Transfer of information across repeated decisions in general and in obsessive–compulsive disorder

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Real-life decisions are often repeated. Whether considering taking a job in a new city, or doing something mundane like checking if the stove is off, decisions are frequently revisited even if no new information is available. This mode of behavior takes a particularly pathological form in obsessive–compulsive disorder (OCD), which is marked by individuals' redeliberating previously resolved decisions. Surprisingly, little is known about how information is transferred across decision episodes in such circumstances, and whether and how such transfer varies in OCD. In two experiments, data from a repeated decision-making task and computational modeling revealed that both implicit and explicit memories of previous decisions affected subsequent decisions by biasing the rate of evidence integration. Further, we replicated previous work demonstrating impairments in baseline decision-making as a function of self-reported OCD symptoms, and found that information transfer effects specifically due to implicit memory were reduced, offering computational insight into checking behavior.

decision-making | obsessive–compulsive disorder | drift diffusion model

People frequently reconsider their decisions. For example, you may engage in a long deliberation about whether or not to take a job in another city, vacillating between the two options before settling on an answer. However, the next day may bring renewed doubt and a second decision episode between the same two options. Such doubt and rechecking may also sometimes take a pathological form. This is most evidently seen in the context of obsessive–compulsive disorder (OCD), which is often characterized by patients revisiting and redeliberating previously settled decisions even about relatively mundane and unambiguous everyday topics, like whether a door is locked or their stove has been turned off (1–3).

Surprisingly, although it is ubiquitous repeated decision-making has received relatively little attention in laboratory studies and computational modeling work. Instead, attention has focused almost exclusively on redeliberation within the context of single decision episodes. In a typical experiment from this line of work, participants are presented with a decision and are required to give their answer using a continuous response modality, for example a mouse or a physical handle like the one employed in motor control experiments (4–7). Decisions are registered by moving the device toward one of two or more decision points. The data from these studies show that while the majority of trials typically begin and end in the direction of a single decision point, on some trials participants change their mind in the middle of their response and start moving toward one option but ultimately choose another. Computational models describing such data have come from the broader class of evidence integration models, in which momentary evidence for and against each option is integrated until a threshold is reached, at which point the option with the largest amount of relative evidence is selected. In order to explain data on continued deliberation and changes of mind, these models make the assumption that a response begins when the evidence reaches an initial threshold, but the evidence integration process also continues and the evidence for another option may subsequently reach threshold (4, 6, 7). As stated at the outset, however, this work has so far been limited to single individual decision episodes.

In addition, and perhaps partially resulting from a scarcity of basic research on continued deliberation across decisions, little is known about the computational properties by which checking manifests in OCD. Previous work has suggested a number of (potentially nonindependent) possible global reasons for excessive checking, including perfectionism (8), intolerance of uncertainty (9), inflated responsibility (10), positive beliefs about worry (11), and an overestimation of threat (12). However, it is unclear specifically how checking manifests computationally. In fact, it is not known which aspects of repeated decision-making are impacted even in a broad sense. Two categories of questions are of interest with regard to repeated decisions generally, including outside the scope of OCD: when and how individuals choose to revisit decisions and how information is transferred between decisions when they are repeated. With respect to OCD, it is not known whether one or both of these aspects of information processing are impacted. We developed a task to study continued deliberation across decision episodes in general and to understand how this process varies with individual differences in subclinical OCD symptom severity in the population. Our focus and scope of the current paper is devoted to questions related to the transfer of information. However, as will be seen, the experimental and modeling framework are general enough to pursue a number of follow-up questions, including questions related to meta–decision-making, which have clear importance.

Significance

Real-life decisions are often repeated. Whether considering taking a new job, or doing something mundane like checking if the stove is off, people frequently revisit decisions. This mode of behavior takes a particularly pathological form in obsessive–compulsive disorder (OCD). Surprisingly, little is known about how information is transferred across decisions, and whether and how such transfer is disrupted in OCD. Data from a repeated decision-making task and computational modeling revealed that different memory systems separately biased repeated decisions toward previous choices by changing how people weigh evidence. Transfer specifically driven by implicit memory was reduced in individuals with higher levels of OCD symptoms on top of other baseline decision-making deficits, offering computational insight into checking behavior.

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In developing the paradigm we chose the dot motion perceptual decision-making task as the baseline decision task which was to be repeated (13–15). In this task, participants are shown an array of dots, some percentage of which consistently move in the same direction (e.g., left or right) while others appear at random. The goal is to determine the average direction in which the coherent dots are moving. Difficulty can be modulated from trivial to chance performance by changing the percentage of dots undergoing coherent motion. We chose this as the baseline task for a number of reasons. First, it is well-studied and has shed light not only on motion and visual perception but on decision-making more generally, for which it has served as a fruitful laboratory model (13–16). As a result, its computational properties are also well-established: Decision-making in this task is well-explained by the drift-diffusion model (13, 15, 17), a particularly tractable exemplar from the class of evidence integration models which is relatively easy to fit and which can be implemented within a multilevel framework to improve parameter recovery (18–20). This also aligned within a more general goal we had for developing a paradigm that was complex enough to ask a wide array of questions about continued deliberation across decisions but simple enough to reliably model and recover parameter values at the individual subject level without onerously long testing sessions. Finally, numerous studies have documented impairments within single decision episodes in this task both in patients with OCD and in subclinical individuals high on the OCD spectrum (21–24) (but see also ref. 25), making it an obvious choice for extending the investigation of individual differences across repeated decision episodes.

The drift-diffusion model describes decisions with two possible outcomes, such as in the present context where the decision is whether motion is left- or rightward. A schematic illustration of the basic form of the model is shown in Fig. 1. The state of the decision, or the evidence or preference for one option versus the other at a particular point in time, evolves in a noisy fashion over the course of the decision. It has an average velocity (drift rate) represented by a free parameter that can be inferred from an individual's behavioral data, but the trajectory is also subject to Gaussian noise. In the dot motion task, drift rate represents the amount of moment-by-moment evidence extracted from the stimulus in the service of decision-making. Although drift rate is generally stimulus-driven, it can also be affected by external factors such as attention (26, 27). The two possible outcomes are represented in the model by decision boundaries that flank the preference state on both ends, and the distance between them is captured by another free parameter. A decision is made when the preference reaches one of these boundaries. Initial bias in preference, before the stimulus is seen, can be incorporated by moving the starting position to be closer to one of the boundaries. The starting preference is captured by a third free parameter. The drift-diffusion model has been studied in a wide range of contexts and domains including the dot motion task and has converging support from both behavioral and neural data (13, 15, 17, 28–30). Previous work has shown specifically that drift rate is impaired (reduced) both in patients with OCD and in subclinical individuals with high levels of self-reported OCD symptoms (21–24). Moreover, impairments are larger for easier decisions (21, 24). Although one might intuitively also expect to see differences in boundary separation, with boundaries placed further apart (i.e., decisions being more deliberate and less impulsive), this has not been borne out by the now ample data on this task. We return to this point in *Discussion* in the context of the present work.

