High-value decisions are fast and accurate, inconsistent with diminishing value sensitivity

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It is a widely held belief that people's choices are less sensitive to changes in value as value increases. For example, the subjective difference between $11 and $12 is believed to be smaller than between $1 and $2. This idea is consistent with applications of the Weber-Fechner Law and divisive normalization to value-based choice and with psychological interpretations of diminishing marginal utility. According to random utility theory in economics, smaller subjective differences predict less accurate choices. Meanwhile, in the context of sequential sampling models in psychology, smaller subjective differences also predict longer response times. Based on these models, we would predict decisions between high-value options to be slower and less accurate. In contrast, some have argued on normative grounds that choices between high-value options should be made with less caution, leading to faster and less accurate choices. Here, we model the dynamics of the choice process across three different choice domains, accounting for both discriminability and response caution. Contrary to predictions, we mostly observe faster and more accurate decisions (i.e., higher drift rates) between high-value options. We also observe that when participants are alerted about incoming high-value decisions, they exert more caution and not less. We rule out several explanations for these results, using tasks with both subjective and objective values. These results cast doubt on the notion that increasing value reduces discriminability.

drift diffusion model | decision-making | neuroeconomics | overall value | response time

Are decision-makers sensitive to the average value of their options? For example, when shopping for a car, does the choice process differ at a bargain lot compared to a luxury dealership? Is it easier to choose between two cars valued at $5,000 or $50,000?

To answer this question, we must first define what we mean by "easier." There are two basic features of easy decisions: they are consistent and fast. For instance, it is well established that choices are inconsistent and slow when the choice options are similar in value to each other, while they are consistent and fast when there is a large difference in the options' values (1–5). The effect of value difference on the stochasticity of choice is predicted by many popular models, dating back at least to Luce (6), and the effect of value difference on response time (RT) is predicted by sequential sampling models (7–12). In fact, the effect of value difference on both choice frequencies and RT has been documented in many laboratory experiments (10, 13).

In comparison, there has been much less research into the effects of overall value (OV), holding value difference constant. Among conventional stochastic choice models, a common assumption is that OV should be irrelevant. One popular economic model is the additive random utility model (2), which implies the probability of choosing an option i over another alternative j should be an increasing function of $\mu_i - \mu_j$, where for any option i the utility assigned to it is μ_i (before the addition of the random error term). Therefore, a constant utility difference should imply the same choice frequencies regardless of whether μ_i and μ_j are two small quantities or two large quantities. The logit (softmax) choice function, commonly used to fit preference models to experimental data, similarly posits choice frequencies of the form

$$P[i \succ j] = \frac{e^{\lambda\mu_i}}{e^{\lambda\mu_i} + e^{\lambda\mu_j}} = \left(1 + e^{-\lambda(\mu_i - \mu_j)}\right)^{-1}$$

for some "inverse temperature" parameter $\lambda > 0$. This model again implies that only utility differences matter. Finally, choice frequencies and RT are often jointly modeled using sequential sampling models. The most popular of these models, the drift diffusion model (DDM), commonly assumes that the drift rate of the decision variable is proportional to the difference in value between the two options (9, 10). Under this assumption, the DDM predicts that both choice frequencies and mean RT should depend only on the value difference and not on OV.

The aforementioned models imply that OV is irrelevant only under the assumption that value representations (i.e., utilities) are linear, monotonic functions of the values measured by the experimenter. However, there are many theories of value representation that instead posit that utilities are nonlinear functions of the measured values, i.e., $\mu_i = \mu(V_i)$. In this case, choice frequencies and RT would depend on more than just the value difference $\Delta V = V_i - V_j$ measured by the experimenter.

Significance

What information about economic value is incorporated into decision-makers' choices? Across the decision sciences, several prominent models ignore average value, assuming that only value differences are incorporated into the decision-making process, while others assume diminishing sensitivity to value, suggesting that it should be more difficult to choose between high-value options. Other models suggest that high-value decisions should, if anything, be treated as less important (holding value difference constant). Across three experiments with very different types of choices (food, art, and learned stimuli), we find violations of these predictions. Contrary to expectations, the presence of high-value options makes decisions easier while also inducing more effort to get them right.

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What form should the function $\mu(V)$ take? A natural proposal would be to assume that $\mu(V)$ is increasing but strictly concave, so that the marginal utility $\mu'(V)$ decreases as V increases. The assumption of diminishing marginal utility is commonplace in economic modeling, dating back to Bernoulli (14). It is typically invoked to explain the imperfect substitutability between different goods in a bundle (15), imperfect substitutability of consumption over time (16), or risk aversion (17)—contexts that might seem orthogonal to stochastic choice or issues of discriminability. Nonetheless, one might conjecture that the same mechanisms that generate diminishing marginal utility in these other contexts should also determine the relationship between measured values and utilities in a random utility model of stochastic choice.

Similarly, Prospect Theory is predicated on the assumption that choices are made based on subjective values generated by nonlinear transformations of objective values (17). Notably, this value function is assumed to reflect diminishing marginal sensitivity to increasing values. Kahneman and Tversky use this value function to explain modal choices but do not propose any model of the stochasticity of observed choices or of RT. They motivate their incorporation of diminishing marginal sensitivity based on an analogy to the psychophysics of perceptual judgements, in which objective sensory magnitudes are often mapped onto an internal scale (18) with a nonlinear function that is typically expected to be concave (as with the logarithmic mapping postulated by the Weber-Fechner Law). The key evidence for such nonlinearity is the way in which the discriminability between two stimuli declines with increases in the absolute magnitudes of the two stimuli (holding the difference constant). Kahneman and Tversky also expected this to be true of comparisons involving economic values, and others have formalized this assumption within stochastic versions of Prospect Theory fit to experimental data (19).

Another way to motivate this type of nonlinear function is with the theory of divisive normalization in neural coding. An influential literature in neuroscience has determined that neural firing rates that represent sensory magnitudes are normalized in such a way that a given difference in objective magnitudes results in a smaller difference in the respective firing rates when the two objective magnitudes increase (20–23). Recent work in neuroeconomics has applied divisive normalization to stochastic, value-based choice under the assumption that there is a one-to-one relationship between the neural representation of value in firing rates and the choice behavior it generates (24–30). A theory of stochastic choice predicated on divisive normalization thus predicts that option discriminability will decrease as OV increases (see *SI Appendix* for details).

