Decision-makers use social information to update their preferences - but choose for others as they do for themselves

John Michael*[1,2], Alina Gutoreva[3], Michele H. Lee[3], Peng Ning Tan[3], Eleanor M. Bruce[3,4], Marcell Székely[2], Thobhani Ankush[5], Hiroaki Sakaguchi[3], Lukasz Walasek[3], and Elliot A. Ludvig[3]

[1]Department of Philosophy, University of Warwick

[2]Department of Cognitive Science, Central European University Budapest

[3]Department of Psychology, University of Warwick

[4]UTS Business School, University of Technology Sydney

[5] WMG, University of Warwick

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* Address correspondence to:
John Michael
Department of Philosophy, Social Sciences Building, University of Warwick
CV4 7AL Coventry, UK
jmichael.2@warwick.ac.uk

Abstract

People's risky decisions are susceptible to the social context in which they take place. Across three experiments we investigated the influence of three social factors upon participants' decisions: the recipient of the decision-making outcome (self, other, or joint), the nature of the relationship with the other agent (friend, stranger, or teammate), and the type of information that participants received about others' preferences: none at all, general information about how previous participants had decided, or information about a specific partner's preference. We found that participants' decisions about risk did not differ according to whether the outcome at stake was their own, another agent's, or a joint outcome, nor according to the type of information available. Participants did, however, adjust their preferences for risky options in light of social information.

Keywords: decision making for others; risk-taking; coordination; social information, social distance

Highlights:

- We extend the literature on how social context influences decisions about risk.
- Risky decision-making did not generally differ according to who the recipient was.
- Participants adjusted their risky choices in light of social information.

Introduction

In everyday life, we often have to choose between options with differing levels of risk and reward, such as when we must decide whether to take the shorter but more traffic-prone of two routes to work, or whether to invest our savings in risky but potentially lucrative stocks as opposed to keeping them safely in the bank. Given the centrality and importance of risky decision-making, it is no wonder that there has been a wealth of research investigating contextual factors influencing such decisions, such as whether the outcome of the decision is framed as a potential gain or loss (Benjamin & Robbins, 2007; Tversky & Kahneman, 1981), and whether the risk pertains to physical health or to financial investments (Weber, Blais & Betz, 2002).

More recently, there has been a surge of interest in the social factors that may influence risky decision-making. For example, researchers have begun to investigate whether risky decision-making differs according to who the recipient of the decision-making outcome is. This question is important insofar as people make risky decisions on behalf of others every day, and this skill is essential for many professions. For example, financial managers suggest and guide the decisions of their clients about how to arrange their investments, doctors suggest courses of treatment with uncertain outcomes to their patients, and we all make decisions for our relatives and friends. If there are systematic differences in the decisions people make about risk when deciding for themselves or for others, this would have serious implications for regulators, policymakers and other professionals.

So far, however, the research findings on self-other differences in risky choice have produced mixed results (Polman & Wu, 2019). Some studies find that people make riskier decisions for others than for themselves (e.g. Agranov, Bisin, & Schotter, 2014; Chakravarty, Harrison, Haruvy & Rutsrom, 2011), while other studies have suggested that decisions for others tend to be more risk averse (e.g. Atanasov, 2015; Bolton & Ockenfels, 2010; Wang et al., 2017). Self-other differences in risky decision-making have been suggested to vary depending on the independent variables manipulated in experiments, such as the number of other individuals implicated by the decision (Eijkelenboom, Rohde, & Vostroknutov, 2018), whether the risky outcomes involve losses (Atanasov, 2015; Raue, Streicher, Lermer, & Frey, 2015), and whether participants harbour competitive or pro-social motives (Olschewski, Dietsch, & Ludvig, 2019).

One theoretical proposal that has been offered in the literature is that differences in decision-making for self and others may be driven by a sense of responsibility for the outcomes of decisions and/or anticipation of blame should the decision result in negative outcomes for others (Botti, Orfali, & Iyengar, 2009; Simonson, 1992; Stone et al., 2002; Wang et al., 2017). If this blame-avoidance hypothesis is correct and most people are risk-averse, people would be more conservative -- i.e. risk-averse -- when deciding on behalf of others. In addition, blame avoidance may lead people to choose options that are easily justified to reduce any potential blame if the outcome turns out to be suboptimal (Kray, 2000). Blame avoidance may also lead people to make more risk-seeking decisions under some circumstances, such as when they believe that their partner is highly risk-seeking and are thus inclined to accommodate the other's preference in order to avoid blame.

A distinct theoretical view that has been proposed to account for self-other differences in risky decision-making is based on construal level theory (Trope & Liberman, 2010). Broadly, this theory argues that psychological distance from a decision influences the types of representations and considerations people take into account during decision-making (Trope & Liberman, 2010). Various factors such as temporal distance, probability, and social distance contribute to psychological distance (Bar-Anan, Liberman & Trope, 2006; Liberman & Trope, 2014; Bhatia & Walasek, 2016), with self-other distinctions being one form of social distance (Liberman, Trope, & Stephan, 2007; Lu, Xie, & Xu, 2012). Decisions for others are hypothesized to involve greater psychological distance, and thus to be shaped by more abstract, context-independent, and reason-driven considerations. Conversely, decisions for self are hypothesized to involve less psychological distance, and thus to be shaped by lower-level concrete representations with greater emphasis on context-specific factors (Liberman et al., 2007; Polman & Emich, 2011). Psychological distance has been shown to influence risk preferences. Participants who are primed with high rather than low construal take more risks, as they pay more attention to the highest positive outcome and become insensitive to probability (Lermer et al, 2015; Streicher et al., 2012; Streicher, Lerner & Frey, 2015). Consequently, construal level theory predicts that people should be more risk averse when they make decisions for themselves as opposed to when their decisions affect others.

Drawing upon these competing theoretical proposals and mixed empirical evidence, we designed three experiments in which we could probe a range of hypotheses about the influence of social information on risky decision-making and about self-other differences in risky decision-making. In all three experiments, the design was such that an increase in risk-seeking would lead to an increase in monetary payoffs. According to the blame-avoidance hypothesis, participants should avoid blame by making more conservative choices when they lack information about their partner's preferences (Experiments 1 and 2). This would lead to more risk-averse decisions--which, in the context of our experiments, would always have led to lower monetary outcomes. In addition, Experiment 1 manipulates the amount of information available to participants about how generic others have previously decided. If decision-making for others is characterized by blame-avoidance, we should expect that when deciding for others, participants should place more weight on information about how generic others have previously decided, compared to when deciding for themselves. This is because such information would provide a basis for justifying the decision and thereby minimizing blame.

As a further test of this idea, we provide participants in Experiment 3 with information about their specific partner's personal preference with respect to risk tolerance. Under these circumstances, the blame-avoidance hypothesis in fact provides a reason to expect *greater* risk-seeking: insofar as blame-avoidance is a critical factor in decision-making for others, participants should be inclined to use this information to align their decisions with the decisions their partner could be expected to make. Given that they have reason to ascribe a preference for high risk-seeking to their partners in Experiment 3, this should lead to greater risk-seeking when their partner's earnings are at stake than when only their own earning are at stake -- and to more earnings on the task.

In contrast, construal level theory generates a different set of predictions for these experiments. Most importantly, this theory predicts greater risk-seeking when deciding for others than when deciding for self in these experiments. Because in our tasks risk-seeking behavior leads to higher monetary outcomes, construal level theory predicts that participants should earn more money when making decisions for others. Experiment 1 of the current study tests this hypothesis directly by manipulating the recipient of the decision-making outcome (Self vs Other). More generally, according to the theory, any factors that reduce social distance to the recipient of the decision-making outcome should increase risk-averse behavior. As a test of this prediction, Experiment 2 varies the nature of the participant's relationship to the non-self recipient (Friend or Stranger). Construal level theory predicts greater risk aversion when deciding for a friend than when deciding for a stranger in this scenario, as friends are psychologically closer than unknown individuals. Experiment 3 also varies the nature of the interaction with the partner (Coordination or No Coordination); in principle, construal level theory should predict greater risk aversion when engaged in a tightly coordinated joint action with a partner than when not coordinating because tight coordination may reduce psychological distance.

