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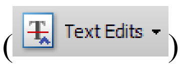
- Use the **Text Edits tool** (

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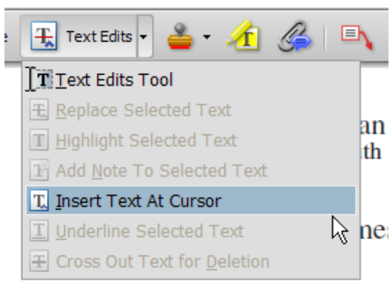

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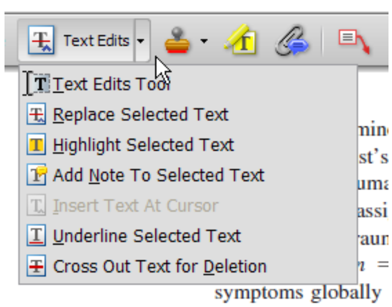

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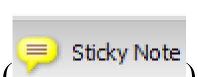
- Use the **Sticky Note tool** (

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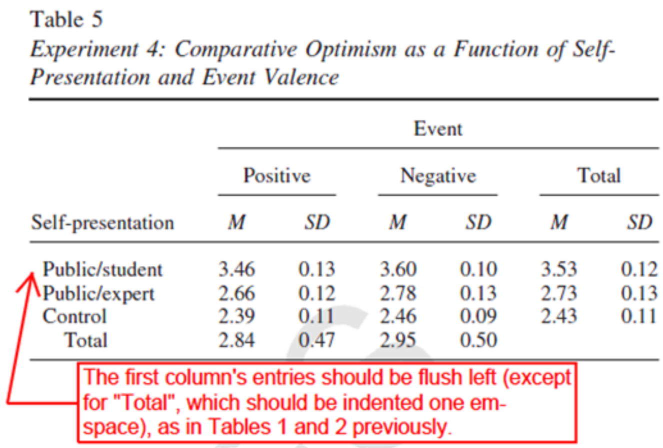
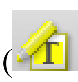
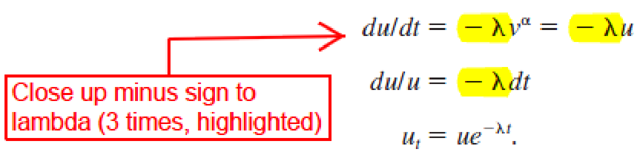


Table 5
Experiment 4: Comparative Optimism as a Function of Self-Presentation and Event Valence

	Event					
	Positive		Negative		Total	
Self-presentation	*M*	*SD*	*M*	*SD*	*M*	*SD*
Public/student	3.46	0.13	3.60	0.10	3.53	0.12
Public/expert	2.66	0.12	2.78	0.13	2.73	0.13
Control	2.39	0.11	2.46	0.09	2.43	0.11
Total	2.84	0.47	2.95	0.50		

- Use the **Highlight tool** () to indicate font problems, bad breaks, and other textual inconsistencies. Describe the inconsistencies with the callout tool (shown) or a sticky note. One callout (or sticky note) can describe many changes.



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Attention, Awareness of Contingencies, and Control in Spatial Localization: A Qualitative Difference Approach

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Universidad de Granada

Chris Fiacconi and Bruce Milliken
McMaster University

The qualitative difference method for distinguishing between aware and unaware processes was applied here to a spatial priming task. Participants were asked simply to locate a target stimulus that appeared in one of four locations, and this target stimulus was preceded by a prime in one of the same four locations. The prime location predicted the location of the target with high probability ($p = .75$), but prime and target mismatched on a task-relevant feature (identity, color). Across 5 experiments, we observed repetition costs in the absence of awareness of the contingency, and repetition benefits in the presence of awareness of the contingency. These results were particularly clear-cut in Experiment 4, in which awareness was defined by reference to self-reported strategy use. Finally, Experiment 5 showed that frequency-based implicit learning effects were present in our experiments but that these implicit learning effects were not strong enough to override repetition costs that pushed performance in the opposite direction. The results of these experiments constitute a novel application of the qualitative difference method to the study of awareness, learning of contingencies, and strategic control.

Keywords: awareness, contingencies, qualitative differences, spatial priming, verbal report

The distinction between conscious and unconscious influences on behavior has been a controversial issue in experimental psychology for decades. One approach to this subject is found in studies that report qualitative differences in performance as a function of conscious awareness of task relevant information (Cheesman & Merikle, 1986; Daza, Ortells, & Fox, 2002; Eimer & Schlagheken, 2002; Merikle & Joordens, 1997). In the present study, we report an application of the qualitative difference approach to a spatial priming task that illustrates a compelling shift in behavior as a function of subjective verbal report. To set the present study in context, the remainder of the Introduction provides brief reviews of the rationale underlying the qualitative difference approach, of other applications of the qualitative difference approach in priming studies, and of the spatial priming literature that is most relevant to the present work.

Joaquín M. M. Vaquero, Departamento de Psicología Experimental y Fisiología del Comportamiento, Universidad de Granada; Chris Fiacconi and Bruce Milliken, Department of Psychology, Neuroscience and Behaviour, McMaster University.

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Qualitative Differences and the Conscious/Unconscious Distinction

The qualitative difference approach to distinguishing between conscious and unconscious processes is an alternative to a research strategy that aims solely to demonstrate dissociations between measures of conscious and unconscious processes. According to the dissociation method, evidence for unconscious processes hinges on two key observations. First, null awareness must be demonstrated when participants are required to report awareness to the best of their ability (e.g., using subjective report or forced-choice discrimination methods). Second, under comparable conditions, a significant influence of that same information must be observed on some other behavioral measure. A limitation of this research strategy is that it requires that the first measure be an exhaustive and exclusive measure of conscious processes. In fact, a number of researchers have argued that such measures may not be sensitive to all information for which the participant is aware (Eriksen, 1960; Holender, 1986; Shanks & St. John, 1994) and other times may be sensitive to both conscious and unconscious processes (Cheesman & Merikle, 1984; Destrebecqz & Cleeremans, 2001; Eriksen, 1960; Jacoby, 1991; Merikle & Reingold, 1992).

In response to this problem, a number of researchers have suggested the need to use an additional criterion to distinguish conscious from unconscious processes, in particular that putative measures of conscious and unconscious influences lead to qualitatively different patterns of performance (e.g., Cheesman & Merikle, 1986; Dixon, 1981; Holender, 1986; Merikle & Cheesman, 1987). Such a demonstration would validate the awareness measure used and would illustrate that conscious and unconscious processes produce effects on behavior that differ profoundly in meaning, rather than merely in size. AQ: 1

Applications of the Qualitative Difference Logic in Priming Studies

Priming procedures have been especially useful in demonstrating qualitative differences in performance as a function of awareness. Cheesman and Merikle (1986) presented participants with a prime color-word such as "red" followed by a color patch. The Stroop effect using this method has been shown to be sensitive to the relative proportions of congruent and incongruent trials, with larger Stroop effects for high proportion congruent than for low proportion congruent conditions (Glaser & Glaser, 1982; Taylor, 1977). This proportion congruent effect is often attributed to adaptations of processing strategies in accord with the predictive information provided by the word primes (e.g. Posner & Snyder, 1975; Underwood, 1982). Under the assumption that changes in processing strategy are brought about by consciously controlled processes, one might expect such a proportion congruent effect only when participants are aware of the proportion of congruent trials. Indeed, this is the result reported by Cheesman and Merikle (1986; Experiments 2 and 3). When color words were presented so briefly that participants claimed not to be able to perceive them consciously, a Stroop effect that was insensitive to proportion congruent was observed. In contrast, when color words were presented for a sufficient duration to be perceived consciously, a Stroop effect that was sensitive to proportion congruent was observed.

An arguably stronger pattern of qualitative differences would be characterized by opposite patterns of results as a function of awareness (Daza, Ortells, & Fox, 2002; Eimer & Schlaghecken, AQ: 2 2002; Merikle & Cheesman, 1987), and this result has also been observed in several studies. For example, in a two-color variant of the Stroop task described above, Merikle and Joordens (1997) set the proportion of incongruent trials at .80 and the proportion of congruent trials at .20. With this procedure, RTs were faster for incongruent trials than for congruent trials, but only when the prime words were displayed long enough to be above the subjective threshold of awareness. In contrast, when the prime words were masked so that they were below the subjective threshold of awareness, the usual Stroop effect was observed.

More recently, Eimer and Schlaghecken (2002) reported an interesting qualitative difference as a function of prime awareness in a study of directional priming. The primes and targets in this study were left-pointing and right-pointing arrows, and participants were asked to respond to the direction indicated by the target arrow. Prior studies had demonstrated that when the presentation of primes is brief and targets follow primes after an interval of about 150 ms, responses to compatible prime-target (e.g., a right-pointing arrow followed by a right-pointing arrow) pairs can be slower than for incompatible prime-target pairs (a right-pointing arrow followed by a left-pointing arrow; see Eimer, 1999). This result has been attributed to an automatic response inhibition process that follows response activation and that helps maintain stability in the motor system. It is interesting to note that this response inhibition effect depends critically on the visibility of the primes. Across two experiments, Eimer and Schlaghecken (2002) manipulated prime visibility, in one case by varying the density of a mask that followed the prime (Experiment 1) and in the other by varying the presentation duration of the prime (Experiment 2). In both experiments, the directional compatibility effect was negative (slower RTs for compatible trials) under conditions in which participants claimed not to be able to consciously identify the prime, and positive under conditions in which participants claimed to be able to consciously identify the prime. In addition, prime identification thresholds (in terms of mask density or presentation duration) were positively correlated with the point at which positive compatibility effects reversed sign and became negative. These results illustrate the qualitative difference logic nicely, as they suggest that aware and unaware states can produce meaningful differences in behavior.

