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Outcome sequences and illusion of control – part II: the effect on post-loss speeding

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ABSTRACT

When gambling, people tend to speed up after losses. This 'post-loss speeding' is in contrast with 'post-error slowing', which is often observed in behavioral tasks in experimental psychology. Importantly, participants can control the outcome in most behavioral tasks, but not in gambling tasks. To test whether perceived controllability over the outcome influences response speed after negative outcomes when gambling, we ran two online studies in which we created an illusion of control without changing the nature of the chance-determined gamble. Using the manipulation by Langer and Roth (1975), whose effect is replicated in Part I, we presented three groups of healthy participants (N = 600 per experiment, crowdsourced samples) with three different sequences of outcomes in a coin-tossing task. We replicated that participants presented with more wins at the beginning of a sequence estimated their ability to predict the outcome of a coin-toss higher than participants presented with more losses at the beginning, or those presented with a random sequence. Additionally, participants generally responded more quickly after a loss than after a win. However, the illusion of control did not influence post-loss speeding. This result is not consistent with several theoretical accounts for changes in response speed after sub-optimal outcomes.

KEYWORDS

Control beliefs; illusion of control; gambling; replication; post-loss speeding

1. Introduction

How do individuals respond if something goes wrong or if they do not get what they want? Many psychological theories assume that outcomes are monitored or appraised, and that this influences subsequent behavior. But how behavior is influenced by sub-optimal outcomes appears to depend strongly on the context. The latter is not always appreciated, partly because researchers often tend to restrict themselves to a particular set of behavioral tasks frequently used in experimental psychology (i.e. fast-paced two-choice reaction time tasks; Williams et al., 2016; Yeung & Summerfield, 2012) but do not take into account findings from other literatures, including the gambling literature (see for example Dixon et al., 2013; Eben et al., 2020; Verbruggen et al., 2017). Here we tried to reconcile two sets of seemingly contradictory findings: slowing after errors and speeding after losses. Typically, slowing after negative outcomes, such as errors, has been

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considered as a marker of cognitive control and adaptive processes (see for example Bogte et al., 2007; Damaso et al., 2020; but see also e.g. Kerns et al., 2005; Notebaert et al., 2009). Here we test an alternative explanation that a lack of slowing or even speeding might not be due to a lack of cognitive control *per se* but rather due to the subjective feeling of control (which in turn might lead to a lack of cognitive control).

In many behavioral tasks, participants slow down after committing an error (Laming, 1979; Rabbitt & Rodgers, 1977). Such post-error slowing is commonly attributed to strategic changes in response speed after sub-optimal outcomes. For example, cognitive-control theories assume that a performance-monitoring system keeps track of ongoing actions and their outcomes. When the action outcome is sub-optimal (e.g. an error or a partial error), task-processing settings are adjusted (e.g. increasing the amount of information required for a decision) to avoid errors in subsequent trials (e.g. Dutilh et al., 2012). Yet, post-error slowing does not always lead to reduced errors, and some even found an increase in error rates (e.g. Notebaert et al., 2009; Rabbitt & Rodgers, 1977). To explain these effects, the orienting account proposes that errors are infrequent events that might orient attention away from the main task. This (re)orienting of attention leads to post-error slowing, without necessarily reducing subsequent errors (Notebaert et al., 2009). More recent theories assume a combination of increased control after sub-optimal outcomes and reorienting after unexpected events (Wessel, 2018). But despite the different theoretical explanations, most studies in the psychology literature find that people slow down after sub-optimal outcomes.

A different pattern is observed though after losses in a gambling context. This might seem surprising as errors and losses show some important similarities. For example, errors in decision-making tasks and losses in gambling tasks are both negative outcomes, which activate similar cognitive control networks typically associated with performance monitoring (Nieuwenhuis, 2004). They also produce a very similar frowning response in facial electromyography (Elkins-Brown et al., 2016; Lindström et al., 2013; Wu et al., 2015), which has been associated with negative affect. Note that some have argued that such negative affect might be the signal for cognitive control adjustments (e.g. Inzlicht et al., 2015). Based on this observation, one may expect people to similarly slow down and become more cautious after losses. However, studies on sequential effects in gambling tasks paint a different picture.

Verbruggen and colleagues (2017) found that participants sped up after losses when gambling. In these experiments, participants chose between a non-gambling option in which they would win a certain amount of points for sure, and a gambling option in which they could win more points with a lower probability of winning. Verbruggen et al. (2017) found that participants started the next game faster after gambled losses compared to gambled wins and non-gambling trials. These findings were recently replicated in both the original task and in a task in which the amount of trials per condition was controlled for (Corr & Thompson, 2014; Dixon et al., 2013; for similar findings on response-speed differences afters wins and losses, see also Eben et al., 2020). These findings seem to contradict the aforementioned accounts of post-error slowing, which would assume that losses (i.e. sub-optimal outcomes) lead to more cautious behavior and therefore slower responses. Instead, these findings are consistent with the idea that blocked reward (i.e. an expected win which was not obtained) is 'frustrating' and leads to invigoration of subsequent behavior (Amsel, 1958; Gipson et al., 2012; Yu et al., 2014). For example, according to the appraisal account by Frijda (2010) a discrepancy between the current state (e.g. a loss) and a desired state (e.g. a win) will activate states of action readiness. Importantly, the larger the discrepancy or the more important the appraised event, the greater the urge to act (aimed at reducing the discrepancy).

