Prime Validity Affects Masked Repetition and Masked Semantic Priming: Evidence for an Episodic Resource-Retrieval Account of Priming

by

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ABSTRACT

In several experiments, masked repetition priming in the lexical decision task was greater when prime validity, defined as the proportion of repetition versus unrelated primes, was high (.8 vs. .2), even though primes were displayed for only 45 or 60 ms. A similar effect was also found with masked semantic primes. Prime validity effects are not predicted on a lexical entry-opening account of masked priming nor are they consistent with the use of prime validity effects as a marker for the consciously controlled use of primes. Instead, it is argued that episodic traces are formed even for masked primes, are available as a resource that can aid word identification, and are generally more likely to be recruited when their validity is high. However, prime validity effects did not obtain when targets varied markedly from trial to trial in how easy they were to process. Here, it appears that trial-to-trial discrepancies made the lexical decision task more difficult, causing an increase in prime recruitment, at least when prime validity was low. Consistent with this claim, prime validity effects emerged when these trial-to-trial discrepancies were minimized.

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Introduction

The concept of repetition priming is an intuitive one: People are better at responding to something they have seen before or recently than they are at responding to something they have not seen before or as recently. In the case of word recognition, which is of interest here, people are typically faster or more accurate at responding to a word if it is preceded by the same word (a repetition prime) than if it is preceded by a different, unrelated word (e.g., Scarborough, Cortese, & Scarborough, 1977). Despite being such an intuitive and commonplace finding, the reason that repetition priming (and priming in general) occurs is not fully understood or commonly agreed upon. In the present work, a new and counterintuitive priming result is introduced that presents a challenge to many theories of how priming affects visual word recognition. This effect provides a compelling example of how relatively subtle manipulations of context can have a dramatic influence on priming.

Recent demonstrations of the influence of list context on single word identification provide a foundation for the present results. The mixture of target items used to assess word identification can have a large influence on individual word identification and word priming. For example, the nature of a set of filler words used in the naming task (e.g., words with regular vs. irregular pronunciations) affects how long subjects take to read critical words aloud (Jared, 1997; Lupker, Brown, & Columbo, 1997). Similarly, word/nonword lexical decisions are influenced by whether low- and high-frequency words are presented in separate blocks or are mixed together within blocks (e.g., Gordon, 1983). Lexical decisions are more difficult when the nonword targets sound like real words such as "bocks" and "pharm" (e.g., Davelaar, Coltheart, Besner, & Jonasson, 1978; Joordens & Becker, 1997). In addition, the amount of benefit derived from a prime depends not only on the nature of targets, but also on the nature of the primes (Smith, Besner, & Miyoshi, 1994; Stolz & Besner, 1997). For example, Smith et al. (1994) found that semantic priming (e.g., faster response to DOCTOR if it follows the related word nurse than if it follows the unrelated word truck) occurred for shorter duration primes only when these were presented in a list that did not contain any longer duration primes. Since words frequently appear in sentences, it is adaptive for the word recognition system to become sensitive to, and influenced by, the information that context provides. But as these studies show, even when words are presented in isolation, the primes and the other targets in the stimulus list act as a type of context that can also influence how each word target is recognized.

It is tempting to separate list context effects into two main categories: Those that influence behavior without the subject's intent or awareness, and those that have their effect by altering the conscious strategies subjects use to perform a task. Distinguishing between these two types of influence is important for researchers who view strategic effects as nuisance factors that interfere with the study of underlying automatic word recognition machinery (e.g., Forster, 1998). In contrast, if a list context variable affects word recognition even in the absence of conscious strategies, then it reflects the inner workings of this machinery, and should be explained by any comprehensive theory of word recognition. However, this distinction may be too simplistic: A list context effect that can result from the use of conscious strategies might also be obtained under certain conditions when conscious strategies cannot be invoked.

The experiments reported here test whether a particular list context effect typically classified as strategic can also be obtained in the absence of conscious strategies. The finding of interest is that the proportions of various types of primes in a stimulus list can affect the amount of priming that is observed (e.g., Snow & Neely, 1987; Tweedy, Lapinski, & Schvaneveldt, 1977). Most commonly, the proportion of semantically related prime-target pairs in the stimulus list is varied between two groups, and the observed semantic priming effect is often larger for the group receiving the higher relatedness proportion. This relatedness proportion effect has consistently been found at prime-target stimulus onset asynchronies (SOAs) of 500 ms or more (Bushell, 1996; de Groot, 1984, den Heyer, 1985; den Heyer, Briand, & Dannenbring, 1983; Seidenberg, Waters, Sanders, & Langer, 1984; Stolz & Neely, 1995). Results with shorter SOAs have been mixed: Some studies have found the interaction (de Groot, 1984; Henik, Friedrich, Tzelgov, & Tramer, 1994; both with a 240-ms SOA), whereas others have not (den Heyer et al, 1983, 75-ms SOA; Stolz & Neely, 1995, 200-ms SOA). For example, Stolz and Neely (1995) compared groups who saw related semantic primes on either .25 or .50 of the word trials, presented at either a 200-ms or an 800-ms SOA. The .50 group showed more priming than the .25 group, but only at the 800-ms SOA; priming was equivalent for the two groups at the 200-ms SOA.

Because the interaction of relatedness proportion and priming is more commonly found at longer SOAs, it has most often been attributed to one of two strategic processes: expectancy or semantic matching (Neely, 1991; Neely & Keefe, 1989). Perhaps the most common explanation for this interaction is that subjects given longer SOAs consciously use the primes to generate expectancies about the identity of the upcoming target, whereas subjects given shorter SOAs do not have enough time to generate these expectancies (Becker, 1980; Keefe & Neely, 1990; Neely & Keefe, 1989). Using the primes to predict the targets becomes a more obvious and fruitful strategy when the proportion of related prime-target pairs in the stimulus list is higher, making subjects more likely to adopt it, resulting in an increase in the priming effect.