Despite the intuitive appeal of diminishing marginal sensitivity and the evidence for it in other sensory domains, there is little direct evidence that OV decreases discriminability once you control for value difference. The behavioral evidence on accuracy rates is controversial (31). Furthermore, the notion that utility differences decrease with OV is typically inferred from the presence of risk-averse behavior, which could arise for other reasons (32–35).

One possible reason for the mixed behavioral evidence is that increasing OV may also increase perceived importance, motivating decision-makers to approach high-value decisions more cautiously (36–40). The well-known speed–accuracy tradeoff (5, 9) implies that more caution could counteract losses in discriminability. On the other hand, there is abundant evidence that high-value decisions tend to be fast (10, 41–45). Even nonhuman primates will choose between juices (including identical ones) faster as the amount of juice increases (46). Based on these results, it appears unlikely that high-value decisions are made more cautiously, but we cannot be sure because both discriminability and response caution affect RT (47).

To properly determine how OV influences discriminability while accounting for response caution, we require analyses that consider both accuracy and RT. Using the DDM, we can account for response caution while simultaneously estimating the effect of OV on discriminability (48).

In this paper, we applied the DDM to behavior in three studies, each with the same structure but different types of decisions. Each experiment involved a series of binary choices, separated into blocks with three categories of OV (low, middle, and high). To study OV effects in naturalistic settings, studies 1 and 2 used snack foods and abstract art, respectively. Subjects first rated how much they liked various items, then later chose between them. These tasks are commonly used in the literature, but also come with a drawback: they rely on subjective ratings. Subjective ratings noisily represent subjects' true values (49), and ratings on different parts of the scale may be more or less noisy (50). To rule out these concerns, study 3 used a paradigm with learned values that were objective and identically distributed in each OV condition.

In each study, we first tested core predictions about discriminability varying with OV in a baseline condition. Specifically, we used the DDM to estimate discriminability (via drift rate) as a function of OV while accounting for response-caution differences (via boundary separation) between OV categories. We tested the hypothesis that discriminability would be reduced in higher OV contexts against the null hypothesis that OV would have no effect on discriminability.

To investigate the impacts of OV on response caution, we included a condition with cues that indicated the value category for the upcoming block. These cues did not provide any additional information. We included the value cues because in the DDM framework, decision-makers adjust their decision boundaries at the block level. Thus, we reasoned that the value cues would allow subjects to set (and reveal to us) their desired level of response caution for each value category. If decision-makers view higher-value decisions as more (less) important, value cues should increase (decrease) boundaries in high-value blocks.

To preview the results, across all three studies (for which studies 2 and 3 were preregistered), we find heightened, not reduced, discriminability as OV increases; we observe both faster and more accurate choices at high OV and a tendency toward slower and less accurate choices at low OV. However, we find that value cues increase response caution for high-value compared to middle-value trials, indicating that decision-makers are motivated to be slower and more accurate for high-value decisions. We find these same effects in all three studies, indicating that they are not due to familiarity/accessibility (51), different uses of the rating scale, or variability within value categories.

Results

Experimental Paradigm. In each experiment (preregistered in studies 2 and 3), participants completed a two-alternative, forced-choice task. The value of each option was determined using a separate rating task (studies 1 and 2) or objective values (study 3). We used these values to compute both the difficulty (i.e., value difference) and OV of each choice.

In phase 1 of study 1 ($n = 44$) and study 2 ($n = 50$), participants used a continuous 0 to 10 scale to rate their desire to consume snack foods (144 items) or view abstract images (107 items). Based on the results of this task, choice sets for phase 2 were generated such that each trial contained two items drawn from the same value category (Fig. 1).

In phase 1 of study 3 ($n = 70$), participants learned the value associated with different colored squares. During this training phase, they learned that there were 12 colors, each with a distinct point value from 1 to 12, based on the color's position on

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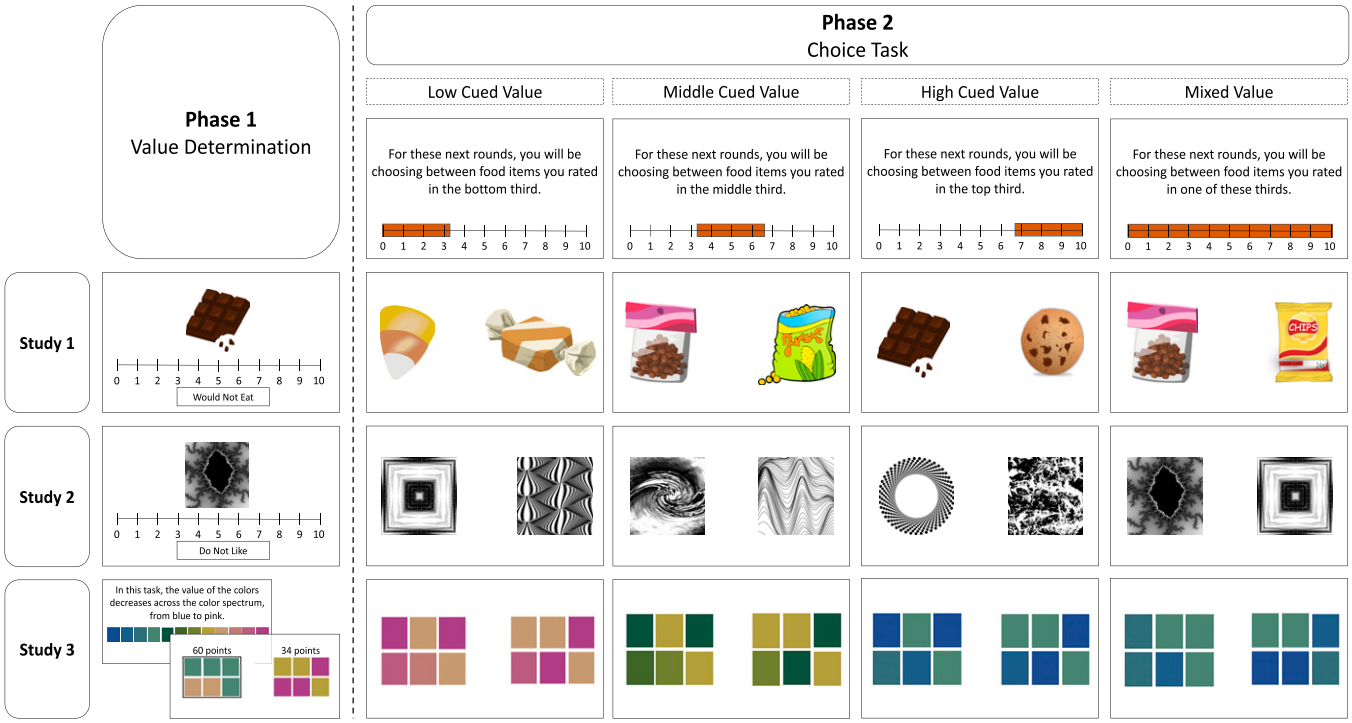

