

Priming is not all bias: Commentary on Ratcliff and McKoon (1997)

running head: visual word priming

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Abstract

R. Ratcliff and G. McKoon (1997) reported a set of findings they claim are inconsistent with all prior accounts of long-term priming, including (1) a pattern of benefits and costs in an identification task suggestive of a bias interpretation of priming, and (2) a restriction on priming such that benefits and costs are only obtained when the alternatives in the forced-choice task are similar. Based on these and related findings, the authors developed a bias theory of visual word priming that is implemented in a mathematical model. However, it is shown that their empirical findings are ambiguous and can be explained more parsimoniously within more traditional frameworks. Furthermore, eight studies are reported that directly contradict their model. On the basis of these and related findings, it is argued that priming is best understood as a by-product of learning within perceptual systems whose main function is to categorize inputs.

Visual word priming often reflects perceptual learning rather than perceptual bias:

Reply to Ratcliff and McKoon (1997)

Long-term priming is one of the more widely studied phenomena in cognitive psychology during the past 20 years (see Roediger & McDermott, 1993, for an extensive review that includes over 300 references on the topic since 1980). On the standard view, long-term priming reflects a facilitation in processing a stimulus as a consequence of having encountered the same stimulus in an earlier episode. So for example, participants are more accurate in identifying a quickly flashed word in a perceptual identification task if it was studied a few minutes or hours previously (e.g., Jacoby & Dallas, 1981). The longevity of this priming distinguishes it from various forms of short term priming phenomena -- such as semantic or masked priming -- that tend to last a few seconds (e.g., Henderson, Wallis, & Knight, 1984; Forster & Davis, 1984; but for exceptions to this general rule, see Becker, Moscovitch, Behrmann, & Joordens, 1997; Joordens & Becker, 1997).

In a recent series of articles, Ratcliff and McKoon (R&M) have challenged the common view that priming reflects a facilitation in processing test stimuli, and argued instead that priming is a bias to interpret test items as previously studied materials (McKoon & Ratcliff, 1995, 1996; Ratcliff, Allbritton, McKoon, 1997; Ratcliff, McKoon, 1995, 1996, 1997, Ratcliff, McKoon, Verwoerd, 1989). That is, rather than simply reflecting a benefit in processing repeated materials, priming is interpreted as a mixture of benefits for repeated and costs for related study/test items, with costs equaling benefits. So for example, in a perceptual identification task these authors developed, a test word such as cable is flashed, immediately followed by two alternative words, e.g., table and cable. Participants are asked to select the alternative that matches the flashed word. In this example, benefits tend to be observed when cable was studied in an earlier episode, and cost when table was studied. Accordingly, the greater accuracy in correctly identifying repeated test words is due to a bias to interpret test words as study items rather than a change in sensitivity or in processing test words. As noted by the authors, this conclusion is inconsistent with the view that priming reflects a facilitation in processing repeated materials.

Their work on this issue culminated in a recent Psychological Review article (R&M, 1997) in which they report a number of additional striking findings they claim are incompatible with all existing theories of priming, and where they describe a mathematical model of word priming in the perceptual identification task that purports to account for these findings. In contrast with previous accounts, the authors argue that the processing benefit observed for a repeated word cannot be attributed to a new memory representation, nor the modification of some pre-existing representation of the study word itself. Instead, priming is thought to reflect an interaction between related perceptual knowledge resulting in a tendency to interpret ambiguous information as the study word, as described below. If indeed this model provides a reasonable account of priming phenomena, it would constitute a major theoretical departure from past work.

In the present article, I argue that many of the conclusions the authors advance are unwarranted, for two main reasons. First, the data the authors present in support of their theory are ambiguous, and alternative explanations for their findings within the framework of more traditional models are possible. I offer one such re-interpretation

of their data that is more parsimonious than their own. Second, new experimental results are described that are inconsistent with their conclusions, but that can readily be accommodated within more traditional frameworks. Indeed, these new results are compatible with all theories but the R&M (1997) model. Nevertheless, I conclude by defending a key premise of the R&M (1997) model. That is, their assumption that theories of word priming should be embedded within theories of word perception. On the present view, however, priming reflecting one-trial learning within these systems that can lead to a change in sensitivity (rather than pure bias) .

Before describing the main findings and model of R&M (1997), a brief review of some key priming results and the theories that have been proposed to account for these findings is included. When considered in the context of past theories, it is clear that the R&M (1997) model represents a radical departure from past proposals.

#### Brief summary of basic priming results and their standard interpretations

As noted above, long-term priming has been the focus of a great deal of research, and detailed summaries of this literature can be found in several review articles (Richardson-Klavehn & Bjork, 1988; Roediger & McDermott, 1993; Schacter, 1987). For present purposes, three well established findings that strongly constrain theories are mentioned. The first, and perhaps the most striking result, is that priming is preserved in densely amnesic patients who often perform at or close to chance levels on recall and recognition tasks (e.g., McAndrews, Glisky, & Schacter, 1987; Squire, Shimamura, & Graf, 1985; Warrington & Weiskrantz, 1974). This finding, more than any other, has focused attention on the relation between memory and consciousness, and led to the common view that priming is a form of unaware or “implicit” memory that is mediated by a qualitatively different system than the episodic memory system that supports recall and recognition (e.g., Schacter & Tulving, 1994). Second, priming is not generally facilitated by levels-of-processing manipulations (e.g., Graf Mandler, 1984; Jacoby & Dallas, 1981), whereas recognition and recall memory is very sensitive to these procedures ( Craik & Tulving, 1975). Third, changes in the modality of the study-test items reduces or eliminates priming effects (e.g., Jackson & Morton, 1984; Jacoby & Dallas, 1981), whereas these manipulations have little effect on recall or recognition (e.g., Roediger & Weldon, 1987). These latter two findings have led to the view that priming is largely mediated by perceptual memory representations and processes -- at least when priming is assessed in tests that stress the perceptual processing of single words, so-called data driven priming tasks (Roediger, Weldon & Challis, 1989; but see Masson & Freedman, 1990).<sup>1</sup>

The nature of these perceptual representations and processes is a point of contention, however. According to one prominent view, priming reflects a general principle of memory: that is, the principle of transfer-appropriate processing according to which memory benefits to the extent that the cognitive operations used at test overlap those at study. On this account, priming tends to be sensitive to perceptual manipulations, such as study-test modality shifts, because the priming tests themselves tend to emphasize perceptual processing. So for example, three of the most common priming tests -- the perceptual identification, stem-completion and fragment-completion tasks -- all involve presenting words under degraded perceptual conditions, and the main challenge for participants is to use perceptual knowledge to respond. Accordingly, the perceptual encoding of the study items is critical in supporting

memory in these tests, with priming observed to the extent that the perceptual processes overlap. As Roediger and his colleagues note, priming tasks can be devised that emphasize the conceptual encoding of the test materials, and in these cases, priming is sensitive to levels-of processing manipulations (e.g., Srinivas & Roediger, 1990) and is insensitive to various manipulations of the perceptual attributes of the study test items, such as study/test modality shifts (Blaxton, 1989; Challis & Sidhu, 1993; Srinivas & Roediger, 1990). Based on these considerations, Roediger and colleagues (e.g., Roediger, 1990; Roediger et al., 1992, Roediger & McDermott, 1993) argue that various forms of memory, including priming, recognition and recall, are mediated by transfer-appropriate-processes embedded within a single memory system (For a similar view, see Jacoby 1983.)