A retrospective semantic matching process has also been used to explain relatedness proportion effects in the lexical decision task (e.g., Keefe & Neely, 1990; Neely & Keefe, 1989). Subjects can make lexical decisions based on whether they detect a semantic relationship between the prime and target. If one is detected, it means that the target must be a word, and thus a positive lexical decision will be supported. Like expectancy, semantic matching is assumed to be a consciously deployed process that depends on the subject noticing relationships between primes and targets, so it, too, is more likely to be invoked when the relatedness proportion is higher.

The finding that relatedness proportion effects are not commonly observed when shorter SOAs are used has been taken as support for the view that by using brief SOAs one can study bottom-up word processing divorced from consciously deployed strategic effects. In particular, some researchers have argued that proportion effects should not be observed when primes are masked to prevent their identification, under the assumption that these effects can be observed only if subjects are able consciously to identify the primes and use an expectancy (Forster, 1998) or semantic matching heuristic (Neely, 1991). To date, however, there have not been any published studies examining whether proportion effects occur with masked primes. The present experiments sought to remedy this situation, not only to examine the extent to which use of conscious strategies determines priming, but also to contrast two very different accounts of masked priming. A lexical entry-opening account does not predict proportion effects with masked primes, whereas an episodically based resource-retrieval account does. These two accounts of priming are described next. The experiments in Series 1 contrast these accounts by investigating whether proportion effects can be obtained with masked repetition primes. In Series 2, the claim that relatedness proportion effects with semantic primes are due to use of conscious strategies is directly tested by seeing whether these effects occur with masked semantic primes.

A Lexical Entry-Opening Account of Repetition Priming

The lexical entry-opening account of priming emphasizes that presentation of a word prime causes its lexical entry to be accessed in a kind of mental dictionary (Forster & Davis, 1984). A lexical entry contains information about the visual and phonological properties of the word. Lexical access leaves the entry in an open state for a brief period, so that if the same word is presented again within this period, the contents of the entry can be accessed more quickly. The repetition priming effect reflects the extent of this benefit. For reasons reviewed elsewhere (Bodner & Masson, 1997; Forster & Davis, 1984), it is generally acknowledged that an episodic component contributes to repetition priming of word recognition, even by those who wish to argue that a lexical entry-opening process makes additional contributions (Forster & Davis, 1984). For instance, lexical entry-opening effects should be short-lived (else too many entries would be open simultaneously), yet priming from a single exposure to a word can last for hours or even much longer (e.g., Jacoby, 1983; Jacoby & Dallas, 1981; Scarborough et al., 1977; Tulving, Schacter, & Stark, 1982). To examine the lexical component of repetition priming in isolation, therefore, one must somehow eliminate episodic contributions to task performance and demonstrate dissociations between cases in which lexical entry-opening versus episodic retrieval forms the basis for a given priming effect. The masked repetition priming paradigm was designed to serve this purpose.

In the standard masked repetition priming paradigm, an uppercase mask (e.g., XXXX) is followed by a briefly presented lowercase prime (e.g., trip), which is followed by an uppercase target (e.g., TRIP) to which the subject makes a lexical decision or a word naming response. The presentation of the mask and the brevity of the prime's presentation (e.g., 45-60-ms) make the prime very difficult to see, yet large repetition priming effects are typically observed. Priming does not occur at a perceptual level because primes are in lowercase letters and targets are in uppercase letters, and also because the effect can survive when another word appears between the prime and the target (Forster & Davis, 1984). Most subjects report being unaware of the presence of the primes, leading Forster and Davis to claim that masking prevents subjects from constructing episodes for the primes. Note that this claim assumes subjects must consciously detect a prime for them to construct an episode of it. For the same reasons, Forster (1998) argued that masked priming effects are not influenced by strategic processes such as expectancy. Instead, masked repetition priming is thought to reflect the bottom-up operation of the lexical processor.

Forster and Davis (1984) found that two priming effects indicative of an episodic influence on priming disappeared when primes were masked. First, in the frequency attenuation effect, low-frequency words show more priming than high-frequency words when primes are clearly visible (e.g., Duchek & Neely, 1989; Jacoby & Dallas, 1981; Norris, 1984; Rajaram & Neely, 1992; Scarborough et al., 1977). If a repetition prime simply opens a target's lexical entry, then responses to low- and high-frequency targets should be facilitated by the same amount. Forster and Davis (1984; Experiment 3) replicated the interaction of frequency and priming with unmasked primes, and showed that when primes were masked, although priming persisted, the interaction disappeared (Experiment 1). Subsequent masked repetition priming experiments using the lexical decision task have consistently replicated Forster and Davis, failing to obtain the interaction

(Bodner & Masson, 1997; Ferrand, Grainger, & Segui, 1994; Forster, Davis, Schoknecht, & Carter, 1987; Rajaram & Neely, 1992; Segui & Grainger, 1990; Sereno, 1991).

Second, nonword repetition priming with unmasked primes suggests an episodic component because nonwords have no lexical entries to open (e.g., Besner & Swan, 1982; Kirsner & Smith, 1974; McKone, 1995; Norris, 1984; Scarborough et al., 1977; but see Forster, 1998, for exceptions to this rule). When primes were masked, Forster and Davis (1984) found word priming, but failed to find any evidence of nonword priming, suggesting that nonword priming was episodically driven and that masking removed this influence, revealing only lexically driven priming. The absence of masked nonword priming has since been replicated several times (e.g., Bodner & Masson, 1997, Experiment 1; Forster, 1985, 1987; Forster et al., 1987; Forster & Davis, 1991; Forster & Taft, 1994; Forster & Veres, 1998; Rajaram & Neely, 1992, for nonstudied nonwords).

An Episodic Account of Priming

Episodic accounts of priming emphasize that word identification, like recognition memory performance, is guided by the retrieval from memory of previous experiences with words (e.g., Jacoby, 1983; Jacoby & Dallas, 1981). From an episodic perspective, priming occurs because the processing episode constructed for the prime is a particularly salient, distinct, and accessible resource that can be quickly retrieved to help guide target processing. Despite the evidence from Forster and Davis (1984), Bodner and Masson (1997) suggested that masking primes does not prevent the construction of episodes.

