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The Automatic and Controlled Influence of Environmental Cues During Recognition Judgments

by

Diana Selmeczy

A dissertation presented to the
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ABSTRACT OF DISSERTATION

The Automatic and Controlled Influence of Environmental Cues During Recognition Judgments

by

Diana Selmeczy

Doctor of Philosophy in Psychology

Washington University in St. Louis, 2014

Professor Ian G. Dobbins, Chairperson

Recognition judgments are often made in the context of environmental information, such as instructions or payout regimens that observers have been shown to use in order to adaptively bias their memory judgments. These adaptive biases are usually characterized as a strategic, controlled process that observers adopt intentionally, and this is formalized in signal detection theory wherein strategic criterion placement is assumed fully independent of recognition evidence signals. However, recent pilot research in our laboratory suggests that the ability to regulate recognition decision biases in the context of environmental cues may not be fully controlled by observers suggesting an automatic or unintentional cue influence operating during recognition judgment. The current set of experiments examined the degree to which observers could disregard environmental cues during their recognition decisions and is the first systematic study of the ability of observers to disregard non-mnemonic cues during recognition decisions. As a whole, they suggest that observers cannot fully isolate their own memorial processes from cue-induced expectations or confirmations, which in turn suggests there is a lower limit to the degree to which decision biases are under strategic control.

Chapter 1

Introduction

The ability of observers to identify whether or not a stimulus originated from a specific previous experience is termed recognition memory. The decision processes underlying recognition have been of great theoretical interest for many decades and in particular researchers have been keenly interested in the degree to which observers can strategically alter their decision processes to maximize accuracy or goals such as expected gains in various contexts (e.g., differing locations, times of day, etc.). For example, deciding whether an individual is recognized should be influenced by our surroundings such that a positive judgment should require less memory evidence at a department meeting, where most people tend to be familiar, than at a foreign airport, where one is a priori unlikely to encounter a familiar individual. Strategically biasing judgments is a form of Bayesian reasoning such that an individual judges current evidence in light of prior probabilities or base-rates of oldness or newness. If the prior highly favors oldness then minimal evidence is required for an old judgment, if it instead favors newness then considerable positive evidence should be required for the same judgment. The current proposal examines the degree to which observers can control these types of adaptive memory biases when explicit environmental cues indicate the prior probability that a given stimulus is old or new.

Ideal observer models assume that observers have full control of adaptive memory biases and intentionally change their decision strategies under varying contexts. In fact, research demonstrates that participants can change their responses by simply being instructed to do so (Healy & Jones, 1975; Hirshman, 1995; Miller, Handy, Cutler, Inati, & Wolford, 2001), suggesting that the decision process can be volitionally manipulated. However, closer examination of the literature suggests that observers may require a great deal of external support to alter decision strategies. The current proposal will explore the degree to which observers are able to strategically alter their decision biases as a function of context and, critically, the potential role of automatic mechanisms during this process. In other words, I will test the possibility that observers may not be able to fully control the biasing process or processes.

I will begin by outlining a common, formal decision model of recognition and summarizing prior research examining explicit factors that influence the decision process. I will draw general conclusions regarding when decisions are likely to be strategically altered and the degree to which this process is explicitly controlled by observers. I will then explore the putative role of automatic influences on memory decision processes and the rationale for the current series of experiments.

1.1 Signal Detection Theory

Recognition decision making has been studied extensively and can be more formally described using the theory of signal detection (Banks, 1970; Egan, 1958; Parks, 1966). Signal detection theory serves as a foundation for many models of recognition memory and I will focus on the simplest form of the model (i.e., one dimensional equal-variance model). In the laboratory, participants study a list of items and then complete a recognition test during which they indicate whether intermixed studied and novel items arise from the study context ('old') or are new to the experiment ('new'). The signal detection model represents old and new items with overlapping normal distributions on a continuous axis of familiarity or memory evidence (Figure 1). The mean of the old item distribution falls to the right of the new item distribution due to the recent exposure of old items during study. Since the item distributions overlap, observers are forced to parse continuous memory evidence into one of the two discrete response categories: old or new.

This results in four possible response types termed a hit (old item correctly classified as ‘old’), miss (old item incorrectly classified as ‘new’), correct rejection (new item correctly classified as ‘new’), and false alarm (new item incorrectly classified as ‘old’). The level of evidence where an observer classifies items falling above the value as old and below the value as new is referred to as a decision criterion and biased judgment reflects the positioning of the criterion so as to generally favor either ‘old’ or ‘new’ classification responses.

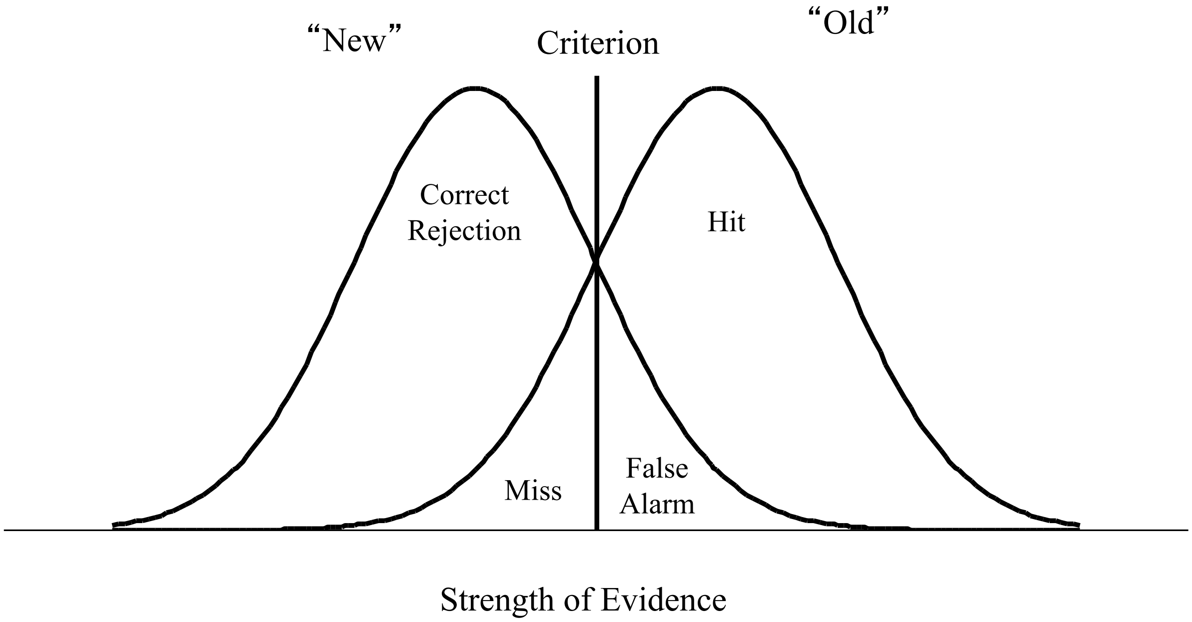


Figure 1. Basic Signal Detection Model. The figure above represents the equal-variance signal detection model. The x-axis is strength of evidence, the distribution to the right is previously studied old items, and the distribution to the left is new items. The decision criterion reflects the point at which observers parse the continuous memory evidence axis into items they report as "Old" vs. "New". This results in four possible response types termed a hit (old item correctly classified as 'old'), miss (old item incorrectly classified as 'new'), correct rejection (new item correctly classified as 'new'), and false alarm (new item incorrectly classified as 'old').

Early and many current models assume that observers strategically place their response criterion in such a way as to maximize the proportion of correct responding (Glanzer, Hilford, & Maloney, 2009; Pastore, Crawley, Berens, & Skelly, 2003; Swets, Tanner, & Birdsall, 1961; Turner, Van Zandt, & Brown, 2011). This ability implies that for every experienced strength value, the observer estimates the probability of it having arisen from the old item distribution vs. the new item distribution. The ideal observer classifies the stimulus by labeling it as originating from the distribution yielding the highest probability, which can be simply formalized as a likelihood ratio decision variable consisting of target divided by lure probability estimates. When this ratio is greater than one, the item is labeled as a target and when it is below one it is labeled as a lure. As seen in Figure 1, the prior probability of old and new items is equal where the distributions overlap and this strength value has a likelihood ratio of 1. This is the only point on the axis during which the observer would be indifferent as to which label was assigned to the stimulus. Hence under this approach, the decision variable is not raw strength but one of likelihood ratios or odds in favor (or against) oldness. Aside from the benefit of being statistically optimal, assuming that observers use such an axis reduces all dichotomous discriminations to a common metric, namely relative likelihoods.

Under signal detection theory, recognition accuracy is measured using the d’ (d-prime) statistic, which estimates the distance between the two internal evidence distribution means in common standard deviation units. Thus, a greater difference between distribution means is associated with greater internal sensitivity or a larger d’. Internal sensitivity within a given context is assumed fixed such that the observer cannot will a greater sensitivity under a particular set of conditions. Analogously, one might deeply wish to be able to see the smallest line of letters during a vision test, but this will not actually improve the internal resolution of the vision system.

In contrast to internal sensitivity, the decision bias or recognition criterion can be altered such that by changing the criterion location one can favor particular judgments in different contexts. This allows one to potentially maximize outcomes even though the internal sensitivity remains fixed. For example, one’s internal sensitivity to familiar versus novel faces may be fixed, but the decision strategy employed when one is at a foreign airport should ideally be different than when one is at a local department meeting. One common measure used to calculate criterion is the C statistic, which estimates criterion location relative to the center of the overlap between the two evidence distributions in standard deviation units. When C equals zero, observers are said to be ‘unbiased’ and respond ‘old’ and ‘new’ at equal rates with the criterion located at the intersection of the evidence distributions (Figure 1). Negative C values, commonly referred to as liberal responding, occurs when participants respond ‘old’ more often, whereas positive C values, commonly referred to as conservative responding, occur when participants respond ‘new’ more often.

Aside from differences in priors, there are a host of other environmental conditions or cues that should lead observers to strategically change their decision criteria and these are described in more detail in the following section. Furthermore, it is important to note that while the previous examples focus on maximizing correct responding, in other contexts the goal of the observer may be to maximize the expected value of judgments (for more information see Lynn & Barrett, 2014). For example, in some situations the mistake of committing a false alarm may be particularly more costly than a miss, such as the embarrassment of mistakenly approaching a stranger as familiar as opposed to not greeting a friend. In this case, the observer more heavily weights the avoidance of a particular class of costly errors, and sets a conservative criterion to avoid committing false alarms and maximize social rewards. Thus, optimal criterion placement depends on the specific goal in a given context. While often the goal may be to maximize correct responding, another potential and functionally different goal may be to maximize the expected value of judgments by placing the criterion to jointly maximize social or economic gains for correct judgments and minimize costs for errors.

1.2 Factors That Influence Criterion Placement

Researchers examining the adjustment of recognition decisions often use language that suggests criterion regulation is a wholly strategic or controlled process. For example, criterion adjustment is described as “difficult or effortful” (p. 256 Verde & Rotello, 2007), requiring “mental energy” (p. 1390 Stretch & Wixted, 1998b), and as a “controlled executive processes” (p. 1186 Dobbins & Kroll, 2005). It is not necessarily surprising that criterion regulation is thought to be a strategic process, since appropriate criterion placement may require observers to reflect on prior encoding experiences (e.g., well learned vs. poorly studied items), consider rewards or penalties for certain response types, and/or consider the relative preponderance of familiar versus novel items in the environment. Additionally, certain contexts may require observers to use multiple decision strategies such that some items should elicit the use of a conservative criterion while other items should elicit the use of a liberal criterion. Most often it is this ability to maintain multiple criteria simultaneously and adaptively switch between them that is described as being particularly effortful and demanding (Benjamin, Diaz, & Wee, 2009; Benjamin, Tullis, & Lee, 2013; Morrell, Gaitan, & Wixted, 2002; Stretch & Wixted, 1998b).

The most basic demonstration of volitional criterion control involves simply asking participants to try to avoid particular types of judgment errors (Healy & Jones, 1975; Hirshman, 1995; Miller et al., 2001). For example, in Miller et al. (2001) participants were told to respond liberally (i.e., avoid misses) to words presented in green and conservatively (i.e., avoid false alarms) to words presented in red during a recognition test. Participants were tested in blocks of intermixed old/new words, where either all the words were presented in one color signifying conservative or liberal responding for that block, or colors were intermixed within a block where some words were presented in green while other words were presented in red. Regardless of whether word color was fixed or intermixed within a block, participants adopted more liberal criteria for words presented in green and more conservative criteria for words presented in red. The fact that participants can change their decision biases through simple instruction suggests that recognition biases are at least in part under volitional control and hence there may be a wide array of environmental cues that participants use to purposefully bias their recognition judgments.

In addition to simply asking participants to adopt a particular recognition bias, participants adopt decision biases in response 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, 1998b), and item memorability or distinctiveness (Benjamin & Bawa, 2004; Brown, Steyvers, & Hemmer, 2007; Dobbins & Kroll, 2005). For example, in Healy and Kubovy (1978) participants studied lists of five digit numbers followed by recognition tests with varying ratios of old and new items. Results revealed that participants adopted more liberal criteria when the majority of test items were old (75% old, 25% new) relative to when the majority of test items were new (75% new, 25% old). Thus in line with our earlier home department vs. foreign airport example, it appears that under some circumstances, observers do factor in statistical priors when making recognition judgments. In contrast to manipulations of base rates, Hirshman (1995) manipulated average strength of targets between test lists in order to induce a criterion shift. Participants studied a purely weak list where all items were presented for 400ms, and a mixed list where half the items were weak items presented for 400ms, and half were strong items presented for 2 seconds. Participants used a more conservative criterion for weak items studied in a mixed list context vs. a pure weak list context, since the average strength of items was higher in a mixed list than a pure weak list. Note that the specific experiments described above used between list manipulations that require observers to maintain a single criterion across one test list and adopt a different criterion across another separate test list.

Criterion placement can also be influenced by using a within list or trial-by-trial manipulation. One example of such a manipulation uses an Explicit Memory Cueing paradigm where participants are provided with reliable external cues indicating the likely status of each upcoming memory probe. Under such situations, an ideal observer should incorporate these cues along with their internal memory evidence before rendering a judgment. In other words, observers should adaptively bias their response in accordance with the environmental cue (i.e. Likely Old or Likely New) on a trial-by-trial basis in order to maximize overall performance (for more detail please see Selmeczy & Dobbins, 2013). A growing number of studies from our laboratory assess the influence of reliable external recommendations during recognition decisions (Jaeger, Cox, & Dobbins, 2012a; Jaeger, Lauris, Selmeczy, & Dobbins, 2012b; O'Connor, Han, & Dobbins, 2010). For example, in Selmeczy and Dobbins (2013) participants studied a list of words in blocks using either shallow encoding (i.e., are the first and last letters of the word in alphabetical order?) or deep encoding (i.e., rating words as concrete vs. abstract). During recognition testing the majority of trials were presented with an environmental cue (reading Likely Old or Likely New) one second prior to the appearance of the recognition probe. The participants were explicitly informed of cue validity (75% valid) and told to use the cues to benefit their performance. Ideal performance requires observers to integrate internal memory evidence with cue information or statistical priors. Thus, a 75% valid Likely Old cue should bias observers to respond old more often since the statistical prior heavily favors an old response (odds of encountering an old item are 3:1). In contrast, a 75% valid Likely New cue should bias observers to respond new more often since the statistical prior heavily favors a new response (odds of encountering a new item are 1:3). Results revealed participants were in fact able to achieve performance benefits by flexibly shifting the decision criterion in line with the cues.

Although the above studies demonstrate a large number of conditions in which observers adaptively bias recognition judgments, there appear to be some situations in which they are surprisingly unwilling or unable to appropriately bias their recognition judgments. For example, Stretch and Wixted (1998b) failed to find criterion shifts when manipulating strength on an item-by-item level, even when using salient perceptual cues to indicate memory strength. In their experiment participants studied a list of words where half of the items were strong items that were repeated five times and presented in one color (e.g., red) and the other half of the items were weak items presented only once and in a different color (e.g., green). At test previously studied items were presented in the same color as during study and intermixed with lures, half of which were presented in one color and half of which were presented in the other color. Despite increased accuracy for strong items, participants' criteria remained the same for both strong and weak items as demonstrated by equivalent false alarm rates across both colors, even after they were explicitly told the relationship between the colors and the two study conditions. Prior work clearly demonstrates that observers can shift strategies based on simple instructions, and given this, Stretch and Wixted (1998b) found the lack of shift surprising leading to the ad hoc explanation that it must have been too effortful for participants to shift their criteria on a trial-by-trial basis. However, it is also the case that the manipulation was potentially contradictory in that while color clearly signaled encoding strength for studied materials, it did not do so when paired with novel materials. That is, half the time seeing a word in red would elicit strong memories since the items were well encoded; however, the other half the time seeing a word in red would elicit minimal memory evidence since half of the novel items were also presented in red. Thus the diagnostic value of a red cue in this design was fairly subtle. On average, it signaled a slightly higher mean strength signal than the green color cue and this ambiguity might not have indicated a clear biasing strategy for the subject on each individual trial. Furthermore, because several studies have demonstrated observers are in fact able to adaptively update their decision criteria in a trial wise manner (e.g., Dobbins & Kroll, 2005; Jaeger, Lauris, Selmeczy, & Dobbins, 2012b; Rhodes & Jacoby, 2007; Selmeczy & Dobbins, 2013), it does not appear to be the case that the simple requirement to repeatedly alter one's decision bias on a trial-by-trial basis is the cause of the above failure to see adaptive recognition biases.

This discrepancy between findings suggests that various factors may influence whether criterion shifts are observed. For example, criterion shifts are likely to occur when participants are provided with explicit knowledge of the manipulation either via instructions or feedback that renders the appropriate strategic bias fairly obvious. Verde and Rotello (2007) demonstrated that participants only appropriately shifted their response criteria to changes in target strength, such that they responded more conservatively to targets repeated four times relative to once repeated targets, when trial-by-trial feedback was supplied. When manipulating base rate proportions and not informing participants of this manipulation, multiple studies have found that the presence of feedback resulted in more appropriate response criterion placement relative to when no feedback was presented (Estes & Maddox, 1995; Kantner & Lindsay, 2010; Rhodes & Jacoby, 2007).

Since a shift in response criterion reflects an atypical way of responding to memoranda on the part of the participants, it presumably must be initiated in response to some environmental cue that signals a bias would be advantageous. Although feedback seems to be critical for both within list and between list criterion shifts, in the absence of feedback the recognized utility of these biasing cues may depend upon their salience (i.e., will it be detected as potentially useful by the observer) and perceived reliability (i.e., is there a way to establish that it generally predicts one type of memory stimulus (e.g., old) compared to another (e.g., new). For example, in Stretch and Wixted (1998a) while differences in word color were likely a very perceptually salient cue, these color cues did not reliably signal whether the stimuli were old or new. However, in Selmeczy and Dobbins (2013) criterion shifts were observed even in the absence of feedback presumably because participants were explicitly provided with cues (Likely Old and Likely New) that were both salient and reliably predictive of item status (75% valid). Furthermore, the appropriate decision strategy on each individual trial is quite clear with such a cue. Overall, although adaptive criterion shifts during recognition judgments often occur, it appears as though observing these shifts may require a great deal of external support such as feedback, explicit instructions, and/or other environmental cues that are both salient and reliably predict item status.

1.3 Cognitive Control and Criterion Regulation

As noted earlier, prior research examining recognition criteria largely assumes that adaptive criterion movements are a result of explicit strategies. This assumption is either stated explicitly when researchers describe criterion adjustments as being cognitively demanding, or it is inherent in the type of manipulation used to alter responding. For example, instructing participants to respond liberally or conservatively necessarily assumes that observers should be able to intentionally manipulate their decision strategy. Additionally, base rate or memorability manipulations imply that observers are aware that some difference exists between contexts in which items occur and they can incorporate this relevant information into a strategic bias. However, as previously discussed, certain manipulations may require external support such as feedback and/or explicit instructions in order to observe criterion shifts. Furthermore, within list criteria manipulations require altering of decision biases on a trial-by-trial, often randomized basis. Since criterion regulation often involves some combination of maintaining an explicit strategy, holding the location of multiple criteria in memory, and shifting between these locations, it is reasonable to conclude that criterion regulation may depend on cognitive control or a conscious, limited capacity, and goal oriented process. Other literature examining control using different attentional tasks (e.g., Stroop) has suggested that list-wide and item-specific control may operate by separate mechanisms (for review see Bugg & Crump, 2012), and this may also be true in the case of recognition criterion regulation. However, the role of cognitive control in adaptive recognition criterion movements is still a largely unexplored topic (however see Dobbins & Kroll, 2005; Konkel, Selmeczy, & Dobbins, 2014) and the current proposal will mainly examine the degree of control observers can exercise during within list manipulations. Thus, if criterion regulation is a volitional or controlled process, observers should be able to exercise an appropriate bias under varying contexts when encouraged to do so and critically to prevent themselves from shifting their decision criteria when instructed to do so. However, the set of studies described in the next section demonstrate that observers may not be able to completely prevent criteria shifts from occurring when provided with probabilistic cues that they are instructed to ignore. These results suggest that in addition to controlled processing, heretofore-unexplored automatic decision processes may also play a role in recognition judgment.

1.4 Putative Role of Automatic Processes in Criterion Regulation

As described previously, observers are often able to capitalize on reliable environmental cues by adaptively biasing their recognition judgments. That is, when participants are provided with probabilistic environmental cues (Likely Old and Likely New) that forecast the upcoming status of a recognition probe, and are explicitly instructed to use these cues, they clearly demonstrate cue related criterion shifts that result in net performance gains compared to when the cues are unavailable (e.g., Selmeczy & Dobbins, 2013). Although capitalizing on reliable environmental cues can clearly be adaptive, as mentioned previously there are instances where the goal of an observer may not be to maximize the proportion of correct responses but instead some other goal is relevant such as maximizing expected value. For example, during eyewitness testimony the goal of the eyewitness is to describe the original memory as faithfully as possible and not be influenced by information he/she may have heard after the event. In other words, the observer is maximizing social rewards by providing a high fidelity report without being contaminated by other environmental cues. Reporting memory events accurately when other influential factors are also present in the environment would require that observers separate their internal memory evidence (e.g., the original memory event one witnessed) from external information signaling a stimulus is likely novel or previously encountered (e.g., hearing something afterwards about a suspect). Critically, under SDT these two processes (i.e., evidence and decision processes) are assumed to be independent such that decision processes can be altered while evidence based processes are fixed within a given context. As mentioned previously, this is analogous to reporting vowels during a vision exam where one might deeply wish to be able to see the smallest line of letters, but this will not actually improve the internal resolution of the vision system. Additionally, even if one overhears a friend’s correct answers during the vision exam this information could influence the decision process but it would not improve internal resolution. The set of pilot experiments described below examined whether observers can in fact separate internal memory evidence from environmental information during recognition judgments by instructing participants to disregard probabilistic environmental cues.

