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Ignoring Memory Hints: The Stubborn Influence of Environmental Cues on Recognition Memory

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Recognition judgments can benefit from the use of environmental cues that signal the general likelihood of encountering familiar versus unfamiliar stimuli. While incorporating such cues is often adaptive, there are circumstances (e.g., eyewitness testimony) in which observers should fully ignore environmental cues in order to preserve memory report fidelity. The current studies used the explicit memory cueing paradigm to examine whether participants could intentionally ignore reliable environmental cues when instructed. Three experiments demonstrated that participants could volitionally dampen the directional influence of environmental cues on their recognition judgments (i.e., whether influenced to respond "old" or "new") but did not fully eliminate their influence. Although monetary incentives diminished the mean influence of cues on responses rates, finer grained individual differences analysis, as well as confidence and RTs analyses, demonstrated that participants were still systematically influenced. These results demonstrate that environmental cues presented at test remain a potent influence on recognition decisions and subjective confidence even when ostensibly ignored.

Keywords: recognition memory, criterion, environmental cues, metamemory

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Memory decisions are not typically made in a vacuum. Instead, they usually occur in the context of environmental cues that signal the general likelihood of familiarity or novelty given the particular context. For instance, when encountering an approaching individual, factors such as location, time of day, or even an accompanying friend's verbal and nonverbal reactions serve as helpful cues as to whether the individual is personally known or unknown. Given this, a judicious observer should integrate this information with his or her recovered memory evidence before deciding about the memory status of an individual or object.

The use of environmental cues during recognition has been examined using the explicit memory cueing (EMC) paradigm (e.g., O'Connor, Han, & Dobbins, 2010; Selmeczy & Dobbins, 2013) during which participants are given probabilistic trial-wise cues indicating the likely status (e.g., likely old or likely new) of each upcoming recognition memory probe. For example, on some recognition trials a participant would be presented with a cue reading "likely old," which would be followed by an old item 75% of the time, and a new item 25% of the time, whereas on other trials s/he would be presented with a cue reading "likely new," which would be followed by a new item 75% of the time, and an old item 25% of the time. When instructed to use the cues to their advantage, observers increase recognition accuracy, relative to uncued recognition performance and a simple signal detection model anticipates this benefit through the appropriate shifting of a decision criterion on a trial-wise basis (see Figure 1). That is, when provided with a 75% valid cue indicating the upcoming recognition probe is likely to be old, an ideal observer should require less direct memory evidence, relative to an uncued condition, to conclude an item is old (i.e., liberal responding) which would be reflected in an increased propensity to identify both old and new items as old. In contrast, when provided with a 75% valid cue indicating the upcoming recognition probe is likely to be new, an optimal observer should increase his or her tendency to identify both old and new materials as new (i.e., conservative responding). Critically, because the cues are correct more often than not (75% vs. 25% of the time) the losses incurred when the cues are invalid are outweighed by the gains achieved when they are valid (for more details, please see Selmeczy & Dobbins, 2013).

In addition to influencing decision biases, generally valid environmental cues should influence participants' subjective confidence in the accuracy of their judgments such that confidence should increase on validly cued trials and decrease during invalidly cued trials. This is because on valid trials the two sources of information (the cue plus the memory signal) are congruent, whereas on invalid trials these two information sources are incongruent or conflicting. Interestingly, this anticipated cue congruence effect on confidence appears to be largely restricted to new/unstudied items (Jaeger, Cox, & Dobbins, 2012) resulting in

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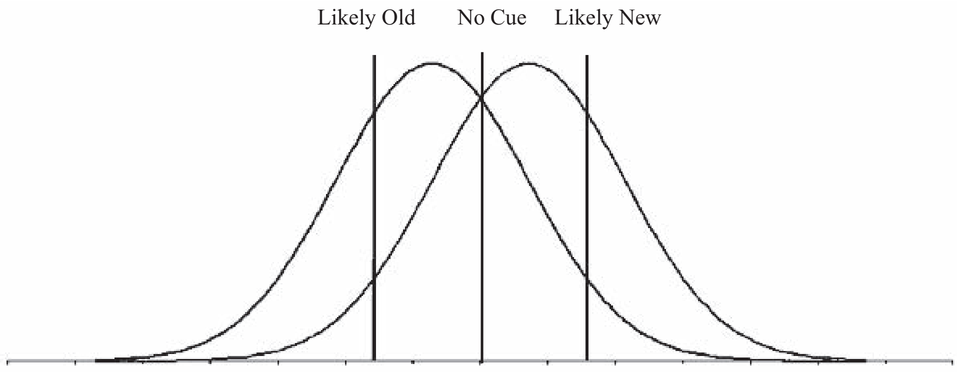

Figure 1. Recognition criterion during explicit memory cueing paradigm. The figure illustrates adaptive criterion shifting as a function of environmental cues under signal detection theory. The *x*-axis represents memory strength using likelihood ratios or the relative odds of encountering an old item (associated with the distribution to the right) versus a new item (associated with the distribution to the left). When no cue is present in the environment decision criteria should optimally be in the center of the overlap of the two item distributions. This would result in equal hit and correct rejection rates. When receiving a 75% reliable likely old cue observers should shift their decision criteria to the left since the cue strongly indicates the upcoming item will be old. This would result in an increase in hit rates and a decrease in correct rejection rates relative to the no cue condition. In contrast, when receiving a 75% reliable likely new cue observers should shift their decision criteria to the right since the cue strongly indicates the upcoming item will be new. This would result in an increase in correct rejection rates and a decrease in hit rates relative to the no cue condition.

greater confidence for correct rejections when validly cued (likely new) than when invalidly cued (likely old). In contrast, the reported confidence for hits, while generally high, appears to be largely insensitive to whether judgments were preceded by valid (likely old) versus invalid (likely new) cues. Jaeger, Cox, and Dobbins (2012) posited that this dissociation occurs because hits can be based both on recollection of context and item familiarity, with recollection dampening the effects of cue-memory congruence on reported confidence (for more details, please see Jaeger et al., 2012).

Thus overall, environmental cues exert a general influence on response rates and also influence the confidence of recognition judgments, at least in the case of correct judgments of new/unstudied materials. Critically, observers often signal the believed accuracy of their memory reports via expressions of confidence, which are often fairly diagnostic of actual recognition accuracy (see Roediger, Wixted, & DeSoto, 2012 for a review) and which also moderate the reliance of others on those recognition reports. For example, in the domain of memory conformity, the confidence of confederates influences the degree of conformity (Schneider & Watkins, 1996; Wright, Self, & Justice, 2000), regardless of whether the confederate was correct or incorrect (Allan & Gabbert, 2008). That is, with all other things being equivalent, we rely more on the memory reports of others when they are expressed more confidently. Metacognitive research has also demonstrated that reports of confidence directly affect behavior such as withholding versus volunteering a memory response (Koriat & Goldsmith, 1996), even when controlling for memory accuracy (Kelley & Sahakyan, 2003). Interestingly, it is not only the case that observers are sensitive to the confidence of others' memory reports, they also appear to use their own metacognitive beliefs to guide how much they rely on the report of another (Allan, Midjord, Martin, & Gabbert, 2012; Gabbert, Memon, & Wright, 2007). For example, using the EMC paradigm, we have demonstrated that the fidelity of observers' self reported confidence in the absence of cues (i.e., their metacognitive awareness) influences the degree to which they benefit from the provision of external cues (Konkel, Selmeczy, & Dobbins, 2015; Selmeczy & Dobbins, 2013). Thus it appears that not only are observers sensitive to the confidence of others' memory reports, they also moderate their reliance on these reports by the degree of confidence they have in their own memory signals.

Keeping Information Sources Separate

The literature reviewed above demonstrates that observers will adjust both the direction (old vs. new) and subjective confidence of their recognition memory reports in light of environmental cues and agents. However, the degree of explicit control that observers exercise over this information integration process is unclear. This question is important for several reasons. First, the utility of environmental cues can rapidly change, as can the goals of the observer. For example, in the case of an external agent, one may learn that that the agent was likely impaired during the putative encoding situation (e.g., was inebriated, did not have his or her glasses or contacts, had an exceptionally poor view, etc.), or is otherwise highly unreliable during the current memorial judgment demand. Thus although this agent's recognition memory may be generally excellent, he or she should be totally disregarded in the current situation. Second, the reliability of an external agent's reports or cues might not be in question but other factors may call for sole reliance on one's own memory evidence regardless of the indications of an external agent. For example, in the case of an eyewitness lineup, an observer should ideally respond solely on the basis of his or her own memory evidence since the fairness of the procedure and integrity of the outcome critically depends on this ability, even if inadvertent (or intentional) cues are present in the environment. Although the legal system presumes information from an eyewitness is independent or unbiased, eyewitness research has demonstrated observers are not always able keep their responses uncontaminated and are influenced by feedback provided after an initial identification (Hasel & Kassin, 2009; Wells & Bradfield, 1998). Moreover, if one is to ignore environmental cues during memory judgment not only should one's selection or decision remain unaffected, but one's confidence should also remain unaffected, because as noted above, the confidence of a recognition report highly influences the degree to which others view it as reliable (Cutler, Penrod, & Dexter, 1990; Wells, Ferguson, & Lindsay, 1981; Whitley & Greenberg, 1986). Finally, the ability to learn about the utility of an external source of information requires that externally provided and internally derived information be kept separate; a requirement linked to what is broadly known as the credit assignment (or blame) problem in reinforcement learning (Sutton & Barto, 1998). Put simply, it would be impossible to ever learn that an external source of information is generally reliable or unreliable if one is unaware of the degree to which each judgment depends on internally derived information versus the information provided by an external source.

Although the above considerations suggest it is useful to keep the relative contributions of external and internal sources of information separate during judgment, it is clear at least in perception that observers sometimes cannot easily isolate the contributions of various cues to the judgment at hand. For example, there are various cues that signal object depth such as binocular disparity, motion parallax, texture gradient, and so forth (Rock, 1983). Researchers have identified these individual cues by selectively manipulating them and showing that these manipulations systematically influence perceived depth (Landy, Maloney, Johnston, & Young, 1995; Rock, 1983). It is very likely that this information is combined quite early in the processing stream since observers are not usually aware of the presence of the separate cues during natural perception and it is unlikely that observers can volitionally ignore each category of cue. Indeed, the fact that individual cues can be manipulated in nonecological ways to induce vivid illusory perceptions rests on the fact that observers cannot selectively ignore such information (i.e., observers cannot will themselves not to see illusions). Thus, to the extent that recognition decisions resemble perceptual judgments, it may be the case that observers find it difficult if not impossible to fully ignore specific environmental cues signaling the recognition status of memory probes.

Cognitive Biases and Recognition Judgment

Even if environmental cues and memorial evidence remain relatively distinct prior to the rendering of recognition judgments, it is still the case that cues might influence the perceived strength of the memorial evidence (or visa versa) because of confirmatory biases (for review, see Nickerson, 1998). Although confirmation bias is often studied in domains where observers have a vested interest in the accuracy of their held beliefs, it also operates during situations in which the beliefs carry no intrinsic value and thus is likely to reflect a general characteristic of human information processing. In the case of the EMC paradigm, the cues typically (although see Experiment 2 below) precede the memory probes and in this sense they set up an expectation or belief regarding the upcoming memoranda. To the extent that memory signals are noisy, multidimensional, or multiprocess information sources (Banks, 2000; Rotello, Macmillan, & Reeder, 2004; Wixted & Mickes, 2010; Yonelinas, 1994) a confirmation bias might result in the incomplete consideration of the available mnemonic evidence or an overweighting of evidence favoring the cued expectation; a possibility consistent with several established item-based memory frameworks that hold that memorial judgments can be fairly inferential in nature (Brown, Lewis, & Monk, 1977; Johnson, Hashtroudi, & Lindsay, 1993; Mandler, 1980; Schacter, 1999). From these perspectives, the perceived significance of memorial signals can be influenced by a host of beliefs and expectations. For example, during recognition testing, participants appear to infer that stimuli that are more easily identified perceptually or linguistically are more likely to have been recently studied than novel (Whittlesea, 1993; Whittlesea & Williams, 2001). That is, they use the fluency of perceptual or linguistic processing as a cue to recognition status, which leads to systematic judgment biases in situations in which these cues are nondiagnostic. These types of effects beg the question as to the degree to which observers are aware and in control of the use of environmental cues during recognition judgment because it is certainly the case that in other decision domains, cues exert a relatively automatic influence.

