Supplementary Materials for
The Persistence of Common-Ratio Effects in Multiple-Play Decisions

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Survey materials and data appear in separate files.

Do Common-Ratio Choice Patterns in Multiple-Play Decisions Violate EU Theory?

Although the initial interest in single- versus multiple-play decisions was driven largely by normative issues (Samuelson, 1963; for reviews, see Aloysius, 2007; Wedell, 2011), we are not aware of any normative analysis of common-ratio choice patterns in multiple-play decisions. As noted in the main text, it is well known that the standard common-ratio choice pattern and the reverse pattern both violate expected utility (EU) theory in single-play decisions. Simple algebra proves the point. However, it does not necessarily follow that the same choice patterns violate EU theory in multiple-play decisions. In multiple play, each risky option has a binomial distribution of aggregate outcomes, which makes it difficult to tell whether a particular choice pattern can be explained by some utility function.

We conducted an initial exploration of whether the standard common-ratio choice pattern and the reverse common-ratio choice pattern can be consistent with EU theory in multiple-play decisions. Our exploration was limited in two ways. First, we considered only the common-ratio problems used in our studies, previous related studies (Barron & Erev, 2003, Study 5; Keren, 1991; Keren & Wagenaar, 1987; see Table S.1), and Kahneman and Tversky (1979) and the numbers of plays used in those studies (1, 5, 10, and 100 plays). Second, for the possible utility functions, we considered only logarithmic functions, $u(x) = \ln(x)$ (with $u(x) = 0$ when $x = 0$) and $u(x) = \ln(x + 1)$, and power functions, $u(x) = x^b$, with the exponent b ranging from 0.01 to 3.00 in steps of 0.01. The logarithmic functions take the place of a power function with an exponent of zero and are not discussed further (for an accessible introduction to the power family of utility functions, see Wakker, 2008). We conducted these analyses in Excel. For brevity, we label the standard common-ratio choice pattern as CR; the reverse common-ratio pattern as RCR;

choosing the riskier, higher-EV option in both problems as HH; and choosing the less risky, lower-EV option in both problems as LL.

Under the conditions described above, we did not observe any instances in which the CR pattern is consistent with EU theory in multiple-play decisions (obviously, this negative finding is not a proof that the CR pattern can never be consistent with EU theory). On the other hand, we did find situations in which the RCR pattern is consistent with EU theory. In other words, for a given number of plays of a given set of problems (say, our possibility-effect problems 4 and 10), there is a range of exponents for the power utility function for which the riskier, higher-EV option has a higher EU than the less risky, lower-EV option in the scaled-up problem (Problem 10) and the less risky, lower-EV option has a higher EU than the riskier, higher-EV option in the scaled-down problem (Problem 4). Choosing in accordance with such preferences yields the RCR pattern.

More specifically, for our possibility-effect problems 4 and 10, the RCR pattern is observed to be consistent with EU for $0.30 \leq b \leq 0.78$ when there are 5 plays, $0.03 \leq b \leq 0.77$ when there are 10 plays, and $0.01 \leq b \leq 0.61$ when there are 100 plays. For higher values of b (less risk aversion), the riskier, higher-EV option is preferred in both problems (the HH pattern); for lower values of b (more risk aversion), the less risky, lower-EV option is preferred in both problems (the LL pattern; this pattern is not observed for 100 plays).

For our certainty-effect problems 2 and 8, the RCR pattern is observed for $0.01 \leq b \leq 0.29$ when there are 5 plays, $0.01 \leq b \leq 0.16$ when there are 10 plays, and never when there are 100 plays. For higher values of b (less risk aversion), the HH pattern is observed (this is always the case for 100 plays); the LL pattern is never observed for 5, 10, or 100 plays. Qualitatively similar results are observed for the certainty-effect problems in Keren (1991) and Barron and Erev

(2003, Study 5). For the near-certainty-effect problems in Keren and Wagenaar (1987), the results are similar except that the LL pattern is observed for $0.01 \leq b \leq 0.16$ when there are 5 plays (but never when there are 10 or 100 plays).

There is an intuitive explanation for these results. In a single set of common-ratio problems in single play, a person who follows EU theory (as assumed throughout this section) chooses the riskier, higher-EV option (H) in both problems if he or she is not especially risk averse and chooses the less risky, lower-EV option (L) in both problems if he or she is more risk averse. The dividing line between people who choose H and people who choose L happens at exactly the same level of risk aversion in both problems (between 0.43 and 0.44 for our certainty-effect problems 2 and 8 and between 0.79 and 0.80 for our possibility-effect problems 4 and 10), so an EU decision maker chooses either LL or HH in each set of problems accordingly.

As the number of plays increases, the means of the binomial outcome distributions increase linearly, but the standard deviations of those distributions increase more slowly (according to the square root of the number of plays).[1] In terms of the coefficient of variation (Klos, Weber, & Weber, 2005; Weber, Shafir, & Blais, 2004), riskiness decreases as the number of plays increases. The result is that the distributions for the two options become more distinct and separated, making the superiority of the higher-EV option more obvious (for example, the likelihood of coming out ahead by choosing H rather than L increases). As this happens, people who are somewhat risk averse switch from choosing L to choosing H, whereas only really risk-averse people stick with L (because they don't value the higher outcomes as much as less risk-averse people do). So there is a general shift from L to H as the number of plays increases.

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[1] For a certain option, increasing the number of plays does not increase the standard deviation of the outcome distribution (it remains equal to zero). In problems that involve a certain option, the effect of increasing the number of plays depends only on the outcome distribution for the risky option, but the rest of the explanation remains the same.

However (and this is key), this switching happens at different rates for different problems, which means that a person can switch from L to H in one problem (where the distinction between options has become clear) but not in the other problem (where the distinction has not yet become clear). More specifically (and this is also key), this transition form L to H happens more quickly for scaled-up problems than for scaled-down problems as the number of plays increases. The intuition for this difference is that the probabilities of the nonzero outcomes are higher in scaled-up problems than in scaled-down ones, so the differences between the outcome distributions for the options in scaled-up problems materialize (i.e., become clear enough for decision purposes) more quickly as the number of plays increases.[2]

This reasoning implies that for some levels of risk aversion, a person can choose H on the scaled-up problem and L and the scaled-down problem (the RCR pattern) without violating EU theory. But the opposite pattern (the standard CR pattern) is not allowed. Eventually, as the number of plays increases, the LL pattern is squeezed out by the RCR pattern, which is then squeezed out by the HH pattern. In other words, given a moderate to high level of risk aversion (a low exponent *b*), the shift from the LL pattern to the HH pattern as the number of plays increases can occur via the RCR pattern but not via the CR pattern.

Interestingly, these transitions from L to H appear to happen faster in certainty-effect problems like our Problems 2 and 8 than in possibility-effect problems like our Problems 4 and 10. As a result, the RCR pattern is more quickly driven out by the HH pattern in certainty-effect problems than in possibility-effect problems (see the *b* ranges for the RCR pattern above). The

[2] Although this section focuses on participants who maximize EU, this reasoning may be more generally applicable. Because the separation of the outcome distributions in multiple play is larger for scaled-up problems than for scaled-down problems, one might expect more switching to the higher-EV option in multiple plays of scaled-up problems than in multiple plays of scaled-down problems. If so, then intuitions of this sort could partially explain the interactions in Figures 1–3 of the main text.

intuition is that the lower probabilities in possibility-effect problems (especially in the scaled-down problem) mean that more plays are required to differentiate the outcome distributions. More specifically, because of the much larger ratio between the probabilities in the scaled-up and scaled-down possibility-effect problems (e.g., a ratio of 45), the outcome distributions become differentiated much more quickly in the scaled-up problem than in the scaled-down problem, creating a larger and longer-lasting window for the RCR pattern.