Experiment 1

Most studies examining self-other differences in risky choice have used a decision-from-description paradigm, which consists of pairwise choice problems between a safe and risky option (e.g., Kahneman & Tversky, 1981). These problems are presented as explicitly described probabilities and corresponding outcomes, based on which participants make choices. In the real world, however, the riskiness of available options must often be learned through experience. And indeed, risky decisions are significantly different when made from description or from experience (e.g., Hertwig et al., 2004; Ludvig & Spetch, 2011). Thus, we adopted a decision-from-experience paradigm here, where the outcomes and their probabilities are unknown at first and can only be learned by sampling from the available options (Hertwig & Erev, 2009). This method was chosen because it provided the opportunity for the participants to learn the outcomes and the underlying probabilities by themselves. The design of the task used in this experiment was adapted from Olschewski et al. (2019) and aimed to probe the following three pre-registered hypotheses:

First, we probed whether social information in the form of illustrated proportions of previous participant choices can influence risky decisions from experience (see Fig 1). Building upon empirical evidence that social information can influence behaviour across various contexts (Parks et al., 2001), we predicted that:

(P1) *participants' risky decisions from experience would be influenced by the social information about past choices made by others.*

In addition, whereas many studies have explored self-other differences in various contexts, none have explicitly focused on whether decision-makers rely on social information differently when deciding for self or others. Some existing research suggests that people make decisions for others that are easy to justify (Kray, 2000) so as to avoid potential feelings of guilt, regret, or blame (Stone et al., 2002; Wang et al., 2017). Building on this research, we hypothesize that when deciding for others, social information may be taken into greater consideration as decision-makers place greater emphasis on desirability factors, and that the social information may be regarded as a convenient justification that helps reduce responsibility for potential negative outcomes when people make decisions for others. As such, we predicted that:

(P2) *decision-makers are more likely to be influenced by social information when making decisions for others than when deciding for themselves;* and, as a later refinement, that:

(P3) *this magnitude of social influence depends on the strength of the social information.*

Finally, we also sought to explore the relationship between the amount of experience a decision-maker has and the influence of social information. In each round of the experiment, the number of samples participants made prior to a decision was varied, manipulating the amount of experience they had regarding the payout range of each gamble. Existing literature indicates that people are likely to give greater weight to private than social information (Puskaric et al., 2017; Weizsacker, 2010). We therefore expect that when participants have insufficient experience, they are likely to rely on social information more heavily. Thus, we predicted that:

(P4) *there would be a negative relationship between the number of samples prior to decision and the influence of social information – i.e., participants' decisions would align less with social information about choices of others when they had more samples prior to decision.*

In addition, we explored a further set of questions that arose post hoc. Most importantly, we were interested in whether participants had exhibited different overall levels of risk seeking when deciding for self or other in this paradigm. In the experiment, the options were set such that the riskier option always had a higher expected value than the safer option. Two hypotheses currently in the literature thus lead to competing predictions in this experiment. Construal level theory hypothesizes that greater psychological distance from the decision when choosing on behalf of someone else would lead participants to take more information into account and to earn more money (here become more risk seeking). It therefore predicts that:

(P5a) *participants should make be more risk seeking and earn more points when deciding on behalf of the partner.*

In this experiment, this increase in earnings would result in greater risk-seeking. In contrast, the blame-avoidance hypothesis predicts that:

(P5b) *participants should make more risk-averse decisions, leading to fewer points, when deciding for others.*

A 2 x 3 within-subject design with one additional randomized variable was used to address the hypotheses. The first independent variable was the recipient of participant choice – Self or Other. The second independent variable was the strength of social information – Strong (10% and 90%), Weak (30% and 70%), or Neutral (50%). Each of the 5 levels of social information (10%, 30%, 50%, 70% and 90%) was operationalized as the proportion of previous participants who chose the riskier of the two provided options. The 10% and 90% levels of social information were regarded as Strong social information because participants were shown a larger majority of previous participants choosing either option. Conversely, the 30% and 70% levels of social information were classified as Weak social information.

The first 4 hypotheses, sample sizes, methods, and initial analyses were all pre-registered before data collection. The pre-registration and Matlab script used for this experiment as well as all the raw data can be accessed at: https://osf.io/t4bzf/?view_only=4f6d5d6eaf464ed1aa95fcb6a46dd594

Method

Participants

A total of 69 participants (26 males, 42 females, 1 undisclosed, M_{age} = 24.3 years, SD_{age} = 3.86 years) were recruited via the University of Warwick SONA paid research online sign-up system. Participants were recruited in 7 sessions of between 6 to 12 participants per session, with each session lasting approximately 30 minutes. Each participant was compensated with a £2 show-up fee and had the opportunity to win up to an additional £6.50. The mean payment received by participants was £5.38 (range: £3.30 - £8.40).

Among the 69 recruited participants, six were excluded as per the pre-registered exclusion criteria for failing the manipulation check during the experiment (choosing the dominated option on the catch trial). The data from these participants were excluded from data analysis, leaving N = 63 for all reported analyses. The 63 participants met the target set at pre-registration of recruiting at least 60 participants. Power analyses indicated that for a medium effect size (d = 0.40) and an alpha level of .05, approximately 55 participants were required for 90% experimental power.

Materials

The experiment was presented to participants as a game comprising 21 rounds. On each round, gambles were represented by images such as treasure chests, and the number of sample plays left on a particular round was denoted by images such as coins serving as counters. Figure 1 provides an example of the screen display for the sampling phase. The social information was illustrated using a total of 20 cartoon characters, with the relative number of cartoon characters above each gamble representing the proportion of previous participants who chose each gamble. The levels of social information were manipulated by adjusting the number of cartoon characters above each gamble. Figure 1B provides an example of the screen display for the choice phase, using the 90% social information condition, illustrating 18 cartoon characters above one gamble and two cartoon characters above the other. Note that the social information did not in fact represent actual past participants, but was explicitly manipulated as the independent variable.

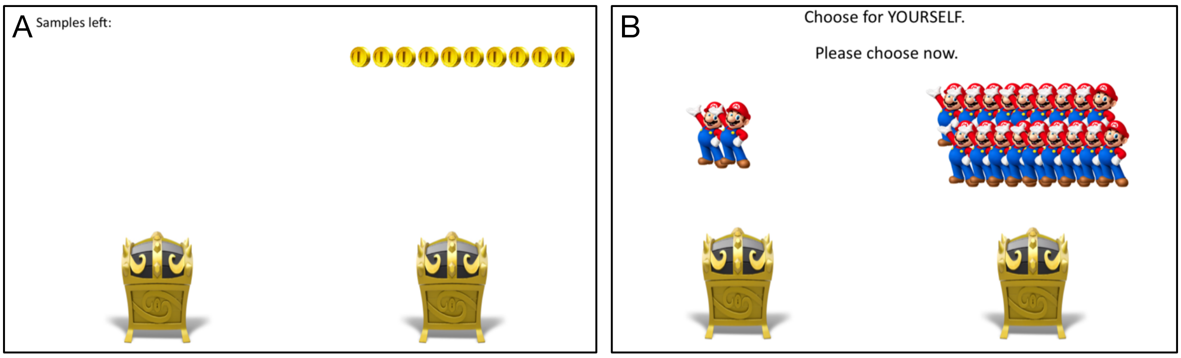

Figure 1. (A) Screenshot of Sampling Phase. The two treasure chests represent the gambles, and the number of coins represents the number of samples left. (B) Screenshot of social information and instructions shown on the decision screen. In this example, the social

information shows 90% of the previous participants choosing the gamble on the right, and participants are told that they are choosing for themselves in this round.

Across the experiment, 4 sets of pictures were used. Each set consisted of 3 different pictures used to illustrate the gambles, the number of samples remaining, and the cartoon characters representing previous participant choices. Each set of pictures was randomly allocated to 5 of the 20 test rounds, and a randomly selected set was used once more for the catch round. All stimulus images are available at the OSF repository.

Procedure

All experimental sessions took place in a computer lab at the University of Warwick. Upon arrival, each participant was randomly assigned to a cubicle with a Windows desktop computer equipped with a standard keyboard. Participants were given general instructions regarding the experiment as well as a participant demographic information sheet to fill in.

After giving their informed written consent, participants were asked to begin reading through further instructions regarding the experiment on their respective desktop screens. Participants were told that they would be making decisions for themselves on some rounds, and for a randomly selected other participant on other rounds. They were also informed that the final payment from the experiment would be their show-up fee plus the average of the payout from a randomly selected gamble they played for themselves and the payout from a randomly selected gamble another participant in the room had played for them. Verbatim instructions are provided in the Appendix.

During the study, each participant played a total of 21 rounds (20 test rounds and 1 catch round). Each round consisted of two phases: the sampling phase, followed by the decision phase (see Fig 1). During the sampling phase, participants sampled from the two available gambles between 6 and 24 times to learn about the possible outcomes from each gamble. During the sampling phase, participants were allowed to sample the two gambles in any order they wished at their own pace and had to finish all allotted sample plays as denoted by the number of counters left on the screen.

On all rounds, the two gambles each had payouts drawn from a uniform distribution with a mean payout and payout range that were determined as follows: the mean payout for the safer gamble was randomly set between £2.5 and £3.5 with a payout range of ±£0.5 from that mean. The riskier gamble had a mean payout set exactly £0.5 higher than the safe gamble, but with a larger range of ±£3 from that mean, representing a risk-return trade-off. Partial feedback was provided for each sample play: directly above the chosen gamble, the outcome was displayed in text, while nothing appeared above the unsampled one. After completing the sampling phase, participants transitioned to the decision phase.