During the first pilot experiment participants were given a recognition test where the majority of trials were preceded with a cue (reading Likely Old or Likely New) intermixed with uncued or baseline trials. Participants were informed of cue validity (75%) and in the first and third study/test cycles they were instructed to use the cues to increase their performance, while in the second and fourth study/test cycles they were instructed to ignore the cues and rely solely on their own memory when making their decision. In order to ensure participants did not forget which test block they were in the words “USE CUES” or “IGNORE CUES” remained on the screen during the entire test period. 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 therefore they do not simply ignore the cues in the following test blocks because they believed them to be unreliable. As expected, when participants were instructed to use cues a large criterion shift was observed between Likely

Old and Likely New cued trials; however, results revealed that under ignore instructions participants still displayed a significant criterion shift between Likely Old and Likely New cued trials, although this effect was dampened compared to the use instructions condition (See Figure 2). If criterion positioning were under full strategic control, then it is unclear why the measured criterion shifted when observers were instructed to ignore the cues. These results suggest that participants may not be able to fully isolate environmental cues forecasting likely memory outcomes from their assessments of internal memory evidence, which is fairly problematic under situations where unbiased reporting is critical. However, it may be the case that observers did not follow instructions to ignore cues because it required them to sacrifice accuracy without a clear payoff for doing so. In other words, since the participants knew the cues were reliable and could result in performance gains in terms of the percentage of correct responses, they may have ignored the instructions asking them to rely purely on their own memory evidence and still incorporated the cues. In this case, the observed results could be due to participants disobeying the instructions and trying to maximize accuracy instead of judgment fidelity.

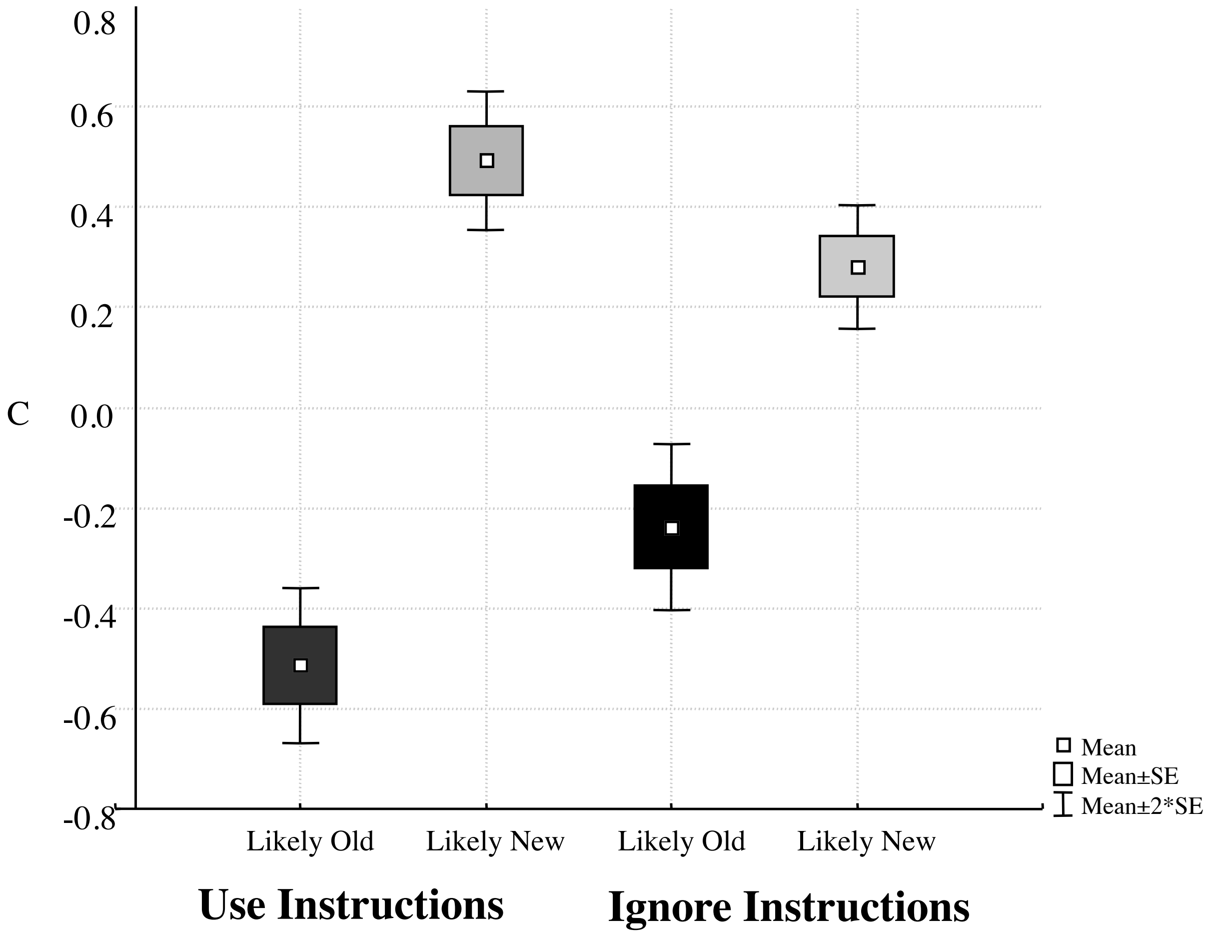


Figure 2. Pilot data results. The y-axis represents criterion (C) and the x-axis represents each cue type (Black: Likely Old, Gray: Likely New) for both use and ignore instructions.

In order to control for the possibility that participants may have disobeyed the instructions in order to maximize accuracy, in a second pilot experiment participants were given two opportunities to respond during each recognition trial. During the first response (Me-Only), they were instructed to base responding solely on their own memory content and completely ignore the cue present in the environment. During their second response, they were instructed to base their responses on joint consideration of their memory content and the environmental cue. This latter response was termed ‘Me+Cue’. The purpose of this two-response procedure was to circumvent the possibility that a desire for accuracy was precluding participants from ignoring the cues in our earlier studies. Because the second response involved joint consideration of the cue and internal evidence, participants would always be able to maximize final trial accuracy.

Overall, the results from the two-response design once again revealed that participants could not fully ignore environmental cues. Critically, during the first/Me-Only response, when cues should have been ignored, participants still shifted their criteria and responded conservatively under a Likely New cue and liberally under a Likely Old cue. More extreme criterion shifts were observed during the second/Me+Cue response, suggesting that participants were further incorporating cues when instructed to do so during their second response (See Figure 3, left panel). Thus, even when using a design where participants could capitalize on the cues during their second responses (and thus were not sacrificing eventual use of the cue) their initial responses were still biased, apparently unwittingly, by the presence of a predictive cue in the environment. Consistent with the first experiment, once again these results suggest that observers cannot fully discount probabilistic cues during recognition judgments.

However, a simple potential explanation for these results could be that the cues were influencing a motor preparatory response as opposed to the recognition decision per se. For example, motor preparation may result in a small number of inadvertent, anticipatory key presses that were minimally or not at all dependent upon accruing memory evidence. To rule out this possibility a third experiment was conducted once again using the two response design, except old/new responses were made via a mouse click to randomized regions of the monitor in order to indicate old (green square) or new (red square) decisions. This precluded the development of a motor response bias because the motor movement required to register a particular decision could not be predicted in advance. Results revealed that participants still rendered biased recognition judgments when cues were present during the initial first/Me-Only response, and thus our prior findings were not simply due to motor preparation errors (See Figure 3, right panel).

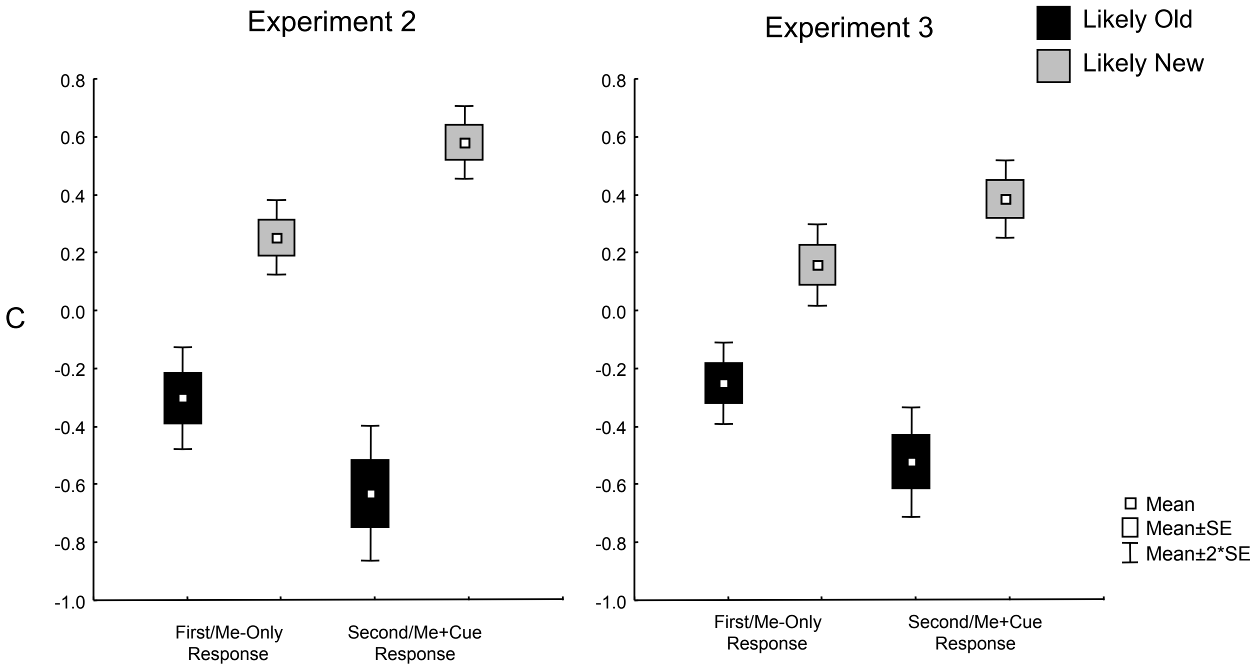


Figure 3. Pilot data results using two-response design. The y-axis represents criterion (C) and the x-axis represents each cue type (Black: Likely Old, Gray: Likely New) under the first/Me-Only and Second/Me+Cue response. The left panel represents Experiment 2 during which fixed keys were used to indicate old/new decisions, while the right panel represents Experiment 3 during which randomized locations on screen were used to indicate old/new decisions.

Despite explicit instructions, my pilot data demonstrate that participants cannot fully ignore probabilistic environmental cues, suggesting that observers may not have complete control over their recognition judgments once a predictive environmental cue has been processed. Instead, it appears as though participants may, in part, be unintentionally or automatically influenced by environmental cues despite their best efforts to ignore them. This is somewhat surprising because the cues themselves are not critically tied to test stimuli, which are never repeated throughout the test, but are instead tied to item status (i.e., old vs. new). Additionally, as already noted above, the effect cannot be explained as a result of advance motor response preparation. These results suggest that there may be some confusion on the part of the observers between the information signaled by the environmental cue (that an upcoming stimulus is likely to be old or new) and the information signaled by the memoranda itself, which broadly means that there is some confusion between internal memory evidence and environmental information.

Although participants are instructed to ignore cues, an unintentional cue influence may result because observers nonetheless anticipate the upcoming sensation or experience of familiarity or novelty signaled by the cue and this expectation then changes the manner in which the signal itself is subjectively experienced. Since the cues are reliable and participants are informed of this information, a preparatory response driven by environmental cues, such as mobilizing attentional resources towards experiences of familiarity or novelty, may develop and result in fairly automatic behavior. In other words, observers presumably build an expectation such that a Likely Old cue elicits an anticipation of feelings of familiarity (or recollection) whereas a Likely New cue elicits anticipation of feelings of novelty. This cue driven expectancy may in fact contaminate the processing of the evoked memory signal, such that items following a Likely Old cue are more likely to be processed as old, whereas items following a Likely New cue are more likely to be processed as new. Under this model, the anticipation itself alters the subsequent subjective experience. We refer to this hypothesis as an expectancy based account of automatic cue influence and the current experiments aimed to test the predictions of this hypothesis.

To summarize, the pilot data suggest that in our previous use of the Explicit Memory Cueing paradigm, where observers were always encouraged to use the cues, there may have been two separate processes occurring- an explicit or controlled process that participants intentionally engage in, and also putatively automatic or cue driven preparation process that participants are unable to override, perhaps because they are unaware that the cues are influencing their judgments during ignore instructions conditions. From this perspective, observed shifts in decision criteria may reflect a controlled response strategy on the part of the observers in which they attempt to maximize some long term goal during responding; however, the observed shifts may also reflect a biased consideration or evaluation of memory evidence itself such that signals that are consistent with expectations are processed more fluently or perceived as more salient. Such an effect would be difficult to control since from the observers' perspective it would not clearly be identifiable as a bias. However, this latter process was only revealed when participants were instead instructed to ignore these environmental cues, where participants appear to only be able to limit one process (i.e., explicit cue use) but an unintentional or automatic influence of cues remains. It is the full pattern of data that suggest an implicit influence is occurring and critically it is not clear one can inhibit an influence of which one is unaware.

Although conceptualizations of automaticity vary (see Moors & De Houwer, 2006), automatic behavior tends to be described as fast acting, unintentional, and difficult to override, while controlled behavior is described as slower, effortful, strategic, and goal oriented (Posner &

Snyder, 1975; Shiffrin & Schneider, 1977). I addressed several questions mainly concerning observers' ability to limit the influence of environmental cues by testing the expectancy hypothesis described above. I tested whether the unintentional influence of environmental cues could be eliminated if cue based expectations no longer occurred by presenting environmental cues after recognition probes (Experiment 1) or providing participants with random or uninformative cues (Experiment 3). Additionally, one possibility is that even if participants are aware that the cues automatically lead them to expect certain outcomes (i.e., they are aware that they experience an expectation of familiarity or novelty), they may not be aware that these expectations color or flavor their interpretation of actual memory evidence (i.e., they are not aware that their responding is actually biased). Therefore, I also examined whether participants have any explicit awareness that an unintentional cue influence has occurred (Experiment 2). Finally, in order to rule out the possibility that previous findings were simply due to a lack of motivation from participants to follow our instructions, I also tested whether providing participants with monetary incentives to ignore cues would eliminate the influence of cues (Experiment 4).

Chapter 2

Experiment 1

Experiment 1 assessed when during the decision process environmental cues exert their influence and whether participants can fully ignore cues when they no longer anticipate recognition experiences. Prior work using the Explicit Memory Cueing paradigm always had the presentation of the cue precede the recognition probe. If a cue is presented prior to the memory probe, it can elicit an expectation of upcoming familiarity or novelty that may influence the processing of the probe itself. However, if the cue is presented after a recognition judgment has already been made, there can be no expectancy mediated processing of the probes and perhaps the influence of the cues could be fully eliminated. Thus, if participants have already made their decision uninfluenced by any environmental cue, asking them to then 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.

Thus, under the expectancy based account participants should be able to ignore cues when they are presented after a recognition probe provided that sufficient time between the probe and cue has elapsed. In contrast, when the cue presentation follows more proximally to the probe an increased influence is predicted because the expectation effect will have time to operate.

2.1 Methods

Participants. Experiment 1 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. Participants entered their responses via keyboard and all responses were self-paced. The timing and presentation of experiments was generated using Matlab's Psychophysics Toolbox (Version 3.0.8; Brainard, 1997; Pelli, 1997). For each participant, study and test words were randomly selected from a 1,216-item pool, with an average of 7.09 letters and 2.34 syllables and average log HAL frequency of 7.74 log.

To overview, participants completed 4 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 (See Figure 4 for timing and details). To avoid fatigue, participants were encouraged to take a short break between each study/test cycle.

Figure 4. Test design Experiment 1. During the first test cycle, participants were instructed to use cues. The cue appeared in isolation for 1 second prior to the probe. During the 2nd-4th test cycles, participants were instructed to ignore cues. The cue appeared after the probe at various lags (see text for more details).

During study participants performed a syllable counting task (i.e., does this word contain 1, 2, 3 or more syllables?) on a list of serially presented words (78 items). Syllable counting was used in all experiments in order to produce moderate levels of subsequent recognition memory performance and limit variation in encoding strategies. 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) (78 old items, 78 new items). After each recognition decision, participants made a confidence rating using a 6-point scale ranging from 50%-100% in 10% increments with 50% indicating guessing and 100% indicating certainty.

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 before the recognition probe (88 cued items-44 old, 44 new), while 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.” In order 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 I was mainly interested in examining whether participants could ignore external cues, 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 2nd-4th 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), while other trials were uncued/baseline trials (12 uncued items- 6 old, 6). For all participants the shortest delay was fixed at 300ms in order 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 reaction times 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 seconds, *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 post-cued trials, whereas at their 90th percentile they would have likely have completed their recognition decision for the majority of post-cued trials. After the cue appeared, participants were not allowed to respond until the cue was on screen for one second, which was done in order to match lexical processing time of the cue between use and ignore test cycles. After this one second processing of the cue, participants could key in their self-paced old/new decision followed by their confidence response.

Although my prior work clearly demonstrates that participants were influenced by cues despite our instructions to ignore them, one potential concern with the current design is that participants may not actually process 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. In order 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 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 in order to avoid confusing the two judgments.

Finally, at the end of the experiment participants answered a short questionnaire assessing how well they followed the instructions. Participants were asked "How well were you able to follow the instructions to USE cues?" and "How well were you able to follow the instructions to IGNORE cues?" using a 5-point likert scale with the verbal anchors "Never" listed for 1 and "Always" listed for 5. These ratings were used to assess whether self-rated judgments of compliance were linked to objective assessments of cue influence (e.g., do individuals who report always ignoring cues display no cue influence).

2.2 Results and Discussion

For all experiments hit rates of one and false alarm rates of zero were replaced with 1-1/(2N) and 1/(2N) respectively, where N is the number of trials on which that proportion is based (Macmillan & Creelman, 2005). This was done to calculate Signal Detection measures that require using a reverse normal transform, which is undefined for perfect performance.

Criterion. See Table 1 for descriptive statistics for response rates, accuracy, and criteria. The first analysis simply investigated whether participants were differentially influenced by cues as a function of use vs. ignore instruction using the Signal Detection measure C. Recall that under the expectancy hypothesis the influence of cues should be largely minimized or completely eliminated when the cue is presented after the recognition probe. Changes in criteria (C) were assessed using a 2 X 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (Likely Old vs. Likely New). Critically, this initial analyses averaged across the three ignore test cycles and collapsed across cue lag (short, medium, long). There was no main effect of instruction, $F(1,25)=0.00$, $p=.98$, $\eta_p=.00$, suggesting that the mean criterion did not change as a function of use vs. ignore instructions. The main effect of cue type was significant, $F(1,25)=70.94$, $p<.001$, $\eta_p=.74$ demonstrating a large effect size with participants responding more liberally under a Likely Old vs. Likely New cue (-0.55 vs. 0.22). However, this main effect was conditioned by a significant interaction between instruction and cue type, $F(1,25)=31.74$, $p<.001$, $\eta_p=.56$. Follow up post-hoc tests (Tukey's HSD) revealed a significant difference between Likely Old and Likely New cues under use instructions (-0.72 vs. 0.39, $p<.001$). Critically, although the magnitude of the effect was smaller, the difference between Likely Old vs. Likely New cues was also significant displaying a large effect size (Cohen's d=1.36) when participants were instructed to ignore cues (-0.39 vs. 0.05, $p<.001$).

This initial finding that participants remained strongly influenced by cues during ignore instructions seems to suggest that the influence of cues was not eliminated when the cue was presented after the recognition probe. However, under the expectancy hypothesis a critical prediction was that the effect of the cues should diminish as the lag between the probe and cue onset increased. That is, when the cue lag is short the cue likely appears before participants have completed their covert recognition judgment and therefore should exert a larger influence resulting in larger criteria shifts. In contrast, as cue lag increases a greater proportion of covert recognition decisions should be completed and hence the influence of the cue itself on processing should presumably be lessened. To examine this prediction we assessed criterion (C) for ignore test cycles using a 2 X 3 repeated measures ANOVA with factors of cue type (Likely Old vs. Likely New) and cue lag (short, medium, vs. long). As expected based on the previous analysis, results revealed a significant main effect of cue type, $F(1,26)=46.82$, $p<.001$, $\eta_p=.64$ with participants responding more liberally under a Likely Old vs. Likely New cues (-0.40 vs. 0.05). The main effect of cue lag was not significant, $F(2,52)=1.05$, $p=.36$, $\eta_p=.04$, simply indicating that the average criterion did not differ across the lags. Finally, the interaction between cue type and cue lag was also not significant, $F(2,52)=1.13$, $p=.33$, $\eta_p=.04$ (See Figure 5). Thus, participants were unable to ignore the lagged cues during the ignore blocks and moreover the influence of the cues was not heightened on short relative to long lagged durations. This challenges the expectancy account because even when the cue no longer served a preparatory role, participants were still influenced and increasing cue lags did not diminish this influence when measured by shifts in criteria.

However, it is important to note that optimal criterion shifting is dependent on baseline recognition skill. Observers with higher baseline accuracy should shift less in response to cues than those with lower baseline accuracy (Selmeczy & Dobbins, 2013). To account for differences in baseline skill and increase power to detect an interaction between cue type and cue lag, another measure called C-prime (C’) was also examined. C-prime (C’) is a criterion measure normalized by accuracy and was calculated by dividing each participants’ criterion (C) by his/her dprime (d’) during uncued/baseline trials (Macmillan & Creelman, 2005). Critically, when rerunning the 2 X 3 repeated measures ANOVA with factors of cue type (Likely Old vs. Likely New) and cue lag (short, medium, vs. long) using C-prime, the interaction between cue lag and cue type was not significant, $F(2,50)=0.58$, $p=.56$, $\eta_p=.02$. Thus, even when controlling for differences in baseline recognition skill the influence of cues was not greater on short relative to long lagged durations.