The proposal that episodes can be formed for very briefly presented stimuli is consistent with findings from the mere exposure paradigm. For example, Kunst-Wilson and Zajonc (1980) gave subjects five 1-ms exposures to a series of novel geometric shapes (randomly), prior to giving them a two-alternative forced-choice task in which they were to choose the alternative they "preferred." Subjects showed above-chance preference judgments for the briefly studied geometric shapes (60%), even though their recognition judgments were at chance (48%). The geometric shapes were novel for subjects (as for nonword stimuli in priming experiments), so the preference results are consistent with subjects having created episodes for these shapes even after extremely brief exposures. The recognition judgment result supports the claim that conscious awareness of these episodes is not necessary for performance to be affected.

There is also evidence to suggest that episodic influences play a role in short-term priming in lexical decision tasks. Ratcliff, Hockley, and McKoon (1985) found that short-term priming effects in lexical decision and recognition tasks were very similar in form (e.g., across various lags), leading them to suggest that contrary to Forster and Davis's (1984) claim, a common mechanism might underlie priming in both tasks. In the masked priming paradigm, Bodner and Masson (1997) argued that masking primes simply reduces the amount of information that can be encoded into processing episodes and the extent to which they will be retrieved and used during target processing.

The retrieval view of priming offered by Whittlesea and Jacoby (1990) provides some justification for this latter claim. These researchers demonstrated that a prime (e.g., GREEN) was more likely to be used to help identify a target word if this second word was degraded (e.g., pLaNt) than if it was not degraded (e.g., PLANT). Naming latencies for a third word that followed the target and was a repetition of the prime (e.g., GREEN) were faster if the target was degraded than if it was not. This result suggests that the form of the target (degraded or normal) determined the extent to which the prime was recruited to assist with target identification: A degraded target was more likely to recruit the prime. Response latency to the re-presentation of the prime provided evidence of the prime's greater availability in the case of degraded targets.

The dependency of prime availability on target degradation is inconsistent with a prospective spreading activation process (e.g., Posner & Snyder, 1975) because the spreading of the prime's activation to related words in a mental network cannot be made conditional on an unforeseen event—in the case of Whittlesea and Jacoby (1990), whether the target word would be degraded. Instead, Whittlesea and Jacoby argued that priming occurs retrospectively—the prime acts as a resource that is brought forward to help in the processing of the target. This prime word plays a greater role when the target is degraded and hence more difficult to identify. Retrospective processes are often included as a partial (e.g., Balota & Lorch, 1986; de Groot, 1984; Neely & Keefe; 1989) or complete account of semantic priming (e.g., Norris, 1986; Ratcliff & McKoon, 1988; Whittlesea & Jacoby, 1990).

Bodner and Masson (1997) extended this episodic view of priming to the masked repetition priming paradigm, arguing that if lexical decisions were made more difficult, subjects might be more likely to rely on masked priming episodes. Consistent with this episodic resource-retrieval account, Bodner and Masson (1997) found strong nonword priming when word and nonword targets were presented using case-alternation (e.g., wOrD; Experiment 2A) and when pseudohomophone nonword targets were used (e.g., BRANE; Experiment 3). For case-alternated nonwords, priming effects were particularly large (93 ms), especially relative to those found for low- and high-frequency word targets (79 ms and 71 ms). Bodner and Masson proposed that retrieval of orthographic and/or phonological information from the nonword priming episode made encoding of the nonword target more efficient on repetition prime trials, resulting in nonword priming. Moreover, Masson and Isaak (1999) found clear evidence of masked repetition priming of nonword targets when a word and nonword naming task was used. They also attributed the priming effect to retrieval of orthographic or phonological information encoded during presentation of the prime. These findings present a clear challenge to the proposal that masked repetition priming is due to the opening of an existing lexical entry.

In the Bodner and Masson (1997) experiments, however, priming remained statistically equivalent for low- and high-frequency words. On an episodic account, low-frequency words might have been expected to produce more priming than high-frequency words, as is typically found with unmasked primes. Bodner and Masson took the additivity of frequency and priming effects as a further indication that masked prime episodes likely contain primarily orthographic information (and/or phonological information; Masson & Isaak, 1999) that would facilitate low- and high-frequency targets equally.

The apparent lack of interaction between frequency and priming, then, does not provide a clear distinction between the lexical and episodic accounts of masked repetition priming. Moreover, Forster (1998) has suggested that demonstrations of masked priming of nonword targets (Bodner & Masson, 1997; Masson & Isaak, 1999) may have been based on procedures that introduce unusual processing operations to word identification tasks. Given the apparently equivocal interpretation of the evidence intended to distinguish episodic and lexical accounts of masked priming, a new means of distinguishing these two accounts was developed by examining their respective predictions regarding a manipulation of the proportion of repetition versus unrelated primes provided to subjects.

Repetition Proportion

Although prime proportion has not previously been manipulated in the masked priming paradigm, a related manipulation has been used in a masked word identification task, where subjects' task is to try to identify briefly presented masked words (e.g., Allen & Jacoby 1990; Jacoby, 1983). In these experiments, the proportion of perceptual identification targets that had been previously studied, called the proportion overlap, was varied (e.g., .1 or .2 vs. .9). Jacoby (1983) found that prior study boosted target identification, and that this benefit was greater when the proportion overlap was higher. Allen and Jacoby (1990) showed that this proportion effect was not due to use of conscious strategies, because the effect was not greater for words that were studied as anagrams than for words that were simply read at study, even though anagrams were more likely to be recognized in a subsequent recognition phase of the experiment. Instead, the effect was attributed to similarity between study and test context having facilitated performance. Hence, an episodic factor that influences recognition was shown to also influence masked word identification, a task which, like the lexical decision task with masked primes, was thought to only be influenced by stable word identification processes.