The decision phase on each round began with on-screen instructions stating that participants would now be asked to choose one of the two gambles they had just sampled to play out for real either for themselves or for a random other participant in the room. Participants then had to press a key to continue to the decision screen. The top of the decision screen indicated whether the recipient of the outcome of this choice would be themselves or someone else. Social information was presented to participants on this screen in the form of proportions of previous participants who had chosen each gamble to play out for real (see Figure 1B). The relative number of cartoon characters on top of each gamble illustrated the social information. The social information purportedly represented 10%, 30%, 50%, 70%, or 90% of previous participants picking the riskier option (2, 6, 10, 14, or 18 of the 20 cartoon characters). This involved a mild form of deception which was necessary in order to implement our experimental manipulation: the social information on each round was pre-determined as the key independent variable manipulated in the study and did not represent the actual behaviour of past participants.

Participants made selections by pressing the “J” or “K” key for the gamble presented on the left or right of their screens respectively. Unlike the sample plays, participants did not receive feedback for their choices. Each round concluded after participants had made their decision on the decision screen. Between the sampling and decision phases on each round and between rounds, on-screen instructions prompted participants to press any key on the keyboard to proceed. Participants were allowed to finish the sample plays and to make the final decision on each round at their own pace.

This general procedure was repeated for all 21 rounds. The 20 test rounds comprised 2 sets of the 2 (decisions for self or others) x 5 levels of social information (10%, 30%, 50%, 70% or 90% favoring the risky option), with the 10 possible numbers of sample plays for each round being independently and randomly allocated across each set of 10 rounds. In the catch round, participants were presented with a gamble that clearly dominated the other in a decision for self, with social information showing that 90% of previous participants picked the dominating gamble. The samples for the dominated gamble were drawn from a uniform distribution between 0 and 1, and the samples for the dominating gamble were drawn from a uniform distribution between 5 and 6. Participants who selected the dominated gamble during the decision phase in the catch round were deemed to have failed the catch round and were excluded from the analysis. The order of appearance of all test and catch rounds was randomised for each participant to prevent order effects. At the end of the experiment, exactly as described to participants at the outset of the experiment, two rounds (one for self and one for other) were selected randomly and realized, and participants were paid accordingly.

Data Processing and Analysis

The primary dependent measure was the proportion of risky choices for each level of social information and/or number of samples. In addition, post-hoc, we created a second dependent measure of social influence score, which represented the degree to which participant's choice was influenced by the displayed social information. The social influence score replaced the original pre-registered dependent measure, which was abandoned because it did not take into account the baseline risk preference for each participant in the neutral social information condition. The social influence score was derived by calculating the difference, for each participant, between their baseline risk level (from the 50% social info) and the proportion risky choice with social information. In the 10% and 30% social information conditions, where the social information indicated that the majority chose the safer option, social influence scores were derived by subtracting the proportion of risky choices from their baseline. Conversely, in the 70% and 90% social information conditions, the calculation was reversed, and social influence scores were derived by subtracting their baseline from the proportion of risky choices in each condition. Positive and negative social influence scores thus represent proportions of decisions deviating away from one's baseline risk preference towards or away from the social information, respectively.

Analyses of the proportion risky choice generally followed our pre-registered data analysis plan, except where indicated. Data processing, plotting, and statistical analyses were conducted using R Studio (Version 1.1.383) and SPSS (Version 22). Bayesian ANOVAs were calculated in R using 'anovaBF' function from Package 'BayesFactor'. Because no random factors were specified, the null model assumed by 'anovaBF' was the grand-mean only model. The analyses with the social influence scores were all conducted post-hoc.

Results

Overall, participants chose the risky option 58.7 ± 9.1% of the time for themselves, very slightly less than the 60.2 ± 9.5% when choosing for others. Figure 2A shows the mean proportion of risky decisions for each level of social information. Participants were sensitive to the level of social information, picking more riskily when the social information indicated that others had done so, but not differently for self and other. A 3 x 2 repeated-measures ANOVA confirmed a main effect of Social Information strength (70%-90% as riskier and 10%-30% as safer, and 50% as neutral social information) (F (2,126) =17.87, p <0.001, η_p^2 = 0.221), but no significant effect of Recipient F (1,63) =.68, p =.413, η_p^2 = 0.011), nor any interaction (F(2,126) =0.68, p =.509 , η_p^2 = 0.011). Ex-post statistical power analyses indicated that, given the sample size, this test had 65% power for detecting an effect size d = 0.3, 97% for d = 0.5, and 100% for d = 0.7. We also conducted a Bayesian version of the ANOVA to quantify the support for the null effect of Recipient. Here we found decisive evidence in support of the model in which there were no differences between the conditions: BF_{10}= 0.113.

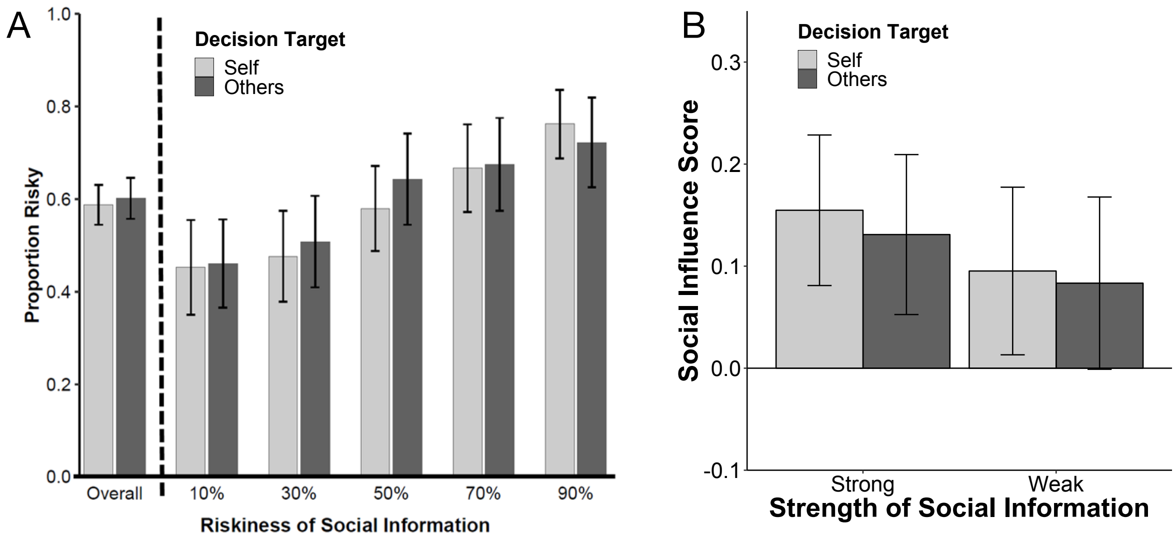

Figure 2. Results from Experiment 1. (A) Proportion of risky decisions increases as a function of the displayed percentage of previous participants choosing the risky gamble and the overall social influence. (B) Social influence score as a function of strength of social information (strong or weak) and decision target (self or other). Error bars represent 95% confidence intervals (CI) of the mean.

Participants were, however, very sensitive to the difference in reward between the safe and risky options on trials with neutral social information (50%), as indicated by a correlation between proportion risky and the magnitude of the difference across trials, $r(61) = .379$, $p = .002$

To further evaluate how social information impacted decisions for self and others, we calculated a social influence score (see methods), which indicated how much people deviated from the baseline risk preference due to the social information. Figure 2B shows, how, on average, people altered their risk preference by 11.6 ± 4.0 percentage points based on the social information (one-sample $t(62) = 5.81$, $p < .001$, $d = 0.731$). The average social influence score was marginally higher (5.36 percentage points, with 95% CI [-0.010, 0.117]) percentage points) for the strong social information [$F(1, 62) = 4.23$, $p = .044$, $\eta_p^2 = 0.064$], but there was no reliable effect of recipient [$F(1,62) = 0.302$, $p = 0.584$, $\eta_p^2 = 0.005$], nor any interaction [$F(1,62) = 0.042$, $p = .839$, $\eta_p^2 = 0.001$]. Ex-post statistical power analyses for the factor Recipient indicated that, given the sample size, this test had 64% power for detecting an effect size $d = 0.3$, 97% for $d = 0.5$, and 100% for $d = 0.7$. We also conducted a Bayesian version of the ANOVA to quantify the support for the null effect of recipient. Here we found decisive evidence in support of the model in which there were no differences in social influence sore based on the recipient of the choice: $BF_{10} = 0.16$

A further question was whether social information would have less of an impact as participants had more experience. Figure 3 illustrates how, contrary to our expectations, the impact of social information, as indexed by the social influence score, actually grew slightly with the number of samples. To quantify this pattern, we conducted a linear-trend analysis. Simple linear regressions were fit for each participant, using the number of samples as the predictor variable and social influence score (averaged across strong and weak) as the predicted variable. The average predictor coefficient (b = 0.0068, 95% CI [0.0018, 0.012]) was significantly different from 0, one-sample *t*(62) = 2.73, *p* = .008, *d* = 0.34. This result means that with each additional sample drawn, the percentage of decisions that were aligned with the majority decision in the social information grew on average by 0.68 percentage points.

