Differential effects of prior outcomes and pauses on the speed and quality of risky choices

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Data, code and materials are available at https://osf.io/rzm8d/. Experiments 1 and 2 were not pre-registered. Pre-registrations of Experiments 3 and 4 are available at https://osf.io/8mkp6, https://osf.io/bzv8y and https://osf.io/63rd9 (amendment for Experiment 4). Correspondence concerning this article should be addressed to Zhang Chen, Department of Experimental Psychology, Ghent University, Henri Dunantlaan 2, B-9000 Gent, Belgium. Email: zhang.chen@ugent.be

Abstract

Failures to obtain rewards influence what people choose to do next and how quickly they execute a chosen action, which are two components of motivated behavior. For instance, in risky decisions, losses can induce faster responses and sometimes increase risk-taking, which may lead to detrimental consequences in some situations (such as gambling). Pauses might reduce these motivational influences of prior outcomes. To examine this question, participants alternated between a guess game, in which they won or lost money, and a choice game, in which they chose between a high probability of winning a small amount of money and a low probability of winning a large amount of money. The pause between a guess and a choice game was made either short (0 or 300 milliseconds) or long (3000 milliseconds). In four experiments, prior outcomes consistently influenced decision speed, such that people chose faster after a loss than after a win. However, prior outcomes did not consistently influence people's choices. In contrast, pauses increased decision quality, such that participants chose the option with a higher expected value more often, without substantially reducing decision speed. Pauses may improve decision quality by influencing predecisional attention allocation to relevant information, as its effect was absent when the overall task attention was high (Experiment 3). These findings have implications for both safer gambling and risky decision research. Future work can examine the underlying computational and cognitive processes, and the generalizability of these findings to other contexts and populations.

Keywords: motivation, response vigor, risky decision, pauses in play, expected value

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Introduction

People are generally motivated to obtain rewards, but do not always succeed. For example, a student may fail to pass an exam, an investor may fail to gain profits from an investment, and a gambler may fail to win money in a bet. Since such attempts to acquire rewards are rarely isolated one-shot events, this raises a question: how do prior successes and failures in obtaining rewards influence subsequent motivated behavior?

Prior outcomes may influence both what people choose to do next (i.e., a 'directing' effect) and how quickly they execute a chosen action (i.e., an 'energizing' effect), which are two components of motivated behavior (Braver et al., 2014; Niv et al., 2006; Niv et al., 2007). In this paper, we use risky choice as a common form of motivated behavior, to investigate how prior outcomes may influence both motivational components. In what follows, we first discuss how wins and losses (as successes and failures in acquiring rewards) may influence subsequent risky choices (i.e., the 'directing' effect) and response speed (i.e., the 'energizing' effect). These motivational influences may lead to detrimental results in some situations, which may be counteracted by inserting pauses in play. We thus proceed to discuss whether and how pauses may influence both motivational components in risky choice, which is also the main research question of the current project.

Wins and losses influence response speed and risky choices

Positive and negative outcomes in risky decisions can influence subsequent response speed. For instance, in both simulated and real gambling (a common form of monetary risky decision-making), one relatively consistent finding is that people tend to initiate a new round more quickly after a loss than after a win (e.g., Chen et al., 2020, 2022; Dixon et al., 2013; Dyson et al., 2018; Eben et al., 2020; Ferrari et al., 2022; Stange et al., 2016; Verbruggen et al., 2017). In addition to response speed, prior wins and losses also influence the choices people make, although the direction of the influence has been inconsistent. While some studies have observed more risk-taking (e.g., choosing the gamble option more frequently) after losing than after winning (Brevers et al., 2017; Brooks & Sokol-Hessner, 2020; Eben et al., 2020; Verbruggen et al., 2017; Xue et al., 2011), others have observed the opposite (Cummins et al., 2009; Suhonen & Saastamoinen, 2018; Thaler & Johnson, 1990). Similarly inconsistent effects of prior wins and losses on subsequent risk-taking have also been observed in financial investment and trading (e.g., Coval & Shumway, 2005; Liu et al., 2010). These discrepancies may be explained by factors such as the presentation format of decisions (Weber & Zuchel, 2005), the probabilities of winning and losing (Demaree et al., 2012), or whether the outcomes are realized (Imas, 2016).

The increased response speed and sometimes increased risk-taking after losing may be problematic in some situations. For instance, in gambling, the tendency to continue or intensify betting after losing is called 'loss-chasing' (Banerjee et al., 2023; Zhang & Clark, 2020). Loss-chasing may lead to a vicious circle between losing and continued gambling, and has been widely considered as a key feature of gambling disorder (American Psychiatric Association, 2013; Blaszczynski & Nower, 2002; Nower et al., 2022). Given the potentially deleterious effects of prior losses on both motivational components of risky choices in some situations, it is important to examine how these effects may be counteracted.

Pauses in play: do they reduce response speed and risky choices?

The increased response speed triggered by losses in gambling may reflect a stronger urge to continue playing after losing (Chen et al., 2022). One way to reduce this urge to continue, and potentially also reduce the influence of prior losses on risky choices, may be to insert pauses in play. This idea can be seen in some safer gambling interventions.

One such intervention is reducing the speed of play in fast and continuous forms of gambling (e.g., slot machines; for a review, see Harris & Griffiths, 2018), by inserting short pauses between two consecutive rounds. In a laboratory-based card game in which the chance of winning decreased over time, participants who had to pause for 5 seconds before drawing a new card played fewer rounds in total, and as a result, won more money than those who played a version without pauses (Thompson & Corr, 2013). Similar effects were observed within participants, when they played both versions (Corr & Thompson, 2014). In both versions, participants drew a new card more quickly after a loss than after a win. Interestingly, pauses did not reduce the overall response speed. Participants actually drew a new card more quickly after a 5-second pause compared to no pause (Corr & Thompson, 2014; Thompson & Corr, 2013, the authors did not test for the interaction effect between prior outcomes and pauses on response speed though). Similarly, in an online roulette game, participants who had to wait for 60 seconds before starting a new spin played fewer rounds compared to those who did not need to wait (Newall et al., 2022). However, when the 60-second pause was factored out, the slowed-down group did not seem to play much more slowly than the normal group (slowed-down version, mean = 28.6 s, median = 8.0 s; normal version, mean = 21.0 s, median = 15.0 s). Moreover, the average bet size was descriptively larger in the slowed-down than the normal group (Newall et al., 2022), which might indicate increased risk-taking induced by pauses. Together, these findings suggest that inserting pauses can reduce the number of rounds played, but it may not be effective in reducing response speed[1].

A second related intervention is mandatory breaks in play, which are implemented

[1] Some other studies on speed manipulation have shown that in general gambles with slower speeds of play were less preferred and led to lower self-reported satisfaction and excitement (Blaszczynski et al., 2005; Delfabbro et al., 2005), especially for individuals with gambling disorder (Linnet et al., 2010; Loba et al., 2001; but see Mentzoni et al., 2012; Sharpe et al., 2005, for some null findings). Furthermore, similar to the findings reported in the main text, gamblers tended to play fewer rounds when playing the slow version of a gamble compared to its fast version, when they could decide themselves to stop playing at any time (Chóliz, 2010; Delfabbro et al., 2005; Ladouceur & Sévigny, 2006). However, since these studies manipulated the duration of a round (e.g., making the wheels on a slot machine spin for a longer duration), rather than inserting pauses between two consecutive rounds, we do not further discuss them here.

differently than speed manipulation. In speed manipulation, the overall speed of a gamble is manipulated, by for instance inserting a pause after every round. Furthermore, the pauses often last only a few seconds (Corr & Thompson, 2014; Thompson & Corr, 2013). In contrast, in mandatory breaks, gamblers often play continuously for a long duration before they are forced to take a break of several minutes or longer (Auer et al., 2019). Evidence for the efficacy of mandatory breaks in real gambling is limited (Auer et al., 2019; Hopfgartner et al., 2021). Parke et al. (2019) found that a mandatory break of 3 minutes did not reliably influence the number of rounds played. However, after a break, people overall initiated new rounds more slowly during a period of sustained losses, suggesting that breaks may reduce response speed. This study thus found an inconsistent pattern than the ones mentioned above (Corr & Thompson, 2014; Newall et al., 2022; Thompson & Corr, 2013). Mandatory breaks may backfire though. For instance, in a laboratory Black Jack game, participants reported increased rather than decreased urge to continue playing after breaks (Blaszczynski et al., 2016), which may reflect increased 'frustration' when the access to a desired activity is blocked (Amsel & Roussel, 1952).

Overall, whether pauses can reduce response speed remains unclear, as inconsistent findings have been observed. Furthermore, previous work has mainly focused on the overall effects of pauses, which left unclear whether pauses may have differential impacts on response speed after losses and wins. On the one hand, inserting pauses after losing may be especially effective in reducing response speed, as people tend to respond more quickly after losing. On the other hand, forcing people to pause after losing may backfire, as it might increase rather decrease the urge to continue (Blaszczynski et al., 2016). Given these divergent predictions, it is important to examine whether pauses may impact response speed differently after losses versus after wins.

The potential effects of pauses on risky choices are also unclear. One assumption with inserting pauses in play is often that by forcing participants to pause, they will take some time to reflect on their inappropriate risk-taking, and take less risk afterwards (e.g., Thompson & Corr, 2013). Put differently, the two components of motivated behavior are assumed to be tightly coupled with each other. While this may indeed be the case in some situations (e.g., Reppert et al., 2015), dissociation between the two components has also been observed. For instance, in risky decisions, previous work has shown that the effect of prior outcomes on response speed was not correlated with their effect on risky choices across participants (Verbruggen et al., 2017). This dissociation suggests that the effects of pauses on response speed may not necessarily translate into effects on risky choices. Whether and how pauses may influence risky choices thus warrant further research.

The current research

The motivational influences of prior outcomes may lead to negative consequences in some situations. Pauses in play may counteract such influences, but its effects on response speed and risky choices after wins and losses have been unclear. The current project examined these questions. All experiments followed the same overall procedure. Participants alternated between two types of games, (1) a guess game, in which they won money if they correctly guessed the color of a spinning wheel, and lost money if they guessed incorrectly, and (2) a choice game, in which they chose between a high probability of winning a small amount, and a low probability of winning a large amount (i.e., the gain-only version of the Vancouver gambling task, Limbrick-Oldfield et al., 2020; Sharp et al., 2012). We used the guess games to manipulate prior outcomes (win versus loss), and the choice games to probe risky choices. Between a guess game and a choice game, we inserted pauses of different duration. On some trials, participants could initiate a choice game (almost) immediately after a guess game (no pause in Experiments 1 and 2, and a pause of 300 milliseconds in Experiments 3 and 4). On the remaining trials, they had to wait for 3 seconds before starting a choice game. Using this setup, we investigated how prior outcomes and pauses would influence subsequent response speed (i.e., how quickly participants started a choice game, and how quickly they decided) and risky choices (i.e., the choices they made in the choice games), as the two components of motivated behavior.

Experiment 1

Methods

Transparency and openness

The current research was conducted according to the ethical rules presented in the General Ethical Protocol of the Faculty of Psychology and Educational Sciences of Ghent University. All participants agreed to an informed consent (by clicking on an 'I agree' button) before the experiments.