On another prominent view, priming reflects the acquisition of new representations within a system that encodes the perceptual attributes of words and objects, but not the semantic or functional properties of these items -- the so-called perceptual representation system (PRS). According to this approach, priming is obtained to the extent that the critical perceptual features of test stimuli match the perceptual features of the study stimuli (Schacter, 1990, 1992; Tulving & Schacter, 1990). A central claim of this view is that recall and recognition generally rely on a memory system other than the PRS, and that the PRS is itself involved in supporting word and object identification. Within the PRS system, the processes that mediate priming (and word and object identification) are not well specified, but Schacter (1990) suggests that these processes may operate according to principles of transfer-appropriate-processing (for related accounts, see Moscovitch & Umiltà, 1990, 1991; Squire, 1987).

Despite differences amongst these theories, they share a number of assumptions, two of which are relevant for present discussion. First, priming is thought to be mediated by new memory codes encoded at study. Indeed, one of the central questions that has arisen from this perspective is whether these new memory codes are encoded within a the same system that supports episodic recall and recognition (e.g., Roediger, 1990) or a separate memory system (e.g., Schacter, 1990), the so-called "unitary" vs. "multiple" memory systems debate. Second, on most of the above accounts, priming is assumed to improve the processing of stimuli repeated at study and test rather than simply reflecting a bias to interpret new information as old (but see Jacoby 1983; Masson & Freedman, 1990). The assumption that priming reflects a facilitation rather than bias is made explicit in various places, for example the statement by Schacter (1994, p 237) that "visual priming may make it easier for the perceptual representation system mechanism involved with visual word form representations to extract visual information from a test cue". As detailed below, both of these assumptions are challenged by Ratcliff & McKoon (1997).

Before describing the R&M (1997) findings, one other standard theory of priming needs to be described that denies both of these implicit assumptions. According to Morton (1979), priming is mediated by pre-existing abstract perceptual representations that support the identification of various information, including written and spoken words, and objects. For present concerns, visual word identification is the most relevant. Briefly, a set of letter detectors are activated to the extent that the visual features of the input match the letter representations, which in turn leads to the

activation of word representations -- so-called logogens -- to the extent that the word codes contain the activated letters. Word recognition is achieved when the activation of a specific logogen unit passes its threshold, and priming is explained as temporarily lowering the threshold of the identified word, so that less activation is required to identify the word when it is later presented at test.

Thus, in contrast with the first assumption, priming reflects the modification of pre-existing perceptual codes (rather than the establishment of new representations). And in contrast with the second assumption, lowering the threshold of a logogen reflects a change in bias rather than sensitivity in target processing. As Morton (1979) explicitly notes, the amount of information available to a logogen is independent of repetition effects (or frequency effects) and is solely determined by the properties of the stimulus (e.g., duration, contrast, signal-to-noise ratio, etc.). The assumption that priming is mediated by pre-existing perceptual codes and reflects a change in bias rather than sensitivity is consistent with the approach of R&M (1997).

#### The Key Ratcliff & McKoon findings.

M&R (1997) report three novel results that, when taken together, appear inconsistent with the above accounts of priming. The first finding has already been mentioned; that is, a pattern of priming in a perceptual identification task suggestive of a bias to perceive test words as study words. This finding was most clearly shown in a task that incorporated a forced choice procedure. As described earlier, consider the example in which a participant studies table, and at test, cable is flashed, immediately followed by two alternatives table and cable. The task of the participant is to select the alternative that matches the flashed word, in this case cable. A key finding reported by R&M (1997) is that the participant shows a tendency to incorrectly select table (consistent with the study word), and this tendency is of the same magnitude as the benefit obtained for words that are repeated at study and test (i.e., when table is flashed). The equal mixture of costs and benefits was taken by the authors to indicate that the sensitivity in processing repeated materials is unaffected, and there is simply a bias to interpret all targets related to study items as the prior study items (Also see Ratcliff, McKoon, & Verwoerd 1989).<sup>2</sup>

In a related procedure, R&M (1997) assessed priming in a forced choice task by comparing performance in a condition in which both alternatives were previously studied to a condition in which neither was studied. So for example, participants might study table and cable, and at test, table is flashed followed by the alternatives table and cable. The critical finding was that no facilitation was obtained in this condition compared to a condition in which neither table nor cable was studied. This is exactly the findings one should expect on a bias interpretation of priming, given that there would be competing biases to perceive the target as table and cable in the repeated condition, eliminating any basis for priming. By contrast, if priming reflects an improved sensitivity to perceive repeated targets, participants should be better able to select the target given that the perception of the target (as well as the alternative) should be improved. Thus, the failure to obtain priming in this task was taken as strong support for a bias theory of priming.

As will become clear shortly, it is important to distinguish between the results obtained with these two forced choice procedures. Accordingly, in the remainder to the article, the term standard forced choice procedure will be used to describe the

situation in which only one (or neither) of the test alternatives was previously studied, and the modified forced choice procedure to describe the situation in which both (or neither) of the alternatives was studied. The term standard is used to describe the former procedure because R&M have repeatedly used this version of the forced choice task in various papers, whereas the latter procedure has only been used in a single study (Ratcliff & McKoon, 1997, Experiment 2).

The second key result obtained by the authors is that priming in the standard forced choice procedure is relatively unconstrained by the stimulus information of the test word that comes into the perceptual processing system. When the authors reduced the flash times of the test words to the point where performance was near chance (items were flashed for 10 ms), the amount of bias did not decrease, but in fact was close to its largest value. Again, this finding was taken to be incompatible with many standard accounts of priming.

The third important finding, and the most striking, is that priming in the standard forced choice task was limited to cases in which the alternatives were orthographically similar to one another. So for example, when the test word died was flashed followed by the alternatives died-~~lied~~, benefits were obtained when died was studied, and costs when lied was studied. However, when died was flashed followed by the alternatives died-~~sofa~~, no benefit was obtained when died was previously studied, and no costs when sofa was studied. This pattern of result was obtained for visually displayed words (R&M, 1989, 1997) and for spoken words (Ratcliff, Allbritton, & McKoon, 1997).

This last finding is particularly crucial because it was taken to be inconsistent with all previous accounts of priming, including Morton's (1979) theory which can be reconciled with the other findings. The problem with past theories, according to R&M, is that priming was assumed to be mediated by a representation of the study word itself (either newly acquired representations that facilitates the processing repeated words or a modification of an old trace that leads to a bias). However, as long as priming is attributed to a property of a study word representation itself, then processing the same word at test should show facilitation relative to processing of all other words, regardless of the nature of the alternatives. So in Morton's (1979) model, for example, studying died should lead to a facilitation in identifying died when the alternatives are died-~~lied~~ or died-~~sofa~~, because priming is mediated by the lowered threshold of the died logogen. It was in response to this last finding that the authors adopted a radically new interpretation of priming phenomena, as implemented in the counter model of priming, described below.

#### Accounting for the Data in Ratcliff & McKoon's Counter Model

The counter-model of R&M (1997) assumes that visual word priming is best understood in the context of a model of visual word identification, consistent with Morton (1979). Like the logogen model, the counter model assumes one decision counter for each word in the lexicon, and word-identification is achieved when counts in a counter pass some threshold (in the counter model, a threshold is reached when the total number of counts in one of the counters exceeds the maximum of the others by some number). Counters are organized by similarity such that the counters of similar words are close together in a cohort whereas counters of dissimilar words are far apart, consistent with various findings in the word perception literature (e.g.,

Andrews, 1989, Coltheart, Davelaar, Jonasson, & Besner, 1977). Thus, the presentation of died results in some counts being assigned to other items in its cohort, such as lied, especially under conditions in which died is flashed quickly so that the perceptual information is degraded.