One final example of a qualitative difference is particularly relevant to the present study. McCormick (1997) used a spatial cuing procedure in which an abrupt onset peripheral cue was followed by a single target, either an X or O. The participants' task was to identify the target as quickly and accurately as possible. Critically, there were two possible locations at which the cues and targets could appear, and the likelihood of the target appearing at the cued location was .25. McCormick reasoned that if participants could perceive the cue consciously then they ought to predict the target to appear at the location opposite the cue, perhaps resulting in faster performance for uncued (expected) than for cued (unexpected) trials. In contrast, if participants could not perceive the cue consciously, then any unconscious/automatic influence of the cue on performance would be left unopposed, perhaps resulting in faster responses for cued than for uncued trials. Indeed, this is the result that was observed across three experiments, with different levels of subjective awareness produced by manipulating contrast/lighting in Experiment 1, and reported verbally on a trial-to-trial basis by participants in Experiments 2 and 3. Again, these results illustrate that subjective reports of awareness and unawareness can be accompanied by meaningful, qualitative differences in behavior.

The Present Study

The present study was somewhat similar to that of McCormick (1997), in that we aimed to examine the role of awareness in the context of a spatial priming procedure. However, there are two critical differences between the McCormick study and that described here. First, whereas McCormick used a conventional spatial cueing procedure that required a target discrimination response (i.e., is the lone target an X or an O?) following an abrupt onset peripheral cue, we used a spatial priming task in which participants could ignore all items in a prime display but were to locate a target while ignoring a distractor in a following probe display. As will become evident, this procedure affords unique opportunities to manipulate awareness in the context of a spatial priming procedure. Second, in the McCormick study, and in all of the qualitative difference studies reviewed above, researchers have been interested in the role of awareness of a prime (or cue) in controlling priming (or cueing) effects, with awareness manipulated in most cases by varying factors that alter visibility of the prime (e.g., prime-mask stimulus onset asynchrony, mask density, prime presentation duration). In contrast, in the present study, we were interested not in the role of awareness of a prime in controlling priming effects but instead in the role of awareness of a predictable relationship between a prime and following probe in controlling priming effects. Furthermore, we did not vary the visibility of the prime to produce aware and unaware participants but instead asked participants questions about the relationship between a clearly visible prime and probe that allowed us to categorize post-experiment whether participants were aware or unaware of a predictable relationship. What we ultimately report is that spatial priming effects can vary qualitatively as a function of awareness of such a relationship.

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The task used in Experiments 1 and 2 of the present study is depicted in Figure 1. The three displays in the top half of the figure are prime displays used to create three different conditions with respect to the probe display in the bottom half of the figure. The task is to indicate the location of the target O in the probe display only. In the Location-repeat/Identity-mismatch condition, the target O in the probe display appears in the location occupied by the X in the prime display. In the Location-repeat/Identity-match condition, the target O in the probe display appears in the location occupied by the matching O in the prime display. And finally, in the Location-change condition, the probe target O appears in a location that was not occupied in the prime display.

Park and Kanwisher (1994) conducted an experiment using a similar procedure, with participants required to locate a target and ignore a distractor in a probe display following presentation of a prime display that required no response. They noted that performance was fastest in the Location-repeat/Identity-match condition, slowest in the Location-repeat/Identity-mismatch condition, and intermediate in the Location-change condition. Following the object file review framework introduced by Kahneman, Treisman, and Gibbs (1992), Park and Kanwisher (1994) proposed that probe stimuli automatically cue the retrieval of temporary episodic representations (object files) of spatio-temporally similar prime stimuli. On trials where a probe appears in a location that was occupied by a mismatching letter in the preceding prime display, some additional time is necessary to update the contents of the object file with the new letter identity. In contrast, when a probe appears in a location that was occupied by a matching letter in the preceding prime display, rapid integration of the current perceptual target into an already existing object file leads to particularly efficient responding. These behavioral consequences of object file updating processes account for the slower responses on Location-repeat/Identity-mismatch trials than on Location-repeat/Identity-match trials (see also Hommel, 1998). The intermediate level of performance for the Location-change condition in the Park and Kanwisher (1994) study suggests that object file updating can produce either a benefit (as in the Location-repeat/Identity-match condition) or a cost (as in the Location-repeat/Identity-mismatch condition) relative to responding in a condition in which a new object file must be encoded (as in the Location-change condition).

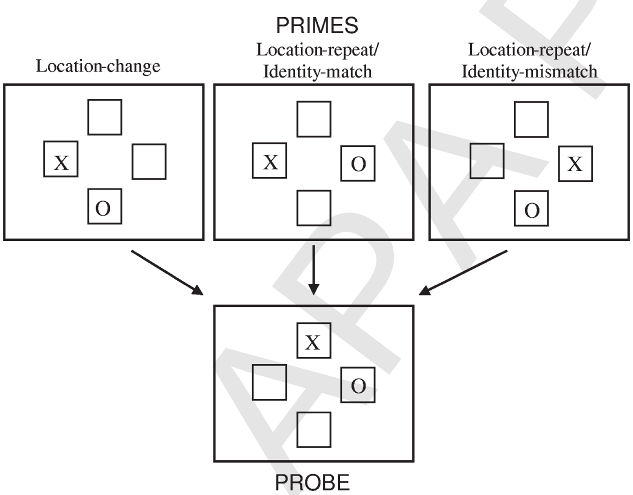

Figure 1. Examples of the three location repetition conditions used in Experiments 1 and 2. In the Location-change condition, note that the probe target O appears in a location that was unoccupied in the preceding prime. In the Location-repeat/Identity-match condition, the probe target O appears in a location that was occupied by an identical O in the preceding prime. In the Location-repeat/Identity-mismatch condition, the probe target O appears in a location that was occupied by an X in the preceding prime. In all three conditions, the probe distractor X appears in a location that was unoccupied in the preceding prime.

Although the object file updating hypothesis has proved useful in interpreting the results of a wide range of studies, other recent studies suggest that object file updating may be less obligatory, or at least may affect behavior less obligatorily, than is widely presumed. In particular, using a range of procedures that were quite similar to that of Park and Kanwisher (1994); Milliken, Leboe, and Bates (2010) found performance in a localization task to be no different for Location-repeat/Identity-match and Location-repeat/Identity-mismatch trials, with both conditions responded to more slowly than control trials. In effect, the pattern of results corresponded to what is referred to as "inhibition of return" (Posner & Cohen, 1984), in that performance was slow in all conditions in which a target appeared in a location that was occupied in the preceding display, independent of the match between those stimuli across the prime and probe displays. If object file updating processes were strongly automatic, then it ought not to have been possible to observe equivalent inhibition of return effects for the Location-repeat/Identity-match and Location-repeat/Identity-mismatch conditions. Contrary to the strong automaticity hypothesis, differences in performance in these two conditions appeared or disappeared as a function of contextual factors that either encouraged or discouraged use of the immediately preceding processing episode (Milliken et al., 2010).

In conducting these experiments, it was noted casually that it was remarkably difficult to encourage participants to use the immediately preceding processing episode, even in the face of contingencies between the prime and the probe that were relatively high. For example, in one experiment, the probe target O appeared in the location of the O in the prime display on 42% of trials, in the location of the X in the prime display on 42% of trials, and in each of the two previously unoccupied locations on only about 8% of the trials. Yet, response times in this experiment were fastest for the very infrequent Location-change trials and equally slow on Location-repeat/Identity-mismatch and Location-repeat/Identity-match trials. This result leads one to the conclusion that a veritable sledgehammer is needed to induce use of the immediately preceding prime episode in some contexts. The realization that such a sledgehammer manipulation might be reliably accompanied by awareness of the high contingency and that it might also shift performance qualitatively (from an inhibition of return to a facilitation effect) served as the motivation for the present set of experiments (see also Milliken & Rock, 1997).

Experiment 1

In this experiment, an X and an O appeared in two of four boxes in two consecutive displays, a prime display followed by a probe display. Participants were required to respond only to the location of the O in the probe display. A key feature of this experiment concerned the relative proportions of three trial types: 75% of the trials belonged to

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the Location-repeat/Identity-mismatch condition in which the probe target O appeared in the same location as the X in the preceding prime display; 8% of the trials belonged to the Location-repeat/Identity-match condition in which the probe target O appeared in the same location as the O in the preceding prime display; and 17% of the trials belonged to the Location-change condition in which the probe target O appeared in a location that was unoccupied in the preceding prime display. If participants were to notice the contingent relation between the location of the prime X and probe target O, then one might expect participants to use a predictive strategy to respond to the probe target O, thus producing particularly fast responses in the Location-repeat/Identity-mismatch condition. On the other hand, our casual observation in prior studies had revealed that participants often do not notice such contingencies, perhaps because attention is unlikely to focus on the prime X when the task requires a response to a following target O. In effect, using identities (X and O) that mismatch across prime and probe might well afford an opportunity to observe "inattentional blindness" to what might otherwise be an obvious contingent relation (Mack & Rock, 1998; Most et al., 2001; Simons & Chabris, 1999).

Method

Participants

Fourteen undergraduate students from an introductory psychology course at McMaster University participated in exchange for course credit. The mean age of participants was 19 years. All participants had normal or corrected-to-normal visual acuity.