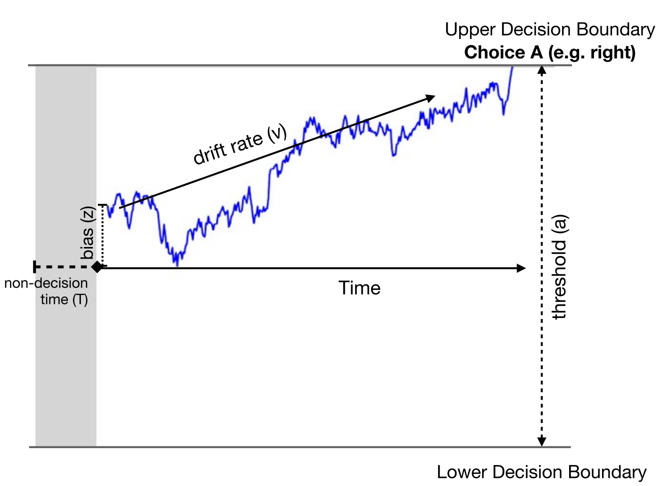

Fig. 1. Schematic illustration of the basic form of the drift-diffusion model, which describes decisions with two possible outcomes. The state of the decision, representing the preference for one outcome versus the other, follows a directed but noisy trajectory. The two outcomes are represented by two decision boundaries, and a decision is made when the preference state reaches one of them. Bias for one outcome versus the other before seeing the stimulus can be incorporated by changing the starting position of the preference state. A separate parameter also captures nondecision time during the trial, such as detecting the stimulus and responding.

In the task (Fig. 2), participants made dot motion decisions that were tagged with markers (words) that individually identified each decision trial. Later, they were asked about the choice they made on the trial associated with a marker and were then asked to repeat the same decision a second time. We used the drift-diffusion model and our task to investigate four sets of questions. First, we asked whether participants' memory of their first decision episode affected the second decision by changing the initial bias in preference, the drift rate, or both, regardless of individual differences. For example, the starting preference may be placed close(r) to the response boundary representing the choice made during the first decision. A small amount of additional confirmatory evidence can then push the decision over threshold. In contrast, a change in drift rate would mean a more persistent and continuous bias entering into the deliberation process. Second, we asked whether the actual choices participants made during the first decision episode affected the second decision's initial bias in preference and/or drift rate separately from the effects due to explicit memory retrieval, which sometimes resulted in retrieving the incorrect choice.

Third, we asked whether we could replicate previous work showing that drift rate during an initial decision negatively correlates with OCD symptom severity (21–24), whether this also extends to differences in baseline drift rate during a repeated decision, and whether similar deficits can be seen during the course of memory retrieval when recalling a previous decision. Notably, the drift-diffusion model can account for memory retrieval using the same set of mechanisms (17, 31), allowing us to ask whether symptoms track similar computational deficits across cognitive domains. Given the above prior work with the dot motion task and single individual decisions, we expected baseline drift rate to be reduced as a function of OCD in both decision episodes in our task, with larger deficits for easier decisions (higher coherence trials). In addition, given previous reports of memory for actions also being impaired (32, 33), we expected that drift rate for the memory retrieval would also be reduced.

Finally, we asked whether the transfer of information from the first decision to the second decision varied with OCD symptom severity, and if so how this manifested. Paralleling the main effects, how the starting preference and/or drift rate are modulated by either or both explicit and implicit memory may be

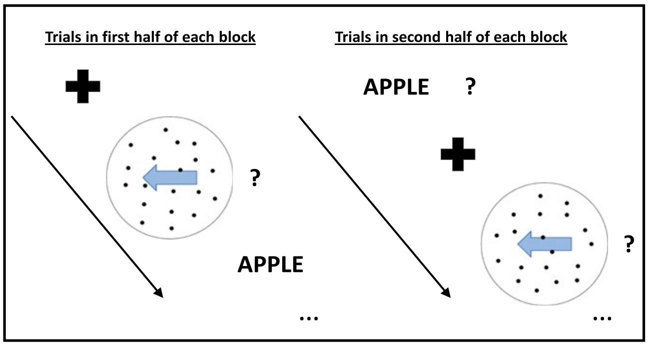

Fig. 2. Order of events within trials that appear during the first and second half of each block. During the first half, trials begin with a fixation cross, followed by a dot motion decision, and then a word that uniquely tags the trial. During the second half, trials begin with a word first and participants have to recall the decision they made on the corresponding trial during the first half. This is followed by a fixation cross and a dot motion decision with the same level of coherence and direction of motion as the corresponding trial.

affected. Although we did not have a strong a priori hypothesis about a particular failure mode, reductions in the carryover by drift rate may perhaps be expected to be more likely given the prior work on reductions in drift rate in single dot motion decisions in high-OC individuals.

Results

The repeated decision-making task was divided into blocks of six trials. In the first half of each block (the first three trials), participants performed dot motion decision trials at three different levels of coherence. Each decision was followed by a word that uniquely identified the trial. In the second half of the block, the same three trials were presented in random order. However, these trials began with the identifying word. Upon seeing the word, participants had to recall their choice from the first decision episode. They were then presented with the same dot motion stimulus (the same level of coherence and direction of motion) a second time. If they were confident about their decision, they could select the same direction right away. Otherwise, they could integrate additional evidence. The order of events in each block half can be seen in Fig. 2. Participants also completed the Padua Inventory (34), a widely used measure of OCD symptoms. We ran two experiments, the second to replicate the results of the first and to establish the specificity of the OCD-related effects while controlling for symptoms of anxiety and depression, cognitive ability, and several demographic factors (*Methods*). The results from both experiments are presented in parallel.