The Effort (or Lack Thereof) of Shifting Decision Criterion

The above considerations suggest that it may be quite difficult to ignore environmental cues during a recognition memory decision, with these cues affecting either the direction and/or the certainty of the rendered decision. However, the majority of research on recognition memory judgment biases implies they are under considerable volitional control. Indeed, the literature has demonstrated participants can explicitly control their recognition decision biases through instruction and appropriately shift response rates when simply asked to employ a liberal or conservative response strategy (Healy & Jones, 1975; Hirshman, 1995; Miller, Handy, Cutler, Inati, & Wolford, 2001; Postma, 1999). In addition to simply asking participants to adopt particular biases, participants shift decision biases when exposed to various manipulations such as differences in base rates (Estes & Maddox, 1995; Healy & Kubovy, 1978; Heit, Brockdorff, & Lamberts, 2003; Kantner & Lindsay, 2010; Rhodes & Jacoby, 2007; Van Zandt, 2000), performance payouts (Curran, DeBuse, & Leynes, 2007; Healy & Kubovy, 1978; Snodgrass & Corwin, 1988; Van Zandt, 2000), memory strength, (Hirshman, 1995; Singer, 2009; Stretch & Wixted, 1998), and item memorability or distinctiveness (Benjamin & Bawa, 2004; Brown, Steyvers, & Hemmer, 2007; Dobbins & Kroll, 2005). Importantly, the majority of these manipulations assume that observers are able to or should be able to adopt a volitional and explicit strategy to shift their responding in accordance with instructions, information on base rates or payouts, salient characteristics of the materials, and/or strategies discovered via explicit feedback.

However, observers sometimes do not adopt the biases suggested by salient manipulations. For example, Stretch and

Wixted (1998) manipulated memory strength by presenting items either once or 5 times during encoding. During recognition testing, targets and lures were randomly intermixed and salient color cues indicated the potential memory strength of probes (i.e., strong items presented in green, weak items presented in red, and half the lures presented in each color). Ideally, participants would have shifted their decision biases on a trial-wise basis, using a more conservative bias for the color associated with items repeated five times. However, Stretch and Wixted (1998) found that decision biases were identical for strong and weak color cues (Experiment 3) and this was in contrast to the authors' earlier findings where shifts occurred when strength was manipulated between test lists (Experiment 1). These types of results have led researchers to suggest that repeatedly altering the decision criterion throughout a single list may be "difficult or effortful" (Verde & Rotello, 2007, p. 256), require "mental energy" (Stretch & Wixted, 1998, p. 1390), or recruit "controlled executive processes" (Dobbins & Kroll, 2005, p. 1186). In addition, the ability to maintain multiple, consistent decision biases would be expected to be effortful if ensuring consistency across trials demands working memory resources (Benjamin, Diaz, & Wee, 2009; Benjamin, Tullis, & Lee, 2013; although see Konkel et al., 2015).

Critically, if shifting the decision criterion in response to environmental cues is truly effortful and explicitly controlled, then it stands to reason that observers can choose not respond to environmental cues if they deem the effort too high or other experimental demands require ignoring the cues. Thus, to the extent that one views regulating the recognition decision criterion as a strategically controlled, effortful decision process, it seems reasonable to assume that ignoring environmental cues should be within the capability of participants, particularly because prior work has demonstrated that salient cues such color (Stretch & Wixted, 1998) or category membership (Morrell, Gaitan, & Wixted, 2002; Verde & Rotello, 2007) can be ineffective in shifting the decision criterion in a trial-wise fashion.

Summary

The current report focuses on the ability of observers to ignore environmental recognition cues that are known to be predictive using the EMC paradigm. In Experiment 1 participants were simply instructed to use or ignore predictive environmental cues that were presented prior to the recognition probe. To preface our results, Experiment 1 revealed that participants could only dampen but not fully eliminate the influence of environmental cues. We then examined if participants could successfully eliminate the influence of reliable cues when presented after the recognition probe (Experiment 2) or when provided with monetary incentives to ignore cues (Experiment 3). Critically, our results revealed that while participants were able to further reduce the influence of cues, they were never able to fully eliminate their influence from their recognition judgments, subjective confidence or RTs.

Experiment 1

In Experiment 1, we used the EMC paradigm and modified the instructions such that participants were explicitly told to use reliable cues on some study/test blocks and ignore them on others. The goal was to simply assess the degree to which participants could prevent either their reports, or the confidence of those reports, from being influenced by cues through basic instructions.

Method

Participants. Experiment 1 included 30 Washington University students (average age = 19.50, 22 females) who received course credit for their participation. All participants provided informed consent in accordance with the University's Institutional Review Board.

Material and procedure. For all experiments, testing was self-paced with participants entering their responses via keyboard, and presentation and timing controlled via Matlab's Psychophysics Toolbox (version 3.0.; Brainard, 1997; Pelli, 1997). For each participant, words were randomly selected from a 1,216 item pool with an average of 7.09 letters, 2.34 syllables, and log HAL frequency of 7.74.

We used a 2 × 3 within subject design with factors of instruction (use vs. ignore) and cue type (likely old, likely new, vs. uncued). Participants completed four study/test cycles. During study participants performed a self-paced syllable counting task (one, two, three, or more syllables?; 75 studied items). Recognition testing immediately followed with participants indicating whether randomly presented words were previously studied (old) or novel (new). On the majority of trials (60 old, 60 new for each cycle) a probabilistic mnemonic cue (referred to as hints to the participants) reading "likely old" or "likely new" appeared 1 s prior to the recognition probe. During the remaining trials (15 old, 15 new for each cycle) no cue appeared (i.e., uncued or baseline trials). Participants were explicitly informed of the cue reliability (75%) and encouraged to use the cues on the first and third study/test cycles and instructed to completely ignore the cues on the 2nd and 4th study/test cycles. During the use study/test cycles the instructions read, "Since the hints are accurate 75% of the time, you should try and use the hints to increase your performance," while during the ignore study/test cycle the instructions read "Even though the hints are accurate 75% of the time, you should ignore the hints and make your decision solely on your own memory evidence." To ensure participants did not forget which study/test cycle they were in a large static reminder remained on top of the screen throughout the test period (i.e., USE THE HINTS in green font or IGNORE THE HINTS in red font). The experiment order remained fixed such that participants always began with instructions to use cues; this was done to help ensure that participants' initial experience clearly demonstrated that cues were useful and to avoid the possibility, if cues were effectively ignored, that this was the result of participants believing them to in fact be unreliable. After each old/new recognition decision, the cue was removed from the screen and participants provided confidence on a 6-point scale ranging from 50% *(guessing)* to 100% (*certain*) in 10% intervals.

Results

Response rates. Analyses will focus on correct response rates for old and new items (viz., hits and correct rejections), since incorrect responses occurred relatively infrequently during valid cueing (see Table 1 for descriptive statistics). Descriptive statistics of commonly used measures of signal detection theory (e.g., C and d') are also reported in Table 1 and analyses of these measures yield the same conclusions as response rate analysis.[1]

Based on previous research we expected response rates to change as a function of cues when participants were instructed to use cues. Importantly, we wanted to examine whether participants could fully disregard cues during ignore blocks, which would be reflected by a lack of change in correct response rates as a function of cueing. For ease of interpretation we report separate analyses for hits and correct rejections below. In addition, because we were interested in the critical interaction between cues (i.e., likely old vs. likely new) and instruction (use vs. ignore), main effects are only reported when this interaction did not reach significance.

Hits. A 2 × 2 repeated measures analysis of variance (ANOVA) with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on hit rates revealed a significant interaction between instruction and cue type, $F(1, 29) = 17.93$, $\eta_p^2 = .38$, $p < .001$. Post hoc tests (Tukey's honestly significant difference test [HSD]) showed a significant difference between hit rates under likely old versus likely new cues when instructed to use cues (.87 vs. .60, $p < .001$, Cohen's $d = 1.33$). Critically, when instructed to ignore cues there was a smaller but still highly significant and large difference between hit rates under likely old and likely new cues (.80 vs. 0.65, $p < .001$, Cohen's $d = 0.90$).

Correct rejections. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on correct rejection rates revealed a significant interaction between instruction and cue type, $F(1, 29) = 19.59$, $\eta_p^2 = .40$, $p < .001$. Post hoc tests (Tukey's HSD) showed a significant difference between correct rejection rates under likely new versus likely old cues when instructed to use cues (.89 vs. .57, $p < .001$, Cohen's $d = 1.50$). Critically, when instructed to ignore cues there was a smaller but still highly significant and large difference between correct rejection rates under likely new and likely old cues (.82 vs. 0.65, $p < .001$, Cohen's $d = 0.81$).

Overall, the response rate analyses demonstrate that while participants were able to dampen the influence of cues during ignore relative to use instructions, they were not able to fully eliminate their influence.

Confidence. See Table 2 for descriptive statistics for confidence. To simplify our findings, separate analyses for hits and correct rejections are reported below.

Table 1
Experiment 1 Mean Response Rates, Accuracy, and Criterion with Standard Deviations in Parentheses

Instructions	HR	CR	d'	C
Use instructions				
Uncued/baseline	.76 (.15)	.74 (.12)	1.48 (.55)	−.05 (.36)
Cued	.80 (.09)	.81 (.07)	1.79 (.34)	.01 (.22)
Likely old cue	.87 (.10)	.57 (.20)	1.40 (.52)	−.51 (.42)
Likely new cue	.60 (.18)	.89 (.07)	1.63 (.48)	.53 (.42)
Ignore instructions				
Uncued/baseline	.72 (.17)	.77 (.12)	1.51 (.70)	.08 (.39)
Cued	.76 (.14)	.78 (.09)	1.59 (.62)	.02 (.26)
Likely old cue	.80 (.15)	.65 (.20)	1.39 (.75)	−.24 (.45)
Likely new cue	.65 (.18)	.82 (.09)	1.44 (.69)	.28 (.34)

Note. HR = hit rates; CR = correct rejection rates.

Table 2
Experiment 1 Mean Confidence with Standard Deviations in Parentheses

Instructions	Hits	Correct rejections
Use instructions		
Uncued/baseline	85.70 (7.00)	80.40 (8.06)
Likely old cue	85.41 (6.80)	77.08 (8.56)
Likely new cue	85.50 (7.63)	83.01 (8.33)
Ignore instructions		
Uncued/baseline	86.54 (7.16)	81.91 (10.02)
Likely old cue	85.65 (8.61)	78.72 (9.96)
Likely new cue	86.74 (8.15)	81.72 (9.03)

Hits. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on hit confidence revealed no significant main effects or interaction ($ps > .20$, $\eta_p^2 < .06$). Thus, hit confidence was largely unaffected by environmental cues, replicating previous work (Jaeger et al., 2012). This invariance in hit confidence occurs even though the hit rate itself is robustly affected by the cues (see above). Critically, one cannot take the findings as indicating that participants can ignore the cues when rendering confidence during hits, because there was also no effect of the cues on confidence when participants were in fact using them during the use blocks.

Correct rejections. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on correct rejection confidence revealed a significant interaction between instruction and cue type, $F(1, 29) = 12.62$, $\eta_p^2 = .30$, $p = .001$. Post hoc tests (Tukey's HSD) showed that during use instructions correct rejection confidence was higher during likely new versus likely old cues (83.01 vs. 77.08, $p < .001$, Cohen's $d = 0.95$). Critically, during ignore instruction there was a numerically smaller but still highly significant and medium to large effect size of cue influence such that confidence was higher during likely new versus likely old cues (81.72 vs. 78.72, $p < .001$, Cohen's $d = 0.67$). Thus, correct rejection confidence data also suggest that participants were not able to fully eliminate the influence of cues during ignore instructions.

Discussion

Overall, the results from Experiment 1 demonstrate that participants did not completely ignore cues, as measured by both response rates and confidence data, suggesting participants may not be able to fully isolate memory evidence and confidence from environmental information. To more fully evaluate the difficulty observers have in ignoring environmental cues, we next considered whether the ability to do so was improved by presenting the cues after the recognition memory probes such that observers might fully evaluate the memorial evidence before encountering the environmental cue.

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[1] For all experiments, hit rates of 1 and false alarm rates of 0 were corrected with the formulas suggested by Macmillan and Creelman (2005), 1-1/(2N) for hits and 1/2N for false alarms, where N is the number of trials.

Experiment 2

During the previous experiment cues always preceded the memory probe and the cues may have elicited an expectation of upcoming familiarity during likely old cues or an expectation of novelty during likely new cues that perhaps influenced the processing of the probe itself. However, if the cue were to be presented after a recognition judgment has already been made, there can be no expectancy moderated processing of the probes and perhaps the influence of the cues could be more fully eliminated. Thus, if participants have already subjectively reached a decision uninfluenced by any environmental cue, asking them to ignore a cue that later appears just requires maintaining one's original judgment, even if this judgment has only covertly been reached by the time the environmental cue appears. In Experiment 2 we instructed participants to ignore environmental cues that appeared after the recognition probe at varying delays. We predicted that participants should be able to fully ignore cues that were presented after a recognition probe when sufficient time between the probe and cue elapsed for participants to complete a covert judgment. However, perhaps at shorter delays an influence may still be observed because participants do not have enough time to complete a covert judgment.