Apparatus and Stimuli

The experiment was carried out on a Pentium IBM compatible computer equipped with a Sony SVGA color monitor. Participants were seated approximately 40 cm from the monitor. Responses were made using a Gravis digital joystick that was interfaced to the computer via a standard game port. Response times were measured using the routines published by Bovens and Brysbaert (1990).

The stimuli in any given display appeared in two of four locations, marked by light grey boxes just above, below, left, or right of fixation. The boxes were positioned such that the horizontal visual angle between the centers of the left and right boxes was 5.0° and the vertical visual angle between the centers of the top and bottom boxes was 4.3°. Each box subtended a visual angle of 1.6° horizontally and 1.7° vertically. The letter O appeared in the center of one of the boxes and the letter X appeared inside another of the boxes in each stimulus display. Both letters were light grey and subtended 0.9° horizontally and 1.0° vertically.

Procedure and Design

Instructions appeared on the screen at the beginning of the experiment and were subsequently clarified by the experimenter to ensure that they were understood. Participants were told that an X and an O would each appear in two of the four boxes on both of two consecutive displays (see Figure 1). The task was to ignore the distractor letter X and indicate the location of the target letter O for the probe display only; no response was required for the prime display. Participants recorded their responses by moving a joystick in a direction that was spatially compatible with the location of the target (up, down, left, or right). Speed and accuracy of responses were both emphasized. Incorrect responses were indicated to the subject by a beep that sounded from the computer and responses that took longer than 3000 ms were also scored as incorrect.

Participants began each trial by depressing the start key on the joystick. The four location markers subsequently appeared on the screen and remained for the duration of the trial. One second after the onset of the location markers, the prime display appeared and remained on the screen for a duration of 157 ms. Following offset of the prime, there was a brief pause of 500 ms, followed by onset of the probe display. The probe display also remained visible for 157 ms. At this point, participants were to indicate the location of the target letter O with the appropriate joystick response. After each joystick response, a brief 50 ms click was produced, which signaled to the participant that their response had been registered. A louder 'beep' was emitted if the participant responded incorrectly. After the participant responded to the probe display, the screen was cleared, and a prompt appeared instructing the participant to begin a new trial.

There were three conditions tested in this experiment. In the Location-change condition, both the O and X of the probe display appeared in locations that were unoccupied in the prime display. In the Location-repeat/Identity-match condition, the O in the probe display appeared in the location occupied by the O in the prime display while the X in the probe display appeared in an unoccupied prime location. In the Location-repeat/Identity-mismatch condition, the O in the probe display appeared in the location occupied by the X in the prime display while the X in the probe display appeared in an unoccupied prime location. The relative proportions of these three conditions were as follows: .75 Location-repeat/Identity-mismatch condition, .08 Location-repeat/Identity-match condition, and .17 Location-change condition. These relative proportions were achieved by including eighteen Location-repeat/Identity-mismatch trials, two Location-repeat/Identity-match trials, and four Location-change trials in each block of 24 trials.

Each participant completed a practice session in which the relative proportions of the three conditions was the same as in the test session and in which they made a minimum of three correct responses per condition, which resulted in a practice session of 48 trials for most participants. The test session consisted of 240 trials with a one-minute break every 100 trials. When participants finished the task, they were shown a drawing that depicted the three experimental conditions (Location-change, Location-repeat/Identity-match, and Location-repeat/Identity-mismatch), and they were required to estimate the percentage of trials that belonged to each of the conditions.[1] Fn1

[1] In Experiment 1, we asked participants to give estimates of the percentage of trials in each of three conditions, but we were primarily interested in their estimate for the Location-repeat/Identity-mismatch condition, as this was condition that occurred on 75% of experimental trials. In Experiment 2, the match condition was of primary interest as it occurred on 75% of trials, and we simplified our question to participants at the end of the experiment by asking for an estimate of the percentage of Location-repeat/Identity-match trials and an estimate of the percentage of all other trials types. Participants were instructed that these two estimates had to sum to 100%. In Experiments 3 and 4, participants were asked for estimates of the percentage of Location-repeat/mismatch (identity-mismatch in Experiment 3, color-mismatch in Experiment 4) and Location-change trials, with these two estimates having to sum to 100%.

Results

Response times for correct trials in each repetition condition (Location-change, Location-repeat/Identity-match, Location-Repeat/Identity-mismatch) were first submitted to an outlier analysis that eliminated suspiciously short or long RTs (Van Selst & Jolicoeur, 1994). Mean correct RTs were then computed using the remaining observations, and these mean RTs and corresponding error percentages were submitted to one-way repeated measures ANOVAs that treated repetition as the lone within-subject variable. Mean RTs in each condition, collapsed across participants, are displayed in Table 1. Corresponding error percentages for each condition are presented in Table 2. All post-hoc tests were evaluated against an alpha criterion of .05, with a Bonferroni correction to protect against alpha slippage.

In the analysis of RTs, there was a significant main effect of repetition, $F(2, 26) = 15.68$, $p < .001$, $\eta_p^2 = .55$. Post-hoc t-tests indicated that responses were significantly faster in the Location-change condition (454 ms) than in both Location-repeat/Identity-match (490 ms) and Location-repeat/Identity-mismatch (483 ms) conditions. The difference between the Location-repeat/Identity-match and Location-repeat/Identity-mismatch conditions was not significant. In the corresponding analysis of error percentages, there were no significant effects.

Participants' estimates of the percentage of Location-repeat/Identity-mismatch trials revealed a striking absence of awareness of the high likelihood of such trials. In particular, the mean percentage estimate for the Location-repeat/Identity-mismatch condition was 40, whereas the actual percentage of these trials was 75. Only two participants gave an estimate of the percentage of mismatch trials that was greater than 50.

Discussion

Response times were slower for both the Location-repeat/Identity-match and Location-repeat/Identity-mismatch conditions than for the Location-change condition. This result demonstrates that object-specific integration processes that produce feature mismatch effects (Hommel, 1998; Kahneman et al., 1992; Park & Kanwisher, 1994) do not impact performance in an obligatory manner. Rather, it appears that there is considerable contextual control over whether feature mismatches produce costs in performance relative to feature matches. We had observed a similar absence of feature integration effects in a prior study that contained a relatively high likelihood of both types of Location-repeat trials ($p = .42$; Milliken et al., 2010). The current experiment extends this observation a step further, illustrating that very high contingencies ($p = .75$) involving Location-repeat/Identity-mismatch trials often go unnoticed by participants, and the consequent strategy choice by participants is one that leads to faster performance for the relatively less frequent Location-change trials ($p = .17$).

The small number of participants that noticed the high likelihood of Location-repeat/Identity-mismatch trials in this task context is an interesting result, particularly so because there was just a 500 ms blank interval between the X in the prime display and the target O in the following probe display. We pursued this issue further in Experiment 2 by introducing a high contingency for Location-repeat/Identity-match trials rather than for Location-repeat/Identity-mismatch trials.

Experiment 2

Participants in Experiment 1 tended not to notice the relation between a prime X and probe O that appeared in the same location separated by just 500 ms. In Experiment 2, we examined whether participants would notice what we assumed would be a more salient relationship. In this experiment, the location of the probe target O matched that of the prime letter O on 75% of the trials.

Method

Participants

Twelve McMaster University undergraduate students from an introductory psychology class participated in exchange for course

Table 1
Mean Correct RTs (in Milliseconds) and High Probability Location Repetition Effects (LRE) for the Location-Change (LC), Location-Repeat/Match (M), and Location-Repeat/Mismatch (MM) Conditions, as a Function of Group (in Experiments 3 and 4)

Experiment	N	Group	LC	M	MM	LRE	% Repeated estimates
1	14		454	490	483	−29*	40.2
2	12		454	417	482	37*	67.3
3	13	Low (≤50)	423		445	−22*	37.3
	14	High (>50)	419		402	17*	74.6
4	10	Low (≤50)	432		460	−28*	28.0
	14	High (>50)	439		419	20	73.8
4	14	No Strategy	443		473	−30*	
	10	Strategy	426		385	41*	

Note. The last column lists mean estimates of percentage Location-repeat trials given by participants at the end of the experimental sessions. Participants were assigned to the low accuracy group if they gave an estimate that was less than or equal to 50%, whereas they were assigned to the high accuracy group if they gave an estimate that was greater than 50%. Note that the final two lines of data describe an alternative criterion for grouping participants in Experiment 4, On the basis of self-reported strategy use. Location repetition effects (LRE) that are statistically significant ($p < .05$) are indicated with an asterisk. The LRE for Experiments 1 and 2 is that for the condition that occurred on a high proportion of trials; That is, for the Location-repeat/Identity-mismatch condition in Experiment 1 and for the Location-repeat/Identity-match condition in Experiment 2.

Table 2
Percentage of Errors and High Probability Location Repetition Effects (LRE) for the Location-Change (LC), Location-Repeat/Match (M), and Location-Repeat/Mismatch (MM) Conditions, as a Function of Group (in Experiments 3 and 4)

Experiment	N	Group	LC	M	MM	LRE	% Repeated estimates
1	14		0.3	0.5	0.4	−0.1	40.2
2	12		2.1	0.6	1.6	1.5	67.3
3	13	Low (≤50)	0.5		0.4	0.1	37.3
	14	High (>50)	0.8		0.8	0.0	74.6
4	10	Low (≤50)	1.7		2.1	−0.4	28.0
	14	High (>50)	0.8		0.3	0.5	73.8
4	14	No Strategy	1.2		1.7	−0.5	
	10	Strategy	1.1		0.2	0.9	

Note. The last column lists mean estimates of percentage Location-Repeat trials given by participants at the end of the experimental sessions. Participants were assigned to the low accuracy group if they gave an estimate that was less than or equal to 50%, whereas they were assigned to the high accuracy group if they gave an estimate that was greater than 50%. Note that the final two lines of data describe an alternative criterion for grouping participants in Experiment 4, on the basis of self-reported strategy use.

credit. The mean age of participants was 19 years. All participants had normal or corrected-to-normal visual acuity.

