American Psychological Association

Decision

© 2019 American Psychological Association
ISSN: 2325-9965

2020, Vol. 7, No. 2, 153–162
http://dx.doi.org/10.1037/dec0000118

On the Impact of Experience on Probability Weighting in Decisions Under Risk

Doron Cohen, Ori Plonsky, and Ido Erev
Technion–Israel Institute of Technology

Previous research demonstrates that feedback in decisions under risk leads people to behave as if they give less weight to rare events. We clarify the boundaries of this phenomenon and shed light on the underlying mechanisms. In a preregistered experiment, participants faced 60 different decisions-under-risk choice tasks. Each task was a choice between a safe prospect (e.g., "59 with certainty") and a "rare disaster" gamble ("60 with $p = .98$; 10 otherwise"). Additionally, each option also incurred a small cost (a draw from the set $\{-8, -6, -4, -2, 0\}$ was added to the payoff). After each choice, participants received full feedback concerning the (total) realized payoffs of each option. The experiment compared 2 conditions that differed in the dependency between the 2 added costs. The results reveal high sensitivity to this dependency. Underweighting of rare events (preference for the rare disaster gamble) emerged with experience only when this dependency implied that in most cases, the rare disaster alternative provides a higher outcome than the safe alternative. In contrast, when in most cases the final outcomes from the safer option were higher, feedback appeared to increase the weighting of rare events (i.e., increased preference for the safe option). Common decisions-under-risk models (e.g., prospect theory) that assume the value of each prospect is judged only as a function of its own payoff distribution cannot account for this difference. Yet, the results can be explained with the hypothesis that choice reflects reliance on small samples of past experiences with similar decision tasks.

Keywords: decisions from experience, the description–experience gap, feedback in risky choice

Supplemental materials: http://dx.doi.org/10.1037/dec0000118.supp

This article was published Online First November 4, 2019.

Doron Cohen, Max Wertheimer Minerva Center for Cognitive Studies, Faculty of Industrial Engineering and Management, Technion–Israel Institute of Technology; Ori Plonsky, Faculty of Industrial Engineering and Management, Technion–Israel Institute of Technology; Ido Erev, Max Wertheimer Minerva Center for Cognitive Studies, Faculty of Industrial Engineering and Management, Technion–Israel Institute of Technology.

This research was supported by the I-CORE program of the Planning and Budgeting Committee and the Israel Science Foundation (Grant 1821/12).

Some of this work was done when Ori Plonsky was at the Center for Advanced Hindsight, Duke University.

The data are available at https://osf.io/de8jv/?view_only=6576e591ce8c40d3980501701112d796

The experiment materials are available at https://osf.io/2xyah/?view_only=74df41798b2f40b0843061273fba456a

The preregistered design and analysis plan is accessible at https://osf.io/de8jv/?view_only=52d2994ef9714c7298f1f77f0b704a52

Correspondence concerning this article should be addressed to Doron Cohen, Max Wertheimer Minerva Center for Cognitive Studies, Faculty of Industrial Engineering and Management, Technion–Israel Institute of Technology, Technion, Haifa 3200003, Israel. E-mail: doronco30@gmail.com

Mainstream studies of human decision making (e.g., Kahneman & Tversky, 1979) focus on binary decisions-under-risk tasks in which decision makers choose between two fully described payoff distributions. One important underlying assumption in these studies is that decision makers read, understand, and fully believe the provided descriptions. Under this assumption, adding feedback concerning the outcomes from previous choice should not affect future choice in the same task, as the feedback does not add any useful information. However, recent research rejects this assumption (e.g., Erev, Ert, Plonsky, Cohen, & Cohen, 2017; Fantino & Navarro, 2012; Jessup, Bishara, & Busemeyer, 2008; Lejarraga & Gonzalez, 2011; Marchiori, Di Guida, & Erev, 2015; Yechiam, Barron, & Erev, 2005). Whereas most studies of decisions under risk without feedback find behavior that appears as overweighting of rare events, studies of repeated decisions-under-risk with feedback find that the feedback triggers behavior consistent with underweighting of rare events.[1]