Another important aspect of Allen and Jacoby's (1990) results was that although studied words were more likely to be identified when the proportion overlap was higher, nonstudied words on the test list were less likely to be identified. The implication of this finding is that proportion overlap, at least in this task, has its effect on bias rather than on sensitivity (see also, Ratcliff, McKoon, & Verwoerd, 1989). Thus, there is evidence to suggest that context manipulations such as proportion overlap can bias performance on tasks when aware uses of memory have been minimized. And as Allen and Jacoby (1990) point out, episodic influences of encoding task and test context are not consistent with a very brief preactivation of a lexical entry.

In Series 1, the relatedness proportion manipulation used in several studies of semantic priming was adapted for use with repetition primes to test an extension of Bodner and Masson's (1997) episodic account of masked priming. Working from that account, the proportion of repetition primed trials in the stimulus list might reasonably be expected to affect the amount of priming observed for word targets. When this repetition proportion is higher, primes are more frequently a valid and helpful source of information about targets than when it is lower. Therefore, memory retrieval processes may be more likely to recruit the prime to help guide target processing when the repetition proportion is higher. On this episodic resource-retrieval account, a group of subjects receiving a higher repetition proportion, by relying on primes to a greater extent, may show more priming for word targets than a group of subjects receiving a lower repetition proportion, producing an interaction of repetition proportion and priming.

Moreover, if repetition proportion, like proportion overlap, has its influence through bias (Allen & Jacoby, 1990; Ratcliff et al., 1989), then enhanced recruitment and use of the prime episode by the high repetition proportion group should generally produce two effects. Greater use of primes by the high repetition proportion group should improve target processing on repetition prime trials, and it should impair target processing on unrelated prime trials. The effect of repetition proportion on unrelated primes is expected if recruitment and attempted use of an unrelated prime episode interferes in any way with the lexical decision about the target.

Subjects will not be sensitive to the repetition proportion on a conscious level, of course, because they are unaware of the primes' identities and often even of their presence. Instead, the claim being put forward is that word recognition processes can become tuned to regularities present in the priming resource, allowing it to play a larger or smaller role in the identification of targets depending on its validity. This proposal is consistent with results from artificial grammar and implicit learning paradigms in which subjects exhibit sensitivity to regularities in a stimulus set without awareness of those regularities (Reber & Allen, 1978; Vokey & Brooks, 1992; Whittlesea & Wright, 1997).

In contrast, Forster (1998), in his review of the benefits and problems associated with masked priming, argued that masking the prime not only eliminates episodic contributions to priming, but also eliminates the opportunity for conscious strategies such as expectancy to influence priming. Therefore, Forster and Davis' (1984) lexical entry-opening account does not appear to predict any influence of repetition proportion on masked priming. On this account, a masked word prime simply opens its corresponding lexical entry in a ballistic or automatic manner and to the same extent, independently of repetition proportion or any other list context manipulation. Moreover, the strategic expectancy and semantic matching processes (e.g., Neely and Keefe, 1989) thought to underlie interactions of relatedness proportion and semantic priming at SOAs of 500 ms or more are not hypothesized to be engaged when primes are masked and presented for such brief SOAs (Forster, 1998; Neely, 1991). Hence, these processes could not underlie an interaction of repetition proportion and masked priming.

Series 1: Manipulations of Repetition Proportion

In the experiments in Series 1, repetition proportion was systematically manipulated and its effects on masked repetition priming of word and nonword targets in the lexical decision task were observed.

Experiment 1

Experiment 1 provided an initial test of whether masked repetition priming of words and nonwords would be affected by the proportion of repetition-primed trials in the stimulus list. For this test, a low repetition proportion group received repetition primes on .2 of the word trials and .2 of the nonword trials, and a high repetition proportion group received repetition primes on .8 of the word trials and .8 of the nonword trials. For both groups, unrelated primes preceded the remaining targets.

Method

Subjects. Subjects in these experiments were undergraduates at the University of Victoria who participated for extra credit in an introductory psychology course. Each subject took part in only one of the reported experiments. Subjects were always randomly assigned to groups. In Experiment 1 there were 40 subjects in each repetition proportion group.

Materials and design. The targets were 220 words and 220 pronounceable nonwords of 3-8 letters in length. Twenty targets of each type were used for practice trials. The frequency of occurrence of the 200 critical word targets ranged from 1 to 2216 per million (median = 31; Kucera & Francis, 1967). The distribution of word frequencies was positively skewed, with 154 of the 200 items having frequencies of less than 100 per million. For each word and nonword target, an additional unique unrelated prime of equal length was selected that shared no more than two letters with the target and had no shared letters in the same position in the two items. Each unrelated word prime was paired with a word target of similar frequency.

The critical word and nonword targets were each divided into five blocks of 40 items. In the high repetition proportion group, four blocks of each target type were paired with repetition primes and one block of each type were paired with unrelated primes. In the low repetition proportion group, the number of blocks paired with each type of prime was reversed. The assignment of blocks of items to repetition or unrelated prime conditions was counterbalanced across subjects. Therefore, across the low and high repetition proportion groups, each target was paired with its repetition prime and its unrelated prime equally often. The repetition proportion of the practice items matched that of the critical items. Once primes were selected for each target, word and nonword items were randomly intermixed within the practice and critical lists. The high repetition proportion group saw 16 repetition and 4 unrelated practice trials, and 160 repetition and 40 unrelated critical trials, for both words and for nonwords, and these numbers were reversed for the low repetition proportion group. Thus, the repetition proportion was .8 for both words and nonwords in the high repetition proportion group, and was .2 for both words and nonwords in the low repetition proportion group.

Procedure. Subjects were tested individually. Stimuli were presented in black 12-point courier font against an off-white background on a monochrome monitor attached to a Macintosh II computer. Stimulus displays were synchronized with the raster scan cycle of the monitor to permit exact timing in increments of 15 ms. The accuracy of the prime-target SOA was verified using a light-sensitive diode connected to an electron storage oscilloscope.