Apparatus and Stimuli

These were the same as in Experiment 1.

Procedure and Design

The only difference between the procedure for this experiment and that for Experiment 1 was the relative proportions of the three repetition conditions. In this experiment, the proportion of Location-repeat/Identity-match trials was .75, the proportion of Location-repeat/Identity-mismatch trials was .08, and the proportion of Location-change trials was .17. In other words, on 75% of the trials the probe target O appeared in the same location as the O in the immediately preceding prime display.

Results

Response times for correct trials in each repetition condition (Location-change, Location-repeat/Identity-match, Location-repeat/Identity-mismatch were first submitted to the same outlier analysis as in Experiment 1 (Van Selst & Jolicoeur, 1994). Mean correct RTs were then computed using the remaining observations, and these mean RTs and corresponding error percentages were submitted to one-way repeated measures ANOVAs that treated repetition as the lone within-subject variable. Mean RTs in each condition, collapsed across participants, are displayed in Table 1, and corresponding error percentages are displayed in Table 2. All post-hoc tests were evaluated against an alpha criterion of .05, with a Bonferroni correction to protect against alpha slippage.

In the analysis of RTs, there was a significant main effect of repetition, $F(2, 22) = 53.04$, $p < .001$, $\eta_p^2 = .83$. Post-hoc t-tests indicated that response times in the three repetition conditions were all significantly different from each other, with fastest responses in the Location-repeat/Identity-match condition (417 ms), next fastest in the Location-change condition (454 ms), and slowest in the Location-repeat/Identity-mismatch condition (482 ms). In the corresponding analysis of error percentages, there were no significant effects. In this case, participants' estimates of the percentage of Location-repeat/Identity-match trials were quite accurate. In particular, the mean percentage estimate for the Location-repeat/Identity-match condition was 67, which is in close agreement with the actual percentage of match trials of 75. Eleven of twelve participants gave an estimate of the percentage of Location-repeat/Identity-match trials that was greater than 50.

Discussion

The results of Experiment 2 were very different from those of Experiment 1. In particular, whereas almost no participants appeared to notice the high proportion of Location-repeat/Identity-mismatch trials in Experiment 1, almost all participants noticed the high proportion of Location-repeat/Identity-match trials in Experiment 2. In addition, whereas repetition effects did not differ for Location-repeat/Identity-match and Location-repeat/Identity-mismatch conditions in Experiment 1, repetition effects were opposite in sign for these two conditions in Experiment 2. The different repetition effects for Location-repeat/Identity-match and Location-repeat/Identity-mismatch conditions suggests that the object integration processes discussed by Kahneman et al. (1992) did occur in this experiment, apparently aided by the knowledge that the prime episode provided predictive information about the probe. Together, the results of Experiments 1 and 2 demonstrate nicely the idea that such object integration effects are subject to contextual control (see also Milliken et al., 2010).

The results of Experiments 1 and 2 might also be argued to demonstrate qualitatively different patterns of repetition effects as a function of awareness of the contingency between prime and probe. In Experiment 1, participants generally appeared to be unaware of the high proportion of trials in which a prime item indicated the location of the following probe target. In contrast, in Experiment 2, participants generally appeared to be aware of this high proportion. The repetition effects that occurred for these two experiments might be argued to differ qualitatively in either of two different ways. First, as proposed above, object integration effects were observed for the aware participants of Experiment 2 but not for the unaware participants of Experiment 1. And second, the

tapraid5/zfn-xhp/zfn-xhp/zfn00510/zfn2537d10z | xppws | S=1 | 8/4/10 | 6:21 | Art: 2008-1299

repetition effect for the high proportion Location-repeat condition (i.e., the Identity-match condition in Experiment 2 and the Identity-mismatch condition in Experiment 1) was positive for the aware participants of Experiment 2 but negative for the unaware participants of Experiment 1.

Yet, each of these qualitative differences might be argued to provide less than optimal evidence that different states of awareness produced qualitatively different patterns of performance. With respect to the presence versus absence of object integration effects, it might be argued that presence versus absence of an effect is a weak form of qualitative difference, and that qualitatively opposite effects are the gold standard qualitative difference effect. And with respect to the opposite effects for the high proportion Location-repeat condition across experiments, it can be argued that the qualitative difference in performance was due to an interaction between awareness and object integration, rather than to awareness on its own. In other words, the negative repetition effect for the Location-repeat/Identity-mismatch condition in Experiment 1 may have been driven as much by the identity mismatch between the X and O that appeared at the same location as by the fact that participants were unaware that such trials occurred frequently. These potential shortcomings, as well as the processing basis of differences in awareness across Experiments 1 and 2, were addressed in Experiments 3 and 4.

Experiment 3

The estimates of percentage repeated in Experiments 1 and 2 differed vastly, and one of the goals of this experiment was to examine the cause of this difference. Two hypotheses in particular are considered here. The first hypothesis we call the fluent object integration hypothesis. By this view, participants discovered the high proportion of Location-repeat/Identity-match trials in Experiment 2 as a direct consequence of frequently encountering that trial type, which according to Kahneman et al. (1992) can be responded to by integrating the current probe O with an episodic representation of the immediately preceding prime O. Assuming that the match in symbols affords fluent integration and that participants might be cued by the fluent integration to discover its cause, one might expect participants to discover that Location-repeat/Identity-match trials occurred with high frequency in Experiment 2 but not that Location-repeat/Identity-mismatch trials occurred with high frequency in Experiment 1. The second hypothesis we call the set-induced attention allocation hypothesis. By this view, participants discovered the high proportion of Location-repeat/Identity-match trials in Experiment 2 because their attention was allocated to the prime display in a way that afforded the discovery of a contingency between the prime and probe displays. In particular, the idea is that a preparatory task set to localize a probe target O may be operative during presentation of the prime display (Folk, Remington, & Johnston, 1992). Such a task set might lead participants to attend to the location of the prime O rather than to the location of the prime X. The processing consequences of this set-induced allocation of attention would be to divert attention away from the location at which the probe target often occurs in Experiment 1 (i.e., the location of the X) but towards the location at which the probe target often occurs in Experiment 2 (i.e., the location of the O). If detection of a contingent relation depends on allocation of attention to that relation, then set-induced allocation of attention to the prime could explain the different levels of awareness of the contingency between prime and probe in Experiments 1 and 2.

The set-induced attention allocation hypothesis seems particularly plausible given recent findings showing that inattentional blindness effects are sensitive to the similarity between the target item to which people are "blind" and the items to which participants are asked to attend (Most et al., 2001; Simons & Chabris, 1999). For example, participants are more likely not to notice a black gorilla amidst a scene when asked to keep track of a basketball being passed among people wearing white shirts than when asked to keep track of a basketball being passed among people wearing black shirts. In effect, dissimilarity between visual properties inherent to one's task set and visual properties of items in a scene reduce the likelihood of noticing those items.

In summary, the fluent object integration hypothesis assumes that it is the experience of frequent Location-repeat/Identity-match trials that cues the participant to look for a relation between the prime and the probe. In contrast, the set-induced attention allocation hypothesis assumes that it is the allocation of attention to the location of the prime O that leads to detection of the high proportion of Location-repeat/Identity-match trials. The procedure used in Experiment 3 constituted a step toward distinguishing between these two hypotheses. In particular, Experiment 3 used a similar procedure to that of Experiments 1 and 2, but rather than presenting both an O and an X in the prime display, only an X was presented in the prime display. As in Experiment 1, the probe target O appeared in the location of the prime X on 75% of the trials. According to the fluent object integration hypothesis, this procedure should result in approximately similar levels of awareness of prime-probe contingencies as in Experiment 1. However, according to the set-induced attention allocation hypothesis, attention might not be pulled away from the prime X as reliably in this experiment as in Experiment 1, given that a prime O that matches the goal state for the following probe is not present in the prime display. In this case, attentional processes ought to be more free to be captured by the lone prime X, leading to a greater number of participants detecting the high proportion of Location-repeat/Identity-mismatch trials than in Experiment 1.

Method

Participants

Twenty-seven McMaster University undergraduate students from an introductory psychology class participated in exchange for course credit. The mean age of participants was 19 years. All participants had normal or corrected-to-normal visual acuity.

Apparatus and Stimuli

These were the same as in Experiment 1 and 2.