Method

Participants. Experiment 2 included 27 participants recruited from the Washington University Experimetrix pool (average age = 22.37, 18 females) who were paid $10 per hour for their participation. All participants provided informed consent in accordance with the University's review board.

Materials and procedure. The software and word list used were the same as in the previous experiment.

We used a 2 × 3 within subject design with factors of instruction (use vs. ignore) and cue type (likely old, likely new, vs. uncued). In addition, within the ignore instruction blocks we added a factor of cue lag (short, medium, vs. long).

To overview, participants completed four study/test cycles where the initial study/test cycle instructed participants to use environmental cues that were presented prior to the recognition probe. The remaining study/test cycles instructed participants to ignore environmental cues that were presented after the recognition probe at various delays. To avoid fatigue, participants were encouraged to take a short break between each study/test cycle.

During study participants performed a self-paced syllable counting task (i.e., does this word contain one, two, three, or more syllables?) on a list of serially presented words (78 items). Immediately after study, participants completed a self-paced recognition test where they indicated whether each presented word was old (previously studied) or new (not previously encountered in the experiment; 78 old items, 78 new items). After each recognition decision, the cue was removed from the screen and participants made a confidence rating using a 6-point scale ranging from 50% (*guessing*) to 100% (*certain*) in 10% increments.

During the first test cycle, the majority of test trials were preceded by a 75% reliable cue reading likely old or likely new that appeared one second prior to the recognition probe (88 cued items-44 old, 44 new), whereas the remaining trials were uncued or baseline trials (68 uncued/baseline items: 34 old, 34 new). Participants were explicitly informed about the reliability of the cue. Critically, during the initial study/test cycle participants were instructed to use cues with the following instructions: "Since the cues are accurate 75% of the time, you should try and USE THE CUES to increase your performance." To remind participants of these instructions, the words *USE THE CUES* in green font were presented at the top of the screen during the test cycle. Although we were mainly interested in examining whether participants could ignore cues in the final three cycles, the first study/test cycle had participants use cues to help ensure that participants' initial experience with the cues demonstrated that the cues were in fact reliable indicators. Otherwise any failure to find a cue influence during ignore instructions may have simply resulted from a disbelief they were reliable indicators of memory status.

During the second through fourth test cycles the majority of test trials were followed by a 75% reliable cue reading likely old or likely new, which participants were told to ignore with the following instructions "Even though the cues are accurate 75% of the time, you should IGNORE THE CUES and make your decision solely on your own memory." The words *IGNORE THE CUES* remained on screen in red font throughout the test cycle. Critically, the cues appeared after the recognition probe following a short, medium, or long lag (48 cued items—24 old, 24 new—at each delay), whereas other trials were uncued/baseline trials (12 uncued items—six old, six new). For all participants the shortest delay was fixed at 300 ms to examine the effect of presenting the cue nearly simultaneously to the recognition probe. The remaining cue lags were titrated to each individual and calculated using measured RTs from uncued/baseline trials during the first study/test cycle. A medium delay was calculated by taking the 50th percentile of each participants' uncued/baseline response time distribution (M = 1.73 s, SD = 0.50, range = 0.96–3.38) and the long delay was calculated by taking the 90th percentile of the distribution (M = 4.12 seconds, SD = 2.00, range = 1.46–8.99). These percentiles were chosen based on the assumption that for the 50th percentile participants would have likely rendered a covert old/new judgment for approximately half of the postcued trials, whereas at their 90th percentile they would have likely completed their recognition decision for the majority of postcued trials. After the cue appeared, participants were not allowed to respond until the cue was on screen for one second, which was done to match lexical processing time of the cue between use and ignore test cycles. After the one second processing of the cue, participants could key in their self-paced old/new decision followed by their confidence response.

One potential concern with the current design was that participants may not actually direct their attention to cues that are presented after the probe and thus any absence of cue influence could be interpreted as simply reflecting the failure to even read the cue. To ensure that results would not be due to participants simply not reading cues, occasional catch trials (48 items—24 old, 24 new—split equally across delay conditions) were included that occurred after confidence ratings. During catch trials the prompt "???CUE???" appeared on screen and participants had to indicate as quickly as possible whether the most recent cue, which was no longer on screen, read likely old or likely new. Catch trial responses were made using a different hand and set of keys than old/new responses in order to avoid confusing the two judgments.

Results

Response rates. See Table 3 for descriptive statistics. To simplify findings separate analysis for hits and correct rejections are reported below. The first analysis collapsed across cue lag (short, medium, long) during ignore instructions to more simply compare use versus ignore instruction blocks.

Hits. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) revealed a significant interaction between instruction and cue type, $F(1, 26) = 13.04$, $\eta_p = .33$, $p = .001$. Follow up post hoc tests (Tukey's HSD) showed that during use instructions there was a significant difference between likely old versus likely new hit rates (.91 vs. .62, $p < .001$, Cohen's $d = 1.13$). During ignore instructions, there was a smaller but also significant difference between likely old versus likely new hit rates (.82 vs. .71 $p = .01$, Cohen's $d = 1.08$).

This initial finding that participants remained strongly influenced by cues during ignore instructions demonstrates that the influence of cues was not eliminated when the cue was presented after the recognition probe. However, we predicted that the size of the cue influence should decrease with lag under the ignore instruction, which was examined with a 2 × 3 repeated measured ANOVA with factors of cue type (likely old vs. likely new) and cue lag (short, medium, vs. long). However, the 2-way interaction between cue type and cue lag was not significant, $p < .66$, $\eta_p = .02$, suggesting that the increased cue lag did not diminish the influence of cues under the ignore instructions.

Correct rejections. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) revealed a significant interaction between instruction and cue type, $F(1, 26) = 18.25$, $\eta_p = .41$, $p = .001$. Follow up post hoc tests (Tukey's HSD) showed that during use instructions there was a significant difference between likely new versus likely old correct rejection rates (.87 vs. .52, $p < .001$, Cohen's $d = 1.29$). During ignore instructions, there was a smaller but also significant difference between likely new versus likely old correct rejection rates (.75 vs. .59, $p < .001$, Cohen's $d = 1.22$).

Table 3
Experiment 2 Mean Response Rates, Accuracy, and Criterion with Standard Deviations in Parentheses

Instructions	HR	CR	*d′*	C
Use instructions				
Uncued/baseline	.83 (.09)	.72 (.14)	1.66 (.54)	−.18 (.29)
Cued	.84 (.10)	.78 (.10)	1.96 (.63)	−.13 (.32)
Likely old cue	.91 (.09)	.52 (.27)	1.60 (.79)	−.74 (.56)
Likely new cue	.62 (.26)	.87 (.09)	1.70 (.81)	.39 (.46)
Ignore instructions				
Uncued/baseline	.77 (.20)	.66 (.18)	1.33 (.75)	−.21 (.51)
Short cue delay				
Cued	.79 (.14)	.72 (.14)	1.55 (.59)	−.12 (.41)
Likely old cue	.82 (.13)	.57 (.21)	1.27 (.71)	−.39 (.49)
Likely new cue	.69 (.19)	.77 (.14)	1.43 (.64)	.12 (.48)
Medium cue delay				
Cued	.80 (.14)	.70 (.15)	1.53 (.52)	−.18 (.40)
Likely old cue	.83 (.14)	.59 (.2)	1.34 (.59)	−.42 (.48)
Likely new cue	.71 (.18)	.74 (.15)	1.38 (.63)	.03 (.45)
Long cue delay				
Cued	.80 (.13)	.70 (.13)	1.48 (.57)	−.18 (.35)
Likely old cue	.82 (.13)	.60 (.18)	1.33 (.58)	−.39 (.45)
Likely new cue	.72 (.17)	.73 (.13)	1.34 (.66)	.01 (.39)

Note. HR=hit rates; CR= correct rejection rates.

The effects of cue lag during ignore instructions was examined with a 2 × 3 repeated measured ANOVA with factors of cue type (likely old vs. likely new) and cue lag (short, medium, vs. long). However, the interaction between cue type and cue lag was not significant, $p > .20$, $\eta_p = .06$. Overall, these results clearly demonstrate that participants were not able to ignore the cues when they were presented after the probes and that this inability remained even for cues following the probes well after an internal conclusion regarding the probe's status could have been rendered.

Confidence. For the analyses below, one participant was removed due to reporting 100% confidence for all but two responses; however, excluding this participant did not change the overall findings. See Table 4 for descriptive statistics for confidence.

Hits. Changes in hit confidence were assessed using a 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new). Once again this initial analysis averaged across all three ignore cycles and collapsed across cue lag conditions. The interaction between instruction and cue type was marginally significant, $F(1, 24) = 2.96$, $\eta_p = .11$, $p = .10$. Although the interaction was only marginally significant, post hoc tests (Tukey's HSD) demonstrated that the effect of cue type was more apparent under use instructions (likely old 87.37 vs. likely new 84.60, $p = .02$, Cohen's $d = .49$), whereas under ignore instructions average hit confidence remained nearly identical as function of cue type (likely old 86.84 vs. likely new 86.20, $p = .91$, Cohen's $d = .20$). Thus, hit confidence does not seem to be affected when instructed to ignore cues.

Correct rejections. Turning to correct rejection confidence, the 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) revealed a significant interaction between instruction and cue type, $F(1, 25) = 11.04$, $\eta_p = .31$, $p = .002$. Follow up post hoc tests (Tukey's HSD) showed that during use instructions there was a significant difference between likely new versus likely old correct rejection confidence (80.99 vs. 73.71, $p < .001$, Cohen's $d = 1.22$). During ignore instructions, there was a smaller but also significant difference between likely new versus likely old correct rejection confidence (79.89 vs. 76.04, $p < .001$, Cohen's $d = 0.96$). Thus, in contrast to hit confidence, correct rejection confidence was reliably influenced by cues during ignore instructions.

Critically, to determine whether cue lag had an effect during ignore instructions a 2 × 3 repeated measured ANOVA with factors of cue type (likely old vs. likely new) and cue lag (short, medium, vs. long) was conducted. Results revealed a significant interaction between cue type and cue lag, $F(2, 50) = 3.13$, $\eta_p = .11$, $p = .05$. Critically, follow up simple effects analyses revealed a significant linear trend for cue lag under likely old, $F(1, 25) = 7.60$, $\eta_p = .23$, $p = .01$, but not likely new cues, $F(1, 25) = 0.29$, $\eta_p = .01$, $p = .60$. That is, the influence of cues decreased at longer cue lags and this effect was driven by a decreasing influence of invalid (likely old) cues. Thus, in contrast to the response rate analyses, correct rejection confidence was influenced by cue lag. Furthermore, these results are in line with our predictions such that the influence of cues was larger during short cue lags relative to longer cue lags. However, even at longest cue lag the effect of cues was still significant (likely new 80.73 vs. likely old 78.36,

Table 4
Experiment 2 Mean Confidence with Standard Deviations in Parentheses

Instructions	Hits	Correct rejections
Use instructions		
Uncued/baseline	86.09 (8.57)	77.50 (12.43)
Likely old cue	87.37 (7.46)	73.71 (13.57)
Likely new cue	84.60 (8.14)	80.99 (11.04)
Ignore instructions		
Uncued/baseline	86.19 (9.92)	79.03 (12.33)
Short cue delay		
Likely old cue	86.09 (8.16)	74.03 (13.85)
Likely new cue	85.30 (9.93)	79.68 (10.68)
Medium cue delay		
Likely old cue	86.98 (7.31)	76.56 (12.10)
Likely new cue	86.72 (9.70)	80.00 (10.54)
Long cue delay		
Likely old cue	86.65 (8.30)	77.52 (11.03)
Likely new cue	85.66 (9.33)	80.00 (10.38)

$t(26) = 2.67$, $p = .01$, Cohen's $d = .51$), suggesting that the cue's influence was never fully eliminated from correct rejection confidence.