The current research tries to clarify the impact of feedback on decisions-under-risk. It compares two competing explanations, each representing a different abstraction of the underlying processes and a different family of descriptive models. The first family of models assumes that the attractiveness of prospects is determined by a subjective representation of their payoff distributions. Cumulative prospect theory (CPT; Tversky & Kahneman, 1992) is one prominent example. This family of models maintains the assumption that participants read, understand, and believe the information provided. It assumes that the described probabilities are subjectively transformed to weights according to an inverted-S shape subjective weighting function that overweighs small probabilities. In addition, CPT assumes that the subjective values of the possible outcomes are sensitive to a reference point. These assumptions suggest that the observed effect of experience on choice behavior can be the product of either the covert effect of experience on the subjective weighting function (see Abdellaoui, L'Haridon, & Paraschiv, 2011; Glöckner, Hilbig, Henninger, & Fiedler, 2016; Ungemach, Chater, & Stewart, 2009) or of reference point adaptation in response to experienced outcomes (see Köszegi & Rabin, 2006).[2]

The second family of models questions the assumption that decision makers necessarily read, understand, and believe the description of the payoff distributions. Rather, these models assume people tend to select the option that led to the best average outcome in a small sample of similar past experiences. Under this explanation, the description of the payoff distributions is only one of several cues that determine which past experiences are deemed most similar to the current task (see Erev et al., 2017; Marchiori et al., 2015). In particular, the most similar past experiences may sometimes be instances in which the decision maker should not have believed the information provided. This "reliance on small samples" hypothesis implies that the impact of feedback on binary decisions under risk is sensitive to dependencies between the two payoff distributions (e.g., Kareev, 2000; Plonsky, Teodorescu, & Erev, 2015). Whereas a decrease in the weighting of rare events with experience is predicted in many settings (as rare events are underrepresented in most small samples), the "reliance on small samples" hypothesis also predicts that certain dependencies will trigger the opposite effect. For example, this hypothesis implies that often agents will learn to prefer an option that includes a bad rare event ("a rare disaster"). Yet, when the dependency

[1] We use the term *overweighting of rare events* to refer to deviations from the prescription of the expected value (EV) rule toward the prescription of an "equal weighting" rule (selecting the option with the higher weighted average payoff assuming that all outcomes are equally likely). The term *underweighting of rare events* refers to deviations from the prescription of the EV rule toward the prescription of the "Median" rule (selecting the option that offers the higher median payoff). That is, we use these terms with reference to the objective probabilities that are explicitly described and not necessarily to the experienced probabilities. Moreover, we use these terms in the "as if" sense (we do not actually fit any probability weighting functions to the data).

[2] Abdellaoui et al. (2011), Glöckner et al. (2016), and Ungemach et al. (2009) focused on one-shot decisions based on free sampling. Previous research suggests that the tendency to underweight rare outcomes in this setting can be an effect of insufficient information (e.g., Broomell & Bhatia, 2014; Fox & Hadar, 2006; Hertwig, Barron, Weber, & Erev, 2004; Hertwig & Pleskac, 2018; Regenwetter & Robinson, 2017; Wulff, Mergenthaler-Canseco, & Hertwig, 2018). In these studies, participants usually do not sample enough observations to allow an accurate representation of the payoff distributions. The current analysis avoids this problem by focusing on decisions under risk based on full and accurate description of the payoff rule.

between the payoff distributions is such that in most small samples, the average payoff of the rare disaster option is lower than its alternative, agents will learn to behave as if they overweight the rare event.

To clarify this prediction, we chose to focus on situations in which the final outcome of each option can be decomposed to two parts: a principal payoff and a transaction cost. For example, the outcome of an investment in a specific asset reflects changes to the value of the asset minus the costs of buying and selling it. Specifically, the current analysis focuses on variants of the following problem[3]:

Problem 0:

Please choose one of the two options:

S: Win 59 + C_S.

R: Win 60 + C_R with probability .98, or win 10 + C_R otherwise.

Note: The values C_S and C_R will be uniformly drawn from the set {0, −2, −4, −6, −8}.