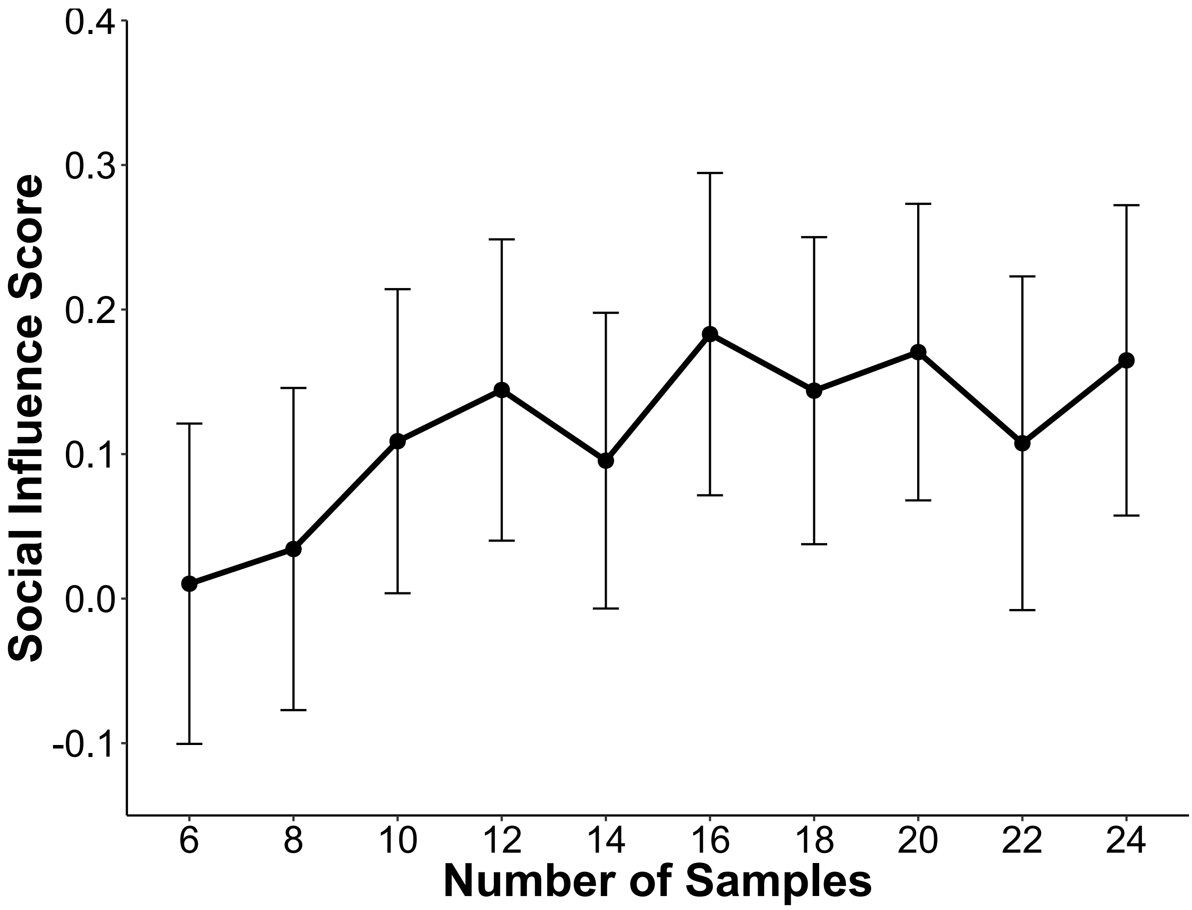

Figure 3. Average participant social influence score increased as a function of number of pre-decision samples, illustrating a positive relationship between the number of samples and compliance to social information. Error bars represent 95% confidence intervals across participants.

Discussion

We implemented a decision-making experiment using a decision-from-experience paradigm focusing on how two aspects of social context may influence risky decisions for self and others: the recipient of the risky decision (Self or Other) and the strength of information about how others have chosen when faced with the same decision (Neutral, Weak, or Strong).

First, we found evidence to support the prediction (P1) that participants' risky decisions from experience would be significantly influenced by the social information. We did not, however, find evidence to support the prediction (P2) that decision-makers are more likely to be influenced by social information when making decisions for others than when they are deciding for themselves. There also was no significant interaction between the recipient of decision outcomes and the strength of social information, although stronger social information did result in significantly higher proportions of decisions aligning with the social information for both self and other.

Thus, the results do not support (P3), i.e., social influence would be greater when the social information is strong than when it is weak. These two predictions (P2 and P3) were motivated by the hypothesis that decisions for others would comply with social information more, because social information might serve as a possible justification when making decisions for others, helping to avoid blame in the event of an unfavourable outcome.

In Experiment 1 we also sought to explore the relationship between the amount of experience a decision-maker had, operationalised by the number of samples, and the influence of social information when making risky decisions from experience. We predicted (P4) that there would be a negative relationship between the number of samples and the alignment with social information – i.e., participants' decisions would align less to social information when they had more samples. Unexpectedly, we found that decisions were more likely to align with social information when there were more opportunities to sample. One speculative explanation for this finding is that participants may have reasoned that the other participants had more information by virtue of the longer sampling period, so the social information was more likely to be reliable. Of course, they themselves also had more information, but the reliability attributed to the social information may have increased more as a result of the extended sampling period than the reliability attributed to their own private information.

Finally, we did not find evidence to support either the prediction (P5a) that participants would make more risk-seeking choices and earn more points when deciding for the partner, nor the opposite prediction (P5b) that they would make more risk-averse choices when deciding for their partner

Experiment 2

The results of Experiment 1 did not reveal any significant differences in participants' willingness to take risky decisions when the outcome at stake was their own compared to when it was the outcome of a randomly selected other participant.

To further test the robustness of our findings in Experiment 2, we used another experience-based risk task: the Balloon Analogue Risk Task (BART). In the BART, participants are required to pump a virtual balloon to accrue points, which are converted to money. At any point, a participant can bank and save all earnings from a given trial. If the balloon bursts, however, all earnings for the trial are lost. The BART has the further virtue of being better correlated with real-life risk taking than many other behavioural measures (Frey et al., 2017; Hunt, Hopko, Bare, Lejuez, & Robinson, 2005; Lejuez et al., 2002).

In Experiment 2, we also sought to further test predictions of the construal level theory by manipulating the social distance to the partner and having our participants earn money in the risk task for themselves, for a friend, or for a stranger. In the BART task, the maximal expected earnings are reached when the balloon is pumped exactly 64 out of the 128 maximum times (Wallsten, Pleskac & Lejuez, 2005), yet people tend to be risk averse and pump well below that (e.g., Lejuez et al., 2002). Thus, in this task, more risk-seeking behaviour typically leads to more earning and, extending our earlier prediction (P5a), we expected that:

(P6) *Risk-seeking behaviour should increase with social distance, and therefore participants in should earn more money in the BART for a friend than themselves, and even more money for a stranger than for themselves.*

Data and materials for Experiment 2 can be viewed at https://osf.io/rgjt6/?view_only=98639867fb59432f8ba020d60144c2d8/. Experiment 2 was exploratory, and the hypotheses of this study were not pre-registered.

Methods

Participants

75 participants signed up to participate in the study through the University of Warwick participant pool. Each participant also brought one friend to the lab so there were 150 participants in total (97 female, M_{age}= 21.3, SD_{age}= 2.7). We sought at least 45 participants per group to achieve 80% statistical power to detect a medium (f = 0.27) effect size (based on the omnibus one-way ANOVA with mean differences in mean adjusted pumps of 5, with standard deviations of 15). Computer error led to data loss from three participants, and one participant failed the manipulation check, resulting in the final sample of 146 responses (self: 52, friend: 48, stranger: 46; sufficient to detect an effect size of f = 0.26).

The recruitment page specified that each participant would earn a minimum of £3 and a maximum of £15 for approximately 30 minutes of their time. They were aware that the £3 flat fee would be payable in cash at the end of the experiment, but that the remaining earnings, which could be based on their performance, would be transferred into their student account within 5 working days from the day of the experimental session. Each person was further told that the only way to be eligible for the study was to bring a friend to the lab with them. They were also informed that the friend would be treated as a participant and would be rewarded in the same way as them.

Materials

Balloon Analogue Risk Task

The classic 30-round version of the BART was used in the study (Lejuez et al., 2002). We used the version implemented in the Inquisit software package (Version 5.0.010.0). In the task, participants were shown an image of a balloon and told that they could earn money by pressing on the "pump up the balloon" button. Figure 4 shows a screenshot in task, wherein the size of the balloon increased incrementally with each pump. Earnings from each round could be secured and added to the total (here bank) by pressing on the "collect points" button. If the balloon burst, all earnings for that round would be lost. The objective of the task was to earn as much money as possible. In the task, the point of explosion was random for each balloon, sampled without replacement from a list ranging from 1 to 128. Every ten points gained in the task equated to a gain of £0.05.

