when presented with a choice under risk. Past work on trait boredom and risk taking, the authors argue, can be partially explained by the association between boredom and choice variability.

The other three papers (Bench et al. 2021; Kılıç et al. 2020; Miao et al. 2020) study state boredom and conclude that it reduces risk aversion. In fact, these papers present some considerably large effect sizes, especially when compared to the effects of other emotions on economic risk taking. For instance, a recent meta-analysis suggests that specific emotional states produce only small changes in risk aversion (average Hedges' g = 0.083, Marini 2022). As a comparison, the effect size of boredom on risk taking presented in Bench et al. (2021) (Hedges' g = 0.77) is approximately 10 times larger.

Although these papers investigating the effect of boredom on risk taking make interesting contributions, they also have important limitations. First, some of the boredom manipulations employed have not been previously used in the literature and raise the concern that boredom might not be the only thing affected by them. For instance, Kılıç et al. (2020) induced boredom by facing people repeatedly with the same type of risky decisions, which might also affect people's choices due to factors such as decreased attention, increased noise, or changes in decision strategies. Second, some of the risk-preference elicitation tasks used in these studies have been said to reflect ambiguity rather than risk preferences, such as the BART in Bench et al. (2021) or the compounded lotteries in one of the studies in Miao et al. (2020) (see Halevy 2007; Groot & Thurik 2018; Dean & Ortoleva 2019; Yakobi & Danckert 2021). The BART has been shown to correlate with some risk-taking behaviors outside the lab (Lejuez et al. 2002, 2003), capturing relevant aspects of behavior under risk and uncertainty in a broad sense. However, precise probabilistic information – a fundamental feature of economic decisions under risk — is not provided in this task. This puts the decisions made in the BART in the realm of ambiguity (or Knightian uncertainty), which could lead to a different relationship with boredom. Third, the studies presented in these papers have modest sample sizes. Focusing on the experiments that manipulated boredom and analyzed its impact on risk preferences, we find an average sample size of approximately 140 subjects. Our studies have an average sample size of over 340 subjects. Fourth, as presented in Yakobi and Danckert (2021), a significant proportion of past findings linking boredom and economic risk-taking can be explained by an increase in choice variability (i.e., noise) following a boredom induction. By using structural estimation methods (Study 4), we can separate the effect of boredom on economic risk preferences from its effect on choice variability.

Overall, the four experiments we present here avoid the limitations explained above and provide a more systematic and powerful test than previous research. Our results cast serious doubts on the alleged relationship between boredom and economic risk taking, at least as it applies to experimental settings.

The rest of the paper is organized as follows. In Sections 2 to 5, we present the design, procedures and results of each of our 4 experiments. In Section 6, we analyze the relationship between trait boredom and economic risk preferences. In Section 7, we discuss the implications and limitations of our results.

2 Study 1

2.1 Subjects

In our first study, we analyzed the relationship between boredom and risk preferences using a sample of 201 American individuals recruited via Mechanical Turk (MTurk.com). A total of 11 responses were omitted due to failing an attention check or answering "no" to the question "In your honest opinion, should we use your data?" Hence, our final sample consisted of 190 individuals (average age = 36.1, 43% female). The experiment took an average time of 9.4 minutes to complete, and the average final payment was $1.1.

2.2 Design and procedures

The experiment consisted of four main elements: (1) a high or a low-boredom induction, creating two randomly assigned conditions between subjects ($N_{\text{High Boredom}} = 91$, $N_{\text{Low Boredom}} = 99$); (2) a risk-preference elicitation task; (3) a manipulation check measuring the self-reported intensity of several emotional states; and (4) an additional questionnaire collecting various individual characteristics. The subjects assigned to the high-boredom condition were asked to complete a 1-back task. Markey et al. (2014) showed that this task induces

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high levels of boredom, especially when used in online experiments. In this task, subjects were presented with a random digit (from 0 to 9) for 3 seconds. After this time, a new digit was drawn from the same set and presented during the same amount of time. The subjects were instructed to click on a tab when the digit that appeared was the same as the previous digit. As in Markey et al. (2014), we programmed the task to ensure that this occurred one third of the time. This procedure was repeated for a total of 5 minutes. The subjects assigned to the low-boredom condition saw a 5-minute video extract from BBC's documentary series "Planet Earth", a neutral control task previously validated in boredom-inducing experiments (Markey et al. 2014).

Then, the subjects completed the Gneezy and Potters (1997) risk-preference elicitation task. In this task, the subjects were given a dollar and decided on the fraction of this dollar that they wanted to invest in a lottery yielding 2.5 times the amount invested with 50% probability and 0 otherwise. Hence, the final payment received for the task could range from 0 to \$2.5. To ensure that the subjects were not answering randomly, they had to respond by providing two numbers – the number of cents invested and saved – adding up to 100. After this task, we measured feelings of boredom, anger, fear, sadness, disgust, anticipation, joy, surprise, and trust using Likert scales from 0 to 8; we also measured the subjects' general affect in terms of arousal and valence levels using sliders. Then the subjects completed the Short Boredom Proneness Scale (Struk et al. 2017), and they provided demographic information on age, gender, ethnicity, and education. Appendix C provides a detailed description of the demographic composition of the samples in all our studies.

2.3 Results

Subjects assigned to the high-boredom condition reported a higher level of boredom than those assigned to the low-boredom condition ($M_{\text{High Boredom}} = 1.95$, $SD_{\text{High Boredom}} = 2.24$, $M_{\text{Low Boredom}} = 1.19$, $SD_{\text{Low Boredom}} = 1.86$, $t(175.61) = 2.51$, $p = 0.013$, $d = 0.37$). This difference in boredom across conditions remains significant when adjusting (in a linear regression) for arousal, valence, and the other eight emotions we measured ($\beta = 0.46$, $t(178) = 2.01$, $p = 0.046$, $d = 0.30$). Additional analyses of the effects of boredom on each specific emotion (for all our studies) are included in Appendix D. Although the difference in boredom across groups is not too large, this is likely due to the experimental design. In order to ensure that the subjects faced the risky choice right after the boredom induction, we postponed the collection of self-reported emotional states to the end of the study. These small differences, therefore, capture the attenuating effects of the risk elicitation task on emotional states, and the time-lag between the boredom manipulation and its measurement. Despite these attenuating effects, differences in boredom across conditions are significant.