Procedure and Design

This procedure was similar to Experiment 1 with the only difference being that an O did not appear in the prime display. The resulting two trial types used in this experiment are depicted in Figure 2. The prime display contained only an X in one of the four F2

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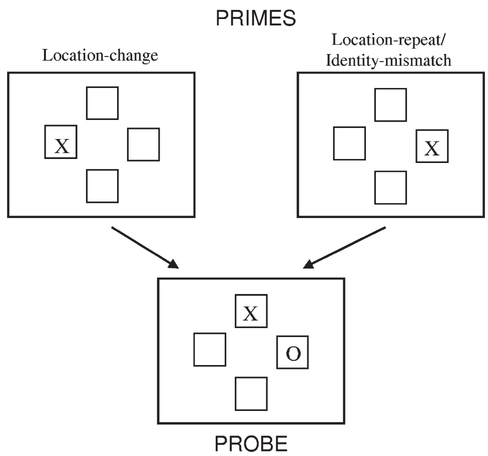

Figure 2. Examples of the two location repetition conditions used in Experiment 3. In the Location-change condition, note that the probe target O appears in a location that was unoccupied in the preceding prime. In the Location-repeat/Identity-mismatch condition, the probe target O appears in the location that was occupied by an X in the preceding prime. In both conditions, the probe distractor X appears in a location that was unoccupied in the preceding prime.

marked locations, and on 75% of the trials the probe target O appeared in the same location as the prime X (Location-repeat/Identity-mismatch trials). On the remaining 25% of trials, the probe target O appeared in a location that was unoccupied in the prime display (Location-change trials). On both types of trials, the probe distractor appeared in a location that was unoccupied in the preceding prime display. After completion of the localization task, participants were required to estimate the percentage of trials in which the location of the X in the prime display was the same as that the location of the O in the probe display.

Results

Outliers were rejected from the analysis of RTs as in prior experiments (Van Selst & Jolicoeur, 1994). A preliminary analysis of variance of these mean RTs that corresponded to that of Experiments 1 and 2 revealed no significant effect of repetition, $F <$ 1. However, estimates of the percentage of repeated trials varied considerably in this experiment, with some participants being quite accurate and others inaccurate. Overall, the mean percentage estimate for repeated trials was 57, which lies between the mean estimates of percentage repeated (percentage estimates for Location-repeat/Identity-mismatch in Experiment 1 and percentage estimates for Location-repeat/Identity-match in Experiment 2) in the previous two experiments. To examine repetition effects as a function of awareness of the high percentage of repeated trials, we categorized participants as high estimators of the percentage of repeated trials if their estimate was greater than 50 and as low estimators if their estimate of the percentage of repeated trials was 50 or less. This procedure produced a high estimate group with 14 participants and a low estimate group with 13 participants. The mean percentage repeated estimate for the high estimate group was 75 whereas the mean percentage repeated estimate for low estimate group was 37.

Mean response times and error percentages for each condition defined by the combination of the repetition (Location-change, Location-repeat/Identity-mismatch) and estimate group (high/low) factors were submitted to mixed factor ANOVAs that treated repetition as a within-subject variable and estimate group as a between-subjects variable. All post-hoc tests were evaluated against an alpha criterion of .05, with a Bonferroni correction to protect against alpha slippage. Mean RTs for Location-change and Location-repeat/Identity-mismatch trials, collapsed across participants within groups, are displayed in Table 1, and corresponding error percentages are displayed in Table 2.

In the analysis of RTs, there was a significant interaction between repetition and estimate group, $F(1, 25) = 25.29$, $p < .001$, $\eta_p^2 = .467$. To examine this interaction further, separate ANOVAs were conducted for the high and low estimate groups. In the analysis of the high estimate group, there was a significant effect of repetition, $F(1, 13) = 5.62$, $p < .05$, $\eta_p^2 = .31$, with faster responses for Location-repeat/Identity-mismatch trials (402 ms) than for Location-change trials (419 ms). In the analysis of the low estimate group, there was also a significant effect of repetition, $F(1, 12) = 84.55$, $p < .001$, $\eta_p^2 = .78$, but in this case responses were slower for Location-repeat/Identity-mismatch trials (445 ms) than for Location-change trials (423 ms). The analysis of error percentages revealed no significant effects.

A final analysis was conducted to examine the correlation between repetition effects in RT and inaccuracy in estimates of the percentage of repeated trials. Inaccuracy in estimate of the percentage of repeated trials was computed simply as the absolute value of the difference between the correct percentage repeated of 75 and the estimate of percentage repeated given by each participant. This correlation was statistically significant ($r = .62$), indicating that small errors in the estimate of the percentage of repeated trials were associated with increasingly fast responses for Location-repeat/Identity-mismatch trials relative to Location-change trials.

Discussion

One of the key results of this experiment is that approximately half of the participants gave relatively accurate estimates (greater than 50) of the percentage of Location-repeat/Identity-mismatch trials, whereas only two of fourteen participants did so in Experiment 1. This result is not consistent with the fluent object integration hypothesis but is consistent with the set-induced attention allocation hypothesis.

According to the set-induced attention allocation hypothesis, participants in Experiment 1 were prepared to locate a target O in the probe display, which in turn led their attention to be captured by the O in the preceding prime display. This capture of attention at the location of the prime O may have prevented discovery of the high percentage of trials in which the probe target O appeared in the same location as the prime X. In effect, participants in Experiment 1 may have been subject to a form of inattentional blindness to the high likelihood of Location-repeat/Identity-mismatch trials (Most et al., 2001; Simons & Chabris, 1999). In contrast, in Experiment 3 participants' attention was less likely to have been diverted away from the prime X because there was no accompanying prime O; the X was the only item in the prime display. As a consequence, at least some participants may have attended the

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prime X on some reasonable proportion of the trials, which in turn led them to discover the high percentage of trials in which the probe target O appeared at the location of the prime X. Discovery of this high percentage of Location-repeat/Identity-mismatch trials would often then lead to strategic use of the location of the prime X to predict the likely location of the probe target O, to relatively fast responses in the Location-repeat/Identity-mismatch condition, as well as to relatively accurate estimates of the percentage of Location-repeat/Identity-mismatch trials following completion of the experiment.

Although several qualitative difference patterns did emerge across Experiments 1 and 2, each of them had a shortcoming in terms of relating the qualitative difference directly to a difference in awareness of the prime-probe contingency. The qualitative difference result observed in this experiment has no such shortcoming. In particular, participants who accurately reported the high percentage of Location-repeat/Identity-mismatch trials produced faster responses for those trials than for Location-change trials. In contrast, participants who were inaccurate in reporting the high percentage of Location-repeat/Identity-mismatch trials responded more slowly to those trials than to Location-change trials. This pattern of results strongly supports the idea that qualitatively different processing occurs when participants are aware of the contingency between prime and probe than when they are unaware of this contingency.

At the same time, we suspect that different states of awareness with respect to the contingency do not directly cause qualitatively opposite repetition effects. Rather, it seems more likely that states of awareness of the prime-probe contingency play a key role in determining the task strategy adopted by participants, which in turns plays a direct role in determining the repetition effect. This hypothesis is supported by inspection of the variability of repetition effects in Experiment 3. In particular, the standard deviation of the repetition effects across participants was considerably higher for the high estimate group than for the low estimate group (27 ms vs. 9 ms). Whereas all participants in the low estimate group produced repetition effects that were less than zero (faster responses to Location-change than to Location-repeat/Identity-mismatch trials), only 11 of 14 participants in the high estimate group produced repetition effects that were greater than zero (faster responses to Location-repeat/Identity-mismatch than to Location-change trials). This variability among participants in the high estimate group suggests that perhaps not all of the participants in that group were using the same strategy. In particular, it seems possible that some of the participants were able to correctly estimate the high percentage of Location-repeat/Identity-mismatch trials, and yet they may not have engaged in the strategy of using the location of the prime X to predict the most likely location of the probe target O. Rather, they may have engaged in the same strategy as participants in the low estimate group despite being fully aware of the high percentage of Location-repeat/Identity-mismatch trials.

To address this issue, in the following experiment, we also asked participants whether they used the first display to predict the location of the target in the second display. If the qualitative difference obtained in the behavioral data of Experiment 3 is best explained by differences in strategy use, then separating participants in accord with their reported strategy use in Experiment 4 may produce an even clearer demonstration than separating participants in accord with their accuracy in estimation of the percentage of Location-repeat/Identity-mismatch trials.

Experiment 4

In addition to examining self-reported strategy use in this experiment, a secondary aim was to demonstrate that the qualitative difference pattern reported in Experiment 3 generalizes to other similar procedures. To this end, the key dimension that defined the probe target in this experiment was color rather than identity. On each probe trial, a red X and a green X appeared, and participants were instructed to locate the identity of the red X. Preceding this probe display was a prime display that contained a single green X that was not to be responded to. In this sense, the present experiment was directly analogous to Experiment 3 but using color-defined rather than identity-defined targets.

Method

Participants

Twenty-four McMaster University undergraduate students from an introductory psychology class participated in exchange for course credit. The mean age of participants was 19 years. All participants had normal or corrected-to-normal visual acuity.

Procedure and Design

As in Experiment 3, a single prime letter was followed by two probe letters, and participants were to respond only to one of the two probes. In this case, a green X appeared in the prime display whereas a red X and a green X appeared in the probe display. The target was defined as the red X on all trials, and on 75% of the trials the red X appeared in precisely the same location as the green X in the preceding prime display. These trials were called Location-repeat/Color-mismatch trials to capture the mismatch in color of the two X's appearing in the same location. As in Experiments 1 and 3, participants were required to estimate the percentages of Location-repeat/Color-mismatch and Location-change trials, but additionally they were asked whether they used the first display to predict the location of the red X in the second display. All other aspects of the method of this experiment were the same as Experiment 3.

Results

Our initial analysis of the results corresponded to that reported in Experiment 3, with participants separated into two groups based on the accuracy of their estimates of percentage Location-repeat/Color-mismatch trials. Assignment of participants to groups followed the same procedure as in Experiment 3, with participants who offered estimates greater than 50 being assigned to the high estimate group, and participants who offered estimates of 50 or less being assigned to the low estimate group. This procedure produced a high estimate group with 14 participants and a low estimate group with 10 participants. Collapsed across groups, the mean percentage estimate was 55, which is in close agreement with the corresponding estimate in Experiment 3. For the high estimate group, the mean percentage repeated estimate was 74, whereas for the low estimate group the mean percentage repeated estimate was 28.