The present work aimed to reconcile the above seemingly contradictory findings by examining the role of controllability. After all, it is generally possible to exert control over the outcome in standard psychology tasks, whereas this is – per definition – not possible in chance-based gambling tasks. This could potentially explain why people typically slow down after errors but speed up after losses, especially as recent work on post-error slowing suggests that controllability can affect how people respond after errors as well. Damaso et al. (2020) showed that participants slowed down after 'response speed' errors but not 'evidence quality' errors. Response speed errors occurred when the participant responded too quickly; by contrast, 'evidence quality errors' occurred when participants were presented with very poor evidence. Thus, response speed errors could be avoided by sampling more information (i.e. slowing down), whereas evidence quality errors could not (i.e. the error was outside the participant's control and could not be avoided by slowing down). Therefore, the findings of Damaso et al. (2020) indicate that the origin of the error (i.e. whether or not participants had control over it) influenced whether or not people subsequently slowed down (for similar results regarding the locus of control see Steinhauser & Kiesel, 2011). Interestingly, controllability also seems to matter in a more gambling-related context. Dyson et al. (2018) showed that participants who played a rock, paper, scissors game against a computer that was unexploitable (i.e. the computer responded randomly as in most gambling tasks), sped up after losses; by contrast, when the computer was exploitable (e.g. the computer responded with the option that would have beaten the player's previous response) and the participants could adjust their decision accordingly, they slowed down after losses. However, behavioral tasks generally used in psychology (in which post-error slowing is typically observed) and gambling tasks differ in some other aspects as well (e.g. task engagement, number of trials); so, to isolate the effect of controllability on performance adjustments, we decided to manipulate the 'illusion of control' within the same task.

To investigate whether (perceived) control indeed determines how people respond after negative outcomes (slowing vs. speeding) in a gambling situation, we induced an illusion of control without giving participants actual control over the outcome of the game. Here we used an 'emphasis of success' manipulation as first used in a seminal study by Langer and Roth (1975), and which we replicated in an online context (see our replication study "Outcome sequences and illusion of control – Part I"). In these studies, participants had to guess the outcome of a coin-toss and subsequently were asked about their perceived ability to do so. Importantly, three groups of participants were presented with three different sequences of wins and losses (whereas the overall probability of winning was always exactly .5). The descending group started with a lot of *wins* and had more *losses* toward the end; the ascending group started with a lot of *losses* and finished the experiment with a lot of *wins*; and for the random group, wins and losses were equally distributed throughout the experiment. Participants in the descending group (i.e. more wins at the beginning) estimated their ability to guess the outcome of the coin-toss higher than the ascending group (i.e. more losses at the beginning) and the random group.

It is important to note that there are various independent individual and contextual variables that can foster the illusion of control, as well as different direct and indirect measures of this construct (for an overview see Stefan & David, 2013). It has been argued though that the various measurement approaches used in previous studies do not all assess the illusion of control but other related constructs instead (Presson & Benassi, 1996). In the case of the study by Langer and Roth (1975), it is possible that what these authors labeled "illusion of control" actually corresponds to expectations of future success (one question used in the original study even asks about the participants future success expectations), which among other things, can also depend on faith or perceived luck (for adetailed discussion see Stefan & David, 2013; Wohl & Enzle, 2009). Importantly, here we adopted the conceptualization of the illusion of control as defined by seminal work of Langer and Roth (1975) and decided to use sequences of outcomes as our manipulation as it seemed to produce the biggest effect sizes with regard to creating an 'illusion of control' in a previous meta-analysis (Stefan & David, 2013).

When giving participants the feeling of control in a gambling task ('illusion of control' as Langer, 1975, frames it), the aforementioned cognitive accounts and the appraisal accounts predict different outcomes. First, the standard cognitive-control accounts predict *post-loss speeding* in situations in which the outcome cannot be controlled, but *post-loss slowing* when outcomes can be controlled (as explained above). Second, the orienting account attributes post-error slowing to reorienting of attention after unexpected events. In our task, losses and wins are equiprobable. However, if participants overestimate their ability to win, then a loss would be an unexpected event; this should lead to an orienting response, and hence, slowing. Thus, the cognitive-control and orienting accounts (and their 'hybrid' versions) all predict that participants slow down after losses when they feel in control but not when they do not feel in control, in line with the findings by Dyson et al. (2018).

In contrast, the appraisal accounts discussed above predict more speeding when participants feel more in control. Note that some appraisal accounts (e.g. Lerner & Keltner, 2001) predict different behavioral responses as a function of the specific emotion experienced. However, here we focus on the appraisal process itself instead of the specificity of emotions. Hence, according to the appraisal accounts mentioned above (Frijda, 2010; Moors et al., 2021), events get appraised by the individual, which means the current state is compared to the desired state and the difference between these two determines the urge to act. This appraisal process is supposedly a very fast process; nevertheless, it is assumed to be highly dependent on the individual's prior beliefs, experiences and expectancies (Frijda, 2010; Moors et al., 2013, 2021). This idea leads to the following prediction: if an individual feels in control over the outcome in a gambling task, they expect to win; when a loss occurs, the difference between the desired state and the current state will be bigger for them than for participants who expect a loss (i.e. those who do not feel in control). As response speed is determined by the size of the discrepancy, the post-loss speeding would be more pronounced for people who feel in control. In line with that, Chen et al. (2020) found that if participants expected to win in a scratch card task before eventually losing, they subsequently responded faster. Here we tested these contrasting accounts in two successive experiments.

2. Experiment 1

2.1. Method

All raw and processed data, code and material for both experiments can be found on https://osf.io/qm2a8. The preregistration for Experiment 1 can be found on https://osf.io/xpr6m and the preregistration for Experiment 2 can be found on https://osf.io/s8j79. We also report (here and in our preregistration) how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study.

2.1.1. Sample size

We used a Bayesian sequential hypothesis testing approach, with a minimum sample size of 50 participants per group (from Part I, we knew that we would need – in all likelihood – more than 32 participants) and a maximum sample size of 200 participants per group (600 in total). We primarily focused on the comparison between the ascending and descending groups for the sum score of the five illusion-of-control questions. Specifically, we used sequential Bayesian hypothesis testing by increasing the sample in steps of 75 participants (25 per group) until a decisive Bayes Factor (BF10 larger than 10 or smaller than 1/10) was obtained, or until we reached the maximum number of 600 participants. However, we tested 150 participants too many due to an error in our original analysis script, resulting in an underestimation of the Bayes Factor. With the corrected script, we would have reached our critical Bayes Factor after 450 participants (150 participants per group). Since the results did not differ between 450 and 600 participants, we decided to report the data for all 600 participants.

2.1.2. Participants

Participants were recruited via the crowd-sourcing platform Prolific where every eligible participant is notified as soon as a study is published online. The researcher can indicate the number of participants per study and participants can then sign up for it (as long as the requested number of participants has not been reached). Once signed up, participants have a certain amount of time to finish the study (56 minutes if the experiment is 15 minutes long) before they get timed-out (and free up a place again).