Discussion

The results from Experiment 2 demonstrated the participants were unable to ignore the influence of cues that were presented after a recognition probe. For response rates, although the ignore instructions yielded smaller effects than the use instructions (which also involved precueing), the influence of the cues was robust and similar at all lags. This influence, particularly at the longest lag, was unexpected because at this lag the participants had considerable time to subjectively decide the status of the recognition probe prior to the cue's presentation (4.12 seconds on average). Furthermore, work in perceptual decision-making has shown that while postdictive cues can have robust influence on participants' responding early on, this influence decreases at longer delays (Bear & Bloom, 2016). In Bear and Bloom (2016) participants simply chose one out of several circles on a screen. After a short, varying delay one circle would turn red and participants indicated whether the red circle was the one they initially chose. Critically, the postdictive colored circle biased participants' responses and this occurred despite participants explicitly indicating they had enough time to make their decision before the circle turned red. However, in contrast to our work, the influence of the postdictive color cue decreased at longer lags. Nonetheless, this work suggests that participants may have difficulty determining when their decision is fully complete and the influence of postdicitive information may occur unconsciously. In our study one possible explanation for the robust postcueing effects is that even though participants intended not to use the cues they nonetheless failed to commit to a conclusion about the memory probe's status before the presentation of the cues, even at the longest lag. It is important to note that this strategy, if occurring, cannot be addressed by having the participants commit an explicit, overt initial decision prior to the cue's appearance, since it would then be trivial for them not to alter the overt judgment upon the arrival of the external cue. Critically, it is possible that a judgment itself is never truly committed until it is acted upon in some overt manner in the sense that information continues to accrue from memory as long as the probe is the focus of attention (Pleskac & Busemeyer, 2010; Van Zandt & Maldonado-Molina, 2004). If so, then observers may be unable to fully distinguish between whether their overt choice, rendered after the cue was delivered, was fully restricted to late accruing memorial evidence or the late arrival of the cue.

Turning to the confidence data, the results largely converge with the prior findings. The confidence in hits was not moderated by cues under the ignore instructions. Although it was modestly influenced under use instructions with precueing, both Experiment 1 and the prior literature demonstrate that hit confidence is generally difficult to moderate via external cueing and so the failure to see effects during ignore instructions cannot be reasonably ascribed to an active process on the part of the participants that is invoked during ignore instructions (since when actively using cues the effects are minimal to nonexistent). In the case of correct rejections, confidence was influenced by cues even during ignore instructions. Critically, although cue lag failed to affect correct rejection rates, cue lag affected correct rejection confidence with cues exerting a smaller influence the longer the probe was presented prior to the cue's appearance. However, even at the longest cue lag correct rejection confidence was still influenced, suggesting that participants were never able to solely isolate subjective confidence assessment to information considered before the lagged cues.

Taken as a whole, the results of Experiments 1 and 2 suggest that it is quite difficult to eliminate the influence of external cues regardless of whether they are presented before or following recognition memory probes. However, in both cases, one could argue that participants are simply insufficiently motivated to ignore the cues because doing so necessarily means they will be somewhat less accurate at the task than when using the cues. To address the potential issue of motivation, in Experiments 3a and 3b we used monetary incentives giving each participant an initial endowment that would be depleted if he or she let themselves be influenced by the cues.

Experiment 3a

Perhaps participants failed to fully ignore cues because they were not sufficiently motivated to do so, since ignoring cues necessarily means sacrificing accuracy gains that could be achieved by using the cues. Although participants followed instructions to ignore cues to some extent, demonstrated by the dampened influence during ignore relative to use instructions, providing additional incentive may motivate participants to fully ignore cues. In Experiment 3a participants were provided with a monetary endowment from which they lost money when their responses were influenced. If our prior results were due to a lack of motivation on the part of the participants, then the influence of cues on response rates and confidence should be much smaller or completely eliminated in the current experiment.

Method

Participants. Experiment 3a included 34 participants recruited from the Washington University Experimetrix pool (average age = 20.56, 22 females) who were given $10 per hour of participation. All participants provided informed consent in accordance with the University's review board. Four participant's data were excluded leaving 30 participants for analyses. One participant was removed due to chance performance during baseline/uncued trials ($d' = 0.04$) suggesting that he or she was not engaged during the tasks. Another participant was removed due to a misinterpretation of ignore instructions and always responding opposite of the cues during the second test cycle. Finally, two additional participants were removed because they were discovered to be visually blocking the cue on screen using their hands during some portion of trials in the ignore instruction condition.

Materials and procedure. The software and word list used were the same as in previous experiments.

We used a 2 × 3 within subject design with factors of instruction (use vs. ignore) and cue type (likely old, likely new, vs. uncued). Participants completed a total of three study/test cycles. During study participants performed a syllable counting task (i.e., does this word contain one, two, three, or more syllables?) on a list of serially presented words (100 items). Immediately after study, participants completed a recognition test where they indicated whether the word presented was old (previously studied) or new (not previously encountered in the experiment; 100 old items, 100 new items). The majority of test trials were preceded by a 75% reliable cue reading likely old or likely new that appeared 1 s prior to the recognition probe (160 cued items—80 old, 80 new), whereas other trials were uncued/baseline trials (40 uncued/baseline items—20 old, 20 new). Participants were explicitly informed about the reliability of the cue using the same instructions as previous experiments. After each recognition decision, the cue was removed from the screen and participants made a confidence rating using a 6-point scale ranging from 50% (*guessing*) to 100% (*certain*) in 10% increments.

During the first test cycle participants were encouraged to use cues with the same instructions as previous experiments and once again the words *USE THE CUES* remained on screen during testing. During the second and third study test cycle participants were encouraged to ignore the cues with the same instructions as previous experiments and the words *IGNORE THE CUES* remained on screen during testing. Critically, to further encourage participants to ignore cues they were incentivized using the following instructions:

> Prior research demonstrates that most people are not able to ignore cues, but some people are able to do so. Your goal is to try as hard as you can to keep your responses uncontaminated by the cue. In order to help motivate you to ignore the cues, we will pay you $5.00 to completely ignore the cues and make your decision purely on your own memory evidence. However, every time you are influenced by the cue and respond differently than you would have relative to uncued trials, you will lose some of this money. We will determine the amount of money you lose by calculating the percent change in your responding on cued trials relative to uncued trials. For example, if your responses change by 20% on cued trials relative to uncued trials, you will lose 20% of your $5.00 (i.e., $1.00), so you would only earn $4.00. This payment is in addition to the one you will earn for your participation time.

After the second test cycle participants were told their percentage of cue influence and how much money they earned out of the $5.00. The percentage of money lost was calculated as the absolute difference between participants' average "old" rate ([hit rate + false alarm rate]/2) under likely old minus likely new cues. This percentage was then subtracted from $5.00. The absolute value of the difference in old rates was taken as opposed to the simple difference to avoid the potential of negative values.

During the third test cycle participants were once again instructed to ignore cues and given another $5.00 endowment with the same instructions as the previous test cycle. At the end of the third test cycle participants were again told their percentage of cue influence and how much money they earned out of the $5.00. After this, participants were told the total additional amount of money they earned through the experiment (up to $10.00) and were paid this amount rounded up to the nearest whole dollar.

Results

Response rates. See Table 5 for descriptive statistics.

Hits. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on hit rates revealed a significant interaction between instruction and cue type, $F(1, 29) = 82.86$, $\eta_p^2 = .74$, $p < .001$. Post hoc tests (Tukey's HSD) showed a significant difference between hit rates under likely old versus likely new cues when instructed to use cues (.90 vs. .59, $p < .001$, Cohen's $d = 1.79$). However, when instructed to ignore cues the difference between hit rates under likely old and likely new cues was not significant (.74 vs. .71, $p = .39$, Cohen's $d = 0.25$) although it was numerically concordant with the direction of cueing. Thus, in contrast to the previous experiments, hit rates were not reliably affected by environmental cues when provided with a monetary incentive to ignore cues.

Correct rejections. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on correct rejection rates revealed a significant interaction between instruction and cue type, $F(1, 29) = 52.68$, $\eta_p^2 = .64$, $p < .001$. Post hoc tests (Tukey's HSD) showed a significant difference between correct rejection rates under likely new versus likely old cues when instructed to use cues (.89 vs. .54, $p < .001$, Cohen's $d = 1.72$). However, when instructed to ignore cues the difference between correct rejection rates under likely new and likely old cues was not significant (.77 vs. .73, $p = .63$, Cohen's $d = 0.28$), although, again, it was numerically concordant with the direction of cueing. Thus, correct rejection rates were not reliably

Table 5

Experiment 3a Mean Response Rates, Accuracy, and Criterion with Standard Deviations in Parentheses

Instructions	HR	CR	d'	C
Use instructions				
Uncued/baseline	.80 (.12)	.75 (.17)	1.69 (.69)	−.07 (.39)
Cued	.82 (.06)	.81 (.09)	1.86 (.45)	.00 (.23)
Likely old cue	.90 (.07)	.54 (.22)	1.52 (.61)	−.60 (.50)
Likely new cue	.59 (.14)	.89 (.08)	1.60 (.58)	.55 (.29)
Ignore instructions				
Uncued/baseline	.78 (.14)	.73 (.14)	1.57 (.76)	−.09 (.37)
Cued	.73 (.10)	.76 (.13)	1.46 (.61)	.08 (.31)
Likely old cue	.74 (.11)	.73 (.14)	1.39 (.64)	.01 (.30)
Likely new cue	.71 (.13)	.77 (.14)	1.45 (.65)	.13 (.38)

Note. HR = hit rates; CR = correct rejection rates.

affected by environmental cues when provided with a monetary incentive to ignore cues.

In contrast to previous experiments, cues did not reliably influence average response rates during ignore instructions in the current experiment, suggesting that perhaps when participants are highly motivated to ignore cues they may be able to largely dampen their influence. Although the influence of cues was absent in average response rates, it was nonetheless the case that the mean effects remained in the same direction during the ignore and use blocks. In the next analyses we examined whether individual differences in cue influence during ignore blocks may be a more powerful or sensitive indicator of cue effects for reasons outlined below.

Individual differences in cue influence. Although the above analysis demonstrated nonsignificant numerical differences in means under ignore instructions, we were concerned that it may not have been the most powerful test of a complete lack of cue influence. To understand why, it is important to consider the possibility that some participants may have chosen to favor discordant responses with respect to the cues so as to limit their apparent influence on their performance; for example, favoring an old response when the cue indicated likely new. Such a strategy might make sense if participants perceived ignoring the cues as challenging; a possibility consistent with our informal observation that two of the participants (who were removed from analysis) attempted to limit cue influences by placing their hands over the computer screens. As an extreme illustration, if half of the participants were strongly negatively influenced by the cues (adopting strong opposite biases) and half the participants were strongly positively biased by the cues (adopting strong consistent biases), an analysis based solely on means would incorrectly suggest the cues had no effect on performance. Thus to more rigorously test for an absence of cue effects, it is necessary to analyze the data in a manner that is insensitive to whether a participant systematically responds in kind or in opposition to the cues under the ignore instructions, since either constitutes failing to ignore the cues. We achieved this by considering the data at the individual differences level as outlined below.

By definition, fully ignoring environmental cues means that the response rates under the likely old and likely new cue conditions only differ based on random fluctuations for both old and for new test probes. In other words, one would be just as likely to observe a numerically higher hit rate under the likely new cue than under the likely old cue, as the reverse pattern. The differences in response rates under the two cues would simply reflect random variation around the zero difference point. Moreover, this means that within any given participant, the difference between the hit rates under the two cue conditions, would be wholly unrelated to the difference between the correct rejection rates under the two cue conditions. In contrast, if participants are systematically influenced by the cues, the difference in hit rates under the two cue conditions will be matched by an analogous difference in the correct rejection rates under the two cue conditions. Moreover, this correspondence would hold regardless of whether the participant was responding in kind or in opposition to the external cues (both representing failures to effectively ignore the external cues). To test this possibility, for each participant we calculated the difference in hit rates under the likely old and likely new cues, and the analogous difference in correct rejection rates under the likely old and likely new cues. The correlation between these two shift scores was then computed.

Results revealed that under use instructions, the cue shift scores for hits and correct rejections were strongly correlated, $r = .65$, $t(28) = 4.56$, $p < .001$. Critically, under ignore instructions the shift score correlation was smaller in magnitude but still remained a significant and a medium effect, $r = .42$, $t(28) = 2.44$, $p = .02$ (See Figure 2). We also found that this relationship was significant under ignore instructions in our previous experiments (Experiment 1: $r = .75$, $t(28) = 5.93$, $p<.001$; Experiment 2: $r = .58$, $t(25) = 3.57$, $p = .001$), demonstrating that this relationship is not unique to the current experiment. However, note in Figure 2 that in Experiments 1 and 2 nearly all participants shift scores were positive and thus concordant with the cues. In contrast, in Experiment 3a several participants had negative shift scores under ignore instructions, suggesting that some participants were countermanding the cues and this explains the weakening of the cue effects at the level of means. Importantly, the individual differences analyses demonstrate that shifts in the hit rates in response to cues are matched by analogous shifts in the correct rejection rates to the same cues; an outcome that could not occur if the cues had no systematic influence over responding under ignore instructions. Next we consider potential cue effects on confidence.

Confidence. See Table 6 for descriptive statistics for confidence.