Fig. 1. Timeline of each study. Each experiment comprised two phases. Phase 1 consisted of liking ratings (studies 1 and 2) or learning the mapping from color to value (study 3). In study 3, the learning process consisted of an initial screen indicating the general relationship between color and value, followed by a training phase where choices were coupled with value feedback. Phase 2 consisted of cues at the beginning of each block, followed by a series of 15 choice trials using items from the same value category. The cues revealed the value range of the upcoming trials (here with value cues from study 1). In CV blocks, participants were informed whether the upcoming trials contained items from the bottom, middle, or top third of the value range. In MV blocks, participants were informed that the upcoming trials contained items from across the value range. In study 3, phase 2 consisted of the same choice task as in the training phase, but with more complex stimuli and no feedback.

a modified rainbow. The mapping from color to value was counterbalanced across subjects to ensure that there was no correlation between color perception and valuation. After the training, participants proceeded to phase 2, in which they chose between 2 × 3 arrays of the colored squares and earned points that were later converted to cash (see Fig. 1 for arrays and colors).

In phase 2 of each experiment, participants completed 270 binary choice trials in blocks of 15 trials. Each trial contained items drawn from the same value category based on each participant's subjective ratings in phase 1 of studies 1 and 2 (i.e., 0.01 to 3.33, 3.34 to 6.66, 6.67 to 10) or based on the value range of the color spectrum in study 3. One very important thing to note about study 3 is that the three OV conditions were matched in every dimension (e.g., range, variance, value difference) except OV. For details, see the *Materials and Methods*.

Blocks also came from one of two information conditions: cued-value (CV) or mixed-value (MV) trials. Prior to CV blocks, participants were informed of the value range of the options in the subsequent trials. Prior to MV blocks, participants were informed that the options in the subsequent trials could be drawn from the entire range of values (but both options would be drawn from the same value category).

Baseline Results. To evaluate the influence of OV and value cues on behavior, we examined the basic RT and choice accuracy data in the MV blocks (Fig. 2). First, we analyzed log-transformed RTs using linear regressions with clustered errors. Second, we analyzed choice accuracy (i.e., choosing the option with higher value) using logistic regressions with clustered errors. In both of these regression models, we assessed the effect of value category (i.e., high-value and low-value trials relative to middle-value trials) on the dependent variables while controlling for trial-level value difference.

Collapsing across value categories, we found strong, negative relationships between OV and log-transformed RT (see Fig. 2*A* for untransformed RTs; studies 1 to 3: $P < 0.001$; *SI Appendix*, Table S2). Comparing OV conditions, participants were faster in high-value trials than in middle-value trials in all three experiments (study 1: $P = 0.001$; study 2: $P < 0.001$; study 3: $P = 0.002$; *SI Appendix*, Table S2) and were faster in middle-value trials than in low-value trials, although this effect was not significant in study 2 (study 1: $P = 0.006$; study 2: $P = 0.172$; study 3: $P < 0.001$; *SI Appendix*, Table S2).

We also found evidence that accuracy (i.e., choosing the higher-rated/higher-valued item) was positively correlated with OV (Fig. 2*B*), even after accounting for value difference (study 1: $P = 0.376$; study 2: $P = 0.001$; study 3: $P = 0.004$; *SI Appendix*, Table S3). Comparing OV conditions, this relationship was more consistent at higher values; accuracy was significantly greater in high-value trials than in middle-value trials in studies 1 and 2 (study 1: $P = 0.021$; study 2: $P = 0.003$; study 3: $P = 0.097$; *SI Appendix*, Table S3) but was significantly greater in middle-value trials than in low-value trials only in study 3 (study 1: $P = 0.349$; study 2: $P = 0.998$; study 3: $P = 0.026$; *SI Appendix*, Table S3).

Cued Value. We next tested whether providing value cues affected behavior using the CV data. Providing participants with prior information about the OV of the upcoming decisions should allow us to observe whether they prefer to take more or less time on higher-value decisions.

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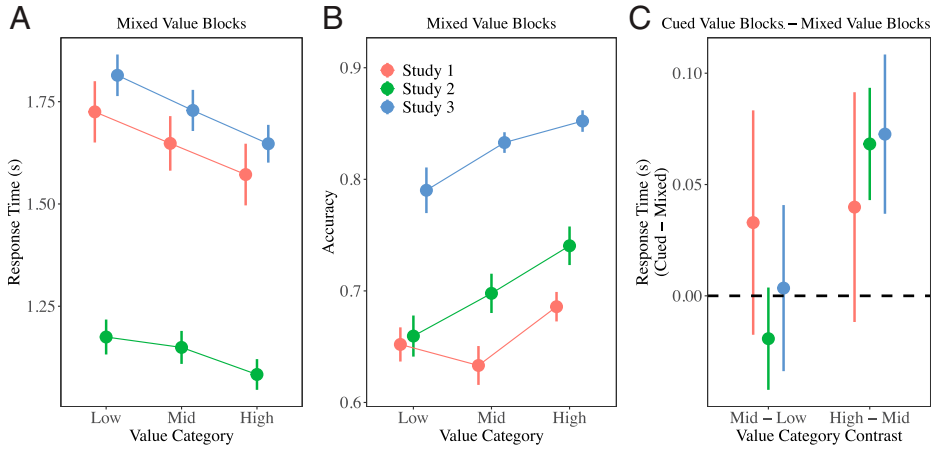

Fig. 2. Behavioral results. (*A*) Mean RT and (*B*) accuracy as a function of OV in the MV condition. (*C*) CV blocks–MV blocks: a difference-in-difference analysis of RT, examining the relative effect of an increase in value category (from Low to Middle or Middle to High) in the CV condition compared to the MV condition. A consistent interaction between value cues and OV would produce point estimates above the dashed line. See *SI Appendix*, Fig. S1 for the raw CV results. (*A*–*C*) Each study is depicted with a different color: study 1 (red), study 2 (green), and study 3 (blue). Error bars represent SE across subjects.