A few caveats are in order. First, our analyses were limited to a small number of problems, three levels of multiple play, and one family of utility functions. We cannot rule out the possibility that the CR pattern might be consistent with EU theory under other circumstances. Second, although our analyses indicate that the RCR pattern is consistent with EU theory for some numbers of plays and some levels of risk aversion, the required levels of risk aversion may have very unreasonable implications for decisions with larger stakes (Rabin, 2000). In Rabin's view, people who show much risk aversion at all in problems like ours (even with the stakes implied by 100 plays) are not really EU maximizers anyway, even if their decisions appear to be consistent with EU in analyses like ours. Third, even if the above results are accepted at face value, the possibility that the RCR pattern can be consistent with EU theory in multiple-play decisions does not in any way imply that this consistency underlies the emergence of the RCR pattern in our primary studies. For example, although the above analyses indicate that there is a greater opportunity for the RCR pattern to be consistent with EU theory in our possibility-effect problems than in our certainty-effect problems, we observed no such asymmetry in the standard conditions of our within-participants studies (see Figure 6 in the main text).

In summary, the reverse common-ratio choice pattern is sometimes consistent with EU theory in multiple-play decisions (Rabin's, 2000, objection notwithstanding). We did not find this result for the standard common-ratio choice pattern, though our analyses were by no means exhaustive. Because other expectation-based models such as prospect theory (Kahneman & Tversky, 1979; Tversky & Kahneman, 1992) are generalizations of EU theory, these results suggest that the inability of these models to account for reverse common-ratio effects in single-play decisions (Blavatskyy, 2010; Nebaut & Dubois, 2014) may not extend to multiple-play decisions. This possibility, which focuses on descriptive explanation rather than normative assessment, warrants further investigation.

Our Original Rationale

Our first study, Study 1a, was originally intended to assess whether the elimination of certainty and possibility effects in multiple-play decisions would also occur in several other situations. More specifically, we expected that the effect of multiple plays would be moderated by participants' views regarding the reasonableness of aggregating outcomes over multiple plays (i.e., by the *perceived fungibility* of the outcomes), as in previous studies of decisions for risky prospects (DeKay & Kim, 2005; also see DeKay, 2011; DeKay et al., 2006).

Our rationale for the study followed from the central role of outcome aggregation in multiple-play decisions. For example, Wedell and Böckenholt (1994, p. 505) expressed the importance of aggregation in decisions about multiple plays of a mixed, positive-EV bet as follows: "Given that one has refused a single play of the bet, a logically necessary condition for accepting multiple plays of that gamble is that the subject recognizes a change in the outcome distribution. Subjects who process multiple plays of a gamble as essentially the 'same' as a single play should likewise refuse multiple plays of the gamble." However, DeKay and Kim (2005) noted that it may not always be reasonable to aggregate outcomes across multiple plays. For example, if different patients respond differently to the same medical treatment, the health gains experienced by some patients do not offset the health losses experienced by other patients in any real sense (Asch & Hershey, 1995). In such cases, participants may consider the distribution of possible aggregate outcomes to be irrelevant, leaving multiple-play decisions no different from single-play decisions.

To test the role of perceived fungibility in multiple-play decisions, DeKay and Kim (2005) developed several scenarios in which the outcomes of multiple plays were expected to be perceived either as “more fungible” (monetary gambles with outcomes going to oneself or to one other person, gambles involving frequent-flier miles in one account, and gambles involving meal tickets that may be used on any day) or as “less fungible” (monetary gambles with outcomes going to different people, gambles involving frequent-flier miles in different accounts, gambles involving meal tickets that may be used only on specific days, and a risky medical treatment for different patients; the latter scenario was based on one developed by Redelmeier & Tversky, 1990, and studied by DeKay et al., 2000). Results indicated that mixed, positive-EV gambles were viewed more favorably in multiple play when the outcomes were considered more fungible (i.e., the usual effect of multiple plays was replicated), but this effect was greatly diminished when outcomes were considered less fungible.

We designed Study 1a to extend DeKay and Kim’s (2005) results to gambles related to common-ratio effects. The new study used the same scenarios and the same rating and choice questions as DeKay and Kim’s study, but treated the number of plays as a between-participants variable. We predicted that common-ratio effects would be eliminated or greatly reduced for multiple plays of gambles with more fungible outcomes, because the riskier, higher-EV option in the scaled-up problem would be more appealing in multiple play, as in Keren and Wagenaar’s (1987) and Keren’s (1991) studies. We also predicted that this reduction would be absent, or much smaller, for multiple plays of gambles with less fungible outcomes (i.e., we predicted that common-ratio effects would persist in those situations). To our surprise, however, the certainty and possibility effects remained large and significant in *all* of the multiple-play conditions, including the standard one that involved monetary gambles for oneself. This failure to replicate a previously well-established effect meant that it was not practical to look for a reduction of that effect in situations with less fungible outcomes. For brevity, we consider only the standard monetary-gamble condition in this article. We ignore all questions related to perceived fungibility.

Primary Differences Between Studies

This section highlights the primary differences among our several studies, along with our rationales for these changes. There is substantial overlap with the corresponding section of the main text, but this section provides some additional details.

In Study 1b, we simplified the design of Study 1a by using only monetary gambles for oneself. We also added a 100-play condition to strengthen the multiple-play manipulation. For simplicity, Studies 1a and 1b are combined in the main text and in all analyses, with 81 participants from Study 1a and 120 participants from Study 1b.

The most obvious difference between our Study 1 and Keren and Wagenaar's (1987) studies is that we assessed the certainty and possibility effects within participants (as did Barron & Erev, 2003, for the certainty effect) rather than between participants. In Study 2, we adopted a completely between-participants design similar to that in Keren and Wagenaar's studies, with each participant making only one of the four key choices (Problem 2, 4, 8, or 10) in either the 1-play, 10-play, or 100-play condition. In order to collect a large sample relatively quickly, we administered the study as a short paper-based survey on a busy university sidewalk.

Because the online and sidewalk administration of Studies 1 and 2 may have led some participants to consider the task less seriously than we would have liked, we conducted Studies 3–6 in a monitored lab setting. We returned to our within-participants assessment of certainty and possibility effects, but tried to strengthen the multiple-play manipulation by clarifying the wording of the problems, as noted in the main text. In Study 3, we also added a new condition designed to encourage participants to adopt a long-run perspective. This condition and similarly motivated conditions in Studies 4 and 5 (see the section on long-run-prompt conditions below) might be expected to facilitate the choice of the higher-EV option, thereby reducing the certainty and possibility effects, especially in multiple play. To allow for these additional conditions, we dropped Study 1's problem-order manipulation in Studies 3–5. In Studies 4 and 5, we also examined the possible moderating effects of participants' *numeracy*, defined as "the ability to process basic probability and numerical concepts" (Peters, Västfjäll, Slovic, Merz, Mazzocco, & Dickert, 2006, p. 407; also see Peters, 2012), using an eight-item scale previously shown to have good psychometric properties (Weller, Dieckmann, Tusler, Mertz, & Peters, 2013).

To further address possible concerns regarding participants' level of engagement, we also altered our procedures to use real monetary payoffs rather than hypothetical ones. We tested the required changes with hypothetical payoffs in Study 4 before implementing them with real payoffs in Study 5. Specifically, we lowered the stakes in both the 1-play and 100-play conditions so that we could afford the payments (see Table S.2) and lowered the stakes in the 100-play condition even further, by multiplying the payoffs by 0.01. For the latter change, we used cents rather than dollars in the 100-play condition. Reducing payments in proportion to the number of plays is a popular way to equate EVs and payoff ranges in the single- and multiple-play conditions (see, e.g., Keren & Wagenaar, 1987, Studies 1 and 2). Participants in our Study 5 played one of the 11 gambles (selected at random) for real money before leaving the session. Keren (1991) used a somewhat similar procedure, though payoffs in the multiple-play (5-play) condition were not reduced and only one participant from each group of 8 to 12 was paid.