In this model, counts are accumulated at a constant rate such that, for each unit of time, one count is accumulated to one (and only one) counter. When a target word in an identification task is presented long enough, most of the counts are determined by the stimulus, but when the flash time is short, counts are also determined by individual features of the stimulus, or by random noise alone. Counts based on a feature can be assigned to any counter that contains this feature (e.g., if the feature “i” is identified, the counter for died, lied, etc. could be incremented by 1), and counts based on noise can be assigned to any counter (e.g., sofa). Accumulation of the counts into counters continues until the total number of counts in one of the counters exceeds the maximum of the other by some criterion.

Priming in this model is explained by assuming that counters can act as attractors. That is, exposure to a word at study causes the word's counter to attract a few counts more than it otherwise would, stealing them away from counters of other similar words. This attraction leads to a benefit in identifying repeated words since the counter “steals” counts that might otherwise have been mistakenly assigned to related words, and leads to costs when the study and test words are different, since the counter of the study word now steals counts away from the counter of the flashed word. It is assumed that this attractive force is quite weak, and only extends to words in its cohort. So for example, the prior study of died could result in extra counts being stolen from lied, but not sofa.

With only a few additional assumptions, this model can account for the priming results that R&M (1997) reported in the naming, yes-no, and forced choice tasks. For present purposes, only the forced choice task is considered. In this task, it is assumed that counts are only accumulated in the two alternative counters as soon as the alternatives are identified (before the alternatives are identified, counts can be assigned to any of multiple counters, based on random noise or the counter sharing a feature with the target). When the test word is clearly seen or some feature is identified that is consistent with only one of the alternatives, then counts are assigned to the proper counter, and no effect of the prior study episode is obtained. However, under more degraded conditions in which diagnostic information is not obtained, then the bias effects emerge that support priming. The bias has two sources. First, there is a tendency for a count to be assigned to the counter of the study word when the information identified from the target is compatible with both items; so called non-diagnostic counts. For example, if the letter “i” is identified in the target lied, there will be a bias to assign counts to the counter of the studied word when the alternatives are similar, such as died-lied (in the absence of a prior study episode, counts in this situation would be assigned to each of alternatives 50% of the time). Second, if only noise is identified from the target, there again will be a bias to assign the counts to the counter of the studied word when the alternatives are similar; so-called null counts. In this way, the counter of the study word tends to pass threshold, and participants select the study word as the target.



In this way, all of the above findings that R&M (1997) reported in the forced choice task can be explained: (a) Priming is the product of bias given that the only consequence of processing a word at study is a tendency to accumulate counts in the counter of this word at test. This results in benefits and costs, with a net change in sensitivity of zero; (b) A pattern of bias continues even when test words are flashed so quickly that performance is close to chance because null and non-diagnostic counts are most frequent when test words are flashed quickly, and these counts tend to be accumulated in the counter of the word that was studied; (c) Priming is restricted to conditions in which the alternatives are similar, because as noted above, the attractive force that mediates priming is assumed to be weak, and only extends between counters in similar cohorts. So when the alternatives are died-sofa, for example, the attractive force of died (if died was studied) does not extend to sofa, and therefore cannot steal null and non-diagnostic counts away from sofa to support priming. So in this case, the null and non-diagnostic counts are assigned to the two counters 50% of the time, and any improvement above chance in identifying targets is attributed to the diagnostic counts (that do not support priming).

As noted by R&M themselves, this characterization of priming is difficult to conceptualize, in part, because it denies the most basic assumption of all past models; that is, that priming reflects something about a memory representation itself that is encoded (or modified) at study. Here, by contrast, priming reflects the interaction between counters within a cohort, and it is not possible to consider a specific word primed outside the context of its cohort. Thus, this theory is a radical departure from all previous accounts of priming phenomena.

#### Alternative Explanation for the R&M (1997) findings

There is a much more straightforward interpretation of the R&M (1997) findings, however, that is compatible with the assumptions that (i) priming is due, at least in part, to an improvement in identifying repeated words (as opposed to only a bias), and (ii) priming is mediated by a representation of the study word itself. This alternative interpretation is outlined below, followed by a set of experiments that provide evidence consistent with these general assumptions and inconsistent with the R&M (1997) interpretation.

The first thing to note is that the pattern of results taken to support a pure bias theory of priming was largely obtained in tasks that employed the standard forced-choice procedure. This is problematic, because this conclusion relies on an untested assumption regarding the locus of priming in this task. That is, the assumption that bias is due to the null and nondiagnostic counts accumulating in the counter of the study word during the processing of the test word. Put another way, it is assumed that the processing the test word is affected by the prior study episode, and the role of the alternatives is only to limit counts to the counters of the two alternative words. However, there is another straightforward interpretation of performance on this task. That is, the alternatives themselves are primed. So studying died tends to increase the likelihood that the participant selects died from the alternatives, because died itself has been primed. In this way, the pattern of results that R&M interpret as bias in identifying the test word would be obtained, but the results would be the product of another source, that is, a preference attached to the repeated alternative. Note, prior studies have obtained priming for stimuli presented in a forced choice procedure at test

(e.g., Dorfman, 1994; Mandler, Nakamura, & Van Zandt, 1987; Zajonc, 1980), and similar priming may occur here as well. If indeed repetition priming for the alternatives affects performance in this task, the procedure cannot be used to address the bias vs. sensitivity debate.

The modified forced choice task, however, is not subject to these same concerns. In this task, both of the test alternatives are studied in the repeated condition, and accordingly, there is no reason to expect that participants would preferentially respond to one alternative over another based on any priming for the alternatives. Furthermore, it is difficult to see how any priming for the alternatives could account for the absence of priming for the targets, which is the key finding taken to support a bias theory of priming. Thus, the null priming obtained in this task does seem to provide stronger evidence in support of a bias theory.

Nevertheless, one key aspect of the R&M(1997) study may compromise this conclusion. That is, the authors did not control the frequency of the words, and many of the items were high in frequency. This is relevant, because priming is reduced for high frequency words, and sometimes eliminated (e.g., Bowers, submitted a, b; Forster & Davis, 1984; Rajaram & Roediger, 1993). So it is possible that priming for high-frequency words is largely mediated by bias, but priming for low-frequency words is mediated, in part, by an increased sensitivity. Indeed, this would make functional sense, as there is more room for improvement in perceiving low- compared to high-frequency words (as demonstrated, among other things, by the faster reading time for high- compared to low-frequency words). Accordingly, the current forced-choice priming results do not provide strong evidence in support of a pure bias theory of priming. And given that the relative role of bias and sensitivity cannot be determined using other standard priming procedures, it is difficult to conclude that priming is all bias.

The above considerations are directly relevant to the second key finding reported by R&M, namely, the robust priming obtained for very briefly presented test words in the standard forced choice procedure. According to R&M (1997), this finding is incompatible with many traditional theories, but in fact, this finding can easily be reconciled with past accounts as long as the priming can be attributed, in part, to the alternatives. That is, the priming under these conditions persist because the alternatives themselves are presented in clear view. So the results may tell us nothing about priming when test words are briefly displayed, and instead reflect a tendency of the participants to select the clearly seen repeated alternatives. Indeed, as the authors note themselves, the pattern of bias was most pronounced when the flash duration was the briefest. This might be expected on the present view, because the alternatives should play a larger role than the targets in just these conditions.

Lastly, consider the finding that priming was not obtained in the standard forced choice task when the alternatives were dissimilar. According to M&R (1997), this finding is inconsistent with all past accounts, and requires a radically different approach to understanding priming phenomena. In fact, this finding was the basis of rejecting Morton's model and developing their counter model.