We designed two experimental conditions to introduce and test the effects of a specific dependency between the additional costs related to each option. In Condition "R-wins more," C_S is dependent on C_R in the following way:

$$C_s = \begin{cases} 0, & if\ C_R = -8 \\ C_R - 2, & if\ C_R \in \{0,\ -2,\ -4,\ -6\}. \end{cases} \tag{1}$$

In Condition "S-wins more":

$$C_R = \begin{cases} 0, & if\ C_S = -8 \\ C_S - 2, & if\ C_S \in \{0,\ -2,\ -4,\ -6\}. \end{cases} \tag{2}$$

Note that in both conditions, C_R and C_S have the exact same distribution, and therefore the payoff distributions of the final outcomes from Option S and from Option R are identical in both conditions. In addition, feedback does not add information concerning the payoff distribution of each option. However, these distinct dependencies imply that in Condition R-wins-more, the final outcome from Option R will be higher than that of Option S in 78.4% of the realizations. Conversely, in Condition S-wins-more, the final payoff from Option S will be larger than the payoff from Option R in 80.4% of the realizations.

A model assuming reliance on one (randomly sampled) past experience predicts, in repeated play of Problem 0, an R rate (proportion of choices of Option R) of 78% in Condition R-wins-more (Equation 1) and an R rate of 20% in Condition S-wins-more (Equation 2). A similar prediction is derived from the hypothesis that decision makers learn to prefer the alternative that minimizes the probability of experienced regret (e.g., Erev et al., 2017; Hart, Kareev, & Avrahami, 2016). For example, the model BEAST (a quantification of the reliance on small samples hypothesis for repeated decisions under risk, which was supported in a recent choice prediction competition, Erev et al., 2017), for an experiment in which decision makers face Problem 0 repeatedly for 60 trials, predicts R rates of 54% and 44% in Conditions R-wins-more and S-wins-more, respectively.

Explanations of the effect of experience under CPT and similar models predict something different. Specifically, the assumption that experience reduces the subjective weighting of rare events predicts a similar decrease in the sensitivity to rare events in both conditions. The exact predictions of the assumption that experience moves the reference point depends both on the assumed updating function of the reference point in light of observed outcomes and on the assumed shape of the CPT value function. For the former, the most natural assumption is that the reference point moves toward the average payoff. Under this assumption, the predictions for the two conditions are not expected to differ regardless of the exact shape of the value function (as the two conditions have the same underlying payoff distributions).

We preregistered the hypothesis that, because choice is a product of reliance on small samples, behavior would shift toward the alternative that is better most of the time (Erev et al., 2017;

[3] Another way to describe Problem 0 is as follows. Choose between:

S: (59, .2; 57, .2; 55, .2; 53, .2; 51),

R: (60, .196; 58, .196; 56, .196; 54, .196; 52, .196; 10, .004; 8, .004; 6, .004; 4, .004; 2), where the notation (x_1, p_1; x_2, p_2; . . . y) implies an option that gives an outcome of x_1 with probability p_1, x_2 with probability p_2, . . . , and y otherwise.

Erev & Roth, 2014). Thus, we predicted risk choice rates will be significantly higher in Condition R-wins-more, compared to risk choice rates in Condition S-wins-more. We also predicted a significant interaction between the order by which problems are presented and the experimental condition—specifically that participants will learn with experience to prefer the safe alternative in Condition S-wins-more and learn with experience to prefer the risky alternative in Condition R-wins-more.

Method

Participants

A power analysis (also reported in the preregistration) based on the results of a pilot study with 97 participants (power $= 0.95$, $\alpha = .05$, $\Omega_0^2 = .235$; see the online supplemental material for a full report of this study) indicated that a sample size of 61 participants in each of the two groups is suitable. We chose to slightly oversample until the random allocation mechanism assigned at least that number in each group, while maintaining roughly equal group sizes. In total, 135 participants were recruited using Prolific Academic (https://prolific.ac). As two participants did not finish the experiment, their data were omitted before any analysis took place and the final sample size consisted of 133 participants (66 females, $M_{\text{age}} = 30.9$, $SD_{\text{age}} = 9.9$, $\text{Range}_{\text{age}} = [18, 65]$).

Procedure

The experiment (and its hypotheses) was preregistered and conducted online. Participants faced a sequence of 60 variants of Problem 0, described above, in random order for one trial each. Appendix presents the 60 problems and the main results. Each of the 60 problems involved a choice between a risky option, R, and a safer option, S. Both alternatives offered the same or virtually the same (the largest difference was 0.01 points) expected values. Option R provided a high payoff (H) with probability .98 or a low payoff (L) with probability .02, plus an additional cost, C_R. Option S provided a medium payoff M with certainty (where $L < M < H$) plus an additional cost, C_S. Both C_R and C_S were uniformly distributed over the set $\{0, -2, -4, -6, -8\}$. Each combination of L, M, and H was presented only once.