Next, we turn to the analysis of the effect of boredom on our risk elicitation task. Boredom did not significantly affect risk preferences as measured by the Gneezy and Potters

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(1997) task. On average, those assigned to the low-boredom condition invested 55.3 cents, 4.5 cents more than those in the high boredom condition, a difference that is not significantly different from zero ($M_{\text{High Boredom}} = 50.8$, $SD_{\text{High Boredom}} = 38.36$, $M_{\text{Low Boredom}} = 55.3$, $SD_{\text{Low Boredom}} = 37.68$, $t(186.05) = -0.81$, $p = 0.416$, $d = -0.12$). Using inferiority tests (Lakens et al. 2018), we can reject the hypothesis that boredom leads to small increments in risk taking (i.e., increments of a Cohen's d of 0.2; $t(186.05) = -2.19$, $p = 0.015$). Controlling in a linear regression for boredom proneness and demographic characteristics (age, gender, ethnicity, and education) does not change our results (null test: $\beta = -4.947$, $t(180) = -0.882$, $p = 0.379$, $d = -0.13$; inferiority test: $t(180) = -2.23$, $p = 0.013$). Our measures of trait boredom suggest a similar (null) relationship between boredom proneness and economic risk preferences (see Section 6).

3 Study 2

In Study 1, our boredom manipulation yielded a modest (yet significant) increase in self-reported boredom. To ensure that our results are robust across established boredom manipulations, in our second study, we analyzed the effect of boredom on risk taking using a different boredom induction and a substantially increased sample size.

3.1 Subjects

For this study, we recruited 590 American subjects using Mechanical Turk (MTurk.com). We omitted 68 responses from individuals who failed an attention check, failed to complete our boredom manipulation or responded "No" to the question "In your honest opinion, should we use your data?" Hence, our final sample consists of 522 individuals (average age = 36.4, 55% female). This experiment took an average time of 9.8 minutes and the average payment was $1.2.

3.2 Design and procedures

As in our first study, the experiment consisted of the following four elements: (1) a high or low-boredom induction, creating two randomly assigned conditions ($N_{\text{High Boredom}} = 256$, $N_{\text{Low Boredom}} = 266$); (2) the Gneezy and Potter's risk elicitation task; (3) a manipulation check measuring emotional states; and (4) a survey collecting several individual characteristics. Our high-boredom induction consisted in copying 5 bibliographical references from the construction materials literature. Our low-boredom task consisted in copying only one bibliographical reference from the same literature. This boredom induction method has been extensively used in psychology (see Van Tilburg & Igou 2012, 2016; Moynihan et al. 2017).

After the boredom manipulation, the subjects faced the Gneezy and Potters risk elicitation task. As in our first experiment, the subjects had to decide on an amount, ranging

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from 0 to 100 cents, to be invested in a lottery yielding 2.5 times the amount invested with 50% probability and 0 otherwise, and this decision was incentivized. Then, the subjects reported their experienced levels of boredom, anger, fear, sadness, disgust, anxiety, desire, happiness, and relaxation using 1 to 7 Likert scales, and their general affect (arousal and valence) using sliders. They also completed a few filler questions (unrelated to the present paper) and provided additional information on boredom proneness, gender, age, education, and ethnicity. As in the previous study, boredom proneness was measured using the Short Boredom Proneness Scale (Struk et al. 2017).

3.3 Results

The subjects assigned to our high-boredom condition (copying 5 references) experienced significantly higher levels of boredom than those assigned to the neutral condition ($M_{\text{High Boredom}} = 3.64$, $SD_{\text{High Boredom}} = 1.95$, $M_{\text{Low Boredom}} = 2.88$, $SD_{\text{Low Boredom}} = 1.70$, $t(504.97) = 4.74$, $p < 0.001$, $d = 0.42$). This difference across conditions remains significant when adjusting (in a linear regression) for the effects of our manipulation on arousal, valence, and the other eight emotions we measured ($\beta = 0.71$, $t(510) = 4.79$, $p < 0.001$), $d = 0.42$). As in Study 1, to maximize the likelihood of finding a significant effect of boredom on risk taking, we positioned our manipulation check at the end of the study, which probably reduced the magnitude of the differences in boredom between conditions.

As in our first study, boredom had no effect on subjects' risk attitudes. On average those assigned to the low-boredom condition (copying 1 reference) invested 64.9 cents, 2 cents more than those in the high-boredom condition, which is a non-significant difference ($M_{\text{High Boredom}} = 62.89$, $SD_{\text{High Boredom}} = 32.37$, $M_{\text{Low Boredom}} = 64.89$, $SD_{\text{Low Boredom}} = 31.57$, $t(517.90) = -0.71$, $p = 0.476$, $d = -0.06$). Again, we can reject the hypothesis that our boredom condition induced even small increases in risk taking ($t(517.90) = -2.996$, $p = 0.001$). Including boredom proneness and the respondents' demographic characteristics (age, gender, ethnicity, and education) as additional explanatory variables in a linear regression does not change our results (null test: $\beta = -2.00$, $t(513) = -0.73$, $p = 0.468$, $d = -0.06$; inferiority test: $t(513) = -3.04$, $p = 0.001$). As in our first experiment, boredom proneness also displays a null relationship with risk taking (see Section 6).

4 Study 3

Our previous studies shared some important commonalities. Both of them used Amazon MTurk samples and incentivized risk elicitation tasks. In this third study, we extend our analyses using a different subject pool and a hypothetical risk-taking task. We also included an additional boredom induction. Moreover, as our previous manipulation checks suffered from the attenuating effects of the time-lag between boredom induction and measurement, here we measured state boredom right after our boredom manipulations.

4.1 Subjects

We recruited 496 British individuals using Prolific (Prolific.ac) but omitted the responses form 78 subjects that failed an attention check, failed a task fidelity screening (explained in the next section) or responded "No" to the question "In your honest opinion, should we use your data?" Hence, our final sample consisted of 418 individuals (average age = 36.7, 72% female). On average, the experiment lasted 8 minutes and the subjects received a fixed amount of £0.8.

4.2 Design and Procedures

This study consisted of five main elements: (1) one of three boredom manipulations, two intended to produce high boredom and one intended to produce low boredom, creating three randomly assigned conditions between subjects ($N_{\text{High Boredom Video}} = 121$, $N_{\text{High Boredom References}} = 137$, $N_{\text{Low Boredom}} = 160$); (2) a manipulation check measuring emotions; (3) the Gneezy and Potters risk elicitation task; (4) a task fidelity questionnaire; and (5) a survey collecting demographic information. Our two high-boredom manipulations consisted in a bibliographical copying task (as in Study 2) and a pre-validated video used in Markey et al. (2014). This 5-minute video portrayed an individual talking in a monotone way about his work at an office supply company. Past research on the psychology of boredom has distinguished two different subjective experiences of this emotion: (1) an apathetic state of disengagement and (2) an agitated state in which the individual tries (but is unable) to engage in satisfying tasks (Danckert 2013; Greenson 1953; Struk et al. 2017). To ensure that our results are not driven by a specific subjective experience of boredom, we included our two different high-boredom manipulations. As copying the references required active engagement from the subjects and watching the boring video did not, we expected the video to induce a more apathetic form of boredom than the references. This way, we did not only test different inductions of boredom but also different types of boredom in this experiment. As in our first study, the subjects assigned to the low-boredom condition saw a 5-minute extract from BBC's documentary series "Planet Earth".