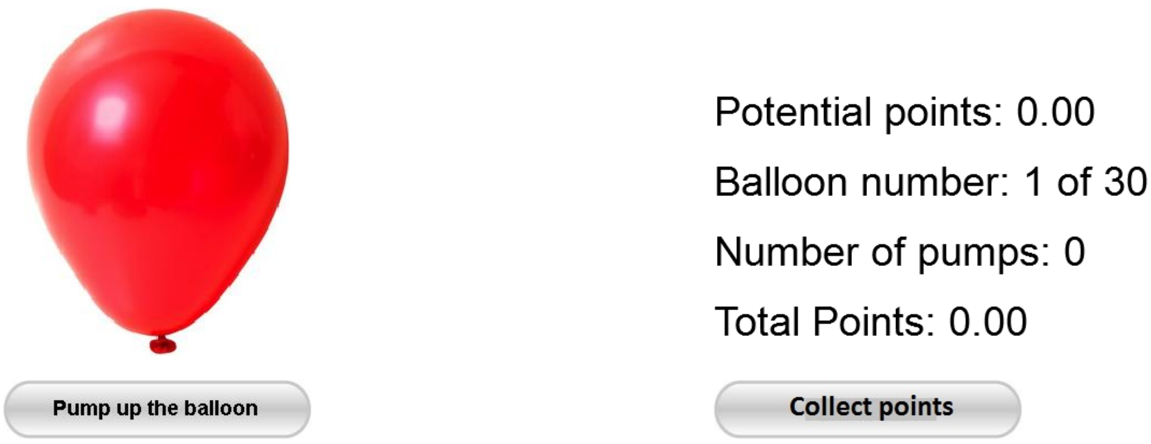

Figure 4. Screenshot from a trial of the BART task using the Inquisit software package. The instructions for the BART were modified for the purpose of the present study. Participants were informed that they would play the game to earn money for the person whose name they found in the envelope on their desk (see procedure for further explanation).

Self-reports of risk attitudes and personality.

After completing the BART, participants were asked to complete a computer-based questionnaire. As a manipulation check, participants were asked to confirm that they were aware for whom they had played the BART game, selecting one of the three options: “myself”, “a friend who came with me to the experiment”, “other people in the session”. They were also asked to type in the name of the person for whom they played the game. If participants had not played for themselves or for the friend who came with them, they were asked whether they personally knew the person (stranger) for whom they played (all participants selected “no”). All participants were also asked how long they had known their friend (in months, ranging from 0 to 100), how much time they spend together (in hours per week, ranging from 0 to 150), and how close they are to them (on a scale from 1 to 10, ranging from not close at all to extremely close).

Participants were also asked about their own risk-taking behaviour. First, they were prompted to indicate how risky they think they are, relative to other people. They were shown a picture of a row of people who were ordered from “people who are least likely to take risks” to “people who are most likely to take risks”, and instructed to click on a location within this row to indicate where they thought they fell in terms of their risk taking (ten categories). Those who completed the BART for others were also asked to answer the same type of question about the person with whom they were partnered. Finally, for exploratory purposes, all participants completed a 30-item DOSPERT to assess their risk taking in five domains of life: financial, health and safety, recreational, ethical and social (Weber et al., 2002). Participants also completed a 30-item BFI personality measure, assessing five personality traits: openness, conscientiousness, extraversion, agreeableness, and neuroticism (Konstabel et al., 2017).

Procedure

Sessions were conducted in groups with four to six pairs of participants. Random allocation to conditions occurred at the group level, so all participants in a single session were allocated to the self, friend, or stranger condition. Participants were seated in individual computer cubicles and first listened to the instructions which were read by the experimenter. At this point they were informed that the envelope placed in front of them contained the name of a participant, present at the lab, who would be the recipient of the earnings in the balloon pumping task. Participants were asked to open the envelope and to start the computer task.

On the screen, they were shown instructions explaining how the BART works and were reminded that all earnings made in the game would go to the person whose name they had found in the envelope (see Appendix for verbatim instructions). They were also informed that if they were not playing for themselves, their own earnings would be determined by the performance of another person in the room who had found their name in their envelope. Upon completing the BART, participants were redirected to the online survey, where they were asked about their demographics (age, gender), knowledge of the person they played the game for, their association with their friend, their risk taking (both relative risk and partner's risk taking as well as DOSPERT), and personalities (30-BFI), in that order (See supplementary material).

After completing all tasks, participants were paid the show-up fee. The earnings from the BART were transferred to their student accounts within five working days, based on the performance of whoever had their name in the envelope (self, friend, stranger), exactly as described to the participants.

Results

Figure 5 shows how there were very small differences in the mean adjusted pumps between the three conditions (M_{self} = 43.1 ± 5.0; M_{friend} = 46.5 ± 4.5; $M_{stranger}$ = 43.2 ± 4.5). We tested whether condition made a difference to risk-taking in the BART with a one-way ANOVA, including condition as a factor and mean adjusted pumps as the dependent variable. Mean adjusted pumps was defined as the average number of pumps on trials that did not end with a balloon bursting. Consistent with Figure 5, there was no effect of condition, $F(2, 143) = 0.65$, $p = 0.52$, $\eta_p^2 = 0.009$. Ex-post statistical power analyses indicated that, given the sample size, this test had 34% power for detecting an effect size $d = 0.3$, 80% for $d = 0.5$, and 97% for $d = 0.7$. We also conducted a Bayesian version of the ANOVA to quantify the support for the null. Here we found decisive evidence in support of the model in which there were no differences between the conditions, $BF_{10} = 0.117$ (conducted using JASP with default priors).

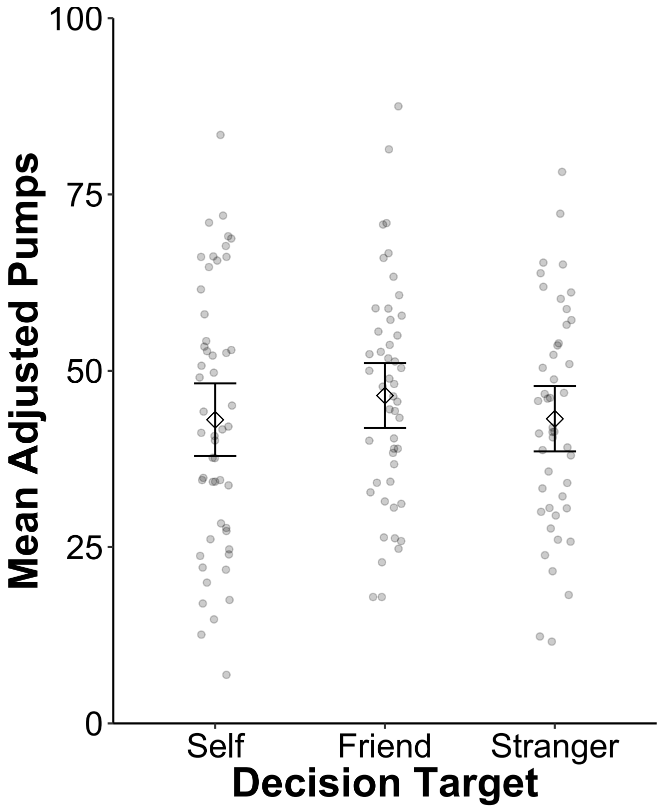

Figure 5. Mean adjusted pumps in the three conditions of Experiment 2. Dots represent individual data points, and the white rhomboids are the means for each condition. Bars represent 95% CI of the mean.

Discussion

As in Experiment 1, the results of Experiment 2 did not reveal any significant differences in participants' willingness to take risky decisions according to whether they themselves, or another person, would receive the rewards. Moreover, Experiment 2 also revealed no differences according to whether the other person was a friend or a stranger. This pattern is not consistent with the hypothesis (P6), motivated by construal level theory, that reducing social distance to the recipient of the decision-making outcome should increase risk aversion. One possible reason why we did not observe any differences between the groups was that participants did not have information about their specific partner's preferences with respect to risk. In the absence of information about their specific partner's preferences, they may have relied upon their own preferences in making decisions for others. We explored this possibility in Experiment 3.

Experiment 3

Experiment 3 investigated whether risky decision-making would differ according to whether the recipient of the decision-making outcome was individual or joint in a situation in which participants had reason to believe that the other person had different preferences regarding risk than they themselves had. Specifically, we asked whether participants would make riskier decisions when playing for joint rewards to be shared with a partner to whom they had reason to attribute a preference for high-risk decisions, compared to when they were playing for individual rewards. Here, our prediction follows from the blame-avoidance account, as knowing that a partner is risk seeking should encourage participants to be risk seeking when the outcomes are shared. Specifically, we predicted that:

(P7) *participants would make more risky decisions for a team than for themselves, given that, as in the context of this experiment, they have reason to believe that their teammate is risk seeking.*

We also aimed to test whether their willingness to take riskier decisions could be affected by increasing the degree of coordination between participants and their partners. Construal level theory and the blame avoidance hypothesis lead to opposite predictions in this case. The prediction based on the construal level theory is that coordinated action reduces psychological distance, and therefore should results in more risk-averse behaviour. Accordingly, construal level theory predicts:

(P8a) *that participants would make less risky decisions when coordinating with a risk-seeking partner than when not coordinating.*

The blame-avoidance hypothesis predicts that a high degree of coordination may lead participants to make decisions that more closely accommodate the preferences they ascribe to their partners. Accordingly, the blame-avoidance hypothesis predicts:

(P8b) that participants *would make more risky decisions when coordinating with a risk-seeking partner than when not coordinating.*

We tested these hypotheses using the BART, by manipulating (1) whether participants played for themselves or for a joint outcome and (2) whether they were coordinating or not with another player during the balloon pumping task.