The problem here is that R&M's conclusion that word priming is lost when the alternatives are dissimilar is based on a set of results that are less than compelling. Consider the following. In R&M (1997), the authors assessed priming with dissimilar

alternatives in two experiments (Experiments 1 & 4). In Experiment 1, they failed to obtain priming in two conditions, one in which the baseline performance was very high (.87) suggesting that a ceiling effect may have reduced any priming effects. In Experiment 4, priming was assessed in three different conditions that varied the target duration, and after collapsing across these conditions, they again failed to obtain priming when overall performance was not close to ceiling (.70). However, the authors did not publish the results at each prime duration. When the results at each duration are considered separately, it is clear that ceiling effects may again have reduced priming effects, as can be seen in Table 1.<sup>3</sup> Indeed, at the two shortest prime durations (where ceiling effects were not a problem), similar priming in the repetition condition was obtained when the alternatives were similar (.06) and dissimilar (.05). It is only at the longest prime duration where robust priming was obtained in the similar (.06), and not in the dissimilar (.01) condition. But as can be seen from the table, ceiling effects may have selectively eliminated priming at the long prime duration when the alternatives were dissimilar (base rate = .88). In addition, Ratcliff et al. (1989, Experiment 5) found a trend for repetition priming in a dissimilar context condition (.06) that approached the repetition effects obtained in a similar context condition (range of .09 to .12 in Experiments 2-4) even though the base rate was slightly higher in the dissimilar (.83) compared to the similar (average of .81) context condition. Thus, the results the authors present in support of their conclusion are not compelling -- only one experiment showed no tendency for repetition priming when ceiling effects were avoided, and other studies showed a tendency for facilitation when ceiling effects were not an issue. Given the inconclusive nature of these results, this issue warrants further investigation.

To summarize, the three main results that R&M (1997) claim are incompatible with all previous accounts of priming are in fact easily accommodated with traditional accounts. The problem is that the authors have not demonstrated that priming is the product of pure bias, they have not demonstrated that priming occurs for targets under conditions in which very little perceptual information from the target is registered, nor have they convincingly shown that priming in the forced choice task is restricted to conditions in which the alternatives are similar. The experiments reported below attempt to clarify the nature of the processes that mediate performance in the standard and modified forced choice tasks in order to contrast the R&M (1997) model with more traditional accounts.

### Experiments 1-3

As discussed above, R&M's failure to obtain priming in the standard forced choice procedure with dissimilar alternatives may have been the product of ceiling effects rather than a fundamental restriction on priming. Accordingly, a series of three experiments was carried out that varied prime duration in order to assess performance at various baseline levels, ranging from near ceiling to near chance. Both similar and dissimilar alternatives were included in all experiments, in order to directly compare the size of priming in these two conditions.

#### Method

Participants. Twenty-four participants were tested in Experiment 1 and 32 were tested in Experiments 2-3, for a total of 88. Participants were recruited from

Rice University in Experiments 1 & 3, and from the University of Houston in Experiment 2. All students participated in return for course credit.

Design and Materials. The design and materials were the same for each experiment. A set of 96 critical word triplets were constructed, with each word of a triple having the same number of letters, always between 4 and 7. Two of the words of a triple had the same visual shape, and the third was as different in shape as possible, where shape refers to the outline of the letters. Most of the triples (80) were taken from R&M (1997; Experiment 1), and an additional 16 were created to increase the item set to 96. As in R&M (1997), word frequency was not controlled.

Each experiment included two within-subject factors: study-test condition with three levels (repeated target: study and target words were the same, vs. repeated alternate: study words were the same as non-target alternative in the forced choice task, vs. baseline: no alternative was studied) and test context with two levels (similar alternatives vs. dissimilar alternatives). One of the similar words in each triplet was randomly selected to be the test word (e.g., died), and in the study phase, either the test word (died), one of the two alternate words (lied or sofa), or nothing was presented, depending on the condition. After the test word was flashed, the two alternatives were presented, the test word (died) along with either the similar alternate (lied) or the dissimilar alternate (sofa). Half of the test words were presented in the baseline condition, and the remaining half were divided into the repeated target and repeated alternate conditions. Eight files were created so that each test word was presented in all conditions equally often, creating counter-balanced designs.

The critical words were divided into four study-test blocks, and within each block, 12 words were studied, followed by 24 test trials. This procedure was similar to R&M (1997) who included three study-test blocks. The study-test blocks occurred in succession.

Procedure The experiments employed a similar procedure to the forced-choice word identification experiments of R&M (1997). Study words were presented one at a time for 1 second, and participants were simply asked to read the words. At the end of each study list, a message was presented indicating that the identification task was to begin, and at the end of each test phase, a message was presented that a list of words to read was about to begin (or that the experiment was over). During each test trial, the trial number (e.g., 1 for the first test item, 2 for the second, etc.) was presented for approximately 400 ms, followed by a blank screen for approximately 300 ms, followed by a row of minus signs for approximately 400 ms that served as a warning signal, followed by a blank screen for approximately 300 ms, followed by the target word, then the mask consisting in a row of "@ @ @ @ @" signs for approximately 300 ms replaced the targets, followed by the two alternative words displayed side by side. Target words were flashed for 9 ms, 22 ms, and 25 ms in Experiments 1-3, respectively. Participants were instructed to identify the targets by pressing the right shift key if the alternative on the right corresponded to the target, and the left shift key if the alternative on the left corresponded to the target. Participants pressed the space bar to advance to the next test trial, whereas the study words were presented automatically every second. Study items were presented in a different random order to each participant, whereas test words were presented in the same random order for all participants.

Prior to the sequence of study and test blocks in each experiment, participants completed a practice set of 24 test trials in order to familiarize them with the test procedure. All practice items were different than the critical materials, and the same procedure was followed in the practice and critical test trials. All experiments were run using the DMASTER display software developed at the University of Arizona by Kenneth Forster and Jonathan Forster that synchronizes the timing of the display with the video raster.<sup>4</sup> Standard IBM text font was used.

### Results and Discussion

The results of Experiments 1-3 are presented in Table 2. As can be seen in the table, a similar pattern of benefits and costs was found in the similar and dissimilar context conditions, and this general pattern was across all prime durations. However, the benefits were reduced in Experiment 3 when the alternatives were dissimilar -- which is the one condition in which baseline performance approached ceiling (.88).

Separate ANOVAs were carried out on the three experiments. In Experiment 1, where targets were flashed for 9 ms, a main effect of test-context approached significance,  $F(1,16) = 3.28$ ,  $MSe = 185$ ,  $p = .08$ , reflecting a tendency for participants to correctly identify words more often when the alternatives were dissimilar (.52) compared to the similar (.48). A main effect of study-test condition was observed,  $F(2,32) = 7.50$ ,  $MSe = 231$ ,  $p < .01$ , reflecting the overall facilitation in the repeated target (.07) and overall inhibition in the repeated alternate (-.05) condition. The interaction between test-context and study-test condition did not approach significance,  $F(2,23) < 1$ , indicating that the same pattern of benefits and costs were obtained in both context conditions.

In Experiment 2, where targets were flashed for 22 ms, the ANOVA revealed a main effect of test-context,  $F(1,24) = 11.20$ ,  $MSe = 52$ ,  $p < .01$ , reflecting the higher proportion of words correctly identified when the alternatives were dissimilar (.63) compared to the similar (.59). A main effect of study-test condition was also observed,  $F(2,48) = 25.01$ ,  $MSe = 371$ ,  $p < .01$ , reflecting the overall facilitation in the repeated target (.12) and overall inhibition in the repeated alternate (-.12) condition. Once again, the interaction between test-context and study-test condition did not approach significance,  $F(2,48) < 1$ , indicating that the same pattern of benefits and costs were obtained in both context conditions.

