Contributions of Processing Fluency to Repetition Effects in Masked Word Identification

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Abstract Prior exposure to a word is shown to improve its later identification in a brief, masked display when a free report task is used, but not in two-alternative forced choice or single-probe matching tasks that eliminate certain bias effects and provide an assessment of discriminability. Modified forced choice and single-probe tasks were also used, in which subjects attempted to identify the target before presentation of the probe(s). This modification produced a discriminability advantage for old words, but only in the single-probe task. We argue that prior exposure does not enhance sensory processing of a target word; rather, it increases the fluency with which the target comes to mind when presented under difficult viewing conditions. In forced choice and single-probe tasks, fluency associated with processing the target may be ignored in deference to discriminating among or evaluating the processing of the probe(s).

Memory for specific prior processing episodes can, under appropriate circumstances, exert an indirect or automatic influence on the performance of certain tasks. In this article, we explore how memory for specific episodes can act automatically to influence task performance. By "automatically," we mean that subjects are not attempting to consciously recollect prior events in an effort to improve performance. Rather, their performance is influenced automatically by the consequences of retrieving a representation of an earlier, relevant event. Our goal here is to investigate how automatic retrieval of such an event comes to influence current task performance.

A useful guide for this investigation is the transfer-appropriate processing framework that Roediger and his colleagues have applied to the study of indirect influences of memory (e.g., Roediger & Srinivas, 1993; Roediger, Weldon, & Challis, 1989). According to this framework, memory for a processing episode can be beneficial for later task performance if the remembered episode and the subsequent task share processes. For example, completion of a word fragment (e.g., _l_p_a_t) is enhanced more by prior exposure to the printed word solution for the fragment (*elephant*) than by prior exposure to a drawing of the object denoted by that word (Weldon & Roediger, 1987). Tasks that show perceptual specificity in benefits derived from prior episodes are assumed to rely heavily on data driven processes.

Application of Jacoby's (1991) process dissociation procedure to the word stem completion task (e.g., complete the stem REA___ to form a word) suggests that the automatic influence of prior study episodes on that task is highly dependent on perceptual processes (Jacoby, Toth, & Yonelinas, 1993; Toth, Reingold, & Jacoby, 1994). For instance, study episodes that involved solving an anagram or generating a target word from a semantic cue led to no reliable increase, relative to nonstudied words, in estimates of the automatic influence of memory on stem completion, whereas reading a visually presented word substantially increased estimates of the automatic parameter.

Our primary concern in this article, however, is with the effect of prior study on the masked word identification task (also known as the perceptual identification task). In this task, a target word is briefly displayed and followed by a pattern mask. The task is to identify the word. Various studies have found evidence to support the view that in this task, as in word stem and word fragment completion, effects of prior exposure are mediated by data driven processes. For example, greater enhancement has been found if target words were read as opposed to heard initially (e.g., Jacoby & Dallas, 1981; Levy & Kirsner, 1989). Moreover, Jacoby (1983b) found little or no enhanced identification of words that had been generated from their antonyms, whereas previously read words yielded marked improvement. Reinitz and Demb (1994) have argued that the masked word identification task is even more dependent on perceptual processes than the word fragment completion task. On a masked word identification task, they found no enhancement for compound words (e.g., *toothache*) that were conjunctions of compound words (e.g., *toothpaste, headache*) studied earlier. In contrast, they obtained enhanced word fragment completion and elevated false alarms on a recognition test for such items. Reinitz and Demb argued that effects of prior exposure on the masked word identification task arose from holistic perceptual memories that improved "perceptual clarity" on the identification task.

Two sources of evidence run counter to the view that prior exposure effects in the masked word identification task are mediated exclusively by perceptual processes.

First, in an earlier report (Masson & MacLeod, 1992), we showed that enhancement due to prior exposure effects can be obtained for words that were generated from semantic clues and never seen during study. Furthermore, this enhancement can be just as strong as that produced by reading words during the study phase. Although Toth et al. (1994) argued that these results were likely due to intentional use of memory, we think there is reason for caution on this point. In particular, the Toth et al. criticism is based on results with the word stem completion task, whereas our results were obtained with the masked word identification task. It is quite conceivable that the automatic influence of memory operates differently in these two tasks. For example, the brief presentation of a word is capable of retrieving semantic information under binocular masking conditions such as those used in masked word identification (Carr & Dagenbach, 1990; Dagenbach, Carr, & Wilhelmsen, 1989; de Groot, 1983). A word stem may not provide enough constraints for this to occur, although we know of no test of this claim. In general, however, we think it is plausible that test cues in these two tasks are differentially capable of retrieving prior episodes grounded on semantic information.

We note that an opposite view regarding the relative potential of word stem completion and masked word identification for retrieving semantic aspects of prior study episodes has been put forward by Keane, Gabrieli, Fennema, Growdon, and Corkin (1991) based on results from patients with Alzheimer's disease. Keane et al. found impaired effects of prior exposure to target words on word stem completion with these patients, but preserved effects in masked word identification. They concluded that prior exposure effects in word stem completion depend on conceptual or lexical processes that are at risk in Alzheimer's disease, whereas improved performance on masked word identification is mediated by perceptual processes. A difficulty with this position is that the Toth et al. (1994) results are not consistent with the supposed conceptual basis for prior exposure effects on the word stem completion task. We are not in a position to resolve this issue at the present time, but anticipate that future research will provide clarification. For current purposes, we have adopted the working assumption that a briefly presented target word in the masked word identification task is capable of recruiting semantic information from related encoding episodes.

A second source of evidence for nonperceptual contributions to enhancement is relevant to the strong view that prior exposure enhances perceptual clarity in the masked word identification task. If perceptual clarity is improved by prior exposure, then variants of the masked word identification task that allow application of signal detection analysis should yield improved discriminability as a result of prior exposure. This has not proved to be the case. Ratcliff and his colleagues (Ratcliff, McKoon, & Verwoerd, 1989; Ratcliff & McKoon, 1993) used a two-alternative forced choice procedure in which masked presentation of a target was followed by two clearly presented alternatives, one of which matched the target. Prior exposure to the target failed to increase accuracy on this task. Similarly, Masson and Freedman (1990) found that when subjects made lexical decisions about masked targets, increased accuracy in responding to previously studied words was offset by increased false alarms to nonwords that resembled old words (e.g., *tuble* instead of *table*). Thus, discriminability for old items was not reliably different from discriminability for new items.

If prior exposure does not improve perceptual clarity, what is the source of improvement on the masked word identification task? Ratcliff et al. (1989) proposed that a form of "perceptual bias" is responsible for this effect. In their forced choice experiments, they found that when the alternatives that followed the masked target consisted of one old and one new word that were orthographically similar (e.g., *died, lied*), subjects were more likely to select the old alternative, regardless of whether the masked target word was old or new. Ratcliff et al. attributed this bias to an early perceptual process, rather than to a deliberate response bias, because (1) subjects reported that old words seemed to "jump" out of the display, and (2) response times were generally less than 1 s, indicating that there was little strategic processing. They proposed that the bias operates when subjects must select one member of a set of candidates invoked by an impoverished stimulus (e.g., a masked target word or word fragment). Under these conditions, subjects preferentially select an old word. If response alternatives provided on a forced choice test are not orthographically similar, the bias has no grounds to operate, and no bias effect is found (Ratcliff et al., 1989, Experiment 5).