Basic Effects. Fig. 3 *A* and *B* displays the basic behavioral results—accuracy and reaction time—for each decision episode. There was a clear effect of coherence (Greenhouse–Geisser-corrected; dataset 1: $F(1.47, 255.07) = 106.17$, $P = 1.20 \times 10^{-27}$, $\epsilon = 0.73$; dataset 2: $F(1.48, 312.85) = 50.76$, $P = 8.60 \times 10^{-16}$, $\epsilon = 0.74$), but not decision episode (dataset 1: $F(1, 174) = 0.9$, $P = 3.5 \times 10^{-1}$; dataset 2: F (1, 212) = 0.01, P = 9.3 × 10^{-1}) on accuracy. In contrast, reaction time was significantly modulated by both coherence (Greenhouse–Geisser-corrected; dataset 1: $F(1.16, 201.11) = 100.23$, $P = 1.90 \times 10^{-21}$, $\epsilon = 0.58$; dataset 2: $F(1.3, 275.46) = 59.27$, $P = 3.90 \times 10^{-16}$, $\epsilon = 0.65$) and decision episode (dataset 1: $F(1, 174) = 134.28$, $P = 2.20 \times 10^{-23}$; dataset 2: $F(1, 212) = 59.42$, $P = 4.90 \times 10^{-13}$), with the second decision episode being substantially faster. The interaction of coherence and decision episode on reaction time was also significant (Greenhouse–Geisser-corrected; dataset 1: $F(1.38, 240.09) = 48.83$, $P = 3.50 \times 10^{-14}$, $\epsilon = 0.69$; dataset 2: $F(1.64, 348.23) = 21.15$, $P = 3.40 \times 10^{-8}$, $\epsilon = 0.82$). Although reaction times were faster during the second decision episode, they were still substantial (1 to 1.5 s; Fig. 3). *SI Appendix*, Table S1 displays the pairwise contrasts.

Fig. 3 *C* and *D* displays the accuracy and reaction time for the memory retrieval—the point in time during the second half of each block when participants saw a word and had to recall the decision they previously made on the corresponding trial. There was no evidence that coherence affected either accuracy (dataset 1: $F(2, 348) = 0.04$, $P = 9.6 \times 10^{-1}$; dataset 2: $F(2, 424) = 0.09$, $P = 9.1 \times 10^{-1}$) or reaction time (dataset 1:

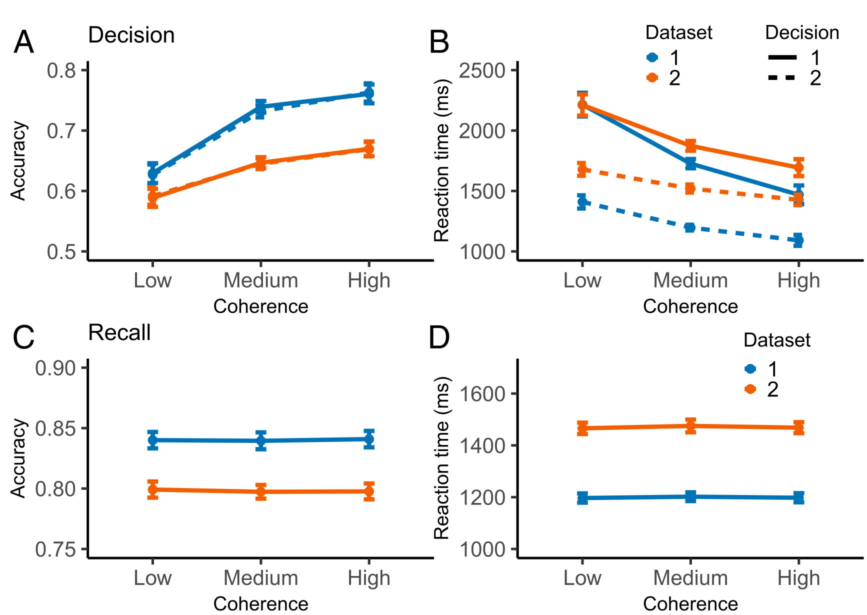

Fig. 3. Raw behavioral data. (*A*) Accuracy for each difficulty level and decision episode. (*B*) Reaction time for each difficulty level and decision episode. (*C*) Accuracy for the memory retrieval prior to the second decision episode for each decision difficulty level. (*D*) Reaction time for the memory retrieval prior to the second decision episode for each decision difficulty level.

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$F(2, 348) = 0.1, P = 9.1 \times 10^{-1}$; dataset 2: $F(2, 422) = 0.17, P = 8.4 \times 10^{-1}$), which was not surprising given that participants were instructed to try to remember all of their decisions regardless of the difficulty or whether they thought the decision was correct.

Taken together, these data suggest that, on average, participants remember their choices across trials but also integrate additional evidence during the second decision. In order to more precisely understand the computational properties of each decision episode and the memory retrieval, and how information flows from the first decision episode to the second, we fit a set of drift-diffusion processes: one for each decision and one for the memory retrieval. Models were fit using a multilevel Bayesian framework (*Methods*). In reporting the results of this analysis, we present the full marginal posterior for the parameters of interest. As is often customary, we also mark the median and the central 95% credible intervals and treat a result as significant if a credible interval excludes the critical value of interest (e.g., 0). These detailed statistics are presented in the figures where they can be readily accessed instead of being buried within the text.

Fig. 4 displays the base drift rate and boundary separation for each decision episode and the memory retrieval. *SI Appendix*, Fig. S1 displays the difference between conditions for these parameters. As expected, drift rate was higher for medium- than for low-coherence trials and for high- compared to medium-coherence trials for the first decision episode. Base drift rate, separate from transfer effects, was also higher for the second decision episode for medium-coherence compared to low-coherence trials and for high-coherence compared to medium-coherence trials. Boundary separation effects went in the opposite direction for the first decision episode, with lower boundaries for medium- versus low-coherence trials and high versus medium trials. The same was true for the second decision episode, although the effects were weaker. Neither drift rate nor boundary separation differed across conditions for the memory retrieval, which again was not surprising given that participants were instructed to try to remember all of their decisions with equal fidelity.

Transfer Effects. Fig. 5*A* displays the boost to drift rate during the second decision episode resulting from 1) the immediately preceding memory retrieval and 2) the choice selected during the first decision episode, that is, a form of implicit memory. Although participants generally had a good recollection of the action they chose during the first decision (accuracy was near 85% in the first dataset and near 80% in the second; Fig. 3*C*), because recall was not perfect we were able to disentangle the effects of decision and explicit memory. The figure shows the effect on drift rate in the direction dictated by each form of memory. For example, if the participant recalled “left” when seeing the word, then drift rate would be biased left by the corresponding amount. Similarly, if the participant selected “left” during the first decision episode, then drift rate would additionally be biased left by that corresponding amount. *SI Appendix*, Fig. S2*A* displays the difference between conditions for these parameters. Explicit memory provided a significant and largely similar boost to drift rate regardless of coherence level, with a small trend toward being larger for low coherence levels. The boost provided by implicit memory was also significant across the board but in contrast was much more clearly graded and larger for higher-coherence trials.