In Experiment 3, where the targets were flashed for 25 ms, the ANOVA revealed a main effect of test-context,  $F(1,24) = 70.70$ ,  $MSe = 104$ ,  $p < .01$ , reflecting the higher proportion of words identified when alternatives were very dissimilar (.86) compared to similar (.73). A main effect of study-test condition was also significant,  $F(2,48) = 17.68$ ,  $MSe = 151$ ,  $p < .01$ , reflecting the overall facilitation in the repeated target (.07) and overall inhibition in the repeated alternate (-.06) condition. In contrast with the prior two experiments, however, the interaction between test-context and study-test condition was significant,  $F(2,48) < 4.48$ ,  $MSe = 69$ ,  $p < .05$ , reflecting the larger benefits and costs obtained with the similar alternatives. As noted above, a ceiling effect in the dissimilar context condition contributed to this interaction.

In sum, these experiments clearly show that a benefits and costs are obtained in the similar and dissimilar context conditions when ceiling effects are avoided. These findings contradict R&M's claim that priming is restricted to similar context condition, undermining the main motivation for developing their counter model.

#### Experiment 4

Although the above findings are inconsistent with the counter model, the results might appear to support R&M's bias theory of priming given that benefits and costs were approximately equal in all three experiments. As noted above, however, the standard forced choice procedure would provide an inappropriate test of the bias-sensitivity distinction if priming occurs for the alternatives (in addition to the test words).

In order to assess the role of the alternatives in mediating priming, the same set of study and alternative words were included in Experiment 4, but the test words were never flashed. Instead, a different set of random set of symbols \$\* & # @ were flashed briefly on each trial for 9 ms, with the number of symbols equally the number of words in the alternatives. If the same pattern of results is obtained in this experiment as above, as revealed by a tendency to select the alternative that is the same as the study word, then the results would suggest that the alternatives themselves play a key role in priming. Such an outcome would compromise the key result used in support of the bias theory of priming.

#### Method

Participants A group of 24 students at Rice University participated in return for course credit.

Materials and Design. The same as the previous experiments, except that random sequence of symbols (e.g., % ^ \* # @) were flashed instead of the targets.

Procedure. A similar procedure was followed. As in the previous experiments, participants attempted to identify the test words and pressed the right shift key if the alternative on the right corresponded to the test word, and the left shift key if the alternative on the left corresponded to the target. It was stressed to the students that the test words were flashed extremely quickly, and that they should not get discouraged if they were only guessing (which of course they were). The nature of the test words was not discussed.

#### Results and Discussion

The data are presented as if the words were presented, in order directly compare performance to Experiments 1-3. As can be seen in Table 3, the same pattern of results were obtained, despite the fact that no test words were flashed. A overall ANOVA revealed a main effect of study-test condition,  $F(2,32) = 13.36$ ,  $MSe = 357$ ,  $p < .01$ , reflecting the overall facilitation in the repeated target (.13) and overall inhibition in the repeated alternate (-.07) condition. By contrast, the main effect of test-context was not significant,  $F(1,16) = 1.3$ ,  $MSe = 197$ ,  $p > .25$ , nor was the interaction between test-context and study-test condition,  $F(2,32) < 1$ . Of course, this is not surprising given that no test words were presented.

Thus, it appears that the alternatives themselves contribute the pattern of results obtained in the forced choice procedure. Given this finding, it is inappropriate to use these findings in support of a bias theory of priming.

#### Experiment 5

One concern in carrying out these experiments is that students might adopt explicit memory strategies and select the alternatives that they recognize from the study list. Indeed, when McKoon & Ratcliff (1996) encouraged participants to use

their episodic memory to facilitate their performance on the priming task, they observed the same pattern of priming in the similar and dissimilar context conditions, with equal benefits and costs (ceiling effects were not an issue in this experiment). Thus, it is important to minimize any episodic influences on this task, since otherwise, priming effects may emerge in the dissimilar condition because of episodic contamination rather than because ceiling effects were removed.

It should be emphasized that Experiments 1-3 followed the same general procedure as R&M (1997), so there is no reason to assume that episodic memory strategies were more of a problem in the present compared to the R&M experiments. In fact, Experiment 2 was carried out at University of Houston in an attempt to reduce episodic contamination: There are currently no faculty members in the department of psychology studying priming, and accordingly, these students were less likely to be knowledgeable about priming research. Furthermore, Experiment 3 was carried out on psychology 101 students during the first 2 weeks of the semester, again minimizing the likelihood of explicit contamination. Still, it is always possible that participants adopted explicit strategies, especially in Experiments 1&4 where the test words were flashed very quickly (or not at all), leaving students with little basis for responding.

In an attempt to reduce any episodic contamination on performance, Experiment 5 included a deadline procedure that required participants to respond quickly. Under these conditions, episodic memory influences are minimized (R&M, 1995).

Procedure. A similar procedure was used as above, except a dead-line to respond was included. A set of exclamation marks (!!!!!!!) replaced the alternatives 750 ms after their onset, and participants were asked to respond before the exclamation marks appeared. Targets were flashed for 22 ms.

#### Results and Discussion.

As can be seen in Table 4, the pattern of priming obtained with the deadline procedure is similar to the results obtained in prior studies, with priming extending to the similar and dissimilar context conditions. The ANOVA revealed a main effect of test-context,  $F(1, 40) = 66.47$ ,  $MSe = 196$ ,  $p < .01$ , reflecting the higher proportion of words correctly identified when the alternatives were dissimilar (.77) compared to similar (.63). A main effect of study-test condition was also observed,  $F(2,80) = 4.11$ ,  $MSe = 154$ ,  $p < .05$ , reflecting the overall facilitation in the repeated target (.04) condition, and a tendency for inhibition in the repeated alternate (-.02) condition. The interaction between test-context and study-test condition did not achieve significance,  $F(2,80) = 1.93$ ,  $MSe = 14$ ,  $p > .15$ , although there was a trend for inhibition for the repeated alternates when the test alternatives were similar (-.04) and facilitation for the repeated alternates when the test alternatives were dissimilar (.02). The basis of this difference is unclear.

Although the present results are similar to the above studies, it is clear that the dead-line procedure reduced the overall size of the priming effects. One possible interpretation of this finding is that prior results were partly contaminated by explicit memory strategies. Alternatively, the priming itself may have been reduced by this procedure, especially if priming partly reflects a preference for the repeated alternate (as preferences might take some time to develop). Or more likely, a combination of these factors may have contributed the reduced priming effects. Whatever the case,

the key finding is that there is no evidence that priming is restricted to the similar context condition, which formed the basis of their counter-model of priming.

#### Experiments 6

Based on the findings of Experiment 5, it is difficult to argue that priming in the dissimilar context condition is the product of episodic memory strategies. But for the sake of argument, consider the hypothetical situation in which dissimilar context priming is attributable to episodic strategies, and that the only non-episodic priming is obtained in the similar context condition -- as R&M (1997) contend. Would this finding provide a strong basis for rejecting prior theories of priming and motivate the need for developing the counter model? Again, I would suggest not, because there is a more parsimonious interpretation of such an outcome.