Building on the Ratcliff et al. (1989) proposal, we have developed a somewhat different account of how prior exposure influences performance on masked word identification. The brief presentation of the target word serves as a retrieval cue, recruiting knowledge and episodes connected to the orthographic information in the target (see also Jacoby & Brooks, 1984; Kahneman & Miller, 1986; Whittlesea & Brooks, 1988; Whittlesea & Cantwell, 1987). For old words, the recent encoding episode in the study phase should be easily retrieved relative to other episodes, in part because of the strong temporal and contextual overlap between study and test conditions (Jacoby, 1983a). Note that, by retrieval, we do not mean that information about the study episode is necessarily available to consciousness, but rather that the target word comes fluently to mind. This fluency can be ascribed to a number of possible sources. In an identification task, subjects often attribute fluency to clarity of perception (see Jacoby, Allan, Collins, & Larwill, 1988, and Witherspoon & Allan, 1985, for other examples of attributions of memory-induced fluency to perceptual causes). It is this attribution that is responsible for the impression that a word seems to "jump" out of the display. As in the Ratcliff et al. account, nothing in the account we present here implies an improvement in sensory processing of a briefly presented target word. Thus, failure to find an effect of exposure on the forced choice task, which Ratcliff et al. took as powerful evidence against the "perceptual clarity" view, is not problematic from our perspective.

Before rejecting the view that prior exposure improves sensory processing (perceptual clarity) in the masked word identification task, we note that there are grounds to question the generality of the Ratcliff et al. (1989) results. Target words in the Ratcliff et al. experiments either were presented in the context of sentences or were inferred from sentences without actually being read. Embedding words in text has been shown to reduce the effect of prior exposure in masked word identification (e.g., Levy & Kirsner, 1989), and there is some controversy regarding the source of prior exposure effects on such tasks for words that were not actually read (Toth et al., 1994).

Given these concerns, we thought it advisable to replicate the forced choice results reported by Ratcliff et al. (1989), using the typical encoding task of reading aloud. Moreover, Ratcliff et al. demonstrated a bias to prefer an old word when presented with an old and a new word as forced choice alternatives. To rule out this particular bias, we equated forced choice alternatives with respect to whether they were old or new. We also considered the possibility that the forced choice procedure might interfere in some way with processes that would otherwise generate a prior exposure effect. For example, when required to discriminate between two available alternatives, subjects may neglect evidence, such as the fluent retrieval of the target word, that would have been used in the standard free report situation. In place of the neglected evidence, subjects might use only contrastive information that permits discrimination between the two alternatives (e.g., noticing a line feature in the second letter position of the target display permits discrimination between the alternatives *list* and *lost*). To mitigate such effects, we used a single-probe task in some of the experiments we report. In this task, only one probe word was presented following the target. The probe was always equated with the target with respect to whether it was old or new and either matched it or was unrelated to it. In each experiment, the crucial question was whether prior exposure to target words would significantly improve performance on a task designed to separate discriminability from response bias.

GENERAL METHOD

All of the experiments reported here were very similar with respect to methodology, so in this section we provide a general description that applies to all of the experiments. Features specific to each experiment are described at appropriate points.

Subjects

The subjects were undergraduates at the University of Victoria. They received extra credit in an introductory psychology course for their participation.

Materials

Two sets of common English words were used for each experiment. Critical words were used in the study and test phases of each experiment. A second set of words was used in a pretest phase that gave subjects practice at the masked word identification tasks and allowed individual adjustment of the visual contrast used in displaying words during the test phase.

Procedure

Experimental events were presented using a Macintosh II computer with two monochrome monitors. Words appeared on the monitors as black letters against a light gray background. One monitor was viewed by the subject; the other was visible only to the experimenter. The experimenter's monitor showed the words that the subject was viewing and allowed the experimenter to determine whether the subject's responses were correct. There were three phases in each experiment. In the study phase, subjects read aloud four practice words, a subset of the critical words presented in random order, and finally four filler words. Each word appeared individually in lowercase at the centre of the subject's monitor. The word remained in view until the subject read it aloud and the experimenter pressed a key to verify a correct reading.

The second phase, which began immediately after the study phase, consisted of a pretest that permitted adjustment of the visual contrast of displayed words and a set of critical test trials. The pretest and critical test trials were identical in form. In some experiments, both free report and forced choice trials were included; in other experiments, only single-probe or only forced choice trials were used. In the former case, the two types of trials were randomly intermixed. On pretest trials, the display contrast was initially set at its maximum value, then automatically adjusted (by varying the darkness of the pixels that formed the word), depending on performance sampled across a small set of trials. In experiments involving both free report and forced choice trials, contrast values were determined independently for each task. In general, performance levels on critical items turned out to be higher than would be expected, given the contrast levels established in the pretest trials. This situation was not problematic, however, because performance levels were well below ceiling.

A summary of the general procedure for free report, forced choice, and single-probe test trials is shown in Figure 1. Each trial began with the word READY presented at the centre of the monitor until the subject pressed a response button. The monitor was blank for 255 ms, then a pair of hyphens separated by a string of blank characters appeared for 255 ms. The target word was then presented in lowercase in the space between the hyphens for 45 or 30 ms. The word and hyphens were replaced with a mask that consisted of a string of ampersands that remained in view for 150 ms. This mask was then replaced by one of three displays, depending on the type of test trial. A row of four ? characters indicated that free report was required, a pair of words indicated that a forced choice was to be made, and a single word was used for single-probe trials. On forced choice trials, the target and foil appeared in uppercase letters side by side, with the location of the target randomly determined. Lowercase letters were used

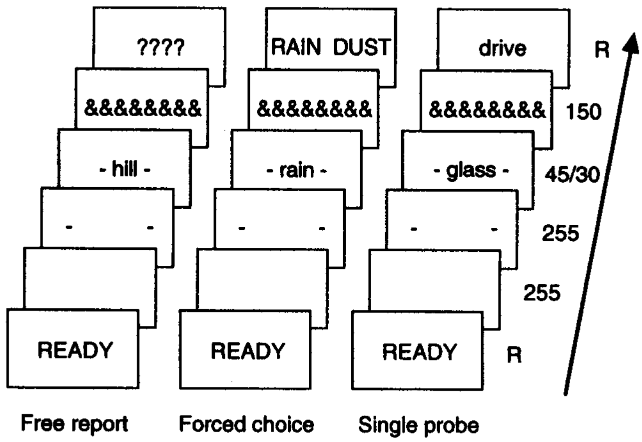

Figure 1. Summary of stimulus events on free report, forced choice, and single-probe test trials. Stimulus durations are shown in milliseconds; events requiring a response are designated with an R. Target words were presented for either 45 or 30 msec.

for probe words on single-probe trials. Uppercase letters were used for forced choice trials because in Experiment 1 the two alternatives differed by only one letter (e.g., *DECK DUCK*). We were concerned that subjects might adopt the strategy of first determining which letter pair distinguished the alternatives (e.g., *E/U*), then deciding which member of the pair was in the target word. We expected that using letters of different case for targets and probes would discourage that strategy. For the sake of consistency, we used uppercase alternatives in the remaining experiments that used the forced choice procedure.