Hits. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on hit confidence revealed a main effect of instruction, $F(1, 29) = 4.79$, $\eta_p^2 = .14$ $p = .04$, with slightly higher confidence during ignore versus use instructions (87.90 vs. 85.88). In addition, there was a main effect of cue type, $F(1, 29) = 10.20$, $\eta_p^2 = .26$, $p = .003$, with higher confidence during likely old versus likely new cues (87.76 vs. 86.03). The interaction between instruction and cue type was not significant $p > .97$, $\eta_p^2 = .00$. Thus, hit confidence was similarly affected by cues for both use and ignore instructions.

Correct rejections. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on correct rejection confidence revealed a significant interaction between instruction and cue type, type $F(1, 29) = 16.75$, $\eta_p^2 = .37$, $p < .001$. Post hoc tests (Tukey's HSD) showed that during use instructions correct rejection confidence was higher during likely new versus likely old cues (82.15 vs. 73.62, $p < .001$, Cohen's $d = .95$). Critically, during ignore instruction there was a numerically smaller but still significant and medium effect size of cue influence such that confidence was higher during likely new versus likely old cues (81.97 vs. 79.22, $p = .05$, Cohen's $d = 0.60$).

Discussion

Overall, when a monetary incentive was provided to ignore reliable cues, the influence of cues on mean response rates was further dampened relative to previous experiments and in fact the mean differences were not reliably influenced by cues (although they were numerically concordant with the cues). However, individual difference analysis suggests that a clear influence of cues remained because cue induced differences in hit rates were consistently matched by analogous cue induced differences in correct rejection rates. This correspondence could not arise if the cues

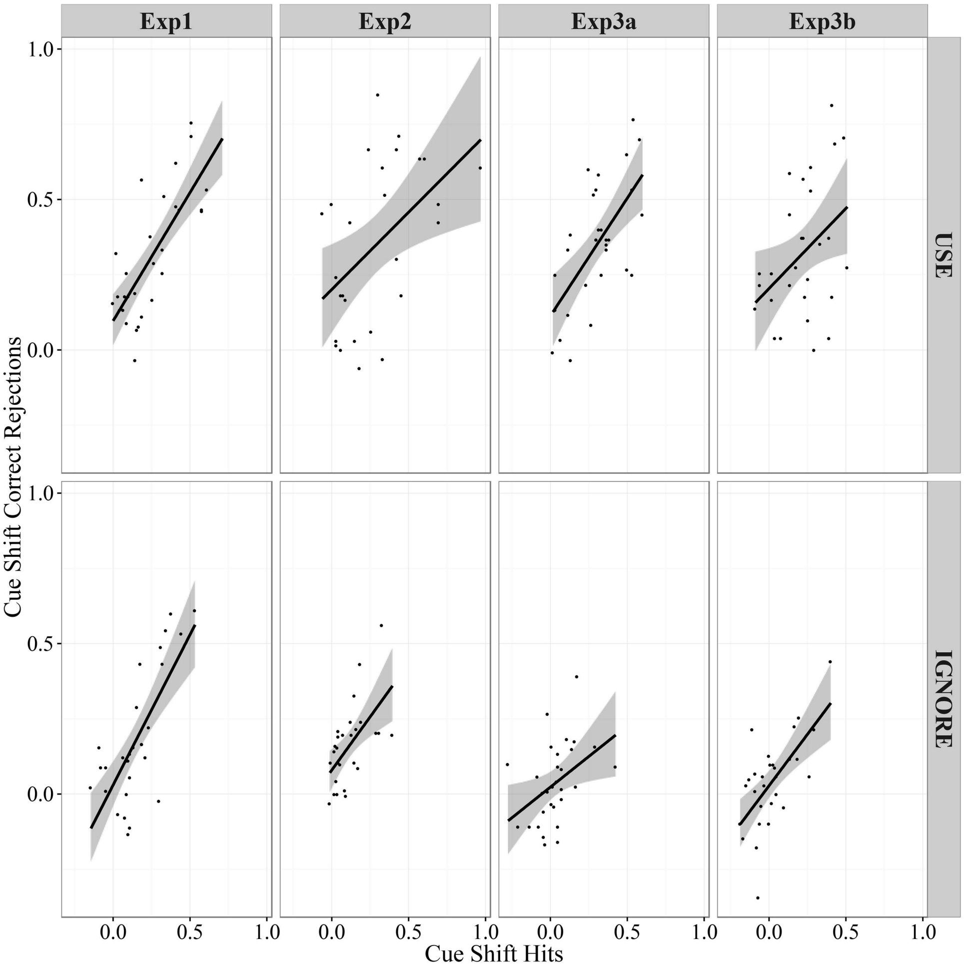

Figure 2. Individual differences in cue shifts for all experiments. The graph depicts the relationship between cue shifts for hits (likely old hit rate–likely new hit rate on *x*-axis) and correct rejections (likely new correct rejection rate–likely old correct rejection rate on *y*-axis) under use (top panels) and ignore (bottom panels) instructions. Shaded regions represent 95% confidence interval around the regression line.

themselves were having no influence under ignore instructions. Thus, although the provision of monetary incentive served to lower the average size of the cue influence under ignore instructions (shifting it closer to a net zero), the pattern of individual differences nonetheless demonstrates that reliable systematic effects remained in the response rates. Turning to confidence, the influence of cues was easily observable in the average correct rejection as well as hit confidence, with the correct rejection effect size (Cohen's $d = 0.60$) being only slightly smaller than in previous Experiments (Experiment 1 Cohen's $d = .67$; Experiment 2 Cohen's $d = .96$). In short then, while the motivation manipulation may have lessened (compared to Experiments 1 and 2) the effects on average response rates, the remaining analyses converged in demonstrating that the cues still exerted systematic effects during ignore instructions on both response rates and confidence.

Interestingly, as noted above, two participants were removed from the analyses because they tried to prevent themselves from reading the cue by covering a portion of the screen using their hands; a behavior not noticed in previous experiments. This suggests that participants may have realized the cues were difficult to ignore after reading them and it is possible that other participants may have also adopted less extreme but similar strategies to try and prevent themselves from reading the cues. Therefore, it could be the case that the monetary incentive did not diminish the

Table 6
Experiment 3a Mean Confidence with Standard Deviations in Parentheses

Instructions	Hits	Correct rejections
Use instructions		
Uncued/baseline	86.4 (9.35)	77.45 (10.09)
Likely old cue	86.73 (7.51)	73.62 (12.33)
Likely new cue	85.04 (8.55)	82.15 (9.90)
Ignore instructions		
Uncued/baseline	88.73 (9.70)	82.57 (11.08)
Likely old cue	88.78 (9.16)	79.22 (12.93)
Likely new cue	87.03 (10.06)	81.97 (10.98)

influence of cues on decision making per se, but instead motivated participants to adopt various strategies that could help them avoid reading the cue (e.g., looking at a different portion of the screen). To rule out this confound, in Experiment 3b participants were occasionally prompted to indicate the status of the previous cue, likely old or likely new, followed by a small punitive timeout period if they failed to respond correctly.

Experiment 3b

Experiment 3b was largely identical to Experiment 3a with the exception of catch trials that encouraged participants to read the cues. Catch trials occurred after confidence ratings and participants were prompted to indicate the status of the cue (which was no longer on screen), with a slow or incorrect response resulting in a time-out period.

Method

Participants. Experiment 3b included 30 participants recruited from the Washington University Experimetrix pool (average age = 20.73, 17 females) who were given $10 per hour of participation. All participants provided informed consent in accordance with the University's review board. One participant was removed due to chance performance during baseline/uncued trials ($d' = 0.06$) suggesting that he or she was not engaged during the tasks.

Materials and procedure. The software and word list used were the same as in previous experiments.

The procedure was identical to Experiment 3a except for the following changes. The trial count for each study/test cycles was decreased to shorten the length of the experiment such that 83 items were presented during encoding and 166 items during test (83 old, 83 new). Of these test trials, 136 (68 old, 68 new) were cued and 30 (15 old, 15 new) were baseline/uncued trials. Critically, during the ignore text cycles, catch trials were added to 40 of the cued trials. During catch trials the word *???CUE???* appeared on screen and participants had to indicate as quickly as possible whether the most recent cue, which was no longer on screen, read likely old or likely new. Catch trial responses were made using a different hand and set of keys than old/new responses to avoid confusing the two judgments. If participants took longer than 1.25 s to respond or they responded incorrectly during catch trials, a time out would occur with the words *incorrect* and/or *too slow* flashing on the screen for 8 s before participants could continue to the next trial. This timeout was included as a way to further motivate reading of the cues since participants would presumably want to avoid this minor negative consequence.

Results

Catch trial accuracy. Catch trial performance was well above chance, $M = 87\%$, $SD = 0.07$, $t(28) = 28.60$, $p < .001$, suggesting that participants attended to the cues during ignore instructions.

Response rates. See Table 7 for descriptive statistics.

Table 7
Experiment 3b Mean Response Rates, Accuracy, and Criterion with Standard Deviations in Parentheses

Instructions	HR	CR	d'	C
Use instructions				
Uncued/baseline	.80 (.15)	.76 (.16)	1.75 (.64)	−.08 (.44)
Cued	.83 (.09)	.80 (.08)	1.89 (.44)	−.05 (.26)
Likely old cue	.88 (.09)	.56 (.22)	1.53 (.71)	−.58 (.48)
Likely new cue	.66 (.16)	.88 (.07)	1.75 (.63)	.39 (.35)
Ignore instructions				
Uncued/baseline	.80 (.15)	.73 (.15)	1.66 (.76)	−.15 (.43)
Cued	.80 (.12)	.74 (.12)	1.60 (.63)	−.10 (.30)
Likely old cue	.80 (.13)	.71 (.13)	1.53 (.70)	−.16 (.31)
Likely new cue	.78 (.16)	.75 (.14)	1.63 (.62)	−.05 (.43)

Note. HR = hit rates; CR = correct rejection rates.

Hits. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on hit rates revealed a significant interaction between instruction and cue type, $F(1, 28) = 36.87$, $\eta_p^2 = .57$, $p < .001$. Post hoc tests (Tukey's HSD) showed a significant difference in hit rates between likely old versus likely new cues when instructed to use cues (.88 vs. .66, $p < .001$, Cohen's $d = 1.32$). However, when instructed to ignore cues the difference in hit rates between likely old versus likely new cues was not significant (.80 vs. .78, $p = .71$, Cohen's $d = 0.17$) although it was numerically concordant with the cues. Thus, replicating Experiment 3a these results suggest that when provided with a monetary incentive to ignore cues, average hit rates were not reliably affected by environmental cues, even when forcing participants to read the cues.

Correct rejections. A 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) on correct rejection rates revealed a significant interaction between instruction and cue type, $F(1, 28) = 28.39$, $\eta_p^2 = .50$, $p < .001$. Post hoc tests (Tukey's HSD) showed a significant difference between correct rejection rates under likely new versus likely old cues when instructed to use cues (.88 vs. .56, $p < .001$, Cohen's $d = 1.46$). However, when instructed to ignore cues the difference between correct rejection rates under likely new versus likely old cues was not significant (.75 vs. .71, $p = .62$, Cohen's $d = 0.29$) although again, the means were numerically concordant with the cues. Thus, average correct rejection rates were not reliably affected by environmental cues when provided with a monetary incentive to ignore cues.

Individual differences in cue influence. Although average response rates were not reliably influenced by cues, we once again examined individual differences by calculating a cue shift scores for both hits and correct rejections (i.e., likely old hit rate–likely new hit rate; likely new correct rejection rate–likely old correct rejection rate) under both use and ignore instructions (see Figure 2). Under use instructions, the cue shift scores between hits and correct rejections were significantly correlated, $r = .40$, $t(27) = 2.29$, $p = .03$. Critically, under ignore instructions the cue shift correlation was significant and large, $r = .65$, $t(27) = 4.49$, $p < .001$. Thus, replicating Experiment 3a, individual difference analyses suggest the cues were not effectively ignored since participants demonstrated a robust correspondence between cue-induced shift scores for old and new materials. Furthermore, in the current experiment the correlation under ignore instructions was similar in magnitude to that of Experiments 1 ($r = .75$) and 2 ($r = .58$), during which average response rate shifts were also significant. Additionally, as observed in Experiment 3a, several participants had negative shift scores, suggesting they were responding in opposition to the cues when attempting to ignore them. Thus, although average response rates were not significantly influenced, the individual difference approach again captures the correspondence between shift scores regardless of direction, and demonstrates that an influence of cues remained during ignore instructions.

Confidence. See Table 8 for descriptive statistics.