After completing the corresponding high or low-boredom manipulation, subjects reported their experienced level of boredom, anger, fear, sadness, disgust, happiness, and surprise on 1 to 7 Likert scales, and also their general affect (arousal and valence) using sliders. To check whether our high-boredom manipulations induced different affective experiences of boredom, we included the high-arousal and low-arousal subscales of the Multidimensional Boredom Scale (Fahlman et al. 2013). In these subscales the subjects rated on 1 to 7 scales how much they agreed with a total of 11 statements regarding their current boredom experience. For example, the high-arousal scale included items such as "I feel agitated" or "I feel tense"; the low-arousal scale included items such as "I feel down" or "I feel empty". We made minor changes to the original scales and included an additional item to the low-arousal scale ("I feel apathetic").

The measurement of emotions was followed by the Gneezy and Potters risk elicitation task (presented in British pounds). In contrast to our previous studies, the payoffs associated with this risk elicitation task were hypothetical, so that subjects' decisions did not affect their payment. Then, subjects completed a task fidelity questionnaire. Specifically, as in Markey et al. (2014), subjects rated how much they agreed with three statements measuring attention and effort during the boredom manipulation (on 1 to 7 Likert scales from "strongly agree" to "strongly disagree"). The statements asked them to evaluate: (1) whether they tried their hardest to complete the manipulation as instructed; (2) whether there was any distraction in their surrounding environment at the time of the manipulation (e.g., TV, noise, etc.); and (3) whether they exited the study screen at any point to check a different webpage. The statements were scored so that higher ratings implied an increased adherence to the experiment's rules. Following Markey et al. (2014), subjects with an average score below 4 were omitted from our analyses. At the end of the study, subjects answered demographic questions on age and gender.

4.3 Results

As in our previous studies, our manipulations were successful in inducing different levels of boredom across conditions. A one-way ANOVA reveals significant differences in self-reported boredom across our 3 conditions ($F = 83.05, p < 0.001$). On average, those assigned to the neutral condition reported a boredom of 1.86 (out of 7; $M_{\text{Low Boredom}} = 1.86$, $SD_{\text{Low Boredom}} = 1.12$), a significantly lower intensity than those assigned to the high-boredom video condition ($M_{\text{Video}} = 4.17$, $SD_{\text{Video}} = 1.84$, $t(185.77) = 12.13$, $p < 0.001$, $d = 1.56$) and the high-boredom references condition ($M_{\text{References}} = 3.66$, $SD_{\text{References}} = 1.92$, $t(212.29) = 9.64$, $p < 0.001$, $d = 1.17$). These differences across conditions remain significant when adjusting (in a linear regression) for the effect of our boredom manipulations on arousal, valence, and the six different emotions we elicited ($\beta_{\text{Video}} = 1.29$, $t(271) = 6.29$, $p < 0.001$, $d = 0.77$; $\beta_{\text{References}} = 0.62$, $t(287) = 2.99$, $p = 0.003$, $d = 0.35$). In this case, the emotion reports were collected right after the boredom manipulations and before the risk elicitation task, providing more precise estimates of the individuals' emotional states when confronting the risk preference elicitation than in previous studies. In other words, contrary to our previous experiments, our measurement of the manipulation validity did not suffer from potential attenuating effects of completing the risk elicitation task or of the time lag between manipulation and measurement. To determine the extent to which subjects in the high-boredom conditions experienced an agitated (as opposed to apathetic) form of boredom, we subtract each individual's score in the low-arousal subscale (apathetic boredom) from the individual's score in the high-arousal subscale (agitated boredom). This reveals that, as we hypothesized, the individuals assigned to the copying references task experienced a significantly more agitated type of boredom than those assigned to watching the video ($M_{\text{Difference}} = 0.24$, $t(255.98) = 2.38$, $p = .018$, $d = 0.29$).

Again, although our boredom manipulations were successful, we found no significant differences across conditions in terms of risk preferences. Using a one-way ANOVA, we fail to reject the hypothesis of no differences in risky behavior across experimental conditions ($F = 0.102$, $p = 0.75$). On average, those assigned to the low-boredom condition invested a higher fraction of their endowment ($M_{\text{Low Boredom}} = 64.22$, $SD_{\text{Low Boredom}} = 30.16$), but differences across conditions were not significant ($M_{\text{Video}} = 61.09$, $SD_{\text{Video}} = 28.82$, $t(264.26) = -0.88$, $p = 0.377$, $d = -0.11$; $M_{\text{References}} = 63.19$, $SD_{\text{References}} = 34.02$, $t(274.31) = -0.28$, $p = 0.783, d = -0.03$). Again, these results allow us to reject even small increases in risk taking in the video ($t(264.26) = -2.64$, $p = 0.004$) and references conditions ($t(274.31) = -1.93$, $p = 0.028$) when compared to the control group. Adjusting for demographic characteristics (in a linear regression) does not alter our results (null test: $\beta_{\text{Video}} = -2.81$, $t(277) = -0.80$, $p = 0.426$, $d = -0.10$; $\beta_{\text{References}} = -0.99$, $t(293) = -0.27$, $p = 0.790$, $d = -0.03$; inferiority test: $t(277)_{\text{Video}} = -2.56$, $p = 0.006$; $t(293)_{\text{References}} = -1.93$, $p = 0.027$).

5 Study 4

As explained in the introduction, past research suggests that choice variability might play an important role in the relationship between boredom and economic risk preferences (Yakobi & Danckert 2021). As our previous risk elicitation tasks did not allow us to capture the role of variability, in this study we used a different task and a structural estimation approach to account for choice variability. We also varied our subject pool to test undergraduate students.

5.1 Subjects

We recruited a sample of 254 undergraduate students (average age = 18.6, 50% female) who completed the experiment for course credit. As our estimation procedure incorporates noise and inconsistent behavior (as explained in the results section), we did not exclude any subjects from our analyses.