SI Appendix, Fig. S3*A* displays the bias dictated by each type of memory on the starting preference of the decision process. Explicit memory had a statistically significant effect on starting preference at all coherence levels, but effect sizes were relatively small. Implicit memory had no effect on starting preference at any coherence level. In summary, both explicit and implicit memory provided substantial boosts to drift rate in the corresponding directions across all coherence levels, with the effect clearly varying by coherence level for implicit memory but not explicit memory. However, effects on starting preference were small to nonexistent.

Differences in Baseline Decision-Making and Transfer Effects as a Function of OCD Symptoms. Fig. 6 displays the regressors governing the relationship between baseline drift rate and boundary separation and OCD symptom severity for each decision episode and the memory retrieval. *SI Appendix*, Fig. S4 displays the

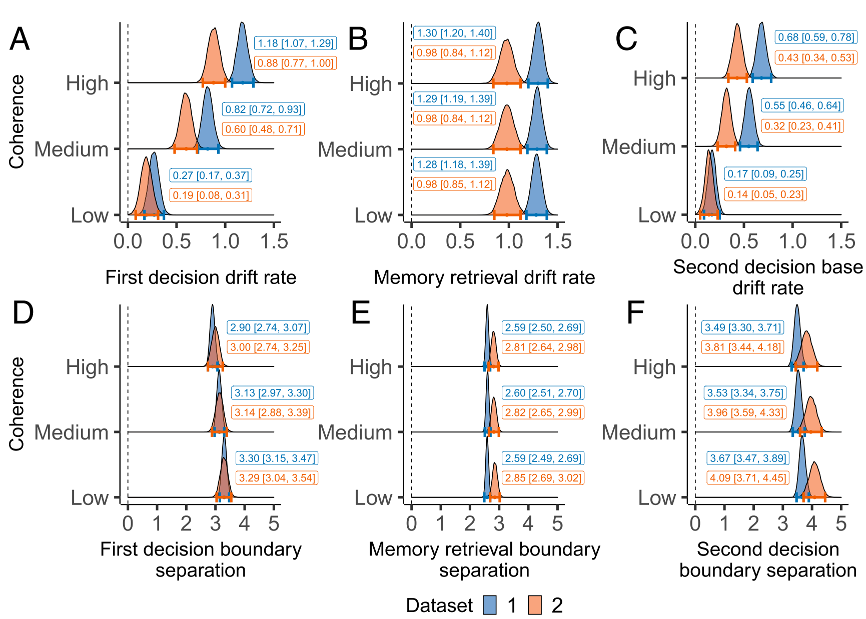

Fig. 4. Marginal posterior distributions of drift rate and boundary separation during the two decision episodes and the memory retrieval for each level of decision difficulty. The dot and bars represent the median and central 95% credible interval. (*A*) Drift rate during the first decision episode. (*B*) Drift rate during the memory retrieval. (*C*) Baseline drift rate during the second decision episode separate from transfer effects. (*D*) Boundary separation during the first decision episode. (*E*) Boundary separation during the memory retrieval. (*F*) Boundary separation during the second decision episode.

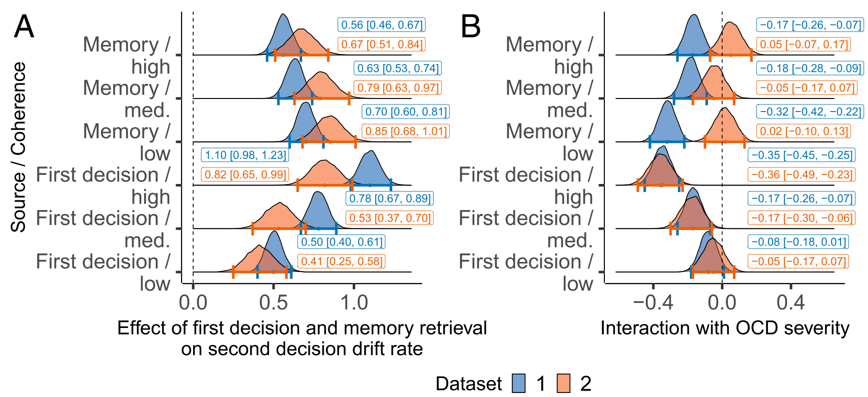

Fig. 5. Marginal posterior distributions of transfer effects driven by changes in drift rate. The dot and bars represent the median and central 95% credible interval. Levels are decision difficulty. (*A*) Bias in drift rate during the second decision episode in the direction of the choice actually selected during the first decision episode and the choice participants recalled making. (*B*) Additional change in the direction of each as a function of OCD symptom severity.

differences between conditions. In both datasets, we replicated previous work showing that drift rate during a new decision (the first decision episode) is reduced as a function of OCD severity, and that this effect is larger for easier trials. Similar deficits were also found for the second decision episode, separate from any transfer effects, although here medium- and high-coherence trials were affected to a similar degree. Deficits in drift rate were also evident for the memory retrieval, equally affecting retrieval across all coherence levels. These results provide a quantitative account of deficits in perceptual decision-making and memory for action as a function of OCD using a common computational mechanism in the same participants. There was evidence that boundary separation for both decision episodes was reduced as a function of OCD severity in the first but not the second dataset. In contrast, there was evidence that boundary separation for the memory retrieval increased slightly as a function of OCD severity in the second but not the first dataset. These results were not surprising given that boundary effects have also been inconsistent in previous work, a point we return to in *Discussion*.

Previous work with the dot motion task and OCD either did not control for anxiety and depression or attempted to equate anxiety and depression between OCD and control groups. Our second dataset, which includes continuous measures of symptoms of OCD and anxiety and depression allowed us to look at the effects of each in the same sample. *SI Appendix*, Fig. S5 displays the relationship between baseline drift rate and boundary separation and symptoms of anxiety and depression, and *SI Appendix*, Fig. S6 displays the relative effects of OCD versus anxiety and depression. There were no consistent effects on drift rate for either decision episode or the memory retrieval. Similar to symptoms of OCD in the first dataset, symptoms of anxiety and depression were associated with decreased boundary separation in both decision episodes across all conditions, potentially explaining why this relationship was evident with OCD when anxiety and depression were not controlled for. The negative effects of OCD on drift rate were significantly larger than the effects of anxiety and depression for both decision episodes and for the memory retrieval across all conditions (*SI Appendix*, Fig. S6).