Consider the following. When the test alternatives are orthographically unrelated (e.g., died/sofa), participants might perform the task by simply noticing a discrepancy between the flashed target and one of the alternatives, and select the alternative without the discrepancy. So for example, if died is flashed, followed by died-sofa, the identification of an ascender in the first position of died would be sufficient to rule out the alternative sofa, leading to the correct response. On this account, then, the participant would perform the task at a sub-lexical (perhaps even sub-letter) level, which would eliminate priming if priming occurs at the word level, as is assumed by Morton (1979) for instance. Furthermore, given that a visual discrepancy is much more likely to be perceived when the alternatives are dissimilar (where there are many visual discrepancies between the target and the incorrect alternative), such an approach would tend to selectively eliminate priming in the dissimilar context condition. Thus, the absence of priming in the dissimilar context condition -- the finding that R&M (1997) claim is incompatible with all prior accounts -- might easily be reconciled with standard views of word priming.<sup>5</sup>

Experiment 6 was carried out to demonstrate the plausibility of this alternative interpretation. Priming in this last experiment was assessed in the lexical decision task, and the key manipulation was the nature of the nonword distracters that were presented at test. In one condition, test words were mixed with a set of pronounceable nonwords that were one letter different from real words (e.g., lecency similar to decency), and accordingly, participants needed to process words to a lexical level in order to distinguish the words from the nonwords. In the other condition, words were intermixed with random letter strings (e.g., kkelckd) so that the lexical decisions could be made at a sub-lexical level. On the present hypothesis, priming should be reduced in the latter case, because participants may not fully access the orthographic word codes when performing the task, and priming is presumed to occur at the word level.

#### Method

Participants. Thirty-six students from Rice University participated in return for course credit.

Design and Materials. The experiment included nonword distracter type as a between subject factor (pronounceable nonwords vs. random letter strings). The critical materials included a set of 40 medium frequency words selected from the Kucera & Francis (1967) norms (ranging in frequency from 45 to 55 occurrences per million, with mean frequency of 50), ranging between 5-7 letters in length. An equal



number of pronounceable nonwords and random letter strings matched in length with the words were also constructed.

During the study phase, participants silently read a set of 20 words, and at test all 40 of the critical words were presented. A set of 6 words and a corresponding number of pronounceable or illegal nonwords were also included at test to provide practice lexical decisions prior to the presentation of the critical items. Two test forms were included so that each word was presented in the repeated and non-repeated conditions, producing counterbalanced designs.

Procedure In the study phase, words were presented one at a time for 1.5 s on the computer monitor, and participants were instructed to silently read each word and press the space bar to advance to the next item. Immediately after completing the study phase, participants completed the lexical decision task. Participants were instructed to press the right shift key as quickly as possible if the letter string was a word and the left shift key otherwise. They were encouraged to respond as quickly as possible, without making too many mistakes.

### Results and Discussion

The reaction time (RT) and error rates in the various conditions are presented in Table 5. For RTs in the pronounceable nonword condition, an ANOVA showed robust priming (27 ms),  $F(1,16) = 9.68$ ,  $MSe = 674$ ,  $p < .01$ , whereas no RT priming was obtained in the random letter string condition (8 ms),  $F(1,16) = 1.39$ ,  $p > .25$ . The error scores paralleled the RT results in the pronounceable nonword condition (2.78% priming), although this effect did not approach significance,  $F(1,16) = 1.44$ ,  $MSe = 30.9$ ,  $p > .2$ . Similarly, there was no significant difference in error rates in the random letter string condition,  $F(1,16) = 1.56$ ,  $MSe = 44.5$ ,  $p > .2$ . Still, it is interesting to note that the error pattern was in the opposite direction to the RT data (priming effect -2.20%), suggesting that the small tendency for RT priming in the random letter string condition was the product of a small speed-accuracy trade off.

These findings suggest that long-term priming is reduced in conditions in which participants encode the target words at a sub-lexical level. Thus, even if one was to accept the R&M (1997) findings -- which the above findings strongly challenge -- there is still an alternative interpretation of the results that would not require the rejection of all prior theories. That is, the results could simply be the product of participants relying heavily on sub-lexical information in order to inform their decisions in the forced choice task, as was found in the lexical decision experiment with random letter foils. Note, a similar reduction of word priming in the masked priming paradigm was also reported by Forster (1992) when random letter string were included as distracters in a lexical decision experiment, showing the generality of this phenomenon.

### Experiments 7-8

The above findings pose a serious challenge to the R&M counter model of word priming. Perhaps most problematic is the robust priming repeatedly obtained in the standard forced choice procedure when the alternatives were dissimilar, contrary to the model's central prediction. But another key problem is that R&M have largely relied on the standard forced choice procedure in order to support their more general claim that priming is the product of a pure bias, and as demonstrated in Experiment 4,

this task may be unsuitable to address this issue. Thus, this latter conclusion is suspect as well.

One finding that does appear to support a bias view of priming, however, is the R&M (1997) failure to obtain priming using the modified forced choice procedure, a task that is not subject to same problems as the standard procedure. The problem with this finding, as mentioned above, is that the authors included many high-frequency words in the material set, and these words often show reduced or no priming. Thus, it is quite possible that R&M would have arrived at a different conclusion if they had assessed priming for low-frequency words in this procedure. In which case, the claim that priming is all bias would not be justified.

In order to assess the role of frequency in modulating priming in the modified forced choice task, two experiments were carried out. In Experiment 7, the same set of words used in the earlier studies was employed, which included many high-frequency words (the list was largely the same as the R&M experiment, with a few extra items), whereas in Experiment 8, a set of low-frequency words was included. As before, the experiments included a condition in which the alternatives were similar and dissimilar.

#### Method

Participants. Forty-eight students from Rice University participated in Experiments 7-8 in return for course credit, 24 in each study.

Design and Materials. The design was the same for both experiments, each with two within-subject factors: study-test condition with two levels (repeated vs. baseline) and test context with two levels (similar alternatives vs. dissimilar alternatives). Experiment 7 included the same list of words as the previous studies, and Experiment 8 included a set of 96 low-frequency word triplets (mean frequency 2.7, with a range of 1-10 occurrences per million; Kucera & Francis, 1967), with each word of a triple having the same number of letters, always between 4 and 5. As with the high-frequency words, two of the words of the triple were orthographically similar, and the third was dissimilar. One of the similar words in each triplet was randomly selected to be the test word, and in the study phase, either both alternatives were presented or neither was presented. Four test forms were constructed for each experiment, so that each test word was presented in the various conditions equally often, producing counter-balanced designs.

Procedure. The procedure was the same as Experiments 1-3. Targets were flashed for 22 ms.

#### Results and Discussion

The results of Experiments 7-8 are presented in Table 6. Consistent with R&M (1997), no priming was obtained in Experiment 7 that included many high-frequency words. This was true when the alternatives were similar, with accuracy rates of 69.28 and 71.01 in the repeated and nonrepeated condition, respectively, as well as when the alternatives were dissimilar, with accuracy rates of 76.01 and 75.87, respectively, with both  $F$  value for priming  $< 1$ . Consistent with prior studies, participants were more accurate when the alternatives were dissimilar compared to similar,  $F(1,20) = 6.66$ ,  $MSe = 121.68$ ,  $p < .05$ . Accordingly, these findings provide some support for a bias theory of priming.

However, in Experiment 8 that included the low-frequency words, robust priming was obtained. More specifically, when the alternatives were similar, the accuracy rates were 69.10 and 62.51 in the repeated and nonrepeated conditions, respectively, and when the alternatives were dissimilar, the accuracy rates were 79.68 and 72.91, respectively. This overall priming was significant,  $F(1,20) = 20.52$ ,  $MSe = 57.17$ ,  $p < .01$ , and this effect did not interact with the alternatives,  $F < 1$ . And again, participants were more accurate when the alternatives were dissimilar compared to similar,  $F(1,20) = 36.62$ ,  $MSe = 72.2$ ,  $p < .01$ . This latter finding provides direct support for the conclusion that priming can reflect a change in sensitivity rather than just bias, at least for low-frequency words. Interestingly, Raaijmakers, Schooler, and Shiffrin (1997) also obtained priming for high-frequency words employing this same procedure, but only after multiple study trials. Thus, following multiple study trials, priming for high-frequency words can also be the product, in part, of a change in sensitivity. This again directly contradicts the R&M counter model of priming, and lends support to the standard assumption that priming often reflects an improvement in processing repeated materials.