Subjects responded orally on free report and forced choice trials, and pressed one of two labeled buttons on a response box when the single-probe task was used. To avoid encouraging subjects to guess, they were not required to provide an answer on free report trials if they believed they could not identify the target word.

When pretest trials had been completed, the critical trials began with no interruption in the flow of events. The contrast value determined for each task during the pretest was used for all test trials. Once all test trials had been completed, the computer displayed a summary of the subject's performance and the subject was debriefed.

Experiment 1

In Experiment 1 we attempted to replicate the Ratcliff et al. (1989) failure to find an effect of prior exposure on a forced choice version of the masked word identification task, while at the same time demonstrating an effect on the free report variant of the task. Subjects read aloud a set of words in a study phase, then were tested on old and new words using a mixture of free report and two-alternative forced choice trials. Each target word was followed by a cue to indicate the task on the current trial. On free report trials, subjects responded to a target by saying what they thought it was (or by stating that they were unable to identify it). On two-alternative forced choice trials, subjects selected the word that they believed matched the target word. As in most of the Ratcliff et al. experiments, foils were orthographically similar to targets (differing in only one interior letter). In addition, we ruled out the bias to select old words that Ratcliff et al. demonstrated by equating each pair of alternatives with respect to old/new status. That is, both members of a pair were old words, or both were new.

METHOD

Twenty-four subjects were tested. The 80 critical pairs were orthographically similar (but semantically unrelated) words that differed only in one interior letter (e.g., *cage, cave*). Half of the 80 pairs were presented in the study phase. These 80 study words were shown individually, in random order. For the pretest phase, a set of 60 single words and 60 orthographically similar pairs were used for free report and forced choice trials respectively. None of these items had appeared in the study phase. In the case of forced choice trials, one member of each pair was randomly selected to be the target.

Automatic adjustment of contrast in the pretest was calibrated to achieve approximately 50% accuracy on free report trials and 70% accuracy on forced choice trials, using only new words in both cases. We measured contrast using the gray scale values associated with the computer's video card. A value of 0 indicates maximum contrast and a value of 255 indicates no contrast. In the test phase, one member of each critical pair was the target item. Half of the studied pairs and half of the nonstudied pairs were tested using the free report procedure (40 trials); the remaining items were tested using the forced choice procedure (40 trials). In the latter case, the two presented alternatives were the target and its orthographically similar partner. Assignment of critical word pairs to study or nonstudy conditions, to free report or forced choice tests, and of word pair members to the role of target item was counterbalanced across subjects so that each critical word served equally often in each condition.

RESULTS

For the statistical tests reported in this and subsequent experiments, the type I error probability was set at .05. On average, the contrast level determined in the pretest was very similar for free report and forced choice trials (120 vs. 116 on the video card gray scale). Differential performance on these two tasks, then, cannot be attributed to substantial differences in contrast.

Performance on both the free report and forced choice tasks was scored as proportion correct, and separate scores were obtained for old and new items. The mean and standard deviation for each condition and task are shown in the first row of Table 1. For the free report task, there was a clear advantage for old over new items, $F(1, 23) = 9.80$, $MS_e = 0.020$. For the forced choice task, however, the mean proportion correct was the same for both old and new items, $F(1, 23) < 1$, $MS_e = 0.010$. We computed an estimate of the power to detect an effect of prior exposure in the forced choice task, based on the

TABLE 1
Mean and Standard Deviation (in parentheses) for Proportion Correct as a Function of Target Status in Experiments Using Free Report and Forced Choice Tasks

Experiment	Special feature	Free report		Forced choice	
		Old	New	Old	New
1	Orthographically similar pairs	.59 (.22)	.46 (.22)	.80 (.12)	.80 (.12)
2	Unrelated pairs	.66 (.20)	.58 (.20)	.80 (.14)	.78 (.14)
4a	1-s delay, reduce forced choice	.70 (.15)	.57 (.22)	.73 (.19)	.71 (.21)
4b	1-s delay, elevate free report	.84 (.14)	.77 (.18)	.78 (.18)	.76 (.17)
6	2-s delay, attempt report	.38 (.23)	.28 (.17)	.81 (.12)	.78 (.12)

Note. Delay refers to the delay between offset of the target's mask and onset of the response cue (a row of ? for free report, two words for forced choice). Free report data in Experiment 6 refer to attempts to report the target before the alternatives appeared.

effect size observed on the free report task. The power estimate was .66.

DISCUSSION
These results constitute a replication of the Ratcliff et al. (1989) findings and provide no support for the view that prior exposure improves the sensory processing applied to old target words. The null effect of prior exposure on the forced choice task was not very convincing, however, because estimated power was rather low. Therefore, in Experiment 2, we sought to replicate the result using a larger sample of subjects. We also changed the foil in each pair of alternatives so that it was orthographically unrelated to its corresponding target, but retained the constraint that both items in a pair had the same old/new status. Ratcliff et al. (Experiment 5) considered the possibility that using orthographically similar alternatives in the forced choice task might constrain the use of visual information and thereby obscure potential discriminability effects. This putative constraint is removed by using unrelated alternatives.

Experiment 2

METHOD
Forty subjects were tested. The materials and procedure were the same as in Experiment 1, except that the word pairs that served as critical items and as pretest items were not orthographically similar (e.g., *cage plow*).

RESULTS AND DISCUSSION
The mean gray scale values were significantly different for the free report and forced choice trials (137 vs. 180), $F(1, 39) = 14.89$, $MS_e = 2{,}423$. Lower contrast settings were required to bring forced choice performance into the desired range (recall that higher gray scale values represent lower contrast), indicating that the use of orthographically distinct probe pairs on the forced choice trials made discrimination easier than in Experiment 1.

The means and standard deviations for proportion correct are shown in the second row of Table 1. As in Experiment 1, prior exposure significantly improved free report performance, $F(1, 39) = 19.07$, $MS_e = 0.008$, but not forced choice, $F(1, 39) = 1.70$, $MS_e = 0.005$. Using the obtained effect size from the free report task, we estimated the power to detect a similarly sized prior exposure effect on the forced choice task in this experiment to be .94.