Response times for correct trials in each condition defined by the combination of the repetition (Location-change, Location-repeat/Color-mismatch) and estimate group (high, low) factors were submitted to the same outlier analysis as in the previous experiments (Van Selst & Jolicoeur, 1994). Mean correct RTs were then computed using the remaining observations, and these mean RTs and corresponding error percentages were submitted to mixed factor ANOVAs that treated repetition as a within-subject variable and estimate group as a between-subjects variable. All post-hoc tests were evaluated against an alpha criterion of .05, with a Bonferroni correction to protect against alpha slippage. Mean RTs for Location-change and Location-repeat/Color-mismatch trials in both groups are displayed in Table 1, and corresponding error percentages are displayed in Table 2.

In the analysis of RTs, there was a significant interaction between repetition and estimate group, $F(1, 25) = 9.15$, $p < .01$, $\eta_p^2 = .29$. To examine this interaction further, separate ANOVAs were conducted for the two groups. In the analysis of the high estimate group, although the direction of the effect was consistent with that observed in Experiment 3, the effect of repetition did not reach significance, $F(1, 13) = 2.38$, $p > .10$, $\eta_p^2 = .15$ (419 ms for Location-repeat/Color-mismatch trials versus 439 ms for Location-change trials). However, in the analysis of the low estimate group, there was again a significant effect of repetition, $F(1, 12) = 30.81$, $p < .001$, $\eta_p^2 = .77$, with slower responses for Location-repeat/Color-mismatch (460 ms) than for Location-change trials (432 ms). The analysis of error percentages revealed no significant effects.

To this point, the results of this experiment fail to replicate the key qualitative difference observed in Experiment 3, in particular because the repetition effect for the high estimate group, although positive in sign, failed to reach significance. However, we again noted that there was much more variability in the repetition effects within the high estimate group than within the low estimate group. The standard deviation of the repetition effects for the high estimate group was 48 ms while that for the low estimate group was 16 ms. This result is consistent with the idea that strategy use may have been homogenous across participants in the low estimate group but not in the high estimate group.

To address this issue, we reassigned participants to groups based on their declared strategy use, rather than on their estimates of the percentage of Location-repeat/Color-mismatch trials. The critical question on the post-test questionnaire asked “Did you use the first display to predict the location of the target in the second display (Y/N)?” Participants who responded “yes” to this question were placed in the strategy group, while participants who responded “no” to this question were placed in the no-strategy group. This procedure led to a relatively straightforward change in group membership, with all but four participants being assigned to the groups as might be expected. In particular, all 10 participants who were assigned to the low estimate group were assigned to the no-strategy group, and 10 of the 14 participants who were assigned to the high estimate group were assigned to the strategy group. However, the four remaining participants from the high estimate group reported not using a predictive strategy despite producing relatively accurate estimates of the high percentage of Location-repeat/Color-mismatch trials and were, therefore, assigned to the no-strategy group. In summary, 10 participants were assigned to the strategy group and 14 participants were assigned to the no-strategy group.

Mean RTs and error percentages in each condition defined by the combination of the repetition (Location-change, Location-repeat/Color-mismatch) and strategy (strategy, no-strategy) factors were then submitted to mixed factor ANOVAs that treated repetition as a within-subject variable and strategy as a between-subjects variable. Mean RTs for Location-change and Location-repeat/Color-mismatch trials in both groups are displayed in Table 1, and error percentages are displayed in Table 2.

In the analysis of RTs, there was a significant main effect of strategy, $F(1, 22) = 4.57$, $p < .05$, $\eta_p^2 = .17$, with faster responses for the strategy group (405 ms) than for the no-strategy group (458 ms). More important, there was a significant interaction between repetition and strategy, $F(1, 22) = 38.08$, $p < .001$, $\eta_p^2 = .63$,. To examine this interaction further, separate ANOVAs were conducted on the data from the two groups. In the analysis of the strategy group, there was a significant effect of repetition, $F(1, 9) = 11.95$, $p < .01$, $\eta_p^2 = .57$, with faster responses for the Location-repeat/Color-mismatch condition (385 ms) than for the control condition (426 ms). In the analysis of the no-strategy group, there was also a significant effect of repetition, $F(1, 13) = 38.46$, $p < .001$, $\eta_p^2 = .75$, but in this case responses were slower for Location-repeat/Color-mismatch trials (473 ms) than for control trials (443 ms).

In the analysis of error percentages, again there was a significant interaction between repetition and strategy group, $F(1, 22) = 5.03$, $p < .05$, $\eta_p^2 = .18$. Separate ANOVAs for the two strategy groups revealed that participants in the strategy group showed a trend toward making more errors for Location-change trials (1.1%) than for Location-repeat/Color-mismatch trials (0.2%), $p > .10$, whereas the reverse trend was observed for the no-strategy group (1.7% for Location-repeat/Color-mismatch trials versus 1.2% for Location-change trials, $p > .10$).

Discussion

The results from this experiment revealed a similar qualitative difference to that reported in Experiment 3, although in this case groups defined by self-reported strategy use rather than accuracy in estimate of the percentage of Location-repeat/Color-mismatch trials produced the clearest contrast. When participants were grouped according to accuracy in estimate of the percentage of Location-repeat/Color-mismatch trials, the qualitative difference finding was not ideal; although the low estimate group produced a significant negative priming effect, the high estimate group failed to produce a significant positive priming effect. In contrast, when participants were grouped according to strategy use, the qualitative difference finding was a strong one; the no-strategy group produced a significant negative priming effect and the strategy group produced a significant positive priming effect. The different criteria for establishing group membership actually affected group placement for just four participants, who all gave high estimates (greater than 50) of the percentage of Location-repeat/Color-mismatch trials but reported not having used the strategy of locating the prime and predicting the probe target to appear in that same location. All four of these participants responded more quickly on Location-change trials than on Location-repeat/Color-mismatch

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trials and, as such, placing these participants in the no-strategy group strengthened the contrast between groups substantially. On the whole, the correspondence between group membership based on self-reported strategy use and the repetition effect was remarkably close. All ten participants in the strategy group responded more quickly on Location-repeat/Color-mismatch trials than on Location-change trials, and 13 of 14 participants in the no-strategy group responded more quickly on Location-change trials than on Location-repeat/Color-mismatch trials.

Experiment 5

It is certainly curious that positive priming effects for Location-repeat/mismatch conditions occurred in Experiments 3 and 4 only when participants were aware of the high likelihood of those trials and when they adjusted their strategy accordingly. One might reasonably expect that implicit learning of the high probability of a Location-repeat trial would result in fast responses to those trials even in the absence of explicit awareness and strategy use. Indeed, increased frequency of particular cue-target combinations have resulted in implicit learning effects in several prior studies of spatial orienting.

In a seminal study on implicit learning and spatial orienting, Lambert and Sumich (1996) presented animate or inanimate words as peripheral cues. Targets appeared with high probability on the same side of space as animate words and with high probability on the opposite side of space as inanimate words, or vice versa. Despite not being aware of this contingent relation between cues and targets, responses to detect target onsets were faster when they occurred at the more probable location, suggesting that participants learned implicitly the contingency between semantic category of the cue and target location (see also Lambert, Naikar, McLachlan, & Aitken, 1999; Lambert, Norris, Naikar, & Aitken, 2000 for related demonstrations of implicit learning of cue-target contingencies).

More recently, Bartolomeo, Decaix, and Siéroff (2007) manipulated the relative proportions of cued and uncued trials in the test phase of an exogenous spatial cueing task that followed a short initial phase in which cueing effects were measured with equal proportions of cued and uncued trials. In addition, participants were asked after the experimental session if they had noted that the proportion of cued trials was different than chance. Some participants did notice the contingency that was introduced, and others did not, thus allowing the researchers to measure cueing effects for aware and unaware groups, which they called “verbalizers” and “non-verbalizers.”It is interesting to note that the patterns of performance were indistinguishable for these two groups, and both groups were sensitive to changes in the proportion of cued trials. When the proportion of cued trials was increased above chance in the test phase, the inhibition of return effect observed in the initial phase was eliminated; and when the proportion of cued trials was reduced below chance in the test phase, the inhibition of return effect observed in the initial phase was magnified. The fact that both verbalizers and non-verbalizers produced this pattern of results suggests that the learning responsible for these effects was implicit in nature, rather than related to explicit awareness and strategy use.

Given prior studies in which implicit learning of cue-target contingencies has been observed, one might reasonably ask why a similar implicit learning effect was not evident in our experiments. However, this question overlooks a key property of the experimental designs used so far. In particular, there was no direct manipulation of the probability of Location-repeat/mismatch trials relative to Location-change trials in the prior experiments and, therefore, no way to establish with certainty whether an implicit learning effect was present or not. All that is certain is that performance was curiously slower for the higher probability condition than for the lower probability condition for a subset of the participants, but not whether that effect would be sensitive to a manipulation of the relative frequencies of these conditions.

To address this issue, we adopted a procedure similar to that used by Bartolomeo et al. (2007). In a first block of trials, the probability of a Location-repeat/Identity-mismatch trial was no greater than chance. Prior research with this procedure suggests that we ought to observe slower responses for the Location-repeat/Identity-mismatch condition than for the Location-change condition (Milliken et al., 2010; Milliken, Tipper, Houghton, & Lupiáñez, 2000). In a following block of trials that was three times the length of the initial block, the probability of Location-repeat/Identity-mismatch trials changed from .25 to .75. The key empirical question was whether the performance difference between Location-repeat/Identity-mismatch trials and Location-change trials would be sensitive to the probability change across blocks for participants who were unaware or not strategic in their use of the high frequency of Location-repeat/Identity-mismatch trials.