Our results partially indicate that decision-makers, when cued, perceive high-value decisions as more important. In all three experiments, value cues increased RTs in high-value CV trials relative to middle-value CV trials (compared to the difference in the MV trials), but this effect was only marginal in studies 1 and 3 (study 1: $P = 0.087$; study 2: $P = 0.003$; study 3: $P = 0.057$; *SI Appendix*, Table S2). On the other hand, there was no evidence for a relative RT increase in middle-value CV trials relative to low-value CV trials (compared to the difference in the MV trials) (Fig. 2*C*; study 1: $P = 0.336$; study 2: $P = 0.296$; study 3: $P = 0.762$; *SI Appendix*, Table S2).

There was some evidence that value cues decreased accuracy in low-value trials. While the coefficient was consistently negative for the interaction between value cues on low-value trials relative to middle-value trials (compared to the difference in MV trials), this effect was only significant in study 2 (study 1: $P = 0.255$; study 2: $P = 0.022$; study 3: $P = 0.742$; *SI Appendix*, Table S3). This general trend is in line with the idea that low-value trials are seen as less important when cued. There were no other consistent effects of value cues on accuracy (see *SI Appendix*, Table S3).

Drift Diffusion Modeling. The behavioral results from our three studies suggest both increased discriminability and motivated cautiousness in high-value contexts. To more thoroughly examine the mechanisms underlying these findings, we employed the DDM (48). This model assumes a noisy sequential sampling process, in which relative evidence is accumulated over time until it reaches one of two decision boundaries (corresponding to the left and right options, respectively).

In the DDM, there are two key components. One is the rate of evidence accumulation, known as the drift rate, which reflects how discriminable the options are and is typically assumed to be independent of OV (13, 48, 52). Higher magnitude drift rates equate to faster and more accurate decisions. Drift rates that decrease with OV would be consistent with diminishing value sensitivity. The second key component is the separation of the boundaries, which reflects how much evidence the decision-maker requires before making a choice (9, 48, 53). Wider boundaries reflect slower and more accurate decisions. If decision-makers view high-value decisions as more important, boundary separation should increase in the presence of value cues for high-value decisions. Low-value decisions should show the opposite effect.

In modeling these data, we allowed the drift-rate (v) and boundary-separation (a) parameters to vary by OV, the presence of value cues, and the interaction between these two factors. The drift rate was calculated as a linear function of trial-level value difference, as well as dummy variables corresponding to OV category (low, middle, high), presence of value cues (MV, CV), and their interactions. The boundary-separation parameters were each calculated as a function of the value category, value-cue variables, and their interactions. For each of these parameters, we used a regression approach with the intercept corresponding to performance in middle-value MV trials, and the parameters in other conditions calculated relative to that reference point (*Materials and Methods*).

We found strong evidence that drift rates were affected by OV (Fig. 3*A*). Compared to middle-value MV trials, high-value MV trials consistently had higher drift rates for larger value differences. The posterior probability of a positive interaction between value difference and high-value trials (p_{post}) was 0.99 in study 1, 1.00 in study 2, and 1.00 in study 3. The opposite effect was not consistently found for low-value MV trials. In particular, in study 2 and study 3, we found evidence that drift rates were higher for larger value differences in middle-value MV trials compared to low-value MV trials (study 2: $p_{post} = 0.79$; study 3: $p_{post} = 1.00$), but in study 1 we found evidence in the opposite direction, with $p_{post} = 0.32$, indicating that it was more likely that drift rates were higher for low-value trials.

We also found that value cues generally increased boundary separation in high-value trials relative to middle-value trials, compared to the difference in the MV trials (study 1: $p_{post} = 1.00$; study 2: $p_{post} = 0.96$; study 3: $p_{post} = 0.78$). However, there was weaker evidence for an increase in middle-value trials relative to low-value trials compared to the difference in the MV trials (Fig. 3*B*) (study 1: $p_{post} = 0.81$; study 2: $p_{post} = 0.46$; study 3: $p_{post} = 0.73$).

Boundary separation also varied across OV conditions in MV trials. Specifically, we found that in MV trials, the boundary separation was smaller in high-value trials than in middle-value trials (study 1: $p_{post} = 1.00$; study 2: $p_{post} = 1.00$; study 3: $p_{post} = 0.69$) and smaller in middle-value trials than in low-value trials (study 1: $p_{post} = 1.00$; study 2: $p_{post} = 0.66$; study 3: $p_{post} = 0.97$). It would therefore appear that participants incorporated value information at trial onset. However, we believe that this is likely an artifact of not accounting for attention in our modeling (for more on this point, see *Discussion*).

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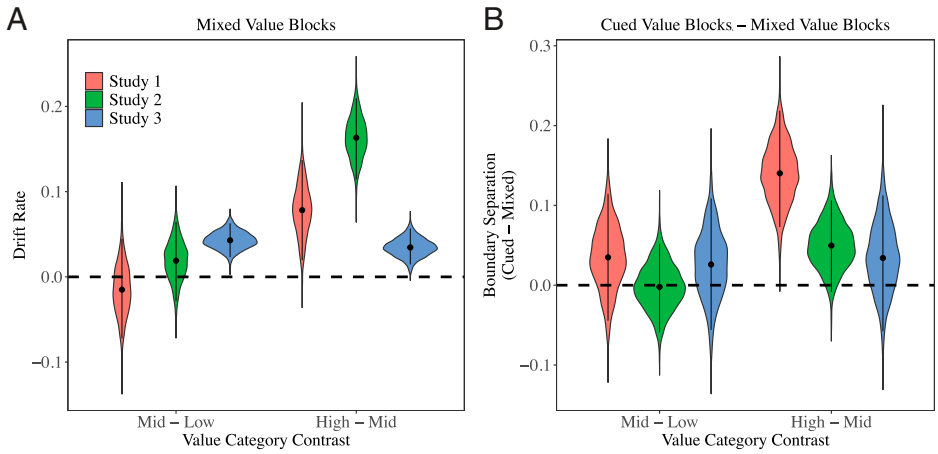

Fig. 3. DDM results. (*A*) The effect on the drift-rate slope (as a function of value difference) of an increase in OV (from Low to Middle or Middle to High) in the MV condition. A consistent, positive effect of OV would produce point estimates above the dashed line. (*B*) CV blocks–MV blocks: a difference-in-difference analysis of boundary separation, examining the relative effect of an increase in OV (from Low to Middle or Middle to High) in the CV condition compared to the MV condition. A consistent, positive interaction between value cues and OV would produce point estimates above the dashed line. See *SI Appendix*, Fig. S1 for the raw CV results. (*A* and *B*) Each study is depicted with a different color: study 1 (red), study 2 (green), and study 3 (blue). 95% highest density intervals (HDIs) are presented in black.