Although participants in Studies 4 and 5 were reminded of the number of plays many times over the course of the study (e.g., the number **ONE HUNDRED** appeared 34 times in the standard multiple-play condition), the results made us wonder whether some participants had simply tuned out that information. In Study 6, we repeated the standard condition of Study 4 (using dollars but not cents and including the numeracy assessment) and added a manipulation check. Immediately after the final choice, participants were asked, "You have just made several choices between two options. In each choice, how many times would the gamble in your chosen option be played?" We expected about 90% of participants to answer this question correctly, but that was not the case (see the section on manipulation-check results below).

Participants' poor performance on the manipulation-check question in Study 6 indicated that many participants were not attending to the number of plays, especially in the single-play condition. Even when participants did attend to the number of plays, as evidenced by their correct responses to the manipulation-check question, this information had essentially no effect on the magnitude of the certainty and possibility effects (see Figures 1 and 2 in the main text). These results raised the possibility that the length of the task (11 choices) led participants to process information about the number of plays rather superficially, even when they noticed and remembered it. Although our between-participants Study 2 was obviously shorter, that study put less emphasis on the number of plays (relative to our later studies) and yielded results that were somewhat messier than those in our other studies (see Figures 1 and 2 in the main text).

Study 7 was initially designed as a much larger between-participants study with a stronger manipulation of the number of plays than in any of our other studies. In addition to explaining the number of plays and putting the number of plays into the primary choice question, we also put the number of plays into the response options for that question. For example:

> Which option would you choose to play ONE AND ONLY ONE time [ONE HUNDRED times]?
>
> ONE AND ONLY ONE play [ONE HUNDRED plays] of this option:
> 25% chance [on each play] that you get $60
> 75% chance [on each play] that you get no money
>
> ONE AND ONLY ONE play [ONE HUNDRED plays] of this option:
> 20% chance [on each play] that you get $100
> 80% chance [on each play] that you get no money

Rather than having each participant make only one choice (as in Study 2), we added a second choice question with the other member of the same problem pair. For example, if a participant answered the scaled-down certainty-effect problem first, he or she answered the scaled-up certainty-effect problem second (Problems 2 and 8, respectively, in Table S.2). For this reason, we could use all of the data in within-participants tests or just the first response from each participant in between-participants tests (Study 1 was similar in the regard, because half of the participants considered the 11 problems in reverse order). In Study 7, we also randomized the order of the response options within problems. To keep the survey short, we did not include any filler problems. However, we did add a manipulation-check question that referred to only the most recent choice: "In the choice that you just made, how many times would your chosen option be played?" The manipulation in Study 7 was much more successful than that in Study 6 (see the section on manipulation-check results below).

Results for Preference Ratings

For each problem in Studies 1–6, participants made a preference rating on a nine-point bipolar scale (from *Strongly prefer option A* to *Strongly prefer option B*) prior to making a binary choice. This procedure was adopted from DeKay and Kim (2005), who observed similar results for ratings and choices. More specifically, those authors found the usual effect of multiple plays on participants' views of mixed, positive-EV monetary gambles for both dependent variables. In the main text, we focus on binary choices rather than preference ratings, both for brevity and for consistency with earlier studies that did not include preference ratings. In Study 7, we omitted preference ratings for greater consistency with those earlier studies. That the results for choices in Study 7 closely matched those for choices in Studies 1–6 (see Figures 1 and 2 in the main text) should allay concerns about the possible effects of preference ratings on participants' choices.

Figure S.1 depicts results for mean preference ratings for problems related to the certainty and possibility effects in the standard conditions of Studies 1–6. These results are nearly identical to those for binary choices in the bottom rows of Figures 1 and 2 of the main text. Based on this similarity, it did not seem worthwhile to repeat our other analyses using preference ratings as the dependent variable.

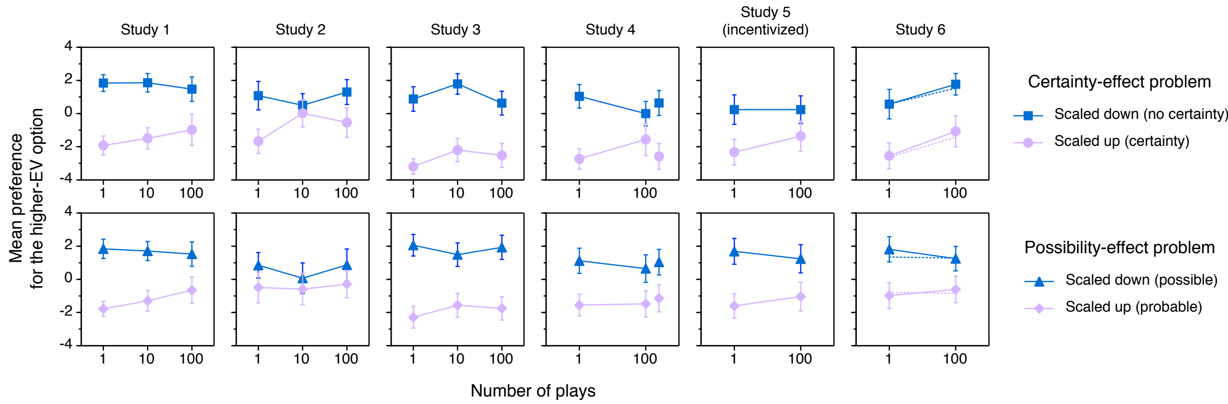

Figure S.1. Mean preference ratings for the higher-EV option in problems related to the certainty effect (top) and the possibility effect (bottom) in the standard conditions of our Studies 1–6. In Study 4, the results for 100 plays with cents appear to the right of those for 100 plays with dollars. In Study 6, solid lines show results for participants who answered the manipulation-check question correctly; dotted lines (without error bars) show results for all participants. Error bars indicate 95% CIs. These results may be compared to those for binary choices in the bottom rows of Figure 1 (for the certainty effect) and Figure 2 (for the possibility effect) in the main text.

Manipulation-Check Results in Studies 6 and 7

In Study 6, seven participants did not answer the manipulation-check question or responded with text that could not be coded as a specific number (e.g., “all” or “100%”). We addressed the latter problem by adding the instruction “Please enter a number” after the first few study sessions. Of the remaining 174 participants, only 60% correctly answered the manipulation-check question (53% in the 1-play condition and 69% in the 100-play condition), despite the fact that the numbers **ONE** and **ONE HUNDRED** were used 22 and 34 times, respectively, over the 11 choices in the task. More positively, responses in the 100-play condition were substantially higher than those in the single-play condition, *M*s = 77.4 and 2.9, respectively, Wilcoxon $p < .001$. Participants in the 100-play condition were also much more likely to say that they had considered “which option would lead to the higher total amount of money in the ‘long run,’ if the gambles were played many times,” 84% vs. 42%, Fisher exact $p < .001$. More important, our primary results regarding the certainty and possibility effects were essentially the same whether we considered all participants or only those who correctly reported the number of plays (compare the dotted and solid lines in the Study 6 panels of Figures 1 and 2 in the main text).

In Study 7, we strengthened the manipulation by using the words ONE AND ONLY ONE in the single-play condition and by including the number of plays in the response options themselves. In that study, 88% of participants correctly reported the number of plays (89% in single play, 87% in multiple play). As in Study 6, responses in the 100-play condition were substantially higher than those in the single-play condition, *M*s = 91.8 and 3.9, respectively, Wilcoxon $p < .001$. Also as before, participants in the 100-play condition were much more likely to say that they considered the total amount of money that could be won, 79% vs. 28%, Fisher exact $p < .001$. Finally, our primary results regarding the certainty and possibility effects were nearly identical whether we considered all participants or only those who correctly reported the number of plays (compare the dotted and solid lines in the Study 7 panels of Figures 1 and 2 in the main text).

Although the manipulation in Study 7 was much more successful than that in Study 6, we cannot say whether this improvement was the result of the stronger manipulation, the slightly different manipulation-check question, the use of 2 rather than 11 choices, or the different sample of participants. Participants who did not answer the manipulation-check question correctly were excluded from further analyses in Studies 6 and 7.