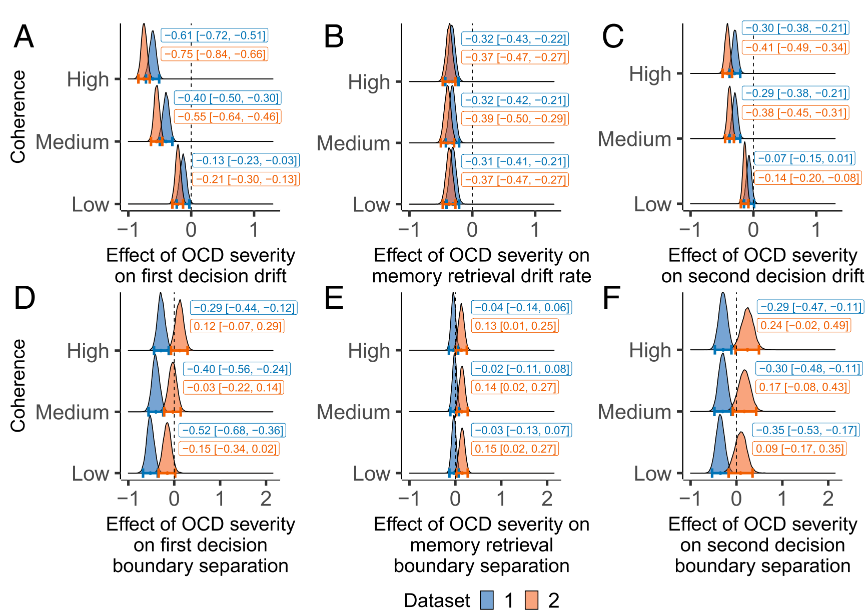

Fig. 6. Marginal posterior distributions of the relationships between OCD symptom severity and drift rate and boundary separation. The dot and bars represent the median and central 95% credible interval. Levels are decision difficulty. (*A*) Drift rate during the first decision episode. (*B*) Drift rate during the memory retrieval. (*C*) Baseline drift rate during the second decision episode separate from transfer effects. (*D*) Boundary separation during the first decision episode. (*E*) Boundary separation during the memory retrieval. (*F*) Boundary separation during the second decision episode.

Fig. 5*B* displays the regressors governing the interaction between OCD severity and the effects of explicit and implicit memory on drift rate during the second decision episode. *SI Appendix*, Fig. S2*B* displays the differences between conditions. The boost to drift rate due to implicit memory was smaller as a function of OCD severity, and similar to baseline effects this reduction was larger for easier conditions. The boost to drift rate due to explicit memory was also smaller as a function of OCD in the first dataset, but this did not replicate in the second dataset. The lack of effect was unchanged when including only OCD in the model and excluding anxiety and depression and other control variables. Thus, this finding was not due to suppression by other variables. *SI Appendix*, Fig. S7 displays the interaction between drift transfer effects and symptoms of anxiety and depression. The interaction was not significant with either memory system in any condition (*SI Appendix*, Fig. S7*A*). The difference between the interaction with implicit memory was larger for OCD compared to anxiety and depression for the high-coherence condition, but the smaller interaction with OCD for the medium-coherence condition was not significantly different from the interaction with anxiety and depression (*SI Appendix*, Fig. S7*B*).

SI Appendix, Fig. S3*B* displays the interaction between OCD severity and the effects of explicit and implicit memory on the starting preference for the second decision episode. There were no consistent interactions across conditions and datasets with either type of memory. *SI Appendix*, Fig. S8 displays the analogous information for symptoms of anxiety and depression, also showing no consistent interactions with either type of memory.

Second decision episodes with reaction times faster than 250 ms were excluded from the model-based analysis under the assumption that such very fast trials did not involve new evidence integration (*Methods*). OCD may be related to a general difference in whether additional information is sought at all. We tested whether the number of excluded trials correlated with OCD severity but found no evidence of such a relationship in either dataset in any condition (*SI Appendix*, Table S2). Finally, the *SI Appendix*, *SI Results* discusses a control analysis for ruling out the effects of attention in information transfer, both in general and in relation to OCD.

Discussion

We developed a task to study the transfer of information across repeated decision episodes. Our goal was to design a task that was simple enough to reliably model even when limited data may be available for each participant but complex enough to study a wide range of features of repeated choice. Participants made perceptual dot motion decisions, judging the average net direction of motion of a noisy array of moving dots. These decisions were tagged with unique words that identified both the choice made and the decision problem (motion coherence and direction). Word cues were subsequently presented a second time, followed by a dot motion stimulus with the same properties. Participants had to both recall their previous choice and make a decision a second time, which they could do with as much or as little additional evidence as they wanted. Using a drift-diffusion framework, we tested several hypotheses related both to general information transfer and whether information transfer and baseline memory and decision-making differed as a function of symptoms of OCD.

We found that both the actual choice made during the first decision episode (implicit memory) and the choice people explicitly remembered making influenced the subsequent decision by biasing drift rate in the corresponding direction. Bias in the starting preference was small for explicit memory and nonexistent for implicit memory by comparison. From a Bayesian perspective, a change in starting preference corresponds to a change in the prior for one option versus the other, while a change in drift rate corresponds to a change in the moment-by-moment log likelihood ratio of the net direction of motion, which is integrated over time during the course of a decision (35–37). Modifying the likelihood to be less reflective of reality certainly seems counterintuitive at first blush. However, this finding is in line with previous work reporting changes in drift rate corresponding to changes in the prior distribution of the experimental stimulus class and the corresponding correct response (38, 39). As a potential explanation for this effect within the context of single decisions, it has been suggested that changes in drift rate that track changes in prior probability may in fact be necessary (though in addition to changes in starting preference) to achieve optimal long-term performance when trial difficulty varies across the experiment (40), although this view has subsequently been challenged (41). Nevertheless, better understanding optimal decision-making in the present context is a direction worth pursuing. This result may also reflect trial-by-trial changes in the drift criterion (38) conveyed by memory of the first decision. Given a particular prior, participants may lower the threshold for the perceptual evidence necessary for it to be classified as corresponding to the prior direction of motion.

The magnitude of the effect of implicit memory differed by decision difficulty, being larger for easier decisions. In contrast, the effects of explicit memory were relatively similar across difficulty levels. Such differences raise an interesting conjecture regarding another aspect of the representational nature of transfer effects: Transfer effects would be expected to vary by difficulty if they were based on a memory of the stimulus itself. In contrast, transfer effects based on a memory of the decision would not. The current design does not allow us to ask directly about which representation is employed by each memory system, but this is an obvious direction for future work.