Main Effect of Value Cues. While we did not initially hypothesize any main effects of the value cues, we did observe effects on both drift rates and boundary separations. We describe these exploratory results here.

We found that value cues generally reduced RTs (Fig. 4*A*) but had inconsistent effects on accuracy (*SI Appendix*, Table S3). Middle-value trials showed a significant decrease in RT from MV to CV in every study (study 1: $P < 0.001$; study 2: $P = 0.037$; study 3: $P < 0.001$).

The DDM analysis revealed positive interactions between CV and value differences on drift rate in the middle-value trials from all three studies (study 1: $p_{post} = 0.91$; study 2: $p_{post} = 0.95$; study 3: $p_{post} = 1.00$) (Fig. 4*B*). There were no consistent interactions between OV, value difference, and CV on drift rate (*SI Appendix*, Table S4).

There was also an effect of CV on boundary separation (Fig. 4*C*). In particular, we found a decrease in boundary separation for cued, middle-value trials (study 1: $p_{post} = 1.00$; study 2: $p_{post} = 0.87$; study 3: $p_{post} = 1.00$).

Discussion

To investigate the effects of OV on the decision process, we ran three choice experiments in different domains. In all three experiments, we found that higher-value decisions were faster and more accurate, although the accuracy result was significant in only two of the three cases. Using the DDM, we found that in five out of six comparisons, higher-value decisions had higher drift rates. We also found that in the presence of value cues, high-value decisions were relatively slower, reflecting larger

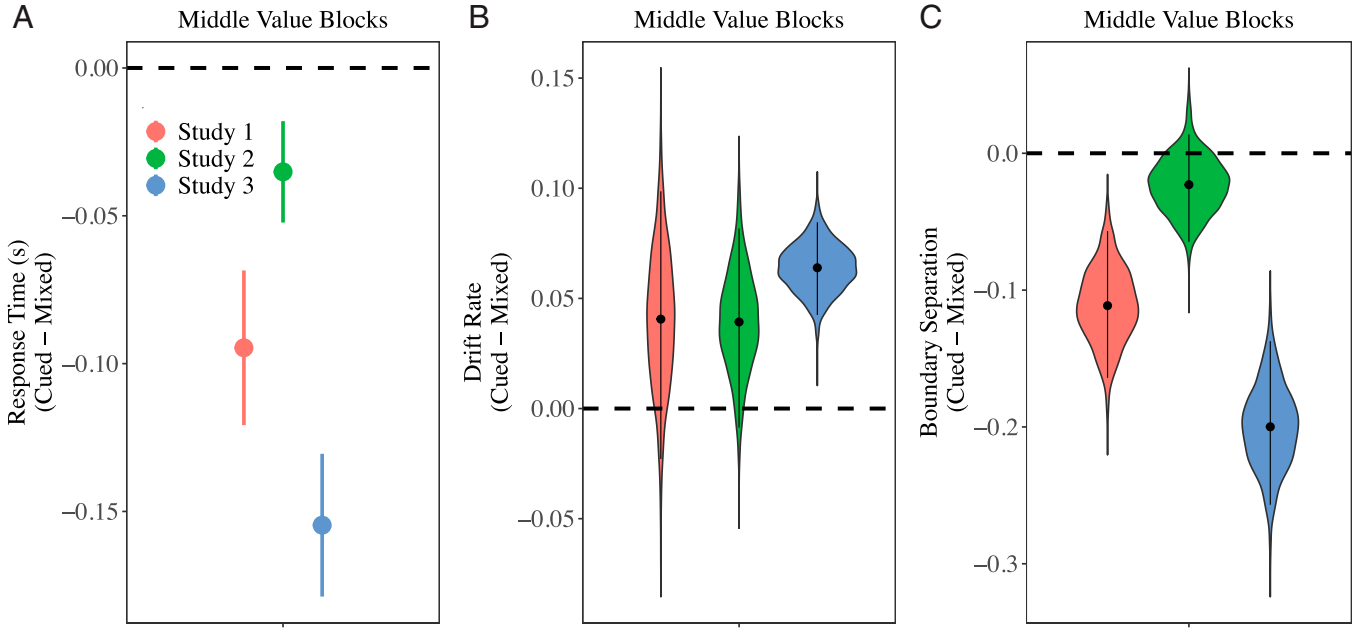

Fig. 4. Effects of value cues for middle-value trials. The difference between middle-value, CV, and MV trials for (*A*) RT, (*B*) the effect of value difference on drift rate, and (*C*) boundary separation for study 1 (red), study 2 (green), and study 3 (blue) is shown. Error bars represent (*A*) SE across subjects and (*B* and *C*) 95% HDIs.

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boundary separation in the DDM. Study 3 rules out that these findings are due to familiarity with higher-value options, distortions in rating scales, or differences in the variability within each value category (more detail in the *Discussion*) (50).

These results are inconsistent with diminishing value sensitivity. Instead, decision-makers are generally better able to discriminate between higher-value options. The results are also inconsistent with an intentional tradeoff between effort and reward (46, 54–60). These evolutionary accounts posit that decision-makers should feel less pressure to make the correct choice when most options are acceptable (46, 60). Instead, decision-makers seem to put more effort into these decisions. Thus, one explanation for our results is that large rewards may boost effort, making people subconsciously more engaged and consciously more cautious (61).

An alternative explanation for our results could be that decision-makers have stronger memory for higher-value options (62). Decision-makers might optimize their limited memory capacity by prioritizing more rewarding stimuli, which would enable them to recall their preference for these options both faster and more accurately (50). While value-based memory could explain the results in study 1, which used familiar snack foods, differences in encoding and retrieval cannot account for the results in study 2, which used novel images composed of abstract patterns, or study 3, which used random combinations of colored boxes. In study 2, subjects' preferences were likely constructed in the moment they evaluated the images, precluding the involvement of preference-based retrieval. Moreover, we explicitly designed study 3 to minimize memory requirements by using a perceptual scale to represent value (i.e., color along a rainbow). While this does not rule out memory-based explanations, the robustness of these results even in tasks in which memory is minimally involved makes such explanations unlikely.