In addition to the 600 participants who completed the experiment, an additional 16 participants finished the experiment and got paid, but were excluded based on the preregistered exclusion criteria: eleven participants started the experiment again, possibly because they missed the completion code for Prolific or wanted to do better to win more money; four did not answer all questions at the end; one had more than 5% missing trials; and one did not complete the study in time. After the last check of the analyses, we identified one participant from the random group who met the preregistered exclusion criteria. However, we decided to not replace this participant as we already exceeded the required sample size (see section Sample Size). Excluding this participant did not alter the findings. Thus, we reported the analyses with a sample size of 599 participants. Furthermore, 3 participants were rejected because they had no completion code. An additional 98 participants signed up for the experiment but never started or completed it. All data were acquired between 24 March 2021 and 2 April 2021. Both experiments were approved by the local research ethics committee of the faculty of Psychology and Educational Sciences at Ghent University. Informed consent was obtained.

2.1.3. Apparatus, stimuli, and procedure

We used the same apparatus, stimuli and procedure as in our replication study Part I. As Figure 1 depicts, the only difference was that participants had to press one of the arrow keys to start the next trial, as in the previous studies on post-loss speeding (Eben et al., 2020; Verbruggen et al., 2017). After that, the trial continued as described in our replication study.

On each trial, participants first had to press a key to start the next trial (without time restrictions). Then a video was presented (1500 ms) in which the avatar asked the participants 'Heads or tails?'. Then participants could guess the outcome (without any time restrictions) by pressing the left (heads) or the right (tails) arrow key. After their response, participants saw a coin for 500 ms followed by an animation of a coin-toss for 500 ms. After that, the outcome in points was shown below the coin for 1000 milliseconds ("+25" for a win, and "-25" for a loss). Importantly, we again presented three different groups of participants with the three different sequences of wins and losses as in the original study and as in our replication study (Part 1 study). Thus, for the latency data we had a 2 × 3 design with previous outcome (win vs. loss) and group (descending, ascending and random. Eventually, after 30 trials of coin-tossing, participants also answered the same five questions about the task as in Langer and Roth (1975) and as in replication study (a)"How good do you think you are at predicting outcomes like these?" (item 'Prediction'); (b)"How well do you think you would do on the task if you were distracted?" (item 'Distraction'); (c)"How much do you think you would improve with practice?" (item 'Practice'); (d)"How many correct predictions did you make on these 30 trials?" (item 'Past'); (e)"How many correct predictions would you make in the next 100 trials?" (item 'Future'). For more details on the methods please see https://osf.io/4e75n.

2.1.4. Analyses

The questionnaire data were analyzed in the same way as in the original study and our replication study (including the sum score), using the same R packages. For the latency data, we measured the time to start the next trial (start response time; start RT in ms) and

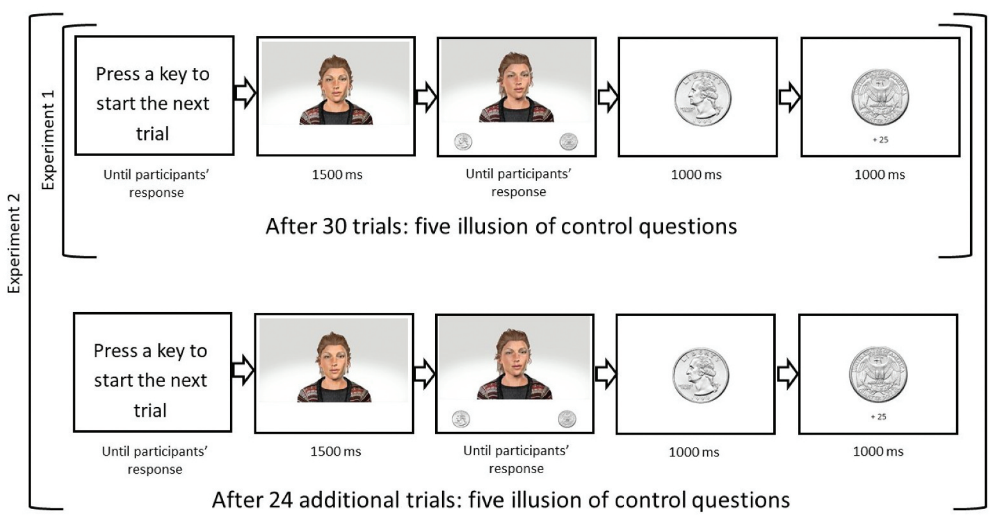

Figure 1. Trial and experimental procedure of experiment 1 and experiment 2.

the time to guess whether the outcome might be heads or tails (choice response time; choice RT in ms). We excluded trials on which choice RT was above 2500 ms and start RT was above 5000 ms (assuming that participants took a break here). As we explored the effect of the outcome of the previous trial, we also excluded the first trial and trials where the previous outcome was unknown (this was not explicitly preregistered, but in line with previous work e.g. Eben et al., 2020). We performed a mixed ANOVA (frequentist and Bayesian) with the factors previous outcome (win vs. loss) and group (ascending vs. descending vs. random) on the start RT and the choice RT. We also conducted three follow-up *t*-tests (frequentist and Bayesian). Here, we calculated the difference between trials following losses and trials following wins and compared this difference between the three groups, with a special focus on the comparison between the descending and the ascending group. We only report the preregistered analyses on the sum score and the exploratory analyses on the start RT in the main text. All other (preregistered) analyses can be found in the online supplementary material (https://osf.io/xfyhs/) and support our conclusions.

2.2. Results

2.2.1. Illusion-of-control questionnaire

For clarity purposes, here we report the results based on the sum scores of all five illusion of control questions. For the comparisons on individual questions, see the online supplementary material as well as Figure 2. Overall, the descending group indicated a higher estimated ability to predict the outcomes (*M* = 187.98; *SD* = 47.23) compared to the ascending group (*M* = 172.34; *SD* = 52.94) and the random group (*M* = 165.49; *SD* = 43.47). These two differences were significant (and with BF > 10; see Table 1).