With a more powerful experiment and the elimination of a potential constraint on the use of visual information, we again found no effect of prior exposure on the forced choice task. If prior exposure improves sensory processing at all, the effect must be substantially smaller than the effect found in free report, otherwise we would have detected it in Experiment 2.

Experiment 3

The failure to obtain an effect of prior exposure in a forced choice variant of masked word identification might be due to unintended effects of task demands associated with the forced choice procedure. In particular, subjects' ability or willingness to use visual information from the target display might actually be disrupted by presentation of the two alternatives. The requirement to discriminate between two available alternatives directs attention to the characteristics of and differences between the alternatives, and away from a direct evaluation of the information obtained from the target. We refer to this basis of responding as the *comparative strategy*. We attempted to attenuate the comparative strategy while continuing to obtain a measure of discriminability by using a single-probe task. Rather than presenting two alternatives after the target, subjects were shown a single word and were to decide whether it matched the target word. Foil words were unrelated to their corresponding targets except that each target-foil pair was matched on old/new status.

METHOD
Thirty-two subjects were tested. The critical and pretest word pairs were the same as in Experiment 2. An additional four pairs of unrelated words were chosen for use as practice items in the pretest. Half of the critical word

pairs were presented in the study phase using the same procedure as in the earlier experiments (i.e., words were presented individually, in random order), except that no filler words were presented. Pretest and test trials were the same as the forced choice trials in the earlier experiments, except that a single probe word was presented in lowercase instead of a pair of words in uppercase. The probe either matched the target or was the target's designated foil. This choice of foil ensured that the foil always had the same study status (old or new) as the target. No free report trials were presented. Four practice trials were presented at the start of the pretest phase, using target durations of 90 , 75 , 60 , and 45 ms, respectively. All subsequent pretest trials and all critical trials used a 30-ms duration for the target display. Automatic adjustment of contrast in the pretest was calibrated to yield approximately 70% accuracy on new items.

RESULTS AND DISCUSSION

The mean gray scale value was 96, which represents a somewhat higher contrast than in the earlier experiments. More contrast was required at least in part because of the reduced exposure duration (30 ms) in this experiment relative to the earlier experiments (45 ms). The mean hit and false alarm rates for old and new target items are shown in the first row of Table 2. There was no reliable difference between old and new items on either hit rates or false alarm rates ($Fs < 1$). A nonparametric signal detection analysis was used to assess accuracy and response bias independently (Donaldson, 1992; Grier, 1971). Accuracy was measured using A'. Chance performance is indicated by an A' value of 0.5 and perfect performance yields an A' of 1.0. Bias was measured using B''_D. Values on this measure range between -1 and 1, with negative values indicating a liberal bias and positive values reflecting a conservative bias toward a positive response. Mean A' and B''_D values are shown in the first row of Table 2. Analyses of the A' and B''_D scores indicated that there was no reliable effect of prior exposure on either measure, $Fs < 1$. Although there was no effect of prior exposure on A', mean discriminability taken across the entire sample of subjects was clearly above chance, $t(31) = 25.64$, SEM = 0.014. The power of Experiment 3 to detect a prior exposure effect on accuracy, of a size similar to that found with free report in Experiments 1 and 2, was estimated to be .96.

Although there was plenty of power to detect an effect of prior exposure, the single-probe task used in Experiment 3 failed to do so. Thus, in keeping with Ratcliff et al. (1989), we were unable to produce evidence to support the proposition that prior exposure improves sensory processing or perceptual clarity in the masked word identification task.

Experiment 4

The consistent failure to obtain an effect of prior exposure on discriminability measured either by a forced choice task or by a single-probe task stands in contrast to the effect observed in free report. The contrast is particularly striking in the case of Experiments 1 and 2 because forced choice and free report trials were interleaved and produced different results with respect to prior exposure. From the perspective of signal detection theory, this pattern could be taken to mean that our choice of foils effectively nullified the bias that generated the effect in free report (e.g., Ratcliff et al., 1989). We believe, however, that the situation is not so straightforward. In our view, during the free report task, subjects had available information that favoured old words but that information was not used in the forced choice task. That information consisted of a sense of fluency associated with retrieval of an old target word. Faced with the forced choice or single-probe situations, subjects apparently disengaged their use of fluency when making either the discrimination between two available alternatives or the judgment about a single probe.

We reasoned that it might be possible to lead subjects to engage their use of fluency in the forced choice and single-probe tasks if presentation of the alternatives or the probe were delayed. During the delay subjects ought to attempt to identify the target in much the same way as they would in a free report task. Thus, they should be sensitive to the influence of processing fluency, and this sensitivity should affect their interim attempt at identification. This influence of fluency, which would favour old words, should then carry over to the subject's decision about the response alternatives or the single probe. Having tentatively identified the target, the subject should be very likely to accept an alternative or probe that is consistent with that preliminary identification. Given that the preliminary identification should be based on the same sources of information as responding under free report, old targets should be identified with greater probability, leading to an advantage on the subsequent forced choice or single-probe decision.

If the introduction of a delay were to produce an effect of prior exposure on the forced choice or single-probe tasks, we would not conclude that prior exposure had increased sensory processing or perceptual clarity of old targets. Clearly, delaying the probe(s) would not have altered the available perceptual information. Instead, the delay should lead subjects to depend on a basis of responding (a sense of processing fluency) that we believe is disengaged in variants of the masked word identification task that involve presentation of response alternatives or probes.

A 1-s delay between the target and the probe(s) was selected because subjects in our experiments had typically responded within that interval during free report. In Experiment 4, we delayed by 1 s the presentation of the alternatives in the forced choice task. Two versions of Experiment 4 were conducted. In Experiment 4a we adjusted the contrast setting to reduce overall performance in the forced choice task, thereby bringing mean accuracy closer to the level achieved in free report. In Experiment 4b, we altered the contrast settings to increase accuracy in the free report task. These changes were designed to address the possibility that differences between free report

TABLE 2
Mean and Standard Deviation (in parentheses) for Hit and False Alarm Rates, Discriminability (A'), and Bias (B''_D) as a Function of Target Status in Experiments Using the Single-Probe Task

		Hit rate		FA rate		A'		B''_D	
Experiment	Special feature	Old	New	Old	New	Old	New	Old	New
3	Immediate	.72	.70	.16	.16	.86	.86	.27	.29
		(.21)	(.18)	(.13)	(.14)	(.09)	(.08)	(.59)	(.67)
5	2-s delay	.73	.70	.24	.23	.81	.80	.13	.21
		(.18)	(.18)	(.18)	(.15)	(.15)	(.13)	(.53)	(.47)
7	2-s delay, attempt	.66	.55	.07	.07	.88	.84	.67	.74
	report	(.20)	(.25)	(.10)	(.10)	(.06)	(.12)	(.53)	(.47)

Note. Immediate and delay refer to the interval between offset of the target's mask and onset of the probe.

and forced choice were due to some kind of scaling artifact, in which the prior exposure effect is weakened at higher levels of accuracy. Finally, we sought evidence regarding the possibility that in the free report task, subjects have a general bias to be more likely to provide a response (even a guess) when the masked target is an old word. A bias in response criterion of this sort could produce a prior exposure effect that would not be revealed on a forced choice test.