Our findings call into question the combination of diminishing value sensitivity with standard stochastic choice models. There has been increasing interest in the links between perceptual and value-based decision-making (41, 63–65), and models of normalization and efficient encoding are central in that work. These models are supported by research indicating that neural reward circuits can adapt to the value range of stimuli in a choice set (30, 45, 50, 66–70). An important aspect of these models is that psychological value sensitivity is bounded by neural firing rates. In divisive normalization models, increased OV results in smaller differences in firing rates and reduced discriminability (ref. 70; see *SI Appendix* for details). However, we find that OV generally increases discriminability (31, 71).

How do we reconcile our results with risk aversion and its association with concave utility (17, 72)? Concave utility has been a prominent explanation for risk aversion since Bernoulli (14) and plays a central role in Expected Utility Theory and Prospect Theory. If decision-makers assign and compare utilities in the process of choosing (68), then concave utility implies that the same objective difference is subjectively smaller and less discriminable when option values increase (11). It is well established that decisions take longer and are more stochastic when the utility difference between options is smaller (4, 11, 12, 40, 73, 74). Thus, a concave utility function embedded in standard stochastic choice models would predict that decisions between high-value options should be slower and less consistent than those between low-value options. Our results show the opposite. There are, however, other models of risk aversion that do not rely on concave utility, including mean-variance tradeoffs (32) and rank-dependent utility (33, 34), leading some to question the relationship between risk aversion and concave utility (35).

In another study looking at risky choice, we failed to observe the same OV effects (*SI Appendix*). Additional work is needed to clarify the relationship between OV and risky choice. Our results could suggest that the OV effects do not apply to risky choice. One critical issue with most risky-choice paradigms, including our own (*SI Appendix*), is that the outcomes are presented as numbers. We see two pathways by which larger numbers could potentially slow the decision process. If decision-makers are conducting mathematical operations, larger numbers would take longer to process (75). On the other hand, if decision-makers encode the presence of large numbers early on (76), they might be able to adjust their response caution (i.e., boundary separation) prior to engaging in the comparison process. Like with our value cues, this would result in slower high-value decisions (*SI Appendix*, Fig. S2). Moreover, our risky-choice task included four outcomes and four probabilities per option, which might have made the task too challenging and encouraged the use of heuristics. Consistent with our riskless data, one risky-choice study using single, nonnumeric outcomes did observe a negative relation between OV and RT (45). A complication arises when evaluating the relation between OV and accuracy, as there is no objective criterion for choosing between risky prospects. Overall, we speculate that the important distinction is between numeric and nonnumeric outcomes rather than between riskless and risky choice.

We must also discuss the generalizability of our results. Intuitively, a $5 value difference, just like a 1-oz weight difference, should be easier to discern when choosing between lunch options than between cars. The problem with this line of reasoning is that values are rarely unidimensional and objective like other perceptions. We can objectively determine which object is heavier because weight can be measured with a scale. There is no such scale for value. Choice options typically have multiple dimensions/attributes, and value is based on the combination of subjective evaluations and weights for each dimension (8, 77). More valuable consumer goods tend to be more complex, involving more dimensions. Study 3 was our attempt to circumvent this issue by making value unidimensional while also keeping the decisions from being perceptually trivial or measures of numeracy. Importantly, study 3 featured choice problems that were identical across OV conditions (except in mean value), ruling out any possible influence of variability within value categories. Moreover, the values of the stimuli were counterbalanced so that values increased across the blue-to-red spectrum for half of participants but decreased for the other half, precluding any possibility that the results could be attributed to participants' ability to discriminate between colors on different parts of the spectrum.

Our results also indicate that people treat high-value decisions as more important, in the sense that they approach them with more caution when cued (this was particularly true in our alternative DDM formulation, presented in the *SI Appendix*). Several prior studies have argued that fast high-value decisions reflect the desire to quickly resolve choice problems when both options are satisfactory (46, 60). Others have argued that increasing OV motivates decision-makers to extract more precise estimates of the relative differences between options, facilitating faster and more accurate decisions in high-value contexts (40). If the OV effects were due to either of these factors, then the value cues should have further reduced high-value RT relative to middle-value RT. Instead, we saw the opposite.

It is worth noting that the cues reversed the baseline OV effects. The DDM fits indicate that the inverse relation between RT and OV at baseline was due to two factors: increased drift rates and decreased boundary separation for high-value decisions. It is difficult to reconcile the latter explanation with the effects of the value cues. Indeed, we believe that the baseline boundary-separation result is likely an artifact of not accounting for attention in our modeling. In the DDM, strategic parameters such as boundary separation are typically held constant across categories that are interspersed within a block (48).

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In other words, boundary-separation adjustments are not expected to appear at the trial level but are expected at the block level when people are given advance warning. This was the motivation for our CV condition. Recent work indicates that the DDM will appear to exhibit boundaries that decrease with OV when fit to data generated by a model with constant boundaries but value-amplifying attentional effects (78, 79). Since our model did not incorporate any measures of attention, these baseline boundary-separation results should be interpreted with caution.

When analyzing the effects of the high- and low-value cues, we first subtracted the effect of the middle-value cues before making comparisons to the MV blocks. This was important because we observed that value cues generally reduced decision boundaries across the board, as evidenced by the negative effect of cue in all three studies. This suggests that narrowing decision-makers' expectations may cause them to set less cautious response boundaries (59, 80), generate more precise beliefs (81), and/or integrate information more clearly (82). This is particularly true in our design, in which there was less OV variability in the CV blocks compared to the MV blocks.

Curiously, we generally observed our two central effects for high-value versus middle-value contrasts but less consistently observed these effects for low value versus middle value. We found mixed evidence that low-value trials had reduced rates of evidence accumulation in the baseline condition or reduced boundaries in the CV condition. It is unclear why this consistent asymmetry exists. One possibility is that our participants saw high-value trials, but not low-value trials, as qualitatively distinct from middle-value trials. Perhaps if we had used narrower value categories or included aversive options, we would have observed these effects in the lower-value categories. A second possibility for the latter finding is that our participants were only willing to put more effort into their decisions, not less. This explanation is supported by evidence that some decision-makers, especially older adults, tend to overemphasize accuracy and struggle with instructions to speed up (83, 84).

In summary, our findings challenge the presence of diminishing value sensitivity in value-based decision-making. Contrary to our expectations, high-value decisions appear to be faster and more accurate, indicating that people are better at them. Moreover, alerting people to high-value decisions leads them to be more cautious. Future research must seek to understand these surprising phenomena and consider the limits of extending perceptual models to value-based decision-making.