Descriptions and Analyses of the Long-Run-Prompt Conditions in Studies 3–5

In addition to the standard conditions discussed in the main text, our Studies 3–5 also included one or more conditions designed to push participants toward adopting a long-run view. Our thinking was that these conditions might increase the appeal of higher-EV options, thereby reducing the magnitude of certainty and possibility effects, especially in multiple play. Thus, we were primarily interested in whether the Problem × Plays interaction noted for our standard conditions (see Figures 1 and 2 and the right panels of Figures 4 and 5 in the main text) would be enhanced in these long-run-prompt conditions.

In the long-run-prompt condition of Study 3, which we label the *more-on-average* condition, participants answered two additional questions before choosing between the two gambles in each pair. The first question asked "On average, would you make more money with Option A or Option B?" The second asked, "Imagine that Andy chooses Option A and plays it ONE [TEN / ONE HUNDRED] time[s] and Brad chooses Option B and plays it ONE [TEN / ONE HUNDRED] time[s]. After ONE [TEN / ONE HUNDRED] play[s], what is the percentage chance that Andy will win more money than Brad?" The latter question was followed by additional instructions that read, "Please enter a percentage between 0% and 100%. You may use decimal numbers like 0.1% (for one tenth of 1%) or 99.99% if you wish. For example, type 50% if you mean 'fifty percent.' Type 0.5% if you mean 'one half of one percent.'" This question was exceptionally difficult in the multiple-play conditions[3] and we did not expect participants to answer correctly; our intent was merely to encourage participants to really think about playing the gambles for the specified number of plays.

In Study 4, there were two long-run-prompt conditions, which we label the *expected-totals* and *distributional-info* conditions. In the expected-totals condition, participants in both the 1-

[3] The exact solution requires computing the binomial probability distribution for each option, combining the results to get the joint probability distribution for all possible combinations of outcomes, and summing the joint probabilities over those cases in which the aggregate payoff for Option A exceeds that for Option B. An additional complication is that the aggregate payoffs can sometimes be equal. For Problem 8, the chances that the higher-EV option would pay more than, the same as, or less than the lower-EV certain option were 80%, 0%, and 20% for 1 play; 87.9%, 8.8%, and 3.3% for 10 plays; and 99.9996%, 0.0002%, and 0.0001% for 100 plays. For Problem 10, the chance that the higher-EV option would pay more than the lower-EV option was 45% for 1 play, 66.3% for 10 plays, and 92.8% for 100 plays.

play and 100-play conditions were asked, "If you played option A ONE HUNDRED times, how much would you expect to win in total? Please enter a dollar amount like $19, $500, or $1136." They were also asked the same question about Option B. In the distributional-info condition, participants in both the 1-play and 100-play conditions were told the mean and 90% confidence intervals for aggregate winnings resulting from 100 plays of each option. In Problem 10, for example, they were told, "If you played Option A 100 times, you could expect to win about $540 total. There is a 90% chance that you would win between $444 and $636," and also, "If you played Option B 100 times, you could expect to win about $450 total. There is a 90% chance that you would win between $425 and $475." In the cents version, dollars were replaced with cents but the numbers remained the same. In Study 5, the distributional-info condition was the only long-run-prompt condition. Note that in the expected-totals and distributional-info conditions (but not in the more-on-average condition), the same long-run prompts were given to participants in the single- and multiple-play conditions, though the prompts might be more relevant in multiple play. Also note that in the more-on-average and expected-totals conditions, participants had to figure out which option was likely to be better in the long run; in the distributional-info condition, we basically told them which option was better.

Because three studies seemed too few to warrant meta-analyses of the possible effects of the long-term prompts, we instead analyzed the combined data for all conditions of Studies 3–5 (n = 900). More specifically, we added a condition variable (standard = –1/2, long-term-prompt = +1/2) and all two- and three-way interactions to our primary logistic regression models for predicting choice of the higher-EV option in problems related to the certainty and possibility effects, controlling for study. These analyses treated the three long-run-prompt conditions as if they were the same, but results were relatively similar in separate analyses for the different studies and long-run-prompt conditions.

In the combined analyses, the main effect of condition was positive and significant for the certainty effect, $b = 0.58$, CI [0.33, 0.82], $OR = 1.78$, $\chi^2(1) = 21.99$, $p < .001$, and for the possibility effect, $b = 0.36$, CI [0.13, 0.60], $OR = 1.44$, $\chi^2(1) = 9.29$, $p = .002$, indicating that a long-run perspective favored the choice of the higher-EV options. The Condition × Problem interaction was negative and nearly significant for the certainty effect, $b = –0.39$, CI [–0.85, 0.06] , $OR = 0.67$, $\chi^2(1) = 2.96$, $p = .085$, and negative and significant for the possibility effect, $b = –0.85$, CI [–1.27, –0.43] , $OR = 0.43$, $\chi^2(1) = 16.44$, $p < .001$, indicating that the two effects were generally smaller in the long-run-prompt conditions. Although it might be expected that the effect of long-run prompts would be enhanced in multiple play, where the long-run view is generally considered more relevant, the Condition × Plays interaction was not significant for the certainty effect or the possibility effect, both *p*s ≥ .49. Finally, The Condition × Problem × Plays interaction was also not significant for either effect, both *p*s ≥ .29.

Controlling for study, both the certainty effect and the possibility effect remained significant in multiple-play decisions in the long-run-prompt conditions, both *p*s < .001. For the most part, these results also held for the different long-run-prompt conditions in each of the studies, with the certainty and possibility effects remaining significant or nearly significant in seven out of eight tests. In the more-on-average condition of Study 3, both effects remained significant in multiple play, both McNemar exact *p*s < .001. In the expected-totals condition of Study 4, the certainty effect remained nearly significant, $p = .093$, and the possibility effect remained significant, $p = .004$. In the distributional-info condition of Study 4, the certainty effect remained significant, $p < .001$, and the possibility effect remained nearly significant, $p = .087$. In the distributional-info condition of Study 5, the certainty effect remained significant, $p < .001$, but the possibility effect did not, $p = .39$. See Tables S.6–S.12 for complete counts and percentages for all conditions in Studies 3–5.

Figure S.2 shows the percentages of participants with each of the possible choice patterns, collapsing across the three long-run-prompt conditions of the three studies ($n = 500$). In combined analyses that controlled for study, the percentage of participants exhibiting the certainty pattern in Problems 2 and 8 dropped significantly, from 52.7% in single play to 43.5% in multiple play, $b = 0.40$, CI [0.04, 0.77], $OR = 1.50$, $\chi^2(1) = 4.74$, $p = .029$.[4] The increase from 5.0% to 6.7% in the reverse certainty pattern was not significant, overall $b = 0.26$, CI [–0.53, 1.05], $OR = 1.30$, $\chi^2(1) = 0.43$, $p = .51$. In Problems 4 and 10, the percentage exhibiting the possibility pattern dropped significantly, from 48.8% to 29.1%, overall $b = 0.91$, CI [0.52, 1.29], $OR = 2.39$, $\chi^2(1) = 22.02$, $p < .001$. The increase from 5.0% to 7.4% in the reverse possibility pattern was not significant, overall $b = 0.42$, CI [–0.35, 1.20], $OR = 1.53$, $\chi^2(1) = 1.19$, $p = .27$. For consistency with the main text (and despite the small number of long-run-prompt conditions), we also analyzed these data using random-effects meta-analyses.[5] The results were similar to those above, except that the drop in the certainty pattern was not significant, overall $b = 0.40$, CI [–0.27, 1.06], $OR = 1.49$, $t(3) = 1.89$, $p = .16$. The drop for the possibility pattern remained significant, but with a much higher p-value, overall $b = 0.87$, CI [0.08, 1.67], $OR = 2.39$, $t(3) = 3.49$, $p = .040$. Overall, the differences between the single- and multiple-play conditions were relatively similar to those in our standard conditions (compare Figure S.2 to Figure 6 in the main text).