Table 1. Average response rates, accuracy, and criterion with standard deviations in parenthesis (Experiment 1)

	HR		**CR**		**d'**		**C**	
Uncued/Baseline	0.83	(0.09)	0.72	(0.14)	1.66	(0.54)	-0.18	(0.29)
Cued	0.84	(0.1)	0.78	(0.1)	1.96	(0.63)	-0.13	(0.32)
Likely Old Cue	0.91	(0.09)	0.52	(0.27)	1.60	(0.79)	-0.74	(0.56)
Likely New Cue	0.62	(0.26)	0.87	(0.09)	1.70	(0.81)	0.39	(0.46)

Ignore Instructions

	HR		**CR**		**d'**		**C**	
Uncued/Baseline	0.77	(0.2)	0.66	(0.18)	1.33	(0.75)	-0.21	(0.51)
Short Cue Delay								
Cued	0.79	(0.14)	0.72	(0.14)	1.55	(0.59)	-0.12	(0.41)
Likely Old Cue	0.82	(0.13)	0.57	(0.21)	1.27	(0.71)	-0.39	(0.49)
Likely New Cue	0.69	(0.19)	0.77	(0.14)	1.43	(0.64)	0.12	(0.48)
Medium Cue Delay								
Cued	0.80	(0.14)	0.70	(0.15)	1.53	(0.52)	-0.18	(0.4)
Likely Old Cue	0.83	(0.14)	0.59	(0.2)	1.34	(0.59)	-0.42	(0.48)
Likely New Cue	0.71	(0.18)	0.74	(0.15)	1.38	(0.63)	0.03	(0.45)
Long Cue Delay								
Cued	0.80	(0.13)	0.70	(0.13)	1.48	(0.57)	-0.18	(0.35)
Likely Old Cue	0.82	(0.13)	0.60	(0.18)	1.33	(0.58)	-0.39	(0.45)
Likely New Cue	0.72	(0.17)	0.73	(0.13)	1.34	(0.66)	0.01	(0.39)

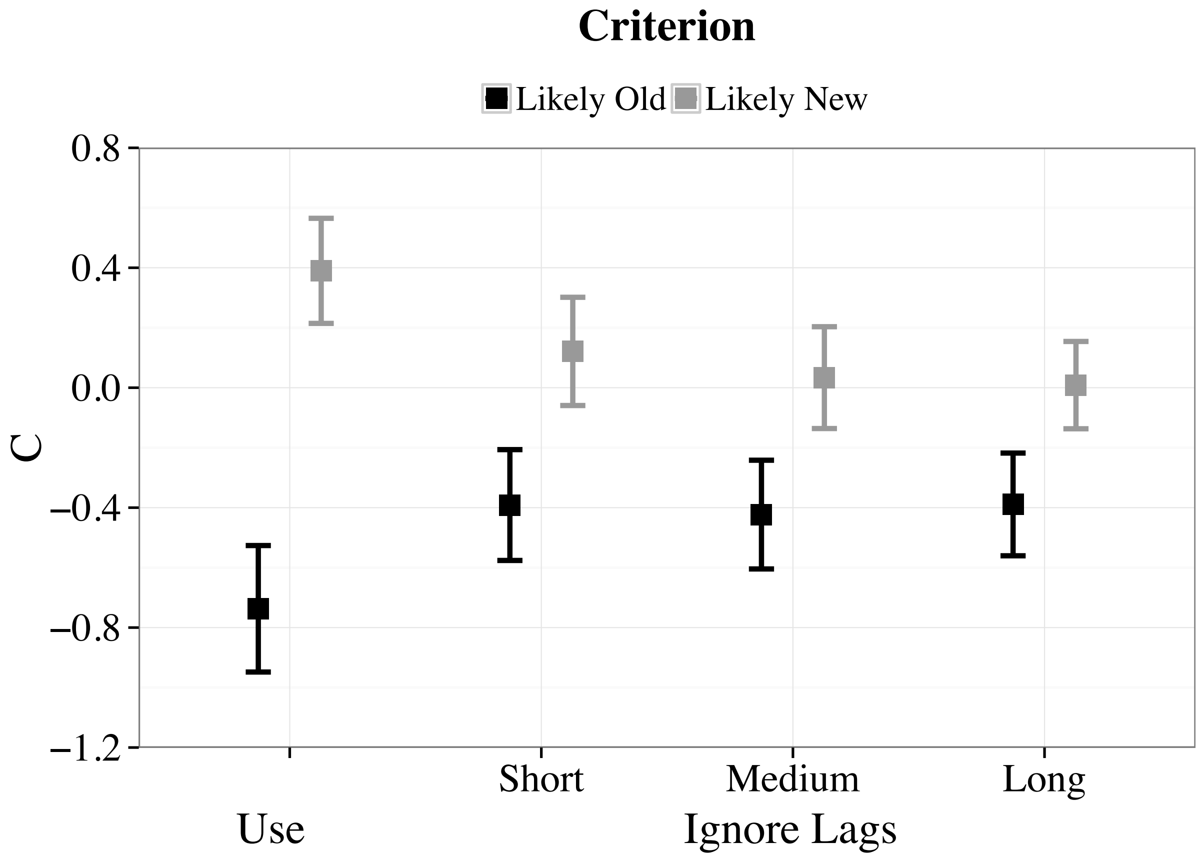


Figure 5. Criterion Experiment 1. The graph depicts average criterion (C) under Likely Old (black) and Likely New (gray) cues during use instructions, and short, medium, and long cue lags during ignore instructions. Error bars represent 95% confidence intervals for the mean.

Confidence. Although the absences of lag effects in the criterion analysis failed to support the expectancy hypothesis, confidence may be a more sensitive indicator of cueing effects because it is more finely grained and more dependent upon introspection. Recall that participants used a 6-point confidence scale (50-100% in 10% increments) after each recognition decision. While simple dichotomous old/new reports may not be influenced by cue lag, subtle differences may be captured through this more finely grained scale. Additionally, confidence is a more introspective judgment regarding one's performance (a.k.a metacognition) and it may be the case the subtle cueing influences would be reflected in one's self assessment of the ease of a judgment without necessarily showing up as an influence in the direction of judgment. For the analyses below, one subject was removed due to reporting 100% confidence for all but two responses; however, excluding this participant did not change the overall findings. Additionally, all confidence analyses focused on correct responses (i.e., hits and correct rejections) since incorrect responses were relatively infrequent and provided much less reliable estimates. See Table 2 for descriptive statistics for confidence.

Table 2. Average confidence with standard deviations in parenthesis (Experiment 1)

Use Instructions

	Hits		**Correct Rejections**	
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

	Hits		**Correct Rejections**	
Uncued/Baseline	86.19	(9.92)	79.03	(12.33)
Short Cue lag				
Likely Old Cue	86.09	(8.16)	74.03	(13.85)
Likely New Cue	85.30	(9.93)	79.68	(10.68)
Medium Cue lag				
Likely Old Cue	86.98	(7.31)	76.56	(12.10)
Likely New Cue	86.72	(9.70)	80.00	(10.54)
Long Cue lag				
Likely Old Cue	86.65	(8.30)	77.52	(11.03)
Likely New Cue	85.66	(9.33)	80.00	(10.38)

Hits

Changes in hit confidence were assessed using a 2 X 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (Likely Old vs. Likely New). Once again this initial analyses averaged across all three ignore cycles and collapsed across cue lag conditions. Results revealed no main effect of instruction $F(1,24)=0.18$, $p=.67$, $\eta_p=.007$, suggesting overall hit confidence remained the same for use vs. ignore instructions. The main effect of cue type was significant $F(1,24)=6.23$, $p=.01$, $\eta_p=.20$, with higher confidence during Likely Old relative to Likely New cued trials (87.20 vs. 85.40). The interaction between instruction and cue type was marginally significant $F(1,24)=2.96$, $p=.10$, $\eta_p=.11$. Although the interaction was only marginally significant, post-hoc tests (Tukey's HSD) demonstrated that the effect of cue type was more apparent under use instructions (Likely Old 87.37 vs. Likely New 84.60, $p=.02$), whereas under ignore instructions average hit confidence remained nearly identical as function of cue type (Likely Old 86.84 vs. Likely New 86.20, $p=.91$). Thus, hit confidence does not seem to be affected when instructed to ignore cues (See Figure 6, top panel).

Correct Rejections

Turning to correct rejection confidence, the 2 X 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (Likely Old vs. Likely New) revealed no significant main effect of instruction, $F\ (1,25)=0.320$, $p=.58$, $\eta_p=.01$. The main effect of cue type was significant, $F(1,25)=48.77$, $p<.001$, $\eta_p=.66$, with higher confidence during Likely New vs. Likely Old cues (80.43 vs. 74.90). However these main effects were conditioned by a significant interaction between instruction and cue type, $F(1,25)=11.04$, $p=.002$, $\eta_p=.31$. Follow up post-hoc tests (Tukey's HSD) revealed that during use instructions there was a significant difference between Likely New vs. Likely Old correct rejection confidence (80.99 vs. 73.71, *p*<.001). During ignore instructions, there was a smaller but also significant difference between Likely New vs. Likely Old correct rejection confidence (79.89 vs. 76.04, *p*<.001). Thus, in contrast to hit confidence, correct rejection confidence was influenced by cues during ignore instructions.

Critically, to determine whether cue lag had an affect during ignore instructions a 2 X 3 repeated measured ANOVA with factors of cue type (Likely Old vs. Likely New) and cue lag (short, medium, vs. long) was conducted. As expected given the prior analysis, the main effect of cue type was significant, *F*(1,25)=26.62, *p*=<.001, η_p=.51, with higher confidence on Likely New relative to Likely Old cued trials (79.89 vs. 76.04). The main effect of cue lag was also significant, *F*(2,50)=5.28, *p*=.008, η_p=.17; however, these main effects were conditioned by a significant interaction between cue type and cue lag, *F*(2,50)=3.13, *p*=.05, η_p=.11. Critically, follow up simple effects analyses revealed a significant linear trend for cue lag under Likely Old, *F*(1,25)=7.60, *p*=.01, η_p=.23, but not Likely New cues, *F*(1,25)=0.29, *p*=.60, η_p=.01 (See Figure 6, bottom panel). Thus, in contrast to the criterion and accuracy analyses, the influence of cues was affected by cue lag for correct rejection confidence and this effect seemed to be driven by Likely New or invalid cues. Importantly, these results are in line with the expectancy hypothesis such that the influence of cues was larger during short cue lags relative to longer cue lags.

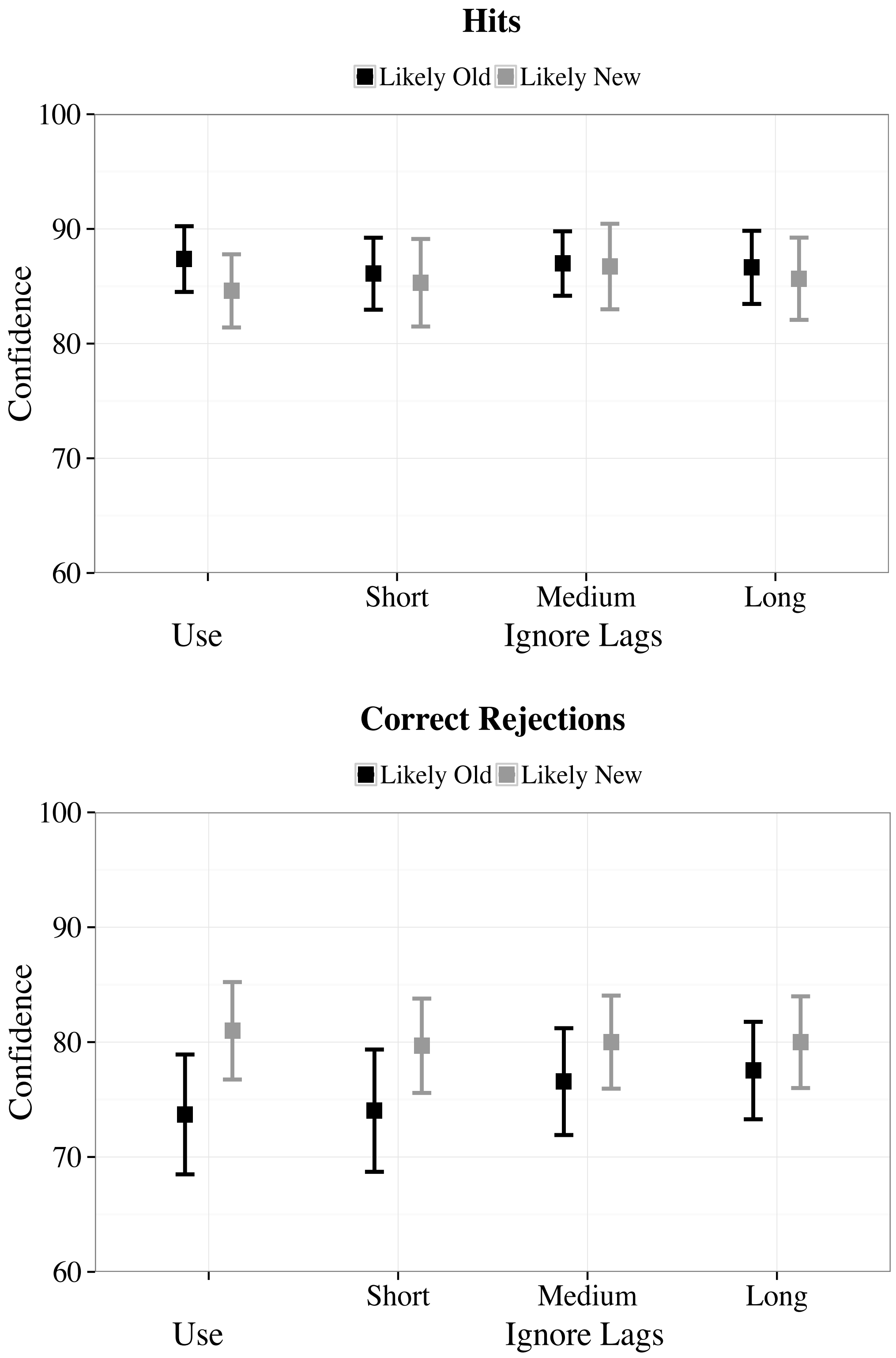


Figure 6. Confidence Experiment 1. The graph depicts average confidence for hits (top panel) and correction rejections (bottom panel) under Likely Old (black) and Likely New (gray) cues during use instructions, and short, medium, and long cue lags during ignore instructions. Error bars represent 95% confidence intervals for the mean.

Reaction Time. The next analyses turned to examining reaction times. While the criterion and accuracy data seemed to suggest there is no effect of cue lag, the confidence data did find an effect in the direction predicted by the expectancy hypothesis (i.e., larger influence of cues during short lags vs. long lags) at least for correct rejections. Like the confidence data, reaction time may prove to be more sensitive to the effects of cue lag.

Medians, as opposed to means, were used for estimating reaction time since response time distributions are positively skewed and the median is less influenced by outliers. Therefore, all the reaction time results reported below will consider the average of median reaction times across participants. Again, the analyses focused on correct responses (i.e., hits and correct rejections) since errors occurred less frequently and provided less reliable estimates. See Table 3 for descriptive statistics in reaction time for old/new judgments.

Table 3. Average median reaction time in seconds for old/new judgments with standard deviations in parenthesis (Experiment 1)

Use Instructions

	Hits		**Correct Rejections**	
Uncued/Baseline	1.60	(0.46)	1.78	(0.54)
Likely Old Cue	1.50	(0.36)	2.57	(1.60)
Likely New Cue	2.27	(0.98)	1.64	(0.41)

Ignore Instructions

	Hits		**Correct Rejections**	
Uncued/Baseline	1.75	(0.51)	2.17	(0.93)
Short Cue lag				
Likely Old Cue	0.64	(0.29)	0.98	(0.52)
Likely New Cue	0.73	(0.24)	0.75	(0.41)
Medium Cue lag				
Likely Old Cue	0.49	(0.23)	0.59	(0.28)
Likely New Cue	0.54	(0.25)	0.50	(0.20)
Long Cue lag				
Likely Old Cue	0.47	(0.16)	0.57	(0.27)
Likely New Cue	0.53	(0.22)	0.49	(0.19)

Reaction Time for Old/New Judgment

Hits

Reaction time analyses focused on ignore test cycles only, since the comparison between use and ignore study cycles is not meaningful due to differences in the timing of the cue. During use instructions, the cue appeared on screen for one second in isolation followed by the old/new prompt and recognition probe. In contrast, during ignore instructions the cue appeared after the recognition probe at varying cue lags and it remained on screen for one second before participants could key in their old/new decision (see Figure 4). Therefore, reaction time is expected to be much faster during ignore test cycles since participants viewed the probe before being explicitly prompted for their old/new decisions. Focusing only on ignore test cycles, the following analyses examined whether there was an influence of cue type and critically whether a heightened influence of cues would be observed during shorter cue lags.

The 2 X 3 repeated measured ANOVA with factors of cue type (Likely Old vs. Likely New) and cue lag (short, medium, and long) revealed a significant main effect of cue type, $F(1,26)=8.17$, $p=.008$, $\eta_p=.23$, with faster reaction times during Likely Old relative to Likely New cued trials (0.52 vs. 0.60). The main effect of cue lag was also significant, $F(2,52)=25.85$, $p<.001$, $\eta_p=.50$. Post-hoc tests (Tukey's HSD) showed that reaction time was significantly slower during short (0.69 seconds) relative to medium (0.51 seconds, $p<.001$) or long cue lags (0.50 seconds, $p<.001$). The effect of cue lag is not necessarily surprising since during short cue lags participants only processed the recognition probe for 1.3 seconds (.30 seconds for the recognition probe in isolation, 1 second for the cue and the recognition probe together) before the old/new prompt appears. In contrast, for the medium and long lags on average participants viewed the recognition probe for 2.73 and 5.12 seconds respectively before the old/new prompt appears.

Therefore, the main effect of cue lag serves as a good manipulation check to verify that during the short cue lag participants were still processing the probe when the cue appeared. Finally, the cue type by cue lag interaction was not significant, $F(2,52)=0.25$, $p=.70$, $\eta_p=.01$, suggesting that the effect of cues did not vary as a function of cue lag.

Correct Rejections

For correct rejections the 3 X 2 repeated measures ANOVA with factors of cue lag (short, medium, vs. long) and cue type (Likely Old vs. Likely New) revealed a main effect of cue type, $F(1,26)=12.32$, $p=.002$, $\eta_p=.32$, with faster reaction times during Likely New vs. Likely Old cues (0.58 vs. 0.72). The main effect of cue lag was also significant, $F(2,52)=24.75$, $p<.001$, $\eta_p=.57$. Once again, post-hoc tests (Tukey's HSD) showed that reaction time was significantly slower during short (0.87 seconds) relative to medium (0.55 seconds, $p<.001$) or long cue lags (0.53 seconds, $p<.001$). Again, this suggests cue lag was effective in manipulating whether the cue was present while probe processing was still ongoing. The two-way interaction between cue lag and cue type did not reach significance, $F(2,52)=2.29$, $p=.11$, $\eta_p=.08$. Thus, these results suggest that correct rejection reaction time was similarly influenced by cues across all cue lag conditions.

Reaction Time for Confidence Judgment

While median reaction times for old new decisions were influenced by cue type, once again the effects of cues did not diminish across cue lags during ignore instructions. This result is consistent with the criterion data and reinforce the conclusion that the dichotomous old/new judgments, while sensitive to the cues under the ignore instructions, were not sensitive to the lag manipulation. However, as the secondary confidence judgments appeared to demonstrate lag effects under the ignore instructions, I further assessed whether an influence of lag was also present in the reaction time of these confidence reports, once again focusing on the median reaction time of each condition for each participant. See Table 4 for descriptive statistics of reaction time for confidence judgments.

Table 4. Average median reaction time in seconds for confidence judgments with standard deviations in parenthesis (Experiment 1)

Use Instructions

	Hits		**Correct Rejections**	
Uncued/Baseline	0.73	(0.44)	0.79	(0.44)
Likely Old Cue	0.66	(0.36)	1.07	(0.72)
Likely New Cue	0.95	(0.64)	0.77	(0.43)

Ignore Instructions

	Hits		**Correct Rejections**	
Uncued/Baseline	0.67	(0.38)	0.63	0.42
Short Cue lag				
Likely Old Cue	0.50	(0.34)	0.68	(0.45)
Likely New Cue	0.61	(0.45)	0.52	(0.31)
Medium Cue lag				
Likely Old Cue	0.49	(0.33)	0.54	(0.40)
Likely New Cue	0.47	(0.31)	0.51	(0.32)
Long Cue lag				
Likely Old Cue	0.49	(0.33)	0.57	(0.40)
Likely New Cue	0.53	(0.38)	0.52	(0.33)

Hits

I was critically interested in examining whether the influence of cues changed across cue lags. The 3 X 2 repeated measures ANOVA with factors of cue lag (short, medium, vs. long) and cue type (Likely Old vs. Likely New) revealed a significant main effect of cue type, $F(1,26)=4.29$, $p=.05$, $\eta_p=.14$, with faster reaction times during Likely Old vs. Likely New cues (0.49 vs. 0.54). The main effect of cue lag was also significant, $F(2,52)=6.15$, $p=.004$, $\eta_p=.19$. Post-hoc tests (Tukey's HSD) showed that reaction time was significantly slower during short (0.56 seconds) relative to medium cue lags (0.48 seconds, $p=.003$), and the difference between short and long cue lags approached significance (0.51 seconds, $p=.09$). Overall this suggests that even during confidence judgments reaction time was affected and took longer when the cue lag was short. Critically, the two-way interaction between cue lag and cue type was also significant, $F(2,52)=3.65$, $p=.03$, $\eta_p=.12$. Follow up simple effects analyses revealed a significant linear trend for cue lag under Likely New, $F(1,26)=4.93$, $p=.03$, $\eta_p=.23$, but not Likely Old cues, $F(1,26)=0.56$, $p=.46$, $\eta_p=.15$ (See Figure 7, top panel). Importantly, these results are in line with the expectation hypothesis since the influence of cues diminishes as cue lag increases and this seems to be due to the timing of the presentation of invalid (Likely New) cues.

Correct Rejections

The 3 X 2 repeated measures ANOVA with factors of cue lag (short, medium, vs. long) and cue type (Likely Old vs. Likely New) revealed a main effect of cue type, $F(1,26)=9.47$, $p=.004$, $\eta_p=.27$, with faster reaction times during Likely New vs. Likely Old cues (0.52 vs. 0.59). Cue lag was also significant, $F(2,52)=9.24$, $p<.001$, $\eta_p=.26$ and post-hoc tests (Tukey's HSD) showed that reaction time was significantly slower during short (0.59 seconds) relative to medium (0.52 seconds, $p<.001$) and long cue lags (0.54 seconds, $p=.02$). Again, this suggests that even during confidence judgments reaction time was affected and took longer when the cue lag was short. Critically, the two-way interaction between cue lag and cue type was also significant, $F(2,52)=7.04$, $p=.001$, $\eta_p=.21$. Follow up simple effects analyses revealed a significant linear trend for cue lag under Likely Old, $F(1,26)=10.20$, $p=.003$, $\eta_p=.28$, but not Likely New cues, $F(1,26)=0.29$, $p=.60$, $\eta_p=.01$ (See Figure 7, bottom panel). Importantly, these results are consistent with the expectation hypothesis since the influence of cues diminishes as cue lag increases and this seems to due to the effect of invalid (Likely New) cues.