Materials and Methods

Participants. A total of 223 individuals were recruited from a large sample of students at The Ohio State University. Forty-nine participants were recruited in study 1. Of these, five were excluded from our analyses for failing to perform significantly above chance in the binary choice task. Ninety-three participants were recruited for study 2. Eleven individuals could not complete the art task due to technical issues. Nineteen individuals were excluded because of insufficient variability in their initial ratings of the images (preventing our automated algorithm from generating the necessary quantity of trials without repetition). An additional 13 individuals were excluded for failing to perform significantly above chance in the binary choice task. Eighty-one participants were recruited for study 3. In study 3, seven individuals were excluded due to self-reported color blindness, and an additional four participants were excluded for failing to perform significantly above chance in the binary choice task. All exclusions were made according to our preregistration. All participants gave informed consent. All experiments involving human participants were approved by the Institutional Review Board at The Ohio State University.

Procedures.

Study 1. Participants performed the experiment in two sequential phases. In phase 1, participants used a mouse to rate their desire to consume each of 144 snack foods on a continuous scale from 0 to 10. Participants were told that a rating of 0 indicates that they neither like nor dislike the food item, and a rating of 10 indicates that they really like the food item. Participants were also given the option of selecting a button with the words "Would Not Eat" written on it. Participants were informed that they should select the "Would Not Eat" button if they would rather eat nothing than eat the presented food item. Participants were allowed to complete the rating task at their own pace.

In phase 2, participants completed a binary choice task using the set of positive snack foods. In each trial, participants were presented with two food items, one on each side of the screen, and were instructed to select the item that they would most like to eat at the end of the experiment. Participants were instructed to make selections with the left and right arrow keys at their own pace. Each trial was designed so that the two items' ratings (from phase 1) were both in the same third of the rating scale (i.e., value category: 0 to 3.33, 3.34 to 6.66, 6.67 to 10). In addition, no item could be shown more than seven times, and no item that elicited a "Would Not Eat" response was included in these trials. In order to construct enough trials for each value category, participants needed to have at least 15 items in each value category. If they did not, we generated random trials for them to complete; however, their data were not analyzed any further.

Trials were grouped into blocks of 15. CV blocks contained 15 trials from a single value category, while MV blocks contained five trials from each of the value categories, presented in a random order. In the CV blocks, participants were informed from which value category the items in the subsequent trials were drawn. In the MV blocks, participants were informed that the subsequent trials could be drawn from any of the value categories. Participants completed nine blocks each of the CV (three per value category) and MV conditions for a total of 270 trials.

We incentivized both phases of the experiment. Participants were informed that they would receive a food item based on their decisions in either phase 1 or phase 2. At the end of the experiment, one of those phases was randomly chosen (each with equal probability) to be paid out. If phase 1 was selected, two food items were randomly drawn, and the participant received the item with the higher rating. If both items had elicited responses of "Would Not Eat," then no item was awarded. If phase 2 was selected, one trial was randomly drawn, and the participant received the item that they chose in that trial. Additionally, all participants received a $15 show-up fee.

Study 2. In preregistered study 2, we wanted to ensure the robustness of our effects by replicating study 1 using different stimulus sets. In order to determine a sufficient sample size, we ran a simulation-based power analysis using a mixed-effects regression from study 1. We determined that a sample size of 50 would give us adequate power. However, in order to generate enough unique trials for all of the trial blocks, participants had to have rated at least 15 items in each value category. For those that did not, we generated random trials for them to complete, but their performance data were omitted from further analyses. In order to account for participant attrition and the exclusion criteria mentioned above, we recruited 93 participants for this study.

Study 2 was composed of two separate experiments: an abstract art task (study 2A) and a complex lottery task (study 2B). We only report the results of the abstract art task (details on study 2B can be found in the *SI Appendix*).

Participants completed three phases occurring within the same study session. Participants were randomly assigned to first complete either study 2A (phases 1 and 2) or study 2B (phase 3). Phases 1 and 2 involved the same procedures as those in study 1. However, in phase 1, prior to rating 107 abstract art images, participants viewed a slideshow of all images, each presented for 0.75 seconds. Subsequently, participants rated the images on a scale from 0 to 10 and were given the option to select a "Do Not Like" option if they would rather receive nothing than receive a button with the image printed on it at the end of the experiment. Based on these ratings, the images were organized using the same procedures from study 1. The procedures for phase 2 were identical to that of study 1.

For study 2A, participants were informed that they would receive a button with an image printed on it, based on their decisions in either phase 1 or phase 2. At the end of the experiment, one of those phases was randomly chosen (each with equal probability) to be paid out. If phase 1 was selected, two images were randomly drawn, and the participant received a button with the higher-rated image. If both items had elicited responses of "Do Not Like," then no button was awarded. If phase 2 was selected, one trial was randomly drawn, and the participant received a button with the image that they chose in that trial. Additionally, all participants received the outcome from one lottery randomly chosen from their selections in study 2B (minimum: $0.50, maximum: $25), as well as a $5 show-up fee.

Study 3. In preregistered study 3, we wanted to replicate the effects of the previous two studies but without relying on subjective value estimates. For this design, we used a reinforcement learning paradigm broken up into two phases. A sample size of 50 was determined using the same power analysis from study 2. In order to account for participant attrition and potential exclusions, we preregistered that we would collect data from a minimum of 60

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participants. Because of the laboratory set-up, in which we ran up to 30 participants at a time, we stopped recruiting participants once we surpassed 60 and ultimately recruited 81 participants for this study.

Participants completed two phases within the same session. In phase 1, participants initially viewed a color spectrum composed of 12 distinct colors. Participants were informed that the value of these colors was either increasing or decreasing (counterbalanced across participants) from left to right across the spectrum. Subsequently, participants completed a training task, in which they chose between two stimuli, each composed of six colored squares. The point values for each colored square ranged from 1 to 12. After each choice, participants saw the values of both stimuli (based on the sum of the colored squares), as well as their total earnings. Participants first completed a block of 30 trials, after which point their accuracy was assessed. If they reached or surpassed 70% accuracy, they proceeded to phase 2; otherwise, they completed another block of 30 trials. This process was repeated until participants achieved 70% accuracy or they completed six blocks. See *SI Appendix*, Fig. S3 for additional details.

In phase 2, participants faced 270 trials in a binary choice task. Trials were constructed by first creating stimulus pairs for the middle-value condition and then subtracting or adding a constant value of 4 to every square. For example, a middle-value trial of {5,5,5,6,6,6} versus {6,6,6,8,8,8} would have an equivalent {1,1,1,2,2,2} versus {2,2,2,4,4,4} low-value trial and a {9,9,9,10,10,10} versus {10,10,10,12,12,12} high-value trial. In this way, we were able to equate the three OV conditions in every way except mean value.