[4] For consistency with the main text, these reductions are written as positive effects.

[5] In these individual-participant-data meta-analyses (Cooper & Patall, 2009), we treated the expected-totals and informational-info conditions of Study 4 as separate studies.

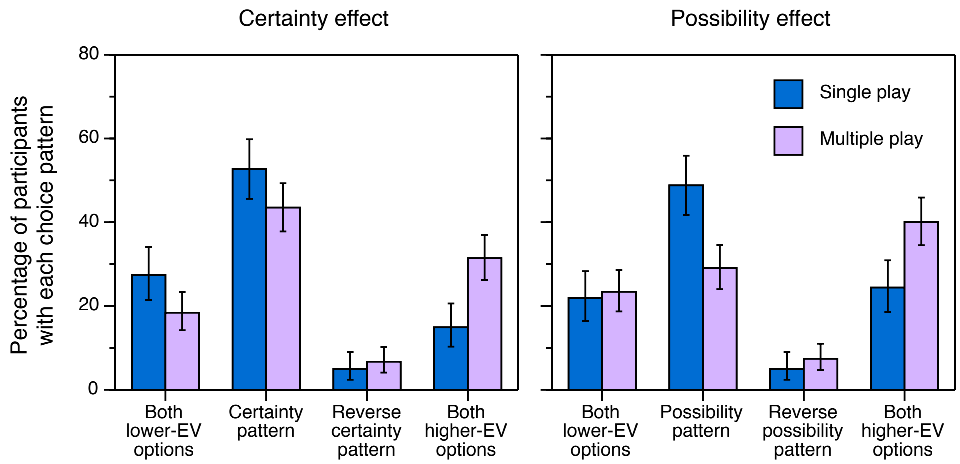

Figure S.2. Percentages of participants with each of the possible choice patterns in problems related to the certainty and possibility effects in the long-run-prompt conditions of Studies 3–5. Error bars indicate 95% CIs, but these ignore between-study variability.

In summary, the effect of multiple plays on the magnitudes of certainty and possibility effects was not consistently affected by manipulations designed to foster a long-run perspective.

Individual Differences Related to the Long-Run-Prompt Conditions in Studies 3 and 4

For the more-on-average and expected-totals conditions (but not for the distributional-info condition), we could also ask whether the effect of multiple plays was larger for participants with good insight regarding long-run payoffs. For the more-on-average condition in Study 3, we defined high-insight participants as those who chose the higher-EV option in response to the question, "On average, would you make more money with Option A or Option B?" in both of the problems related to the certainty effect (Problems 2 and 8; 30% of participants) or both of the problems related to the possibility effect (Problems 4 and 10; 39% of participants) (some participants were high-insight for one effect but not the other). Similarly, for the expected-totals condition in Study 4, we defined high-insight participants as those who ordered options A and B properly in response to the question, "If you played option A [B] ONE HUNDRED times, how much would you expect to win in total?" in both of the problems related to the certainty effect

(54% of participants) or both of the problems related to the possibility effect (52% of participants).

We added insight (low = –1/2, high = +1/2) and its interactions to our primary logistic regression models for predicting choice of the higher-EV option in problems related to the certainty and possibility effects in the relevant long-run-prompt conditions. For the certainty effect in Study 3, the model could not be fit, apparently because all 43 high-insight participants chose the higher-EV option for Problem 2 in multiple play (however, 12 of these same participants chose the lower-EV certain option in Problem 8 in multiple play). For the certainty-effect problems in Study 4, high-insight participants were more likely to choose the higher-EV option, $b = 1.41$, CI [0.71, 2.12], $OR = 4.11$, $\chi^2(1) = 13.97$, $p < .001$, but insight did not interact significantly with problem or plays, all ps ≥ .29 for the two- and three-way interactions.

For the possibility-effect problems in Studies 3 and 4, high-insight participants were more likely than low-insight participants to choose the higher-EV option, $b = 1.95$, CI [1.29, 2.61], $OR = 7.04$, $\chi^2(1) = 26.32$, $p < .001$ and $b = 1.20$, CI [0.48, 1.93], $OR = 3.33$, $\chi^2(1) = 10.67$, $p = .001$ in the two studies, respectively. This was more true in multiple play than in single play, as indicated by a significant Insight × Plays interaction, $b = 1.78$, CI [0.46, 3.09], $OR = 5.91$, $\chi^2(1) = 5.60$, $p = .018$ in Study 3 and $b = 1.59$, CI [0.15, 3.04], $OR = 4.92$, $\chi^2(1) = 4.72$, $p = .030$ in Study 4. However, high-insight participants were not significantly less likely to show possibility effects, as evidenced by the nonsignificant Insight × Problem interaction in each study, ps = .14 and .24. Finally, the three-way Insight × Problem × Plays interaction was not significant in either study, ps = .15 and .26, and was in opposite directions in the two studies.

To recap, those participants who could correctly order gambles in terms of their average or long-run payoffs were more likely to choose the higher-EV option, as one would expect. At least for the possibility-effect problems, this was more true in multiple play, when long-run payoffs are presumably more relevant. However, there was no indication that these high-insight participants showed significantly smaller certainty and possibility effects or that the effect of multiple plays on certainty and possibility effects was reliably different for these participants.

Individual Differences in Numeracy in Studies 4–6

Previous research indicates that individuals with greater numeracy are more likely to choose higher-EV options (Cokely & Kelley, 2009; Ghazal, Cokely, & Garcia-Retamero, 2014; Pachur & Galesic, 2013; but see Mather et al., 2012), in part because they consider more information and deliberate more than less numerate individuals do. More numerate individuals also appear to have more linear value functions (Milroth & Juslin, 2015; Schley & Peters, 2014), though they may (Milroth & Juslin, 2015) or may not (Petrova, van der Pligt, & Garcia-Retamero, 2014; Schley & Peters, 2014) have more linear weighting functions for probabilities. These results suggest that higher numeracy might be associated with smaller common-ratio effects, with more numerate individuals being more likely to choose the relatively unpopular higher-EV options in our Problems 8 and 10. However, because current numeracy scales do not assess individuals' ability to aggregate information (Pachur & Galesic, 2013), it is not obvious whether numeracy should play a greater role in single or multiple play. To our knowledge, no study has assessed the effect of numeracy on common-ratio effects or the effect of multiple plays.

Participants in Studies 4–6 completed Weller et al.'s (2013) eight-item numeracy scale (Cronbach's α = .63, .53, and .55 in the three studies, respectively; Weller et al. reported .71 in two studies).[6] Because three studies seemed too few to warrant meta-analyses of the possible effects of numeracy, we instead analyzed the combined data for the standard conditions of these studies ($n = 336$). More specifically, we added the continuous numeracy measure and its interactions to our primary logistic regression models for predicting choice of the higher-EV option in problems related to the certainty and possibility effects in the standard conditions, controlling for study.

For the certainty effect, there were no significant effects of numeracy or its interactions, all ps ≥ .14. In particular, the three-way interaction between numeracy, problem, and plays was small and not significant, $p = .79$. For the possibility effect, there was a significant positive effect of numeracy, $b = 0.28$, CI [0.14, 0.42], $OR = 1.32$, $\chi^2(1) = 13.25$, $p < .001$, and a significant positive interaction between numeracy and problem, such that more numerate participants showed *larger* possibility effects than less numerate participants, $b = 0.38$, CI [0.11, 0.65], $OR = 1.46$, $\chi^2(1) = 6.41$, $p = .011$. The interaction between numeracy and plays was positive, indicating that the effect of multiple plays was more positive for more numerate participants, but it was not quite significant, $b = 0.24$, CI [–0.04, 0.52], $OR = 1.27$, $\chi^2(1) = 2.65$, $p = .10$. The three-way interaction between numeracy, problem, and plays was not significant, $b = –0.43$, CI [–0.98, 0.11], $OR = 0.65$, $\chi^2(1) = 2.32$, $p = .13$. Despite the nonsignificant three-way, the interesting two-way interaction between numeracy and problem (larger possibility effects for more numerate participants) was significant in single play, $b = 0.60$, CI [0.17, 1.03], $OR = 1.82$,

[6] In Study 6, the mammogram question was updated so that the correct answer was 9 out of 19 rather than 9 out of 18, so that guesses of one half or 50% would not be correct (see http://faculty.psy.ohio-state.edu/peters/lab/Rasch-basedNumeracyScale.html). Also in Study 6, $\alpha = .60$ when participants with incorrect or missing values on the manipulation-check question were included. Subsequent analyses excluded those participants.