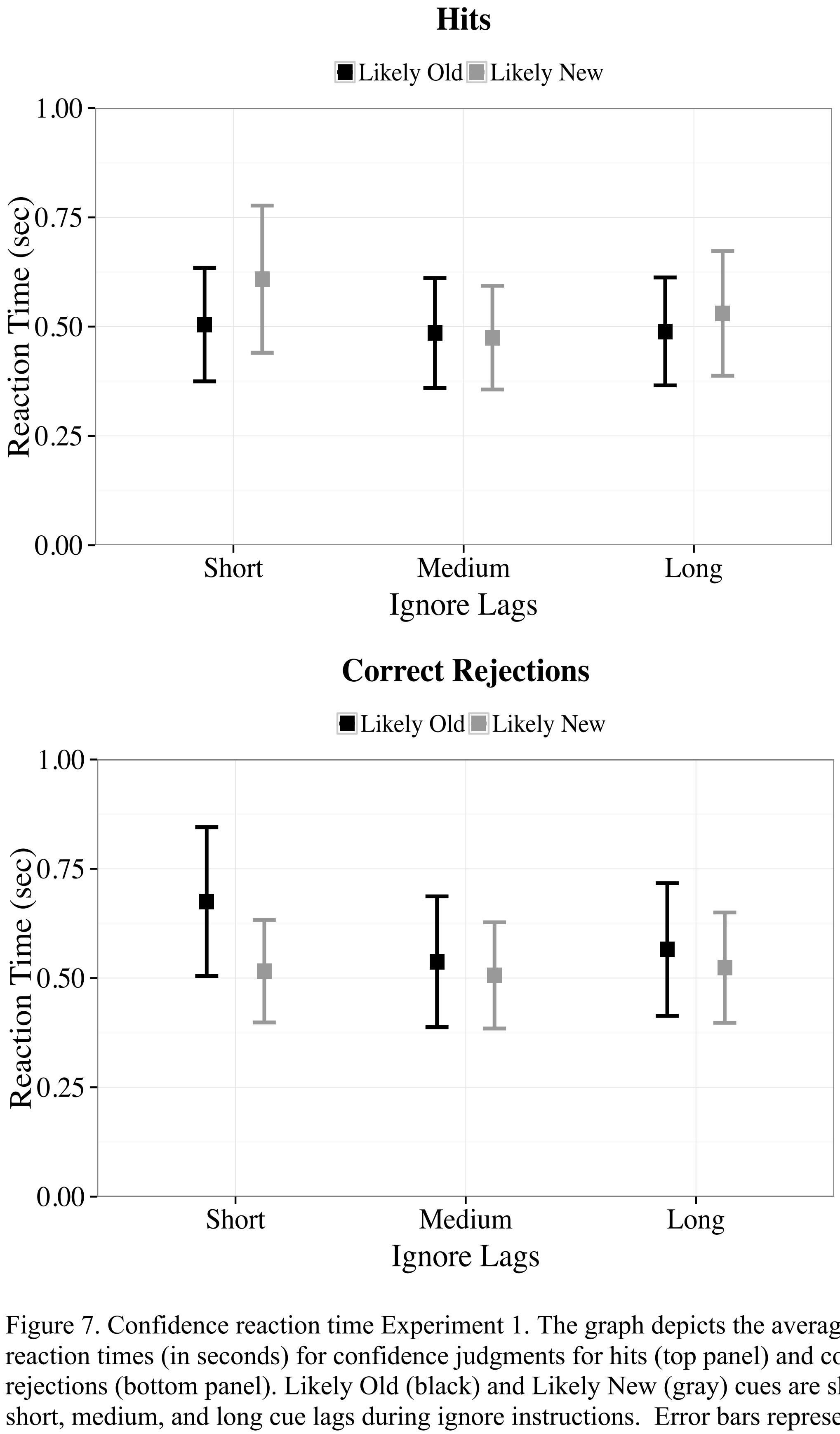


Figure 7. Confidence reaction time Experiment 1. The graph depicts the average of median reaction times (in seconds) for confidence judgments for hits (top panel) and correction rejections (bottom panel). Likely Old (black) and Likely New (gray) cues are shown for short, medium, and long cue lags during ignore instructions. Error bars represent 95% confidence intervals for the mean.

Additional Analysis. Additional analyses briefly examined performance on catch trials and questionnaire responses. Recall that during some trials participants also completed catch trials where they had to indicate whether the cue was Likely Old or Likely New on the preceding trial. This was done in order to ensure that participants processed the cues during ignore instructions. Proportion correct on catch trials was relatively high overall (*M* =.88, *SD*= 0.12). Additionally, catch trial performance was not significantly different as a function of cue lag (short, medium, vs. long) (means of .89, .88, vs. .89 respectively, *F*(2,52)=0.27, *p*=.77, η_p=.01). Although it is clear that participants did not ignore cues based on the criteria analyses above, the relatively high performance on catch trials serves to further indicate that participants were reading the cues. However, since catch trial performance varied between individuals, it could be the case that those individuals who performed worse on catch trials, and were less likely to have processed the cue, may have also been less influenced by the cues under ignore instructions. However, the correlation between catch trial performance and the degree of criteria shift (measured as the difference in criterion (C) between Likely New vs. Likely Old cued trials) was not significant for any of the cue lag conditions (*p*s>.15).

Finally, the relationship between self-reported measures of use or ignore instruction compliance and actual measures of cue influence was also examined. Using a 5-point likert scale (1-Never followed instructions, 5-Always followed instructions), participants on average reported 3.26 (*SD*=1.02) for use instructions and 3.33 (*SD*=1.07) for ignore instructions. Thus, it appears as though participants reported following instructions moderately and to a similar extent in use and ignore instructions. Using simple correlations, I assessed whether there was a relationship between objective measures of cue influence (measured as Likely New C minus Likely Old C) and participants' self reports of instruction use. However, this relationship was not significant for either use (r=.09, p=.65) or ignore instructions (short lag: r=-.09, p=.65; medium lag: r=-.29, p=.15.; long lag: r=-.10, p=.60). Additionally, I also assessed cue influence using C-prime (C') in order to account for individual differences in baseline accuracy. However, once again the relationship between cue influence (Likely New C' minus Likely Old C') and self reports of instruction compliance were not significant for use (r=-.04, p=.86) or ignore instructions (short lag: r=-.01, p=.97; medium lag: r=-.22, p=.30; long lag: r=-.03, p=.89).

2.3 Discussion

Experiment 1 investigated whether participants could ignore reliable environmental cues that were presented after a recognition probe. Under an expectancy hypothesis, participants should be able to ignore lagged cues since they no longer serve a preparatory role and cannot drive expectancy based processing of the probe. More specifically, I predicted that lagged cues should not have an influence when presented after a covert recognition judgment has likely occurred (i.e., long cue lag condition). In contrast, lagged cues may still exert an influence if presented before a covert judgment has been reached (i.e., short or medium cue lag conditions).

Results revealed that participants were not able to ignore reliable environmental cues, despite being presented after the recognition probe. While criteria shifted to a lesser degree during ignore relative to use instructions, large shifts were still observed during ignore instructions. Thus, clear cue influences were present in the decision bias measure during post cueing. However, the size of this effect was not dependent upon the lag with similar induced shifts in C (and C') across lags. This insensitivity was also mirrored in the reaction time of the old/new judgments under ignore instructions. Although responding was reliably quicker when it was consistent with the cue, this effect was also insensitive to the lag manipulation.

These initial results demonstrated a robust cue influence under ignore instructions but they were not consistent with the expectancy model because they were not modulated by the lag manipulation. However, further analyses of confidence and confidence reaction time did yield some support for the expectancy account. Specifically, the influence of cues diminished at longer cue lags particularly for correct rejections, and this diminished influence was mostly driven by invalid cues. That is, invalid cues tended to reliably diminish confidence and slow the reporting of confidence at the short lag, with this effect lessening with increased cue lags. Additionally, I just briefly want to note that prior work has demonstrated that cues more heavily affect correct rejection as opposed to hit confidence (for more details please see Jaeger, Cox, & Dobbins, 2012a) and therefore it is not necessarily surprising that in the current study the influence of cues was larger for correct rejections.

Overall the results indicate an effect of cueing even when it occurs after probe presentation, and this effect is sensitive to the timing of the cue when measured by introspective reports of confidence. Nonetheless, it is unclear why participants could not fully ignore cues at long cue lags if the expectancy account is correct because these cues appeared after the majority of recognition decisions should have been completed based on individually titrated reaction times from the first test cycle. If one takes the presence of lag effects in the confidence rating data, and the presence of an overall effect in the old/new criterion, as supportive of the expectancy account, then it must be concluded that subjects did not fully commit to a judgment on a large majority of trials even during the longest lag. That is, participants know that reliable information will be presented and, despite the instructions to ignore it, they may not fully commit to a decision until they are explicitly prompted allowing the cue to still exert an influence. It is perhaps important to note that this problem cannot be addressed by having the participants commit an explicit initial decision prior to the cue's appearance, since it would then be trivial for them not to alter the overt judgment. Indeed, altering the initial judgment even if given a possibility to do so would simply mean they failed to understand the instruction to ignore the cue. Given this, future experiments may benefit from encouraging rapid covert judgments with interspersed trials during which participants were unexpectedly prompted to make a very quick recognition judgment before any cue was presented. Additionally, the failure to do so would result in a negative consequence (e.g., long time out with blank screen). Using this manipulation, participants would presumably learn to commit covertly to judgments early on in order to prevent the negative experience of a time out and lag effects might then spread to the old/new judgments as well as being reflected in the secondary confidence judgment data.

Chapter 3

Experiment 2

Participants' decision criteria were clearly affected by predictive environmental cues even when they were instructed to ignore them. However, it is currently unclear whether participants have any awareness or insight into the fact that their responses are being somehow influenced despite their efforts to ignore the cues. The above interpretation of an automatic cue influence may blur a distinction between an influence that is automatic and implicit versus one that may occur largely automatically, but of which one may have awareness ex post facto. This distinction is potentially important since one can attempt to correct an influence after it has occurred by taking some form of a countermanding strategy. Thus, although automatic processes have been described as unconscious or occurring outside of observer's explicit awareness (Posner & Snyder, 1975), this does not mean that observer's cannot become aware of an automatic influence after it has occurred. For example, during the Stroop (1935) Task the mistake of accidentally responding red when seeing the word RED in blue ink may become incredibly obvious after it has occurred. This is a case where there is an automatic influence, but also ex post facto awareness of that influence. Although the prior work and current findings converge in suggesting that the cues automatically bias recognition judgments to some extent, I currently have not applied the most direct test of whether or not observers are completely unaware of this influence. For example, observers may be automatically influenced by the cues and then attempt to correct for this influence by altering their response strategy. Under this account the observed criterion shifts during ignore instructions would not reflect an entirely implicit biasing process, but would instead reflect the fact that the observers' correction strategies underestimated the degree of initial influence or were somehow otherwise ineffective.

This distinction might be possible to detect if one examined the awareness of potential cue influence more directly. To do this, Experiment 2 directly asked participants to rate the degree to which they felt they might have been influenced by the cues during both use and ignore instructions. If participants demonstrate awareness of the cues' influence a positive relationship should be observed such that as cue influence ratings increase, shifts in bias between Likely Old and Likely New cued trials should also increase. In contrast, if participants were unaware of the cues' influence, then they would not report being influenced even when reliable cue-induced shifts were occurring.

Additionally, in the current experiment a secondary question was whether individual differences in criterion regulation are related to measures of cognitive control. Despite the assumption in the literature that criterion regulation is controlled, very little work has actually tested whether a relationship exists between adaptive recognition biases and measures of cognitive control (but see Dobbins & Kroll, 2005) and prior work does not examine whether individual differences in criterion regulation are related to measures of cognitive control (but see Konkel, Selmeczy, and Dobbins, 2014). Thus, the current experiment also assessed whether individual differences in criterion shifting were related to measures of attentional control (i.e., working memory) when participants were intentionally or unintentionally biasing their responding. Cognitive control was assessed using a working memory or complex span task, since complex span tasks are highly valid and reliable measure of working memory and predict performance on a whole host of higher order cognitive tasks (Engle & Kane, 2004; Engle, Kane, & Tuholski, 1999a; Engle, Tuholski, Laughlin, & Conway, 1999b). Additionally, a complex span task requires maintaining and updating relevant information and this may tap similar processes that occur during criterion regulation such as maintaining and shifting between multiple criterion locations and jointly considering two sources of information (cue and memory based) during judgments. Thus, if criterion regulation is a controlled process then a relationship may be observed between individual differences in the ability to capitalize on environmental cues and complex span performance during use instructions. Critically, while working memory plays an important role in maintaining goals or strategies, it is thought to be particularly important for maintaining goals while inhibiting interfering responses (Engle, 2002; Engle et al., 1999a; Kane, Conway, Hambrick, & Engle, 2007; Unsworth & Engle, 2007). In the case of the cueing paradigm, when participants are instructed to ignore cues they may have to engage in controlled processes to inhibit the automatic response associated with the cue (e.g. respond 'old' when see Likely Old cue), and instead focus on responding based on their own internal memory evidence. If this is the case then complex span may not only be associated with individual differences in the ability to capitalize on external cues, and/but also with individual differences in the ability to suppress cues.

3.1 Methods

Participants. Experiment 2 included 25 participants recruited from the Washington University Experimetrix pool (average age=19.32, 13 females) who were paid $10 or given one course credit per hour of 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 Experiment 1. An additional OSPAN task was implemented using E-prime (Psychology Software Tools; www.pstnet.com) during which responses were made via mouse click.

Participants completed a total of 4 study/test cycles and were encouraged to take a brief break between each study/test cycle. During study participants performed a syllable counting task (i.e., does this word contain 1, 2, 3 or more syllables?) on a list of serially presented words (75 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) (75 old items, 75 new items). The majority of test trials were preceded by a 75% reliable cue (referred to as hints to the participants) reading Likely Old or Likely New that appeared 1 second prior to the recognition probe (120 cued items-60 old, 60 new), while other trials were uncued or baseline trials (30 uncued/baseline items-15 old, 15 new). Participants were explicitly informed about the reliability of the cue. For the first and third study/test cycles participants were instructed to use cues, while during the second and fourth study/test cycles participants were instructed to ignore cues. The order of use vs. ignore instructions was fixed such that participants' initial experience was to actively use the cues. This was done to help illustrate the utility of the cues before having them attempt to ignore them. In contrast to the prior experiment where participants made confidence judgments, they instead provided a subjective cue influence rating using a 6-point scale ranging from 1-6 with the verbal anchors "not at all" placed at the 1 value and "completely" placed at the 6 value.

When instructed to use cues instructions read:

> "You will be asked to make a rating about how much your response was influenced by the hint. For example, the hints can influence your old/new response or the speed of confidence of your decision."

When instructed to ignore cues instructions read:

> "Even though you are not to use the hints at all, you may accidentally be influenced. For example, the hint may accidentally influence the speed or confidence of your response. Additionally, you might respond old or new to an item and only then realize after the fact that you may have been influenced by the hint...Please try and rate your degree of

> influence as accurately as possible and indicate anytime the hint may have accidentally influenced any aspect of your response. However, if you are not at all influenced by the hint then go ahead and rate your response as not at all influenced."

After recognition testing, participants completed the automated operation span (OSPAN) task (Unsworth, Heitz, Schrock, & Engle, 2005). During this task participants solved a series of simple math problems while trying to remember an unrelated set of letters. Participants were presented with a math equation (e.g., 2 + 5=?) followed by a number (e.g., 8), and indicated whether the number is the true or a false solution to the given math equation. Immediately after this, a single letter (e.g., H) was presented for participants to memorize. After a set of these trials varying in length (3-7), participants recalled the presented letters in correct order. Participants initially completed a practice phase and final scores were determined by summing all the perfectly recalled sets (for more details please see Unsworth et al., 2005).

3.2 Results and Discussion

Two participants only completed a single ignore study/test cycle due to a computer shut down. One participant's data were excluded due to near chance performance during baseline/uncued performance (d'<0.13) suggesting that he/she was not engaged during the encoding task, leaving 24 participants for analyses. However, removing this participant did not change any overall findings.

Additionally, as a warning to the reader in the current and several following experiments standard ANOVA analyses are conducted since they are conventionally used and easy to interpret. However, a mixed level modeling approach was also used for certain targeted questions for several reasons later outlined in the results section.

Criterion. See Table 5 for descriptive statistics for response rates, accuracy, and criteria. Changes in criteria were assessed with Signal Detection measure C using a 2 X 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (Likely Old vs. Likely New). Results revealed no main effect of instruction, $F(1,23)=0.01$, $p=.91$, $\eta_p=.00$, suggesting overall criteria did not change as a function of use vs. ignore instructions. The main effect of cue type was significant, $F(1,23)=69.92$, $p<.001$, $\eta_p=.75$, with participants responding more liberally under Likely Old vs. Likely New cues (-0.52 vs. 0.52). However, these main effects were conditioned by a significant interaction between instruction and cue type, $F(1,23)=9.15$, $p=.006$, $\eta_p=.28$. Follow up post-hoc tests (Tukey's HSD) revealed a significant difference between Likely Old and Likely New cues during use instructions (-0.62 vs. 0.61, $p<.001$). During ignore instructions, this difference between Likely Old vs. Likely New cues was smaller but also highly significant (-0.42 vs. 0.42, $p<.001$). Thus, once again participants were not able to completely eliminate the influence of external cues when instructed to do so.

Table 5. Average response rates, accuracy, and criterion with standard deviations in parenthesis (Experiment 2)

Use Instructions

	HR		**CR**		**d'**		**C**	
Uncued/Baseline	0.74	(0.12)	0.72	(0.14)	1.33	(0.6)	-0.03	(0.31)
Cued	0.79	(0.07)	0.79	(0.1)	1.68	(0.44)	0.00	(0.22)
Likely Old Cue	0.87	(0.07)	0.49	(0.2)	1.20	(0.59)	-0.62	(0.4)
Likely New Cue	0.55	(0.21)	0.88	(0.09)	1.49	(0.62)	0.61	(0.5)

Ignore Instructions

	HR		**CR**		**d'**		**C**	
Uncued/Baseline	0.71	(0.11)	0.71	(0.15)	1.16	(0.49)	0.00	(0.32)
Cued	0.76	(0.09)	0.77	(0.13)	1.55	(0.52)	0.02	(0.29)
Likely Old Cue	0.82	(0.09)	0.57	(0.23)	1.20	(0.59)	-0.42	(0.51)
Likely New Cue	0.59	(0.19)	0.83	(0.13)	1.33	(0.66)	0.42	(0.43)

Subjective Cue Influence Ratings. The main purpose of the current experiment was to assess whether participants were subjectively aware that their responses were influenced by cues even when they were attempting to ignore them. Two separate sets of analysis were conducted on the self-reported cue influence ratings. The first analysis simply examined average subjective cue influence ratings across instructions, cue types, and outcomes. The second analysis examined my targeted question, which was whether participants' could subjectively rate the degree of cue influence on a trial wise basis.

Average Cue Influence Ratings

This first analysis assessed which factors contributed to participants' subjective cue influence ratings. For ease of interpretation, separate analyses were conducted for use vs. ignore instructions. Starting with use instructions, influence ratings were assessed using a 2 X 2 X 2 repeated measures ANOVA with factors of cue type (Likely Old vs. Likely New), item status (old vs. new), and response ('old' vs. 'new'). Critically, the three way interaction between cue type, item status, and response was significant, $F(1,22)=7.80$, $p=.01$, $\eta_p=.26$ and therefore lower order interactions will not be reported. The top panels of Figure 8 demonstrate the significant three-way interaction. Focusing on the left-hand panel when participants' response was 'old', influence ratings were higher when the cue was Likely Old vs. Likely New. In other words, influence ratings were higher when the cue and participants' response aligned, and this was true when the actual item status was old (i.e., hits) represented in dark gray as well as when the actual item status was new (i.e., false alarms) represented in light gray. When focusing on the right-hand panel when participants' response was 'new', influence ratings were higher when the cue was Likely New vs. Likely Old. Once again, influence ratings were higher when the cue and participant's response aligned, and this was true when the actual item status was new (i.e., correct rejections) represented in light gray as well as when the actual item status was old (i.e., misses) represented in dark gray. The three-way interaction was likely significant due to the influence of cues being slightly larger for new items when the participant responds 'old' (i.e., false alarms). Thus, overall cues were subjectively rated as more influential when they cue aligned with participants' responses.

Turning to ignore instructions, the 2 X 2 X 2 repeated measures ANOVA with factors of cue type (Likely Old vs. Likely New), item status (old vs. new), and response ('old' vs. 'new') did not reveal a significant 3-way interaction, *F*(1,23)=1.03, *p*=.32, η_p=.04. However, comparing the bottom panels to the top panels of Figure 8 demonstrates that the pattern between use and ignore instructions was qualitatively similar. Namely, participants reported higher influence ratings when the cue aligned with their response and this pattern was the same for old as well as new items. Additionally, when comparing the top and bottom panels it is clear that influence ratings on average were lower during ignore relative to use instructions. This is consistent with the criteria analyses that demonstrated participants were less influenced by cues during ignore instructions. Since the 3-way interaction was not significant, the 2-way interactions are reported separately. Critically, the 2-way interaction between cue type and response was significant, *F*(1,23)=16.23, *p*<.001, η_p=.41. Post-hoc (Tukey's HSD) demonstrated that for 'old' responses influence ratings were higher under Likely Old vs. Likely New cues and this effect approached significance (2.19 vs. 1.42, *p*=.09). For 'new' responses influence ratings were significantly higher under Likely New vs. Likely Old cues (2.21 vs. 1.33, *p*=.05). Once again, these results demonstrate that participants rated higher influence of cues when the cue aligned with their response. The interaction between cue type and item status was also significant, *F*(1,23)=12.28, *p*=.001, η_p=.35. Post-hoc (Tukey's HSD) revealed that for old items influence ratings were higher under Likely New relative to Likely Old cues and this effect approached significance (1.80 vs. 1.64, *p*=.06). For new items, influence ratings were numerically higher under Likely Old relative to Likely New cues but this effect was not significant (1.89 vs. 1.74, *p*=.13). This result demonstrates that within a particular item type, influence ratings were slightly higher when the cue and item type did not match. Finally, the interaction between item status and response was also significant, *F*(1,23)=17.96, *p*<.001, η_p=.44. Post-hoc tests (Tukey's HSD) revealed that for old items participants' influence ratings were significantly higher when participants responded 'new' vs. 'old' (1.88 vs. 1.55, *p*=.001). For new items participants' influence ratings were not significantly different when they responded 'old' vs. 'new' (1.88 vs. 1.75, *p*=.36).

In summary, subjective ratings of cue influence were heavily affected by response outcomes such that participants reported being more influenced by cues when the cue aligned with their responses as opposed to when the cue disagreed with their responses. This occurred under both use and ignore instructions, although the effect was smaller under ignore instructions. Potential explanations for this effect will be further elaborated in the discussion section.