Turning to individual differences, we also found a number of consistent differences in both baseline memory and decision-making and transfer effects as a function of OCD symptom severity. First, we replicated previous work showing that baseline drift rate (for the first decision episode) is reduced (21–24). A reduction in drift rate was also evident during the second decision episode, independent of any transfer effects. Moreover, a reduction in drift rate was in addition evident during the memory retrieval of the previous decision, demonstrating that similar deficits exist across cognitive domains. Like decision-making, memory retrieval also involves the integration of "evidence," or what might more intuitively be considered signal strength, here for and against different memories or the existence or absence of a memory rather than different options (17, 31, 42). The fact that there is evidence for similar computational deficits across cognitive domains raises the possibility that a common underlying low-level neural mechanism may be at fault. It may relate to the general representation of moment-by-moment evidence, the flow of such evidence to downstream integrators, or the ability of the integrator regions themselves to properly account for the evidence they receive. These deficits were all specific to OCD relative to anxiety and depression.

Potentially speaking to a common mechanism driving deficits, reductions in drift rate as a function of OCD severity were also observed in transfer effects across decision episodes due to implicit memory. A limitation of the current design is that it is not possible to tell whether differences in transfer effects reflect deficits in the transfer process, whether baseline deficits from the first decision episode are carried over, or both. It is tempting to attempt to answer this question by comparing the magnitude of the effect of OCD on transfer effects with the magnitude of the deficits during the first decision. If the former is larger, this might serve as evidence that there are additional deficits that ride on top of the baseline effects. However, lacking a more specific mechanistic model of how transfer works, such a comparison is difficult to interpret. Indeed, the magnitude of the reduction in transfer effects is actually smaller than the deficits during the first decision episode (compare Figs. 5*B* and 6*A*). This does not mean, however, that transferred deficits are somehow "fixed" along the way. Understanding how differences in transfer come about requires a more detailed mechanistic model that can explain how decisions are stored in memory, how they are retrieved, and how they are weighted to influence later decisions.

Differences related to implicit memory transfer were specific to OCD relative to anxiety and depression for the high-coherence condition only, which had the largest OCD effect. This pattern where drift rate effects were larger for easier conditions was a common theme as discussed above, lending additional support for this finding. In contrast, for the medium-coherence condition, although the effect of OCD was significant and the effect of anxiety and depression was not significant the difference between them was not significant. Given that the OCD effect was small to begin with, it remains to be determined whether the lack of specificity for harder trials is due to sampling error or reflective of a mechanistic difference.

The current work also has other limitations, with the experiment lacking at least two features possessed by real-world checking scenarios. First, real mistakes, if they were to occur, could in theory result in more drastic consequences under a worst-case scenario. A stove left on could cause a fire, a door left unlocked could make it easier for a burglar to enter, whereas misjudging the direction of motion of a collection of dots in a computer-based task would at worst keep the participant from earning a point for performance. Relatedly, mistakes in the real-world often result in negative consequences rather than simply the withholding of reward. Although it is not possible to recreate the possibility of similarly devastating effects in an experimental setting, a fruitful avenue for future work is to test how changes in incentive structure, both in the form of highly negative and highly positive (and thus potentially high opportunity cost) outcomes, affect information processing. Second, the current experiment does not allow the participant to freely choose whether to repeat the decision after seeing the cue and is limited to two episodes per decision. These features were chosen purposefully in order to increase power and precision for detecting and quantifying transfer effects, which was the goal of the present work. However, the task can be readily modified to allow participants to choose whether to repeat a trial and to do so multiple times throughout the experiment for each decision. Looking at the interaction between repetitions and incentive manipulations (e.g., trials with potentially highly negative outcomes) would be especially interesting.

Another important avenue of extension is to understand how repeated decision-making interacts with decision and memory confidence and metacognition. Specifically, little is known about how confidence drives the meta-decision about whether or not to revisit a previous decision, and how much information from a previous decision episode to take on board. Two aspects of decision and memory confidence may influence repeated decision-making: one's overall level of confidence and how good one is at correctly mapping confidence to accuracy (i.e., having high confidence in responses that are objectively accurate and low confidence in responses that are inaccurate). Previous studies have suggested that individuals with OCD have reduced confidence in their memory (43, 44) and an impairment in mapping decision confidence in the same dot motion task used here (23). If one does not trust their memory or decision-making ability as a result of either type of deficit, this could lead them to revisit decisions more often and carry over less information from previous decision episodes.

Finally, we found that individuals with more severe OCD symptoms had a smaller boundary separation in the first dataset, but this effect could be explained by symptoms of anxiety and depression and boundary separation had no relationship to OCD in the second dataset. Unlike differences in drift rate, and seemingly counter to the perseverative nature of OCD, the data on OCD-related differences in boundary separation in perceptual decision-making have been mixed, with two studies reporting that patients place decision boundaries further apart (21, 25) and three studies reporting no relationship between boundary separation and OCD status or subclinical severity (22–24). Previous work did not take anxiety and depression into account in this context or attempt to control for them either by excluding patients with comorbidities or by equating anxiety and depression between patient and control groups, both of which can be achieved imperfectly at best. Our data suggest that anxiety and depression may be an important covariate in boundary placement during perceptual decision-making.

Although consistent with prior data, participants with higher OCD severity scores were not more perseverative within individual decisions and they were in a sense more deliberative across decision episodes, transferring less information from the first to the second decision. Ascertaining where one decision episode begins and another ends can be difficult in the wild. For example, an individual stuck checking their stove may be undergoing a single elongated decision, or a series of multiple repeated decisions which are continuously spawned. Our task provides a controlled environment for delineating the components of decision-making that drive deliberation on both short and long time scales, and our data suggest that differences related to the transfer of information between decisions, rather than caution within individual decision episodes, may play an important role in OCD.

To conclude, our task revealed a number of findings regarding how people make repeated decisions, and how both baseline processing and information transfer differs as a function of OCD symptom severity. It also raises a number of new research questions, many of which can be readily pursued using the same basic framework.

Methods

Participants. All experimental procedures were approved by the Institutional Review Board at the University of Maryland. Electronic informed consent was obtained in accordance with the approved procedures. Data were collected on Amazon Mechanical Turk from 212 participants in Experiment 1 and 324 participants in Experiment 2, with the following exclusion criteria applied. First, following the instructions and a practice round, participants completed a multiple-choice quiz to gauge their understanding of the task. Participants that failed to answer the quiz questions were not able to continue and are not included in the above count. Second, the Padua Inventory included a catch question which asked participants to select "A little" for that item. Participants that did not do so were excluded from analysis. Finally, trial sets (both the first and second decisions and the memory retrieval) where the reaction time for the first decision or the memory retrieval was faster than 250 ms or slower than 10 s were removed. Trial sets where the reaction time for the second decision was slower than 10 s were similarly removed. Participants could but did not have to integrate additional evidence during the second decision in order to do well (discussed below and in the main text) and therefore no minimum reaction-time exclusion criterion was applied to the second decision episode. Participants that did not have at least 25 trials remaining for each of the three coherence levels (discussed below) were excluded entirely.