$\chi^2(1) = 7.87$, $p = .005$, but not in multiple play, $b = 0.16$, CI [–0.19, 0.50], $OR = 1.17$, $\chi^2(1) = 0.77$, $p = .38$.

Considering only those participants with above-average numeracy scores (five or higher on the eight-item scale), the certainty and possibility effects remained significant in multiple-play decisions in the standard conditions of these studies, again controlling for study, both *p*s < .001. Thus, although we observed one unanticipated result (more numerate participants showed larger possibility effects in single-play decisions), certainty and possibility effects in multiple-play decisions were largely unrelated to participants' numeracy.

Meta-Regressions Using the Within- Versus Between-Participants Distinction as a Predictor of Effect Size

To investigate the possible effects of study design more formally, we reran the certainty-effect meta-analyses in Figure 4 of the main text using the within- versus between-participants distinction (between = –1/2, within = +1/2) as a predictor of effect size. We treated Studies 1 and 7 as between-participants studies to attain a more even split and to make the predictor variable less like a distinction between our studies and previous studies. The effect of study design on the reduction of the certainty effect in multiple play was nearly significantly, $b = 0.69$ (indicating a *greater* reduction for within-participants studies), CI [–0.12, 1.50], $OR = 1.80$, $t(9) = 1.93$, $p = .085$, but its effect on the residual size of the certainty effect in multiple play was not, $b = 0.18$, CI [–1.14, 1.50], $OR = 1.20$, $t(9) = 0.31$, $p = .77$.

When we reran the possibility-effect meta-analyses in Figure 5 of the main text using study design as a predictor (and again treating our Studies 1 and 7 as between-participants studies), study design was not significantly related to the reduction in the possibility effect in multiple play, $b = –0.79$ (indicating a smaller reduction for within-participants studies), CI [–2.03, 0.44], $OR = 0.45$, $t(6) = –1.57$, $p = .17$, or to the residual size of the possibility effect in multiple play, *b*

= 0.87, CI [–0.64, 2.38], OR = 2.39, $t(6) = 1.41$, $p = .21$. However, given these relatively large regression coefficients and the small number of studies, one should be especially cautious in interpreting these results. A firm conclusion is precluded by the diversity of results for the possibility effect in between-participants studies.

Standard or Reverse Common-Ratio Effects in Single-Play Decisions?

Though less important than our primary findings, another more specific result may also have implications for the study of reverse common-ratio effects. In particular, we found a strong and reliable certainty effect for our Problems 2 and 8. This pair of problems (see Table S.2) was remarkably similar to a pair used by Blavatskyy (2010), in which the scaled-up problem was a choice between $60 for sure and a 75% chance (instead of our 80% chance) of $100 and the scaled-down problem was created by dividing the winning probabilities by three (rather than four). Contrary to our results, Blavatskyy reported a strong reverse certainty effect for this pair of problems, attributing this result to the sure thing being "far below" the EV of the risky option in the scaled-up problem. This explanation seems inadequate, however, as the EV gap in our Problem 8 is even greater than that in Blavatskyy's problem. We suspect that Blavatskyy's reverse certainty effect in this instance was due, at least in part, to the manner in which the gambles were presented (visually, using different numbers of playing cards)—a possibility that the author acknowledged for a different unexpected reversal. We note here that Blavatskyy's method of presentation was somewhat similar to that used in the *records* condition of Hau, Pleslac, and Hertwig's (2010) first study. The results for that condition closely followed those for a yoked *experience* condition in which participants actively sampled from the decks of cards before choosing. This connection may be important because reverse certainty effects are often observed in decisions from experience (Barron & Erev, 2003; Hertwig, Barron, Weber, & Erev, 2004).

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Supplementary Tables

Table of Problems Used in Previous Studies of Certainty and Possibility Effects in Single and Multiple Play

Table S.1

Problems, Gambles, and Expected Values (EVs) in the Single-Play Conditions of Previous Studies.

		Higher-EV option			Lower-EV option		
Study	Problem	Probability of winning	Amount to win	EV	Probability of winning	Amount to win	EV
Keren & Wagenaar (1987, Study 1 & follow-up)	Certainty	.50	250 guilders	125 guilders	.99	100 guilders	99 guilders
	No certainty	.10	250 guilders	25 guilders	.25	100 guilders	20 guilders
Keren & Wagenaar (1987, Study 2)	Probable	.45	12,000 guilders	5400 guilders	.90	5000 guilders	4500 guilders
	Possible	.01	12,000 guilders	120 guilders	.02	5000 guilders	100 guilders
Keren (1991)	Certainty (Set 1)	.80	25 guilders	20 guilders	1.00	15 guilders	15 guilders
	No certainty (Set 1)	.20	25 guilders	5 guilders	.25	15 guilders	3.75 guilders
	Certainty (Set 2)	.70	60 guilders	42 guilders	1.00	30 guilders	30 guilders
	No certainty (Set 2)	.07	60 guilders	4.20 guilders	.10	30 guilders	3 guilders
Barron & Erev (2003, Study 5)	Certainty	.80	4 agorat	3.20 agorat	1.00	3 agorat	3 agorat
	No certainty	.20	4 agorat	0.80 agorat	.25	3 agorat	0.75 agorat
Li (2003)	Certainty	.88	100 yuan	88 yuan	1.00	77 yuan	77 yuan

Note. All options with probabilities of winning less than 1.00 included a complementary outcome of no money. Problem labels and EVs were not shown to participants. Keren and Wagenaar (1987) displayed their gambles using pie charts, whereas other authors described their gambles using text, as we did in our Studies 1–7. At the time of the previous studies, 1 guilder = about \$0.50, 1 agora = about \$0.0025, and 1 yuan = about \$0.12. In Barron and Erev (2003, Study 5), the tiny payoffs were presumably intended to match those in an earlier study where the choice was repeated 400 times. In Keren and Wagenaar (1987, Study 1) and Li (2003), the values were also reflected to create gambles involving losses, which reversed the EVs of the two options.

Table of All Problems Used in the Single-Play Condition of Our Studies 1–7

Table S.2

Problems, Gambles, and Expected Values (EVs) in the Single-Play Condition of Studies 1–7

	Higher-EV option			Lower-EV option		
Problem and label	Probability of winning	Amount to win	EV	Probability of winning	Amount to win	EV
			Studies 1–3 and 7			
1	**.30**	**$110**	**$33**	.40	$70	$28
2 (No certainty)	.20	$100	$20	**.25**	**$60**	**$15**
3	.30	$80	$24	**.15**	**$140**	**$21**
4 (Possible)	.01	$120	$1.20	**.02**	**$50**	**$1**
5	**.50**	**$60**	**$30**	.25	$90	$22.50
6 (Attention check)	**.40**	**$80**	**$32**	.30	$70	$21
7	.85	$120	$102	**.65**	**$150**	**$97.50**
8 (Certainty)	**.80**	**$100**	**$80**	1.00	$60	$60
9	.04	$70	$2.80	**.02**	**$130**	**$2.60**
10 (Probable)	**.45**	**$120**	**$54**	.90	$50	$45
11	.25	$70	$17.50	**.10**	**$150**	**$15**
			Studies 4–6			
1	**.30**	**$22**	**$6.60**	.40	$14	$5.60
2 (No certainty)	.20	$10	$2	**.25**	**$6**	**$1.50**
3	.30	$24	$7.20	**.15**	**$42**	**$6.30**
4 (Possible)	.01	$12	$0.12	**.02**	**$5**	**$0.10**
5	**.50**	**$6**	**$3**	.25	$9	$2.25
6 (Attention check)	**.40**	**$8**	**$3.20**	.30	$7	$2.10
7	.85	$12	$10.20	**.65**	**$15**	**$9.75**
8 (Certainty)	**.80**	**$10**	**$8**	1.00	$6	$6
9	.08	$35	$2.80	**.04**	**$65**	**$2.60**
10 (Probable)	**.45**	**$12**	**$5.40**	.90	$5	$4.50
11	.25	$35	$8.75	**.10**	**$75**	**$7.50**

Note. All options except the certain option in Problem 8 included a complementary outcome of "no money." Labels and EVs were not shown to participants. Odd-numbered problems were fillers. The option shown first in each problem (Option A) appears in bold (option order was randomized in Study 7). Studies 2 and 7 used only Problems 2, 4, 8, and 10.