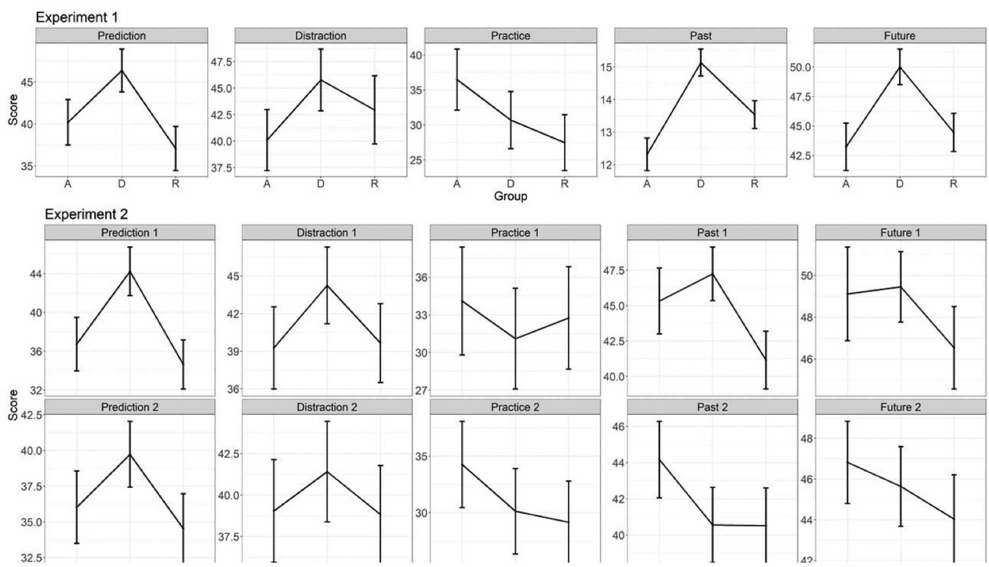

Figure 2. The estimated ability to guess the outcome of a coin-toss in Experiment 1 and in Experiment 2 after the first 30 trials and after the additional 24 trials displayed by item and group in Experiment 2. The error bars reflect the 95% confidence intervals.

Table 1. Pairwise comparisons between the three groups on the sum score in Experiment 1.

	diff	Lower CI	Upper CI	df	*t*	*p*	BF_{10}	*gav*
ascending vs. descending	−15.64	−25.51	−5.78	392.92	−3.12	**.002**	11.60	.31
ascending vs. random	6.85	−2.67	16.37	383.49	1.41	.158	.29	.14
descending vs. random	22.50	31.42	13.57	395.30	4.96	<**.001**	> 100	.49

diff = difference; CI = confidence interval (95%); BF = Bayes Factor 10; g_{av} = Hedge's average *g*.

2.2.2. Start-latency data

The descriptive statistics of the latency data are displayed in Figure 3. The ANOVA (Table 2) revealed a significant main effect of previous outcome on start RT, indicating faster initiation of the next trial following a loss (*M* = 670 ms; *SD* = 476 ms) compared to a win (*M* = 843 ms; *SD* = 525 ms). The ANOVA revealed no significant main effect of group, showing no overall difference between the descending group (*M* = 757 ms; *SD* = 313 ms), the ascending group (*M* = 756 ms; *SD* = 339 ms) and the random group (*M* = 743 ms; *SD* = 317 ms). The interaction between group and previous outcome was significant. The follow-up comparisons (Table 2) revealed a bigger post-loss speeding effect

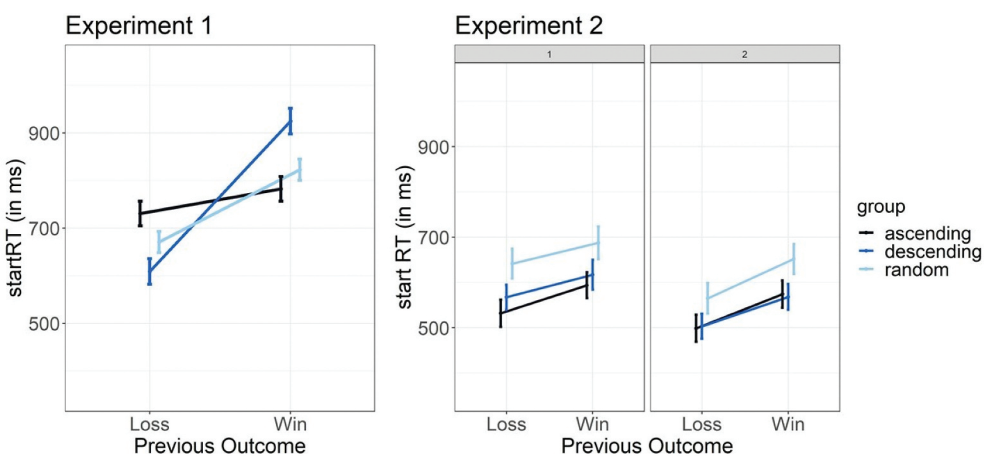

Figure 3. Start RT (in ms) as a function of previous outcome and group in Experiment 1 and as a function of previous outcome group and part of the additional 24 trials (first 12 trials vs. second 12 trials) in Experiment.

Table 2. Inferential statistics of the start RT (in ms) data in Experiment 1.

ANOVA Effect	DFn	DFd	MSE	*F*	*p*	ges	BF_{10}	
group	2	597	213566.04	0.19	.827	0.00	0.06	
previous outcome	1	597	32523.63	276.05	<**.001**	0.06	> 100	
group x previous outcome	2	597	32523.63	54.49	<**.001**	0.02	> 100	
pairwise comparisons								
Comparison	**diff**	**Lower CI**	**Upper CI**	**df**	***t***	***p***	$\boldsymbol{BF_{10}}$	***gav***
ascending vs. descending	−263.65	−316.44	−210.86	397.30	−9.82	<**.001**	> 100	.98
ascending vs. random	−99.64	−147.80	−51.48	389.09	−4.07	<**.001**	> 100	.41
descending vs. random	164.01	114.65	213.38	383.84	6.53	<**.001**	> 100	.65

ANOVA: DFn = degrees of freedom in the numerator; DFd = degrees of freedom in the denominator; MSE = mean squared error; ges = Generalized Eta-Squared measure of effect size. *Pairwise comparisons* diff = difference; CI = confidence interval (95%); BF_{10} = Bayes Factor 10; g_{av} = Hedge's average *g*.

in the descending group compared to the ascending and the random group. The post-loss speeding effect in the random group was also bigger than in the ascending group.