We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (Simmons et al., 2012). All data, analysis code, and materials are available at https://osf.io/rzm8d/ and GitHub. Data were analyzed using R (version 4.2.1; R Core Team, 2022), with afex (version 1.2.0; Singmann et al., 2022), bayesplot (version 1.10.0; Gabry & Mahr, 2022), bayestestR (version 0.13.0; Makowski et al., 2022), brms (version 2.18.0; Bürkner, 2022), cmdstanr (version 0.5.3; Gabry & Češnovar, 2022), ggpubr (version 0.6.0; Kassambara, 2023), kableExtra (version 1.3.4; Zhu, 2021), knitr (version 1.41; Xie, 2022), loo (version 2.5.1; Vehtari et al., 2022), MASS (version 7.3.58.1; Ripley, 2022), Rmisc (version 1.5.1; Hope, 2022), sjPlot (version 2.8.12; Lüdecke, 2022), tidybayes (version 3.0.2; Kay, 2022), and tidyverse (version 1.3.2; Wickham, 2022).

Experiments 1 and 2 were not pre-registered. Experiments 3 and 4 were pre-registered (Experiment 3: https://osf.io/8mkp6; Experiment 4: https://osf.io/bzv8y, and Experiment 4 amendment: https://osf.io/63rd9). All deviations from the pre-registrations are transparently reported, and the pre-registered results without deviations are provided in Supplemental Materials (https://osf.io/qw63b/).

Participants

Fifty participants (16 females, 34 males, $M_{age} = 27.2$, $SD_{age} = 5.5$) took part in Experiment 1 in November, 2022 via Prolific.co. Another three participants initially started but then quit. One participant did not finish the experiment within the allotted time and was timed out. No data was recorded for the latter four participants. The eligibility criteria were: (1) between 18 and 55 years old; (2) having an approval rate of at least 85% on Prolific.co; (3) being fluent in English; and (4) having no issues seeing colors. The same eligibility criteria were used for all experiments. The sample size was not based on any *a priori* power analysis, but chosen to be close to a previous study with the same risky choice task (Limbrick-Oldfield et al., 2020).

Apparatus and materials

The experiment was programmed in jsPsych (version 6.2.0; de Leeuw, 2015), and ran in Google Chrome and Firefox. Other web browsers were disabled as they might lead to compatibility issues. Participants could do the experiment on either a desktop or a laptop with a keyboard. In the guess game (see below), we used a wheel (diameter = 250 pixels) that was evenly divided into 10 slices, with half of them colored yellow and the other half colored blue. In the choice game, we used two wheels (diameter = 250 pixels) that were also evenly divided into 10 slices. Depending on the probability of winning for each option, some slices were colored green, with the rest colored gray (see Figure 1 for examples of the wheels used). Win outcomes were accompanied by a cash register sound of one second, while loss outcomes were accompanied by a buzz sound of one second.

Procedure

Participants who met the eligibility criteria could sign up via Prolific.co. After reading and agreeing to the informed consent, they could then start the experiment. They were first asked to turn on the speaker of their device, and adjust the volume to a

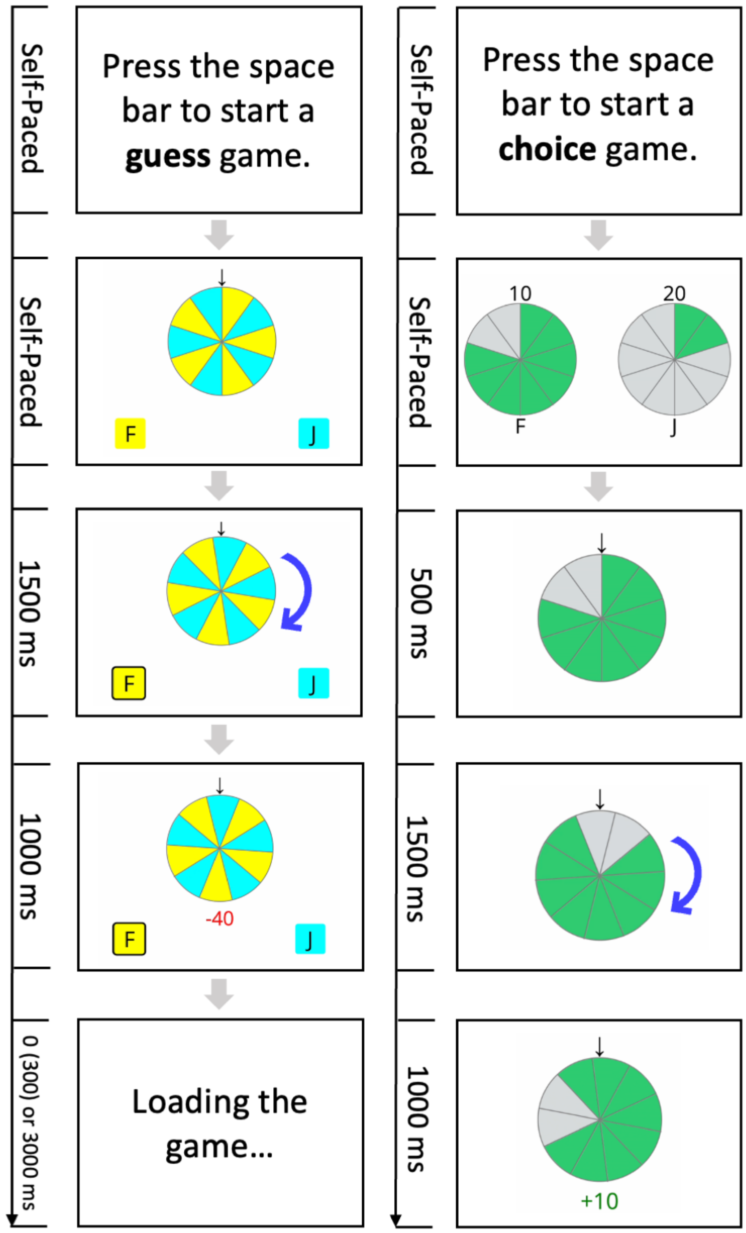

Figure 1

Schematic of the experimental procedure for the guess game (left) and the choice game (right). This figure is for the purpose of illustration. For a screen recording of the experiment (Experiment 1), see https://doi.org/10.6084/m9.figshare.21647108.v1 comfortable level, using the cash register sound as the test sound. They then entered their prolific ID, age, nationality and gender (male, female, non-binary or I don't want to say).

Practice for the guess game. The experiment started with a practice block for the guess game. At the beginning of each game, a start message was shown on screen, telling participants to "press the space bar to start a guess game". This message also showed the number of games left in the current block (not shown in Figure 1). After starting a guess game, participants saw a blue-and-yellow wheel. Their task was to guess whether the black arrow above the wheel would point at yellow (the F key) or blue (the J key) after spinning the wheel. After they made a guess, the chosen color was surrounded by a black frame as a reminder. The wheel then rotated for 1500 milliseconds. If participants guessed the color correctly, they won 40 British pence, otherwise they lost 40 British pence. Unbeknown to them, the outcomes of the guess games were pre-determined, regardless of which color they guessed. Wins were indicated by a text of "+40" in green beneath the wheel, accompanied by a cash register sound. Losses were indicated by a text of "-40" in red beneath the wheel, accompanied by a buzz sound. Both wins and losses were shown for 1 second. The practice block consisted of 4 guess games, with 2 wins and 2 losses.

Practice for the choice game. The second block was a practice block for the choice game. Again, at the beginning of each game, a start message told participants to "press the space bar to start a choice game", with the number of games left in the current block printed (not shown in Figure 1). After starting a choice game, participants saw two wheels presented side by side. Some slices of the wheels were green, indicating the chances of winning for both options. Two numbers were presented above the two wheels, showing the amount of money participants could potentially win for each option. For example, in Figure 1, 8 out of the 10 slices in the left wheel were green, and the number above the wheel was 10. This option thus offered an 80% chance of winning 10 pence, and a 20% chance of winning 0 penny. The right wheel in Figure 1 offered a 20% chance of winning 20 pence, and an 80% chance of winning 0 penny. For each choice game, participants chose which wheel they wanted to play with, by pressing the F or the J key. After they made a choice, the chosen wheel was shown alone statically for 500 milliseconds, followed by a spinning animation of 1500 milliseconds. If the black arrow ended up pointing at green, participants won the amount of money associated with the chosen option. If the black arrow ended up pointing at gray, they did not win any money. Wins were shown by a text in green (e.g., "+10") with the same cash register sound as in the guess games; no-wins were shown by a text in black (i.e., "0") with no sound (we did not use the buzz sound, as no-wins were not real losses). Both outcomes lasted for 1 second. Outcomes in the choice games were determined randomly using the probability of winning of the chosen option. The second block consisted of 4 practice choice games.

The real task. Participants were told that they would alternate between the guess game and the choice game, with the same rules as in the practice. Furthermore, they were told that at the end of the experiment, the program would randomly pick 4 guess games and 4 choice games. The results on these 8 games would be added up, and paid to them as an extra bonus. They could win a maximum bonus of 1 British pound. However, if the sum of the 8 selected games was 0 or negative, they would not receive any bonus. Since participants did not know which games would be selected, they were explicitly told that the best strategy was to treat each game as if it was the only one that would count.

From the participants' perspective, the two types of games were independent. However, from the perspective of experimental design, one guess game and one choice game made up one pair. The whole task consisted of 100 pairs (i.e., 100 guess games plus 100 choice games), with 80 experimental pairs and 20 catch pairs. For the experimental pairs, we used the 10 choices from the gain-only version of the Vancouver Gambling task[2] in the

[2] The Vancouver Gambling task also has a loss-only version, in which the two options denote a high probability of losing a small amount of money, versus a low probability of losing a large amount of money. Since we were mainly interested in how prior outcomes might influence subsequent reward-seeking behavior, we thus only used the gain-only version in the current project. We come back to the issue of not including real losses in the choice games in General Discussion.

choice games (Limbrick-Oldfield et al., 2020). Each choice offered two options, a high probability of winning a low(er) amount (hereafter the HP option) and a low probability of winning a high(er) amount (hereafter the LP option). Importantly, the expected values (i.e., probability × amount of winning; hereafter EV) of both options varied across the 10 choices. For half of them, the HP option had a higher EV. For the remaining half, the LP option had a higher EV. For the 10 experimental choices, see Table 1. We used the Vancouver Gambling task, as it allowed us to examine not only the tendency to take risks (i.e., choosing the LP option), but also the 'quality' of decisions (defined here as the sensitivity to EV). For the experimental pairs, we adopted a 2 (outcome of the guess game: win versus loss) by 2 (pause between two games: yes versus no) design, with each of the 10 choices presented twice in each cell, to counterbalance the left-right position of the options.

Table 1

Choices used in the choice games in the experimental pairs.

HP prob	HP amount	HP EV	LP prob	LP amount	LP EV	EV ratio	High EV option
0.6	10	6	0.4	40	16	-0.909	LP
0.7	10	7	0.3	50	15	-0.723	LP
0.6	20	12	0.4	50	20	-0.500	LP
0.7	10	7	0.3	30	9	-0.250	LP
0.6	30	18	0.4	50	20	-0.105	LP
0.6	40	24	0.4	50	20	0.182	HP
0.7	20	14	0.3	30	9	0.435	HP
0.7	30	21	0.3	40	12	0.545	HP
0.8	10	8	0.2	20	4	0.667	HP
0.8	20	16	0.2	30	6	0.909	HP

Note. HP = the high-probability option. LP = the low-probability option. EV = Expected value. EV ratio = (EV of the HP - EV of the LP) / [(EV of the HP + EV of the LP)/2].