METHOD

There were 24 subjects in Experiment 4a and 32 in Experiment 4b. Both experiments used the same critical, pretest, and filler items and procedure as in Experiment 2, with the following exceptions. A 1-s delay was inserted between offset of the target's mask and onset of the test probe (a row of question marks or a word pair) that indicated which type of response was required (free report or forced choice). The only procedural difference between the two versions of Experiment 4 was that the gray scale adjustments during pretest trials were calibrated differently. In Experiment 4a, forced choice performance was calibrated for 60% accuracy, whereas free report remained calibrated at 50%. In Experiment 4b, the calibration for free report was increased to 70%. Finally, a record was kept of response errors made on the free report task in both experiments so that these errors could be examined.

RESULTS

As in Experiment 2, the mean gray scale values for the free report and forced choice tasks were reliably different in Experiment 4a (120 vs. 178), $F(1, 23) = 13.38$, $MS_e = 3{,}008$, and in Experiment 4b (68 vs. 208), $F(1, 31) = 125.79$, $MS_e = 2{,}484$. The gray scale differences indicated that the target was more clearly visible in the free report than in the forced choice task for both experiments.

The mean proportions of correct responses for both tasks are shown separately for Experiments 4a and 4b in the third and fourth rows of Table 1. The effect of prior exposure was significant on the free report task in both experiments, $F(1, 23) = 10.49$, $MS_e = 0.018$, and $F(1, 31) = 6.62$, $MS_e = 0.012$, for Experiment 4a and 4b, respectively. Neither experiment, however, yielded evidence for a reliable effect of prior exposure on the forced choice test, $Fs < 1$. The estimated power of the forced choice test to detect an exposure effect equal to that observed in the corresponding free report test was .99 for Experiment 4a and .74 for Experiment 4b.

Free report trials on which subjects failed to respond correctly involved either an intrusion (responding with an incorrect word) or no response. These trials were examined for evidence of a general response bias associated with old targets. We computed the proportion of these trials that involved an intrusion. In Experiment 4a, the mean proportions of intrusions associated with old and new targets were .61 and .62, respectively. These means were not reliably different, $F < 1$. In Experiment 4b, data from four subjects were not included in this analysis because they committed no errors in one of the two study conditions. The proportion of intrusions was somewhat lower for old than for new targets (.65 vs. .81), $F(1, 27) = 3.19$, $MS_e = 0.106$, $p < .10$, which is actually in the opposite direction from what would be expected from a general bias to produce a response to old targets.

DISCUSSION

Despite changing the relative accuracy level of forced choice and free report, and inserting a delay before presentation of the alternatives in the forced choice task, the pattern of results from Experiments 1 and 2 was replicated. This outcome provides evidence against the possibility that a scaling artifact is at work, but does not support our prediction that a prior exposure effect would "carry over" to the forced choice task. Furthermore, in the free report task, we found no evidence of a liberal bias in the response criterion for old targets. When subjects failed to identify correctly the target in free report, they were at least as likely to generate some response (an intrusion) for new as for old targets. We also examined intrusion rates in subsequent experiments that involved free report (Experiments 6 and 7), and found no evidence of a reliable difference between old and new targets in the probability of intrusions when the target was not correctly reported. Therefore, the issue of response criterion is not considered further.

Experiment 5

In a further effort to induce subjects to engage the attribution of fluency that putatively arises from an old target, we extended the delay to 2 s and used the single-probe task. We supposed that by presenting only a single probe, subjects would not heavily rely on the comparative strategy, to the possible exclusion of other relevant evidence. Thus, they might be more likely to be sensitive to the fluency experienced during the earlier processing of the target.

METHOD

Twenty-four subjects were tested using the same materials and procedure as in Experiment 3, except that there was a delay of 2 s between the offset of the mask that followed the target word and the onset of the probe word.

RESULTS AND DISCUSSION

When the nonparametric signal detection analysis was applied to the data of Experiment 5, it was found that one subject had an unusually low A' score on new items (.09, where chance is .50) because the hit rate was lower than the false alarm rate (.15 vs. .35). We assumed that this aberrant score was due to measurement error, and that this subject's true score was much closer to chance. No subject in any of the single-probe experiments reported here obtained such a deviant A' score. To avoid introducing an excessive amount of variability into our analyses, we omitted this subject's data from the results reported here. Additional analyses that included this subject's data found exactly the same pattern of results, but for the sake of cross-experiment comparisons we report only the analyses that excluded those data.

The mean gray scale value was 106, similar to the value of 96 obtained in Experiment 3. The mean hit and false alarm rates and the results of a nonparametric signal detection analysis are shown in the second row of Table 2. There was no reliable difference between old and new items on either hit rates or false alarm rates, *F*s < 1.1. Analyses of the A' and B''_D scores indicated that there was no reliable effect of prior exposure on either measure, *F*s < 1.3. The power to detect an effect of prior exposure of a size similar to that found with free report in the earlier experiments was estimated to be .75. Thus, extending the delay of the probe and moving to a single-probe task failed to produce an effect of prior exposure.

Experiment 6

It is possible that despite the delay between the masked target and the presentation of the probe in Experiments 4 and 5, subjects may not have made an attempt to identify the target prior to the onset of the probe. Failure to make such identification attempts might account for the absence of an effect of prior exposure on accuracy in the forced choice and single-probe tasks. Therefore, in Experiment 6, we used a delay between the masked target and the presentation of the alternatives in a forced choice task, but we also required subjects to attempt to identify the target during the delay interval. We refer to this requirement as a preliminary free report task. In this experiment, we did not include any regular free report trials, thereby increasing the number of forced choice trials in an effort to increase power to detect an effect on the forced choice task. The delay interval was set at 2 s to permit subjects enough time to make a preliminary free report response before the alternatives were presented. Finally, we reduced the exposure duration of the target to 30 ms (as in the single-probe experiments). The shorter duration was used because we anticipated that smaller contrast reductions would be needed to move subjects into the desired performance range. We were concerned that the larger contrast reductions used in the earlier experiments might have produced additional variability in performance.

METHOD

Twenty-four subjects were tested using the same materials and procedure as in Experiment 4, with the following exceptions. First, the target words were exposed for only 30 ms before the mask was presented. Second, the delay between the offset of the mask and the onset of the response alternatives was increased to 2 s. Third, during the 2-s delay on each trial, subjects attempted to report the target word, as subjects in earlier experiments had done on regular free report trials. When the response alternatives appeared, subjects made a choice between the two alternatives as in Experiments 1, 2, and 4. The pretest was calibrated to yield 60% correct for new items on the forced choice task.