This left 175 participants for analysis in Experiment 1 and 213 participants in Experiment 2. As detailed in the main text, as a basic sanity check we replicated both the basic accuracy, reaction time, and drift rate effects as well as the negative relationship between drift rate and OCD seen in the dot motion task in previous studies (15, 21–24). Although we generally refer to the population we sampled as being "subclinical" throughout, it is of course possible some individuals on the high end of the Padua Inventory may meet clinical criteria.

Repeated Decision-Making Task. Participants completed 62 blocks of trials, each block divided into two parts with three trials in the first half and three paired trials in the second half. The structure of the trials from each half is displayed in Fig. 2. In trials that appeared during the first half, participants

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saw a fixation cross (1,000 ms), followed by a dot motion decision (with no time limit), and finally a word that uniquely identified the trial (2,000 ms). Words were chosen randomly without replacement across the experiment for each participant. The three trials included one of each of 7.5, 20, and 45% coherence levels in random order. The net average direction of motion (left or right) was randomly chosen for each trial with equal probability. The three trials were randomly rearranged and repeated during the second half of the block, but with a different ordering of events within each trial (Fig. 2). Each trial started with the word, and participants had to recall the decision they made on the associated trial in the first half of the block (with no time limit). This was followed by the fixation cross (1,000 ms) and finally the dot motion decision (again with no time limit). The coherence level and direction of motion were the same as on the corresponding trial in the first half of the block. If participants were sure they made the correct decision the first time, and were also sure they correctly remembered their choice, they could respond without processing the stimulus at all during the second decision episode and still do well. The task was programmed using the jsPsych library (45), using the RDK plugin for the dot motion stimulus with 30 dots.

Psychiatric Symptom Measures and Demographic Information. We measured self-reported OCD severity using the Padua Inventory, a widely used psychometric scale (34). In Experiment 2, we tested the specificity of OCD-related effects against symptoms of anxiety and depression measured using the scale derived by Gillan et al. (46) (see also ref. 47). Gillan et al. (46) conducted a factor analysis of a large number of existing questionnaire items that crossed traditional Diagnostic and Statistical Manual boundaries and derived a three-factor symptom structure that included a transdiagnostic compulsive behavior and intrusive thought factor (of which OCD was a part), trait anxiety and depression, and social anxiety. Although our focus in this work was on OCD and not a transdiagnostic OC factor, we used their same anxiety and depression measure to test for the specificity of our effects. Participants completed items that had a factor loading of at least 0.4 in the analysis of Gillan et al. (46). To derive factor scores, we regressed each participant's item scores on the items' factor loadings. In Experiment 2 we also controlled for cognitive ability measured with Form A of the short form of Raven's standard progressive matrices (48), using the Poisson regression model of Bilker et al. (48) to convert the short-form item scores to full scores. Finally, Experiment 2 controlled for sex, age, years of education, and income.

Model Fitting. We fit a Bayesian multilevel (hierarchical) version of the drift-diffusion model (Wiener process with drift) implemented in the statistical programming language Stan (Stan Development Team). Inference was performed via Markov chain Monte Carlo using the No-U-Turn sampler. Proper mixing was assessed both quantitatively, computing the Gelman–Rubin $\hat{R}$ statistic and ensuring it was less than 1.1 for all variables and qualitatively based on traceplots. We ran four chains with 4,000 samples each, using the first 1,000 as warm-up.

For the first decision episode, drift rate for subject s, coherence c, and trial t with z-scored Padua Inventory total $padua_s$ was set as

$$drift^1_{s,c,t} = drift^1_c + drift^1_s + \sigma^1_{drift,trial} \cdot \epsilon^1_t + drift^1_{c,padua} \cdot padua_s.$$

$drift^1_c$, $drift^1_s$, $\sigma^1_{drift,trial}$, and $drift^1_{c,padua}$ were free parameters. ϵ^1_t was a per-trial random variable with a $N(0, 1)$ prior defining the deflection from the subject mean for the current trial, modeling across-trial variability. $padua_s$ was the z-scored Padua Inventory score. The drift rate for the memory retrieval ($drift^r$) was equivalently defined, as was the boundary separation for the first decision episode and the memory retrieval ($bound^1$ and $bound^r$), although the boundaries did not vary across trials. The starting point for both was fixed to be the midpoint between the two boundaries. In Experiment 2, there were additional regressors equivalent to the Padua regressors for anxiety and depression severity, Raven's progressive matrices score, sex (binary variable with 0 = female and 1 = male), age, years of education, and income (all z-scored except sex). For income, 11 out of 213 participants had values that although valid appeared unlikely compared to the rest: greater than 0 but less than \$100 (perhaps mistaking the input field to be in thousands of dollars rather than dollars, but we cannot be sure), \$500,000, and \$750,000. Income for these participants was replaced by the mean income of the remaining participants.

The boundary separation for the second decision episode ($bound^2$) was also defined as above. The drift rate and the starting position for the second decision episode had additional regression terms to account for transfer effects. Rather than fitting the effect of the first decision episode (implicit memory) and the memory retrieval directly, we modeled the effect of a matching trial (a trial where the choice made during first decision episode matched the choice the participant recalled making) and a nonmatching trial and then converted these to effects of choice and memory as described below. This reduced correlation in the posterior that was sampled for more efficient Markov chain Monte Carlo sampling. Specifically, drift rate for the second decision episode was defined as

$$\begin{aligned} drift^2_{s,c,t} = {} & drift^2_c + drift^2_s + \sigma^2_{drift,trial} \cdot \epsilon^2_t + \\ & drift^2_{c,padua} \cdot padua_s + \\ & (drift^2_{c,match} + drift^2_{s,match} + \\ & \quad drift^2_{c,padua,match} \cdot padua_s) \cdot match + \\ & (drift^2_{c,nonmatch} + drift^2_{s,nonmatch} + \\ & \quad drift^2_{c,padua,nonmatch} \cdot padua_s) \cdot nonmatch. \end{aligned}$$

We defined the upper boundary to be the correct response (left or right) on each trial and the lower boundary to be the incorrect response (the choice is arbitrary and the two can be swapped). Further, *match* was set to 0 when the first decision and memory retrieval did not match, 1 when they matched and the first decision was correct, and -1 when they matched and the first decision was incorrect. Likewise, *nonmatch* was set to 0 when the first decision and memory retrieval matched, 1 when they did not match and the first decision was correct, and -1 when they did not match and the first decision was incorrect. The starting preference ($start^2_{s,c,t}$) was equivalently defined but was then also transformed through a logistic function to the 0 (lower boundary) to 1 (upper boundary) range. This allows the starting preference to be defined independent of the boundary separation. Experiment 2 included additional regressors equivalent to the Padua regressors as described above.