For the 20 catch pairs, we used 5 unique choices, where the HP option had a larger winning amount than the LP option. See Table 2 for the catch choices. Individuals who sought to maximize winnings should choose the HP option. The catch pairs were included to filter out potentially inattentive participants. Each choice was presented 4 times, once in each cell of the 2 (outcome of the guess game: win versus loss) by 2 (position of the HP option: left versus right) design. For the catch pairs, we did not include any pauses.

Table 2

Choices used in the choice games in the catch pairs.

HP prob	HP amount	HP EV	LP prob	LP amount	LP EV	EV ratio	High EV option
Catch trials used in Experiment 1							
0.6	40	24	0.2	10	2	1.692	HP
0.8	50	40	0.2	20	4	1.636	HP
0.7	40	28	0.4	10	4	1.500	HP
0.8	50	40	0.3	30	9	1.265	HP
0.7	30	21	0.4	20	8	0.897	HP
Catch trials used in Experiments 2 and 4							
0.8	40	32	0.2	20	4	1.556	HP
0.7	30	21	0.3	10	3	1.500	HP
0.8	50	40	0.2	30	6	1.478	HP
0.6	10	6	0.5	50	25	-1.226	LP
0.7	10	7	0.6	50	30	-1.243	LP
0.8	10	8	0.7	50	35	-1.256	LP
Catch trials used in Experiment 3							
0.8	40	32	0.2	20	4	1.556	HP
0.7	30	21	0.3	10	3	1.500	HP
0.8	50	40	0.2	30	6	1.478	HP
0.6	0	0	0.2	40	8	-2.000	LP
0.7	0	0	0.4	30	12	-2.000	LP
0.8	0	0	0.3	20	6	-2.000	LP

Note. HP = the high-probability option. LP = the low-probability option. EV = Expected value. EV ratio = (EV of the HP - EV of the LP) / [(EV of the HP + EV of the LP)/2].

To manipulate pauses, we varied the duration between two games. For half of the experimental pairs, the start message for the choice game appeared immediately after the guess game ended. For the remaining half, a text message "Loading the game..." was shown for 3 seconds after the guess game, before the start message of the choice game appeared. During this 3-second pause, participants had to wait. We nevertheless registered any key presses made with the F, J or the space bar as exploratory measures. Note that the pauses only occurred when participants transitioned from a guess game to a choice game, but never from a choice game to a guess game. The 20 catch pairs did not include any pauses.

The 80 experimental pairs and 20 catch pairs were randomly mixed, and divided into 4 parts, with each part containing 20 experimental pairs and 5 catch pairs. After each part, participants could take a short break if necessary. The whole experiment took about 23 minutes. At the end of the experiment, 4 guess games and 4 choice games were randomly picked, and the sum of these 8 games was used to determine the extra bonus for each participant (between 0 and 1 British pound). Participants were then debriefed, thanked, and paid (3.5 British pounds for their time, plus any extra bonus).

Data Analysis

Since we inserted pauses only from a guess game to a choice game, in the analysis we focused on the data from the second game in each pair, namely the choice game only.

Analysis on start RTs and choice RTs of the choice game

Bayesian hierarchical models were fitted to the data using the R package brms (Bürkner, 2022). For the model on how quickly participants started a choice game (hereafter start RT), we first excluded trials in which the start RT was above 5000 milliseconds (1.73% of all trials excluded; for the same exclusion criterion, see e.g. Eben et al., 2020; Verbruggen et al., 2017). Since the distribution of start RTs was positively skewed, we used the natural logarithms of start RTs as the dependent variable. Note that previous work has sometimes observed start RTs of 0 milliseconds, suggesting that sometimes participants might initiate their response before the start message appeared.

Since log(0) is undefined, we added 1 millisecond to all observations before the logarithm transformation. The dependent variable was therefore log(start RT + 1). The outcome of a preceding game (loss = 0.5, win = -0.5), whether there was a pause or not (pause = 0.5, no pause = -0.5) and their interaction were used as predictors. We used the Student's t rather than the normal distribution as the likelihood function, since the former was more robust against outliers (Kurz, 2019)[3]. In the brms syntax, we truncated the log(start RT + 1) with an upper bound of log(5001), since our exclusion meant that no dependent variable would be larger than log(5001). For how quickly participants selected an option in a choice game (hereafter choice RT), we used the same data analysis strategy for the sake of consistency. 0.32% of all trials had a choice RT above 5000 ms and were excluded before data analysis. All results remained the same when no data exclusion and no truncation in the models were applied (see https://osf.io/zxvh7).

To facilitate the comparison with previous work (e.g., Chen et al., 2020; Eben et al., 2020; Verbruggen et al., 2017), we also computed the median RT in each cell for each participant. The median RTs were then submitted to a frequentist repeated-measures ANOVA, with prior outcome and pause condition as within-subject factors. The results of repeated-measures ANOVAs for all experiments can be found in Table A3 in the Appendix.

Analysis on risky choices

For the choices in the choice games from the experimental pairs, we used the same data analysis method from previous work (Limbrick-Oldfield et al., 2020; Sharp et al., 2012). More specifically, we used hierarchical logistic regressions. Whether participants chose the HP option or not on each trial was used as the dependent variable (choose HP = 1, choose LP = 0), while the outcome of a preceding guess game (loss = 0.5, win = -0.5),

[3] The normal distribution did not fit log(start RT + 1) well. The degree of freedom parameter from the Student's t distribution was estimated to be round 1.55, with a 95% CI of [1.41, 1.71], suggesting that the distribution of log(start RT + 1) was not normal (a Student's t distribution becomes a normal distribution when the degree of freedom parameter is set to infinity).

whether there was a pause or not (pause = 0.5, no pause = -0.5), the expected value ratio of the two options (hereafter EV ratio) and all interactions were included as predictors. The EV of an option was computed by multiplying the win amount with the probability of winning. The EV ratio between two options was then computed as:

EV ratio = (EV of the HP - EV of the LP) / [(EV of the HP + EV of the LP)/2]

Positive EV ratios meant that the HP option had a higher EV, while negative EV ratios meant that the LP option had a higher EV (see Table 1).

Model fitting procedure

For all models, we used the maximum random structure, meaning that the random intercepts and all random slopes per participant, as well as the correlations among the random effects were estimated. Wide uninformative priors were used (see Table A1 and Table A2 in the Appendix). For the models on reaction times, adding a truncation to the dependent variable significantly slowed down the MCMC sampling process. Therefore, we ran 4 MCMC chains with 2000 iterations during the warm-up phase, and 5000 iterations during the sampling phase for each chain. For the logistic regression on choices, we ran 4 MCMC chains with 5000 iterations during the warm-up phase, and 10000 iterations during the sampling phase for each chain. For each model, we checked the trace plots, the R-hat values and the effective sample sizes to ensure that the model had converged and the estimates were stable. For the models on RTs, we deem an effect statistically credible if the 95% credible interval does not include 0. For the model on risky choices, we deem an effect statistically credible if the 95% credible interval on odds ratio does not include 1.

Results

One participant restarted the experiment while almost finishing it (and finished the second attempt). Data from this participant were excluded. For three participants, one trial was missing. Data from these three participants were retained. In total, data from 49 participants remained for further analyses.

Choices on the catch trials

On the 20 catch trials, participants on average chose the HP option 18.8 times ($SD = 1.5$, $range = [14, 20]$), suggesting that they overall paid good attention to the task. Although the overall performance on the catch trials was high, we nevertheless adopted the *post-hoc* inclusion criterion that participants needed to choose the HP option on at least 15 catch trials (i.e., 75% of catch trials). Two participants did not meet this criterion, leaving a final sample of 47 participants.

Start RTs and choice RTs in the choice games

Results of linear regressions on start RTs and choice RTs are in Table 3. The model estimates are small because they are on the log(RT) scale. To facilitate the interpretation of the results, we transformed the estimates back into the original reaction time scale (in milliseconds; see Figure 2, panel A). For comparison, panel B of Figure 2 shows the means of median RTs in each cell across participants (i.e., the frequentist approach).

Table 3

Results of linear regressions on reaction times (log-transformed) in all experiments.

Start RT	Experiment 1	Experiment 2	Experiment 3	Experiment 4
Intercept	6.179 [6.121, 6.238]	6.273 [6.202, 6.344]	6.237 [6.191, 6.285]	6.236 [6.204, 6.271]
Prior outcome (loss vs. win)	-0.005 [-0.030, 0.020]	-0.022 [-0.049, 0.006]	-0.034 [-0.048, -0.019]	-0.033 [-0.046, -0.021]
Pause (long vs. short)	0.134 [0.081, 0.186]	0.063 [-0.012, 0.137]	0.069 [0.033, 0.104]	0.079 [0.056, 0.102]
Prior outcome * Pause	-0.053 [-0.098, -0.008]	-0.027 [-0.082, 0.026]	-0.026 [-0.058, 0.005]	-0.018 [-0.042, 0.005]
Choice RT	**Experiment 1**	**Experiment 2**	**Experiment 3**	**Experiment 4**
Intercept	6.828 [6.753, 6.909]	6.908 [6.838, 6.981]	6.991 [6.939, 7.047]	6.909 [6.864, 6.953]
Prior outcome (loss vs. win)	-0.040 [-0.068, -0.013]	-0.050 [-0.074, -0.025]	-0.037 [-0.053, -0.021]	-0.045 [-0.058, -0.032]
Pause (long vs. short)	0.003 [-0.024, 0.031]	0.014 [-0.011, 0.038]	0.017 [0.002, 0.032]	0.013 [-0.000, 0.026]
Prior outcome * Pause	-0.033 [-0.091, 0.023]	-0.000 [-0.047, 0.047]	0.004 [-0.025, 0.034]	0.020 [-0.006, 0.045]

Note. The values show the estimates for the predictors on the population level, with 95% credible intervals in brackets.