We used a conversion rate of 30 points per $1. In order to properly constrain participants' earnings, participants started out with a deficit of 10,500 points. Participants who ended the study with a negative balance only received the show-up fee. Participants were informed of this deficit and the conversion rate at the beginning of the study.

We also constrained the stimuli so that the colored squares were never all the same color, but there were no other restrictions on color repetition. Additionally, the value difference in each trial was always between 1 and 5. Otherwise, the procedures for phase 2 were identical to that in studies 1 and 2. At the end of the study, participants received the monetary equivalent of the total points they earned throughout the study (minimum: $0; maximum: $13; median: $10.70; mean: $9.81; SD: $2.87), as well as a $5 show-up fee.

Data Preprocessing. Outlier trials were identified by focusing on RTs using the interquartile range (IQR) method at the subject level. The IQR method eliminates trials when RTs are above the 0.75 quartile by more than 1.5 times the IQR or below the 0.25 quartile by more than 1.5 times the IQR. Additionally, trials were removed if the RTs were less than 0.25 s after the IQR treatment. Using this method, we removed 6.76% of trials in study 1, 7.30% of trials in study 2, and 3.24% of trials in study 3.

Behavioral Analyses. Behavioral data were analyzed with linear and logistic regressions using clustered errors (at the subject level) in R (version 3.6.1). RTs were log transformed before being analyzed. For each experiment, we ran a trial-level regression of log(RT) on the absolute value difference between items, value category (dummy variable for low, middle, or high OV), block type (dummy for MV or CV blocks), and the interaction between value category and block type. We also ran a similar logistic regression (with clustered errors) of choice accuracy as a function of absolute value difference, value category, block type, and the interaction between value category and block type.

Computational Modeling. We fitted the choice and RT data in each experiment using the HDDM package (85) in Python (version 3.6.7). This hierarchical Bayesian model assumes that the model parameters for individual participants are sampled from group-level distributions. The model uses Bayesian statistical methods to estimate parameters at the group and individual levels.

The model incorporates several parameters related to this decision process. The parameter drift rate (v) accounts for the average rate of evidence accumulation and is driven by the value difference between the left and right options. Boundary separation (a) is the total amount of evidence required to initiate a decision. Nondecision time (t) accounts for the time related to processes outside of the decision process, such as encoding information and initiating a motor response. The starting point parameter (z) accounts for response bias in the decision process. Here, we assumed symmetrical boundaries and fixed z at 0.5. We did not incorporate across-trial variability parameters in drift rate (sv), nondecision time (st), or starting point (sz). In practice, incorporating these parameters does not alter our main conclusions, and ultimately reduced the model fit metrics. Using standard priors in HDDM (85), we estimated the model using two chains, each with 15,000 samples, where the first 10,000 samples were discarded as burn-in. We assessed model convergence by calculating the Gelman-Rubin statistic (86). All chains for each data set had a Gelman-Rubin statistic below 1.1, indicating successful convergence.

To test our hypotheses, we fitted the data in each experiment with a model that allowed the boundary-separation parameter (a) to vary as a function of the value category (low value, middle value, or high value) and block type (MV or CV). In this regression, we used middle-value MV trials as the baseline with the formula

$$a = a_0 + a_{\text{Low vs Mid}}LV + a_{\text{High vs Mid}}HV + a_{\text{Cue}}Block + a_{(\text{Low} \times \text{Cue}) \text{ vs } (\text{Mid} \times \text{Cue})}(Block \times LV) + a_{(\text{High} \times \text{Cue}) \text{ vs } (\text{Mid} \times \text{Cue})}(Block \times HV), \quad [1]$$

where LV is the dummy for low-value trials, HV the dummy for high-value trials, and $Block$ the dummy variable for CV trials.

The drift-rate parameter (v) was estimated as a linear function of the value difference, the value category, and block type, all interacted. Again, we used middle-value MV trials as the baseline, with the formula

$$\begin{aligned} v = {} & v_0 + v_{\text{VD}}(r_L - r_R) + v_{\text{Low vs Mid}}LV + v_{\text{High vs Mid}}HV \\ & + v_{\text{Cue}}Block + v_{(\text{Low} \times \text{Cue}) \text{ vs } (\text{Mid} \times \text{Cue})}(Block \times LV) \\ & + v_{(\text{High} \times \text{Cue}) \text{ vs } (\text{Mid} \times \text{Cue})}(Block \times HV) + v_{(\text{Low} \times \text{VD}) \text{ vs } (\text{Mid} \times \text{VD})}(LV \times (r_L - r_R)) \\ & + v_{(\text{High} \times \text{VD}) \text{ vs } (\text{Mid} \times \text{VD})}(HV \times (r_L - r_R)) + v_{(\text{Cue} \times \text{VD})}(Block \times (r_L - r_R)) \\ & + v_{(\text{Low} \times \text{Cue} \times \text{VD}) \text{ vs } (\text{Mid} \times \text{Cue} \times \text{VD})}(Block \times LV \times (r_L - r_R)) \\ & + v_{(\text{High} \times \text{Cue} \times \text{VD}) \text{ vs } (\text{Mid} \times \text{Cue} \times \text{VD})}(Block \times HV \times (r_L - r_R)), \end{aligned} \quad [2]$$

where r_L and r_R are the values of the left and right items, respectively. Since responses were coded left (1) and right (0), we only interpret the parameter estimates that interact with value difference [i.e., $(r_L - r_R)$].

In *Drift Diffusion Modeling*, we report the Low versus Mid parameters as Mid versus Low by flipping the signs. This makes it so that the lower-value category is consistently the reference point. To calculate the probability of change in boundary separation and drift rate, we estimated the average of the posterior estimates above or below 0 (depending on the test).

Posterior-based simulations. We evaluated the quality of the DDM by comparing participant behavior to behavior predicted by the posterior estimates. Using the posteriors obtained from HDDM, we simulated choice and RTs separately for each participant in each experiment. The simulated choice probabilities matched the data well and the simulated RTs provided reasonable fits to the distribution of correct RTs in the data (*SI Appendix*, Figs. S4–S6). However, in study 3, the DDM failed to fully capture the error RTs (*SI Appendix*, Fig. S6).

Data Availability. All materials, data, and analysis code for all experiments and studies and preregistrations for studies 2 and 3 are available on the Open Science Framework (https://osf.io/hypnc/).

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