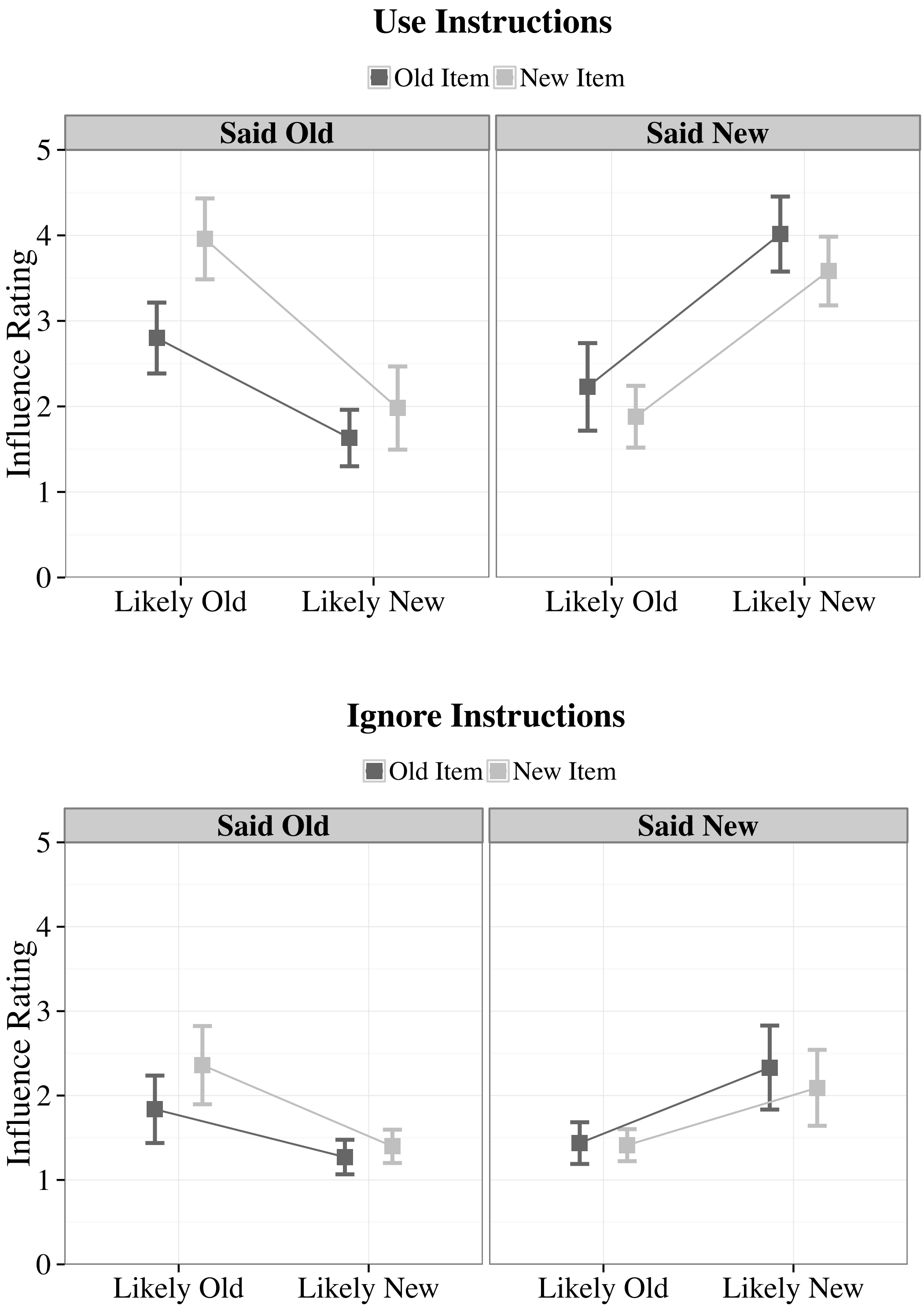


Figure 8. Influence ratings Experiment 2. The graph above depicts influence ratings for old (dark gray) and new (light gray) items under Likely Old and Likely New cues when participants responded ‘old’ (right panel) vs. ‘new’ (left panel). The top plots depict use instruction and the bottom plots depict ignore instructions. Error bars represent 95% confidence intervals for the mean.

Additional Analysis of Cue Influence Ratings

Although the analysis above demonstrated under what conditions individuals on average report the greatest dependence upon the cues, it did not directly ascertain whether reports of greater influence actually predicted greater correspondence between the cues and the observer's judgments on each trial. To accomplish this, I turned to mixed level modeling (MLM) for several reasons. First, MLM is robust to situations in which different individuals use different portions of the scales whereas ANOVA models require case-wise deletion of levels in which not all subjects provide a response. In this particular case, participants were less likely to use high influence ratings during ignore instructions and this would have resulted in a substantially large number of case-wise deletions using simple ANOVA models. Second, MLM allows one to model each individual response at the trial level, providing more powerful approach to test my question of interest.

Additionally, MLM allows one to jointly model fixed and random effects and to fully model the nested structure of repeated measure designs. More specifically, in a recognition paradigm each subject encounters hundreds of old and new test items and thus item types are a factor that is nested within subjects. Additionally, each subject may vary considerably in both the tendency towards old or new responses (bias) and the sensitivity to the item type distinction (accuracy). This type of variability is properly modeled as a random effect since the subjects are sampled from a larger population that varies in bias and accuracy. To capture this MLM jointly models the dependent variable as influenced by fixed and random components. This can be thought of as expanding upon a basic simple regression model (ignoring the nesting of trials within subjects) including all trial level data where the dependent variable response (coded 1-old, 0-new) is predicted only from a single fixed effect of item status (coded 1-old, 0-new). In this case, a significant positive estimate of item status would indicate a higher ‘old’ rate for old vs. new items, or in other words this estimate captures recognition accuracy. However, each participant has hundreds of trials contributing to the analysis, and this model does not account for variability that may be due to participants. In other words, this model assumes a single intercept and slope of item status for all participants and does not capture any variability between participants. To account for this, the model could be extended to include random effects such that variability due to individual differences across participants is appropriately modeled as explained variance (for more details please see Zuur, Ieno, Walker, Saveliev, & Smith, 2009). For example, when modeling participant as random intercept the intercept is allowed to vary across participants. This means the baseline probability of responding ‘old’ when item status is 0 (i.e., new) is allowed to vary across individuals, reflecting individual differences in response bias. Additionally, a random slope component can also be included allowing the slope of item status to vary across participants. This means the relationship between response and item status is also allowed to vary across participants, reflecting individual differences in response accuracy. Thus, a mixed level model predicting response using a fixed effect of item status, random intercept of participant, and random slope of item status allows one to assess the effect of item status while appropriately accounting for variance due to individual differences in response bias and accuracy (see Wright, Horry, & Skagerberg, 2009 for more details). Importantly, this simple model can be extended to assess the fixed effects of other variables, as I outline below.

All mixed level modeling was done with R (version 3.1.0) using the lme4 package (version 1.0-4). MLM was done using linear probability models since they provide solutions that are very close to those of logistic regression and provide easily interpretable parameters (i.e., they indicate how the variables influence the probability of response). Using a linear probability model I predicted response (coded 1-old, 0-new), using the fixed effects item status (coded 1-old, 0-new), instruction (coded 1-use, 0-ignore), cue type (coded 1-Likely Old, 0-Likely New), and cue influence rating (ranging from 1-6). Additionally, a random intercept and slope component was included such that the intercept and slope of items status was allowed to vary across participants in order to account for individual differences in response bias (i.e., the probability of responding 'old') and accuracy respectively (Wright et al., 2009). Initially the full model was fit including all main effects and interactions. The final model was determined by systematically removing the highest order interactions until there was a significant decrease in fit using chi-squared tests of log-likelihoods. Interactions at the same level were removed in order of least to most significant. Coefficient estimates were tested using the Satterthwaite approximation for degrees of freedom.

The full model included all possible main effects and interactions using item status, instruction, cue, and cue influence rating. Removing the 4-way interaction did not significantly reduce fit $\chi^2(1)=0.01$, $p=.91$. Removing the 3-way interactions item status by cue by instruction as well as item status by cue by influence did not significantly reduce fit, $ps>.51$. However, removing the 3-way interaction item status by influence by instructions significantly reduced fit $\chi^2(1)=3.72$, $p=.05$. The final model is reported in Table 6.

Table 6. Final mixed level model (Experiment 2)

Random Effects:

Groups-Subjects	Variance	St. Dev.	Correlation
(Intercept)	0.01	0.11	
Item Status	0.01	0.12	-0.83

Fixed Effects:

	Estimate	Std. Error	df	t	p-value
(Intercept)	0.29	0.03	42	11.06	<.001***
Item Status	0.58	0.03	60	18.04	<.001***
Cue Type	-0.09	0.02	11224	-4.72	<.001***
Influence	-0.06	0.01	10318	-10.15	<.001***
Instruction	0.04	0.02	11231	1.76	0.08
Item Status x Cue Type	0.00	0.02	11220	0.31	0.75
Item Status x Influence	-0.10	0.01	9160	-11.60	<.001***
Cue Type x Influence	0.19	0.01	11225	23.80	<.001***
Item Status x Instruction	0.14	0.03	11226	4.89	<.001***
Cue Type x Instruction	-0.16	0.03	11223	-5.81	<.001***
Influence x Instruction	0.00	0.01	11139	0.28	0.78
Item Status x Influence x Instruction	-0.02	0.01	11035	-1.93	0.05*
Cue Type x Influence x Instruction	0.02	0.01	11225	2.14	0.03*

*** p<.001 ** p<.01 * p<.05

While both the significant higher order interactions included instruction, separate models were estimated under use and ignore instructions in order to more easily interpret the results. Under use instructions MLM was estimated using main effects and interactions of item status by cue influence rating and cue type by cue influence rating (see Table 7). The top panel in Figure 9 shows the cue type by influence interaction demonstrating that as cue influence ratings increased the probability of responding 'old' increased under Likely Old cues and decreased under Likely New cues. Thus, the influence of cues was more robust as subjective ratings of cue influence increased, suggesting that under use instructions participants could subjectively monitor their degrees of influence. The bottom panel shows the influence rating by item status interaction demonstrating that as cue influence ratings increased the probability of responding 'old' decreased for old items and increased for new items. That is, as cue influence ratings increased the probability of responding old approached 0.5 or guessing. This suggests that participants gave higher subjective ratings of cue influence as their memory evidence or accuracy weakened.

The analogous model for ignore instructions revealed a similar pattern (see Table 8). The top panel in Figure 10 shows the cue type by influence interaction once again demonstrating that the influence of cues were more robust as subjective ratings of cue influence increased. Recall that in the original model there was a significant interaction between cue type, influence, and instruction, p=0.03. This three-way interaction was likely driven by the steeper slope of Likely New cues during ignore (top panel of Figure 10) relative to use instructions (top panel of Figure 9). Critically, however, even when participants were actively attempting to ignore cues they were able to subjectively monitor their degree of cue influence. Thus, although participants were not able to override the influence of cues, they appeared to have awareness of their influence. The bottom panel shows the item status by influence rating interaction demonstrating that as cue influence ratings increased the probability of responding ‘old’ decreased for old items while it remained relatively stable for new items. Thus, once again participants rated higher influence of cues as their probability of responding old approached guessing (i.e., .50) but this effect seems to be mostly driven by old items while new items remained largely unaffected. Note, however, that during ignore instructions participants were less likely to give high cue influence ratings resulting in less reliable estimates for these high ratings. This can also be observed by the increased size of confidence bands for higher values of cue influence ratings. Additionally, recall that in the original model there was a significant interaction between item status, influence, and instruction, p=0.05. This three-way interaction was likely driven by the relatively flat slope of new items during ignore instructions (bottom panel of Figure 10) compared to positive slope under use instructions (bottom panel of Figure 9).

Table 7. Separate mixed level model for use instructions (Experiment 2)

Use Instructions

Random Effects:

Groups-Subjects	**Variance**	**St. Dev.**	**Correlation**
(Intercept)	0.00	0.08	
Item Status	0.01	0.10	-0.80

Fixed Effects:

	Estimate	**Std. Error**	**df**	**t**	**p-value**
(Intercept)	0.32	0.02	65	13.45	<.001***
Item Status	0.74	0.03	76	23.73	<.001***
Cue Type	-0.25	0.02	5728	-11.56	<.001***
Influence	-0.06	0.00	4193	-13.44	<.001***
Item Status x Cue Type	0.00	0.02	5712	0.14	0.89
Items Status x Influence	-0.13	0.01	3952	-21.31	<.001***
Cue Type x Influence	0.21	0.01	5728	39.06	<.001***

Table 8. Separate mixed level model for ignore instructions (Experiment 2)

Ignore Instructions

Random Effects:

Groups-Subjects	**Variance**	**St. Dev.**	**Correlation**
(Intercept)	0.02	0.14	
Item Status	0.02	0.15	-0.85

Fixed Effects:

	Estimate	**Std. Error**	**df**	**t**	**p-value**
(Intercept)	0.33	0.03	35	9.60	<.001***
Item Status	0.55	0.04	51	13.84	<.001***
Cue Type	-0.10	0.02	5472	-4.31	<.001***
Influence	-0.08	0.01	3454	-10.54	<.001***
Item Status x Cue Type	0.01	0.02	5466	0.37	0.71
Items Status x Influence	-0.09	0.01	1899	-8.23	<.001***
Cue Type x Influence	0.19	0.01	5476	22.42	<.001***

*** p<.001   ** p<.01   * p<.05

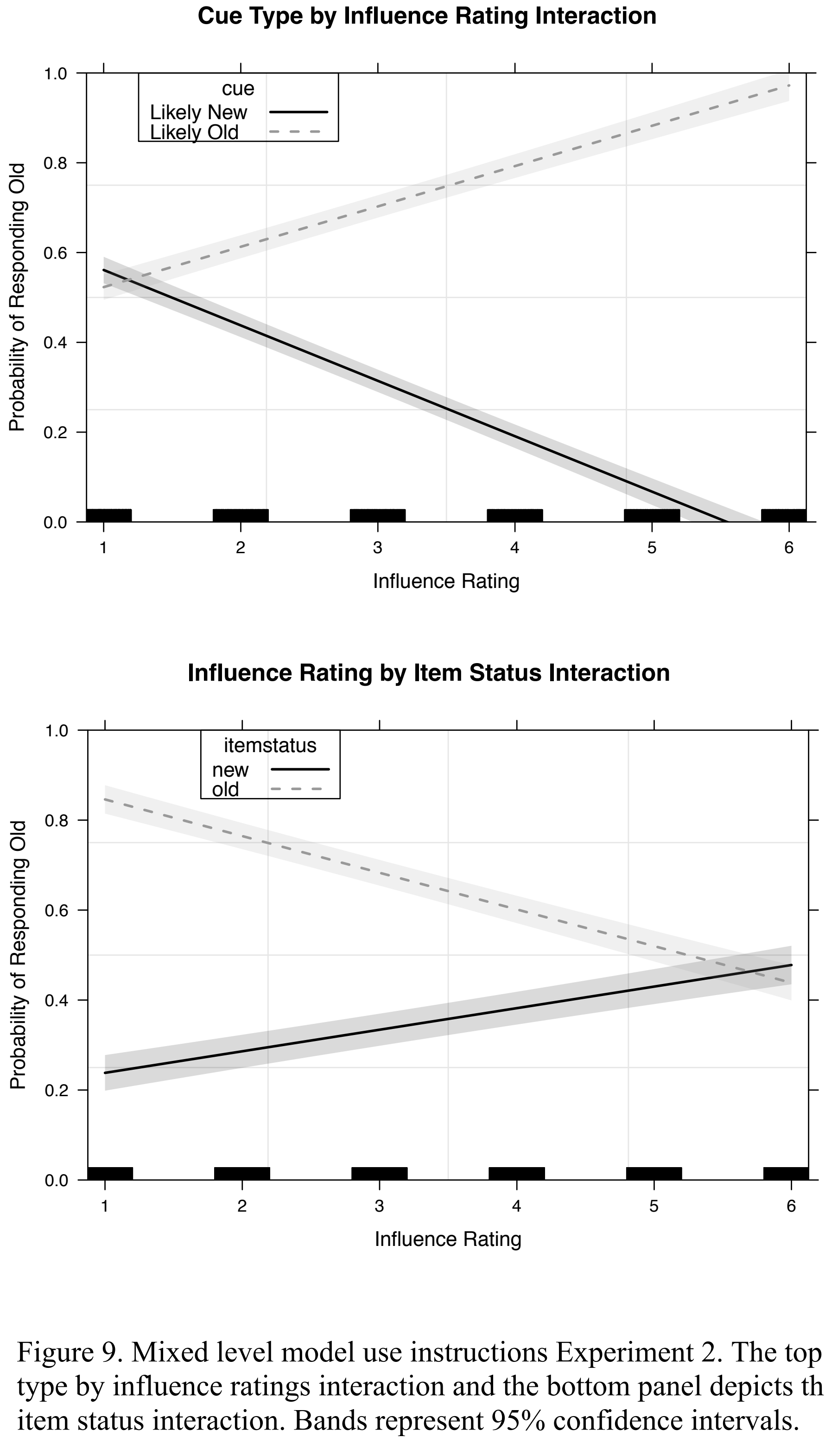
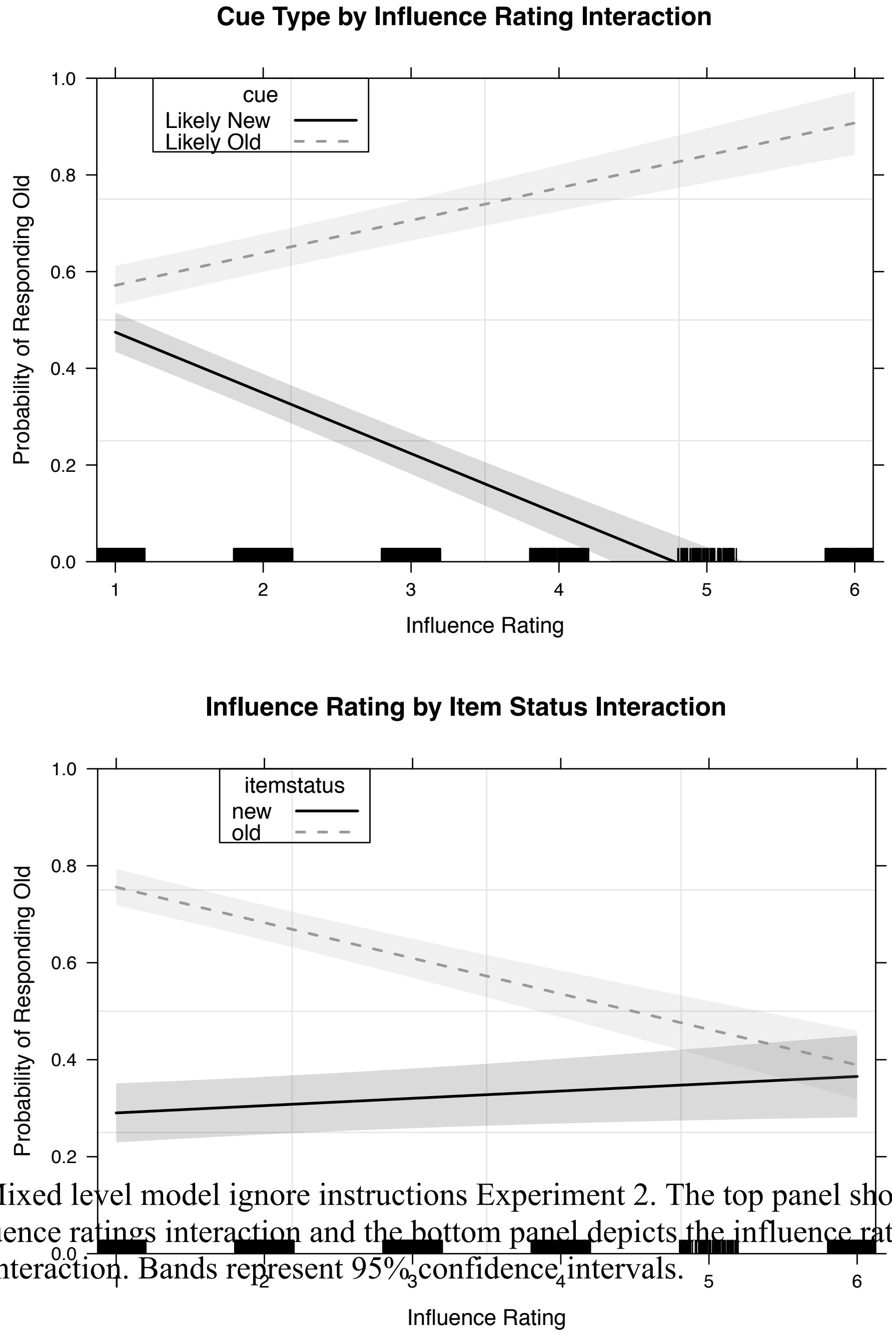


Figure 9. Mixed level model use instructions Experiment 2. The top panel shows the cue type by influence ratings interaction and the bottom panel depicts the influence rating by item status interaction. Bands represent 95% confidence intervals.



igure 10. Mixed level model ignore instructions Experiment 2. The top panel shows the cue ype by influence ratings interaction and the bottom panel depicts the influence rating by em status interaction. Bands represent 95% confidence intervals.

Working Memory. Recall that a secondary question of the current experiment was whether individual differences in criteria shifts were linked to measures of cognitive control, namely working memory. Participants' working memory was estimated using a traditional absolute OSPAN score, which is the sum of all the perfectly recalled sets (Unsworth et al., 2005). Table 9 shows the simple correlations between OSPAN scores, shifts in response criterion, and baseline accuracy. When examining Table 9, it is clear that OSPAN scores did not correlate significantly with criterion swing (measured as the difference between criterion measure (C) under Likely New minus Likely Old cues) or uncued/baseline accuracy measures. However, to control for differences in uncued/baseline accuracy, I also conducted hierarchical regression analyses predicting shifts in response criterion using baseline/uncued accuracy in Step 1 as a covariate and adding OSPAN scores in Step 2, separately for both use and ignore instructions. For use instructions, Step 1 revealed a significant negative relationship between criterion swing and uncued/baseline performance, b=-0.50, *t*(22)=-2.24, *p*=.04, suggesting that those with higher uncued/baseline accuracy had smaller shifts in response criterion. This result was not unexpected, since theoretically those individuals who have higher baseline performance have less room to benefit from environmental cues, and they should shift their response criterion to a lesser degree (see Selmeczy & Dobbins, 2013). Critically, when adding OSPAN score as a predictor in Step 2, it was not significant, ΔR^2=0.05, *F*(1,21)=1.40, *p*=.25; b=0.00, *t*(21)=1.18, *p*=.24.