Participants overall started a choice game more slowly after a 3-second pause than after no pause, mean difference = 64.5 ms, 95% CI = [40.1, 88.0]. Contrary to previous

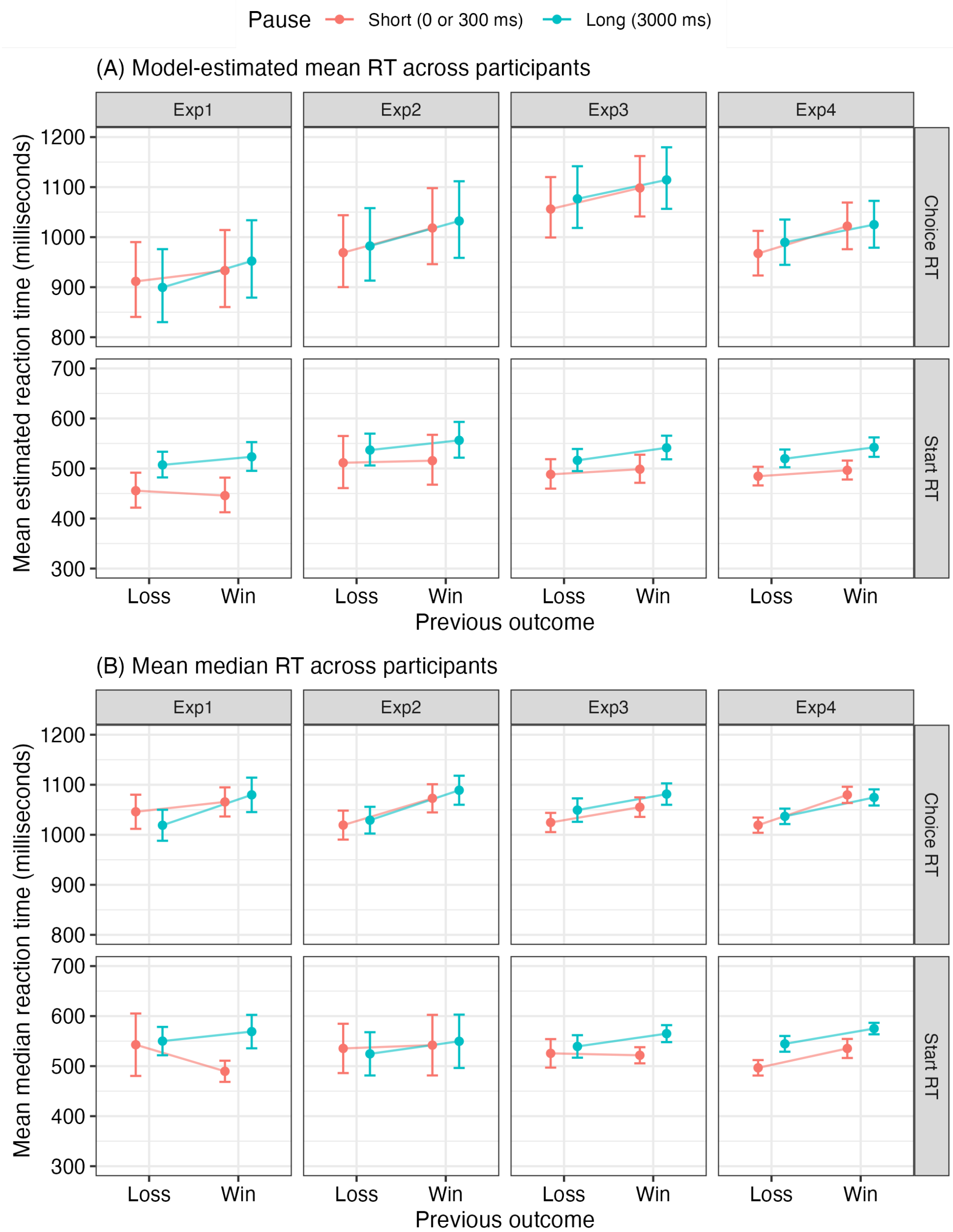

Figure 2

(A) Estimated choice RT and start RT from the models. Note that we plotted choice RTs above start RTs, as choice RTs in general were larger than start RTs. The error bars show 95% credible intervals of the estimated population mean. (B) Median choice RT and start RT across participants. The error bars show 95% within-subjects confidence intervals.

findings (e.g., Chen et al., 2020; Eben et al., 2020; Verbruggen et al., 2017), they overall did not start a choice game more quickly after a loss than after a win, mean difference = -2.3 ms, 95% CI = [-14.4, 9.8]. There was an interaction effect between both factors. After a pause, participants initiated a choice game more quickly after losing than after winning. However, after no pause, the effect was not reliable.

Participants overall *chose* more quickly after a loss than after a win, mean difference = -37.1 ms, 95% CI = [-62.9, -11.8]. In contrast, pauses did not have an effect on choice RT, mean difference = 3.2 ms, 95% CI = [-22.2, 28.6]. The interaction effect between prior outcomes and pauses was not statistically reliable. See https://osf.io/j79zu for all main and simple effects on the RT scale.

Choices on the experimental trials

Table 4

Results of logistic regressions on choices in all experiments.

Predictors	Experiment 1	Experiment 2	Experiment 3	Experiment 4
Intercept	4.47 [3.36, 6.05]	4.87 [3.54, 6.84]	5.66 [4.43, 7.34]	6.81 [5.53, 8.43]
EV ratio	6.52 [4.39, 9.68]	9.05 [5.67, 14.54]	16.58 [12.05, 23.04]	7.79 [6.13, 9.92]
Prior outcome (loss vs. win)	0.91 [0.74, 1.11]	0.96 [0.79, 1.17]	0.88 [0.77, 1.00]	1.03 [0.92, 1.15]
Pause (long vs. short)	1.02 [0.84, 1.25]	0.96 [0.80, 1.17]	0.91 [0.79, 1.05]	1.06 [0.95, 1.21]
EV ratio * Prior outcome	0.80 [0.57, 1.15]	1.28 [0.91, 1.80]	0.78 [0.61, 1.00]	0.97 [0.79, 1.17]
EV ratio * Pause	1.40 [0.99, 1.96]	1.44 [1.03, 2.02]	0.98 [0.76, 1.26]	1.20 [0.98, 1.47]
Prior outcome * Pause	0.98 [0.67, 1.44]	0.76 [0.52, 1.10]	1.08 [0.82, 1.43]	1.11 [0.89, 1.40]
EV ratio * Prior outcome * Pause	0.79 [0.40, 1.51]	1.30 [0.67, 2.45]	1.06 [0.65, 1.73]	1.11 [0.75, 1.63]

Note. The values show the mean odds ratio, with 95% credible intervals in brackets.

Results of the logistic regression are in Table 4. We converted the estimates from the log odds ratio scale into odds ratio for ease of interpretation. As expected, the EV ratio was positively associated with the probability of choosing HP options: when the EV ratio increased (i.e., became more in favor of the HP option), participants also chose the HP

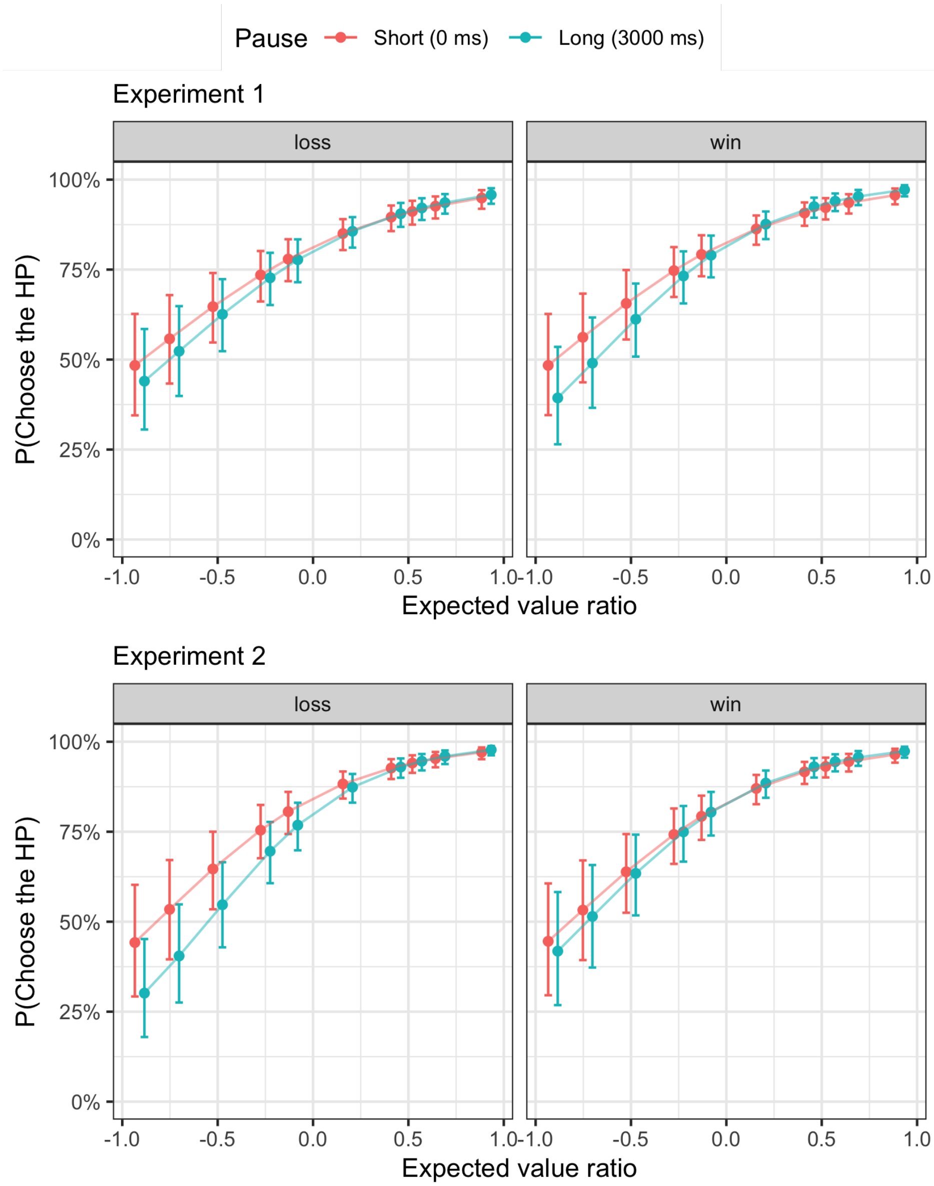

Figure 3

Probability of choosing the HP option as a function of prior outcome, pause, and EV ratio in Experiment 1 (top) and 2 (bottom). We added small horizontal jitters to the lines to avoid overlap. The dots show the mean predicted probabilities of choosing the HP option by the logistic models. The error bars stand for 95% credible intervals of the estimated population mean.

option more often. This effect was modulated by pause, such that after a 3-second pause, participants became more sensitive to EV ratios in their choices (top panel in Figure 3). Note that although the two-sided 95% credible interval for the odds ratio for the EV ratio * Pause interaction included 1 (Table 4), 97.11% of the posterior estimates were larger than 1. We thus deemed this observation promising and worthy of further investigation. The remaining effects were not statistically reliable.

Discussion

Contrary to previous findings (e.g., Chen et al., 2020; Eben et al., 2020; Verbruggen et al., 2017), participants did not *start* a choice game more quickly after a loss than after a win. Instead, the effect of prior outcomes on response speed was more visible on choice RT. Previous work has observed that participants chose more quickly after a loss than after a win (Eben et al., 2020; Verbruggen et al., 2017), and the current finding on the choice RT was in line with this observation. Pauses also influenced response speed, yet its effect was mainly visible on start RT but not choice RT. One unexpected finding was that losses reduced start RTs only after pauses, but not after no pauses, which warrants replication.

Also contrary to some previous findings (e.g., Brevers et al., 2017; Brooks & Sokol-Hessner, 2020; Eben et al., 2020; Verbruggen et al., 2017; Xue et al., 2011), winning or losing did not reliably influence subsequent risk-taking. This finding, however, was in line with Limbrick-Oldfield et al. (2020), who used the same task and also observed no effect of previous outcomes on subsequent risk-taking. We will return to this inconsistency between this finding and some previous studies in the General Discussion. Interestingly, although pauses did not influence overall risk-taking, participants' choices became more aligned with EV ratios after a pause (i.e., increased decision quality). Although this effect of pauses on decision quality (but not risk-taking) was not expected, we deemed this novel finding potentially interesting, and thus further explored this in Experiment 2.

Experiment 2

Experiment 2 was a close replication of Experiment 1, with one small modification. In Experiment 1, all catch trials had a higher EV for the HP option than the LP option, which might have made the HP options overall more attractive than the LP options across all trials. In Experiment 2, we slightly changed the catch trials. For half of them, the LP option had a much larger EV than the HP option (e.g., a 70% chance of winning 50 pence versus an 80% chance of winning 10 pence). The EV ratio was thus more extremely in favor of the LP option than on some of the experimental trials (Table 2). We will refer to these catch trials as LP-optimal catch trials, 'optimal' in the sense that an agent who maximizes the expected values would choose the LP options (i.e., the current definition of 'decision quality'). The remaining catch trials followed the same structure as those in Experiment 1: the HP option had a larger amount than the LP option. We call these trials HP-optimal catch trials.