5.2 Design and procedures

This last study consisted of the following four main elements: (1) a high or low-boredom induction, creating two randomly assigned conditions between subjects ($N_{\text{High Boredom}} = 118$, $N_{\text{Low Boredom}} = 136$); (2) a manipulation check measuring emotional states; (3) a risk elicitation task based on multiple-price lists (Holt & Laury 2002; Andersen et al. 2008); and (4) additional questions collecting information on age and gender. As in two of our previous studies, those assigned to the low-boredom condition saw an extract of BBC's Planet Earth. The subjects allocated to the high-boredom condition watched the same pre-validated boring video used in study 3 (Markey et al. 2014). Then, subjects reported their

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experienced intensity of boredom, anger, fear, sadness, disgust, happiness, and surprise on 1 to 7 Likert scales, and their general affect (valence and arousal) using sliders. In contrast to our previous experiments, we employed a different risk elicitation task. Specifically, we used the multiple-price lists developed by Andersen et al. (2008). Subjects were presented with 4 multiple-price lists consisting of 10 binary choices between lotteries (see Appendix A). Hence, each of our 254 subjects completed 40 risky choices, yielding a final sample of over 10,000 choices.

5.3 Results

Consistent with our previous studies, our high-boredom manipulation induced higher levels of boredom than our control condition (($M_{\text{High Boredom}} = 4.83$, $SD_{\text{High Boredom}} = 1.64$, $M_{\text{Low Boredom}} = 2.62$, $SD_{\text{Low Boredom}} = 1.6$, $t(244.73) = 10.79$, $p < 0.001$, $d = 1.36$)). The effect of our treatment on boredom remained significant and large in size even when controlling for arousal, valence, and the six other emotions we measured (($\beta = 0.7$, $t(244) = 2.44$, $p = 0.016$, $d = 0.31$)).

To analyze the effect of boredom on risk preferences, we started by looking at the proportion of risky choices made by individuals in our high and low-boredom conditions. On average, subjects assigned to low-boredom selected the riskier lottery 48.9% of the time, only 1.3% of the time less than those in the high-boredom condition, which is a non-significant difference ($M_{\text{High Boredom}} = 0.50$, $SD_{\text{High Boredom}} = 0.14$, $M_{\text{Low Boredom}} = 0.49$, $SD_{\text{Low Boredom}} = 0.16$, $t(251.98) = 0.68$, $p = 0.497$, $d = 0.08$). Using inferiority tests, these results allow us to reject the existence of small to medium increases in risk taking (Cohen's d of 0.35) in the high-boredom condition as compared to the control condition ($t(251.98) = -2.12$, $p = 0.017$). Adjusting for age and gender does not affect our results (null test: $\beta = 0.019$, $t(250) = 1.00$, $p = 0.317$, $d = 0.12$; inferiority test: $t(250) = -1.74$, $p = 0.041$). Thus, this reduced form analysis suggests that boredom did not affect risk preferences. We can also analyze whether — as in Yakobi and Danckert (2021) – bored individuals were more likely to randomize between safe and risky lotteries. In our case, state boredom does not predict choice variability (i.e., the individual's number of switching points between safe and risky lotteries; $\beta = -0.25$, $t(250) = -0.401$, $p = 0.689$, $d = 0.05$). These analyses, however, suffer from certain limitations. A structural approach has computational and power advantages, makes it possible to estimate a full utility function, and allows for the separation of parameters describing risk preferences from those describing choice variability and preference imprecision.

5.3.1 Structural approach

To use a structural approach, we estimated a Random Parameters Model (Apesteguia & Ballester 2018) assuming Expected Utility Theory and a Constant Relative Risk Aversion (CRRA) utility function (see Appendix B for the full estimation details) to jointly identify

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how boredom affected three parameters: (1) the risk aversion level (r); (2) a preference precision parameter (λ); and (3) a tremble parameter (κ), representing the probability of making a decision error (i.e., not selecting the utility-maximizing option). Specifically, when presented with a lottery paying X_1 with probability P_1 and paying X_2 with probability $1 - P_1$, we assume that each subject evaluates the gamble in the following manner:

$$U_r^{crra} = P_1 \cdot \frac{(X_1)^{1-r}}{1-r} + (1 - P_1) \cdot \frac{(X_2)^{1-r}}{1-r} \tag{1}$$

Table 1 presents our estimation results. The risk aversion, preference precision and tremble parameters do not significantly differ across conditions. There is a decrease of approximately 10% in the estimated risk aversion parameter in the high-boredom condition, but this pattern is not statistically significant ($\beta_{\text{High Boredom}} = -.053$, $z = -0.94$, $p = 0.349$). Our estimated parameters resemble those of Apesteguia and Ballester (2018), although our subject pool displays lower levels of risk aversion. Overall, both our parametric and non-parametric analyses show that boredom did not affect risk preferences.

Table 1: Estimates of the effects of our boredom treatment variable ("High Boredom") on the risk aversion parameter (r), the precision parameter (λ) and the tremble parameter (κ). Standard errors (in parenthesis) are clustered at the subject level.

Parameters:	r	λ	κ
	(1)	(2)	(3)
High Boredom	−0.053	0.011	−0.016
	(0.056)	(0.095)	(0.019)
Constant	0.446***	1.298***	.066***
	(0.041)	(0.067)	(0.014)
Observations		10,160	
Clusters		254	
Log - pseudolikelihood		−4194.3	
Note:			*p<0.05; **p<0.01; ***p<0.001

6 Boredom proneness and risk preferences

Our four studies demonstrate that exogenously manipulating boredom has no effect on risk preferences. In this section, we analyze the association between people's propensity to experience boredom as a personality trait (boredom proneness) and risky economic decisions.

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In Studies 1 and 2, we collected a measure of boredom proneness developed by Struk et al. (2017), the Short Boredom Proneness Scale. This scale consists of 8 items measuring different aspects of an individual's propensity to experience boredom (e.g.: "I find it hard to entertain myself"). Subjects rated how much they agreed with each of the 8 items on 1 to 7 Likert scales (from "Strongly disagree" to "Strongly agree"), with higher scores implying a higher propensity to experience boredom. The unidimensionality, internal consistency and construct validity of the scale have been shown to be comparable to those of longer boredom proneness scales (Struk et al. 2017). The Short Boredom Proneness Scale has been extensively used in psychology, including studies of boredom and risk-taking such as Kılıç et al. (2020) or Yakobi and Danckert (2021). It is important to note, however, that in our data the Short Boredom Proneness Scale shows relatively poor internal consistency (Cronbach's alpha Study 1 = 0.4, Cronbach's alpha Study 2 = 0.6).

Table 2: OLS coefficients of the association between boredom proneness (BP) and the amount allocated to the risky investment in the Gneezy and Potters (1997) task. All models include age, gender, education and race as controls. Standard errors are in parenthesis. Model 1: Subjects in the low boredom condition of Study 1. Model 2: Subjects in the high boredom condition of Study 1. Model 3: Subjects in the low boredom condition of Study 2. Model 4: Subjects in the high boredom condition of Study 2. Model 5: Subjects pooled across experiments and conditions. In this model, apart from demographic variables, we included experiment and condition-specific dummy variables to control for baseline differences in risk-taking across experimental conditions.