Hits. Changes in hit confidence were assessed using a 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new). Results revealed a main effect of instruction, $F(1, 28) = 04.46$, $p = .04$, $\eta_p = .14$, with higher hit confidence during ignore relative to use instructions (88.03 vs. 90.41). The main effect of cue type was not significant, $F(1, 28) = 2.59$, $p = .12$, $\eta_p = .08$, and the interaction between instruction and cue type was marginally significant, $F(1, 28) = 3.50$, $p = .07$, $\eta_p = .11$. Although the interaction was only marginally significant, post hoc tests (Tukey's HSD) demonstrated that the effect of cue type was more apparent under ignore instructions (likely old 91.67 vs. likely new 89.14, $p = .08$, Cohen's $d = .80$), whereas under use instructions hit confidence remained nearly identical as function of cue type (likely old 87.97 vs. likely new 88.09, $p = .99$, Cohen's $d = .02$). Although it is surprising that hit confidence was numerically affected for ignore instructions and not use instructions, this effect did not reach statistical significance and was not observed in previous experiments.

Correct rejections. Turning to correct rejection confidence, the 2 × 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (likely old vs. likely new) revealed a significant interaction, $F(1, 28) = 5.174$, $p = .03$, $\eta_p = .16$. Follow up post hoc tests (Tukey's HSD) showed that during use instructions there was a significant difference between likely new versus likely old correct rejection confidence (83.80 vs. 75.36, $p < .001$, Cohen's $d = 1.05$). Critically, during ignore instructions there was a smaller but also significant and large effect of cues such that correct rejection confidence was higher for likely new versus likely old cues (86.55 vs. 81.69 $p < .001$, Cohen's $d = 1.03$). Thus, replicating Experiment 3a, a significant influence of cues remained in correct rejection confidence during ignore instructions.

Discussion

Overall the results of Experiment 3b replicated those of Experiment 3a. Although average response rates were not significantly influenced by cues during ignore instructions, the individual differences analysis nonetheless demonstrated that the cues continued to exert a systematic effect on judgment tendencies. In the case of response confidence, the influence of the cues was easily observable in the averaged confidence data. Thus even with monetary motivation, and while ensuring the cues were read, cue influences remained detectible in the data during ignore instructions. However, before turning to the General Discussion we first examine an additional measurement model that may be able to further capture the differential influence of the cues during use versus ignore instructions.

Table 8
Experiment 3b Mean Confidence with Standard Deviations in Parentheses

Instructions	Hits	Correct rejections
Use instructions		
Uncued/baseline	89.14 (7.56)	81.21 (10.34)
Likely old cue	87.97 (7.00)	75.36 (11.76)
Likely new cue	88.09 (8.80)	83.80 (8.14)
Ignore instructions		
Uncued/baseline	91.58 (7.37)	84.95 (9.24)
Likely old cue	91.67 (7.14)	81.69 (9.81)
Likely new cue	89.15 (8.41)	86.55 (8.35)

A drift diffusion model of cue influence. One potential limitation of examining response rates in the current Experiment is that the standard signal detection model described in Figure 1 is incapable of isolating cue influences that shift the decision criterion from those that may generally alter the interpretation or registration of memory evidence (i.e., joint distribution shifts). More concretely, a shift of say .25 standard deviations to the right of both evidence distributions in Figure 1 will produce the same change in response rates (hits and false alarms) as a shift of the decision criterion .25 standard deviations to the left; hence both will lead to the same calculated decision bias. However, these are very likely to be different cognitive or perceptual phenomena and so labeling both as a decision bias may be inappropriate (for a related discussion see Wixted & Stretch, 2000). To address this issue we consider a decision model potentially capable of isolating two different types of biasing influences known as the drift diffusion model (DDM; Ratcliff, 1978; A. Voss & Voss, 2007; A. Voss, Nagler, & Lerche, 2013). This model is one of several sequential sampling models that jointly model both response rates and reaction time (RT) effects during simple choice tasks and one primary motivation for their development was the modeling of speed/accuracy tradeoffs during judgment (Voss et al., 2013). For the current study the key benefit of the DDM is that this measurement model offers the possibility of isolating biases that reflect response strategies from those that reflect perceptual or interpretive phenomena.

Under the DDM choices are based on a noisy evidence variable that accumulates over time toward one of two decision bounds, in this case old or new recognition judgments (see Figure 3a). The average speed of this accumulation is referred to as the drift rate (v) and the degree to which the two item classes tend to evoke high drift rates toward the appropriate bound indicates the accuracy of the participant (measured as the average rate toward both bounds). It is the variability of the drift within and across the trials that generates errors and is responsible for the typically right skewed RT distributions during simple choice tasks (for both correct and incorrect judgments). The second key parameter of the model is boundary separation (a), which governs how much evidence must accumulate before a decision is made. This parameter is typically conceptualized as representing observer caution and its modulation is how speed/accuracy tradeoffs are accommodated. In Figure 3a the starting point of accumulation (zr) is shown as unbiased. That is, evidence accumulation begins halfway between the two judgment boundaries. Freeing this parameter to move closer to one or the other bound is one manner of modeling a judgment bias. For example, if starting point is shifted upward in Figure 3b, then old judgments will begin to dominate. Thus in Figure 3b one can see that the shifting the starting point upward or downward will speed

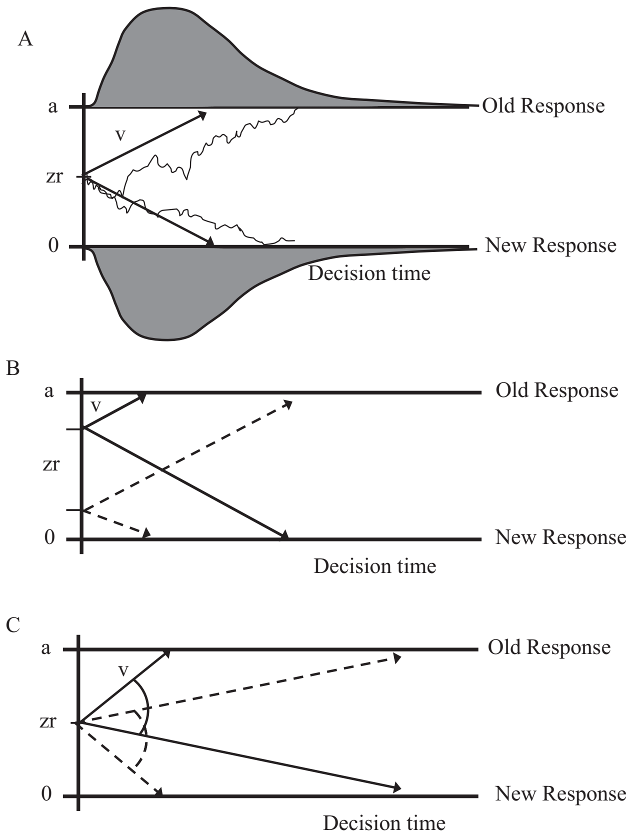

Figure 3. Drift diffusion model. Panel A represents a drift diffusion model with the following parameters: (v) drift rate, (a) boundary separation, and (zr) starting point of accumulation, where the starting point is set in an unbiased location. The noisy thin lines represent single trial examples of evidence accumulating toward each response category. For each panel, the positively sloped arrows are the average drift rate for old items, and the negatively sloped arrows are the average drift rate for new items. Panel B demonstrates the effects of changing the start point to be biased toward an old response (solid lines) or new response (dashed lines), such that only the starting point parameter changes while the slope of drift rates does not change. Panel C demonstrates changes in the drift criterion (dc) toward an old response (solid lines) or new response (dashed lines), such that average drift rates increase for old items (positive drift rates) and new items (negative drift rates), but the change in drift rates is equivalent for both item types (i.e., the angle between the slopes remains constant).

and increase one type of judgment relative to the other. Nonetheless, the angle between the drift rates for old and new materials remain identical under the two biasing conditions when a start point shift occurs. Manipulations of base rates in which the observer is informed that one versus the other class of stimulus is far more frequent, are thought to influence the start point of accumulation (Criss, 2010).

A second, and far less studied potential bias influence occurs when the drift rates for both item classes are generally increased toward one or the other decision boundary, even though the angle between them remains roughly constant. This so-called change in drift-criterion (dc) is illustrated in Figure 3c (see also Leite & Ratcliff, 2011 for more information). Conceptually this is thought to reflect a change in the way evidence is interpreted such that the zero point of what is considered evidence in favor of one versus the other conclusion is shifted. For example, a manipulation might result in a change such that fluent identification or perception is emphasized as a signal of familiarity and critically, this interpretive change would apply to all presented items. Graphically this would mean the drift rates for both old and new items would be similarly angled or tilted upward toward the old decision bound. An analogous way to think about this is that the average drift rate, collapsed across both item classes, shifts upward for a liberal bias whereas the average drift rate for both item classes drifts downward for a conservative bias. For example, White and Poldrack (2014) manipulated both the relative proportion of items classes in mini blocks of trials (e.g., most items in the mini block would be old or studied) and also manipulated the strength or veracity that participants should demand for one versus the other conclusion (e.g., only respond old only if a strong memory signal is perceived). They referred to these as response and stimulus biases respectively, and accordingly the former had a stronger influence on the starting point estimate and the latter on the measured drift criterion. Despite this finding, given the lack of direct comparisons of start point and drift criterion effects in the extant literature, and the lack of clear double dissociations between the two, it is perhaps best to reserve judgment on their psychological separability.

Here we use the DDM as a measurement tool that is at least capable in principle of isolating two different decision biases that arise for different reasons; one perhaps anticipatory (starting point) and the other interpretive (drift criterion) and we consider whether the model's parameters might be sensitive to the distinction between controlled and uncontrolled biases as a function of use or ignore instructions. Thus, we fit the model to Experiments 3a and 3b to see if a) it readily demonstrated that prominent residual cue influences remained under ignore instructions (despite only very small influences on the average responses rates), and b) the parameters of the model suggested a dissociation between starting point and drift-criterion effects when transitioning from use to ignore conditions. When using the model in this manner the goal is not model comparison via fit indices. Instead, one is assuming the model to be correct and then asking whether under this assumed model, an experimental dissociation is confined to or reflected in specific model parameters. This is akin to using the signal detection model and asking whether, given this model, an experimental manipulation yields bias or accuracy differences; a question that necessarily presumes the model is appropriate. Nonetheless, below we do use the fit indices to verify that observed parameter dissociations are not systematically linked with quality of fit across participants.

DDM parameters. To fit the DDM model we used the fast-dm algorithm developed by Voss and colleagues (Voss et al., 2013; Voss & Voss, 2007). The upper response boundary reflected old judgments and the lower response boundary reflected new judgments, and use and ignore blocks were jointly modeled for each participant. Boundary separation (a) was assumed to potentially vary across instruction (use vs. ignore). The drift rates (v) were assumed to vary across the combination of instruction (use or ignore), cue (likely old, likely new, or uncued), and item type (old or new probe). In this parameterization, general changes in the average drift rate of old and new items under the cue conditions would reflect a bias in the drift criterion analogous to Figure 3c. For example, if the drift rates for both old and new items tilted upward for the likely old cue, whereas they both tilted downward for the likely new cue, this would indicate that the cues altered the drift criterion. In contrast, starting point (zr) was assumed to vary across the combination of instruction and cue, but not item type. Restricting starting point to not reflect differences in item type simply reflects the fact that the beginning of evidence accumulation cannot be logically influenced by the items themselves because doing so would mean that an effect temporally preceded its cause. Returning again to Figure 3b, the likely old cue may shift the start point upward toward the old response boundary whereas the likely new cue may shift the start point downward toward the new response boundary. The nondecision time parameter (t0), which is thought to capture things such as lexical access and motor response execution, was fixed across all the above conditions. Finally, the model also incorporated parameters for intertrial variability in starting point (szr), drift rate (sv), and nondecision time (st0). Individual participants were fit using the Kolomogorov-Smirnov objective function in fast-dm and the obtained parameters were used as dependent variables below focusing on the two active cue conditions (likely old and likely new) for simplicity. The key question is whether start point or drift criterion effects are similarly present under use versus ignore instructions. We begin with Experiment 3a and then consider Experiment 3b.

Focusing first on starting point, Figure 4a demonstrates that the likely old and likely new cues robustly influenced the start point under both use, $t(33) = 6.54$, $p < .001$, Cohen's $d = 1.22$ and ignore, $t(33) = 4.04$, $p < .001$, Cohen's $d = 0.69$, instructions. However, the cue influence was somewhat smaller during the ignore instruction resulting in a modest interaction between instruction and cue type, $F(1, 33) = 4.05$, $\eta_p = .11$, $p = .05$, that approached conventional significance. Thus, this particular DDM parameterization shows that prominent cueing effects remain present in the starting point even during ignore instructions.