Methods

Participants

Sixty new participants (16 females, 44 males, $M_{age} = 32.0$, $SD_{age} = 9.45$) took part in the experiment in November, 2022. Another two participants initially started but then quit. One did not finish the experiment within the allotted time and was timed out. No data was registered for the latter three participants. The sample size was again not based on *a priori* power analysis. Compared to Experiment 1, we increased the sample size by 10 to leave some room for potential exclusion.

Apparatus and materials, Procedure, Data analysis

The same apparatus, materials, procedure and data analysis approach as in Experiment 1 were used. The only change was the catch pairs. We used 6 unique catch trials. For half of them, as in Experiment 1, the HP option had a higher amount than the

LP option (HP-optimal trials). For the remaining half, the LP option had a much larger win amount than the HP option (LP-optimal trials), which made the EV of the LP option larger than that of the HP option. Each catch trial was repeated four times, once in each cell of the 2 (outcome of the guess game, win versus loss) by 2 (position of the HP option, left versus right) design, resulting in 24 catch trials in total. Again, no pause was inserted in the catch pairs. Each experimental block consisted of 20 experimental pairs and 6 catch pairs, resulting in 104 pairs (or 208 games) in total.

Results

Choices on the catch trials

Experiment 2 contained 24 catch trials (12 HP-optimal trials and 12 LP-optimal trials). For the HP-optimal catch trials, participants on average chose the HP option 11.5 times ($SD = 0.93$, $range = [8, 12]$); for the LP-optimal catch trials, they on average chose the LP option 10.2 times ($SD = 2.35$, $range = [1, 12]$). For the HP-optimal trials, we used the same inclusion criterion as in Experiment 1. Participants needed to choose the HP option on at least 9 HP-optimal catch trials (i.e., 75% of the trials) in order to be included. For the LP-optimal trials, we did not use any inclusion criterion. Data from 2 participants were excluded, leaving 58 participants in further analyses.

Start RTs and choice RTs in the choice games

0.99% of the trials had a start RT above 5000 ms and were excluded. None of the effects was statistically reliable (Table 3). Since the interaction effect between prior outcome and pause on start RT as observed in Experiment 1 was not statistically reliable (and also not in Experiments 3 and 4), we will not discuss this effect further.

0.6% of the trials had a choice RT above 5000 ms and were excluded. The main results on choice RT were in line with Experiment 1. Participants chose more quickly after a loss than after a win, mean difference = -49.7 ms, 95% CI = [-74.6, -25.4]. Again, pauses did not reliably influence choice RT, mean difference = 13.7 ms, 95% CI = [-11.1, 38.6]. For all main and simple effects on the RT scale, see https://osf.io/d24rb.

Choices on the experimental trials

Experiment 2 replicated the main results of Experiment 1 on choices (see Table 4 and the bottom panel of Figure 3). As the EV ratio became more in favor of the HP option, participants chose the HP option more often. Prior outcomes and pauses again did not change the overall tendency of choosing the HP option (i.e., risk-taking tendency). Most importantly, in line with Experiment 1, the choices became more aligned with EV ratios after a pause. The three-way interaction effect involving prior outcome, pause and EV ratio was again not statistically reliable.

Discussion

Using a different set of catch trials, we largely replicated the main findings of Experiment 1. Prior outcomes influenced choice RT, but did not influence people's choices. Pauses did not change people's overall risk-taking, but increased their sensitivity to EV.

In both Experiments 1 and 2, we did not include any pauses in the catch pairs. This may have inadvertently created two contexts for the choice games, namely those that immediately followed a guess game (the no-pause context), and those that followed a pause (the pause context). Recent work has shown that the representation of value is highly context-dependent. Of special relevance here is range adaptation, the idea that the subjective value of an item is adapted to the range of all values experienced in a certain context, as defined by the highest and lowest values (Palminteri & Lebreton, 2021; Rangel & Clithero, 2012). A similar range adaptation principle might be applied to the EV ratios. In Experiments 1 and 2, the no-pause context had a wider range of EV ratios than the pause context, due to the inclusion of catch trials in the no-pause condition only. As a result, participants may become less sensitive to the EV ratios in the experimental trials in the no-pause context, as there were trials with more extreme EV ratios in this context (i.e., the catch trials). According to this range adaptation idea, pauses did not increase the sensitivity to EV ratios per se. Rather, in the current setup, they served as a cue for a context with a narrower range of EV ratios. We carried out Experiment 3 to test this idea.

On the LP-optimal catch trials, the majority of the participants preferred the LP options, in line with the EVs of the two options. However, a few still predominantly chose the HP options. It was unclear whether these few participants were more risk-averse than the rest, or that they had adopted a different strategy, for instance to choose the HP option without considering the EV. Although including or excluding these participants did not change the main results, we thought it might still be worthwhile to identify them. In Experiment 3, we therefore changed the LP-optimal catch trials to achieve this.

Experiment 3

The main difference between Experiment 3 and Experiments 1 and 2 (among other changes, see below) was that we evenly distributed the catch pairs into the two pause conditions. According to the *range adaptation* hypothesis, the reduced sensitivity to EV ratios with no pause in Experiments 1 and 2 could be (at least partly) due to the inclusion of the catch pairs in this context, rather than the manipulation of pauses per se. By matching the catch pairs in both pause conditions, the difference in sensitivity to EV ratios should disappear. However, the *pause* hypothesis still predicted a higher sensitivity to EV ratios after a pause. We expected to obtain evidence in line with the *pause* hypothesis, and thus rule out the *range adaptation* hypothesis. The sampling plan, data analysis and prediction of Experiment 3 were pre-registered (see https://osf.io/8mkp6).

Methods

Sample size

We planned to recruit 100 participants after exclusions. Assuming that the EV ratio * pause interaction in Experiment 2 was caused by the pauses per se (i.e., the *pause* hypothesis), we evaluated the statistical 'power' of replicating this finding with 100 participants. The posterior distributions of the parameters from the logistic regression in Experiment 2[4] were used to simulate data for 100 experiments, with 100 participants in each experiment. For each simulated experiment, the experimental choice trials were then analyzed with the same logistic regression. For 83 out of the 100 experiments, the lower bound of the 95% CI for the EV ratio * pause interaction effect exceeded 1. The current design thus had around 80% statistical power of replicating the effect.

Participants

In total, 131 new participants participated via Prolific.co in December, 2022. Thirty-one met the pre-registered exclusion criteria (see below), leaving 100 participants as planned (30 females, 70 males, $M_{age} = 30.3$, $SD_{age} = 8.5$). Another 6 participants initially started but then quit. One was timed out. None of these seven participants did any experimental trials.

[4] For the power simulation, we used the effect size of the EV ratio * pause effect with the inclusion criterion that participants needed to choose the HP option on ≥ 9 HP-optimal trials, and choose the LP option on ≥ 6 LP-optimal trials (N = 55). This resulted in a slightly larger effect size for the interaction effect (odds ratio = 1.57, 95% CI = [1.10, 2.22]) than the one reported in the main text. We initially used the cutoff value on the LP-optimal trials to filter out inattentive participants. However, as discussed above, participants who predominantly chose the HP options on the LP-optimal catch trials might not necessarily be inattentive. They might just be more risk-averse than the rest of the participants. As such, we decided to report the results with these participants included in Experiment 2. For the power analysis, we report the results with them excluded though, to truthfully reflect how the experiment was actually planned. We acknowledge that the simulated power could be over-estimated. Furthermore, with only 100 simulated experiments (due to the long computational time), there was probably much uncertainty in the estimated power. The power simulation therefore served to provide a rough estimate of how likely we might expect to replicate the key EV ratio * Pause interaction effect on choices.

Apparatus and materials, Procedure

The same apparatus and materials were used. For the procedure, we made the following changes. First, instead of using a pause versus no pause manipulation, we used a long pause (3000 milliseconds) versus a short pause (300 milliseconds), to rule out some potential low-level confounds, as the message "loading the game..." was now shown in both conditions. We did not expect this change to influence the results (too much). Second, we changed the catch trials. In Experiment 2, for the LP-optimal catch trials, we made the expected value of the LP option larger than the HP option. However, the EV of the HP option was still positive, which might still be attractive to risk-averse individuals. Here we changed the win amount of the HP option to 0 on these LP-optimal catch trials. For instance, an example catch trial asked participants to choose between a 60% chance of winning 0 penny (i.e., the HP option), versus a 20% chance of winning 40 pence (i.e., the LP option). Note that the HP option was essentially equivalent to a 100% chance of not winning anything. Even very risk-averse individuals should now prefer the LP options on these trials, while inattentive participants might still choose the HP options. The same HP-optimal catch trials as in Experiment 2 were used.

The third, and theoretically most important change, was that we now divided the catch trials evenly into both the short and long pause conditions. We used the 6 catch pairs as described above, and presented each once in each cell of the 2 (outcome of the guess game, win versus loss) by 2 (pause, long versus short) design, resulting in 24 catch trials in total. The left versus right position of the HP option was counterbalanced to be equally likely after a win or after a loss, and after a long or a short pause. At the end of the experiment, participants filled out the short version of the UPPS-P impulsive behavior scale (Cyders et al., 2014). We included this scale as part of a future project on potential individual differences in the effects of prior outcomes and pauses on risky choices. The UPPS-P data will not be analyzed here.

Data analysis

We pre-registered to exclude and replace participants who met one of the following four exclusion criteria: (1) re-starting the experiment during the experimental blocks (1 participant); (2) having more than 10% of the trials missing from the experimental blocks (2 participants); (3) choosing the HP option on fewer than 9 HP-optimal catch trials (2 participants); and (4) choosing the LP option on fewer than 9 LP-optimal catch trials (29 participants). In total, 31 participants met at least one exclusion criterion (some met two or more simultaneously). A substantial amount of participants (29 out of 131) failed to meet criterion (4), suggesting that they might have adopted the strategy of choosing the HP options, rather than considering the EV information on each trial. Such participants were probably also present in Experiments 1 and 2, although it was not possible to identify them there. To make Experiment 3 more comparable with Experiments 1 and 2, we decided to deviate from the pre-registration and *not* exclude any participants based on criterion (4). In hindsight, we also reasoned that the failure to consider the EV information in some participants might even partly underlie the observed effect of pauses on decision quality (e.g., participants might be more likely to consider the presented information after a pause). The main results remained the same when using the pre-registered sample (N = 100; see https://osf.io/7yxk8).

Results

By using only the first three exclusion criteria, the final sample consisted of 127 participants (82 males, 44 females, 1 did not report gender; $M_{age} = 30.9$, $SD_{age} = 8.9$).

Choices on the catch trials

Participants chose the HP option on average 11.8 times on the HP-optimal catch trials ($SD = 0.54$, $range = [9, 12]$), and chose the LP option on average 9.8 times on the LP-optimal catch trials ($SD = 3.25$, $range = [0, 12]$), similar to Experiment 2.