	Dependent variable: Amount Invested				
	Study 1		Study 2		Pooled Data
	Low Boredom	High Boredom	Low Boredom	High Boredom	
	(1)	(2)	(3)	(4)	(5)
BP	−0.462	−0.073	−0.158	−0.352	−0.158
	(1.105)	(0.914)	(0.202)	(0.203)	(0.146)
Observations	99	91	266	256	712
R^2	0.035	0.051	0.051	0.113	0.051
Adjusted R^2	−0.051	−0.042	0.025	0.087	0.036
Residual Std. Error	38.634	39.161	31.170	30.926	33.34
F Statistic	0.408	0.547	1.967	4.491^{***}	3.693^{***}

Note: $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Table 2 presents the results of OLS regressions analyzing the association between boredom proneness (BP) and risk preferences as measured by the Gneezy and Potters (1997) task. We included a separate regression model for each condition in Studies 1 and 2 and also

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an additional regression pooling all observations together (n = 712). All models include age, gender, education, and race as covariates. Across regressions, boredom proneness shows a negative but non-significant association with the amount of cents allocated to the risky investment in the Gneezy and Potters task. These findings are in line with the recent work of Yakobi and Danckert (2021) mentioned in the introduction section, in which the authors also failed to find a significant association between boredom proneness and risk preferences. These results also add to the overall null relationship between boredom and economic risk preferences documented in this paper. Further analyses showing a null relationship between trait boredom and risk taking are presented in Appendix E.

7 Discussion

Across four studies (N = 1,384) employing different subject pools, different incentives schemes, different boredom manipulations, different risk elicitation tasks, and both reduced form and structural approaches, we found no effect of state boredom on economic risk preferences. We also failed to find a relationship between boredom proneness and risky decisions. Using inferiority tests, we rejected the presence of even small increases in risk taking when measured through a one-shot elicitation task (Studies 1 to 3), and small to moderate increases in risk taking when measured through a multiple-choice elicitation (Study 4) – see Figure 1.

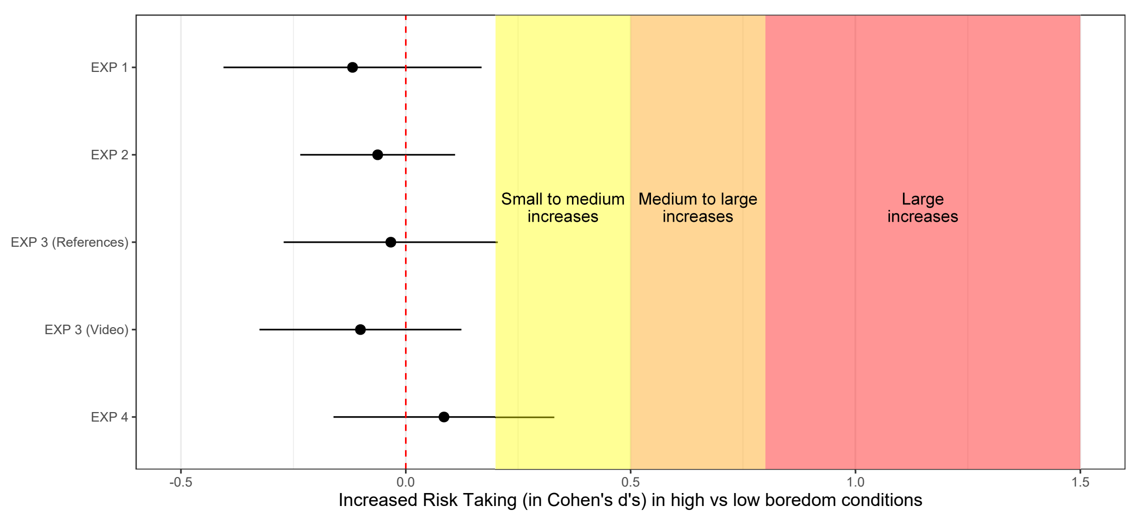

Figure 1: Overview of experimental results. Compared to its respective control conditions, none of our boredom inductions had an effect on risk taking significantly different from 0. Using inferiority tests, we reject the presence of small increases (an increase of a Cohen's d = 0.2) in risk taking in Studies 1 to 3 and small to medium increases (Cohen's d = 0.35) in Study 4.

Our results challenge previous findings linking state boredom and economic risk preferences (Bench et al. 2021; Kılıç et al. 2020; Miao et al. 2020). Given that our studies were not

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designed to measure the impact of specific experimental design choices on the relationship between boredom and risk, we can only speculate about the reasons for these contrasting results. As explained in the introduction, the previous literature has some limitations in terms of using non-established boredom inductions, employing risk elicitation tasks that do not separate risk from ambiguity preferences, modest sample sizes, and a lack of accounting for choice variability. These factors could be behind the contrasting results presented in this paper. We believe that our research provides an improved test of the effects of boredom on economic risk taking, and future work should expand our analyses to evaluate the circumstances (if any) under which boredom leads to riskier economic choices.

Our findings are in line with those of Yakobi and Danckert (2021). These authors showed that boredom-prone individuals do not make riskier economic choices. Although the authors did not manipulate boredom, they collected a measure of state boredom that also displayed a null relationship with risk taking. We expanded these analyses to settings where state boredom is exogenously manipulated. In addition, Yakobi and Danckert (2021) showed that boredom-prone individuals are more likely to randomize across different choice alternatives. Our experimental design precludes us from testing the replicability of this result (as we only measured boredom proneness in studies using a one-shot risk elicitation task). However, our structural and reduced form approaches in Study 4 found no relationship between state boredom and choice variability, suggesting that the link between trait boredom and choice variability is not generalizable to state boredom.