Method

Participants

We determined that a sample size of 76 would give 80% power for finding a small-to-medium-sized effect ($\eta_p^2 = .025$) with an alpha of 0.05, assuming a two-way repeated-measures ANOVA on normally distributed data. For the experiment, 79 participants (50 females, 28 males, 1 undisclosed) were recruited through the University of Warwick's online experiment recruitment system and were between the ages of 19 and 49 ($M_{age} = 24.3$, $SD_{age} = 4.6$). This slightly exceeded the pre-registered sample size of 76 because the experiment was conducted in large groups of 10-12, and participants sometimes cancelled, making it impossible to precisely target the desired sample size. Instead, the recruitment procedure was repeated until the target of 76 was met. Participants were paid a £4 show-up fee and an additional bonus of up to £7 based on their earnings. Two participants were excluded prior to analysis because there was at least one condition in which they never ended any trials prior to the balloon bursting, which meant there was no data for them for that condition, leaving a total sample size of 77. All procedural details, including hypotheses, recruited participant numbers, exclusion criteria, and planned analyses were preregistered at the Open Science Framework: https://osf.io/8ty5m/?view_only=6364bb3c436f47cbb010fa73daa093c2

Materials

The program for BART was written using PsychoPy (Peirce, 2007). The experimental task was preceded by a phase in which participants selected an avatar from a predetermined list to represent them during the experiment. The avatars were used to indicate whose outcome was at stake. When only the participant's individual outcome was at stake, only the participant's avatar was displayed; when a joint outcome was at stake, both the participant's and their partner's avatars were displayed. We also administered a paper-and-pencil version of the DOSPERT questionnaire.

Procedure

After giving informed written consent, participants were seated, in groups of 10-12, at desktop computers in individual workspaces within a larger lab space. Participants first selected their avatar and were then paired with a partner. This pairing process involved a very mild form of deception: participants were in fact paired with a virtual partner, who was programmed to make highly risk-seeking choices. This was necessary in order to maintain experimental control over the partner's choices, and more specifically to implement our manipulation—i.e., it enabled us to probe participants' decisions when paired with a partner whom they had reason to believe preferred a higher level of risk than they themselves did. Participants were informed that their bonus payment would be calculated based upon the number of pumps they achieved without bursting balloons (verbatim instructions are in the Appendix). At the end of each experimental block, participants were informed of their earnings during that block. At the end of the experiment, the bonus payment was determined by calculating the mean number of pumps per trial during the individual baseline block, and dividing by ten—i.e., if the mean was forty, the participant received a bonus of £4, up to the maximum bonus payment of £7.

To establish an individual baseline, each participant performed one baseline block of ten balloons individually. They were then informed of their partner's baseline. The partner's baseline was calculated using the following formula: Partner's average baseline = Own average baseline + 0.25*(Max pumps – own average baseline). They were not informed of the partner's earnings during the partner's baseline block. This baseline block was followed by the four test blocks, one for each of the four experimental conditions, in counterbalanced order.

In the Decisions-for-Self conditions, participants performed the task for rewards which accrued to them alone. In the Decisions-for-Team conditions, they performed the task for rewards which accrued to them and their partner jointly. The overall rewards in these blocks were twice as high as in the Decisions-for-Self conditions; this increase ensured that, when the rewards were split with the partner, participants could still achieve the same maximum payoffs as in the Decisions-for-Self conditions.

In the Coordination conditions, participants and partners alternated pressing the key to pump the balloon, but it was always the participant who had the role of deciding when/whether to cash out and end the trial. In the No-Coordination conditions, participants pressed the key to pump the balloon while the partner waited. Participants decided when to cash out and end the trial.

Each test block consisted of ten trials (i.e., ten balloons). Participants were informed that for each test block, either they or their partner would decide when to end each trial. Because we were only interested in when participants decided to end trials, the participant was always selected to play the role of deciding when to end the trial. To make it plausible that this allocation was due to chance, we inserted a filler block in the middle of the four test blocks. The filler block had the same format as the No-Coordination condition with the partner in the role of the balloon pumper and decider, and playing only for the partner's individual rewards -- i.e., the participant watched passively as the partner pumped the balloon, and the partner decided when to end the trials.

At the end of the experiment, participants completed the DOSPERT questionnaire while awaiting payment (See supplementary material).

Results

Figure 6 shows how exposure to a more risk-seeking partner influenced participants' decisions, leading them to take greater risks even when working towards their own rewards. A two-tailed paired-sample t-test confirmed that participants made significantly more mean-adjusted pumps in the No-Coordination-Decisions-for-Self condition (Mean ± 95% CI: 54.0 ± 5.4) than in the baseline condition (M = 36.3 ± 5.8), $t(76) = 8.53$, $p < 0.001$, $d = 0.46$.

Next, we conducted a two-way repeated measures ANOVA with Coordination (Yes vs. No) and Decision-Outcome Recipient (Self vs. Team) as two within-subject factors. The results revealed no significant main effects of either Coordination or Decision-Outcome Recipient. The overall mean adjusted number of pumps was not different between the Coordination (52.4 ± 5.0), and the No-Coordination conditions (52.6 ± 5.3), $F(1, 76) = 0.22$, $p = .64$, $\eta_p^2 = .003$. Nor did we observe any significant difference in the overall mean adjusted number of pumps between the Decisions-for-Team (52.8 ± 5.2), and the Decisions-for-Self Conditions (M = 52.3 ± 5.1), $F(1, 76) = 0.05$, $p = .82$, $\eta_p^2 = .001$. As in Experiment 1, we also conducted a Bayesian version of the ANOVA to quantify the support for the null for the two factors Coordination and Decision-Outcome Recipient. Here we found decisive evidence in support of the model in which there are no differences between the conditions: $BF_{10} = 0.13$ for Coordination and $BF_{10} = 0.12$ for Decision-Outcome Recipient.

There was, however, a significant interaction between these two factors, $F\,(1, 76) = 8.21$, $p < .01$, $\eta_p^2 = .098$. Looking at Figure 6, we can see that the mean differences between the groups were small. These small differences between Coordination and No-Coordination conditions, however, went in opposite directions for the Decisions-for-Self and the Decisions-for-Team conditions, leading to the significant interaction. Post-hoc pairwise comparisons using a Bonferroni correction (alpha = .0125) revealed no significant difference in mean adjusted pumps between the Decisions-for-Self and the Decisions-for-Team Conditions when there was coordination ($t(76) = 1.79$, $p = .077$, $d = 0.21$), nor when there was no coordination ($t(76) = 1.57$, $p = .122$, $d = 0.18$). Similarly, there was no significant difference in mean adjusted pumps between the Coordination and the No Coordination Conditions when participants decided for the team ($t(76) = 1.35$, $p = .182$, $d = 0.15$), nor when they decided for themselves, though this latter comparison was borderline, $t(76) = 2.44$, $p = .017$, $d = 0.28$. Ex-post statistical power analyses indicated that, given the sample size and an alpha level of .0125, all these tests had 57% power for detecting an effect size $d = 0.3$, 99% for $d = 0.5$, and 100% for $d = 0.7$.

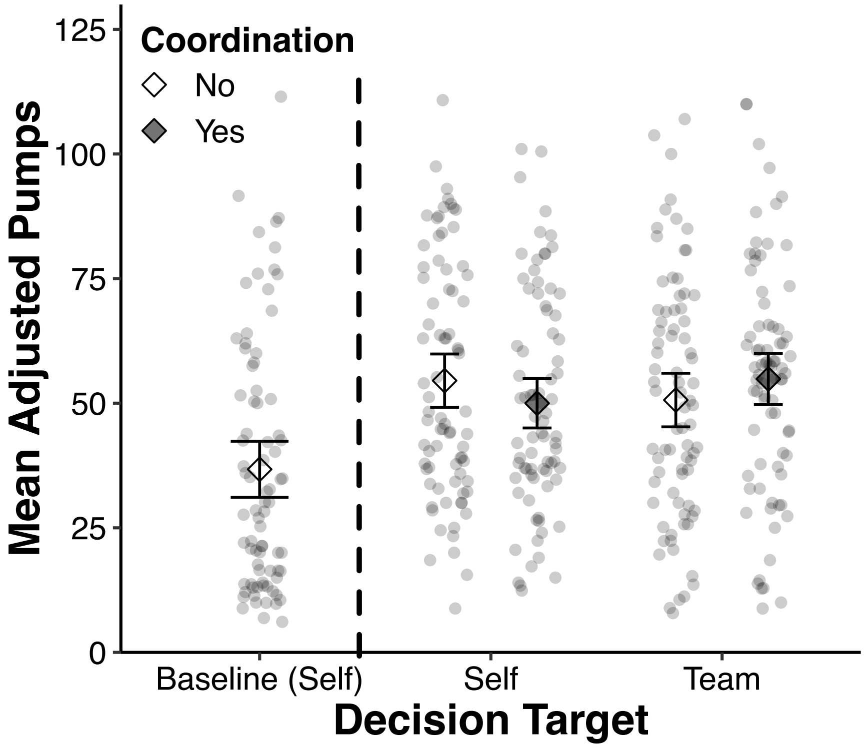

Figure 6. Mean adjusted BART score for the baseline condition and for the test conditions, split by the recipient of the decision outcome and whether or not there was coordination. Dots represent individual data points, and rhomboids represent the mean. Error bars represent 95% CI of the mean.