2.3. Discussion

The illusion-of-control manipulation was again successful: participants who were presented with more wins at the beginning estimated their ability to guess the outcome of a coin-toss higher than participants presented with more losses at the beginning or with an even sequence of wins and losses. Consistent with the previous work, participants were generally faster after a loss than after a win (analyses of choice RT showed a similar pattern; see online supplementary materials). Most importantly, both effects interacted, as the descending group showed a larger post-loss speeding effect than the other two groups. These results provide some support for the appraisal account, which posits that participants who feel more in control should show a bigger post-loss speeding effect. However, there might be an alternative explanation for this group difference. Participants across all conditions sped up over the course of the experiment (showing generally faster responses at the end of the experiment compared to the beginning; see Figure A2 in the Appendix). As the sequences of wins and losses were pre-determined, the descending group had more wins at the beginning and more losses toward the end of the coin-tossing task. Thus, for the descending group, trials after a loss were more likely to occur toward the (fast) end of the experiment. This could have caused (or at least contributed to) the difference between the descending group and the two other groups. We thus designed a second experiment to control for this.

Based on our replication study, we used the sum score for the illusion-of-control questions in Experiment 1. The analyses on the individual questions revealed that the item "Practice", which corresponds to perception that practice can improve performance on the task over time, stood out and even showed the opposite pattern compared to the other four items. We assume that this is related to the fact that the sequence of wins and losses changes over time: whereas the descending group (which started with more wins) ended with more losses, the ascending group (which started with more losses) ended with more wins. Thus, the number of wins dropped over time in the descending group but increased in the ascending group. This might have influenced the answers to this practice question. Therefore, we omitted this question when calculating the sum score in Experiment 2.

3. Experiment 2

In Experiment 1, we found more pronounced post-loss speeding in the descending group. However, this effect could also have been caused by general speeding (as wins and losses were – intentionally – distributed differently). To rule out this alternative explanation, we added two extra sets of twelve trials at the end of the experiment. The wins and losses were randomly distributed in these extra 24 trials, thus general speeding was no longer a confound. We were mainly interested in the first 12 additional trials, but also analyzed the second 12 trials to see if a potential effect of illusion of control remained stable over time.

3.1. Method

3.1.1. Sample size

We set a minimum sample size of 50 participants per group (same reasoning as in Experiment 1) and a maximum sample size of 200 participants per group (600 in total). In the sequential Bayesian hypothesis testing, we focused on two comparisons. First, we compared the ascending and descending groups on the sum score of illusion-of-control questions after the first 30 trials, excluding the 'Practice' question (see above). Second, we calculated the difference score in start RT between trials following wins and trials following losses for each group in the first 12 additional trials (i.e. after the first set of illusion-of-control questions). We then compared this difference score between the descending and the ascending groups with a Bayesian independent samples *t*-test. Only if both Bayes Factors reached our crucial Bayes Factor (> 10 or < 1/10), we stopped testing (before reaching the maximum sample size). The eventual Bayes Factor for the comparison on start RT was not decisive, so we reached the maximum number of 600 participants.

3.1.2. Participants

In total 600 participants (200 per group, recruited via Prolific) completed the entire online experiment and were included in the analyses (273 females and 318 males, 6 non-binary and 3 preferred not to say; age $M = 27.5$ years, $SD = 8.9$ years; *range* = 18–75; for a detailed distribution across the three groups, see the Appendix). The same eligibility criteria and payment structure as in Experiments 1 were used.

In addition to the 600 participants who completed the experiment, an additional 14 participants finished the experiment and got paid but were excluded from data analyses in accordance with the preregistered exclusion criteria: nine started the experiment again, and five had more than 5% missing trials. Furthermore, 6 participants were rejected and not paid (in accordance with the Prolific guidelines) as they never provided a completion code nor any data sets. An additional 98 participants signed up for the experiment but never started or completed it. All data were acquired between 25 May 2021 and 7 June 2021.

3.1.3. Apparatus and stimuli

We used the same apparatus and stimuli as in Experiment 1, apart from the use of a visual analogue scale for Questions 4 and 5 (in the previous experiment, participants had to manually enter a numerical response but some failed to do so).

3.1.4. Procedure

Participants started with 30 trials, using the exact same procedure (and sequences) as in Experiment 1. After they had answered the questions, they completed two additional sets of 12 coin-tossing trials. They then answered the five illusion-of-control questions again. This second set of questions was included to see whether the 24 additional trials changed the perceived level of control.

3.1.5. Analyses

To analyze the questionnaire data, we performed the same analyses as in Experiment 1 (but without the Practice question). Here, we analyzed the first set (i.e. after the first 30 trials) and the second set of questions (i.e. after the additional 24 trials) separately. We only report these analyses for the sum score. All other (preregistered) analyses can be found in the online supplementary material and support our conclusions.

To examine the effect of illusion of control on post-loss speeding, we primarily focused on the extra trials at the end of the experiment. The same exclusion criteria as in Experiment 1 were used. As we explored the effect of the previous outcome, we also excluded trial number 31 (first trial of the additional 24 trials) and trials in which the previous outcome was unknown (not explicitly preregistered, but in line with previous work). We preregistered to analyze the first set and the second set (of twelve trials) separately. However, after further consideration (and to save space), we decided to conduct a (not preregistered) mixed 2 × 3×2 ANOVA on the start RT with the factors previous outcome (win vs. loss), group (descending vs. ascending vs. random) and part (1st 12 trials vs 2nd 12 trials). Due to a missing cell, we excluded one participant from the random group from these latency analyses. The remaining preregistered analyses are in the online supplementary material.