A skeptical reader might wonder whether our studies were sufficiently powered. Our experimental designs took into consideration the effect sizes of the relationship between boredom and risk taking reported by the previous studies that exogenously manipulated this emotion. For example, Miao et al. (2020) found that the Cohen's d of the effect of boredom on risk taking ranges approximately between 0.4 and 0.6, a medium to large effect size. Bench et al. (2021) found an even bigger effect size of d = 0.87, a large effect. Each of our studies has a statistical power of over 80% to detect an effect with an approximate Cohen's d of 0.4. That is, all our studies are well powered to find the smallest effect size reported in the existing literature, yet we failed to find this effect, not once but four times. Furthermore, our second study has over 80% power to detect an effect with a Cohen's d of 0.25. That is, our second study has enough power to detect a small effect size, half the size of the median effect presented in the literature. Of course, it could be that our boredom manipulations were weak, making our power estimations unreliable, but this is unlikely. In fact, the manipulation checks in Studies 3 and 4 showed that our emotion inductions had a large effect on boredom (average Cohen's d = 1.2). While we found more modest effects of our inductions on boredom in Studies 1 and 2, this is likely produced by the time lag and the attenuating effects of having the risk elicitation task placed between our boredom induction and our manipulation check. Aside from demonstrating the robustness of our manipulations, having different boredom levels across treatment groups even after the risk elicitation task shows that presenting the subjects with a risky decision is not enough to fully mitigate the experience of boredom.

The results of our studies, nevertheless, present some limitations. First, the choices our subjects faced involved small rewards or were hypothetical. For example, in both Study 1 and Study 2 each subject was given $1 and, by investing a fraction of it, the subject could receive up to $2.5. Although this represents a modest earning, it is important to note that $2.5 for less than 10 minutes of work is well above the minimum wage in the US and considerably higher than typical Amazon MTurk payments (see Hara et al. (2018)). Future research should address the role of incentives in the relationship between boredom and economic risk preferences. Second, we limited our investigation to short term boredom. In doing so, we focused on relatively brief (approximately 5 minutes) boredom inductions. Past work (Markey et al. 2014) and our own manipulation checks revealed that these short inductions successfully induced boredom in our subjects. Nevertheless, the consequences of experiencing boredom for a short period of time might be different from those of experiencing this emotion for a longer time (e.g., during a long flight or a quarantine). Further research should evaluate the differences (if any) in economic preferences induced by these two forms of boredom. Finally, while our findings have important implications for economic decisions under risk, generalizations from our results to other domains should be made with caution, especially if the decisions involved are not economic in nature. Measures of economic risk-taking have been shown to have limited predictive power when it comes to risky behaviors outside the lab (Galizzi et al. 2016; Charness et al. 2020). For example, Galizzi et al. (2016) found that economic risk-taking tasks do not predict health-related risky behaviors such as smoking or heavy alcohol drinking. Furthermore, as outlined in the introduction, economic choices under risk in experimental settings tend to have little stimulating power – as they often involve low monetary outcomes, the repetition of very similar scenarios, and the presentation of numerical probabilities (Rottenstreich & Hsee 2001; Pachur et al. 2014; Suter et al. 2015). More research is needed to systematically evaluate the impact of boredom on risky choices in different domains, paying special attention to contexts where risky choices might resolve an increased demand for stimulation under boredom.

Despite these limitations, our results challenge existing ideas about the effects of boredom on economic decisions, at least as they apply to experimental environments. The widespread assumption that boredom decreases risk aversion simply does not hold in our studies. This has important implications for the design of experiments. Our findings show that experimenters should not be overly concerned about the carryover effects of boring tasks on subsequent risk attitudes. More generally, to the extent that these results hold beyond the stylized experimental settings we have used, they suggest that boredom might not be as powerful in driving economic decisions as previously thought. This would apply to a variety of domains in which people are likely to experience boredom before making important risk-related decisions, such as investment, insurance or debt accumulation decisions. These relationships would of course need to be established in further research using relevant field data. Meanwhile, experimental researchers can rest assure that boredom should not be a major concern in experiments on risky decision making.

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Appendix A. Multiple price lists (Study 4)

Tables A1 and A2 present the multiple-price lists used to elicit risk preferences in Study 4. Each row represents a choice. Individuals had to decide between a lottery paying X1 with probability P1 (and X2 otherwise) and a lottery paying Y1 with probability P1 (and Y2 otherwise). This yielded 10 choices per list, for a total of 40 choices. Monetary outcomes (X1, X2, Y1, Y2) were presented in Euros.

TABLE A1: Multiple-price lists 1 and 2.

	List 1				*List 2*			
P1	X1	X2	Y1	Y2	X1	X2	Y1	Y2
0.1	20	16	38.5	1	22.5	15	40	5
0.2	20	16	38.5	1	22.5	15	40	5
0.3	20	16	38.5	1	22.5	15	40	5
0.4	20	16	38.5	1	22.5	15	40	5
0.5	20	16	38.5	1	22.5	15	40	5
0.6	20	16	38.5	1	22.5	15	40	5
0.7	20	16	38.5	1	22.5	15	40	5
0.8	20	16	38.5	1	22.5	15	40	5
0.9	20	16	38.5	1	22.5	15	40	5
1	20	16	38.5	1	22.5	15	40	5

TABLE A2: Multiple-price lists 3 and 4.

	List 3				*List 4*			
P1	X1	X2	Y1	Y2	X1	X2	Y1	Y2
0.1	20	17.5	40	1.5	25	10	45	0.5
0.2	20	17.5	40	1.5	25	10	45	0.5
0.3	20	17.5	40	1.5	25	10	45	0.5
0.4	20	17.5	40	1.5	25	10	45	0.5
0.5	20	17.5	40	1.5	25	10	45	0.5
0.6	20	17.5	40	1.5	25	10	45	0.5
0.7	20	17.5	40	1.5	25	10	45	0.5
0.8	20	17.5	40	1.5	25	10	45	0.5
0.9	20	17.5	40	1.5	25	10	45	0.5
1	20	17.5	40	1.5	25	10	45	0.5

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Appendix B: Structural estimation of risk preferences (Study 4)

Our structural estimation assumes Expected Utility Theory and Constant Relative Risk Aversion (CRRA). Specifically, an agent with risk aversion r, facing a lottery that pays X_1 with probability P_1 and X_2 otherwise, evaluates the gamble in the following manner:

$$U_r^{crra} = P_1 \cdot \frac{(X_1)^{(1-r)}}{(1-r)} + (1-P_1) \cdot \frac{(X_2)^{(1-r)}}{(1-r)} \quad .$$

We, furthermore, assume logistically distributed errors, so that the probability of choosing lottery 1 over (a safer) lottery 2 is given by:

$$\frac{e^{\lambda\omega^{(x,y)}}}{e^{\lambda\omega^{(x,y)}} + e^{\lambda r}} \quad ,$$

where $\omega^{(x,y)}$ is implicitly defined by:

$$U_{\omega^{(x,y)}}^{crra}(l_1) = U_{\omega^{(x,y)}}^{crra}(l_2) \quad .$$

That is, $\omega^{(x,y)}$ is the risk aversion level that equates the value of the two lotteries under consideration. As in Apesteguia and Ballester (2018), our computations are performed by maximizing the conditional log-likelihood of the model. In our estimations, the probability of choosing the safer lottery l_A over l_B is given by:

$$P_i(\theta;\omega_i) = (1-\kappa)F(\lambda(r-\omega_i)) + \kappa(1-F(\lambda(r-\omega_i))) \quad ,$$

where F denotes the logistic cumulative distribution, $\theta = (r, \kappa, \lambda)$ the set of parameters to estimate, κ a tremble probability, λ a precision parameter, ω_i the risk aversion level that equates the valuation of both lotteries and r the population risk aversion level.