Turning to the drift rates in Figure 4b and 4c there are clearly robust cue effects. The easiest way to assess these is to contrast the average drift rate for new and old materials under the likely old versus likely new cues. If there is a difference, then it means that the drift criterion has been influenced by the cue condition. Beginning with the use condition the average drift rate under the likely old cue (collapsed across old and new materials) was reliably greater than the average drift rate under the likely new cue (collapsed across old and new materials), $t(33) = 11.17$, $p < .001$, Cohen's $d = 1.92$). In contrast, the average drift rate was similar during the likely old and likely new cue conditions under ignore instructions, $t(33) = -1.16$, $p = .25$, Cohen's $d = 0.20$). These differences were reflected in a reliable two-way interaction between instruction condition and cues on the average drift rate, $F(1, 33) = 99.22$, $\eta_p = .75$, $p < .001$.

Overall, the DDM parameter behavior indicates that during use instructions cueing affects both starting point and drift criterion. However, under ignore instructions, only an influence on starting point clearly remains; drift criterion is not reliably influenced by the cues and in fact demonstrates numerically reversed means.

Turning to Experiment 3b this pattern is largely replicated (see Figure 5). likely old and likely new cues modestly influenced the start point under the use instruction, $t(29) = 2.41$, $p = .022$, Cohen's $d = .44$, but more robustly influenced it under the ignore instructions, $t(29) = 8.08$, $p < .001$, Cohen's $d = 1.48$. These differences yielded an interaction between instruction and cue type, $F(1, 29) = 7.59$, $\eta_p = .21$, $p = .010$. These findings converge with Experiment 3a in demonstrating reliable cueing effects in the start point regardless of whether participants were actively using or attempting to ignore the cues. However, the interactions differed with the effect being larger during use instructions in Experiment 3a but larger in ignore instructions in Experiment 3b. Whether these different interaction patterns are genuine or the result of the catch trial manipulation is unclear and will require future study. Given this, it is safest to merely conclude that start point effects remain present regardless of instruction, which as we show next, is not the case for the drift criterion.

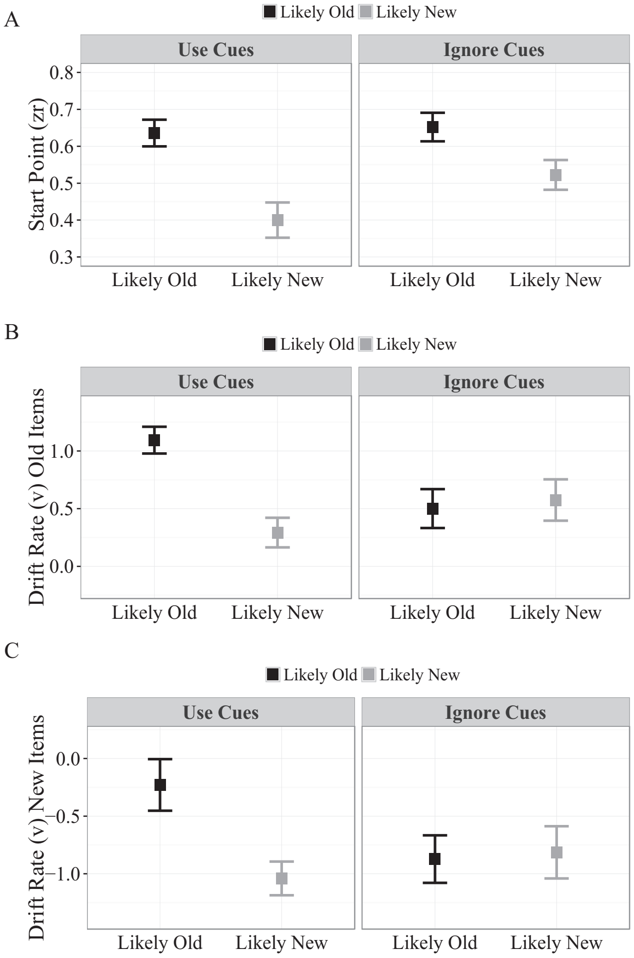

Figure 4. Experiment 3a drift diffusion model analysis. Panel A represents the average start point (zr) parameter collapsed across old and new items during use instructions and ignore instructions. Panel B represents the average drift rate (v) parameter for old items during use instructions and ignore instructions. Panel C represents the average drift rate (v) parameter for new items during use instructions and ignore instructions. Error bars represent ± 1.96 standard errors around the mean.

Beginning with the use condition the average drift rate under the likely old cue (collapsed across old and new materials) was reliably greater than the average drift rate under the likely new cue (collapsed across old and new materials), $t(29) = 8.12$, $p < .001$, Cohen's $d = 1.48$. In contrast, the average drift rate was similar during the likely old and likely new cue conditions under ignore

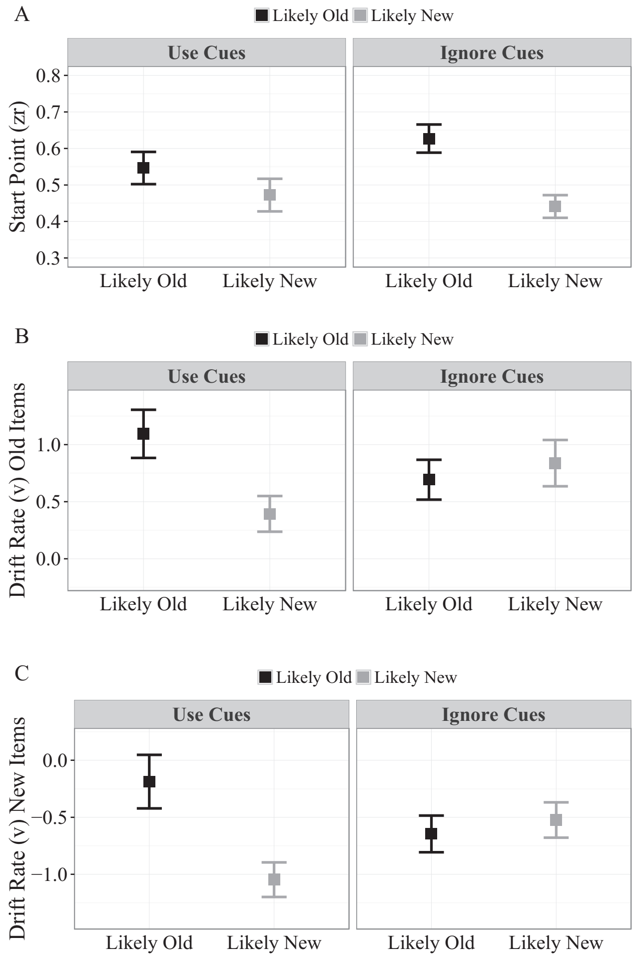

Figure 5. Experiment 3b drift diffusion model analysis. Panel A represents the average start point (zr) parameter collapsed across old and new items during use instructions and ignore instructions. Panel B represents the average drift rate (v) parameter for old items during use instructions and ignore instructions. Panel C represents the average drift rate (v) parameter for new items during use instructions and ignore instructions. Error bars represent ± 1.96 standard errors around the mean.

instructions, $t(29) = -1.46$, $p = .15$, Cohen's $d = 0.27$. These differences were reflected in a reliable two-way interaction between instruction condition and average drift rates during the two cues, $F(1, 29) = 59.55$, $\eta_p = .67$, $p < .001$.

Overall, the DDM parameter behavior in Experiment 3b again indicates that during use instructions cueing affects both starting point and drift criterion. However, under ignore instructions, only an influence on starting point remains; drift criterion is not reliably influenced by the cues and in fact demonstrates numerically reversed means.

Overall, DDM models dissociate the behavior of the start point and drift criterion as a function of the cues and instructions. When participants are actively using the cues, both start points and drift rates show prominent biases. However, when participants are attempting to ignore the cues, drift criterion effects are eliminated whereas start point effects remain. Although the behavioral data demonstrate that the mean response rates are only slightly (and not statistically reliably) influenced by the cues under ignore instructions, it is important to note the DDM jointly considers response rates and RTs, the latter of which were robustly affected by invalid versus valid cueing (see the online supplemental material). Therefore the parameter dissociations seen in the DDM largely reflect differences in the effects of cueing on the shapes and/or locations of the RT distributions under the ignore versus use instructions (for more detail, please see Ratcliff & McKoon, 2008).

Regardless, the data emphasize that even in Experiments 3a and 3b the influence of cues is robust under ignore instructions as the cueing effect sizes on the start point parameter were medium to large (Cohen's *d*s of .69 and 1.48, respectively). When combined with the fact that confidence was also reliably altered by cueing under ignore instructions, and the individual differences analysis on shift scores, it becomes clear that even when monetarily penalized for succumbing to cue influences in Experiments 3a and 3b participants were nonetheless robustly influenced.

General Discussion

We often make recognition decisions in the context of environmental information and ideally, it is important to be able to determine to what degree our decisions are based on information from our environment versus our own internally generated memory content. The current set of experiments examined whether observers could fully isolate these two sources of information by instructing participants to ignore reliable probabilistic cues and instead judge memoranda purely on their own recognition memory. In the current set of studies, we manipulated the timing and order of cue onset, and the incentives to resist cue influences. Although participants were able to reliably dampen the influence of the external cues when instructed to ignore versus use them, in no case were they completely effective in eliminating cue influence. Although participants were able to eliminate the expected directional influence of cues on average response rates when provided with a monetary incentive to do so, even under this condition a significant cue influence was found using more powerful individual differences analyses focused on shift scores. Moreover, mean level effects remained in the confidence and RT data and DDM analysis also demonstrated clear bias effects in the start point parameter. We refer to the remaining influence as the residual cue influence and it suggests that there may be both automatic and controlled aspects of cue incorporation during recognition. Under this conceptualization the clear reduction in cue effects when transitioning from use to ignore instructions constitutes the volitional biasing of memory judgments, and the remaining effects under the ignore instructions constitute the more automatic influence of cues on responding. The volitional effects are consistent with how criterion shifts are typically characterized under the basic signal detection framework; namely, as a strategically regulated criterion for mapping underlying evidence onto overt responding (see Figure 1). Moreover, as noted in the introduction, the frequent repositioning of this standard is often assumed to require considerable mental effort (Benjamin et al., 2009, 2013; Dobbins & Kroll, 2005; Stretch & Wixted, 1998) consistent with the idea that such shifts represent the parsing of internal evidence in nonhabitual, contextually specific ways (although see Konkel et al., 2015 and Starns & Olchowski, 2014). Thus, the overall pattern of data in Experiments 1 through 3 suggest there are both controlled and uncontrolled aspects linked to external cueing. With respect to the residual cue influence then, the question is how this influence arises and below we consider several alternatives beginning with processing fluency.

Item Processing Fluency

A considerable body of work has demonstrated that manipulations of recognition probes' appearance or presentation characteristics can influence recognition judgments (Busey, Tunnicliff, Loftus, & Loftus, 2000; Rajaram, 1993; Whittlesea, 1993; for review see Yonelinas, 2002). For example, in the widely cited Jacoby and Whitehouse (1989) phenomenon, briefly presenting a subliminal lexical prime before each recognition probe influences the degree to which observers conclude it is old. When the prime matches the probe old response, rates are increased compared to when the probe is not primed, and this is assumed to reflect the fact that observers mistake the increased fluency of lexical processing as indicative of prior study. Critically, however, this happens to a similar extent for both studied and novel materials and hence it is often described as a decision bias and is accompanied by a difference in signal detection estimates of criterion across the primed versus unprimed conditions. However, as discussed previously, signal detection theory is incapable of isolating influences that shift the decision criterion from those that may generally alter the perception or registration of evidence (i.e., joint distribution shifts). Thus, it is unclear if item processing fluency effects represent shifts in the decision criterion or joint distribution shifts. Here, however, we note that the traditional processing fluency accounts do not accommodate the current residual cue influence effects because they are built upon the idea that the increased processing fluency of the individual probes themselves contribute to perceived recognition. In contrast, the cues used in the present study (likely old and likely new) do not convey information that would facilitate the orthographic, lexical, phonological, or semantic processing of individual recognition probes because the cues signal upcoming mnemonic, not linguistic information. For example, providing the participant with a likely new cue cannot appreciably affect the item-specific processing of a particular probe (e.g., pickle) since regardless of the cue's validity it does not convey or forecast item-specific processing information. Therefore, while processing fluency effects may often be difficult for participants to fully ignore (as with the residual cue effects here), the two likely arise from different bases. We next consider whether the residual cue influence may be linked with action-based priming influences.