Still, bias may play an important role in priming phenomena as well, given that robust priming was obtained in Experiments 1-5 that included a mixture of high and low-frequency words in the stimulus set -- words that did not support priming in Experiment 7. Presumably, bias plays a role in standard priming tasks as well (see Ratcliff & McKoon, 1996; Schacter & Cooper, 1995 for discussion). Even so, the present results show that bias takes a form that is incompatible with counter-model of priming, since bias manifests itself in the forced choice task when the alternatives are similar or dissimilar.

#### General Discussion

R&M (1997) developed a model of visual word priming based on three key findings obtained in the forced-choice identification task: (1) a pattern of benefits and costs suggestive of a bias interpretation of priming, (2) a restriction on priming such that benefits and costs are only obtained when the alternatives in the forced-choice task are similar, and (3) when the alternatives are similar, priming is obtained when test words are flashed so quickly as to produce near chance performance in the baseline condition. According to the authors, this combination of findings is incompatible with all prior theories of priming.

However, the results reported above challenge these findings and conclusions. Experiments 1-3 demonstrate that priming does extend to the similar and dissimilar context conditions in the forced choice priming task as long as ceiling effects are avoided. Indeed, R&M's own results do not strongly support their model when experiments with ceiling problems are set aside. Experiments 4-5 show that the pattern of benefits and costs obtained in the forced choice task has a different source than R&M (1997) considered. That is, the results are mediated, at least in part, by priming for the alternatives (and not just the test word). This finding undermines their argument that priming is all bias, and provides a straightforward explanation as to how robust priming occurs when test words are flashed so quickly as to lead to chance performance. And as shown in Experiment 6, even if all of the above findings are set aside and the results of R&M (1997) are accepted as valid, there is still a plausible and parsimonious explanation for these findings that is consistent with traditional theories

of priming. Finally, the results of Experiment 7-8 provide direct evidence that priming does reflect a change in sensitivity, at least for low-frequency words. Given these considerations, the counter-model should be rejected.

Where do we go from here?

Although the specific model proposed by R&M (1997) is undermined by the present set of results, the authors have adopted a general framework for understanding priming phenomena that deserves serious consideration. That is, the view according to which priming is best understood within the context of a theory of pattern recognition. As such, theories of priming cannot be developed independently from theories of pattern identification. On the present account, however, priming is a by-product of learning that occurs during the normal course of categorizing perceptual inputs, and this learning can improve the perceptual encoding of materials in addition to causing bias.

It is important to contrast this perceptual learning perspective with theories that have been developed within the context of memory research and theory. In the memory framework, there has been little consideration to basic issues of perception, nor much of an attempt to relate existent theories of pattern perception to theories of priming (but see Biederman & Cooper, 1991; Marsolek, 1995; Schacter, 1990). Instead, the primary question has been to relate priming phenomena to other expressions of memory, most notably, recall and recognition. From the perceptual learning perspective, however, a different set research questions immediately arise. For example, in the case of visual word priming, the role of various orthographic variables, including frequency, morphological structure, neighborhood density, etc. become central factors to consider (e.g., Bowers, 1996, submitted a, b; Bowers & Michita, 1998; Cristoffanini, Kirsner, & Milech, 1986; Kirsner, Smith, Lockhart, King, Jain, 1984; Monsell, Matthews, & Miller, 1992; Murrell, & Morton, 1974; Napps, 1989; Wheeldon & Monsell, 1992). More importantly, these theories of priming are now are constrained by a large body of research not previously considered relevant, namely, work on visual word identification and reading (and perception in general).

Consider the following findings, all of which are easily interpretable (and predictable) from a perceptual learning perspective, but which are more surprising from more traditional memory accounts. First, visual word priming tends to be unaffected by study-to-test changes in the visual format of words, such as changes in case, font, etc. when words are presented in a highly legible format that allows the orthographic system to be engaged in the normal way (Carr, Brown, & Charalambous, 1989; Clarke & Morton, 1983; Feustel, Shiffrin, & Salasoo, 1983; Scarborough, Cortese, & Scarborough, 1977; for review of these findings, see Bowers, 1996). Importantly, a pattern of abstract and modality specific priming is obtained even when the within-modality perceptual changes are substantial, for example, the study-test words READ/read (Bowers, 1996). Indeed, priming extends between Hiragana/Kanji scripts in Japanese that are arbitrarily related in their perceptual form, and again, the priming is modality specific (Bowers & Michita, 1998). These results are consistent with an orthographic basis of priming given that a variety of evidence suggests that orthographic knowledge is also coded in a modality specific but abstract format (Besner, Coltheart, & Davelaar, 1984; Bowers, Arguin, & Bub, 1996; Bowers,

Vigliocco, & Haan, in press; Coltheart, 1981; Evett & Humphreys, 1981; McClelland, 1976; Rayner, McConkie, & Zola, 1980).

Second, visual word priming is constrained by morphological structure. For example, equivalent priming tends to be obtained between repeated words (e.g., cars/cars) and morphologically related words (e.g., cars/car), and this priming cannot be attributed to the visual overlap between morphological relatives because little or no priming occurs between form related words (e.g., card/car) under similar test conditions (e.g., Napps, 1989). Nor can morphological priming be attributed to the semantic overlap between items, given a failure to obtain any priming between synonyms or translation equivalents in bilingual speakers (e.g., Durgunoglu, & Roediger, 1987; Roediger & Challis, 1992). Again, this fits naturally with an orthographic account of priming since a variety of non-priming evidence suggests morphological structure is represented within the orthographic system itself (Caramazza, Laudanna, & Romani, 1988; Rapp, 1992). In a similar vein, cross-language priming is obtained for cognates, but not between translation equivalents (Cristoffanini, Kirsner, and Milech, 1986; Kirsner et al., 1984; See Bowers, Mimouni, & Arguin, submitted, for conditions in which cognate priming occurs). As with the morphological priming results, this is a lexical-orthographic (as opposed to a semantic or visual) constraint on priming.

Third, word priming is sensitive to frequency, with more priming obtained for low- compared to high-frequency words. This is exactly what should be expected from an account according to which priming is mediated by orthographic codes that are themselves frequency coded. Note, an apparent problem with this account is that Kirsner and colleagues (Kirsner, Dunn, & Standen, 1989; Kirsner, Milech, & Standen, 1983) have argued that the modality specific component of priming is insensitive to frequency, and that frequency effects are restricted to the cross-modal component. This is problematic for an orthographic account according to which the modality specific visual component of priming (the orthographic component) should be frequency sensitive. However, in a more recent series of studies (Bowers, submitted a; Kirsner, Dunn, Kinoshita, Standen, & Hasslacher, 1993), frequency effects were obtained for both the visually specific and non-specific components, in both the lexical decision and perceptual identification tasks.

These three sets of results, in combination with the new findings reported above, provide a strong set of constraints for future theorizing. On the one hand, the finding that visual word priming abstracts across large changes in visual form (e.g., READ/read) and is sensitive to morphological structure is difficult to reconcile with the view that priming is mediated by new memory codes, and where priming is obtained to the extent that the perceptual features of study and test items overlap (e.g., Roediger, 1990; Schacter, 1990). Instead, these results suggest that priming is mediated by pre-existing orthographic knowledge, with priming mirroring the structure of this knowledge. On the other hand, the finding that priming can reflect a change in sensitivity is incompatible with bias theories of priming (Morton, 1979; R&M, 1997). Accordingly, I would suggest that visual word priming is best understood as a form of learning within the orthographic system that results in an improvement in processing repeated words. To date, however, no well developed theory of this sort has been developed.