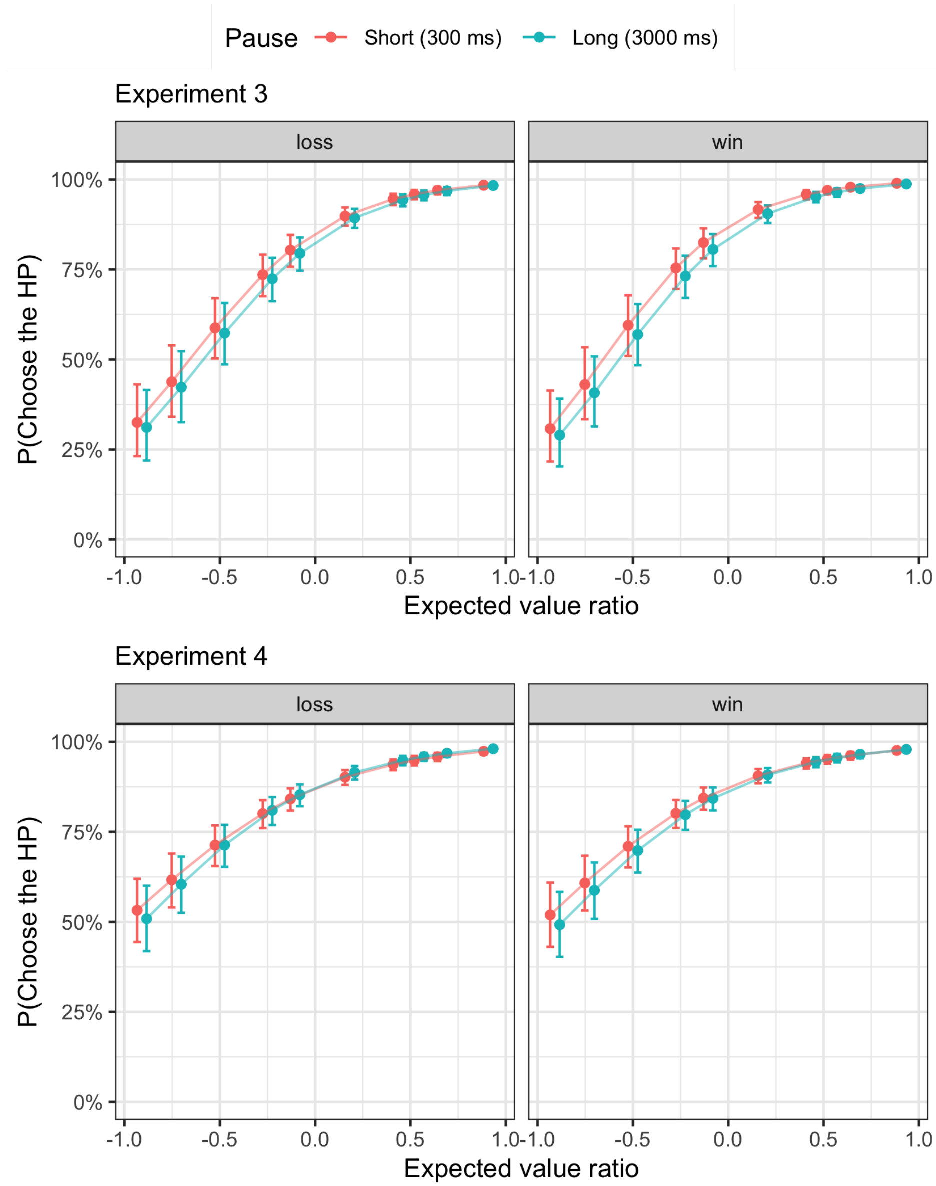

Figure 4

Probability of choosing the HP option as a function of prior outcome, pause, and EV ratio in Experiment 3 (top) and 4 (bottom). We added small horizontal jitters to the lines to avoid overlap. The dots show the mean predicted probabilities of choosing the HP option by the logistic models. The error bars stand for 95% credible intervals.

Start RTs and choice RTs in the choice games

The analyses on the start RT (1.28% of data excluded) and choice RT (0.83% of data excluded) revealed a similar pattern of results (see Figure 2 and Table 3).

Participants started a game more quickly after a loss than after a win (mean difference = -17.4 ms, 95% CI = [-24.7, -9.9]), and started a game more slowly after a 3-second pause than a 300-ms pause (mean difference = 35.3 ms, 95% CI = [17.4, 52.4]). Note that this is the first time that we observed a reliable effect of prior outcomes on start RT in the current project. Similarly, they chose more quickly after a loss than after a win (mean difference = -40.0 ms, 95% CI = [-57.6, -22.7]), and more slowly after a 3-second pause than a 300-ms pause (mean difference = 18.4 ms, 95% CI = [1.8, 35.0]). For all main and simple effects on the RT scale, see https://osf.io/h4rtk.

Choices on the experimental trials

Overall, participants were highly sensitive to the EV ratios in their choices (Figure 4). However, contrary to our prediction and the results of Experiments 1 and 2, taking a long pause did not reliably further increase their sensitivity to EV ratios compared to a short pause (the EV ratio * Pause effect in Table 4). Instead, losing reduced the tendency to choose the HP option (i.e., increased risk-taking; the Prior outcome effect in Table 4), and reduced their sensitivity to EV ratios (the EV ratio * Prior outcome effect in Table 4). These effects were not statistically reliable in the pre-registered dataset though.

Discussion

Both prior outcomes and pauses influenced the speed of both start and choice responses. Participants initiated a new trial and made a decision faster after a loss than after a win, and faster after a short pause than after a long pause. Contrary to our prediction, however, pausing for 3 seconds did not increase participants' sensitivity to EV ratios in Experiment 3. Initially, this result seemed to support the *range adaptation* hypothesis, and argued against the *pause* hypothesis. However, range adaptation cannot easily explain the whole pattern of results. That is, the range of EV ratios was slightly wider in Experiment 3 than in Experiments 1 and 2. Range adaptation alone would therefore predict an overall reduction in sensitivity to EV ratios in Experiment 3 compared to Experiments 1 and 2, which was not observed.

Instead, we suspected that the inclusion of the novel LP-optimal catch trials in Experiment 3 might have increased people's overall attention to the task, which in turn eliminated the pause effect. On these LP-optimal catch trials in Experiment 3, we made the win amount of the HP option 0. Choosing the HP option essentially meant a 100% chance of winning nothing. Consequently, the participants who noticed these novel catch trials may have paid more attention to the options in Experiment 3. Previous work has shown that after attention checks, participants in online studies exhibited more systematic thinking on the Cognitive Reflection Test and in a probabilistic reasoning task (Hauser & Schwarz, 2015). The novel LP-optimal catch trials may have had a similar influence as these attention checks[5]. In hindsight, Experiment 3 revealed a perhaps unsurprising boundary condition of the effect of pauses on risky decisions. That is, when people are already attentive to their choices, there may be little room for pauses to further increase their sensitivity to EV ratios.

This *post-hoc* reasoning attributed the lack of EV ratio * pause interaction to the inclusion of the novel LP-optimal catch trials in Experiment 3. However, Experiment 3 differed from Experiments 1 and 2 in two other aspects, namely (1) the use of a long versus short pause manipulation, and (2) the even distribution of catch trials in both pause conditions. If the lack of a pause effect on decision quality in Experiment 3 was indeed due to the novel LP-optimal catch trials rather than these other two changes, we should be able to replicate the effect if we used the same catch trials from Experiment 2. Experiment 4 was conducted to test this idea.

[5] One way to test this idea directly would be to compare participants' choices before and after encountering the LP-optimal catch trials, as Hauser and Schwarz (2015) did in their experiments. However, participants encountered these LP-optimal catch trials quite early on in Experiment 3. There was thus not a sufficient number of choice trials before these LP-optimal catch trials.

Experiment 4

Experiment 4 aimed to test the idea that the novel LP-optimal catch trials in Experiment 3 may have increased people's overall sensitivity to EV ratios, and diminished the effect of pauses on decision quality. To do this, we used the same procedure from Experiment 3, but with the catch trials from Experiment 2.

Methods

Sample size

Data collection for Experiment 4 proceeded in two phases. For the first phase, we planned to recruit 130 participants after exclusions. Note that in Experiment 4, we decided to *not* exclude any participants based on the LP-optimal catch trials, to be in line with previous experiments. We hence decided to recruit 130 participants, which was close to the sample size in the complete dataset in Experiment 3 (N = 127). For the pre-registration for phase 1, see https://osf.io/bzv8y. To preview the results, after the first phase, we observed that participants became more sensitive to EV ratios after pausing for 3 seconds in a one-sided test (although the two-sided 95% CI just included 1). We therefore decided to recruit another 50 participants in the second phase, to obtain more precise estimates for the effects of interest. This sample size of 50 extra participants was based on resource constraints (i.e., the maximum number of extra participants that we were willing to recruit for this experiment; Lakens, 2022). For the pre-registration for phase 2, see https://osf.io/63rd9. Adding 50 participants did not change the results. For brevity, here we report the results based on the complete dataset from both phases. For the pre-registered results on the first phase only, see https://osf.io/qhnez.

Participants

In total, 181 participants met all inclusion criteria and remained in the analysis (66 females, 115 males, $M_{age} = 31.4$, $SD_{age} = 8.6$)[6]. Data collection took place in January, 2023.

Apparatus and materials, Procedure

The same apparatus, materials and procedure as in Experiment 3 were used. The only difference was that we used the catch trials from Experiment 2 (Table 2). The HP options in the LP-optimal catch trials thus had a positive EV.

Data analysis

We pre-registered to exclude and replace participants who met one of the following three exclusion criteria: (1) re-starting the experiment during the experimental blocks (1 participant); (2) having more than 10% of the trials missing from the experimental blocks (2 participants); (3) choosing the HP option on fewer than 9 HP-optimal catch trials (7 participants). In total, 9 participants were replaced (one participant met two criteria). For the analyses, we pre-registered to use the same models as in the previous experiments.

Results

Choices on the catch trials

On the HP-optimal catch trials, participants on average chose the HP option 11.6 times ($SD = 0.69$, $range = [9, 12]$). On the LP-optimal catch trials, they on average chose the LP option 9.9 times ($SD = 2.90$, $range = [0, 12]$), similar to Experiments 2 and 3.

[6] We pre-registered to recruit 130 participants in the first phase after exclusions. One participant typed a comma in their response, which caused an error when saving their data into comma-separated value files. This participant was incorrectly excluded during data collection, but the error was corrected (by removing the comma) during data pre-processing. We decided to keep this participant in the analysis and thus exceeded the planned sample size by one. This decision was made before conducting data analysis.

Start RTs and choice RTs in the choice games

Data from 1.47% of the trials was excluded for the analysis on start RT, and 0.49% of the trials was excluded for the analysis on choice RT. Replicating the results of Experiment 3, participants started a game more quickly after a loss than after a win (mean difference = -17.1 ms, 95% CI = [-23.7, -10.6]), and started a game more slowly after a 3-second pause than a 300-ms pause (mean difference = 40.3 ms, 95% CI = [28.5, 52.1]). Both effects were statistically reliable. Similarly, they chose more quickly after a loss than after a win (mean difference = -45.1 ms, 95% CI = [-58.2, -32.2]). The effect of pause on choice RT was not statistically reliable (mean difference = 12.8 ms, 95% CI = [-0.4, 26.0]). For all main and simple effects on the RT scale, see https://osf.io/c7jr6.