RESULTS AND DISCUSSION

The mean gray scale value was 89, which is comparable to the average contrast settings used in the single-probe experiments. The mean proportions of correct responses in the forced choice task are shown in the last row of Table 1. There was no reliable effect of prior study on these scores, $F(1, 23) = 1.15$, $MS_e = .005$. Attempting a preliminary free report before presentation of the response alternatives did not lead to a reliable effect of prior exposure. The estimated power of the forced choice test to detect an exposure effect of half a standard deviation, which is roughly the effect size observed with the free report task in the earlier experiments, was .83.

The failure of preliminary free report attempts to generate a prior exposure effect on the forced choice test might have been due to subjects' inability either to identify words or to differentially identify old versus new words. The mean proportions of correctly identified targets on preliminary free report attempts are shown in Table 1. Although these proportions were not as large as those obtained on regular free report trials in earlier experiments, it is clear that subjects were able to identify a substantial number of targets before the response alternatives were presented. Moreover, old targets were reported significantly more often than new targets, $F(1, 23) = 20.54$, $MS_e = .005$. When making the subsequent forced choice, subjects always selected the correct alternative after responding correctly in their preliminary free report attempt. On the other hand, when subjects failed to report the target, their likelihood of being correct on the forced choice task was much lower than when preliminary free report was successful, and it was not affected by prior exposure (.72 for old items and .71 for new items), $F < 1$.

Although subjects were able to identify old targets more often than new targets when making a preliminary free report, this effect was not sufficiently robust to produce a reliable effect of prior exposure on forced choice performance. We see two possible reasons for this outcome. First, the proportion of trials on which correct free report occurred may have been too low for report attempts to influence substantially forced choice performance. Second, it may be particularly difficult to steer subjects away from basing a forced choice response exclusively on the comparative strategy discussed earlier. In the final experiment, we attempted to reduce the role of the comparative strategy by using the single-probe task and requiring subjects to attempt a preliminary free report before the probe appeared.

Experiment 7

METHOD

Twenty-four subjects were tested using the same materials and procedure as in Experiment 5, with the following exception. During the delay of 2 s between the offset of the mask and the onset of the probe word, subjects attempted to report the target, as in Experiment 6. When the probe appeared at the end of the delay, subjects responded as in Experiments 3 and 5, indicating whether they believed the probe matched the target. The experimenter recorded the preliminary free report attempt for later analysis.

RESULTS AND DISCUSSION

The mean gray scale value was 97, which was similar to the means in Experiments 3 and 5. The mean hit and false alarm rates and the results of a nonparametric signal detection analysis are presented in Table 2. An analysis of hit rates showed that subjects were more likely to detect a matching probe when the target was an old word, $F(1, 23) = 9.76$, $MS_e = 0.015$. There was no offsetting influence of prior exposure on false alarm rates, $F < 1$. As might be expected from the hit and false alarm results, there was a reliable advantage for old words in the discriminability measure, A', $F(1, 23) = 5.58$, $MS_e = 0.004$. There was no effect of prior exposure on the bias measure, $F < 1$.

The mean proportions of correct free report responses made before the probe appeared are presented in Table 3. Old targets were more often reported than new targets, $F(1, 23) = 15.90$, $MS_e = 0.006$. Once a target had been reported correctly, subjects usually, but not always, made a correct response to the probe, as indicated by the conditional probabilities in Table 3. None of the measures of single-probe responding, conditionalized on correct preliminary identification, yielded a reliable effect of prior exposure, $Fs < 1.2$. Thus, the increased discriminability observed for old items on the single-probe test was due at least partly to the advantage held by these items when making preliminary free report responses.

The fluency associated with old targets extended beyond those instances in which a correct preliminary free

TABLE 3
Mean and Standard Deviation for Preliminary Free Report Responses and for Conditionalized Single-Probe Responses in Experiment 7

Dependent variable	Old	New
Free report	.50 (.20)	.41 (.19)
p(Hit | FR)	.93 (.15)	.93 (.14)
p(FA | FR)	.02 (.05)	.02 (.04)
A' | FR	.97 (.05)	.98 (.04)
B"$_D$ | FR	.66 (.52)	.25 (.99)
p(Hit | ¬FR)	.42 (.32)	.29 (.24)
p(FA | ¬FR)	.10 (.16)	.10 (.13)
A' | ¬FR	.78 (.18)	.71 (.18)
B"$_D$ | ¬FR	.68 (.58)	.74 (.46)

Note: FR = correct free report; ¬FR = failed free report; FA = false alarm.

report response was given. Single-probe performance on those trials on which the preliminary free report response was incorrect showed evidence of an advantage for old items (see Table 3). The hit rate for old items was reliably higher than for new items, $F(1, 23) = 4.74$, $MS_e = 0.047$, whereas the false alarm rates did not differ reliably, $F < 1$. The effect of prior exposure, however, did not reach significance on the A' measure, $F(1, 18) = 2.55$, $MS_e = 0.021$.[1]

The results involving single-probe performance conditionalized on correct or incorrect preliminary free report serve to deflect a criticism regarding the prior exposure effect on the overall measure of discriminability. Namely, it might be claimed that the effect merely indicates that once subjects committed themselves to a particular free report response, they "stuck to their guns" when the probe appeared. The situation is clearly not that simple. First, subjects occasionally failed to accept the probe, even after correctly giving it as the preliminary free report response. Second, even when the preliminary free report was incorrect, subjects were more likely to accept a correct probe if it was an old target word than if it was a new one. Thus, even though subjects may not have been confident enough to emit a response (or perhaps responded with an incorrect word that seemed more plausible than the real target), they apparently were sensitive to the fluency associated with old targets. We suggest that this sensitivity was responsible for the higher hit rate among old targets on trials involving an incorrect preliminary free report.

As evidence for our claim that subjects in Experiment 7 were sensitive to fluency associated with old targets in

[1] The prior exposure effect on A' scores was unusually variable across subjects (compare the MS_e value for this test to that based on all trials) and data from five subjects were not included in the analysis because, for these subjects, the hit and false alarm rates for at least one condition were both zero. Two additional subjects were excluded from the analysis of B"$_D$ because of perfect discrimination in one condition (hit rate of 1.0 and false alarm rate of 0.0). The analysis of B"$_D$ scores indicated no reliable effect of prior exposure, $F < 1$.

making their responses to probes, we offer a comparison between Experiment 7 and Experiments 3 and 5, with respect to the bias measure. Given that an additional basis for responding was invoked in Experiment 7, we might expect subjects to be less likely overall to accept the probe as a valid match to the target. This conservatism was borne out by comparisons between B''_D scores in Experiment 7 and those in Experiments 3 and 5. The B''_D scores were reliably larger in Experiment 7 than in either of the earlier experiments, $F(1, 54) = 9.00$, $MS_e = 0.556$, and $F(1, 45) = 16.13$, $MS_e = 0.420$, indicating that subjects were more conservative in Experiment 7.