Method

Participants

Sixteen McMaster University undergraduate students were paid $10 for their participation. The mean age of participants was 21 years. All participants had normal or corrected-to-normal visual acuity.

Procedure and Design

As in Experiment 3, a single prime letter X was followed by a probe that contained an X in one location and an O in another location. Participants were to respond only to the O in the probe display. In Block 1, which had 48 trials, the probe O appeared in the same location as the prime X on 25% of the trials (Location-repeat/Identity-mismatch), and in one of the three unoccupied prime locations on 75% of the trials (Location-change). In Block 2, which had 144 trials, 75% of the trials belonged to the Location-repeat/Identity-mismatch condition while 25% of the trials belonged to the Location-change condition. As in prior experiments, participants were asked to estimate the percentages of Location-repeat/Identity-mismatch and Location-change trials and to indicate whether they used the prime display to predict the location of the target O in the probe display. All other aspects of the method of this experiment were the same as Experiment 3.

Results

Our analysis of the results corresponded to that in Experiment 4, with participants first separated into two groups based on their reported strategy use. Four participants reported using the strategy of predicting the location of the probe target on the basis of the location of the prime, while 12 participants reported not using this strategy. As in Experiment 4, this allocation to groups corresponded closely to participants' estimates of the percentage of Location-repeat/Identity-mismatch trials, with three of the four strategic participants giving estimates greater than 50%, and 11 of the 12 non-strategic participants giving estimates of 50% or less. The mean estimate of percentage Location-repeat/Identity-mismatch trials for the strategy group was 59%, while the corresponding mean estimate for the non-strategy group was 35%. Correct RTs for each condition defined by factorially combining the repetition (Location-repeat/Identity-mismatch, Location-change) and block (1/2) variables separately for each group (strategy, no-strategy) were submitted to the same outlier analysis as in prior experiments, and mean RTs for each condition were computed from the remaining observations. These mean RTs and corresponding error rates are presented in Tables 3 and 4.

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The key issue in this experiment concerns performance for the no-strategy participants, and in particular whether learning of the contingency introduced after the first block of 48 trials would be expressed in their performance. Mean RTs and error rates for the no-strategy group were submitted to a repeated measures analysis of variance that treated repetition and block as within-subject factors.

In the analysis of RTs, there was a significant main effect of block, $F(1, 11) = 13.61$, $p < .01$, $\eta_p^2 = .55$, with faster responses in block 2 (459 ms) than in block 1 (492 ms). There was also a significant main effect of repetition, $F(1, 11) = 36.62$, $p < .001$, $\eta_p^2 = .77$, with faster responses in the Location-change condition (458 ms) than in the Location-repeat/Identity-mismatch condition (492 ms). Of most importance, there was a significant interaction between block and repetition, $F(1, 11) = 8.99$, $p < .02$, $\eta_p^2 = .45$. Subsequent analyses of the two blocks separately revealed faster responses for the Location-change condition than for the Location-repeat/Identity-mismatch condition both for block 1 (469 ms vs 515 ms) and for block 2 (448 ms vs 470 ms). Thus, although the effect of repetition did not change sign across the two blocks, the effect was significantly smaller in block 2 than in block 1, a pattern that was produced by 10 of the 12 participants in this group.

There were no significant effects in the analysis of error rates for the no-strategy group. In addition, there were too few participants in the strategy group to conduct meaningful analyses of either RTs or error rates. However, the pattern of means presented in Table 3 is consistent with the idea that participants who became aware of the high proportion of Location-repeat/Identity-mismatch trials in block 2 and who declared having used a predictive strategy produced qualitatively opposite repetition effects in blocks 1 and 2.

Discussion

The purpose of this experiment was to address the puzzling finding that, for participants who gave low proportion estimates in Experiment 3 or who claimed not to use predictive strategies in Experiment 4, performance for frequent Location-repeat/Identity-mismatch trials was slower than for infrequent Location-change trials. One might reasonably have expected implicit learning processes to lead to the opposite result. This issue was addressed directly in this experiment by varying the frequencies of the two trial types across blocks. The results revealed a clear shift in the repetition effect as a function of block, suggesting that participants who were not engaging in predictive strategies were nonetheless sensitive to the different frequencies of trial types in the two blocks. In particular, although responses were significantly slower in the Location-repeat/Identity-mismatch condition than in the Location-change condition in both blocks, this difference was smaller in block 2. Thus, the repetition effects reported in Experiments 3 and 4 appear to reflect the joint contributions of an implicit learning process that speeds performance for Location-repeat/Identity-change trials and another process that slows performance for those trials. The puzzling findings of these two experiments then do not imply that implicit learning is absent but instead imply that implicit learning is present but insufficient to override other influences that produce the opposite effect on performance.

A potential concern with the conclusion offered above is that mere practice with the experimental task, rather than sensitivity to the different frequencies of trial types in blocks 1 and 2, could have produced the pattern of results observed here. To address this issue, an additional control experiment with eight participants was conducted. The procedure was identical to Experiment 5 with the exception that the relative frequencies of the two critical trial types did not change across blocks; that is, the proportion of Location-repeat/Identity-mismatch trials was .25 across the entire experimental session. In this experiment, the difference in mean RT between the Location-change and Location-repeat/Identity-mismatch conditions was nearly identical across blocks 1 (−47 ms) and block 2 (−46 ms), with four participants producing a numerically larger effect in block 1 and four participants producing a numerically larger effect in block 2. This result confirms that the change in repetition effect observed across blocks 1 and 2 in

Table 3
Mean Response Times for the Location-Change (LC) and Location-Repeat/Identity-Mismatch (MM) Conditions, as a Function of Group, in Experiment 5

Group	N	Block	LC	MM	LRE	% Repeated estimates
No strategy	12	1	469	515	−46	35
		2	448	470	−22	
Strategy	4	1	392	427	−35	59
		2	412	385	27	

Note. Location repetition effects (LRE) are computed as the response time difference between LC and MM conditions.

Table 4
Mean Error Percentages for the Location-Change (LC) and Location-Repeat/Identity-Mismatch (MM) Conditions, as a Function of Group, in Experiment 5

Group	N	Block	LC	MM	LRE	% Repeated estimates
Low (≤50)	12	1	1.4	0.0	1.4	37
		2	0.5	1.0	−0.5	
High (>50)	4	1	4.2	10.4	−6.2	59
		2	4.2	0.9	3.3	

Note. Location repetition effects (LRE) are computed as error percentage difference between LC and MM conditions.

Experiment 5 is best interpreted as evidence of a frequency-sensitive implicit learning process.

General Discussion

The qualitative difference method has been used successfully in a host of prior studies to distinguish between conscious and unconscious processes (e.g., Cheesman & Merikle, 1986; Daza et al., 2002; Eimer & Schlaghecken, 2002; McCormick, 1997; Merikle & Joordens, 1997). According to this approach, qualitatively different effects on performance can provide converging evidence for the meaningfulness of the distinction between aware and unaware subjective states of experience. The present study adds to this literature in that we report qualitatively different effects on localization performance as a function of participants' subjective reports of awareness of a contingency between two events (Experiment 3) and as a function of participants' subjective reports of strategy use (Experiment 4). As in prior qualitative difference studies, different states of awareness led to qualitatively different patterns of performance because of the tight relation between awareness of the task context, on one hand, and strategic processes that directly mediate performance, on the other hand. At the same time, there are two salient properties of our experiments that set them apart from prior qualitative difference studies.

First, in most prior studies that have adopted the qualitative difference approach, the unaware (or no-strategy) condition was associated with automatic processing of a prime that produced facilitation for related probes, while the aware (or strategy) condition was associated with strategies that produced interference for related probes (Cheesman & Merikle, 1986; Daza et al., 2002; AQ: 5 McCormick, 1997; Merikle & Cheesman, 1987; Merikle & Joordens, 1997).

In contrast, in the present study, an interference effect for related probes was observed for the unaware/no-strategy condition and a facilitation effect for related probes was observed for the aware/strategy condition. To our knowledge, only two other formal studies have noted the dependence of an "inhibition" effect on unawareness in this manner. One of these studies was that of Eimer and Schlaghecken (2002), in which negative compatibility effects were observed in directional priming when participants were unaware of primes, while positive compatibility effects were observed when participants were aware of primes. The second study is one reported long ago by Tipper (1985), in which he reported that a certain subclass of "aware" participants often produced positive priming effects for ignored primes in contexts in which "unaware" participants produced negative priming for ignored primes. The aware participants were those who reported having noticed that the ignored prime in a picture identification task often matched the following attended probe, and although the small number of aware participants precluded formal analysis in most cases, these results constitute at least anecdotal evidence of the type of qualitative difference finding of interest here. Although it is unclear whether the processes underlying our results are similar to those in these two prior studies, they do share the property that they capitalize on readily observable and well-documented interference/inhibition effects.

A second property of our experiments that sets them apart from other qualitative difference studies concerns our measure of awareness and strategy use. In most prior qualitative difference studies, researchers have manipulated the visibility of primes to produce groups that differ in their awareness of the utility of a particular strategy. In contrast, we presented clearly visible primes and allowed participants to select a strategy on their own. In turn, we made inferences about awareness and strategy based on accuracy of report of percentage repeated and on self-reported descriptions of strategy use. Despite concerns one might have about the validity of self-reported strategies, in our experimental context, we observed a striking correspondence between self-report and behavior, in particular when strategies were asked about directly in Experiment 4.