The dependency between C_S and C_R was manipulated in two experimental conditions. In Condition "R-wins-more," Equation 1 determined the dependency of C_S on C_R, while in Condition "S-wins-more," Equation 2 determined the dependency of C_R on C_S. We used a between-subjects design, with 66 and 67 participants in Conditions R-wins-more and S-wins-more, respectively. The payoff distributions were fully described to participants in each of the 60 problems (as in Problem 0 above). The relationship between C_S and C_R was described in the instructions that participants read before starting the experiment. It stated that for each round, the cost for one of the alternatives would first be drawn from a uniform distribution over the set $\{0, -2, -4, -6, -8\}$, and then the cost for the second alternative would depend on the first draw, so that if the first draw was -8, the second cost would be 0. Otherwise, the second cost would be equal to the first draw -2 points (full instructions and typical screenshots appear in the online supplemental material).

Following each choice, participants saw the outcomes from their chosen alternative as well as the forgone outcome from the unchosen alternative. The alternatives were labeled as either "Option A" (appearing on the left of the screen) or "Option B" (appearing on the right). Allocation of the risky and safe alternatives to each label was decided randomly for each participant at the start of the experiment and remained fixed throughout the 60 rounds.

Participants were informed that they would earn a fixed show-up fee of £0.85 (about $1.12) and would also receive the outcome of their choice in one randomly selected round as a bonus, with a conversion rate of 1 experimental point = £0.005. Mean bonus was about £0.36 (about $0.46). The experimental session lasted about 10 min.

Results

Figure 1 presents the choice rates of Option R for the R-wins-more and S-wins-more conditions, as a function of the order in which the 60 problems were played (across participants). Choice rates by problem are presented in the Appendix. Across the 60 problems, the mean R rate was .66 in Condition R-wins-more, 95% CI [.604, .714], and .31 in Condition S-wins-more, 95% CI [.269, .356].

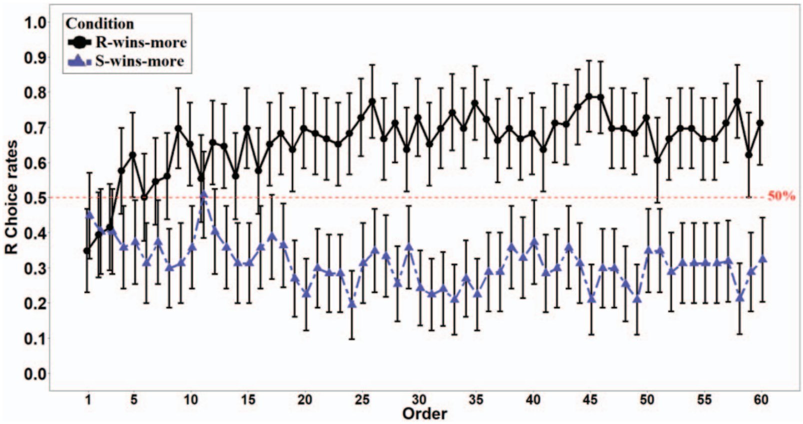

Figure 1. Average R choice rates across the 60 problems, as a function of order (Rounds 1–60) and different experimental conditions. Error bars represent the 95% CIs. See the online article for the color version of this figure.

To test the effects statistically, we implemented (using R package lme4; Bates, Mächler, Bolker, & Walker, 2015) a logistic mixed-effects model[4] with risk choice as the outcome variable, random intercepts for participants (SD = 1.08), and fixed effects for condition (two levels: R-wins-more and S-wins-more) and order (Rounds 1–60, treated as continuous variable), as well as the interaction between them. Parameter estimates use the penalized least squares method.[5]