Action-Based Priming Effects

One feature to note about the current experiments is that the recognition response options remained fixed throughout testing such that old responses were always indicated by pressing the 1 key and new responses were indicated by pressing the 2 key. Thus, it is possible that the residual cueing effects during ignore instructions could be driven by a preparatory motor phenomenon, such that one anticipated action is easier to release than another. Alternatively, action-based priming accounts suggest that perceived and produced events and actions have highly overlapping representations, making it difficult for observers to separate their own actions from perceived actions (Hommel, 2009). For example, work by Kim and Hommel (2015) demonstrated that participants' subsequent attractiveness ratings for face stimuli can be biased by presenting participants with an unrelated intervening event signifying a number on the same scale as the target decision. Furthermore, this effect occurred even when participants were explicitly told the intervening number event was random, and occurred both when participants were shown a video clip of someone pressing a number on a keyboard and when they were shown a static image of a single number. Thus, given this finding that conformity effects can occur by simply being shown a randomly selected number on the same scale as one's own future action, it is possible that in our design predictive cues were associated with a particular motor response (e.g., seeing likely old cue and the pressing the 1 old button) and this action plan may have biased participants' responding.

To rule out this possibility, we ran an additional experiment that precluded participants from developing a motor preparatory bias (see the online supplemental material). Although the supplement describes the study in full detail, the key manipulation was that participants were required to make their responses via mouse click to randomized portions of the screen. That is, participants would respond old by clicking on a red box and new by clicking on a green box. Critically, the locations of these boxes changed randomly on a trial-wise basis and therefore a preparatory motor plan could not occur. Results revealed that participants' response rates and confidence were still reliably influenced by the to be ignored environmental cues, suggesting that the residual cueing effect observed in our studies were unlikely to be driven by a motor preparatory response bias. In addition, although theories of event encoding argue that perceived and produced action plans are overlapping (Hommel, 2009), it is less clear what action plans overlap in the EMC paradigm since it is not necessary to translate verbal memory cues into imagined action plans to interpret their meaning. Nonetheless, in Experiments 1–3 it is still possible that participants may have tried to prepare motor responses in order to finish the task more quickly. However, as demonstrated by the supplemental experiment the residual cueing effect remains even when controlling for this possibility.

As discussed above, neither item-processing fluency nor motor preparation or action-based priming phenomena explain the residual cue influence documented in Experiments 1 through 3 (and supplemental experiment). Before turning to a heuristics and biases account that seems to fare better, we first discuss the implications of DDM results.

Drift Diffusion Account

As mentioned previously, the benefit of the DDM is that it can potentially isolate two different types of biasing influences, one that is perhaps related to expectations and measured by the start point of the evidence accumulation (zr) and one that is perhaps more an alteration of evidence interpretation and measured by the drift rate (v); that is, the rate at of evidence accumulation (White & Poldrack, 2014). The results of Experiments 3a and 3b demonstrate robust cueing differences in both start point and drift rates under use instructions, although under ignore instructions the influence of cues were only apparent for the start point parameter (see Figures 4 and 5). Importantly, despite only observing slight and nonsignificant changes in average response rates under ignore instructions, the DDM findings clearly indicate that cueing affects the shapes and locations of the RT distributions under the ignore instructions.

The clear dissociation of the start point and drift criterion parameter effects as a function of instructions raise the question as to their potential psychological significance. Based on the work of White and Poldrack (2014) one might have expected that residual cue influences would be evident in drift criteria rather than in the start point since the latter is typically characterized as a response bias parameter, which implies that it might be under volitional control. In other words, if the start point parameter reflects how much evidence the participant demands before giving a particular response and he or she is ignoring the cues, then the start point should be similar regardless of cue condition. However, there are alternate ways to view the start point parameter that are not necessarily exclusive with the response bias interpretation. For example, the parameter could also reflect the degree of baseline activation favoring a mnemonic expectation analogous to proposed biasing mechanisms in visual attention (Desimone & Duncan, 1995) or the degree to which attention is steered toward particular subsets of memory features as a function of expectations (Tversky, 1977). In either case, the activation prior to the gathering of probe evoked evidence would be skewed toward a conclusion in a way that may be hard to control by the observer because it is driven by expectations and not intentions. In short, it may be difficult to ‘shut off’ an expectation of a subsequent experience even if one has no intention of acting on the expectation. However, because this is the first investigation of the ability of observers to ignore reliable environmental cues during recognition judgment it is premature to speculate further on the basis of the DDM parameter dissociation until further convergence can be sought. For example, a future study could examine an analogous paradigm in high level perception, for example gender discrimination, using analogous cues (e.g., likely male or likely female) and the question would be whether there are residual cueing effects under ignore instructions and if so, whether they are again reflected in the start point parameter estimate and not the drift criterion. Here however, we simply emphasize that the start point parameter is highly sensitive to the residual cue influence in the current paradigm and is by demonstration able to reveal robust residual cue influences even when the mean overt response rates are only modestly affected by the cues.

Confirmation Bias in Recognition

The current set of experiments demonstrates reliable influences of both pre- and postenvironmental cueing on recognition judgments even when participants are highly motivated to ignore this information and in fact will lose money if they fail to do so. These effects appear to be difficult to control even in a fairly basic memory task such as recognition. One would expect their expression would be even more considerable as the complexity of the memory task increased and memory search and self-cueing were rendered increasingly important (and hence more susceptible to expectations) and this appears to be the case (Madey & Gilovich, 1993; Schacter, 2001).

The apparent automaticity of the cue influence revealed in the residual cue effect is also consistent with the broader literature on confirmatory biases which are also difficult if not impossible for observers to completely control (Nickerson, 1998). When considering the overall findings it is important to distinguish between the observers’ expectations versus their intentions with respect to the cues. The former are presumably largely uncontrollable. For example, consider receiving a likely old cue under the ignore instruction. Even if one does not intend to demand less than usual recognition evidence before reaching an old conclusion following the likely old cue, it is nonetheless the case that given the predictive nature of this cue, one expects such evidence to be forthcoming and subjectively experienced. Based on the current set of experiments we suggest that it is this expectation itself that influences recognition judgment in an uncontrollable (or difficult to control) fashion because it alters perceived ease with which favoring evidence is recovered or detected. In addition, this idea is also consistent with attentional accounts which assume that anticipatory cues restrict the observers’ processing to certain dimensions or aspects of stimulus processing. The most famous of these paradigms is the visuospatial cueing paradigm of Posner and colleagues (Posner, Snyder, & Davidson, 1980). In one variant, central cues are used to cue attention to different regions in space in which potential targets appear and such cueing, when generally valid, improves processing at the attended location (or for the attended object attribute; Carrasco, 2011), perhaps through anticipatory increases in the gain of favored representations (Desimone & Duncan, 1995). Extending these accounts to the EMC paradigm merely requires that one view memory processing as open to top down attentional influences in a manner analogous to perceptual processing; a possibility wholly consistent with the idea that memory information is multidimensional and that recognition judgments are informed by various retrieval and fluency attribution processes (Busey, Tunnicliff, Loftus, & Loftus, 2000; Rajaram, 1993; Whittlesea, 1993; Yonelinas, 2002). However, we are not aware of a single perceptual study that has asked observers to attempt to ignore endogenous cues during discrimination or detection in order to see if such cueing yields difficult to control effects on target processing. The current findings suggest that it would, even if there were monetary consequences for exhibiting cue influences.

Potential Limitations

Although we demonstrated a persistent influence of cues during ignore instructions, it is possible that participants were simply unwilling to follow our instructions to ignore cues because it requires sacrificing accuracy gains. However, in Experiments 3a and 3b we strongly motivated participants to follow our instructions and created a situation where attempting to increase accuracy through predictive cues would yield a loss in monetary rewards. Thus, it would be particularly surprising if under this condition participants would still elect to rely on the cues to increase task performance; particularly since there was never any indication of actual recognition accuracy, only indications of failure to ignore the cues. Furthermore, there is little reason to think that participants routinely attempt to maximize long term accuracy during recognition research when doing so conflicts with the stated goals of the research. For example, work by Wallace (Wallace, 1982; Wallace, Sawyer, & Robertson, 1978) and Cox and Dobbins (2011) has examined participant recognition across standard lists, in which old and new items were intermixed, and pure lists in which all of the tested materials were studied or novel. When participants were informed of this pure list manipulation, both their accuracy and distribution of confidence remained highly similar to that of the standard intermixed testing conditions even though their foreknowledge of the lists construction would have enabled them to artificially boost their measured performance (Cox & Dobbins, 2011). In other words, they did not seek to maximize success rates simply because it was possible, but instead complied with the experiment instructions to report their actual recognition experiences to the materials. Thus, participants are capable of not attempting to capitalize on situations that could inflate accuracy when asked to do so.

Although participants in the current paradigm were given explicit information on the cues' validity they were not provided with performance feedback or reinforcement during the initial test in which they were encouraged to use the cues. This raises the possibility that individuals may have varied to the degree they believed the cues to actually be valid, which in turn may have influence the degree of cue influence even under ignore instructions. Such a finding might suggest that residual cue influences reflected volitional discounting of the ignore instructions in favor of accuracy. After Experiment 3a participants were asked whether they believed the cues to be 75% accurate using a simple yes/no question, and for participants answering no, they further provided estimates of the actual cue validity. However, there was no difference in the degree of cue influence for either hits or correct rejection rates during ignore instructions ($ps > .32$) between those who indicated the cues were at their stated validity and those who indicated they were not (mean subjective rating of 53% validity). In Experiment 3b, we simply asked participants to indicate how accurate they thought the cues were using a percentage. This estimate also did not correlate with the degree of cue influence on response rates during ignore instructions ($ps > .16$) and on average participants estimated the cues to only be 63% accurate. These findings, along with the fact that exhibiting cue influences in Experiments 3a and 3b incurred a clear monetary cost, suggest that the residual cue influence is not easily explained as simply an artifact of participants choosing to use cues against instructions in order to boost performance; particularly since they received no external feedback demonstrating that the cues did in fact increase their actual performance. Nonetheless, it remains the case that future work should continue to examine the degree to which the residual cue influence documented for the first time here remains outside observer control. For example, the cues could be described as resulting from an algorithm designed to negatively influence recognition in a manner similar to how someone might attempt to distort the reports of a crime scene eyewitness (with no mention of cue validity). This combined with a background story linking resistance to the algorithm to intellectual capacity or some other socially desirable trait would provide a compliment to the monetary cost approach used in Experiment 3a and 3b.

Finally, we have other work from our lab which demonstrates that participants are still highly influenced by cues even in the absence of explicit foreknowledge regarding cue validity (Jaeger et al., 2012). In Jaeger et al. (2012) participants were told that external cues were the responses of prior participants who took the same test, when in fact they were computer controlled. Robust cue influences were observed when the cues were 75% valid, as well as when the cues were random or unreliable (i.e., 25% valid). This work suggests that the confirmatory influence of cues is still present even when participants are not explicitly told the validity of the cues and even when the cues themselves are at or below chance.

Conclusion and Future Directions

To our knowledge, the current set of studies represent the first instance of asking participants to actively ignore diagnostic environmental cues during recognition memory judgments and indeed, we are unaware of any analogous manipulation in any other cognitive or perceptual domain. Given this, the residual nature of predictive cue influences on classification judgments is a fairly new domain of inquiry and the current data suggests cues may have a pernicious ability to influence judgments in spite of observers' best efforts to the contrary. Furthermore, in contrast to the extensive literature in eyewitness testimony that focuses on how misinformation during encoding can influence later memory retrieval (see Loftus, 2005 for a review), here we show that observers cannot fully isolate environmental information introduced during memory retrieval. Furthermore, the cues provided were computer generated and not presented in a social context (i.e., introduced by a confederate). Thus, our work suggests that participants' responses and confidence can be contaminated by environmental information introduced during decision making (e.g., a lawyer biasing an eyewitness report in court) even in the absence of additional social information (e.g., confidence expressed in one's tone, descriptive phrases, etc.), and although observers may attempt to provide reports based purely on their own memory evidence, this may not be wholly possible. One interesting avenue of future research would be to examine whether analogous influences arise in high-level perception, such as gender classification, and further, to see if they are linked to individual differences in cognitive control and metacognition more broadly. For example, are participants who demonstrate large residual cue effects in recognition also likely to demonstrate large analogous effects during perceptual judgments? Moreover, recent work suggests that older adults benefit as much as younger adults during the EMC task when attempting to use the cues to bolster performance (Konkel et al., 2015). However, older adults' ability to resist or ignore cues remains untested and work in false hearing, where older adults appear particularly susceptible to cued interpretations of noisy speech (Rogers, Jacoby, & Sommers, 2012), suggests they may also have difficulty ignoring environmental cues about memory even in basic recognition situations. Regardless, the current studies demonstrate that biases in recognition may have controlled and uncontrolled aspects and delineating the operating characteristics and neural substrates of the two remains an important topic of future research.

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