Subjects were told that several briefly displayed items would be shown on each trial in the center of the screen, and that their task was to respond to the uppercase target which would remain on the screen until they responded. Each trial began with a 495-ms mask consisting of a row of uppercase Xs matched for length with the prime and target. The prime was then immediately presented in lowercase letters for 60 ms. The uppercase target was then immediately displayed until the subject responded. Once the target appeared, subjects made a lexical decision as quickly and as accurately as possible by pressing one of two labeled keys on a response box. Subjects used their right index finger to press the YES key if the target was a word and their left index finger to press the NO key if the target was not a word. To encourage conscientious responding, the instructions correctly told subjects they would be shown a summary of their performance at the end of the experiment. In addition, a corrective feedback message appeared on the monitor for 1 s and a tone sounded on trials where the subject made an incorrect response (INCORRECT RESPONSE) or took more than 1500 ms to respond (TOO SLOW). The next trial began 500 ms after a correct response or corrective feedback.

Following the 40 practice trials, a message was displayed encouraging subjects to ask questions if they were unclear about the task. Otherwise, they pressed a key and began the 400 critical trials. Self-paced rest breaks were provided after every 50 critical trials. To assess awareness of the primes, at the end of the testing session subjects were asked what they saw on each trial, just before the target appeared. If they initially reported seeing only Xs, they were asked follow up questions (e.g., "Did you see anything else?"). The experiment took 25-30 minutes for each subject to complete.

Results

Reported prime awareness. According to their responses to questions about what was displayed during the trials, 77% of subjects across the experiments reported in Series 1 were not aware that anything other than a row of Xs had been presented prior to each target. The remaining subjects were aware that "something" other than a row of Xs had been flashed; the majority of these reported having seen "flickering letters" or "something flashing." A few subjects in each experiment (range = 0 to 7) reported noticing that some of the primes were words, but only a few of the 540 subjects tested in Series 1 reported noticing any sort of relationship between primes and targets. The level of prime awareness implied by these responses is typical of previous masked repetition priming experiments (e.g., Bodner & Masson, 1997; Forster & Davis, 1984) and other experiments using similarly masked primes (e.g., Milliken, Joordens, Merikle, & Seiffert, 1998). Therefore, it is unlikely that the priming effects discussed below were due to some consciously deployed strategy such as expectancy or semantic matching (Neely, 1991; Neely & Keefe, 1989). Not surprisingly, reports of prime awareness tended to be less frequent in experiments where a 45-ms rather than a 60-ms SOA was used. Across the Series 1 experiments, the mean percentage of subjects in the low and high repetition proportion groups who did not report seeing anything except a row of Xs was nearly identical (78% vs. 77%). Additional questionnaire data reported in Experiment 6 suggest that subjects in the low and high repetition proportion groups make similar estimates of the repetition proportion they experienced.

Response latency and errors. In all experiments, the following conventions were used. Trials with response latency shorter than 300 ms or longer than 3000 ms were excluded from analysis, resulting in less than 0.5% of the trials being eliminated from any single experiment (Ulrich & Miller, 1994). Reducing the upper cut-off to 1500 ms did not change the pattern of results. Separate analyses of variance (ANOVA) with subjects as the random variable were performed on the mean response latencies and error rates for words and nonwords. For reasons discussed elsewhere (e.g., Raaijmakers, Schrijnemakers, & Gremmen, 1999), item analyses are not reported although they were generally consistent with the subject analyses. Reported effects were significant at the $p < .05$ level unless otherwise noted. The means and associated confidence intervals (Loftus & Masson, 1994) are displayed graphically. Statistical tests of the main effects and interactions for each experiment appear in Appendix A (Table A1 for Experiment 1). For brevity, in the text only the tests of main theoretical interest are reported—whether priming occurred and whether it interacted with the repetition proportion factor. In experiments in which word frequency was varied systematically, whether frequency interacted with priming is also reported. Except where noted, priming effects reflected shorter response latencies or fewer errors on repetition prime trials than on unrelated prime trials.

The results reported for Experiment 1 were derived from mixed-factor ANOVAs in which priming (repetition vs. unrelated) and repetition proportion (low vs. high) were the respective within- and between-subject factors. Figure 1 displays the obtained means and priming effects for Experiment 1.

Word targets. In the response latency analysis for word targets, there was a main effect of priming that interacted with repetition proportion. As Figure 1 (word data, left panel) illustrates, the high repetition proportion group showed substantially more priming than the low repetition proportion group (65 ms vs. 36 ms).[1] In the error rate analysis, only the main effect of priming was significant.

Nonword targets. For nonword targets, there was a main effect of priming for nonwords in the response latency analysis that interacted with repetition proportion. Only the high repetition proportion group produced significant nonword priming. There were no reliable effects in the error rates.

Discussion

The results of Experiment 1 support the claim that the word recognition system can become tuned to regularities in priming stimuli such that when primes are a more valid source of information concerning the target (i.e., when the repetition proportion is high), they exert a greater influence on target processing and hence on lexical decisions. The finding of greater priming for both word and nonword targets when the repetition proportion was high compared to when it was low is consistent with the extension of the episodic account of priming. Moreover, the positive benefit of high repetition proportion on nonword responses runs contrary to the claim that repetition priming of nonwords inevitably biases the incorrect "word" response (e.g., Feustel, Shiffrin, & Salasoo, 1983; Salasoo, Shiffrin, & Feustel, 1985). Instead, Experiment 1 suggests that nonword repetition primes can be used to support correct "nonword" decisions.

The finding that masked priming is modulated by repetition proportion is not consistent with the operation of a lexical entry-opening process. According to Forster and Davis (1984; Forster, 1998), masked repetition primes open lexical entries, producing a savings in the bottom-up lexical processes that need to be performed on the upcoming target. The benefit from opening a target's entry is thought to be an automatic consequence of the processing applied to the prime, so that the repetition proportion instantiated across a series of trials should not affect word or nonword priming—yet repetition proportion clearly affected both in Experiment 1. Moreover, the observed interactions of repetition proportion and priming are not consistent with the suggestion from the semantic priming literature that proportion effects are a signature of an intentional expectancy or semantic matching process (Neely, 1991; Neely & Keefe, 1989). Because the primes in Experiment 1 were masked and presented for only 60 ms, it is unlikely that subjects consciously attempted to facilitate their lexical decisions by generating expectancies or by looking for semantic relationships between primes and targets (i.e., semantic matching).