Appendix C: Demographic composition of our samples

Table C1 presents the demographic composition (gender, race, and education) of the samples used in each one of our studies. Figure C1 presents the age distribution for each of our samples.

Table C1: Demographic composition for each study (gender, race, education).

Study	Female	Basic Education	College	Masters/PhD	Asian	Black	White	Other Race
1	42.1%	41.1%	50%	8.9%	3.7%	9.5%	82.1%	4.7%
2	54.4%	41%	46.4%	12.6%	7.3%	7.3%	81%	4.4%
3	71.8%	-	-	-	-	-	-	-
4	50%	-	-	-	-	-	-	-

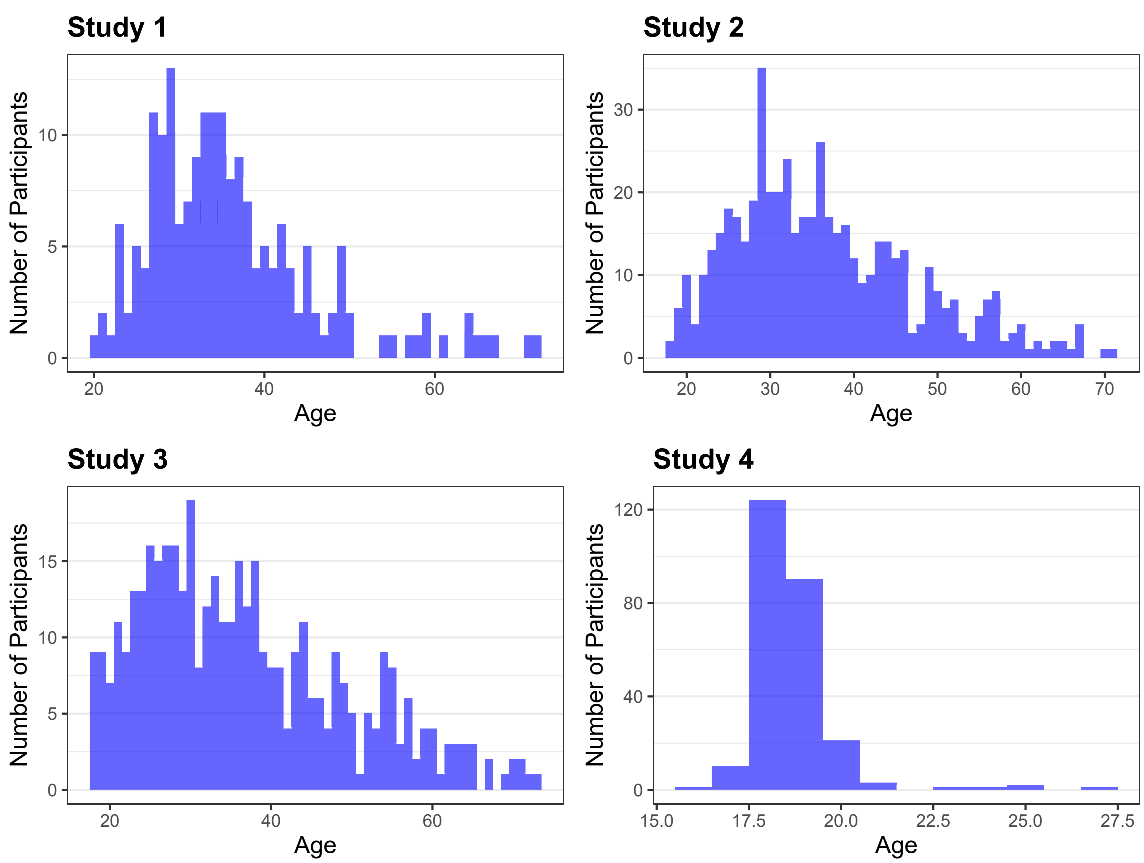

Figure C1: Age distribution for each study.

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Appendix D: Manipulation checks for all emotions

Our boredom manipulations had been previously validated (Markey et al. 2014; Van Tilburg & Igou 2012, 2016; Moynihan et al. 2017), and in the main body of the paper we explain how they increased boredom in our samples. However, to better understand the relationship between boredom and risk taking, we decided to measure the impact of our manipulations on a few additional emotions. In this appendix, we report the effect of our boredom manipulations on each specific emotional state. The main points in relation to these results can be summarized as follows:

1. Our boredom manipulations had a negligible effect on most emotional states.
2. For those emotional states that were significantly affected, our boredom manipulations tended to have a small effect (especially when compared to the effect that our manipulations had on boredom).
3. A change in boredom could lead to a change in other emotions. For instance, by definition, a boring situation carries less joy than a neutral one. Some of the changes in these emotional states are likely to be a direct consequence of a change in boredom.
4. As indicated in the paper, the main results of our studies hold when we control for the additional emotional states we collected. Self-reported boredom is not associated with an increase in economic risk-taking (even when we control for other specific emotional states).

Table D1: Manipulation check Study 1

	Dependent variable:								
	Anger (1)	Anticipation (2)	Boredom (3)	Disgust (4)	Fear (5)	Joy (6)	Sadness (7)	Surprise (8)	Trust (9)
High Boredom	0.177	-0.673^{*}	0.753^{**}	0.042	−0.050	-1.004^{***}	−0.185	−0.021	-0.801^{**}
	(0.268)	(0.357)	(0.298)	(0.226)	(0.253)	(0.362)	(0.262)	(0.330)	(0.344)
Constant	0.768^{***}	4.465^{***}	1.192^{***}	0.606^{***}	0.929^{***}	3.455^{***}	0.899^{***}	1.889^{***}	3.636^{***}
	(0.185)	(0.247)	(0.206)	(0.157)	(0.175)	(0.250)	(0.182)	(0.228)	(0.238)
Observations	190	190	190	190	190	190	190	190	190
R^2	0.002	0.019	0.033	0.0002	0.0002	0.039	0.003	0.00002	0.028
Adjusted R^2	−0.003	0.013	0.028	−0.005	−0.005	0.034	−0.003	−0.005	0.023
Res Std. Error	1.843	2.460	2.050	1.558	1.745	2.492	1.807	2.269	2.369
F Stat	0.439	3.554^{*}	6.400^{**}	0.035	0.039	7.699^{***}	0.496	0.004	5.422^{**}