For ignore instructions, in Step 1 uncued/baseline performance was not a significant predictor of criterion swing, b=-0.41, *t*(22)=-1.49, *p*=.15. Thus, it appears as though when participants were actively trying to eliminate the influence of cues, there was not a strong relationship between criterion swing and uncued/baseline performance. Critically, once again when adding OSPAN score as a predictor in Step 2, it was not significant, $\Delta R^2=0.06$, *F*(1,21)=1.51 *p*=.23; b=0.01, *t*(21)=1.23, *p*=.23. However, as seen in Table 9 the degree of criterion shift between use and ignore instructions was highly correlated (r=0.57) and it could be the case that when participants were instructed to ignore cues they attempted to lessen their cue influence relative to their swing under use instructions. Therefore, another regression was run predicting shifts in criterion during ignore instructions while controlling for shifts in criterion under use instructions. In Step 1 uncued/baseline performance was not a significant predictor of criterion swing, b=-0.28, *t*(21)=-1.17, *p*=.25. Additionally, criterion swing under use instructions was also included in Step 1 and as expected it was a significant predictor, b=0.52, *t*(21)=3.00, *p*=.006. Critically, when adding OSPAN score as a predictor in Step 2, it was not significant, $\Delta R^2=0.02$, *F*(1,20)=0.46, *p*=.51; b=0.00, *t*(20)=0.68, *p*=.50. Overall, in the current experiment working memory does not appear to be related to the degree of criterion shift observed from environmental cues for both use or ignore instructions. However, the failure to detect a relationship could result from the limited power of the current individual differences analysis and this issue will be further examined in the General Discussion section.

ble 9. Simple correlations between OSPAN scores, baseline accuracy, and criterion
ing (Experiment 2)

	OSPAN Score	**Use Baseline Accuracy**	**Use Criterion Swing**	**Ignore Baseline Accuracy**
OSPAN Score				
Use Baseline Accuracy	0.14			
Use Criterion Swing	0.16	-0.43*		
Ignore Baseline Accuracy	0.37	0.50*	-0.18	
Ignore Criterion Swing	0.12	-0.20	0.57**	-0.30

*** p<.001 ** p<.01 * p<.05

3.3 Discussion

Experiment 2 investigated whether observers could subjectively rate their degree of cue influence on a trial-by-trial basis. While previous experiments demonstrated participants could not fully ignore environmental cues when instructed to do so, it was unclear whether participants were aware of this putatively automatic cue influence. The current experiment assessed participants' awareness of cue influence by having them complete a cue influence rating after each cued recognition test trial. As expected, participants were not able to fully eliminate the influence of cues during ignore instruction and still responded more liberally under Likely Old vs. Likely New cues. Once again this effect was dampened relative to use instructions. Furthermore, mixed level modeling revealed that as the influence of cues increased (measured as the difference in the probability of responding 'old' under Likely Old vs. Likely new cues) subjective ratings of cue influence also increased. This occurred under both use and ignore cue instructions, suggesting that even during ignore instructions participants had some level of awareness that they were influenced. Additionally, participants were also more likely to have higher cue influence ratings when the cue aligned with their response (e.g., 'old' response under Likely Old cue) relative to when the cue disagreed with their response (e.g., 'old' response under Likely New cue).

Under the previously described expectancy based hypothesis, cues are thought to alter the subjective experience of evoked memory signal and the current experiment suggests that observers have awareness of this influence ex post facto. Thus, observed criterion shifts during ignore instructions may not reflect an entirely implicit biasing process, but instead participants' correction strategies may have been inefficient or they may have underestimated the degree of initial influence. That is, environmental cues may have exerted a large degree of initial influence and when participants were instructed to ignore cues they may have used some form of countermanding strategy to try and correct for this influence. The fact that participants were not able to fully eliminate the influence suggests an inefficient use of this strategy or participants may have underestimated their initial degree of influence. Importantly, the finding that participants were aware of the influence of cues suggests that there could be circumstances where perhaps participants may be more efficient at overcoming the influence of environmental cues, and the results from Experiments 3 and 4 suggests this may be the case.

Additionally, participants also gave higher ratings of cue influence when the cue matched their old/new response. This result could reflect a post-recognition judgment heuristic such that regardless of the true influence of cues, when participants overtly disagreed with a cue they indicated a low degree of influence to appear consistent or reasonable (i.e., it may appear inconsistent for participants to be highly influenced by a cue that they disagreed with). Thus, in this case influence ratings would reflect the degree of agreement participants felt between the cue and their judgment. However, it is unlikely that participants' cue influence ratings were solely based on the match between the cue form and their judgment since subjective ratings of influence were related to the actual degree of cue influence as measured by the change in response rates. Alternatively, participants could also have used a pre-recognition judgment heuristic where influence ratings were linked to the quality of memory evidence recovered. Recall that the degree of cue influence was also associated with accuracy such that participants rated higher degrees of cue influence when their probability of responding 'old' approached 0.5 or guessing. Therefore, it could also be the case that participants appropriately assessed their quality of memory evidence and when memory evidence was weak they rated a higher degree of cue influence than when memory evidence was strong. Under this account, cue influence ratings reflect the degree to which participants depended on the cue. Furthermore, the reason cue influence ratings would be higher when participants' responses aligned with the cue is because these trials contained a large proportion of instances where participants actually changed the original judgment in favor of the cue's recommendation. In other words, if participants changed their judgment to align with the cue when memory evidence was weak, then they would more frequently agree with the cue while also giving higher ratings of cue influence. If participants kept their initial judgment when memory evidence was strong, then they would more frequently disagree with the cue while also giving lower ratings of cue influence.

Finally, the current experiment also assessed whether individual differences in criterion shifting were linked to working memory ability. However, no relationship was found between working memory scores and criterion shifting during both use or ignore cue instructions. While this seems to suggest that cognitive control, at least measured by working memory span, may not be particularly important at predicting criterion regulation, the current experiment may have been underpowered to detect this relationship. A more complete discussion of this topic will occur in the General Discussion.

Chapter 4

Experiment 3

The prior work assessing whether participants can eliminate the influence of environmental cues always used reliable (i.e., 75% valid) cues. Although this work shows that participants cannot separate the influence of *reliable* cues from internal memory evidence, the current study examined whether participants can ignore *random,* uninformative cues. If the automatic influence of cues is due to learned expectancies (i.e., the cue becomes associated with an anticipation of experiencing familiarity or novelty and this in turn alters the processing of the memory signal), then reliable cues may be particularly difficult to ignore since the cues are positively correlated with item status (see Melara & Algom, 2003 for similar ideas using Stroop Task). However, random cues are uninformative since they are not correlated with item status so learned expectancies cannot form. Therefore, under an expectancy based account participants should easily ignore random cues. Alternatively, if the automatic influence of cues is instead driven by strategies or heuristics, participants' decision criteria may still be influenced even when provided with random cues. For example, previous memory conformity research shows that when two speakers recalled an event, participants were more likely to rate the first speaker as more confident and accurate even when participants were explicitly told the first speaker was picked at random (Wright & Carlucci, 2011). This result suggests that even when information is uninformative, heuristics could still influence how people use this information. Additionally, although Experiment 2 did not find a relationship between working memory and criterion shifting in the context of reliable cues, the current experiment assessed whether working memory may be linked to the ability to effectively disregard random cues.

4.1 Methods

Participants. Experiment 3 included 31 participants recruited from the Washington University Experimetrix pool (average age=19.73, 23 females; one participant chose not to report age and sex) who were given one course credit per hour of 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 Experiment 2.

Participants completed a total of 3 study/test cycles and were encouraged to take a brief break between each study/test cycle. During study participants performed a syllable counting task (i.e., does this word contain 1, 2, 3 or more syllables?) on a list of serially presented words (106 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) (106 old items, 106 new items). The majority of test trials were preceded by a cue reading Likely Old or Likely New that appeared 1 second prior to the recognition probe (192 cued items-96 old, 96 new). Half the cued trials were preceded by a random (50%) cue (96 items-48 valid, 48 invalid), while the other half were preceded by a reliable (75%) cue (96 items-72 valid, 24 invalid). Some items were also were also uncued (20 uncued/baseline items-10 old, 10 new) and all trials were randomly intermixed. Participants were explicitly informed about the reliability of the cue using the following instructions:

> "Some cues will be reliable and correct 75% of the time. This means about 7 out of 10 times the cue will give you the correct answer and should be generally helpful for your recognition judgment. Other cues will be random and correct only 50% of the time. This means 5 out of 10 times the cue will give you the correct answer and is not helpful for your recognition judgment."

To distinguish between the two cue reliabilities random cues were presented in blue font and included the word 'random' (e.g., Random Likely Old) and reliable cues were presented in yellow font and included the word ‘reliable’ (e.g., Reliable Likely New). For the first study/test cycle participants were encouraged to incorporate cues using the following instructions:

> “Overall your goal should be to respond as accurately as possible using the combination of your own memory evidence and cues.”

In the current design the words USE CUES were not presented throughout the test cycle since random and reliable cues were intermixed and I did not want to encourage participants to explicitly use random cues. During the second and third study/test cycle, participants were told to ignore cues using the following instructions:

> “You should IGNORE THE CUES and make your decision solely on your own memory. Although some of the cues are helpful, you should ignore any cue presented.”

For the ignore study/test cycles, the words IGNORE CUES were presented on the screen using red font since the goal of the participant should be to ignore all cues. After each recognition decision, participants made a confidence rating using a 6-point scale ranging from 50%-100% in 10% increments with 50% indicating guessing and 100% indicating certainty. At the end of recognition testing participants were asked whether the reliable cues were accurate approximately 75% of the time and whether the random cues were accurate approximately 50% of the time using a Yes/No response. If participants responded ‘No’, a follow up question asked them to indicate how accurate they thought the cue was using a percentage. These questions were asked in order to evaluate whether participants believed the instructions regarding cue validity. After the recognition tests participants also completed the OSPAN task using the same methods as described in Experiment 2 in order to assess whether working memory is linked to the ability to prevent the influence of random cues.

4.2 Results and Discussion

As a reminder standard ANOVA analyses were conducted since they are conventionally used and easy to interpret. However, a mixed level modeling approach was also used for certain targeted questions outlined in the results section.

Criterion. See Table 10 for descriptive statistics response rates, accuracy, and criteria. Please note that for all tables in the current experiment uncued/baseline measures were estimated collapsing across use and ignore instructions. This was done in order to increase reliability since only a low number of uncued/baseline trials were included during each study/test cycle (20 items) in order to keep the experiment length at an appropriate level and maximize the number of cued trials.

Changes in criteria were assessed with Signal Detection measure C using a three-way repeated measure ANOVA with factors of instruction (use vs. ignore), cue condition (random vs. reliable), and cue type (Likely Old vs. Likely New) (See Figure 11). Critically, the three-way interaction between instruction, cue condition, and cue type, was significant $F(1,30)=24.87$, $p<.001$, $\eta_p=.45$ Follow-up interaction analyses separately considered the use and ignore instruction conditions. During the use instruction there was a significant interaction between cue type and cue condition, $F(1,30)=26.62$, $p<.001$, $\eta_p=.47$ that resulted because criterion differed during Likely Old and Likely New cues when they were reliable (-0.36 vs. 0.34, $p<.001$) but not when they were random (-0.04 vs. 0.02, $p=.87$). In contrast, during ignore instructions cue type and cue condition did not interact $F(1,30)=0.83$, $p=.37$, $\eta_p=.03$ and there were no main effects of either factor as well ($ps>0.07$) (See Figure 11). Thus, neither reliable nor random cues induced significant shifts in criterion under the ignore instructions, contrasting with the prior experiments.

Use Instructions

	HR		CR		d'		C	
Random Cue								
Cued	0.76	(0.09)	0.75	(0.11)	1.47	(0.45)	0.00	(0.26)
Likely Old Cue	0.77	(0.11)	0.75	(0.13)	1.56	(0.50)	-0.04	(0.38)
Likely New Cue	0.75	(0.13)	0.76	(0.12)	1.49	(0.62)	0.02	(0.3)
Reliable Cue								
Cued	0.80	(0.08)	0.80	(0.09)	1.74	(0.38)	0.01	(0.26)
Likely Old Cue	0.85	(0.10)	0.64	(0.20)	1.53	(0.52)	-0.36	(0.47)
Likely New Cue	0.64	(0.16)	0.85	(0.09)	1.52	(0.57)	0.34	(0.34)

Ignore Instructions

	HR		CR		d'		C	
Random Cue								
Cued	0.73	(0.12)	0.74	(0.12)	1.37	(0.57)	0.02	(0.30)
Likely Old Cue	0.73	(0.13)	0.74	(0.12)	1.37	(0.60)	0.00	(0.33)
Likely New Cue	0.73	(0.13)	0.74	(0.13)	1.39	(0.63)	0.03	(0.33)
Reliable Cue								
Cued	0.73	(0.12)	0.75	(0.14)	1.41	(0.59)	0.04	(0.33)
Likely Old Cue	0.75	(0.12)	0.73	(0.16)	1.42	(0.69)	0.00	(0.36)
Likely New Cue	0.70	(0.13)	0.75	(0.15)	1.36	(0.71)	0.10	(0.35)

	HR		FA		d'		C	
Overall Uncued/Baseline	0.75	(0.12)	0.73	(0.15)	1.40	(0.49)	-0.02	(0.39)

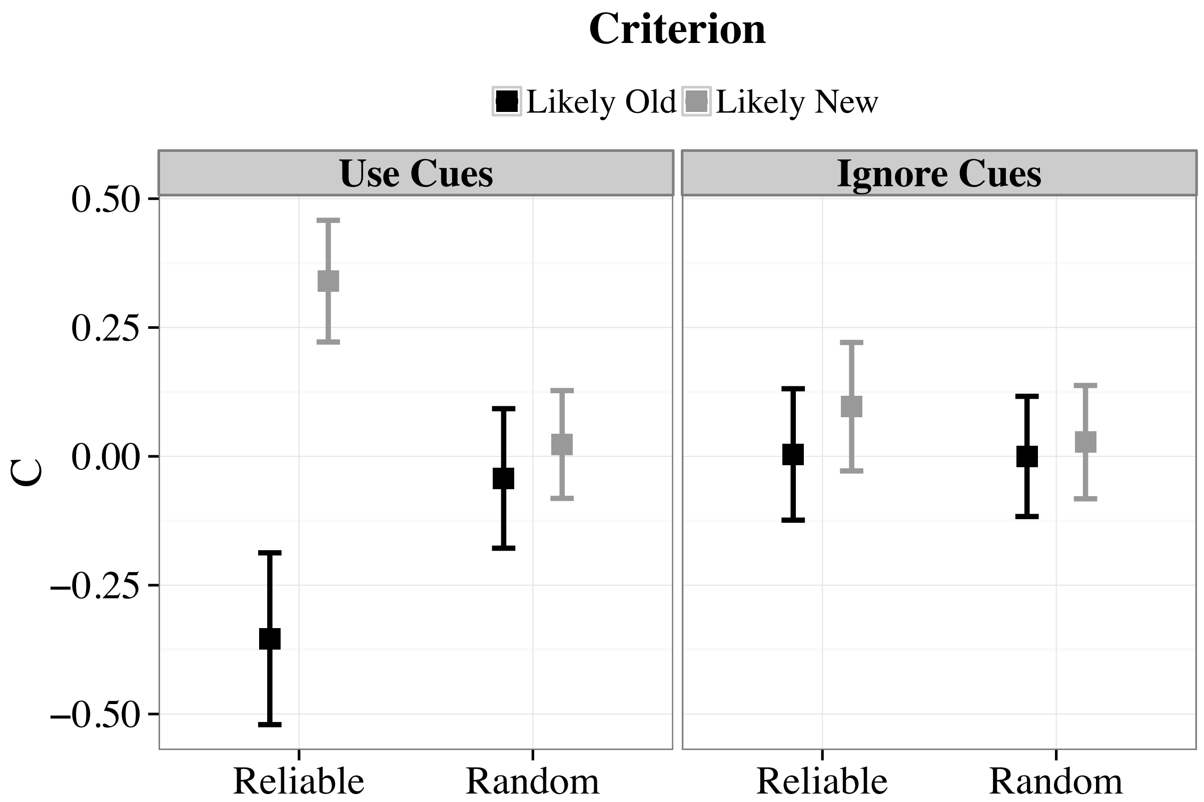


Figure 11. Criterion Experiment 3. The graph depicts average criterion (C) under Likely Old (black) and Likely New (gray) cues during use and ignore instructions for both reliable and random cues. Error bars represent 95% confidence intervals for the mean.

Given the prior experiments it was surprising to find that reliable cues were effectively ignored when intermixed with random cues in the current study. Therefore, I further assessed criterion by examining results as function of subjective ratings of cue validity. Recall that after recognition testing participants were asked whether they found the cues to be 50% and 75% valid using a yes/no response. If they responded 'no', they were asked to provide a subjective percentage estimate of cue validity. Overall, 48% of participants (N=15) reported that the reliable cues were 75% accurate. Those who reported not believing the reliable cues to be 75% accurate tended to estimate a lower percentage (*M*=52%, *SD*=9.99). For random cues, 61% of participants (N=19) reported that they were 50% accurate, and those who reported not believing the random cues to be 50% tended to estimate a lower percentage (*M*=40%, *SD*=11.43). To examine whether criterion shifting was related to subjective reports of cue validity, a simple correlation was conducted between criterion swing scores (Likely New C- Likely Old C) and subjective percentages of cue validity, with the actual percentages of cue validity (i.e., 75% and 50%) used for those participants who provided a 'yes' response. For use instructions, the correlation was not significant during both reliable (*r*=.13, *p*=.52) and random cues (*r*=-.08, *p*=.68). Similarly, for ignore instructions, the correlation was also not significant during both reliable (*r*=.30, *p*=.11) and random cues (*r*=-.02, *p*=.92). Thus, subjective estimates of cue validity did not seem to affect the degree of criterion shifting for both use and ignore instructions.

To summarize, the finding that participants' decision criteria were not influenced by random cues supports the expectancy based account such that when cues no longer reliably engender an expectation of item status, participants were able to effectively ignore them. However, in contrast to prior experiments, the reliable cues also failed to demonstrate shifts of the criterion during ignore instructions. However, the standard ANOVA approach does not jointly model variance in subject bias and accuracy when assessing cue influences and therefore mixed level modeling at the trial level was also conducted.

Mixed level Modeling. Although the ANOVA analysis failed to find reliable cue influences under the ignore instruction, the numerical differences in the mean criterion were in the direction consistent with a cue influence (reliable cues, right panel Figure 11). Critically, the standard ANOVA model does not allow one to simultaneously model variation in overall bias and differences in recognition accuracy across subjects, and this in turn may inflate the error term reducing power to detect an effect. More specifically, subjects with high baseline accuracy would not be expected to show much of a cue influence under use or ignore instructions since even if the cues affected criterion placement or probe processing to a slight degree, it would be difficult to detect in the response rates. For example, under signal detection theory, an observer with a d' of 2.5 would only show a change in response percentages of 3.7% for a criterion shift of .20 standard deviation units away from the unbiased point, whereas an individual with a d' of 1 would show a shift of 7.0% for the same criterion movement. Additionally, overall response bias will also affect the degree to which response rates change as a function of shifting decision criteria. One can attempt to control for either subject differences in accuracy or general decision bias via an ANCOVA, but MLM allows for the joint modeling of both subject effects (bias and accuracy and their covariance) while estimating the average affect of cues (fixed effect) with these subject variations controlled.

To address this I turned to mixed level modeling to try to improve the power to detect effects for reliable cues under the ignore instruction condition. The MLM was restricted to ignore instructions since the predicted effects were clearly observed under use instructions with an ANOVA approach (i.e., significant shifts under reliable cues but not random cues) and therefore the results from a more powerful MLM approach would simply reaffirm these results. Restricting analyses to only cued trials during ignore test cycles, response (coded 1-old, 0-new) was predicted using item status (coded 1-old, 0-new), cue condition (0-random, 1-reliable), and cue type (coded 1-Likely Old, 0-Likely New). The intercept of the model was treated as a random effect across subjects as well as the effect of item status. The intercept term captures subject differences in the propensity towards positive responses (viz., bias) and the item term captures differences in the degree to which each subjects responses are tightly coupled with the status of the test probe (viz., accuracy) (Wright et al., 2009). Linear probability models were used since they provide solutions that are very close to those of logistic regression and provide easily interpretable parameters (i.e., they indicate how the variables influence the probability of response). Initially the full model was fit including both main effects and higher order interactions. The final model was determined by systematically removing predictors starting with the highest order interaction until there was a significant decrease in fit using chi-squared tests of log-likelihoods. Interactions at the same level were removed in order of least to most significant. Coefficient estimates were tested using the Satterthwaite approximation for degrees of freedom.

The full model included both main effects and higher order interactions between item status, cue condition, and cue type. Removing the three way interaction did not significantly reduce fit $\chi^2(1)=0.75$, $p=.39$. Removing the two way interactions between item status and cue type and item status and cue condition also did not significantly reduce fit, *p*s>0.66. Removing the two way interaction between cue type and cue condition marginally reduced fit $\chi^2(1)=2.86$, $p=.09$. Importantly, removing the main effect of cue type significantly reduced fit, $\chi^2(1)=4.89$, $p=.03$. Thus, the final model (see Table 11) included a significant main effect of item status, $b=0.46$ $t(30)=16.37$, $p<.001$, which simply reflected accuracy and indicated that participants were more likely to respond ‘old’ when the item was old vs. new. Critically, the main effect of cue type was also significant, *b*=0.02 *t*(11840)=2.21, *p*=.03, indicating that participants responded ‘old’ more often under Likely Old vs. Likely New cues and demonstrating that participants did not fully ignore cues. The main effect of cue condition was not significant, *b*=0.00 *t*(11840)=-0.24, *p*=.81, indicating that overall ‘old’ rates did not differ between random and reliable cueing conditions.

While removing the interaction between cue condition and cue type was only marginally significant, *p*=.09, separate models were conducted for random and reliable cueing conditions. Under the expectancy account, cue type should not be significant when cues were random since cue driven expectancies should not form when cues were uninformative. In contrast, cue type should be a significant predictor for reliable cues since these cues should result in cue driven expectations. For reliable cues, a MLM was conducted including the main effects and interaction between item status and cue type. Removing the interaction between item status and cue type did not significantly reduce fit, $\chi^2(1)=0.89$, *p*=.35. However, removing the main effect of cue type did significantly reduce fit, $\chi^2(1)=7.18$, *p*=.01. Thus, this significant main effect of cue type, *b*=0.03 *t*(5888)=2.68, *p*=.01, indicated that participants responded ‘old’ more often under reliable Likely Old vs. Likely New cues. For random cues, the analogous model revealed no significant reduction in fit when removing the interaction between item status and cue type $\chi^2(1)=0.06$, *p*=.80. Critically, the model fit was also not significantly reduced when removing the main effect of cue type, $\chi^2(1)=0.24$, *p*=.62, suggesting that there was not a significant influence of cue type on ‘old’ rates when cues were random. Overall, the more powerful MLM approach does suggest that reliable cues exerted a small influence during ignore instruction, although clearly the effect was much smaller relative to the previous experiments. More specifically, the linear probability model suggested that subjects shifted their 'old' response probability by 3% in the face of the cues. Additionally, the MLM demonstrates no influence of random cues confirming ANOVA analyses that participants were able to effectively disregard random cues.