Consistent with our preregistered hypotheses, we found significant main effects of Condition ($\chi^2(1) = 64.5$, $p < .001$) and Order ($\chi^2(1) = 6.83$, $p = .009$), as well as a significant interaction, $\chi^2(1) = 68.32$, $p < .001$ (see online supplemental material for the full regression table). A post hoc analysis shows that in Condition R-wins-more, the probability of choosing Option R increases as a function of order: $OR = 1.017$, 95% CI [1.013, 1.021], $Z = 7.62$, $p < .001$, whereas in Condition S-wins-more, the probability of choosing Option R decreases as a function of order: $OR = 0.992$, 95% CI [0.988, 0.996], $Z = -3.85$, $p < .001$. This finding supports the assertion that the attractiveness of Option R changes as a function of the common ranking of its payoff in previous similar tasks.[6]

Under one explanation of the current results, participants neglected the rare possibility of the risky option generating a low payoff (L with probability .02) and, because the high payoff from the risky option (H) and the sure payoff from the safe option (M) were very similar in our problems, participants chose only according to the ranking of the added costs (C_S and C_R). To evaluate this "similarity" hypothesis, Figure 2 summarizes the results obtained in the current study compared to the results of a study by Marchiori et al. (2015, Study 3, p. 97) that involved similar choice tasks, without additional costs. As in the current experiment, the participants in Marchiori et al. (N = 20) were presented with 60 distinct binary choice problems between two alternatives with similar EVs. Option R provided a high payoff (H in the range 101 to 104) with probability .95 or a low payoff (L in the range 80 to 0) with probability .05, and Option S provided M with certainty ($M = .95H + .05L + u$, u equals −1, 0, or 1). There

[4] Although we originally planned to use linear mixed-effect models to analyze the current results (as reported in the preregistered analysis plan), we later realized a logistic mixed-effect model is more appropriate for the current outcome variable. The originally planned analysis, which is in the online supplemental material, leads to essentially identical conclusions.

[5] Laplace approximation yielded nearly identical results but unfortunately did not converge.

[6] Our results demonstrate that biased past experiences can be highly influential in how rare unattractive outcomes are treated. In another study (reported in the online supplemental material), we obtain very similar results for rare attractive outcomes. Also, to answer some practical questions, in that study, participants were not informed about the relationship between C_S and C_R.

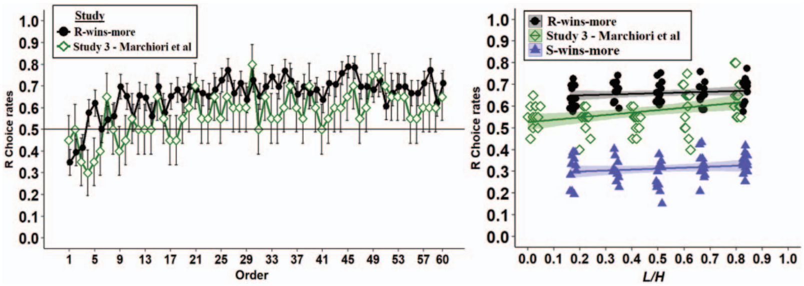

Figure 2. Left panel: Participant's average R choice rates as a function of order (Rounds 1–60) in Condition R-wins-more (black line) and Study 3 in Marchiori et al. (2015, p. 97). Error bars represent standard errors. Right panel: Average R choice rates in the two conditions of the current experiment (black and blue lines) and in Study 3 in Marchiori et al. (2015), as a function of the ratios between parameters L and H in each problem. Solid lines represent regression lines by condition, and shaded strips represent the 95% CIs. See the online article for the color version of this figure.

were no added "costs" involved, and Option R led to a better payoff in 95% of the trials.

The left-hand side of Figure 2 compares R rates as a function of the experience in Marchiori et al. (2015) and our R-wins-more condition, showing similar increasing R rates. This resemblance suggests that the added costs are not the sole driver of the effect of experience: If decision makers in such situations tend to neglect the difference between the payoff distributions, we would expect the choices in Marchiori et al. to be approximately random and with minimal learning.

The right-hand side of Figure 2 compares the aggregated R rates as a function of the ratio between L (a rare event implied by Option R and the lowest possible payoff) and H (the common event implied by Option R and the highest possible payoff) for each of the problems in our two conditions and in Marchiori et al. (2015). The correlation was not significant in our R-wins-more condition ($r_{\text{Pearson}} = .15$, $t(58) = 1.2$, $p = .250$) or in our S-wins-more condition ($r_{\text{Pearson}} = .17$, $t(58) = 1.3$, $p = .206$), yet it was significant in Marchiori et al. ($r_{\text{Pearson}} = .37$, $t(58) = 3.0$, $p = .004$). We plan to address this difference in sensitivity to the description of the possible payoffs in future research.