3.2. Results

3.2.1. Illusion-of-control questionnaire

After the first 30 trials, the descending group (*M* = 185.19; *SD* = 43.34) indicated a higher estimated ability to predict the outcomes compared to the ascending group (*M* = 170.44; *SD* = 50.40) and the random group (*M* = 161.97; *SD* = 48.14). As can be seen in Table 3, these differences were reliable. Thus, we could again replicate the illusion-of-control effect. However, after 24 additional trials, there was no longer a difference between the descending group (*M* = 167.33*SD* = 49.47), the ascending group (*M* = 166.05; *SD* = 50.79) and the random group (*M* = 157.91; *SD* = 52.56; see Table 3). The follow-up ANOVA revealed that the interaction between these two time-points was significant (see online supplementary material).

3.2.2. Start-latency data of the additional 24 trials

The start RTs are displayed in Figure 3. The ANOVA revealed three main effects. First, start RTs were generally faster after a loss (*M* = 551 ms; *SD* = 313 ms) than after a win (*M* = 615 ms; *SD* = 342 ms). Second, start RTs were slower for the first 12 trials (*M* = 606 ms;

Table 3. Pairwise comparison between the three groups on the sum score after 30 trials and after additional 24 trials in Experiment 2.

	diff	Lower CI	Upper CI	df	*t*	*p*	BF_{10}	*gav*
After 30 trials								
ascending vs. descending	−14.75	−23.99	−5.51	389.25	−3.14	**.002**	12.28	0.31
ascending vs. random	8.47	−1.22	18.16	397.16	1.72	.086	0.46	0.17
descending vs. random	−23.22	−32.23	−14.21	393.68	−5.07	**<.001**	19759.98	0.51
After additional 24 trials								
ascending vs. descending	−1.28	−11.14	8.57	397.72	−0.26	.798	0.11	0.03
ascending vs. random	8.13	−2.03	18.30	397.53	1.57	.116	0.36	0.16
descending vs. random	−9.42	−19.45	0.61	396.54	−1.85	.066	0.57	0.18

diff = difference; CI = confidence interval (95%); BF = Bayes Factor 10; g_{av} = Hedge's average *g*.

Table 4. Inferential statistics of the start RT (in ms) latency data in the additional 24 trials in Experiment 2. Here we conducted an ANOVA with the factors group, previous outcome and part.

Effect	DFn	DFd	MSE	*F*	*p*	ges	BF_{10}
group	2	596	373407.66	4.62	**.010**	.010	1.82
previous outcome	1	596	51437.62	47.88	< **.001**	0.01	> 100
part	1	596	59524.77	21.64	< **.001**	0.00	> 100
group x previous outcome	2	596	51437.62	0.14	.866	0.00	0.01
group x part	2	596	59524.77	1.00	.367	0.00	0.03
previous outcome x part	1	596	36181.70	2.23	.135	0.00	0.14
group x previous outcome x part	2	596	236181.70	0.33	.720	0.00	0.03

DFn = degrees of freedom in the numerator; DFd = degrees of freedom in the denominator; MSE = mean squared error; ges = Generalized Eta-Squared measure of effect size.

SD = 333 ms) compared to the second 12 trials (*M* = 560 ms; *SD* = 329 ms). And third, the ascending group (*M* = 550 ms; *SD* = 283 ms) was unexpectedly faster than the descending group (*M* = 564 ms; *SD* = 272 ms), which was in turn faster than the random group (*M* = 636 ms; *SD* = 355 ms). Importantly,

none of the interactions was significant. For detailed inferential statistics see Table 4.

3.3. Discussion

In Experiment 2, we again successfully induced an illusion of control in the descending group. However, this effect appeared to be short-lived as the (questionnaire) differences were observed after the first run of 30 trials, but no longer after the additional run of 24 trials (in which all sequences were random). Consistent with previous research, people responded faster after a loss than after a win in the second part of the experiment (i.e. for the additional 24 trials). However, this effect was not modulated by group (not even at the beginning of this second part). This suggests that the influence of group on the post-loss speeding effect in Experiment 1 (note that we replicated this pattern for the first 30 trials in Experiment 2 as well – see Figure A3 in the Appendix) was caused by uneven distribution of wins and losses in the different groups and not by the differences in perceived control.

4. General discussion

The current study aimed to investigate the influence of the illusion of control over the outcome on invigorated behavior after losses (i.e. post-loss speeding). Across experiments, we could again replicate the finding by Langer and Roth (1975) in an online experimental setting (see also our replication study in Part 1). Thus, introducing a start response did not alter the illusion-of-control effect. Moreover, we replicated the finding that participants speed up after losses compared to wins (Eben et al., 2020; Verbruggen et al., 2017). Furthermore, in Experiment 1, the descending group showed a bigger post-loss speeding effect than the other two groups. However, we also observed a general trend to speed up throughout the experiment. As wins and losses were not distributed equally for the three groups, this general speeding could have contributed to the group differences in post-loss speeding. Therefore, in Experiment 2 we added 24 trials in a random sequence at the end of the experiment to control for between-group differences in win/loss distribution. Here, participants responded faster after losses compared to wins in these

24 additional trials, but this post-loss speeding effect was comparable for the three groups. There are two possible explanations for the discrepancy between the results from the first 30 trials and those from the additional 24 random trials in the second experiment. First, post-loss speeding might be influenced by the illusion of control manipulation in the first 30 trials, but the effect of this manipulation might be short-lived and have disappeared at the moment the random sequences were introduced. This idea is consistent with the finding that the three groups no longer showed a difference in their estimated ability to guess the outcome after the additional 24 random trials. Second, the modulation of post-loss speeding by sequences in the first 30 trials might not be caused by differences in illusion of control per se, but rather by a general speeding over trials. Figures A1 and Figure A2 in the Appendix indicate that there is a general tendency toward speeding over trials, and that this contributes substantially to the difference between wins and losses in the first 30 trials (exploratory analyses revealed similar patterns for the choice RT in our replication study, reported in Part I). Based on these additional analyses, we conclude that even if the illusion of control modulates post-loss speeding, the modulation effect is at best rather small, and may dissipate very quickly.