Tables of Frequencies and Percentages for All Choice Patterns in All Conditions of Barron and Erev's (2003) Study 5 and our Studies 1–7

Barron and Erev's (2003) Study 5

Table S.3

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect in Barron and Erev's (2003) Study 5

	Certainty (Their Problem 4)		
No certainty (Their Problem 5)	Lower-EV option (1.00, 3)	Higher-EV option (.80, 4)	Total
Lower-EV option (.25, 3)	11 (12%) **20 (29%)**	6 (7%) **17 (24%)**	17 (19%) **37 (53%)**
Higher-EV option (.20, 4)	30 (33%) **7 (10%)**	44 (48%) **26 (37%)**	74 (81%) **33 (47%)**
Total	41 (45%) **27 (39%)**	50 (55%) **43 (61%)**	91 **70**

Note. Plain text = 1 play. Bold = 100 play. Amounts are in agorot (1 agora was about $0.0025 at the time). Data courtesy of Greg Barron (personal communication, 2003).

Study 1

Table S.4

Numbers (and Percentages) Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in Study 1

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $60)	Higher-EV option (.80, $100)	Total
Lower-EV option (.25, $60)	12 (15%) *7 (9%)* **5 (12%)**	4 (5%) *10 (13%)* **6 (15%)**	16 (20%) *17 (22%)* **11 (28%)**
Higher-EV option (.20, $100)	53 (65%) *49 (62%)* **21 (52%)**	13 (16%) *13 (16%)* **8 (20%)**	66 (80%) *62 (78%)* **29 (72%)**
Total	65 (79%) *56 (71%)* **26 (65%)**	17 (21%) *23 (29%)* **14 (35%)**	82 *79* **40**
	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $50)	Higher-EV option (.45, $120)	Total
Lower-EV option (.02, $50)	13 (16%) *6 (8%)* **3 (8%)**	5 (6%) *9 (11%)* **6 (15%)**	18 (22%) *15 (19%)* **9 (22%)**
Higher-EV option (.01, $120)	57 (70%) *49 (62%)* **23 (58%)**	7 (9%) *15 (19%)* **8 (20%)**	64 (78%) *64 (81%)* **31 (78%)**
Total	70 (85%) *55 (70%)* **26 (65%)**	12 (15%) *24 (30%)* **14 (35%)**	82 *79* **40**

Note. Plain text = 1 play. Italics = 10 plays. Bold = 100 plays. Some percentages may not sum properly because of rounding. Study 1 had two parts: Study 1a had 1-play and 10-play conditions, whereas Study 1b had 1-play, 10-play, and 100-play conditions, which is why the number of participants was lower in the 100-play condition. Otherwise, the studies were identical.

Study 2

Table S.5

Numbers (and Percentages) of Participants Choosing the Higher-EV Option in Problems Related to the Certainty and Possibility Effects in Study 2

	Certainty effect		Possibility effect	
	No certainty (Problem 2)	Certainty (Problem 8)	Possible (Problem 4)	Probable (Problem 10)
Higher-EV option	27 (68%)	7 (18%)	28 (70%)	18 (45%)
	23 (55%)	*13 (31%)*	*20 (49%)*	*19 (46%)*
	30 (70%)	**15 (38%)**	**26 (65%)**	**19 (46%)**
Total	40	40	40	40
	42	*42*	*41*	*41*
	43	**40**	**40**	**41**

Note. Plain text = 1 play. Italics = 10 plays. Bold = 100 plays.

Study 3

Table S.6

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in the Standard Condition of Study 3

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $60)	Higher-EV option (.80, $100)	Total
Lower-EV option (.25, $60)	17 (35%) *8 (13%)* **18 (32%)**	1 (2%) *5 (8%)* **4 (7%)**	18 (38%) *13 (21%)* **22 (39%)**
Higher-EV option (.20, $100)	29 (60%) *43 (70%)* **26 (46%)**	1 (2%) *5 (8%)* **8 (14%)**	30 (62%) *48 (79%)* **34 (61%)**
Total	46 (96%) *51 (84%)* **44 (79%)**	2 (4%) *10 (16%)* **12 (21%)**	48 *61* **56**

	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $50)	Higher-EV option (.45, $120)	Total
Lower-EV option (.02, $50)	9 (19%) *9 (15%)* **7 (12%)**	1 (2%) *6 (10%)* **2 (4%)**	10 (21%) *15 (25%)* **9 (16%)**
Higher-EV option (.01, $120)	33 (69%) *35 (57%)* **34 (61%)**	5 (10%) *11 (18%)* **13 (23%)**	38 (79%) *46 (75%)* **47 (84%)**
Total	42 (88%) *44 (72%)* **41 (73%)**	6 (12%) *17 (28%)* **15 (27%)**	48 *61* **56**

Note. Plain text = 1 play. Italics = 10 plays. Bold = 100 plays. Some percentages may not sum properly because of rounding.

Table S.7

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in the More-on-Average Condition of Study 3

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $60)	Higher-EV option (.80, $100)	Total
Lower-EV option (.25, $60)	18 (29%) *10 (18%)* **8 (13%)**	5 (8%) *3 (5%)* **2 (3%)**	23 (37%) *13 (23%)* **10 (17%)**
Higher-EV option (.20, $100)	35 (56%) *28 (50%)* **32 (53%)**	4 (6%) *15 (27%)* **18 (30%)**	39 (63%) *43 (77%)* **50 (83%)**
Total	53 (85%) *38 (68%)* **40 (67%)**	9 (15%) *18 (32%)* **20 (33%)**	62 *56* **60**
	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $50)	Higher-EV option (.45, $120)	Total
Lower-EV option (.02, $50)	10 (16%) *10 (18%)* **11 (18%)**	3 (5%) *4 (7%)* **2 (3%)**	13 (21%) *14 (25%)* **13 (22%)**
Higher-EV option (.01, $120)	37 (60%) *23 (41%)* **21 (35%)**	12 (19%) *19 (34%)* **26 (43%)**	49 (79%) *42 (75%)* **47 (78%)**
Total	47 (76%) *33 (59%)* **32 (53%)**	15 (24%) *23 (41%)* **28 (47%)**	62 *56* **60**

Note. Plain text = 1 play. Italics = 10 plays. Bold = 100 plays. Some percentages may not sum properly because of rounding.

Study 4

Table S.8

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in the Standard Condition of Study 4

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $6)	Higher-EV option (.80, $10)	Total
Lower-EV option (.25, $6)	16 (31%) **15 (31%)** 15 (33%)	0 (0%) **6 (12%)** 2 (4%)	16 (31%) **21 (44%)** 17 (38%)
Higher-EV option (.20, $10)	30 (59%) **19 (40%)** 22 (49%)	5 (10%) **8 (17%)** 6 (13%)	35 (69%) **27 (56%)** 28 (62%)
Total	46 (90%) **34 (71%)** 37 (82%)	5 (10%) **14 (29%)** 8 (18%)	51 **48** 45

	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $5)	Higher-EV option (.45, $12)	Total
Lower-EV option (.02, $5)	11 (22%) **18 (38%)** 16 (36%)	4 (8%) **3 (6%)** 2 (4%)	15 (29%) **21 (44%)** 18 (40%)
Higher-EV option (.01, $12)	27 (53%) **19 (40%)** 16 (36%)	9 (18%) **8 (17%)** 11 (24%)	36 (71%) **27 (56%)** 27 (60%)
Total	38 (75%) **37 (77%)** 32 (71%)	13 (25%) **11 (23%)** 13 (29%)	51 **48** 45

Note. Plain text = 1 play. Bold = 100 plays. Underlined = 100 plays with cents. Some percentages may not sum properly because of rounding.