Experiment 2

Previous experiments have consistently found that masked repetition primes facilitate lexical decisions to low- and high-frequency targets to a similar extent (e.g., Bodner & Masson, 1997; Ferrand et al., 1994; Forster et al., 1987; Rajaram & Neely, 1992; Segui & Grainger, 1990; Sereno, 1991). In Bodner and Masson's (1997) experiments, a trend toward greater priming for low-frequency words than for high-frequency words was consistently observed, but even a pooling of the data from three of their experiments failed to produce a reliable frequency by priming interaction. Bodner and Masson took the additivity between word frequency and priming as evidence that the episodes constructed during the presentation of masked primes contain mostly orthographic or phonological information that is not sensitive to word frequency. In contrast, Forster and Davis (1984) took this additivity as evidence that masked primes open lexical entries, producing equivalent savings for low- and high-frequency targets because frequency effects are assumed to arise from a frequency-ordered search of opened entries.

Experiment 2 provided further tests of whether word frequency and masked priming can interact, this time in the context of the repetition proportion manipulation. On the episodic resource-retrieval account, masked primes may be more likely to be used to help guide target processing when they are a valid source of information regarding the target, and when the need for such information is high (i.e., when target processing is made difficult). The episodes for masked primes become a more valid resource when the repetition proportion is high rather than low, and need for this resource may be greater when a low-frequency target is being processed because such targets are less practiced than high-frequency targets. On this account, then, an interaction of frequency and priming may be more likely to be observed when the repetition proportion is high (.8), relative to when it is low (.2 here; .5 in typical experiments). In contrast, the lexical entry-opening account predicts that priming should be insensitive to this factor, thus low- and high-frequency words should be equally facilitated by repetition primes at both levels of the repetition proportion factor.

Earlier reports of the absence of a frequency by priming interaction when masked primes were involved may have been due to a relatively weak manipulation of word frequency. For example, in Bodner and Masson (1997) and Forster and Davis (1984), low-frequency words were selected from the 1-2 per million range and high-frequency words were selected from the 40-60 per million range (Kucera & Francis, 1967). A much more robust frequency manipulation was used in Experiment 2: Low-frequency words were chosen from the 1-10 per million range, and high-frequency words were chosen from the 100-1000 per million range. By increasing the difference between low- and high-frequency words, low-frequency targets might stand out in the stimulus list as being particularly difficult to process. If so, relatively greater use may be made of the masked primes on trials with low-frequency word targets relative to trials with high-frequency word targets, which could generate an interaction of frequency and priming. In contrast to this prediction of the episodic resource-retrieval account, the lexical entry-opening account predicts additivity between frequency and priming effects independent of the relative frequency difference between low- and high-frequency words in the stimulus list. On this latter account, masking the primes supposedly removes the episodic influence that produces the advantage for low-frequency words that is often observed when primes are not masked (e.g., Forster & Davis, 1984).

Two versions of Experiment 2 were conducted. Experiment 2A was the same as Experiment 1, except target word frequency (low vs. high) was now systematically varied within subjects. Experiment 2B was the same as Experiment 2A, except targets were presented in case-alternated letters, following Bodner and Masson (1997, Experiment 2B). The idea here was that case-alternated targets, being difficult to process, might increase the word recognition system's reliance on the masked primes, which would maximize the chance of obtaining a frequency by priming interaction. Also, as discussed below, the SOA in Experiment 2B was 45 ms instead of 60 ms.

Method

Subjects. There were 40 subjects in each version of Experiment 2. Half of the subjects in each experiment were randomly assigned to the low repetition proportion group and half were assigned to the high repetition proportion group.

Materials and design. The critical targets were 100 low-frequency and 100 high-frequency words and 200 pronounceable nonwords of 4-6 letters in length. An additional 10 low-frequency and 10 high-frequency word targets and 20 nonword targets were used for practice trials. Kucera and Francis (1967) frequencies of the critical low-frequency word targets ranged from 1-10 (median = 3), and frequencies for the high-frequency words ranged from 100-967 (median = 193). Many of the nonword targets were taken directly from Experiment 1 (some of which were modified to match the length restriction of 4-6 letters) and the rest were newly constructed. Unrelated primes were chosen using the same constraints described in Experiment 1. The design was the same as in Experiment 1, except the critical low- and high-frequency word targets were each divided into five blocks of 20 items for counterbalancing.

Procedure. The testing procedure for Experiment 2A was the same as that used in Experiment 1, including a 60-ms SOA. In Experiment 2B, all targets were presented in case-alternated letters beginning with a lowercase letter (e.g., pOwEr). Also, the stimuli for Experiment 2B were presented using a Power Macintosh G3 computer rather than a Macintosh II, although the same font and display characteristics were in effect. The display durations on the monitor used with the Macintosh G3 were verified using an electron storage oscilloscope. Nevertheless, casual observation gave the impression that the primes were somewhat easier to detect on the Macintosh G3 monitor, perhaps due to differences in display contrast. To ensure that the primes were not more identifiable in Experiment 2B, primes were presented for 45 ms rather than 60 ms. The effects obtained with 45-ms versus 60-ms primes are directly compared in Experiments 4A and 4B.

Results

The data for Experiments 2A and 2B were each analyzed as in Experiment 1, except that for the word targets there was a word frequency factor (low vs. high) in addition to the priming (repetition vs. unrelated) and repetition proportion (low vs. high) factors. The means and corresponding priming effects for Experiments 2A and 2B appear in Figures 2 and 3, respectively. Statistical tests of the main effects and interactions appear in Appendix A (Table A2).