Our findings seem not to be in line with any of the theoretical accounts mentioned earlier. The cognitive-control account predicts that participants feeling in control would adjust their behavior to avoid subsequent sub-optimal outcomes (causing longer response latencies), whereas participants who do not feel in control would speed up (akin to findings of e.g. Dyson et al., 2018). The orienting account predicts the same outcome, but for a different reason: Participants who feel in control expect to win, so a loss would be an unexpected event. Unexpected events lead to an orienting response, causing longer response latencies. Inconsistent with these predictions, we observed post-loss speeding in all groups. Even when participants felt in control, latencies were still shorter after a loss than after a win. The appraisal accounts (Frijda, 2010; Moors et al., 2013, 2021) do predict speeding after losses, but they also predicted that the speeding should be more pronounced for the descending group (i.e. for participants who felt more in control). After all, the discrepancy between the current and the desired state after a loss is bigger when a win is expected. The null result is thus also not in line with the appraisal accounts.

Thus, all theoretical accounts discussed above predicted latency differences between the groups, based on the idea that the groups would differ in their perceived ability to control the outcome of a coin-toss. We did not observe such group differences. It should be noted that there were also large individual differences within groups. Therefore, in a further exploratory analysis, we examined the correlation between the sum score after the first 30 trials (which is a measure of illusion of control) and post-loss speeding effect in the start RT in Experiment 1 and Experiment 2 (Figure 4). These correlations were not significant, further indicating that control beliefs and post-loss speeding do not interact.

It is also important to note, that the frequentist and Bayesian statistics indicated that descending group participants estimated their ability to guess the outcome higher compared to the other groups, but the effect size was much smaller than that reported in a meta-analysis (Stefan & David, 2013) and in the original study (Langer & Roth, 1975). More importantly, across all groups, participants still estimated their ability to guess the outcome close to chance level (despite the reliable differences between groups). This suggests that overall, participants still realized they were in a game of chance. Thus, we could shift the estimated ability to guess the outcome a bit, but the overall task context

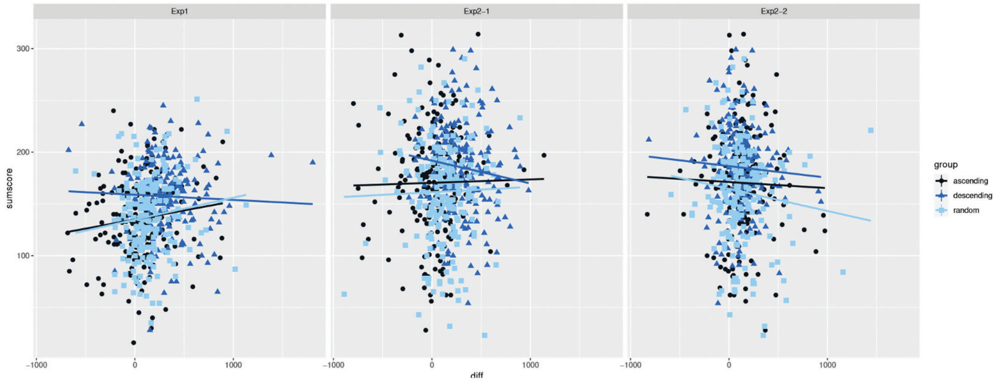

Figure 4. The relationship between the sum score per participant and the difference score between wins and losses in the start RT in Experiment 1, the first 30 trials in Experiment 2 and the additional 24 trials in Experiment 2 displayed as a function of group.

of a game of chance had presumably a bigger effect (for similar conclusions after a failed replication see Ladouceur & Mayrand, 1984). Therefore, it is likely that participants did not feel much more in control in the descending group, and this might be the reason why we did not find group differences in the post-loss speeding effect.

In line with this, one could argue that we did not create an illusion of control per se as participants were not 'overestimating' their chances to win (i.e. raw means were on average below chance level); instead, they were on average even underestimating their chances to win. Nevertheless, the descending group did this to a lesser extent. This indicates that our manipulation counteracts an underestimation, and that we created a small (relative) difference between the three groups (which was also the case in the original study). We also want to emphasize that we were mainly interested in seeing whether differences in perceived control would influence subsequent behavior. As we were able to create such differences between the groups, we can at least conclude that such (albeit small) differences in perceived control do not have an impact on post-loss speeding. To test whether illusion of control influences post-loss latencies however, further experiments (with stronger manipulations) might be needed. One possibility would be to use a spinning wheel task as used in Tobias-Webb and colleagues (2017), in which participants either were not able to spin the wheel, were able to spin the wheel themselves (at no cost), or had to pay to spin the wheel. Importantly, in the last condition participants actively disadvantage themselves to exert control, which could even enhance the illusion of control and might lead to bigger behavioral effects (Tobias-Webb et al., 2017).

Another limitation in this study is that we did not have a baseline condition. Earlier studies showed that the difference between wins and losses can also be due to a slowing after wins (i.e. post-reinforcement pause, see Eben et al. (2020); Verbruggen et al. (2017). Here we are not able to tell whether the difference between wins and losses is indeed due to a speeding after losses or due to a slowing after wins. However, earlier studies showed that a difference between wins and losses is mainly due to speeding after losses (Eben et al., 2020; Verbruggen et al., 2017), at least in tasks similar to the one used here.

In conclusion, as in Part I of the present study, we could replicate the seminal work by Langer and Roth (1975), showing that the emphasis of success at the beginning of a run enhances participants' estimation of their ability to predict outcomes of a coin-toss. However, this effect seemed short-lived. We could also replicate post-loss speeding in yet another gambling task. Importantly though, it seems that the illusion of control (at least, as defined by Langer and Roth (1975)) did not influence post-loss speeding, which is not in line with any of the theoretical accounts presented earlier.

Disclosure statement

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Ethical approval

Ethical committee at the Faculty of Psychology and Educational Science of Ghent University No 2019/86.

Preregistration statement

The pre-registrations associated with the experiment can be found on OSF: https://osf.io/qm2a8/registrations.

Data availability statement

The data that support the findings of these studies are openly available on OSF (https://osf.io/qm2a8/) at DOI 10.17605/OSF.IO/QM2A8 in the folder Langer & Roth replication.

Open Scholarship

This article has earned the Center for Open Science badges for Open Data, Open Materials and Preregistered. The data and materials are openly accessible at https://osf.io/qm2a8/

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Appendix.