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

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Table D2: Manipulation check Study 2

	Dependent variable:								
	Anger	Anxiety	Boredom	Desire	Disgust	Fear	Happiness	Sadness	Relaxation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High Boredom	0.026	0.003	0.761***	0.063	−0.085	−0.036	0.037	0.223**	−0.048
	(0.111)	(0.126)	(0.160)	(0.095)	(0.102)	(0.068)	(0.117)	(0.097)	(0.141)
Constant	1.767***	1.970***	2.880***	1.406***	1.628***	1.286***	1.756***	1.406***	2.376***
	(0.078)	(0.088)	(0.112)	(0.067)	(0.072)	(0.048)	(0.082)	(0.068)	(0.099)
Observations	522	522	522	522	522	522	522	522	522
R^2	0.0001	0.0001	0.042	0.001	0.001	0.001	0.0002	0.010	0.0002
Adjusted R^2	−0.002	−0.002	0.040	−0.001	−0.001	−0.001	−0.002	0.008	−0.002
Res Std. Error	1.265	1.443	1.828	1.088	1.170	0.777	1.337	1.106	1.612
F Stat	0.055	0.0005	22.611***	0.433	0.686	0.275	0.102	5.300**	0.115

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

Table D3: Manipulation check Study 3

	Dependent variable:						
	Anger	Boredom	Disgust	Fear	Happiness	Sadness	Surprise
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Boredom (Video)	0.186	2.303***	0.125	−0.293***	−2.394***	−0.367**	−1.372***
	(0.123)	(0.197)	(0.079)	(0.084)	(0.161)	(0.157)	(0.165)
High Boredom (References)	0.498***	1.794***	0.208***	−0.283***	−2.365***	−0.636***	−0.882***
	(0.119)	(0.190)	(0.076)	(0.081)	(0.155)	(0.152)	(0.159)
Constant	1.269***	1.862***	1.106***	1.400***	4.394***	2.169***	2.744***
	(0.081)	(0.129)	(0.052)	(0.055)	(0.105)	(0.103)	(0.108)
Observations	418	418	418	418	418	418	418
R^2	0.041	0.277	0.018	0.039	0.431	0.041	0.151
Adjusted R^2	0.036	0.274	0.013	0.034	0.428	0.037	0.147
Res Std. Error	1.025	1.634	0.655	0.698	1.334	1.302	1.367
F Stat	8.779***	79.498***	3.791**	8.391***	157.049***	8.937***	36.808***

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

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Table D4: Manipulation check Study 4

	Dependent variable:						
	Anger	Boredom	Disgust	Fear	Happiness	Sadness	Surprise
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Boredom	0.145	2.207***	0.547***	−0.291*	−3.206***	0.236	−1.247***
	(0.167)	(0.204)	(0.162)	(0.172)	(0.171)	(0.229)	(0.208)
Constant	1.728***	2.632***	1.419***	1.926***	5.426***	2.824***	3.713***
	(0.114)	(0.139)	(0.110)	(0.117)	(0.116)	(0.156)	(0.142)
Observations	254	254	254	254	254	254	254
R^2	0.003	0.317	0.043	0.011	0.584	0.004	0.125
Adjusted R^2	−0.001	0.314	0.040	0.007	0.582	0.0003	0.121
Res Std. Error	1.330	1.623	1.286	1.369	1.356	1.817	1.654
F Stat	0.750	116.834***	11.424***	2.850*	353.075***	1.064	35.932***

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

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Appendix E: Trait boredom and risk taking

In this appendix, we present a more detailed account of the relationship between trait boredom and risk taking. In order to do so, we include additional regressions in which we pool conditions together and have a dummy variable capturing whether an individual was assigned to the high boredom (HB Condition = 1) or low boredom (HB Condition = 0) condition. We also include regressions in which we do not use additional variables as controls. In all cases, the dependent variable is the number of cents allocated to the risky lottery in our risk elicitation task. These analyses show a null relationship between trait boredom and risk taking, complementing the results presented in Table 2 of the main manuscript. Tables E1 and E2 present our estimation results.

Table E1: Boredom proneness and risk taking in Study 1. Models 1 and 2 include all subjects in Study 1. Models 3 and 4 include only those subjects assigned to the low-boredom condition. Models 5 and 6 include only those subjects assigned to the high-boredom condition.

	Dependent variable:					
	Amount Invested					
	(1)	(2)	(3)	(4)	(5)	(6)
BP	0.032	−0.170	−0.196	−0.462	0.186	−0.073
	(0.671)	(0.681)	(1.050)	(1.105)	(0.878)	(0.914)
HB Condition	−4.511	−4.948				
	(5.542)	(5.607)				
Controls	No	Yes	No	Yes	No	Yes
Constant	54.964***	27.999	57.592***	25.502	48.617***	20.376
	(8.633)	(21.471)	(12.699)	(29.324)	(11.243)	(32.648)
Observations	190	190	99	99	91	91
R^2	0.004	0.037	0.0004	0.035	0.001	0.051
Adj R^2	−0.007	−0.011	−0.010	−0.051	−0.011	−0.042
Res Std. Error	38.115	38.192	37.875	38.634	38.570	39.161
F Stat	0.331	0.767	0.035	0.408	0.045	0.547

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

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Table E2: Boredom proneness and risk taking in Study 2. Models 1 and 2 include all subjects in Study 2. Models 3 and 4 include only those subjects assigned to the low-boredom condition. Models 5 and 6 include only those subjects assigned to the high-boredom condition.

	Dependent variable:					
	Amount Invested					
	(1)	(2)	(3)	(4)	(5)	(6)
BP	−0.067	−0.227	−0.005	−0.158	−0.129	-0.352^{*}
	(0.139)	(0.145)	(0.194)	(0.202)	(0.199)	(0.203)
HB Condition	−1.947	−2.004				
	(2.803)	(2.760)				
Controls	No	Yes	No	Yes	No	Yes
Constant	66.430^{***}	69.489^{***}	65.002^{***}	83.283^{***}	65.956^{***}	58.783^{***}
	(3.737)	(9.808)	(4.854)	(13.346)	(5.126)	(14.416)
Observations	522	522	266	266	256	256
R^2	0.001	0.047	0.00000	0.051	0.002	0.113
Adjusted R^2	−0.002	0.032	−0.004	0.025	−0.002	0.087
Res Std. Error	31.988	31.438	31.625	31.170	32.410	30.926
F Stat	0.372	3.135^{***}	0.001	1.967^{*}	0.423	4.491^{***}

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

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