ıble 11. Mixed level model for ignore instructions during reliable cues (Experiment 3)

Ignore Instructions

Random Effects:

Groups-Subjects	**Variance**	**St. Dev.**	**Correlation**
(Intercept)	0.02	0.12	
Item Status	0.02	0.15	-0.71

Fixed Effects:

	Estimate	**Std. Error**	**df**	**t**	**p-value**
(Intercept)	0.25	0.02	33	10.80	<.001***
Item Status	0.47	0.03	30	16.37	<.001***
Cue Type	0.02	0.01	11840	2.21	0.03*
Cue Condition	0.00	0.01	11840	-0.24	0.81

*** p<.001 ** p<.01 * p<.05

Confidence. The influence of cues during ignore instructions was further assessed using confidence, which may be a more sensitive indicator of cue influence because it is more finely grained and potentially more dependent upon introspection. Once again, recall that participants used a 6-point confidence scale (50-100% in 10% increments) after each recognition decision. All confidence analyses focused on correct responses (i.e., hits and correct rejections) since incorrect responses were relatively infrequent and provide much less reliable estimates. While the main analyses only focused on ignore test cycles, Table 12 and Figure 12 contain descriptive statistics for confidence under both use and ignore instructions.

ble 12. Average confidence with standard deviations in parenthesis (Experiment 3)

Use Instructions

	Hits		Correct Rejections	
Random Cue				
Likely Old Cue	85.93	(8.05)	77.38	(9.61)
Likely New Cue	85.17	(8.66)	79.85	(9.04)
Reliable Cue				
Likely Old Cue	86.19	(7.60)	76.69	(9.62)
Likely New Cue	85.00	(8.13)	82.57	(7.40)

Ignore Instructions

	Hits		Correct Rejections	
Random Cue				
Likely Old Cue	87.95	(8.38)	80.33	(8.32)
Likely New Cue	87.93	(8.92)	80.22	(9.18)
Reliable Cue				
Likely Old Cue	87.91	(7.79)	78.65	(9.52)
Likely New Cue	88.08	(8.72)	80.98	(8.51)

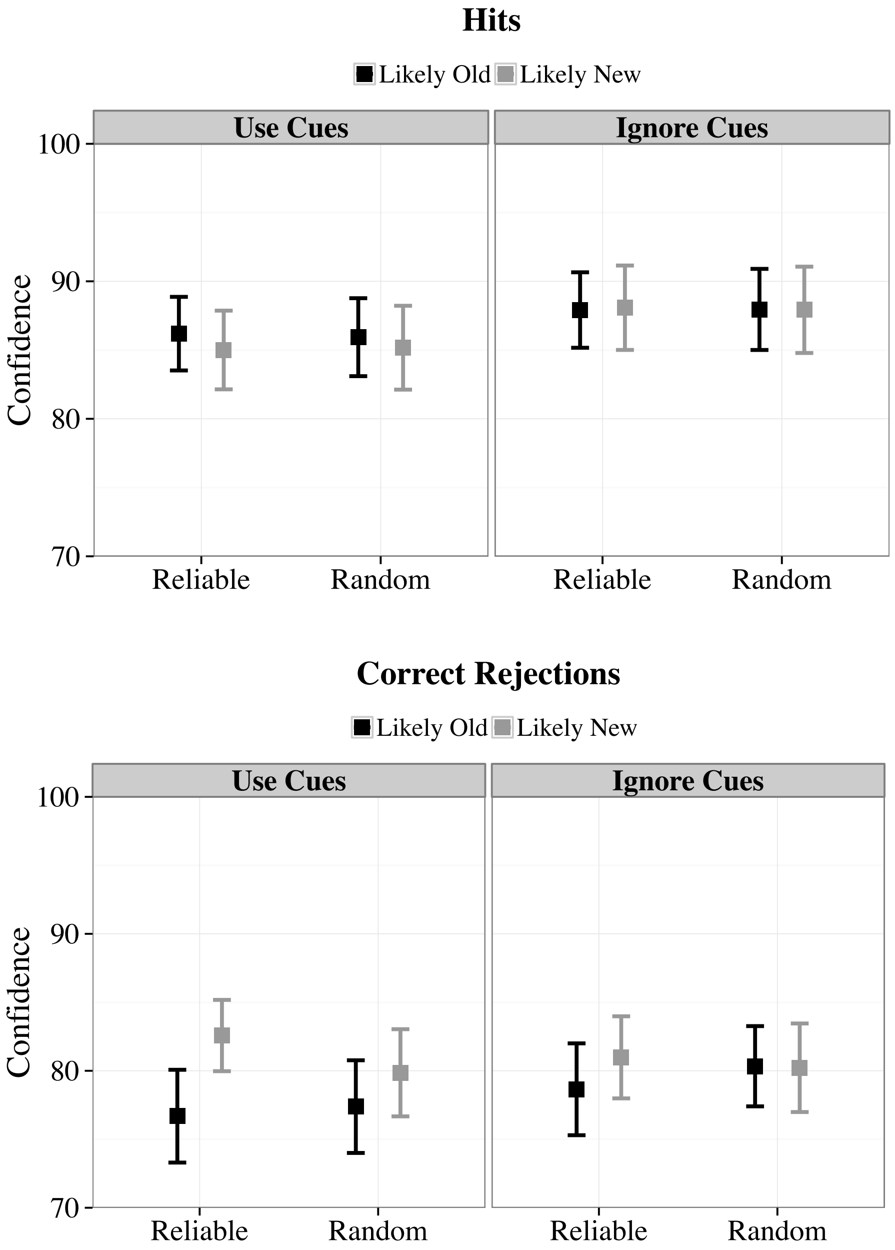
	Hits		Correct Rejections	
Overall Uncued/Baseline	86.68	(8.79)	78.59	(8.59)

Hits

For hits, a 2 X 2 repeated measures ANOVA with factors condition (random vs. reliable) and cue type (Likely Old vs. Likely New) was conducted only on the ignore test cycles. However, the main effects and interaction were not significant (*ps*>0.82), suggesting the cues did not exert an influence on hit confidence during ignore instructions (see Figure 12 top right panel).

Correct Rejections

The 2 X 2 repeated measures ANOVA with factors cue condition (random vs. reliable) and cue type (Likely Old vs. Likely New) revealed a significant main effect of cue type, $F(1,30)=5.38$, $p=.03$, $\eta_p=.15$ due to higher confidence during Likely New vs. Likely Old cues (80.60 vs. 79.49). The main effect of cue condition was not significant, $F(1,30)=1.25$, $p=.27$, $\eta_p=.04$. Critically, the interaction between cue type and cue condition was significant, $F(1,30)=6.72$, $p=.01$, $\eta_p=.18$ (see Figure 12 bottom right panel). Post-hoc tests (Tukey's HSD) revealed no effect of random cues (Likely Old 80.33 vs. Likely New 80.22, $p=.99$); however, confidence was significantly higher during reliable Likely New vs. Likely Old cued trials (80.98 vs. 78.65, $p=.007$). Importantly, this result suggests that at the level of confidence, reliable cues still exerted an influence during ignore instructions.



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Reaction Time. Like the confidence data, reaction time might also prove to be more sensitive to the effects of reliable cues. Descriptive statistics for reaction time under both use and ignore instructions are in Table 13 and Figure 13, but analyses will once again focus on ignore test cycles. For each participant the medians, as opposed to mean reaction time was used for estimating reaction time since response time distributions are positively skewed and the median is less influenced by outliers.

Use Instructions

	Hits		Correct Rejections	
Random Cue				
Likely Old Cue	2.11	(0.88)	2.13	(0.73)
Likely New Cue	2.05	(0.82)	1.77	(0.62)
Reliable Cue				
Likely Old Cue	1.91	(0.64)	2.13	(0.92)
Likely New Cue	2.36	(1.36)	1.95	(0.62)

Ignore Instructions

	Hits		Correct Rejections	
Random Cue				
Likely Old Cue	1.19	(0.27)	1.36	(0.32)
Likely New Cue	1.13	(0.24)	1.33	(0.34)
Reliable Cue				
Likely Old Cue	1.16	(0.27)	1.43	(0.47)
Likely New Cue	1.20	(0.28)	1.32	(0.34)

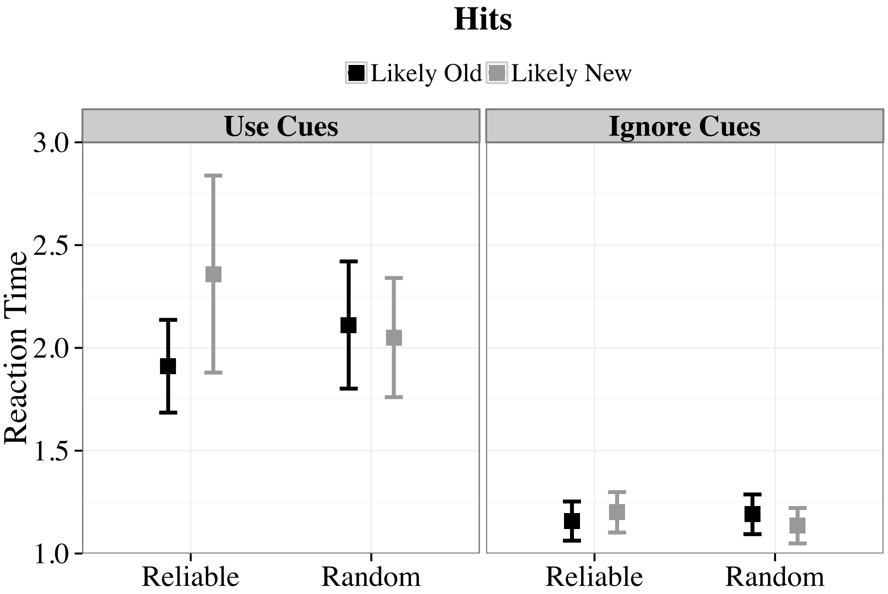
	Hits		Correct Rejections	
Overall Uncued/Baseline	1.46	(0.38)	1.69	(0.46)

Hits

For hits, a 2 X 2 repeated measures ANOVA with factors of cue condition (random vs. reliable) and cue type (Likely Old vs. Likely New) was conducted. While neither the main effect of cue condition or cue type was significant (*ps*>.43), the interaction between cue condition and cue type was significant, $F(1,30)=9.19$, $p=.004$, $\eta_p=.23$ (see Figure 13 top right panel). Follow up post-hoc tests (Tukey's HSD) revealed a marginal effect of random cues such that participants responded slightly faster during Likely New relative to Likely Old cues (1.13 vs. 1.19, $p=.09$). However, this effect was not significant for reliable cues (Likely Old 1.16 vs. Likely New 1.20, $p=.27$). Note the interaction likely reached significance because participants responded numerically slower for Likely New (invalid) vs. Likely Old (valid) cues for the reliable cue condition, but this pattern was reversed for the random cue condition. Overall, however, hit reaction time was largely unaffected by environmental cues during ignore instructions.

Correct Rejections

The 2 X 2 repeated measures ANOVA with factors of cue condition (random vs. reliable) and cue type (Likely Old vs. Likely New) revealed a significant main effect of cue type, $F(1,30)=7.00$, $p=.01$, $\eta_p=.18$, with faster reaction times during Likely New relative to Likely Old cues trials (1.32 vs. 1.39). The main effect of cue condition was not significant ($p>.35$). The interaction between cue condition and cue type approached significance, $F(1,30)=3.16$, $p=.09$, $\eta_p=.10$ (Figure 13, bottom left panel). While the interaction did not reach significance, post-hoc tests revealed a significant influence of reliable cues (Likely Old 1.43 vs. Likely New 1.32, $p=.001$), but not random cues (Likely New 1.33 vs. Likely Old 1.36, $p=.35$). Thus, consistent with confidence analyses, there was some evidence that correct rejection reaction time was also influenced by reliable cues during ignore instructions.



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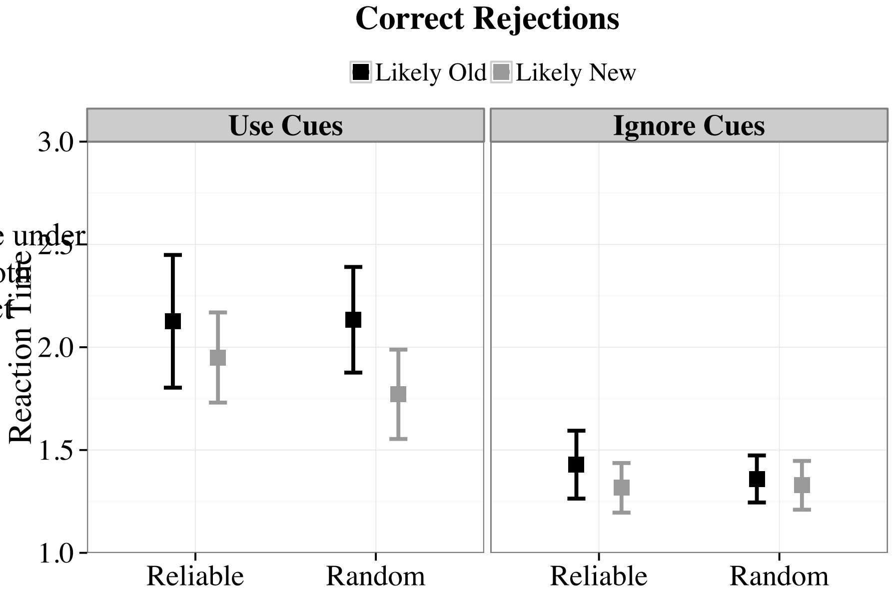

Working Memory. My final set of analyses examined whether participants' ability to use or ignore cues was linked to working memory. Once again, participants' working memory was estimated using a traditional absolute OSPAN score, which is the sum of all the perfectly recalled sets (Unsworth et al., 2005) Table 14 shows the simple correlations between OSPAN scores, shifts in response criterion, and baseline accuracy. When examining Table 14, it is clear that OSPAN scores do not correlate significantly with criterion shifting in any condition. In order to account for individual differences in uncued/baseline accuracy we also conducted hierarchical regressions predicting shifts in response criterion (measured as the difference between criterion measure (C) under Likely New vs. Likely Old cues) using baseline/uncued accuracy in Step 1 as a covariate, and adding OSPAN scores in Step 2, separately for both use and ignore instructions. Critically, during use instructions when adding OSPAN to the model it did not predict increments in criterion shifts for random cues, $\Delta R^2=0.01$, $F(1,28)=0.00$, $p=.99$, nor reliable cues, $\Delta R^2=0.02$, $F(1,28)=0.60$, $p=.45$. Furthermore, during ignore instructions OSPAN also did not predict significant increments in criterion shifts for random cues, $\Delta R^2=0.00$, $F(1,28)=0.16$, p=.69, nor reliable cues, $\Delta R^2=0.00$, $F(1,28)=0.05$, p=.82. The results suggest no relationship between working memory as defined by this span task and participants' shift in response criteria during both use and ignore instructions, for both random and reliable cues.

Table 14. Simple correlations between OSPAN Scores, baseline accuracy, and criterion swing (Experiment 3)

	OSPAN	**Baseline Accuracy**	**Use Random Cues Swing**	**Use Reliable Cues Swing**	**Ignore Random Cues Swing**
OSPAN					
Baseline Accuracy	-0.19				
Use Random Cues Swing	0.02	-0.07			
Use Reliable Cues Swing	0.15	-0.11	0.14		
Ignore Random Cues Swing	-0.09	0.07	0.22	-0.22	
Ignore Reliable Cues Swing	-0.01	-0.18	-0.03	0.32	-0.09

*** p<.001 ** p<.01 * p<.05

4.3 Discussion

Experiment 3 examined whether participants could ignore random environmental cues. Under an expectancy based account, random cues should be ignored since they no longer engender the expectation of familiarity or novelty. Consistent with this prediction, the current experiment revealed that when random and reliable cues were intermixed participants' criteria were not influenced by random cues regardless of whether they were instructed to generally use or ignore cues within the blocks. Importantly, this is the first example to demonstrate a situation where participants could completely eliminate the influence of environmental cues. However, in contrast to previous experiments, the influence of the reliable cues during ignore instructions was also considerably muted, which presumably had something to do with the fact they were encountered intermixed with random cues. The initial analyses of criterion measures failed to detect an effect of reliable cues during ignore instructions, but a more powerful mixed level modeling of trial level response data, as well as the examination of correct rejection confidence and reaction time data, suggested that reliable cues still exerted an influence. Regardless, it was clearly the case the influence of reliable cues was much smaller than in previous experiments.

The finding that participants were able to more effectively ignore reliable cues when intermixed with random cues was unexpected. However, it is currently unclear whether this finding is due to intermixing random cues specifically, or whether this effect was due to intermixing two levels of cue validity in general. Prior experiments where participants were instructed to ignore cues always had a fixed cue validity of 75%, and perhaps it is important to have a consistent mapping between the cue and recognition probe in order for an automatic influence of cues to occur. Additionally, the use of multiple cue validities theoretically increases the number of decision criteria the subject must use during the session because a separate set of confidence standards must be maintained for each cue validity. In fact, prior work using confidence ratings scales has shown that when increasing the number of confidence criteria needed to be maintained, recognition performance was lowered presumably due to the increased criterion noise resulting from the burden of maintaining numerous criteria locations simultaneously (Benjamin et al., 2013). Therefore, it could be the case that intermixing cue types (i.e., the number of criterion needed to be maintained) could change the degree of cue influence. Future work could directly assess this possibility by contrasting a group with 75% cue validity to other groups in which in addition to the 75% condition, there would be other cue validities present. Critically, across groups there would be a matched subset of yoked trials such that all groups would encounter the same exact stimuli during 75% validly cued trials. By comparing the yoked 75% validly cued trials across groups, one could assess how intermixing validities influences 75% valid cues and also determine whether random cues specifically dampen the influence of reliable cues or whether it is a more general phenomenon resulting from intermixing two cue validities.

The current experiment also assessed whether individual differences in criterion shifting were linked to working memory ability or subjective beliefs of cue validity. Once again no relationship was found between working memory scores and criterion shifting during both use or ignore cue instructions for reliable cues, and additionally there was also no relationship observed with random cues. Furthermore, the degree of criterion shifting observed was also unrelated to participants' subjective assessment of cue validity for both use and ignore instructions. This suggests the explicit beliefs regarding the level of cue validity may not be particularly important in how participants rely on environmental cues. However, it is important to note that subjective assessments of cue validity were taken after all recognition testing was completed. Thus, it could be the case that participants' beliefs in cue validity changed as the experiment progressed and collecting subjective ratings of cue influence ratings between each study/test cycle could perhaps assess this change. Additionally, while a large number of participants reported not believing our instructions regarding cue validity, another weakness of posttest questionnaires is that in some cases prompting participants could cause them to reflect and only consider after the fact that they did not find the cues to be the stated probabilities. While the current experiment suggests that global beliefs about the validity of cues may not play a role in the degree of cue influence, based on Experiment 3 it is clearly the case that participants can demonstrate awareness of the degree of cue influence on trial by trial basis.

Chapter 5

Experiment 4

Experiment 4 explored the effect of monetary motivation on the putatively automatic cue influence. Experiments 1 and 2, as well as prior work, clearly demonstrated that participants can dampen but not eliminate the influence of cues through simple instruction. However, one potential argument as to why participants cannot fully ignore cues may be that they are not sufficiently motivated to do so. While participants follow instructions to ignore cues to some extent, demonstrated by the dampened criterion shift during ignore relative to use instructions, it could be the case that providing additional incentive would motivate participants to fully ignore cues. In the current experiment motivation to ignore cues was increased by providing participants with a small monetary endowment from which they lost money when their responses were influenced. If our prior results were simply due to a lack of motivation on the part of the participant then criteria shifts should be much smaller or completely eliminated in the current experiment.

5.1 Methods

Participants. Experiment 4 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.

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

Participants completed a total of 3 study/test cycles and were encouraged to take a brief break between each study/test cycle. During study participants performed a syllable counting task (i.e., does this word contain 1, 2, 3 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 second prior to the recognition probe (160 cued items-80 old, 80 new), while other trials were uncued or 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, participants made a confidence rating using a 6-point scale ranging from 50%-100% in 10% increments with 50% indicating guessing and 100% indicating certainty.

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 given a monetary incentive 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 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. Cue influence was measured using 'old' rates as opposed to criterion (C) in order to avoid hit and false alarm rate corrections. Additionally, 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. Finally, to assess participants' belief regarding cue validity, at the end of recognition testing they were asked whether the cues were accurate approximately 75% of the time using a Yes/No forced response. If participants responded 'No', a follow up question asked them to indicate how accurate they thought the cue was using a percentage.

5.2 Results and Discussion

Four participant's data were excluded leaving 30 participants for analyses. One participant was removed due to chance performance during baseline/uncued performance (d'=0.04) suggesting that he/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, 2 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. Once again while most results will be reported using a conventional ANOVA approach, MLM will be used for additional targeted questions.

Criterion. See Table 15 for descriptive statistics for accuracy and criterion. Changes in criteria were assessed with Signal Detection measure C using a 2 X 2 repeated measures ANOVA with factors of instruction (use vs. ignore) and cue type (Likely Old vs. Likely New). The main effect of instruction was significant, $F(1,29)=4.20$, $p=.05$, $\eta_p=.13$, with participants responding more liberally during use vs. ignore instructions (-0.02 vs. 0.07). Since prior experiments did not find a main effect of instruction, it is likely the addition of the monetary incentive that caused participants to respond more conservatively on cued trials during ignore instructions. The main effect of cue type was significant, $F(1,29)=81.70$, $p<.001$, $\eta_p=.74$, with participants responding more liberally under Likely Old vs. Likely New cues (-0.30 vs. 0.34). However, these main effects were conditioned by a significant interaction between instruction and cue type, $F(1,29)=80.76$, $p<.001$, $\eta_p=.74$. As expected, follow up post-hoc tests (Tukey’s HSD) revealed a significant difference between Likely Old vs. Likely New cues under use instructions (-0.59 vs. 0.55, $p<.001$). However, under ignore instructions the difference between Likely Old vs. Likely New cues was not significant (0.01 vs. 0.13, $p=.50$). Thus, in contrast to Experiments 1 and 2 where cues still exerted an influence during ignore instructions these results suggest that when provided with a monetary incentive to ignore cues, decision criteria are not reliably affected by environmental cues using a basic ANOVA on the summary criterion statistic.

Since participants received feedback regarding how much money they lost after the first ignore test cycle, the two ignore test cycles were assessed separately to examine whether participants learned to more effectively disregard cues during the second ignore test cycle. The 2 X 2 repeated measures ANOVA with factors of ignore test cycle (first vs. second) and cue type (Likely Old vs. Likely New) revealed a marginal main effect of test cycle, $F(1,29)=3.40$, $p=.08$, $\eta_p=.10$, with more conservative responding on the second test cycle relative to the first test cycle (0.04 vs. 0.12). However, the interaction between test cycle and cue type was not significant, $F(1,29)=00$, $p=.99$, $\eta_p=.00$, suggesting that the lack of shift between cue types was similar across ignore test cycles.

Finally, changes in criterion were also assessed as a function of subjective ratings of cue validity. Overall, 50% of participants (N=15) reported that the reliable cues were 75% accurate. Those who reported not believing the reliable cues to be 75% accurate tended to estimate a lower percentage (*M*=53%, *SD*=9.39). To examine whether criterion shifting was related to subjective reports of cue validity, a simple correlation was conducted between criterion swing scores (Likely New C- Likely Old C) and subjective percentages of cue validity, with the actual percentages of cue validity (i.e., 75%) used for those participants who provided a 'yes' response. However, this correlation was not significant for either use ($r=-.12$, $p=.54$) or ignore ($r=-.03$, $p=.88$) instructions. Similar to the previous experiment, these results suggest that subjective estimates of cue validity did not affect the degree of criterion shifting.