Table A1. Demographics per group.

group	N	Age	sd	female	male	non-binary	prefer not to say
Experiment 1 ascending	200	26.16	8.81	86	112	1	1
descending	200	26.20	7.95	88	109	3	0
random	199	25.59	7.93	77	119	2	2
Experiment 2 ascending	200	25.70	8.54	84	115	2	0
descending	200	28.41	9.03	95	102	2	1
random	200	28.43	9.08	95	101	2	2

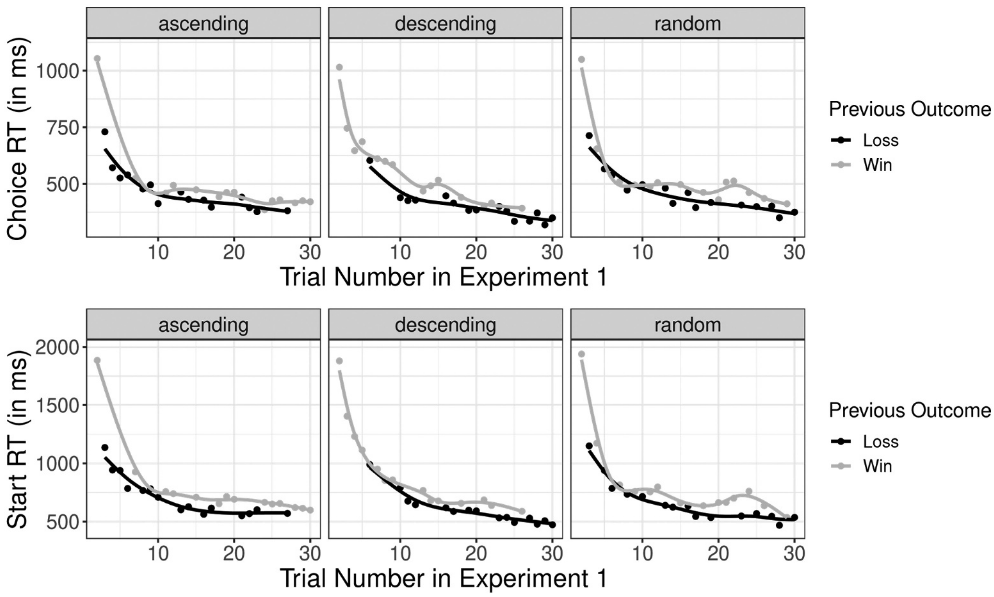

Figure A1. The mean latency data (in ms) of Experiment 1 as a function of trial number.

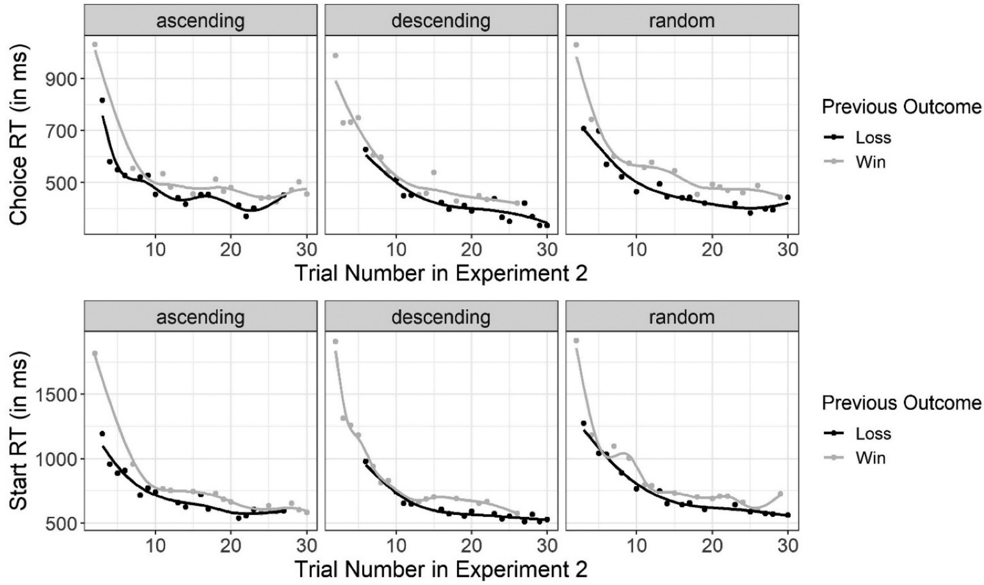

Figure A2. The mean latency data (in ms) of Experiment 2 as a function of trial number.

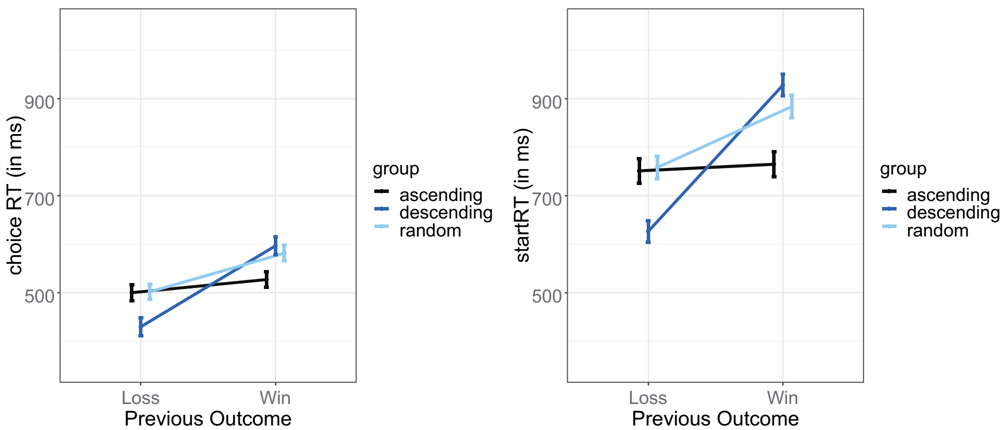

Figure A3. Mean choice RT and mean start RT (both in ms) in the first 30 trials in Experiment 2 as a function of previous outcome and group.