It might be argued that the increased conservatism in responding to probes in Experiment 7 is responsible for producing the effect of prior exposure on A'. In particular, by reducing the false alarm rates and the variability in those alarm rates, more stable A' estimates were obtained in Experiment 7. Although the standard deviations in Table 2 indicate that variability in A' scores for old items was reduced relative to Experiment 5, very little reduction in variability was found for new items. Moreover, the actual source of the prior exposure effect on A' is the hit rates. A reliably higher hit rate for old items was found in Experiment 7, but not in Experiment 5. Furthermore, the number of subjects with a higher hit rate for old than for new items in Experiment 7 was 17 out of 24, but in Experiment 5 it was only 11 out of 23. In contrast, only 6 of 24 subjects in Experiment 7 had lower false alarm rates for old than for new items (equal false alarm rates for the two types of items was common), whereas in Experiment 5, 9 of 23 subjects had lower false alarm rates for old items. It is clear, then, that the reduced false alarm rate in Experiment 7 is not responsible for the prior exposure effect on A' scores.

Finally, we address the possibility that the effect of prior exposure on discriminability in Experiment 7 is attributable to a change in perceptual clarity brought about by prior exposure (Reinitz & Demb, 1994). This possibility seems unlikely because, with respect to presentation of targets, identical perceptual experiences were provided across Experiments 3, 5, and 7. If prior exposure generated an improvement in perceptual clarity, it should have been observed in all three experiments. We also note that, despite the widely held impression that changes in discriminability measured by signal detection analysis reflect changes in perceptual processes (e.g., Farah, 1989; Rhodes, Parkin, & Tremewan, 1993), discriminability effects constitute necessary but not sufficient evidence for changes in perception (Masson & Borowsky, 1996; Norris, 1995). Any information, perceptual or otherwise, that helps discriminate between targets and foils will improve accuracy as measured by signal detection analysis. Our claim is that in the masked word identification task, one source of discriminative information is the fluency with which a briefly presented target word brings to mind the recent encoding episode from the study phase. This effect is assumed to go forward without any increase in perceptual clarity of the target word.

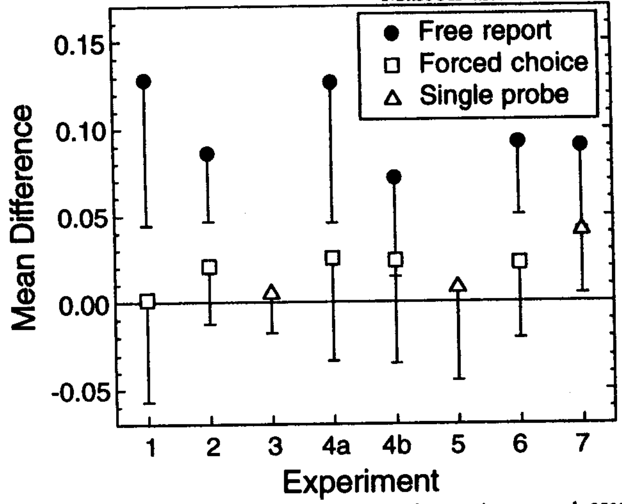

Figure 2. Mean difference between old and new items, and 95% confidence interval, for the free report, forced choice, and single-probe tasks in each experiment. Means for free report and forced choice tasks are based on proportion correct; means for the single-probe task are based on A'.

General Discussion

To assess the influence of prior exposure on performance of a masked word identification task, we used forced choice and single-probe variants of the task to minimize certain bias effects. To this end, we used foil words that always had the same old/new status as the corresponding targets. Thus, when making a choice among two possible alternatives, conscious recollection of prior occurrence could not increase the likelihood of making a correct response. Similarly, when responding to a single probe, the ability to remember that the probe had occurred in the study phase would not provide information relevant to the question of whether the probe matched the target.

The results of our seven experiments, expressed as the mean difference between old and new targets on the free report, forced choice, and single-probe variants of the masked word identification task, are summarized in Figure 2. The figure also presents the 95% confidence interval for each difference between means. This summary of the data highlights a number of important features of the experiments. First, only the free report task yielded a consistent effect of prior exposure. Second, although there was a small advantage for old items in the forced choice and single-probe tasks, only in Experiment 7 did the effect reach significance. Third, the size of the confidence intervals for the forced choice and single-probe tasks were, on average, somewhat smaller than the confidence intervals for the free report task (.05 and .04 versus .06), indicating that the experiments generally had reasonable levels of power to detect mean differences of the size observed in the free report task (an observation substantiated by power analyses reported for each experiment).

Across experiments, the pattern with respect to the forced choice and single-probe tasks is suggestive of an effect of prior exposure, albeit much smaller than that obtained with free report. In Experiment 7, we apparently were able to reveal this effect on the single-probe task when we encouraged subjects to take into account the processing fluency associated with old targets.

Our view is that prior exposure results in creation of an episode that is, at least in some cases, automatically retrieved by the brief presentation of the target word. This fluent, automatic retrieval of a word is naturally experienced as perception, an attribution that leaves the subject with the sense that the target "popped" out of the display. In turn, this experience of fluency increases the probability that subjects will respond with the retrieved candidate.

The failure to find a reliable effect of prior exposure in the majority of forced choice and single-probe experiments indicates that subjects can easily be derailed with respect to their use of fluency. In particular, we suggest that presentation of a pair of alternatives on the forced choice task invites the subject to direct attention to the characteristics of the alternatives, perhaps restricting analysis or attention only to the physical features of the target display that contribute to the discrimination between the alternatives. As a result, attention is unintentionally directed away from other aspects of the experience with the target, such as processing fluency associated with a particular candidate response, that otherwise could make a contribution to responding. A similar process is believed to be at work in the single-probe task except that, as was shown in Experiment 7, presentation of a single alternative is not as disruptive, permitting the target itself to exert more influence.

As argued in the discussion of Experiment 7, we do not interpret the finding of a significant effect of prior exposure on the discriminability measure, A', as evidence for improved perceptual clarity. A discriminability effect, by itself, is not sufficient evidence for such a conclusion. Moreover, the perceptual characteristics of Experiment 7 were identical to those of Experiments 3 and 5, where A' did not show a reliable effect.

The discriminability effect in Experiment 7 cannot be due to the kind of bias observed by Ratcliff et al. (1989). For one thing, the foils in Experiment 7 were not orthographically similar to the targets, a feature that was critical in producing the bias effect obtained by Ratcliff et al. Moreover, the foils and targets were matched with respect to prior exposure, so conscious recollection of prior occurrence cannot account for the result either. Although Ratcliff et al. have uncovered a valid and interesting response bias, we do not think that that bias is responsible for the effect of prior exposure in the free report version of the masked word identification task. Further research is under way to substantiate this proposal.