Discussion

Experiment 3 was designed to investigate whether participants would make riskier decisions when performing the BART for rewards to be shared with a riskier partner, compared to when they were playing for individual rewards. We also aimed to test whether willingness to take riskier decisions could be increased by increasing the degree of coordination between participants and their partners.

People made riskier decisions when having been exposed to a risk-seeking partner, compared to their own individual baseline. This increase indicates that observing the choices of a risk-seeking person influences one's preferences for risk taking. Neither the degree of coordination nor the decision target, however, had a significant impact across the board. Participants did not make riskier decisions when coordinating with a risk-seeking partner than when not coordinating (P8a; P8b), and they did not make riskier decisions when performing the task for team rewards than when only their own individual outcome was at stake (P7).

General Discussion

Across three experiments, with nearly 300 participants, we found that participants' decisions about risk did not differ consistently according to whether it was their own outcome that was at stake, or that of a friend, a stranger or a teammate. They consistently chose for others as they chose for themselves.

These results have important implications for theoretical proposals which have been developed to account for differences between risky decision-making for self and others. For example, construal level theory hypothesizes that decision-making for others may be characterized by greater psychological distance than decision-making for oneself. As a result, people may use relatively abstract, context-independent and reason-driven considerations when deciding for others, which would lead to participants being more risk seeking when deciding for others than for themselves. The results of Experiments 1 and 2, in which we found no differences between self and other, are not consistent with this prediction. In Experiment 2, we also manipulated the nature of the relationship with the recipient (Friend vs Stranger). Assuming the social distance to a friend is less than the social distance to a stranger, construal level theory would predict that participants would make more risk-seeking decisions for a stranger than for a friend. Again, however, we observed no reliable differences. In Experiment 3, if we conceptualize the coordination manipulation as reducing social distance to the partner, then construal level theory predicts more risk-averse decision-making in the Coordination than in the No-Coordination condition. In fact, however, the results do not show this pattern.

One possible explanation for why we did not observe a significant difference between coordinating and not coordinating when deciding for joint rewards with a risk-seeking partner is that coordination may have triggered two distinct factors pulling in opposite directions: one the one hand, coordination may have made participants more inclined to choose as they believed the partner wanted (i.e., more risk seeking). This tendency is consistent with research showing that coordination increases a sense of commitment to a partner, increasing the willingness to act in accordance with the expectations one takes that partner to have (Michael, Sebanz & Knoblich, 2016; Rusch & Luetge 2016). On the other hand, coordination may have reduced psychological distance to the partner, leading participants to choose in a more risk-averse fashion.

A further theoretical proposal that has been offered in the literature is that differences in decision-making for self and others may be driven by an anticipation of blame in the event of a negative outcomes for others (Botti, Orfali, & Iyengar, 2009; Simonson, 1992; Stone et al., 2002; Wang et al., 2017). Kray (2000) suggested that people making decisions for others were more likely to choose options that scored highest on the most important choice attributes, as this would ensure a valid justification that could help reduce any potential blame if the outcome is suboptimal. This line of thought suggests that in Experiment 1, participants would place more weight on social information when deciding for their partner than when deciding for themselves. This emphasis arises because information about the choices previously made by other participants would provide a basis for justifying the decision and thereby minimizing blame. The results of Experiment 1, however, do not corroborate this prediction.

The blame-avoidance hypothesis also generates the prediction that participants in Experiments 1 and 2 should make more conservative choices, and thus be more risk averse when deciding for others, in order to minimize the risk of blame. Again, our results do not support this hypothesis. As a further test of this idea, participants in Experiment 3 were provided with information about their specific partner's personal preference with respect to risk tolerance. Under these circumstances, we reasoned that the blame-avoidance hypothesis would predict *greater* risk-seeking. Insofar as blame-avoidance is a critical factor in decision-making for others, participants should be inclined to use this information to align their decisions with the decisions their partner could be expected to make. Given that they had reason to ascribe a preference for high risk-seeking to their partners in Experiment 3, this should lead to greater risk-seeking. And yet we do not see this pattern. One possible explanation is that different motives aiming to avoid blame pulled in different directions in this context. That is, the information that participants had about their partner's preferences may have elicited a tendency to accommodate those preferences, which would have pulled participants towards more risk-seeking decisions. At the same time, a distinct motive to avoid very bad outcomes may have neutralized any such tendency by pulling participants towards more conservative decision-making.

A different lens through which to view the current results is the learning of the preferences of others (Behrens et al., 2008). The consistency with which participants chose the same option for their partner as for themselves may be due to participants applying their own risk preference when making decisions for others, because they did not know what preferences their partners held. In other words, the results of Experiment 1 showed no difference between decisions made for self and for other because participants were making decisions for "a random other participant in the room" without knowing their preferences. Similarly, it may be that there was no difference in decisions for a stranger or friend in

Experiment 2 because participants were not aware of the risk preferences of their partners. Supporting this interpretation, the ratings of strangers' and friends' riskiness were not associated with number of pumps participants made for each type of partner. In Experiment 3, however, participants had direct experience of their partner's preference and may have adapted this not only for the team decisions, but also for their own, possibly because they adopted a new strategy in the task by learning from their partner (Suzuki, Jensen, Bossaerts, & O'Doherty, 2016).

This interpretation suggests that in order to decide differently for others than for oneself, decision-makers may need to be aware of any differences between their own preferences and the preferences of the person for whom they are deciding. In other words, the absence of knowledge about the preferences of others in Experiments 1 and 2 may explain why participants chose for others as for themselves. In contrast, when participants were provided with information about the preferences of the other (Experiment 3), we observed a shift in participants' risky decisions (cf. Suzuki et al., 2016). After learning the risk preference of their partners, participants adjusted their risk preferences to correspond more to their partner's when they were coordinating as a team. If this is correct, it implies that such factors as generic guilt- and blame-avoidance are secondary insofar as their influence will strongly depend upon what people believe about the preferences of the other individual.

The findings observed here provide reason to doubt that there are any *general* differences in attitudes towards risk between decision-making for self and decision-making for others. Rather, it may be the case that some factors incline people towards more risky decision-making for others, whereas some other factors incline people towards less risky decision-making for others. If so, we may observe more or less risky decision-making for others than for oneself, or no difference at all, depending on which factors predominate. This interpretation is consistent with the findings of a recent review and meta-analysis, which concluded that “self-other decision making differences are robust – the differences may not produce large effects, but they yield many small- to-medium effects. And because these differences occur in different kinds of decisions and situations, there does not appear to be an overall risky or cautious shift that manifests across all decisions” (Polman & Wu, 2019: 34). Indeed, building on this point, our findings highlight the possibility that some factors may have different effects in different contexts: the results of Experiment 3 suggest that coordination with a teammate may have opposite effects on risky decision-making depending on whether one is deciding on behalf of oneself or on behalf of the team (see Fig 6). If this is correct, it would contribute to explaining the mixed pattern of results that we find in the literature, as well as the lack of clear differences in the three experiments here.

Further research should probe this possibility by attempting to identify more specifically the circumstances under which psychological distance and blame-avoidance may be expected to predominate. Moreover, it would be important to explore individual differences in the motives underlying decision-making for others. As Olschewski, Dietsch, & Ludvig (2019) have shown, for example, competitive motives may lead some people to make suboptimal decisions for others. It would also be valuable to further investigate the effects we observed of social information upon risky decision-making. In particular, in Experiments 3, participants wound up earning more money as a result of integrating information about how others had chosen. This may have important practical implications, insofar as it suggests that pairs of people with conflicting biases may make better decisions when exposed to each other’s risk preferences than either would make alone. Future research is needed to explore the conditions under which such dynamics may unfold.

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Statement of Declaration of Interests

The authors declare no competing interests.