Choices on the experimental trials

We observed an EV ratio * Pause interaction effect, in the same direction as we predicted. Although the two-sided 95% CI for the odds ratio just included 1, 96.3% of the posterior estimates were larger than 1. The effect was thus statistically reliable in a one-sided test in the expected direction. In line with Experiments 1 and 2, after pausing for 3 seconds, participants became more sensitive to EV ratios (Figure 4). In exploratory analyses, we directly compared Experiment 4 and 3 (see https://osf.io/np76b). In line with our explanation, changing the LP-optimal catch trials changed participants' overall sensitivity to EV ratios. Those in Experiment 3 made choices more aligned with EV ratios than those in Experiment 4, and this difference was statistically reliable.

Discussion

The effects of prior outcomes and pauses on response speed were largely consistent between Experiments 3 and 4. Participants initiated a choice game and made choices more quickly after a loss than after a win, while pauses influenced how quickly they started a new trial. Using the same procedure as in Experiment 3 but with the catch trials from

Experiment 2, we again observed that people became more sensitive to EV ratios after pausing for 3 seconds. Importantly, the modulation effect by pauses emerged when catch trials were evenly divided into the two pause conditions. Range adaption of EV ratios in different pause contexts thus cannot fully explain the effect of pauses on decision quality. Furthermore, participants in Experiment 3 were overall more sensitive to EV ratios than those in Experiment 4, in line with the idea that the novel LP-optimal catch trials in Experiment 3 increased their overall attention to risky choices.

Further exploratory analysis on decision quality

In the main analysis, we used the EV ratio between two options as a predictor, in line with previous work (Limbrick-Oldfield et al., 2020; Sharp et al., 2012). To explore whether the results still hold without assuming that choices were related to EV ratios, we conducted an exploratory analysis. We used the prior outcome (loss = 0.5, win = -0.5), the pause condition (long = 0.5, short = -0.5), and whether the HP option or the LP option had a higher EV (the HP option had a higher EV = 0.5, the LP option had a higher EV = -0.5) as predictors. Whether participants chose the option with a higher EV (coded as 1) or a lower EV (coded as 0) was used as the outcome. The same priors in Table A2 were used. In Experiments 1, 2, and 4 (but not in Experiment 3), participants were more likely to choose the option with a higher EV after pausing for 3 seconds. This effect was statistically reliable in all three experiments, with the two-sided 95% CIs for the odds ratio excluding 1 (Table 5). Prior outcomes did not have a consistent influence on decision quality, except that in Experiment 3 decision quality was lower after a loss (in the complete dataset only). These results thus further corroborated the finding that pauses increased decision quality in risky choice.

General discussion

In four experiments, we examined how prior outcomes and pauses might influence two components of motivated behavior in risky choice, namely what choices people make

Table 5

Results of logistic regressions on choosing the high-EV options in all experiments.

Predictor	Experiment 1	Experiment 2	Experiment 3	Experiment 4
Intercept	2.71 [2.26, 3.27]	3.40 [2.72, 4.28]	4.15 [3.54, 4.90]	2.86 [2.54, 3.23]
Prior outcome (loss vs. win)	**0.93 [0.76, 1.14]**	**1.06 [0.88, 1.28]**	**0.82 [0.72, 0.94]**	**1.03 [0.92, 1.14]**
Pause (long vs. short)	**1.30 [1.07, 1.58]**	**1.23 [1.02, 1.49]**	**1.00 [0.87, 1.14]**	**1.13 [1.01, 1.27]**
High EV option (HP vs. LP)	16.37 [9.47, 27.90]	19.78 [10.69, 35.93]	28.70 [18.53, 45.06]	39.00 [26.60, 57.23]
Prior outcome * Pause	1.00 [0.68, 1.47]	1.24 [0.86, 1.80]	0.99 [0.75, 1.29]	1.02 [0.82, 1.28]
Prior outcome * High EV option	0.86 [0.58, 1.26]	0.91 [0.62, 1.33]	0.72 [0.55, 0.95]	1.09 [0.87, 1.36]
Pause * High EV option	1.13 [0.77, 1.66]	0.98 [0.67, 1.43]	0.85 [0.65, 1.12]	1.15 [0.92, 1.47]
Prior outcome * Pause * High EV option	1.03 [0.50, 2.13]	0.69 [0.34, 1.40]	1.09 [0.64, 1.84]	1.17 [0.75, 1.82]

Note. The values show the odds ratio for each predictor, with 95% credible intervals in brackets. The effects of prior outcome and pause are highlighted in bold.

and response speed. Table 6 provides an overview of the procedural features, and Table 7 provides an overview of the results. Overall, we observed differential effects of prior outcomes and pauses, most notably on the choice response. In all four experiments, participants chose more quickly after a loss than after a win, but prior outcomes did not systematically influence their choices. In contrast, pauses increased the quality of risky choices (except in Experiment 3), while its effect on choice speed was limited. We discuss the implications of these findings below.

Table 6

An overview of the design and procedure features in all experiments.

	N	Pause duration	Catch trial EV ratio	Zero outcome	Catch trial distribution
Exp1	47	0 vs. 3000 ms	HP-optimal trials only	Not applicable	In the short pause condition only
Exp2	58	0 vs. 3000 ms	HP-optimal and LP-optimal trials	No	In the short pause condition only
Exp3	127	300 vs. 3000 ms	HP-optimal and LP-optimal trials	Yes	In both pause conditions
Exp4	181	300 vs. 3000 ms	HP-optimal and LP-optimal trials	No	In both pause conditions

Note. N = Sample size after exclusion. ms = milliseconds. HP-optimal trials = trials in which the high-probability option has a higher expected value. LP-optimal trials = trials in which the low-probability option has a higher expected value. Zero outcome = whether the high-probability options in the LP-optimal trials offer a win amount of 0.

Table 7

An overview of the observed patterns of results in all experiments.

	Prior outcome			Pause		
	Start RT	Choice RT	Decision quality	Start RT	Choice RT	Decision quality
Exp1	Loss ≈ Win	Loss < Win	Loss ≈ Win	Long > Short	Long ≈ Short	Long > Short
Exp2	Loss ≈ Win	Loss < Win	Loss ≈ Win	Long ≈ Short	Long ≈ Short	Long > Short
Exp3	Loss < Win	Loss < Win	Loss </≈ Win[a]	Long > Short	Long > Short	Long ≈ Short
Exp4	Loss < Win	Loss < Win	Loss ≈ Win	Long > Short	Long ≈ Short	Long > Short

Note. The greater than ('>') and smaller than ('<') signs show the directions of statistically reliable effects in all experiments. For the inference on decision quality, we used results from both Table 4 and Table 5. a = Statistically reliable in the complete dataset, but not the pre-registered dataset.

Differential effects of prior outcomes and pauses on decision speed and quality

Prior outcomes and pauses had differential effects on the speed and quality of risky choices. Sequential evidence accumulation models may provide a useful framework to understand these effects (Forstmann et al., 2016; Ratcliff et al., 2016). These models assume that decision-makers sequentially accumulate noisy information, and commit to a decision when the accumulated evidence reaches the threshold for a certain option. The behavior of evidence accumulation models can generally be described by four parameters: (1) the drift rate, which represents the average amount of accumulated evidence per unit of time, (2) the decision boundary, which represents how much evidence is needed for making a decision, (3) the starting point, which represents *a prior* bias or preference for one choice alternative over another, and (4) the non-decision time, which represents the time needed for non-decision processes, such as the encoding of sensory stimuli and the execution of motor responses (Forstmann et al., 2016). The time spent on accumulating evidence (i.e., decision time) plus the non-decision time constitute the total observed response time. Evidence accumulation models thus provide a description of both the choices that people make and the choice response times (i.e., two components of motivated behavior), and have been successfully applied to choices in various decision domains (e.g., Johnson et al., 2017; Krajbich et al., 2010; Ratcliff, 1978; Voss et al., 2004; Zhao et al., 2020).

Prior outcomes influenced decision speed, but did not systematically impact the choices themselves. Within the evidence accumulation framework, prior outcomes may thus mainly influence non-decision processes, such as the execution of motor responses, but not the decision process. In contrast, pauses improved decision quality (i.e., more high EV choices), even though participants did not choose substantially more slowly after pauses. Pauses may therefore influence the decision process. More concretely, pauses might increase the decision threshold and the drift rate. With a higher decision threshold, more evidence is required before making a decision. Higher decision thresholds therefore generally lead to more accurate decisions, which in the current context may mean choosing the option with a higher EV. All else being equal, higher decision thresholds mean longer decision times, as more evidence needs to be accumulated. We therefore speculate that pauses may also increase the drift rate, so that the evidence may be accumulated more efficiently. The increased drift rate may be related to predecisional attentional dynamics (see below). Together, an increased decision boundary and an increased drift rate may lead to more 'accurate' risky choices after a pause, without substantially increasing the choice RT.

How do pauses increase decision quality in risky choices?

Sequential evidence accumulation models provide a useful theoretical framework to explain the results. However, it does not specify the cognitive processes via which the parameters may be influenced by pauses. We speculate that pauses may exert its effects by influencing predecisional attentional dynamics. Recent work has shown that attention allocation during risky decision can be systematically linked to the choices that people make (Zilker & Pachur, 2022). More concretely, more balanced attention between different attributes (e.g., probability versus outcome information) or between different options (e.g., the LP option versus HP option here) is associated with less distortion of relevant choice information (Pachur et al., 2018; Zilker & Pachur, 2021, 2023), which in turn may enable people to make choices that are more aligned with EV. Wins and losses are emotionally arousing events that may bias subsequent attention allocation (Mather & Sutherland, 2011). Inserting pauses may allow such affective reactions to dissipate, thereby allowing people to allocate attention to choice information in a more balanced manner. Such attentional dynamics may also explain why we did not observe an effect of pause in Experiment 3, where we included catch trials of 0 win amount. These trials may have increased people's overall attention to the choices, which may leave little room for pauses to further increase participants' sensitivity to EV.

Process-tracing methods such as eye-tracking can be used to test these hypothesized attentional dynamics. Furthermore, the idea that attentional dynamics may be influenced by prior wins and losses can be tested by including neutral outcomes and examining whether the pause effect is only present after a win or a loss, but not after a neutral outcome. Including neutral outcomes as a baseline will also help address another question, namely whether the observed effect of wins and losses on decision speed is best conceptualized as speeding up after losses (Verbruggen et al., 2017), or slowing down after wins (Dyson, 2023), which will help us better understand the origin of the motivational influences of wins and losses.

Implications for safer gambling and risky decision research

Our results have implications for safer gambling research. As mentioned above, both speed manipulation and breaks in play assume that inserting pauses may curb excessive gambling. However, evidence for their efficacy is limited, and the underlying mechanisms of such interventions are unclear. Our results suggest that one route via which pauses may influence gambling behavior may be to increase people's sensitivity toward EV. Most (if not all) gambling products have negative EV, so that most gamblers lose money in the long run. Increased sensitivity toward (negative) EV may reduce the overall appeal of gambling, and thus allow people to cease gambling more quickly when pauses are inserted (Corr & Thompson, 2014; Newall et al., 2022; Thompson & Corr, 2013). More generally, increasing gamblers' sensitivity to EV could help reduce gambling-related harm. For instance, a recent interview study with elite professional online poker players showed that despite a high level of behavioral dependence on gambling, this group generally did not experience harm. One factor that distinguishes professional poker players from disordered gamblers (who experience both behavioral dependence and harm from gambling) may be the former group's ability to assess decision quality based on expected value (Newall & Talberg, 2023). Introducing pauses may be one way to increase people's sensitivity to EV while gambling. However, we acknowledge that our task differs substantially from gambling products. The choices used here provided complete information to decision-makers, while in most gambling products, such information is not explicitly provided. Future research needs to examine whether similar processes are involved when pauses are inserted into more realistic gambling products (e.g., certain poker games where the expected value may be estimated).