Our automatic retrieval explanation of the prior exposure effect ties in with a number of other results in the literature. First, the masked lexical decision results reported by Masson and Freedman (1990, Experiment 1) indicated that subjects were more likely to make a false alarm to a nonword (e.g., *tuble*) that was orthographically similar to a previously studied word (e.g., *table*) than to a nonword that did not resemble an old word. In this task, the nonword serves as a viable retrieval cue, capable of calling forward the study episode involving its orthographically related word (see Masson & Freedman, 1990, p. 359). Second, the circumstances under which prior exposure is adequate to enhance masked word identification can be taken as an indication of how the proposed automatic retrieval process works. For example, the finding that auditory study is not as effective as visual study (Jacoby & Dallas, 1981; Levy & Kirsner, 1989) suggests that the retrieval process can be modality dependent as would be expected from the transfer-appropriate processing framework. At the same time, our earlier results that show enhancement following generation of targets from conceptual cues (Masson & MacLeod, 1992) indicate that automatic retrieval can also be triggered by overlapping conceptual knowledge. We view these results as another example of transfer-appropriate processing, except that the process involves conceptual knowledge.

Finally, we consider the issue of whether the effects of processing fluency we have discussed operate without conscious control. One way to address this issue would be to apply the process dissociation procedure to the masked word identification task, as Jacoby et al. (1993) and Toth et al. (1994) have done with the stem completion task. We have tried to do so, but despite various attempts were never able to get performance in the exclusion instruction condition above zero for more than a few subjects. That is, when subjects successfully identified an old item, they were almost always aware that it had been presented in the study phase. Some might wish to take this result as meaning that conscious recollection is a prerequisite for the prior exposure effect. Instead, we suspect that subjects typically recognized the targets as old items after making a successful identification.

In support of the possibility that the prior exposure effects observed in free report and in the single-probe task of Experiment 7 were not entirely mediated by conscious recollection, we make three observations. First, the single-probe task was designed to rule out this possibility by using foils that were matched to targets with respect to prior exposure. Thus, conscious recollection of the probe, whether it be a target or foil, could provide no valid evidence with respect to the match between the probe and the target. Second, it is unlikely that conscious recollection of the target word experienced prior to presentation of the probe could account for the benefit of prior exposure in Experiment 7. Even when subjects failed to make a preliminary free report of the target in that experiment, the hit rate associated with the subsequent presentation of a target probe was greater for old than for new targets, despite no difference in false alarm rates for old and new targets. Third, Richardson-Klavehn, Lee, Joubran, and Bjork (1994) have demonstrated that although nearly all subjects are aware that some test items are old when performing the free report version of the masked word identification task, little in the way of strategic use of memory for prior occurrence operates when performing this task unless subjects are instructed to apply such strategies.

In conclusion, we have put forward the view that a briefly presented target word can automatically retrieve memory for an earlier encoding episode involving that word. Successful retrieval of the relevant episode is experienced as perceptually fluent processing and serves as crucial evidence in favour of the associated target word. Although subjects probably are aware that many of the old words were presented in the study phase of the experiment, we believe there is substantial evidence to support the view that this awareness is not entirely responsible for the observed enhancement effects. Rather, the beneficial effect of retrieving the relevant prior study episode appears to operate through an attribution error. Subjects incorrectly attribute enhanced processing fluency to an illusory sense that the target word was clearly visible.

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Sommaire

La fluidité du traitement
lors de l'identification de mots masqués

L'exposition antérieure à un mot améliore l'exécution d'une épreuve d'identification subséquente dans laquelle le mot est présenté brièvement et suivi d'un masque. Cette amélioration a été démontrée dans une épreuve de rapport libre où les sujets devaient nommer un mot cible présenté brièvement. Deux autres versions de l'épreuve d'identification de mots masqués ont été utilisées. Dans ces deux versions, la tendance à répondre par un mot connu ne pouvait affecter l'effet d'amélioration. Une de ces deux versions était une épreuve à deux choix forcés dans laquelle le mot cible masqué était suivi de deux mots. Un de ces mots correspondait à la cible et l'autre pas. Toutefois, les deux mots avaient été étudiés auparavant ou il s'agissait de deux mots nouveaux. L'autre version de l'épreuve d'identification était une épreuve à essai unique dans laquelle la cible masquée était suivie d'un seul mot, lequel correspondait au mot cible ou pas. Le mot stimulus-sonde correspondait toujours au mot cible qu'il ait ou non été étudié auparavant. Donc, dans les deux versions de l'épreuve d'identification, choix forcé et essai unique, toute tendance à vouloir choisir un mot connu ne pouvait générer un avantage pour les mots cibles déjà étudiés dans cette épreuve. Aucune des deux versions n'a avantagé de façon significative les mots cibles étudiés par rapport aux mots cibles non étudiés, ce qui suggère que l'effet d'amélioration dans l'épreuve de rapport libre était imputable à la tendance à répondre par une cible étudiée. Nous avons testé l'autre hypothèse qui veut que l'exposition antérieure à un mot cible augmente la fluidité avec laquelle le mot vient à l'esprit quand il est présenté dans une épreuve d'identification de mots masqués. La présentation de choix de réponse ou d'un stimulus-sonde unique peut amener les sujets à ignorer cette fluidité pour ne pas juger le test, ni évaluer le traitement du ou des stimulus-sonde. Pour encourager les sujets à rester sensibles à la fluidité de traitement associée à la présentation brève d'un mot cible, nous avons utilisé des épreuves à choix forcé modifiées et des épreuves à stimulus-sonde unique. Dans ces épreuves modifiées, il y avait un bref délai entre la présentation du masque et la présentation des deux mots à choix forcé ou du mot stimulus-sonde. Pendant l'intervalle, les sujets essayaient d'identifier le mot cible de la même manière qu'ils l'auraient fait dans un rapport libre. Quand les deux mots à choix forcé ou le mot stimulus-sonde apparaissaient, les sujets répondaient de la même manière que dans la version habituelle de ces épreuves. Cette modification a produit un avantage discriminatoire pour les mots connus, mais seulement dans l'épreuve à stimulus-sonde unique. Nous pensons que l'exposition antérieure à un mot établit un épisode déjà extrait au moment où ce mot a été présenté comme cible masquée. Bien que plusieurs autres épisodes semblent également être extraits, quelques-uns reliés au mot cible et d'autres reliés à des mots semblables, l'épisode de codage récent relié au mot cible permet à ce dernier de se rendre au cerveau plus facilement que les autres mots. Les sujets attribuent cette impression de fluidité associée au mot cible à une apparente clarté de la présentation de la cible. Le mot semble carrément «sauter» hors de l'écran. L'attribution de cette fluidité à la clarté perceptuelle augmente la probabilité que les sujets identifient le mot cible comme étant l'item présenté. Cet effet est très clair dans l'épreuve de rapport libre, quand toute l'influence du processus de fluidité est en mesure de fonctionner. La présentation de deux choix forcés ou d'un stimulus-sonde unique peut cacher cet effet en amenant les sujets à ignorer la fluidité ressentie au cours de la présentation de la cible, au profit de l'évaluation du stimulus-sonde.