Implications to Descriptive Models

The current study rejects CPT-like models that assume a process of independent evaluations of the payoff distributions of each option, as this predicts the same choice rates should be observed in the two conditions of the current study. Our results agree with the predictions derived from the reliance-on-small-samples family of models. Importantly, the latter group of models we consider here can be seen as a subset of a wider set of models that compare the alternatives based on differences in their payoffs. These models can predict a gap between the current conditions but differ regarding the nature of the gap. To clarify the implications of the results, we chose to consider three interesting members of this class of "between-alternative comparison" models.

Regret theory (Bell, 1982; Loomes & Sugden, 1982) was proposed to highlight an alternative explanation to the (decisions-from-description) phenomena explained by prospect theory. For the current experimental design without "additional costs," as in Marchiori et al. (2015) described above, regret theory predicts overweighting of rare events. This prediction depends on the assumption that the magnitude of anticipated regret (i.e., the additive expected differences between the outcomes of each alternative) is more important than how probable the regret is. Thus, as the additional costs introduced in the current study do not change the theory's predictions, they cannot capture the difference in preferences observed in our two conditions.

The *priority heuristic* (Brandstätter, Gigerenzer, & Hertwig, 2006) assumes that alternatives are compared according to an ordered set of rules, starting with the assertion that if the min-

imal possible gains in both options differ by more than 1/10 of the maximal possible gain, the alternative with the higher minimal gain should be chosen (Brandstätter et al., 2006, p. 413). In the current settings, this heuristic implies a choice of S, regardless of the added costs. Thus, it is consistent with the initial behavior (in the first few rounds), but it is inconsistent with the observed effect of experience.

The *proportional difference model* (González-Vallejo, 2002) assumes situation-specific weighting of different dimensions implied by the choice alternatives. Whereas quantifications of this model used in previous research (González-Vallejo, 2002; Scheibehenne, Rieskamp, & González-Vallejo, 2009) do not predict the current results, the model can capture our results by assuming that experience increases sensitivity to the probability of success. Under this assumption, the proportional difference model approximates the predictions of the reliance-on-small-samples hypothesis.

Discussion

Previous research suggests that experience reduces sensitivity to rare and extreme outcomes. For example, experience often leads decision makers to favor an option that includes a rare "disaster" (an extreme bad outcome). This pattern is observed even in decisions-under-risk tasks, where the payoff distributions are fully described, and experience does not add any relevant information concerning the payoff distributions. The current research highlights a counterexample to this pattern: When choice of a "rare-disaster" alternative leads to a worse outcome with high probability, experience leads decision makers to behave as if their weighting of rare extreme outcomes increases.

The contradicting effects of experience on the implied weighting of rare events can help shed light on the underlying cognitive processes. First, these contradicting effects reject the hypothesis that feedback has a general effect on the shape of the subjective weighting function that underlies decisions under risk. To capture the current results, there is no need to assume any underlying weighting function. Second, the contradicting effects lend credence to the assumption that decision makers tend to select the strategy that led to the best outcome in a small set of similar past experiences. This assumption commonly predicts behavior that appears as underweighting of rare events but also predicts that in some cases, the opposite pattern, behavior that appears as overweighting of rare events, would emerge. Specifically, this happens when the common past experience from choosing the option that includes the rare event is in the same direction as the rare event (i.e., the common past experience and the rare event are either both bad or both good). Third, the contradicting effects of experience question the assumption that participants "read, understand, and believe" the instructions and/or the descriptions of payoff distributions. We believe our results suggest instructions of tasks are used instead as a cue to draw on the experiences that are most similar to the current situation. Another likely possibility is that feedback clarifies ambiguities stemming from the instructions. Thus, in complex tasks, feedback might be a necessary condition for the type of learning we observe.