### Conclusions

R&M (1997) have described a model of visual word priming in the perceptual identification task that is wrong for a number of reasons. But they have adopted a similar perspective as advocated here; namely, that theories of priming are best embedded within theories of pattern perception. On the present view, however, priming reflects a form of learning within perceptual systems that can improve pattern identification.

A number of important implications follow from this view. First the long-standing debate of whether "multiple memory systems" are needed to account for recall and recognition on the one hand, and priming phenomena on the other, is no longer the central question. Instead, on this view, priming is mediated by perceptual systems that learn, and the key challenge is to characterize these learning principles so as to relate theories of priming to theories of pattern perception.

Second, by characterizing priming as a form of perceptual learning, theories are constrained by a large body of research not previously considered relevant, that is, research in visual and spoken word identification, object identification, sensory-motor control, and other research areas in perception. Indeed, if this view is correct, priming research provides new constraints on theories of perception, providing evidence as to how perceptual systems learn.

The third and last implication to be noted is that students of priming will have to become familiar with theories of perception. On the present view, studying priming as a memory phenomena per se., ignoring the structure of the perceptual systems involved in identifying information, will not get us very far.

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### Footnotes

1. There are various other conditions in which long-priming is obtained, for instance, in experiments that require the re-reading of paragraphs or word combinations (Carr, Brown, & Charalambous, 1989; Levy & Kirsner, 1989; Kolers, 1976) and tests that emphasize an analysis of the conceptual features of test items, such as generating the test item from a category or answering general knowledge questions (Jacoby, Levy & Steinbach, 1992). Although the term "priming" is used in all of these this conditions, there is no reason to presume that the same mechanisms are responsible -- any more than the "priming" observed in the masked or semantic priming paradigms. The present paper is only concerned with single word priming phenomena in tests that emphasize the perceptual analysis of items, as was the case with R&M (1997).
2. The authors reported a similar pattern of bias in a variety of related priming tasks, including the standard perceptual identification task in which participants attempt to name briefly displayed test words (Ratcliff & McKoon, 1989, 1997), object decisions (Ratcliff & McKoon, 1995), picture naming and picture identification with a forced choice procedure (Ratcliff & McKoon, 1996), stem- and modified fragment-completion tasks (Ratcliff & McKoon, 1996), and auditory word identification with the forced choice procedure (Ratcliff, Allbritton, & McKoon, 1997). For example, consider the results from the standard perceptual identification task. Participants show a facilitation in naming test words that were previously studied, but they show a bias to report study words when test words are orthographically related to the study items, i.e., study word = table, test word = cable, response = table. However, given that participants often omit responses in these tasks, it is impossible to experimentally evaluate bias and sensitivity in these tasks.
3. I would like to thank R. Ratcliff for providing these results.
4. The number of lines the raster scans during a refresh cycle can be specified in DMASTER, and accordingly, non-standard refresh time can be employed when displaying materials. For more details, see the DMASTER home page: <http://www.u.arizona.edu/~kforster/dmastr/dmastr.htm>
5. It should be noted that words can be processed at the letter level without automatically accessing the entire word code, as required by the above analysis. For example, Hayman & Jacoby (1989) assessed long-term priming following a study phase in which participants performed a letter-identification task. For one group, the letter target was presented immediately before the study word in a position that indicated where the critical letter might appear in the word (so that the entire word did not need to be encoded to perform the task in principle), and for the other group the target letter was presented after (so that the complete word needed to be encoded). Priming was restricted to the latter condition, suggesting that words were not encoded at the word level in the former condition. Perhaps more relevant, the word superiority effect is lost under conditions that encourage participants to encode words at a sub-lexical level (e.g., Hayman & Jacoby, 1989; Johnston & McClelland, 1974; Thompson & Massaro, 1973). For example, Hayman & Jacoby (1989) assessed the word superiority effect by flashing words and pronounceable nonwords briefly, and then asking participants to select one of two letters that was embedded in the flashed item. The typical word superiority effect -- the higher accuracy in identifying letters

embedded in words compared to pseudowords -- was restricted to conditions in which the letter alternatives were presented after the flashed target. When the alternatives were presented before the target, the lexical status of the item did not affect performance, suggesting that participants completed the task without encoding the words completely.

Table 1

Probability Correct in the Standard Forced Choice Task in Experiment 4 of R&M (1997), as a Function of Study-Test Condition and Test-Context (Probabilities in brackets refer to priming scores).

Flash time (ms)	Study conditions					
	Similar alternatives (died-lied)			Dissimilar Alternatives (died-sofa)		
	Test word studied	Alternate studied	Baseline	Test word studied	Alternate studied	Baseline
10	.585 (.08)	.408 (-.10)	.507	.572 (.03)	.541 (.00)	.540
20	.667 (.03)	.564 (-.07)	.635	.718 (.07)	.661 (-.02)	.646
30	.804 (.06)	.686 (-.06)	.745	.883 (.01)	.883 (.01)	.875
Average	.685 (.06)	.552 (-.08)	.629	.724 (.04)	.695 (-.01)	.687



Table 2

Probability Correct in the Standard Forced Choice Task in Experiments 1-3 as a Function of Study-Test Condition and Test-Context (Probabilities in brackets refer to priming scores).

Flash time (ms)	Study conditions					
	Similar alternatives			Dissimilar Alternatives		
	Test word studied	Alternate studied	Baseline	Test word studied	Alternate studied	Baseline
9	.538 (.06)	.431 (-.04)	.477	.594 (.08)	.462 (-.05)	.514
22	.719 (.13)	.461 (-.13)	.594	.740 (.12)	.516 (-.11)	.624
25	.831 (.11)	.664 (-.06)	.721	.904 (.03)	.813 (-.06)	.871
Average	.696 (.10)	.518 (-.08)	.597	.746 (.07)	.597 (-.07)	.669

Table 3

Probability "Correct" in the Standard Forced Choice Task in Experiment 4 when test words were not flashed.

Alternatives	Study condition		
	Test word studied	Alternate studied	Baseline
Similar	.604 (.11)	.420 (-.07)	.493
Dissimilar	.597 (.15)	.389 (-.06)	.451

Table 4

Probability Correct in the Standard Forced Choice Task in Experiment 5 as a Function of Study-Test Condition and Test-Context (Probabilities in brackets refer to priming scores).

Alternatives	Study condition		
	Test word studied	Alternate studied	Baseline
Similar	.670 (.03)	.601 (-.04)	.638
Dissimilar	.795 (.05)	.769 (.02)	.747

Table 5

Response Latencies (ms) and Errors Rates for Making Lexical Decisions to Words as a Function of Study Condition and Nonword Distracter Type (priming scores in brackets).

Nonword Type	Study condition	
	RT	% Errors
Pronounceable		
Studied	504	4.7
Baseline	531 (27)	7.5 (2.8)
Random Letters		
Studied	471	5.3
Baseline	479 (08)	3.1 (-2.2)

Table 6

Proportion Correct in the Modified Forced Choice Task in Experiments 7-8 as a Function of Study-Test Condition and Test-Context (Probabilities in brackets refer to priming scores).

Experiment #	Test Context	
	Similar Alternatives	Dissimilar Alternatives
Experiment 7		
Repeat	.693 (-.02)	.761 (.00)
Baseline	.710	.759
Experiment 8		
Studied	.691 (.07)	.797 (.07)
Baseline	.625	.729