Word targets. In the response latency analyses for Experiment 2A (uppercase targets), a main effect of priming was observed, but contrary to Experiment 1 it did not interact with repetition proportion. There was, however, a robust interaction of word frequency and masked priming. As Figure 2 illustrates, priming effects were almost twice as large for low-frequency targets as for high-frequency targets (71 ms vs. 39 ms). Post-hoc tests revealed that this interaction was reliable for both the low repetition proportion group, $F(1, 19) = 10.45$, $MSE = 561$, and the high repetition proportion group, $F(1, 19) = 7.53$, $MSE = 543$.

The pattern of results in the error rates for Experiment 2A exactly paralleled the pattern in the response latency analyses. There was a main effect of priming that did not interact with repetition proportion but that did interact with word frequency.

Comparing Figures 2 and 3, one can see that the pattern of response latency results for Experiment 2B (case-alternated targets) was the same as that found in Experiment 2A. There was a main effect of priming that did not interact with repetition proportion, as well as a frequency by priming interaction similar in magnitude to that found with uppercase targets in Experiment 2A. Priming effects were twice as large for low-frequency words as for high-frequency words (69 ms vs. 33 ms). As in Experiment 2A, post-hoc tests revealed that this interaction was significant both for the low repetition proportion group, $F(1, 19) = 9.20$, $MSE = 934$, and the high repetition proportion group, $F(1, 19) = 10.26$, $MSE = 410$.

Comparing Figures 2 and 3 once again, one can see that the pattern of error rates in Experiment 2B was somewhat different than the pattern obtained in Experiment 2A. Although the main effect of priming was reliable in both experiments, the repetition proportion by priming interaction was significant only in Experiment 2B, reflecting a greater reduction of errors due to repetition primes in the high repetition proportion than in the low repetition proportion group. There was no frequency by priming interaction in the error rates for Experiment 2B.

Nonword targets. There were reliable nonword priming effects in the response latency analyses for Experiments 2A and 2B. The interaction between repetition proportion and priming observed in Experiment 1 was also observed in Experiment 2A. As Figure 2 illustrates, reliable nonword priming occurred only in the high repetition proportion group. In Experiment 2B, priming effects were about the same for low and high repetition proportion groups—there was no interaction (see Figure 3). There were no reliable effects in the nonword error rates for either experiment.

Discussion

Two important results came out of Experiment 2. First, low-frequency words benefited from repetition primes more than high-frequency words in both Experiments 2A and 2B—an effect not previously reported for masked repetition priming in the lexical decision task. The use of a much stronger manipulation of word frequency than usual (cf.

Bodner & Masson, 1997; Forster & Davis, 1984) appears to be the basis for the frequency by priming interaction found here. The interaction does not appear to be mediated by prime validity because it was statistically significant both when validity was high (the high repetition proportion group) and when it was low (the low repetition proportion group) in each experiment. Moreover, the interaction does not appear to be mediated by need for the prime resource, since it obtained both when targets were presented in uppercase letters and when target processing was made more difficult by presenting targets in case-alternation (note, however, that a shorter SOA was used in the latter case, which may have worked generally to reduce priming in Experiment 2B).

Independent of its locus, a frequency by priming interaction is not consistent with a lexical entry-opening process, which predicts equal priming for low and high-frequency targets. One might argue that a second pass through the lexicon may sometimes be required before an entry for a low-frequency word can be located and opened, and that this could produce the observed interaction. This argument is problematic, however, because the low-frequency targets used here formed a higher frequency (range: 1-10) than those used in experiments in which the frequency by priming interaction did not obtain (range: 1-2). It is not clear why a second pass would be more likely when words of somewhat higher frequency are involved. In any case, the interaction between priming and frequency eliminates one of the critical dissociations distinguishing masked repetition priming from long-term repetition priming.

The second major finding of interest was that a systematic mixed-list manipulation of word frequency in Experiment 2 appears to have made the repetition proportion by priming interaction that was found with words in Experiment 1 disappear (except in the error rates of Experiment 2B). The remaining experiments attempted to establish the reason for this outcome, and to document that the interaction nonetheless occurs reliably under specifiable conditions. That repetition proportion interacted with priming for nonwords in Experiment 2A but not in Experiment 2B suggests that a 60-ms SOA might be necessary for masked nonword priming to be observed. This hypothesis is tested in Experiment 4. Differences in SOA may also explain why nonword priming effects were not larger with case-alternated targets than with all uppercase targets, in contrast to the large difference reported by Bodner and Masson (1997, Experiment 1 vs. 2A).

Experiment 3

The purpose of Experiment 3 was to determine why repetition proportion interacted with priming when word frequency was wide-ranging although not systematically varied (Experiment 1), but not when word frequency was wide-ranging and systematically manipulated to form a bimodal distribution of frequency (Experiment 2). One hunch is that subjects' lexical decisions in Experiment 2 were affected by the presence of two distinct bands of word frequency in the stimulus list, and that this discontinuity altered their sensitivity to repetition proportion. Having distinctly easy versus hard targets appearing randomly throughout the list may have led subjects consistently to make use of priming resources, regardless of repetition proportion. This account has the virtue of being consistent with the observed interaction between frequency and priming in Experiment 2. To test this idea, word frequency was manipulated between subjects in Experiment 3, so that word targets for each group were selected only from a single frequency band.

On the lexical account, even when frequency is manipulated between subjects, there should be no interaction between repetition proportion and priming. Nor should priming be modulated by word frequency. According to the episodic resource-retrieval account, making word frequency homogeneous within a list may allow repetition proportion to once again influence the use of priming resources. If so, the high repetition proportion groups should show more priming than the low repetition proportion groups. Moreover, lexical decisions are easier when high-frequency rather than low-frequency word targets are used, suggesting that the need for priming resources should generally be greater when low-frequency words are used. It is possible, then, that prime validity will play a larger role when the need for the resource is higher (i.e., when low-frequency words are used). If so, subjects tested with low-frequency words might be more likely to show a repetition proportion by priming interaction than subjects tested with high-frequency words. Finally, to the extent that nonwords behave similarly to words, repetition priming should be greater when word frequency is low rather than high (i.e., when resource need is greater), and when the repetition proportion is high rather than low (i.e., when resource validity is greater).