One practical implication of the current results involves the possibility to design a choice architecture that nudges people toward a desirable choice by altering the relative ranking of the available options and without changes to the payoff distributions (i.e., Thaler & Sunstein, 2009). For example, assume regulations do not allow health providers to punish patients for missing their doctor's appointment. Yet, the provider can alter the distribution of an outside cost function, for instance, the imposed friction or delays when scheduling a new appointment. Our results imply that increasing such friction in most cases for "no-show" patients, and greatly reducing the friction in rare cases (such that the average friction does not change), should decrease the rate of patients missing their appointments. That is, if in most cases missing an appointment slightly increases the cost of scheduling a new appointment, patients would learn to miss fewer appointments.

In sum, the current work clarifies the observation that experience affects choice behavior even when it does not objectively provide useful information. It shows that the results cannot be captured by assuming a general impact of experience on the underlying weighting functions. Yet, it can be naturally captured by assuming that people react to the descriptions of payoff distributions by selecting the options that led to the best outcomes under similar descriptions in the past.

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Appendix

The 60 Choice Problems and Mean Choice Rates in Both Conditions in the Current Study

				Choice alternatives			
	Problem parameters			R	S	R rates by condition, %	
Problem	*L*	*M*	*H*	(*L* with probability .02; *H* otherwise) + C_R	$M + C_S$	R wins more	S wins more
1	51.3	60.6	60.8	(51.3, .02; 60.8) + C_R	60.6 + C_S	71	33
2	51.1	60.4	60.6	(51.1, .02; 60.6) + C_R	60.4 + C_S	67	30
3	50.9	60.2	60.4	(50.9, .02; 60.4) + C_R	60.2 + C_S	69	37
4	50.7	60	60.2	(50.7, .02; 60.2) + C_R	60 + C_S	77	34
5	50.8	60.6	60.8	(50.8, .02; 60.8) + C_R	60.6 + C_S	71	32
6	50.6	60.4	60.6	(50.6, .02; 60.6) + C_R	60.4 + C_S	73	42
7	50.4	60.2	60.4	(50.4, .02; 60.4) + C_R	60.2 + C_S	69	25
8	50.2	60	60.2	(50.2, .02; 60.2) + C_R	60 + C_S	62	39
9	50.3	60.6	60.8	(50.3, .02; 60.8) + C_R	60.6 + C_S	61	29
10	50.1	60.4	60.6	(50.1, .02; 60.6) + C_R	60.4 + C_S	58	36
11	49.9	60.2	60.4	(49.9, .02; 60.4) + C_R	60.2 + C_S	71	33
12	49.7	60	60.2	(49.7, .02; 60.2) + C_R	60 + C_S	61	34
13	41.3	60.4	60.8	(41.3, .02; 60.8) + C_R	60.4 + C_S	71	28
14	41.1	60.2	60.6	(41.1, .02; 60.6) + C_R	60.2 + C_S	65	33
15	40.9	60	60.4	(40.9, .02; 60.4) + C_R	60 + C_S	58	27
16	40.7	59.8	60.2	(40.7, .02; 60.2) + C_R	59.8 + C_S	68	31
17	40.8	60.4	60.8	(40.8, .02; 60.8) + C_R	60.4 + C_S	65	36
18	40.6	60.2	60.6	(40.6, .02; 60.6) + C_R	60.2 + C_S	58	33
19	40.4	60	60.4	(40.4, .02; 60.4) + C_R	60 + C_S	59	36
20	40.2	59.8	60.2	(40.2, .02; 60.2) + C_R	59.8 + C_S	65	43
21	40.3	60.4	60.8	(40.3, .02; 60.8) + C_R	60.4 + C_S	63	30
22	40.1	60.2	60.6	(40.1, .02; 60.6) + C_R	60.2 + C_S	76	36
23	39.9	60	60.4	(39.9, .02; 60.4) + C_R	60 + C_S	64	21
24	39.7	59.8	60.2	(39.7, .02; 60.2) + C_R	59.8 + C_S	69	42
25	31.3	60.2	60.8	(31.3, .02; 60.8) + C_R	60.2 + C_S	71	15
26	31.1	60	60.6	(31.1, .02; 60.6) + C_R	60 + C_S	62	32
27	30.9	59.8	60.4	(30.9, .02; 60.4) + C_R	59.8 + C_S	68	35
28	30.7	59.6	60.2	(30.7, .02; 60.2) + C_R	59.6 + C_S	75	33
29	30.8	60.2	60.8	(30.8, .02; 60.8) + C_R	60.2 + C_S	65	21
30	30.6	60	60.6	(30.6, .02; 60.6) + C_R	60 + C_S	61	33
31	30.4	59.8	60.4	(30.4, .02; 60.4) + C_R	59.8 + C_S	70	38
32	30.2	59.6	60.2	(30.2, .02; 60.2) + C_R	59.6 + C_S	64	24
33	30.3	60.2	60.8	(30.3, .02; 60.8) + C_R	60.2 + C_S	74	30