Table 15. Average response rates, accuracy, and criterion with standard deviations in parenthesis (Experiment 4)

Use Instructions

	HR		**CR**		**d'**		**C**	
Uncued/Baseline	0.80	(0.12)	0.75	(0.17)	1.69	(0.69)	-0.07	(0.39)
Cued	0.82	(0.06)	0.81	(0.09)	1.86	(0.45)	0.00	(0.23)
Likely Old Cue	0.90	(0.07)	0.54	(0.22)	1.52	(0.61)	-0.60	(0.50)
Likely New Cue	0.59	(0.14)	0.89	(0.08)	1.60	(0.58)	0.55	(0.29)

Ignore Instructions

	HR		**CR**		**d'**		**C**	
Uncued/Baseline	0.78	(0.14)	0.73	(0.14)	1.57	(0.76)	-0.09	(0.37)
Cued	0.73	(0.10)	0.76	(0.13)	1.46	(0.61)	0.08	(0.31)
Likely Old Cue	0.74	(0.11)	0.73	(0.14)	1.39	(0.64)	0.01	(0.30)
Likely New Cue	0.71	(0.13)	0.77	(0.14)	1.45	(0.65)	0.13	(0.38)

Mixed level modeling. While the ANOVA examining criteria did not detect a reliable shift in criterion during ignore instructions, numerically the effect was in the expected direction. Once again, since the standard ANOVA model does not allow one to simultaneously model variation in overall bias and differences in recognition accuracy across subjects, mixed level modeling was used to try to improve the power to detect effects for cues under the ignore instruction condition. Analysis was restricted to ignore test cycles since during use test cycles the anticipated effect of cues was observed using an ANOVA approach and no monetary manipulation was included. Using MLM response (1-old vs. 0-new) was predicted using cue type (1-Likely Old vs. 0-Likely New) and item status (1-old vs.0-new) at the trial level for each participant under ignore instructions. As in Experiment 3, a random intercept and slope component was included such that the subjects' intercepts and slopes of items status were allowed to vary, capturing subject differences in bias and accuracy. Once again linear probability models were used. Initially the full model was fit including both main effects and the two-way interaction and again the final model was determined by systematically removing predictors starting with the highest order interaction until there was a significant decrease in fit using chi-squared tests of log-likelihoods. Coefficient estimates were tested using the Satterthwaite approximation for degrees of freedom.

The full model included both main effects and interaction between item status and cue type. Removing the interaction between item status and cue type did not significantly reduce fit, $\chi^2(1)=0.02$, $p=.89$. However, removing the main effect of cue type did significantly reduce fit, $\chi^2(1)=12.66$, $p<.001$. Thus, the final model (See Table 16) included a significant main effect of item status, $b=0.48$ $t(31)=17.06$, $p<.001$, which simply reflected accuracy and indicated that participants were more likely respond ‘old’ when the item was old vs. new. Critically, the main effect of cue type was also significant, *b*=0.04 *t*(9537)=3.56, *p*<.001, indicating that participants responded ‘old’ more often under Likely Old vs. Likely New cues. Thus, the MLM suggests that cues exerted an influence during ignore instruction, although again this effect was dampened relative to the earlier experiments and pilot work and the model suggests only a 4% shift in response probabilities in response to the cues.

Table 16. Mixed level model for ignore instructions (Experiment 4)

Ignore Instructions

Random Effects:

Groups-Subjects	Variance	St. Dev.	Correlation
(Intercept)	0.01	0.12	
Item Status	0.02	0.14	-0.76

Fixed Effects:

	Estimate	Std. Error	df	t	p-value
(Intercept)	1.23	0.02	30	53.09	<.001***
Item Status	0.48	0.03	31	17.06	<.001***
Cue Type	0.04	0.01	9537	3.56	<.001***

*** p<.001 ** p<.01 * p<.05

Confidence. As in the previous experiment, confidence reports were analyzed in order to assess whether the influence of cues during ignore instructions would perhaps be present using this more finely grained measure. All confidence analyses focused on correct responses (i.e., hits and correct rejections) since incorrect responses were relatively infrequent and provide much less reliable estimates. The main analyses only focused on ignore test cycles, but Table 17 contains descriptive statistics for confidence under both use and ignore instructions. Additionally, ignore test cycle (first vs. second) did not interact with cue type for any of the analyses so only the simple t-tests comparing Likely Old vs. Likely New cues are reported. For hits, there was a significant effect of cue type such that confidence was higher under Likely Old vs. Likely New cues, *t*(29)=3.36, *p*=.001(88.78 vs. 87.03). For correct rejections, there was also a significant effect of cue type with higher confidence under Likely New vs. Likely Old cues, *t*(29)=3.25, *p*=.002 (81.97 vs. 79.22). Critically, these results suggest that participants were not able to fully eliminate the influence of cues from their subjective confidence assessments.

Table 17. Average confidence with standard deviations in parenthesis (Experiment 4)

Use Instructions

	Hits		Correct Rejections	
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

	Hits		Correct Rejections	
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)

Reaction Time. Finally, reaction time was also assessed in order to determine whether the influence of cues during ignore instructions would be present. Subject medians, as opposed to means, were used for estimating reaction time since response time distributions are positively skewed and the median is less influenced by outliers. Once again, the main analyses only focused on ignore test cycles, but Table 18 contains descriptive statistics for reaction under both use and ignore instructions. Additionally, ignore test cycle (first vs. second) did not interact with cue type for any of the analyses so only the simple t-tests comparing Likely Old vs. Likely New cues are reported. For hits, there was a significant effect of cue type such that reaction time was faster under Likely Old vs. Likely New cues, $t(29)=3.58$, $p=.001$ (1.19 vs. 1.42). For correct rejections, there was also a significant effect of cue type with faster reaction under Likely New vs. Likely Old cues, $t(29)=3.04$, $p=.005$ (1.43 vs. 1.58). Analogous to the confidence analyses, the reaction time analyses also suggest that participants were not able to fully eliminate the influence of cues.

Table 18. Average median reaction time in seconds with standard deviations in parenthesis (Experiment 4)

	Hits		Correct Rejections	
Uncued/Baseline	1.62	(0.50)	1.99	(0.78)
Likely Old Cue	1.49	(0.42)	2.17	(0.96)
Likely New Cue	2.08	(0.93)	1.54	(0.37)

Ignore Instructions

	Hits		Correct Rejections	
Uncued/Baseline	1.33	(0.45)	1.55	(0.47)
Likely Old Cue	1.19	(0.33)	1.58	(0.56)
Likely New Cue	1.42	(0.61)	1.43	(0.46)

5.3 Discussion

Experiment 4 examined whether participants could ignore reliable environmental cues when provided with a monetary incentive to do so. A simple explanation for the prior results demonstrating that participants could not fully ignore cues could be that participants were not sufficiently motivated to follow the task instructions. However, it is important to note that participants never completely failed to follow task instructions in prior work since criterion shifts were always found to be smaller during ignore relative to use instructions suggesting that participants were in fact attempting to ignore cues. Additionally, there is also reason to believe that subjects are generally responsive to task instructions during memory research. For example, work by Wallace (Wallace, 1982; Wallace, Sawyer, & Robertson, 1978) and Cox and Dobbins (2011) has examined subject recognition across standard lists, in which old and new items were intermixed, and pure lists in which all of the tested materials were studied. When subjects were informed of this pure list manipulation, their hit rates and confidence assessments were nonetheless highly similar to that of the standard intermixed condition (Cox & Dobbins, 2011). Thus, participants are capable of not attempting to capitalize on situations that could inflate accuracy when asked to do so.

The current experiment increased motivation by providing participants with a small monetary endowment from which they would lose money if they did not fully ignore environmental cues. Critically, results revealed that decision criteria measures did not shift reliably between Likely Old vs. Likely New cues during ignore instructions. However, when using mixed level modeling and assessing confidence and reaction time data a reliable effect of cue type was found, suggesting that the influence of cues was again not fully eliminated.

Nevertheless, it is clearly the case the influence of cues was greatly dampened relative to Experiments 1 and 2. Additionally, replicating Experiment 3 once again it appears as though subjective ratings of cue validity were not linked to the degree of criterion shifting during either use or ignore instructions.

Providing monetary incentive to ignore cues largely dampened the influence of cues, although the effect was not fully eliminated. However, one important factor to consider in the current study is that participants were not forced to read the environmental cue when it appeared on screen. Therefore, it could be the case that the monetary incentive did not diminish the influence of cues on decision making per se, but instead it motivated participants to adopt various strategies that could help them avoid reading the cue (e.g., looking at different portion of the screen). In fact some informal evidence suggest this may have been the case. Two participants were removed from the analyses since they tried to prevent themselves from reading the cue by covering a portion of the screen using their hands; a behavior not noticed during earlier studies. 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. Follow-up work could control for this confound by including catch trials similar to Experiment 1 where following old/new and confidence judgments some portion of the trials would prompt participants to indicate the status of the cue, Likely Old or Likely New, for that trial. Additionally, to further encourage participants to read the cue a negative consequence such as a long time out period could occur every time participants were incorrect or took a long period of time to respond during these cue catch trials.

Chapter 6

General Discussion

While recognition criterion regulation is often thought to be a largely controlled or intentional process (see Introduction), the current set of experiments used an Explicit Memory Cueing paradigm to assess the potential for an automatic or involuntary influence of environmental cues during recognition judgment. Pilot data demonstrated that when participants were instructed to ignore reliable environmental cues (i.e., Likely Old or Likely New) during recognition judgments, they were able to dampen their influence relative to when they were instructed to use these cues. Critically, however, they were not able to fully eliminate the influence of cues as demonstrated by a large and significant swing in decision criteria between Likely Old vs. Likely New cues. These results suggested that perhaps criterion regulation is not entirely under observer control and participants may have difficulties separating internal memory evidence from environmental information.

One hypothesis by which this automatic influence of cues may occur is what I termed the expectancy account of cue influence. Under this account, cues engender an expectation such that Likely New cues elicit an expectation of novelty while Likely Old cues elicit an expectation of familiarity. This cue driven expectancy may then contaminate the processing of the memory signal, such that items following a Likely Old cue are more likely to be processed as old, whereas items following a Likely New cue are more likely to be processed as new. The notion that expectancy can drive or distort the interpretation of memory signals is somewhat similar to literature examining fluency or misattribution effects, where fluency (i.e. ease of processing) is misattributed as familiarity or some other characteristic relevant to a current judgment (e.g.

Jacoby & Dallas, 1981; Jacoby & Whitehouse, 1989; Whittlesea, 1993; Whittlesea, Jacoby, & Girard, 1990). For example, in a basic fluency task participants are more likely to falsely recognize an item (e.g., melon) as studied when it is preceded by an identical context prime (e.g., 'melon') relative to an unrelated prime (e.g., 'xoxoxoxo') (Jacoby & Whitehouse, 1989). However, during this paradigm the prime drives *perceptual fluency* such that it facilitates the identification of the probes presumably at the lexical level (i.e., ease of perceptual processing), which is then misattributed to feelings of familiarity. In contrast, in the explicit cueing paradigm the cues likely drive *memory-processing fluency* (i.e., oldness or newness), which is then inappropriately (since participants are supposed to be ignoring cues) attributed to familiarity or novelty.

The current experiments assessed the expectancy hypothesis in light of the following questions: (1) Could the influence of cues be eliminated if the cues no longer served a preparatory role and were instead presented after the recognition probe? (2) Could observers demonstrate any awareness of the automatic influence of cues? (3) Could observers eliminate the influence of cues that are no longer predictive (i.e., random cues), and finally (4) could increased motivation through monetary incentives eliminate the automatic influence of cues?

The expectancy hypothesis was partially supported in the current set of experiments. In Experiment 1, cues still exerted a large effect when they occurred after the recognition probe and this effect did not diminish across cue lags when measured by mean shifts in criteria. While this finding is inconsistent with an expectancy account, partial support was found for the expectancy hypothesis when assessing confidence and confidence reaction time during which the effect of cues decreased as the probe to cue lag became longer. However, as outlined in the Experiment 1 discussion, it could be the case that participants did not fully commit to a decision before the cue appeared allowing cues to still exert a fairly robust influence even at long lags. Regardless, overall Experiment 1 demonstrated that the automatic influence of cues occurred even if a cue appeared after the recognition probe. Furthermore, Experiment 2 demonstrated that participants had some awareness of the automatic influence of cues such that participants were able to accurately assess trials during which cues exerted a greater influence. In Experiment 3, participants were able to ignore random cues intermixed with reliable cues, supporting the expectancy hypothesis which predicts that uninformative cues should not exert an influence because they are no longer reliable indicators of expectations; however, the influence of reliable cues was also considerably dampened (relative to Experiments 1 and 2) in this condition and only present when using more powerful analyses, such as mixed level modeling, or potentially more sensitive measures, such as confidence and reaction time. In Experiment 4, when participants were motivated through monetary incentives to ignore cues, the influence of cues was not present when measured by shifts in decision criteria but again it was present using mixed level modeling of responses and during confidence and reaction time analyses.

Overall, the findings demonstrate that in no case were subjects able to fully eliminate the effect of reliable environmental cues they were instructed to ignore. Indeed, the only condition during which the influence of cues was fully eliminated was in Experiment 3 when participants were provided with random cues. While the influence of reliable cues was greatly dampened in Experiments 3 and 4 such that significant mean level shifts in the signal detection criterion measure were not observed, more powerful analyses and more sensitive and introspective measures demonstrated a reliable influence of cues during ignore instructions. The fact that cueing effects were often more robust in correct rejections confidence as opposed to hits is not necessarily surprising given prior work examining the influence of cues under a dual process framework (Jaeger, Cox, & Dobbins, 2012a). In Jaeger, Cox, and Dobbins (2012) the authors concluded that cues were more likely to affect familiarity as opposed to recollective processes and, because of the high confidence associated with recollected hits, the effect of cues on hit confidence were largely eliminated. Furthermore, the finding that the effects of cueing were sometimes observed in reports of confidence as opposed to summary accuracy or criterion measures is consistent with other work which suggests that introspective (i.e., metacognitive) measures are sometimes more sensitive to context effects than measures of recognition (Hanczakowski, Zawadzka, & Coote, 2014). However, further research will need to be conducted to rule out the possibility that these dampened effects of cues were not simply due to participants avoiding reading the cues and subsequent designs should uniformly use catch trials and penalties to help force participants to fully process the cues and maintain their status in working memory while assessing memoranda. Should such designs converge with the current findings it will suggest that environmental cues are particularly pernicious in their influence and that they fundamentally alter the way memoranda are processed despite observers' intentions otherwise.

The finding that participants were able to track gradations of cue influence during ignore instructions suggests that they had some level of awareness regarding the automatic influence of cues and they could perhaps be using strategies that try to offset this influence. Somewhat similar to the anchoring and adjustment heuristic (Tversky & Kahneman, 1974), one hypothesis regarding the nature of this influence could be that cues automatically anchor participants towards a particular response or judgment. The iconic example of the anchoring heuristic in Tversky and Kahneman (1974) described a study where participants were asked whether the percentage of African countries in the United Nations was more or less than a random number between 0-100 that was determined by a spinning wheel. After this initial judgment, participants were asked to indicate their specific percentage estimate by moving their answer up or down from this initial starting point. Results revealed that the initial starting point had a large impact on final estimates, with the initial starting points of 10% and 65% yielding median estimates of 25% and 45%. Thus, these types of anchoring effects demonstrates that observers' initial starting point can highly influence their judgment on a particular task and this occurs even when participants were made aware of anchoring effect or when they were incentivized to ignore the anchors (Tversky & Kahneman, 1974; Wilson, Houston, Etling, & Brekke, 1996). It is also important to note that these anchoring effects occur despite the anchors being completely uninformative (Chapman & Johnson, 2002).

Given this pervasive influence of anchors (see Furnham & Boo, 2011 for a review) it may not be particularly surprising that environmental cues that are reliable could perhaps anchor participants towards a particular response. When instructed to use cues, participants may evaluate the strength of their memory evidence and adjust their decision criteria accordingly. Under this characterization, when instructed to ignore cues, participants' attempts to overcome the anchoring effect would somehow be inefficient and lead to biased responding. For example, participants could underestimate their initial degree of cue influence, presumably because they may not be able to fully separate internal memory evidence from environmental cues, and therefore they may not apply the appropriate adjustment. The finding that cues exert an influence even when presented at long lags after the recognition probe may be somewhat problematic for the hypothesis that cues may serve as anchors; however, if we assume that participants did not fully commit to their decision during the long cue lag period, then the cues could still potentially result in these anchoring type effects. Furthermore, if one assumes that the ability to adjust away from environmental cues may require some form of cognitive control then speeding participants during their recognition judgments may even further prevent participants from appropriately adjusting. Future work could assess this question by varying response deadlines during recognition judgments, with the prediction that shorter response deadlines may result in the largest cueing effects under ignore instructions since participants do not have sufficient time to recruit the controlled processes needed for adjustment.

Importantly, the ability to overcome an effect of anchoring would likely require that participants are aware that anchoring effects occurred. In the case of environmental cues, Experiment 2 demonstrated that participants were perhaps aware of the influence of cues; however, they were clearly not able to able to eliminate their influence despite this awareness. While research in the judgment and decision making literature is somewhat mixed on whether warnings can decreases anchoring effects (Chapman & Johnson, 2002), there is some evidence to suggest that forewarning participants does not decrease the size of the anchoring effect (Wilson et al., 1996), particularly for experimentally provided anchors (Epley & Gilovich, 2005). This is consistent with the notion that the automatic influence of environmental cues may be difficult to eliminate despite awareness. Nevertheless, if observers are aware of the influence of cues there could be situations during which they could overly adjust in order overcome this unwanted influence. This type of over adjustment could result in a contrarian stance such that participants start responding opposite of the cues (e.g., more liberally under Likely New as opposed to Likely Old cues).

In the current experiments, there was some evidence that a proportion of participants started to adopt a contrarian stance, specifically during Experiment 4 during which participants were given a monetary incentive to ignore cues. When calculating criterion shifts scores (Likely

New C minus Likely Old C) those participants who respond in accordance with the cue would have positive values, while those participants who take a contrarian stance and respond opposite of the cues would have negative values. Generally, most participants' criteria shifts were in the direction of the cues during ignore instructions, and this was particularly true for Experiments 1 and 2 where reliable shifts in response criterion were observed. Specifically, in Experiment 1 the number of participant who had had negative criterion shift scores was 14.8% (N=4) for short cue lag, 11.1% (N=3) for medium cue lag, and 18.5% (N=5) for long cue lag. In Experiment 2, not a single participant had a negative shift score. In contrast, for Experiments 3 and 4 where no mean level criterion shifts were observed, a much greater number of participants had negative shift scores. In Experiment 3, 32% of participants (N=10) had negative shift scores for reliable cues under ignore instructions and for Experiment 4 it was 47% (N=14). Thus, it could be that case that one of the reasons mean level criterion shifts during these experiments failed to be detected was because a larger proportion of participants attempted to limit criterion influences by counter-responding to the cues on some trials. Nonetheless, average effects were demonstrated via MLM and inspection of confidence and reaction time. Particularly in the case of Experiment 4 where participants were given large incentives to ignore cues, they may be more likely to overcompensate using some type of contrarian strategy. However, this idea is speculative at this point since an increased number of negative shift scores would also occur simply due to noise around the ideal shift score of zero if participants fully ignored cues.

A secondary question examined in Experiments 2 and 3 was whether working memory was linked to criterion shifting during either use or ignore instructions. Neither experiment found a relationship between criterion shifts and performance on the operation span task. However, the sample size in each individual experiment was relatively small and the analyses could potentially be underpowered to detect small effects. In order to increase power, I also conducted a hierarchical regression combining data from both experiments, using only data from reliably cued trials in Experiment 3. After controlling for uncued/baseline performance and experiment, adding OSPAN to the model did not predict increments in criterion shifts for use, $\Delta R^2=0.03$, $F(1,51)=2.19$, $p=.15$, or ignore instructions, $\Delta R^2=0.00$, $F(1,51)=0.50$, $p=.49$. Additionally, OSPAN was also not significant when predicting shifts in criterion during ignore instructions after controlling for experiment and shifts in criterion under use instructions, $\Delta R^2=0.00$, $F(1,50)=0.02$, $p=.88$. While not reported, OSPAN was also not significantly linked with several other measures of cue influence including changes in reaction time and confidence, *p*s>.14. Furthermore, recent work from the laboratory has also failed to detect a relationship between working memory and criterion shifting in both younger and older adults (Konkel, Selmeczy, & Dobbins, 2014). Thus, it appears as though there may be some evidence that cognitive control mechanisms tapped by complex span tasks are unrelated to criteria regulation. However, the range of span scores may be relatively limited in the current population ($M=52.76$, SD=14.10, *Range*=15-75), and therefore the failure to detect a relationship with working memory could be due to this restricted range. Additionally, while working memory is thought to be reflect maintenance and manipulation of information, particularly when control is needed to override automatic response tendencies (Engle & Kane, 2004; Unsworth & Engle, 2007), other research in executive functioning treats various factors such as shifting, updating, and inhibition as potentially separable factors that contribute to controlled processes (Miyake et al., 2000). Thus, it could be the case that a relationship may be observed with criterion regulation when using a different measure that may tap different aspects of cognitive control. For example, if ignoring cues requires inhibiting a prepotent cue driven response, then measures of response inhibition (e.g., go-nogo tasks) may better correlate with shifts in criteria. Regardless, future work is needed to more thoroughly address the link between criterion regulation and cognitive control using several measures of control and a larger, more diverse sample.

Finally, Experiments 3 and 4 also demonstrated that subjective beliefs regarding cue validity were not associated with the degree of criterion shifting that occurred either during use or ignore instructions. This again serves to demonstrate the cueing effects were robust and even participants who did not believe the instructions regarding cue validity still displayed cueing effects. However, the current experiments only prompted participants to indicate their belief in cue validity after all recognition testing was completed and thus it is possible that these reports do not map closely onto those that were present and perhaps fluctuating during the course of actual testing. Additionally, asking all participants to report a percentage estimate of cue validity, as opposed to only those participants that gave a 'No' response, may provide a better measure with more variability to detect a relationship between beliefs in cue validity and degree of criterion shifting.

6.1 Conclusions

The current experiments demonstrated a pernicious influence of reliable environmental cues. While the influence of cues was dampened during certain manipulations, across four experiments participants were never able to fully eliminate the influence of reliable environmental cues with cueing effects present in either overall criterion estimates, mixed level linear probability model betas, mean confidence, and/or median reaction time. This demonstrates a novel finding that automatic or unintentional non-memory cue driven processes can influence recognition decision processes. Furthermore, despite evidence that participants seem aware of that unintentional influences may be occurring, their responses were still contaminated. Thus, observers were unable to fully separate internal memory evidence from environmental information, even in a simple laboratory based task where explicit instructions were provided to ignore environmental cues. In real world situations recognition judgments may often be made in the context of several environmental cues, yet we probably feel as though we have complete control over our decision and we can limit their influence if we wish to do so. For example, when a spouse indicates that the movie you consider renting is one you have likely seen before, your decision about whether the movie is familiar or novel may be influenced such that your original memory signal cannot be fully separated from your spouse's recommendation. Critically, unwanted cue driven expectancies on observers' memory signal are particularly problematic for situations where one's goal is to keep memories uncontaminated by environmental information (e.g., eyewitness testimony) or situations during which a source of information may suddenly become unreliable and must now be ignored. The current data demonstrate that even if we are aware that our recognition may have been involuntarily influenced, we lack the ability to fully negate or offset the influence.

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