These results also have implications for risky decision research. Risky choices as the ones used here have been used extensively to examine risky decision process. However, as far as we know, the potential influence of inter-trial intervals on risky decisions has not received much attention. Our results show that inter-trial intervals (i.e., pauses between trials) can influence decision quality in risky choices. In some experimental setups, such as neuroimaging, the inter-trial intervals can be longer than 3 seconds, and vary from trial to trial (e.g., Xue et al., 2010, 2011). This variation in inter-trial intervals may influence risky choices, in addition to factors of theoretical interest. Researchers may need to pay more attention to inter-trial intervals as a potential confounding factor in experimental design.

Inconsistencies with previous work

There are some inconsistencies between the current findings and previous work. Some previous studies have observed more risk-taking after losing than after winning

(Brevers et al., 2017; Brooks & Sokol-Hessner, 2020; Eben et al., 2020; Verbruggen et al., 2017; Xue et al., 2011), while we did not observe a consistent effect of wins and losses from the guess games on risky choices in the choice games. Several procedural differences may have contributed to this inconsistency. First, previous studies mainly used choices between a risky and a safe option, whereas we used two risky options in the choice game. Choice format (risky/safe or risky/risky) has been shown to modulate various phenomena in risky decision (e.g., the description-experience gap, Wulff et al., 2018; age difference in risk attitudes, Zilker et al., 2020). The influence of prior outcomes on risky choices may also be modulated by choice format. Second, we used a guess game to manipulate wins and losses, whereas previous work mainly used outcomes from participant's own risky decisions. In the guess game, participants might perceive no control over the outcomes, and therefore did not change subsequent risky choices. However, since we did not measure the feeling of control, this hypothesis cannot be directly examined here. Whether the influence of prior outcomes on risky choices depends on perceived control over outcomes thus needs to be examined in future work. Lastly, we used two different tasks (i.e., the guess game and the choice game), whereas previous studies used a single risky decision task. Participants may have perceived the wins and losses in the guess games to be unrelated to the choice games, and therefore did not change risky choices after a win or a loss here. Here participants also received outcomes from the choice games. To explore whether outcomes from the same task may influence current risky decisions, we added the outcome of a previous choice game as an extra predictor, and repeated all analyses (see https://osf.io/4nrpb). Adding the outcome of a previous choice game as a predictor did not change the results, thus ruling out prior choice outcome as a confounding factor for the main findings. Importantly, in Experiments 3 and 4 (but not in Experiments 1 and 2), we observed that after a no-win outcome in a previous choice game, participants were less likely to choose the HP option (i.e., more risk-taking) in the current choice game. There is therefore some evidence that participants might modulate their risky choices more when prior outcomes were more related to current choices (e.g., from the same task).

Previous work has observed that participants start a new trial faster after a loss than after a win or a neutral outcome (Eben et al., 2020; Verbruggen et al., 2017), whereas here we only observed an effect of wins and losses on start RT in Experiments 3 and 4. Furthermore, the effect in Experiments 3 and 4 (around 17 milliseconds) was smaller than those in previous work (40 milliseconds or larger; Eben et al., 2020; Verbruggen et al., 2017). One exception is Experiment 5 in Verbruggen et al. (2017), in which participants alternated between a gamble trial and a stop-signal trial, in which they had to occasionally inhibit responses. In this experiment, the effect of prior gamble outcomes on the RT of starting a new gamble trial was attenuated (around 16 milliseconds) and not statistically significant. Pauses may be similar to the stop-signal trials in Experiment 5 of Verbruggen et al. (2017). By requiring participants to occasionally pause, the effect of wins and losses on start RT may have been attenuated compared to previous work. This may explain why we observed a small effect of wins and losses on start RT in Experiments 3 and 4, and not in Experiments 1 and 2 (i.e., due to low statistical power).

Limitations and future directions

We note several limitations and future directions. First, although evidence accumulation models and attentional dynamics may explain the results, we did not fit computational models to the data, nor did we measure predecisional attention allocation. The discussions above are thus speculative and need to be tested in future research. Although evidence accumulation models have been applied successfully to risky choices (e.g., Busemeyer & Townsend, 1993; Clay et al., 2017; Peters & D'Esposito, 2020; Zhao et al., 2020), a relatively large number of trials is required for fitting the model. Due to the limited number of unique choices (i.e., 10 in total) here, we did not attempt to fit computational models to the data. We consider the present findings as a first demonstration of the behavioral effects, and leave the examination of the underlying computational and cognitive processes to future research.

The generality of the pause effect on decision quality also requires further examination. Experiment 3 already showed a potential boundary condition, namely that the effect of pauses could be reduced or disappear when the overall task attention was already high. How individuals allocate attention in different contexts may therefore be an important factor in determining the generality of this effect. For instance, we used risky choices with only potential gains. Furthermore, since the minimum bonus was 0, participants could not actually lose money, which may have reduced the effects of the 'losses' in the guessing games. When real potential losses are involved, the pause effect may be reduced, as previous work has shown that people generally allocate more attention when evaluating losses than when evaluating wins (Lejarraga et al., 2019). Consistent with this, minor potential losses have been shown to lead participants to maximize the expected value in their choices (Yechiam et al., 2015). Follow-up work could therefore examine the observed pause effect when losses are involved, for instance by using the loss version of the Vancouver Gambling task (Limbrick-Oldfield et al., 2020; Sharp et al., 2012). Furthermore, individuals with gambling disorder are less sensitive to EV (Limbrick-Oldfield et al., 2020), which may suggest more room for pauses to potentially increase their sensitivity to EV. However, it is unclear whether the reduced sensitivity to EV in gambling disorder arises from attentional dynamics as discussed above, or other processes that may not be impacted by pauses. Whether the pause effect may generalize along these dimensions is thus uncertain and needs to be further examined.

Here we inserted pauses between two games. As mentioned in Footnote 1, some previous studies have decreased the speed of play of a gambling product by increasing the duration between a bet and an outcome, such as making the wheels on a slot machine spin for a longer duration (e.g., Chóliz, 2010; Delfabbro et al., 2005; Ladouceur & Sévigny, 2006). Furthermore, pauses can also be introduced by other features of a gambling product, such as requiring players to insert credits before starting a new round, which slows down the pace of play. Dyson (2021) examined whether different credit systems (i.e. whether participants were required to insert credits before starting a new round or not) affected responses in Rock, Paper, Scissors game, and found no consistent effects of different credit systems. However, the pauses caused by inserting credits in this particular study were short (around 400 milliseconds), which might explain why pauses did not influence subsequent responses. Whether these different ways of introducing pauses involve similar underlying cognitive processes or not will need to be further tested in future research.

Conclusion

Motivated behavior involves two components, namely choosing what to do and how quickly to execute a chosen action. Both components may be influenced by prior outcomes, which in some situations may lead to detrimental results. In the context of risky choices, we examined whether pauses would counteract the motivational influences of prior outcomes. Prior outcomes and pauses show disparate effects on decision speed and quality. Within the framework of evidence accumulation models, prior wins and losses may mainly influence the execution of motor responses, whereas pauses may influence the decision process, such as an increased decision threshold and an increased drift rate. These effects may in turn be explained by predecisional attention allocation after pauses. Future work needs to examine the computational and cognitive processes underlying these effects, and the generalizability of these findings to other risky choice contexts and populations.

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Appendix

Priors used in brms models

Table A1

Priors used in the hierarchical linear regressions on reaction times (log-transformed).

Parameter	brms prior class	Prior
Group-level intercept	Intercept	normal(6.5, 1.5)
Group-level slopes	b	normal(0, 1)
Standard deviation of by-subject intercepts and slopes	sd	normal(0, 1)
Correlations among by-subject intercepts and slopes	cor	lkj(2)
Standard deviation of reaction times (log) within subjects	sigma	normal(0, 1)
Degree of freedom parameter for Student's t distribution	nu	gamma(2, 0.1)

Note. normal = the normal distribution. lkj = the LKJ correlation distribution. gamma = the gamma distribution.

Table A2

Priors used in the hierarchical logistic regressions on choices.

Parameter	brms prior class	Prior
Group-level intercept	Intercept	normal(0, 2)
Group-level slopes	b	normal(0, 1)
Standard deviations of by-subject intercepts and slopes	sd	normal(0, 1)
Correlations among by-subject intercepts and slopes	cor	lkj(2)

Note. normal = normal distribution. lkj = the LKJ correlation distribution.

Frequentist repeated-measures ANOVAs on median reaction times

After excluding trials with RTs larger than 5 seconds, we computed the median RT of the remaining trials in each cell for each participant. The median start RT and choice RT in each experiment were submitted to a frequentist repeated-measures ANOVA, with the prior outcome (loss vs. win) and pause (long vs. short) as within-subject factors. The results of the ANOVAs are shown in Table A3. The results on choice RT were consistent with those from Bayesian linear regressions (Table 3 in the main text). The results on start RT showed some inconsistencies, which might be explained by the large start RT in a few participants (see https://osf.io/zxvh7). The effect of pauses on start RT was somewhat visible in Experiments 1, 3 and 4, although it was statistically significant only in Experiment 4.

Table A3

Results of frequentist repeated-measures ANOVAs on median RTs in all experiments.

Variable	Experiment	Effect	df	MSE	F	ges	p
Start RT	Exp1	Prior outcome	1, 46	16251.10	0.84	.002	.364
		Pause	1, 46	27817.13	3.17	.016	.082
		Prior outcome * Pause	1, 46	10394.05	5.88 *	.011	.019
	Exp2	Prior outcome	1, 57	7920.35	1.83	<.001	.182
		Pause	1, 57	94588.90	0.00	<.001	.969
		Prior outcome * Pause	1, 57	14665.13	0.34	<.001	.562
	Exp3	Prior outcome	1, 126	6596.64	2.27	<.001	.134
		Pause	1, 126	26678.93	3.88	.005	.051
		Prior outcome * Pause	1, 126	12086.73	2.27	.001	.134
	Exp4	Prior outcome	1, 180	14257.55	15.20 ***	.008	<.001
		Pause	1, 180	12625.72	27.54 ***	.013	<.001
		Prior outcome * Pause	1, 180	7280.85	0.41	<.001	.522
Choice RT	Exp1	Prior outcome	1, 46	10896.94	6.98 *	.005	.011
		Pause	1, 46	10716.43	0.18	<.001	.673
		Prior outcome * Pause	1, 46	14541.12	1.37	.001	.249
	Exp2	Prior outcome	1, 57	11404.62	16.36 ***	.008	<.001
		Pause	1, 57	11425.73	0.86	<.001	.359
		Prior outcome * Pause	1, 57	11732.69	0.05	<.001	.826
	Exp3	Prior outcome	1, 126	15889.22	7.85 **	.002	.006
		Pause	1, 126	11642.06	7.05 **	.001	.009
		Prior outcome * Pause	1, 126	15167.45	0.00	<.001	.950
	Exp4	Prior outcome	1, 180	11279.78	38.78 ***	.006	<.001
		Pause	1, 180	12714.73	0.56	<.001	.456
		Prior outcome * Pause	1, 180	10602.60	2.21	<.001	.139

Note. df = degrees of freedom. MSE = mean-squared errors. ges = generalized eta-squared. Statistical significance codes: 0 '***' 0.001 '**' 0.01 '*' 0.05.