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Supplementary Material

Experiment 2: Self-Reported Measures

Overall, self-rated relative risk attitude (relative risk standing) was positively and significantly correlated[1] with the mean adjusted pumps in the self ($r(48) = .32$, $p = .024$) and stranger conditions ($r(43) = .33$, $p = .03$), but not when participants played for their friends ($r(37) = .11$, $p = .52$). Additional analyses showed that there was no significant correlation between self-rated risk-taking and judged risk-taking of others, in neither the Friend ($r(36) = .09$, $p = .59$), nor the Stranger condition ($r(42) = 0.11$, $p = .50$). In the Friend condition, ratings of riskiness of a friend was not significantly correlated with BART performance, $r(40) = -.11$, $p = .49$. The same was true of the ratings of strangers' risk attitudes in the Stranger condition, $r(42) = -.22$, $p = .15$. Thus, overall, self-reported attitude towards risk were associated with performance on the BART when participants played for themselves and for strangers, but the ratings of the riskiness of the strangers and friends were not associated with the mean adjusted pumps.

Table S1 shows correlations between individual domains of the DOSPERT and mean adjusted pumps across the entire sample. None of the measures correlated significantly with the behaviour on the BART task.

Table S1. ***Correlations of DOSPERT risk domains with the mean adjusted BART scores***

Risk Domains	**Pearson's *r***	***p*-value**
Social	.06	.47

[1] The correlations reported above exclude individuals who failed to properly answer the risk attitude question (by for example selecting more than one position in the relative risk standing).

Recreational	.02	.79
Gambling	.08	.35
Health	.09	.30
Ethical	.08	.37
Investments	.05	.54
Investments and Gamble Domains (combined)	.08	.33

Note: DOSPERT scores were calculated by taking the average of the responses for items in each domain.

Table S2 shows results of the exploratory analyses using people's personality scores. Here, none of the five traits were significantly correlated with the BART performance.

Table S2. ***Correlations of the Big Five Personality Traits with BART score***

Big Five Personality Traits	**Pearson's *r***	***p*-value**
Openness	-.12	.20
Conscientiousness	.001	1.0
Extraversion	-.06	.50
Agreeableness	.04	.63
Neuroticism	-.04	.64

Note: Personality trait scores were calculated by taking the average of the responses for items in each domain.

Experiment 3: Self-Reported Measures

We also correlated DOSPERT scores with the baseline BART score. The results revealed significant correlations between the baseline BART score and the overall DOSPERT score as well as with the investment-risk score (See Table S3).

Table S3. **Correlations of DOSPERT risk domains with the baseline adjusted BART score.**

Risk Domains	**Pearson's *r***	***p*-value**
Social	.20	.80
Recreational	.15	.18
Gambling	-. 1	.95
Health	.14	.21
Ethical	.22	.05
Investments	.29	.008**
Overall	.28	.014*

Note: DOSPERT scores were calculated by taking the average of the responses for items in each domain. Asterisks represent the significance level: *$p<.05$** $p < .01$.*

APPENDIX

Experiment 1 Instructions

The instructions below are shown on the computer screen before participants start the task:

Welcome! Please read all instructions very carefully. In this experiment, you will choose between 2 virtual slot machines. The different slot machines will be indicated by images on the screen. Make your selections by pressing “J" to select the left option, or "K" to select the right option.

There will be a total of 21 rounds, with different slot machines each round. Each round, you will have between 6 to 24 sample plays to learn the machines possible payouts. The number of sample plays left will be indicated by the counters at the top of the screen. After finishing your sample plays on each round, you will choose one slot machine to play for real.

On some rounds, you will play the slot machine for yourself, while on others, you will play the slot machine for a random other participant in this room. Who you are playing for will be stated clearly on the decision screen where you make your choice. On this screen, you will also be shown information about the proportion of previous participants that chose each slot machine. This will be illustrated using the number of characters above each machine. The next screen will show an example of the information you will see on the decision screen of each round.

Here's an example. The proportion of characters above each machine reflects previous participant choices. On an actual decision screen, information about who you are playing for (yourself or someone else) will be stated here.

During the experiment, you will only see the payouts from each machine during your sample plays. The payouts from the slot machines you choose to play for real are recorded, but not displayed to you. Your final payment will be the show-up fee plus the average of the payout of one randomly-selected machine you played for yourself, and the payout of one randomly-selected machine someone else played for you.

As your payment is partly determined by the choices of other participants, you may have to wait for a few moments after you finish the experiment before you payment can be calculated and prepared. Remember that the slot machines reset on every round, so you have to use 'your sample plays during each round to learn which machine is the best.

When you are ready, please press any key to begin the experiment.

Experiment 2 Instructions

Verbally (and on a form):

In this experiment, you are going to play a computer-based balloon pumping task to gain points that will determine the total amount of money that will be earned. Each 10 points (pumps) earn you 5 pence. You can press on “bank” and secure your winnings for that round. If your balloon bursts, you don’t earn any points for that round.

The envelope in front of you contains a name of a participant. This is the person you will be play the balloon game for. In other words, the money you earn will go to that person. The envelope could contain your own name in which case you will be playing for yourself. It could also contain the name of any other participant in the lab today.

You will earn a guaranteed £3 as a show-up fee and potentially earn a total of up to £15 based on your own or other players’ performance.

The show-up fee will be given to you in cash at the end of the study. The reward from the balloon task is going to be transferred directly into your university account no later than 5 working days after the experiment. Once the payment has been processed we will delete your student ID from our records.

Please look into your envelope right now and write the name you find inside below:

In the computer-based balloon task, I will be earning money for:

____

On the computer screen:

1st screen:

Now you’re going to see 30 balloons, one after another, on the screen. For each balloon, you will use the mouse to click on the button that will pump up the balloon. Each click on the mouse pumps the balloon up a little more.

BUT remember, balloons pop if you pump them up too much. It is up to you to decide how much to pump up each balloon. Some of these balloons might pop after just one pump. Others might not pop until they fill the whole screen.

You get MONEY for every pump. Each 10 pumps earns you 5 pence. But if a balloon pops, you lose the points you earned on that balloon. To keep the money from a balloon, stop pumping before it pops and click on the button labelled “Collect points”.

After each time you collect points or pop a balloon, a new balloon will appear.

Remember that you are playing this game for the person whose name you found in the envelope.

ALL EARNINGS MADE IN THIS GAME WILL GO TO THAT PERSON.

Click the left mouse button to see the summary.

2nd screen

Summary

- You earn one point for each pump.
- You save the points from a balloon when you click “Collect points”.
- You lose points from a balloon when it pops.
- There are just 30 balloons.

Now, do you have any questions?

Click the left mouse button to begin.

Experiment 3 Instructions

Verbally:

Thank you for agreeing to participate in today’s experiment. You are about to participate in a decision-making experiment, and at the end of the session you will be paid in cash for your participation. Different participants may earn different amounts, depending upon the decisions made.

Now that the experiment has started, please do not talk with other participants during this experiment. Please remember to switch off your cell phones, or set them to ‘Silent’ mode.

Should you have any question or problem at any point in today’s experiment,
please raise your hand and I will come to you. Before you begin, please read the information sheet and sign the consent form.

Thank you for listening. You may now begin the experiment.

Prior to familiarization:

In this experiment, you will pump up virtual balloons. There will be a few rounds, and each will be a variation of a basic balloon-pumping task, in which your goal is to inflate each balloon by as much as you can without popping it.

In some rounds, you may be asked to play for yourself, which means that any rewards you earn will be added to your own winnings.

In other rounds, you may be asked to play for a partner, or together with a partner. Rewards earned may be added to your winnings, your partner's winnings, or the team's winnings.

Before each round begins, you will be told who you are playing for, and where the rewards will be added.

Try to get the highest total winnings while being as fast as you can.

Each pump is worth £0.025, and you could earn up to £6.40 on a balloon. Ten balloons will be selected during the experiment, and your bonus will be the average of the performance during these trials.

You will not know which ten balloons have been chosen until after the experiment.
Beware, each of the 10 balloons is different and may pop at different times!

First part of familiarization:

At any point, you always have two choices:

1) Inflate the balloon by one pump.

If the balloon does not burst, your temporary reward gets larger. If the balloon bursts, you get no reward for this balloon, and move on to the next one.

2) Collect the reward for the current balloon.

Your temporary reward for this balloon gets added to the wallet, and you move on to the next balloon.

Second part of familiarization:

In the second part of the tutorial, you will be familiarized with what happens when you play with your partner.

When the "Collect" and "Pump" buttons are in blue, you get to decide whether to pump the balloon another time, or collect the reward.

Sometimes, you will see just a "Pump" button in blue. This means that:

1. You are taking turns with your partner to pump the balloon, and it is now your turn to pump
2. Your partner gets to decide when to stop pumping and collect the rewards

Whenever "Wait for your partner" is displayed in yellow, it means that it is your partner's turn to make a decision.

You have a new box of balloons. Your goal is to inflate each balloon by as much as you can without popping it.

Who pumps: You [or ‘Your Partner’ or ‘You and your partner take turns’, depending on experimental block]
Who decides when to stop (collect): You [or ‘Your Partner’, depending on experimental block]
Who keeps the earnings: You [or ‘Your Partner’, or ‘You and Your Partner’, depending on experimental block]

Try to get the highest total winnings while being as fast as you can. Remember, you always have two choices:

1) Inflate the balloon by one pump.
2) Collect the reward for the current balloon.

Beware, each of the 10 balloons is different and may pop at different times!