(*Appendix continues*)

Appendix (*continued*)

	Problem parameters			Choice alternatives		R rates by condition, %	
				R	S		
Problem	L	M	H	(L with probability .02; H otherwise) + C_R	$M + C_S$	R wins more	S wins more
34	30.1	60	60.6	(30.1, .02; 60.6) + C_R	60 + C_S	62	24
35	29.9	59.8	60.4	(29.9, .02; 60.4) + C_R	59.8 + C_S	66	23
36	29.7	59.6	60.2	(29.7, .02; 60.2) + C_R	59.6 + C_S	62	25
37	21.3	60	60.8	(21.3, .02; 60.8) + C_R	60 + C_S	59	23
38	21.1	59.8	60.6	(21.1, .02; 60.6) + C_R	59.8 + C_S	68	27
39	20.9	59.6	60.4	(20.9, .02; 60.4) + C_R	59.6 + C_S	74	34
40	20.7	59.4	60.2	(20.7, .02; 60.2) + C_R	59.4 + C_S	67	29
41	20.8	60	60.8	(20.8, .02; 60.8) + C_R	60 + C_S	67	29
42	20.6	59.8	60.6	(20.6, .02; 60.6) + C_R	59.8 + C_S	71	25
43	20.4	59.6	60.4	(20.4, .02; 60.4) + C_R	59.6 + C_S	70	30
44	20.2	59.4	60.2	(20.2, .02; 60.2) + C_R	59.4 + C_S	67	30
45	20.3	60	60.8	(20.3, .02; 60.8) + C_R	60 + C_S	62	37
46	20.1	59.8	60.6	(20.1, .02; 60.6) + C_R	59.8 + C_S	62	40
47	19.9	59.6	60.4	(19.9, .02; 60.4) + C_R	59.6 + C_S	62	31
48	19.7	59.4	60.2	(19.7, .02; 60.2) + C_R	59.4 + C_S	71	30
49	11.3	59.8	60.8	(11.3, .02; 60.8) + C_R	59.8 + C_S	60	30
50	11.1	59.6	60.6	(11.1, .02; 60.6) + C_R	59.6 + C_S	64	33
51	10.9	59.4	60.4	(10.9, .02; 60.4) + C_R	59.4 + C_S	70	39
52	10.7	59.2	60.2	(10.7, .02; 60.2) + C_R	59.2 + C_S	64	19
53	10.8	59.8	60.8	(10.8, .02; 60.8) + C_R	59.8 + C_S	59	36
54	10.6	59.6	60.6	(10.6, .02; 60.6) + C_R	59.6 + C_S	73	39
55	10.4	59.4	60.4	(10.4, .02; 60.4) + C_R	59.4 + C_S	58	39
56	10.2	59.2	60.2	(10.2, .02; 60.2) + C_R	59.2 + C_S	68	37
57	10.3	59.8	60.8	(10.3, .02; 60.8) + C_R	59.8 + C_S	62	21
58	10.1	59.6	60.6	(10.1, .02; 60.6) + C_R	59.6 + C_S	70	33
59	9.9	59.4	60.4	(9.9, .02; 60.4) + C_R	59.4 + C_S	64	28
60	9.7	59.2	60.2	(9.7, .02; 60.2) + C_R	59.2 + C_S	59	21

Note. Each problem presented a choice between an Option R that yields a payoff of L with probability .02 plus the outcome of C_R or H with probability .98 plus the outcome of C_R, and an Option S that yields a payoff of M with probability 1 plus the outcome of C_S. C_R and C_S are uniformly drawn from the set {0, −2, −4, −6, −8}.

Received April 21, 2019
Revision received July 31, 2019
Accepted September 23, 2019 ■