Table S.9

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in the Expected-Totals Condition of Study 4

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $6)	Higher-EV option (.80, $10)	Total
Lower-EV option (.25, $6)	15 (34%) **8 (18%)**	1 (2%) **7 (16%)**	16 (36%) **15 (33%)**
Higher-EV option (.20, $10)	20 (45%) **16 (36%)**	8 (18%) **14 (31%)**	28 (64%) **30 (67%)**
Total	35 (80%) **24 (53%)**	9 (20%) **21 (47%)**	44 **45**

	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $5)	Higher-EV option (.45, $12)	Total
Lower-EV option (.02, $5)	12 (28%) **12 (27%)**	4 (9%) **3 (7%)**	16 (36%) **15 (33%)**
Higher-EV option (.01, $12)	18 (41%) **16 (36%)**	10 (23%) **14 (31%)**	28 (64%) **30 (67%)**
Total	30 (68%) **28 (62%)**	14 (32%) **17 (38%)**	44 **45**

Note. Plain text = 1 play. Bold = 100 plays. Some percentages may not sum properly because of rounding.

Table S.10

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in the Distributional-Info Condition of Study 4

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $6)	Higher-EV option (.80, $10)	Total
Lower-EV option (.25, $6)	8 (17%) **12 (27%)** 11 (23%)	3 (6%) **2 (4%)** 5 (10%)	11 (23%) **14 (31%)** 16 (33%)
Higher-EV option (.20, $10)	28 (60%) **15 (33%)** 19 (40%)	8 (17%) **16 (36%)** 13 (27%)	36 (77%) **31 (69%)** 32 (67%)
Total	36 (77%) **27 (60%)** 30 (62%)	11 (23%) **18 (40%)** 18 (38%)	47 **45** 48
	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $5)	Higher-EV option (.45, $12)	Total
Lower-EV option (.02, $5)	14 (30%) **10 (22%)** 17 (35%)	2 (4%) **4 (9%)** 5 (10%)	16 (34%) **14 (31%)** 22 (46%)
Higher-EV option (.01, $12)	17 (36%) **10 (22%)** 9 (19%)	14 (30%) **21 (47%)** 17 (35%)	31 (66%) **31 (69%)** 26 (54%)
Total	31 (66%) **20 (44%)** 26 (54%)	16 (34%) **25 (56%)** 22 (46%)	47 **45** 48

Note. Plain text = 1 play. Bold = 100 plays. Underlined = 100 plays with cents. Some percentages may not sum properly because of rounding.

Study 5

Table S.11

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in the Standard Condition of Study 5

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $6)	Higher-EV option (.80, $10)	Total
Lower-EV option (.25, $6)	16 (36%) <u>16 (35%)</u>	3 (7%) <u>5 (11%)</u>	19 (42%) <u>21 (46%)</u>
Higher-EV option (.20, $10)	22 (49%) <u>14 (30%)</u>	4 (9%) <u>11 (24%)</u>	26 (58%) <u>25 (54%)</u>
Total	38 (84%) <u>30 (65%)</u>	7 (16%) <u>16 (35%)</u>	45 <u>46</u>
	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $5)	Higher-EV option (.45, $12)	Total
Lower-EV option (.02, $5)	8 (18%) <u>13 (28%)</u>	2 (4%) <u>0 (0%)</u>	10 (22%) <u>13 (28%)</u>
Higher-EV option (.01, $12)	30 (67%) <u>19 (41%)</u>	5 (11%) <u>14 (30%)</u>	35 (78%) <u>33 (72%)</u>
Total	38 (84%) <u>32 (70%)</u>	7 (16%) <u>14 (30%)</u>	45 <u>46</u>

Note. Plain text = 1 play. Underlined = 100 plays with cents. Some percentages may not sum properly because of rounding.

Table S.12

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in the Distributional-Info Condition of Study 5

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $6)	Higher-EV option (.80, $10)	Total
Lower-EV option (.25, $6)	14 (29%) 6 (13%)	1 (2%) 1 (2%)	15 (31%) 7 (16%)
Higher-EV option (.20, $10)	23 (48%) 20 (44%)	10 (21%) 18 (40%)	33 (69%) 38 (84%)
Total	37 (77%) 26 (58%)	11 (23%) 19 (42%)	48 45
	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $5)	Higher-EV option (.45, $12)	Total
Lower-EV option (.02, $5)	8 (17%) 10 (22%)	1 (2%) 4 (9%)	9 (19%) 14 (31%)
Higher-EV option (.01, $12)	26 (54%) 8 (18%)	13 (27%) 23 (51%)	39 (81%) 31 (69%)
Total	34 (71%) 18 (40%)	14 (29%) 27 (60%)	48 45

Note. Plain text = 1 play. Underlined = 100 plays with cents. Some percentages may not sum properly because of rounding.

Study 6

Table S.13

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in Study 6

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $6)	Higher-EV option (.80, $10)	Total
Lower-EV option (.25, $6)	16 (34%) **9 (17%)**	4 (9%) **2 (4%)**	20 (43%) **11 (20%)**
Higher-EV option (.20, $10)	22 (47%) **23 (43%)**	5 (11%) **20 (37%)**	27 (57%) **43 (80%)**
Total	38 (81%) **32 (59%)**	9 (19%) **22 (41%)**	47 **54**
	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $5)	Higher-EV option (.45, $12)	Total
Lower-EV option (.02, $5)	7 (15%) **11 (20%)**	4 (9%) **4 (7%)**	11 (23%) **15 (28%)**
Higher-EV option (.01, $12)	22 (47%) **22 (41%)**	14 (30%) **17 (31%)**	36 (77%) **39 (72%)**
Total	29 (62%) **33 (61%)**	18 (38%) **21 (39%)**	47 **54**

Note. Plain text = 1 play. Bold = 100 plays. Some percentages may not sum properly because of rounding. Data are for participants who answered the manipulation-check question correctly.

Study 7

Table S.14

Numbers (and Percentages) of Participants With Various Choice Patterns in Problems Related to the Certainty Effect (Top) and the Possibility Effect (Bottom) in Study 7

	Certainty (Problem 8)		
No certainty (Problem 2)	Lower-EV option (1.00, $60)	Higher-EV option (.80, $100)	Total
Lower-EV option (.25, $60)	33 (20%) **36 (22%)**	8 (5%) **10 (6%)**	41 (25%) **46 (28%)**
Higher-EV option (.20, $100)	89 (55%) **68 (42%)**	32 (20%) **49 (30%)**	121 (75%) **117 (72%)**
Total	122 (75%) **104 (64%)**	40 (25%) **59 (36%)**	162 **163**

	Probable (Problem 10)		
Possible (Problem 4)	Lower-EV option (.90, $50)	Higher-EV option (.45, $120)	Total
Lower-EV option (.02, $50)	47 (24%) **56 (32%)**	3 (2%) **11 (6%)**	50 (25%) **67 (38%)**
Higher-EV option (.01, $120)	118 (60%) **68 (38%)**	29 (15%) **42 (24%)**	147 (75%) **110 (62%)**
Total	165 (84%) **124 (70%)**	32 (16%) **53 (30%)**	197 **177**

Note. Plain text = 1 play. Bold = 100 plays. Some percentages may not sum properly because of rounding. Data are for participants who answered the manipulation-check question correctly.