We subsequently transformed the match and nonmatch effects to effects of the first decision episode and the memory retrieval as follows, using $drift^2_c$ as an example:

$$drift^2_{c,choice1} = (drift^2_{c,match} + drift^2_{c,nonmatch})/2,$$

and

$$drift^2_{c,memory} = (drift^2_{c,match} - drift^2_{c,nonmatch})/2.$$

This relationship can be more intuitively understood from the opposite direction. $drift^2_{c,match} = drift^2_{c,choice1} + drift^2_{c,memory}$, that is, when there is a match, this is the total amount by which drift rate is biased by both types of memory in the same matching direction. Likewise $drift^2_{c,nonmatch} = drift^2_{c,choice1} - drift^2_{c,memory}$, that is, when there is not a match, this is the amount by which drift rate is biased in the direction of implicit memory after subtracting the effect of explicit memory, which acts in the opposite direction.

For parameters defined for each coherence level (e.g., $drift^1_c$) we used the low-coherence condition as the baseline and fit additive terms (referred to as c' below) for the remaining two conditions. This also reduced correlation in the posterior to aid sampling. We used very broad, weakly informative priors for all parameters. The prior for each of $drift^1_{c=1}$, $drift^1_{c'}$, $drift^1_{c=1,padua}$, $drift^1_{c',padua}$, $drift^r_{c=1}$, $drift^r_{c'}$, $drift^r_{c=1,padua}$, $drift^r_{c',padua}$, $drift^2_{c=1}$, $drift^2_{c'}$, $drift^2_{c=1,padua}$, $drift^2_{c',padua}$, $drift^2_{c=1,match}$, $drift^2_{c',match}$, $drift^2_{c=1,padua,match}$, $drift^2_{c',padua,match}$, $drift^2_{c=1,nonmatch}$, $drift^2_{c',nonmatch}$, $drift^2_{c=1,padua,nonmatch}$, and $drift^2_{c',padua,nonmatch}$ was $N(0, 20)$. The priors for $drift^1_s$, $drift^r_s$, $drift^2_s$, $drift^2_{s,match}$, and $drift^2_{s,nonmatch}$ were hierarchical, $N(0, \sigma^1_{drift})$, $N(0, \sigma^r_{drift})$, $N(0, \sigma^2_{drift})$, $N(0, \sigma^2_{drift,match})$, and $N(0, \sigma^2_{drift,nonmatch})$, respectively, with the prior for each of σ^1_{drift}, σ^r_{drift}, σ^2_{drift}, $\sigma^2_{drift,match}$, and $\sigma^2_{drift,nonmatch}$ set to $N(0, 20)$. The prior for each of $\sigma^1_{drift,trial}$, $\sigma^r_{drift,trial}$, and $\sigma^2_{drift,trial}$ was similarly $N(0, 20)$. Each corresponding parameter for the starting preference for the second decision episode had equivalent priors.

The prior for each of $bound^1_{c'}$, $bound^1_{c=1,padua}$, $bound^1_{c',padua}$, $bound^r_{c'}$, $bound^r_{c=1,padua}$, $bound^r_{c',padua}$, $bound^2_{c'}$, $bound^2_{c=1,padua}$, and $bound^2_{c',padua}$ was similarly $N(0, 20)$. The prior for each of $bound^1_{c=1}$, $bound^r_{c=1}$, and $bound^2_{c=1}$ was $N(1, 20)$, biased positive (because it does not make sense for the boundary separation to be 0), but with a large variance. For the same reason, the latter three parameters were constrained with a lower bound of 0. The prior for $bound^1_s$, $bound^r_s$, and $bound^2_s$ was hierarchical, $N(0, \sigma^1_{bound})$, $N(0, \sigma^r_{bound})$, and $N(0, \sigma^2_{bound})$, respectively, with the prior for each of σ^1_{bound},

σ^r_{bound}, and σ^2_{bound} set to $N(0, 20)$. Priors for all additional regressors in Experiment 2 as described above were equivalent to the priors for the Padua regressors.

The prior for each participant's nondecision time for the first decision and the memory retrieval was hierarchical, $N(\tau^1, \sigma^1_\tau)$ and $N(\tau^r, \sigma^r_\tau)$. The prior for τ^1 and τ^r was $N(0.25, 5)$, and the prior for σ^1_τ and σ^r_τ was set to $N(0, 5)$. The means were biased slightly positive, but all had relatively large variances. A single τ^2 was used for all participants for the second decision's nondecision time with a $N(0.25, 5)$ prior. Fitting a separate nondecision time for each participant resulted in model convergence issues due to pathologies in the shape of the posterior, which were caused by participants' having more similar nondecision times for the second decision episode than the first decision episode or memory retrieval. The nondecision time is constrained by the fastest trials, and because substantial evidence integration was not required during the second decision many more participants had similarly fast trials. The explanation for seeing convergence issues also justifies treating this parameter as fixed across subjects. All τ parameters had a lower bound of 0.

Although as described above we did not exclude trials or whole participants based on whether or not the second decision episode was faster than 0.25 s, we did exclude such decisions from the drift-diffusion model analysis. The assumption here is that such very fast responses involve no new evidence integration and are based strictly on prior knowledge from the first decision episode or reflect lapses in attention. The fits between the model and data are shown in *SI Appendix*, Figs. S9–S14. We also refit both experiments using log-normal (0, 1) priors instead of half-normal priors for all zero-bounded parameters (baseline boundary separation, nondecision time, and all SD parameters). Our results remained unchanged with log-normal priors. Finally, we also conducted a simulation study to test our ability to recover parameter values. We simulated 20 synthetic datasets each with three conditions similar to the real experiment for a range of parameter values guided by the results of the first experiment. We applied the same model fitting procedure as above and plotted the median of the posterior against the actual value used in the simulation for the parameters of interest. The results are displayed in *SI Appendix*, Figs. S15 and S16, which demonstrate excellent parameter recovery ability.

Throughout, an effect was treated as significant if its central 95% credible interval excluded the critical value of interest (e.g., 0).

Statistical Tests. ANOVA was performed using the afex package in R. Nonsphericity was assessed using Mauchly's test, and degrees of freedom and *P* values were corrected using the Greenhouse–Geisser correction where noted. Reported *t* tests were double-sided.

Data Availability. Behavioral data have been deposited on the Open Science Framework (OSF), https://osf.io/7f542/.

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