The fact that we made use of self-report rather than visual masking to define awareness and strategy use brings into play questions concerning processes that give rise to awareness of clearly visible stimuli, such as those addressed in studies of inattentional blindness (Mack & Rock, 1998; Most et al., 2001; Simons & Chabris, 1999). Indeed, the results of Experiment 2 suggest that attentional set likely played an important role in producing the profoundly inaccurate estimates of percentage of Location-repeat/Identity-mismatch trials in Experiment 1. In that experiment, only two of fourteen participants gave estimates of the percentage of Location-repeat/Identity-mismatch trials greater than 50 when, in fact, the actual percentage of such trials was 75. The results of Experiment 2 suggest that participants' set to localize target O stimuli in the probe display may have deflected attention toward the prime O and away from the prime X, which in turn led to poor awareness that the probe target O most often appeared in the location of the prime X. If this account of the inaccurate estimates in Experiment 1 is correct, then it seems reasonable to describe this result as an extension of the inattentional blindness phenomenon. In particular, it seems that inattentional blindness to the redundant spatial location of the prime X and probe O on a trial-to-trial basis can have the long-term consequence that participants fail to learn explicitly a strong contingent relation between consecutive events separated by a mere 500 ms. Other consequences of inattentional blindness for longer-term learning may well be a ripe area for additional research.

Implicit Learning Effects

A key result reported in this study was relatively slow performance for the Location-repeat/mismatch conditions for those who underestimated the percentage of those trials (Experiment 3) or claimed not to be using a predictive strategy (Experiment 4). This result is broadly consistent with the work of McCormick (1997) in which shifts of attention to high probability cue-contingent targets depended on awareness of the cue. Yet, from another perspective this result is somewhat surprising, as a host of prior studies have revealed implicit learning effects in spatial orienting that would be expected to produce faster responses for the relatively more frequent trial types in our study (Bartolomeo et al., 2007; Lambert et al., 1999; Lambert et al., 2000; Lambert & Sumich, 1996; see also Kentridge, Heywood, & Weiskrantz, 1999, for a demonstration of frequency-sensitive allocation of attention to targets in the blind field of a patient with blindsight). One might even be tempted to conclude that the slower responses for the frequent Location-repeat/mismatch trials than for the infrequent Location-change trials imply that implicit learning effects were absent in our experiments. Experiment 5 addressed this issue directly varying the relative frequencies of Location-repeat/Identity-mismatch and Location-change trials across blocks. In this experiment, performance was sensitive to the frequency of our two critical trial types, with the relative slowing for Location-repeat/Identity-mismatch trials being less pronounced when those trials were frequent than when they were infrequent. This pattern of results is consistent with the idea that the overall slow performance for Location-repeat/mismatch trials reflects the joint contribution of two processes that produce opposite influences on performance. In line with the idea that implicit learning often introduces relatively subtle influences on behavior (Cleeremans & Jiménez, 2002), a weak implicit learning influence appears to have been insufficient to counter a second influence that slowed performance for Location-repeat/mismatch trials relative to Location-change trials.

Finally, the procedure used in Experiment 5 was modeled after that used by Bartolomeo et al. (2007), in the sense that a first block of trials with a chance relation between prime and probe was followed by a second block of trials with a strong probabilistic contingency between prime and probe. Although evidence for implicit learning was present in both studies, the results from these two studies differed in an important way. Whereas Bartolomeo et al. (2007) observed similar patterns of performance for those who were aware and those who were unaware of the contingency, we observed opposite patterns of performance for aware/strategy and unaware/no-strategy groups. We suspect that two factors may be critical to the different findings across these two studies. First, it appears that group membership was established by Bartolomeo et al. (2007) on the basis of awareness of a contingency between cue and target but not on the basis of whether participants used that contingency in some strategic way to produce fast responses for the most frequent trial type. As noted in our Experiments 3 and 4, some portion of participants can be expected to notice a contingency between cue (or prime) and target (or probe) but not use that contingency strategically. It may be that a relatively small portion of the aware participants in the Bartolomeo et al. (2007) study also engaged in strategies favoring the frequent trial type, thus producing similar patterns of behavior for the aware and unaware groups. Second, the relative benefit of engaging in strategic behavior, and perhaps also the likelihood of engaging in strategic behavior, may vary as a function of task. From this perspective, it follows that performance in the detection task used by Bartolomeo et al. may benefit less from strategic behavior than the localization task used in the present study. In particular, we suspect that requiring participants to respond to a target of a particular identity (or color) and also having that target occur in the same location as a prime of different identity (or color) are factors that invited strategic behavior in the current study and that were not present in the Bartolomeo et al. study.

Strategic Control Over Event Integration

The results reported here suggest that strategic control plays an important role in performance in spatial localizations tasks, but precisely how is the role of strategic control best conceptualized? At first blush, the role of strategic control in the current study may appear rather straightforward. In particular, aware/strategy participants may maintain attention to the location of the prime X, thereby pre-activating the internal representation of that location, giving it a competitive advantage over others upon onset of a following probe. This competitive advantage would produce the positive priming effect observed for aware/strategy participants. In contrast, when participants are unaware of the high proportion of repetitions, they may disengage their attention from the prime, leaving localization performance open to the inhibition of return effect; that is, localization may be slow for location repetitions because attention is inhibited from returning to the location at which a prime letter appeared (Posner & Cohen, 1984).

This description of the qualitative difference result reported here appears applicable in studies that involve spatial orienting but perhaps applies less well to related findings in studies outside the spatial orienting arena (e.g., Eimer & Schlaghecken, 2002). Indeed, we have observed similar results to those described here in a series of experiments that requires participants to name words presented centrally and that cannot reasonably be explained by reference to processes that govern shifts of spatial attention (Skye & Milliken, 2008). Although separate explanations might be forwarded for each of a host of such demonstrations, an explanatory framework that encompasses all such demonstrations and that focuses broadly on strategic control over event integration processes might ultimately prove useful.

To that end, we suggest that the object file updating framework of Kahneman et al. (1992) provides a reasonable starting point for discussion. Recall that Kahneman et al. proposed that onset of a perceptual event cues the retrieval of temporary episodic memory representations (i.e., object files) that match spatio-temporally with the current perceptual event. If such a spatio-temporal match is found, then an updating process unfolds in which properties of the perceptual event are integrated with those of the retrieved object file. In contrast, if no spatio-temporal match for the current perceptual event is found, then a new object file is created. In cases in which perceptual events are integrated with existing object files, when properties of the current perceptual event match the corresponding properties of the retrieved object file, then integration occurs quickly. Otherwise, for mismatching properties this integration occurs more slowly. Hommel and colleagues (Hommel, 1998; Hommel, Musseler, Aschersleben, & Prinz, 2001) have since introduced the term "event file" to emphasize the idea that response-related representations can also be integrated into these memory episodes, and they have shown that event file updating processes can be cued by perceptual and response feature matches that are not strictly spatio-temporal in nature. The challenge here is to examine whether strategic control processes can be fit into this event integration framework in a way that captures a range of qualitative difference results reported here and elsewhere.

According to this framework, inhibition of return effects can be cast as occurring under conditions in which creating a new event file (when cue and target differ in location) occurs more quickly than updating an existing event file (when cue and target appear in the same location). The fact that inhibition of return effects often occur only for relatively long cue-target stimulus onset asynchrony (SOA) fits well with this framework if one assumes that increases in the cue-target SOA slow the integration of a perceptual target with a memory representation of the preceding cue, thereby increasing the likelihood that new event file creation is faster than old event file updating.

Within the event file updating framework, the strategic processes that control whether participants respond more quickly to location repetition or location change trials (i.e., the qualitative difference of interest here) may be seen to modulate this event file integration process. In effect, when participants are not aware of the high likelihood of repetition, they may engage in preparatory processes that optimize the encoding of new perceptual events. These preparatory processes may involve some form of flushing of working memory of existing event representations that could interfere (or that participants assume will interfere) with encoding of the upcoming target. This preparation to encode a new event may indeed not only lead to rapid encoding of targets that occur in locations that were not occupied in
AQ: 6 the preceding prime display, but also to slow updating of existing representations for targets that occur in locations that were occupied in the preceding prime display. In contrast, when participants are aware of the high likelihood of location repetition, they may engage in processes that optimize the updating of an existing event representation. This preparation process may involve retaining a representation of the prime event in working memory, or actively re-coding that representation, with particular emphasis on the prime event's location and any other task relevant features that may be bound to that location (see Hommel, 1998). Strategic retention of this event representation may then lead to particularly fast responses when a probe matches this representation but slow responses when a probe mismatches this representation.

In summary, participants may be capable of preparing optimally to encode a new event, in which case they will respond quickly to new events (i.e., targets that appear in a different location than the preceding prime) but slowly to old events (targets that appear in the same location as a preceding prime). This preparatory set favoring the encoding of new events may well be used by our participants when targets are unlikely to occur in the same location as a preceding prime or when participants are unaware of a high likelihood of targets appearing in the same location as a preceding prime. Alternatively, they may prepare optimally to update an existing event representation, in which case they will respond quickly to old events but slowly to new events. Whether the qualitative differences as a function of awareness and self-reported strategy in the present study indeed reflect this type of strategic trade-off remains an interesting issue for future research.

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