Method

Subjects. There were 20 subjects randomly assigned to each of four groups defined by a factorial manipulation of repetition proportion (low vs. high) and word frequency (low vs. high).

Materials and design. The materials from Experiment 2 were used again, but 100 additional low-frequency and 100 additional high-frequency unrelated prime and target pairs were selected because frequency was now manipulated between subjects. To find enough high-frequency pairs that did not share any letters in common in the same position, some of the additional high-frequency items had to be chosen from the 80-99 per million range. These items were used as high-frequency practice items and an equivalent number of the high-frequency practice items used in Experiment 2 were used as critical high-frequency items in Experiment 3. Thus, critical high-frequency items in both experiments were selected from the same frequency range. Kucera and Francis (1967) frequencies of the critical targets ranged from 1 to 10 for low-frequency word targets (median = 4), and from 100-937 for high-frequency word targets (median = 217). Through an oversight, three low-frequency and three high-frequency targets appeared twice in the stimulus list (paired with different unrelated primes). Also, there were three more cases in which a target also appeared as an unrelated prime (two for high-frequency and one for low-frequency targets). Because the duplicates were nearly evenly distributed across the frequency manipulation, they were not omitted from the analyses. There were no duplicates in the remaining experiments.

Procedure. The equipment and procedure used in Experiment 3 were the same as in Experiment 1 and a 60-ms prime-target SOA was used.

Results

The three factors (priming, frequency, and repetition proportion) were analyzed as in Experiment 2, except word frequency was now a between-subjects variable. Statistical tests for the effects reported below appear in Appendix A (Table A3). The means and priming effects for each of the four groups in Experiment 3 appear in Figure 4.

Word targets. In the response latency analysis, there was a main effect of priming. Critically, manipulating word frequency between-subjects so that each group of subjects saw targets from only one frequency range allowed the repetition proportion by priming interaction found in Experiment 1 to reemerge. The size of the difference in priming effects for the low and high repetition proportion groups was similar to that found in Experiment 1. The frequency by priming interaction was not significant, but there was a reliable three-way interaction with repetition proportion. Additional tests revealed that there was reliably more priming for low-frequency than for high-frequency words when repetition proportion was high, $F(1, 38) = 5.39$, $MSE = 358$, but no difference when repetition proportion was low, $F < 1$. The three-way interaction can also be interpreted as repetition proportion by priming interactions at each level of the frequency factor. Taking this approach showed that repetition proportion interacted with priming when the list contained only low-frequency words, $F(1, 38) = 14.28$, $MSE = 388$, but not when the list contained only high-frequency words, $F < 1.43$. In the error rates, there was a main effect of priming that did not interact with repetition proportion or frequency.

Nonword targets. The pattern of results for nonwords was rather complicated. Here the main effect of priming and its interactions with repetition proportion and frequency are emphasized. The response latency analysis revealed a main effect of priming that did not interact with repetition proportion, but that did interact with frequency. Nonword priming was present at both levels of the repetition proportion factor, but only when low-frequency word targets were used; $F(1, 38) = 7.94$ MSE = 462 for the low repetition proportion group, and $F(1, 38) = 8.51$, MSE = 863 for the high repetition proportion group.

The pattern of results in the error rate analysis differed from that in the response latency analyses in one major respect. Whereas repetition priming typically reduced or had no effect on nonword error rates in earlier experiments, in Experiment 3 there tended to be more errors on repetition than on unrelated prime trials, particularly for the high repetition proportion/low-frequency target group (see Figure 4). This resulted in a negative effect of repetition priming on the nonword error rate, suggesting a speed-accuracy trade-off.

Discussion

When low and high repetition proportion groups in Experiment 3 were given only low-frequency word targets (hereafter, Experiment 3-LF), there was significantly more priming in the high repetition proportion group, replicating the repetition proportion by priming interaction found in Experiment 1. In contrast, when low and high repetition proportion groups in Experiment 3 were given only high-frequency word targets (hereafter, Experiment 3-HF), the two repetition proportion groups showed similar amounts of priming, as observed in Experiment 2 when groups were given both low- and high-frequency word targets in a mixed list.

The lexical account does not provide an explanation for the repetition proportion by priming interaction, nor for the frequency by priming interaction found both within subjects (Experiment 2) and between subjects (for the high repetition proportion groups in Experiment 3). The episodic resource-retrieval account allows for both kinds of interactions, but it remains to be explained why they occur in some situations but not others. Before examining possible explanations, however, it seemed important firmly to establish whether repetition proportion modulates priming for low-frequency and high-frequency targets; Experiments 4 and 5 provide the respective tests.

Experiment 4

In Experiment 4, a replication of the repetition proportion by priming interaction was sought for low-frequency targets. Moreover, because the SOA in the earlier experiments was sometimes 60 ms and other times 45 ms, this variable was manipulated across Experiments 4A (60-ms SOA) and 4B (45-ms SOA) to examine its possible influence on the pattern of priming effects. This manipulation allows a test of the prediction that primes have to be presented for 60 ms for consistent nonword priming to be observed.

Method

Subjects. There were 20 subjects randomly assigned to the low repetition proportion group and 20 subjects to the high repetition proportion group in Experiment 4A, and 30 subjects randomly assigned to low and high repetition proportion groups in Experiment 4B.

Materials and design. The stimuli were the same as in Experiment 3, with accidentally duplicated items replaced. Four blocks of 50 word and 50 nonword trials were assigned repetition or unrelated primes so that the designated repetition proportion of .8 or .2 was precisely attained within each block of 100 critical trials. One random assignment of items to blocks was used for all subjects, but within a block assignment of repetition and unrelated primes to targets was counterbalanced across subjects as in the earlier experiments. Counterbalancing of the assignment of primes to targets was applied to the practice items as well.

Procedure. The experiment was run using the Macintosh G3 computer. The prime-target SOA was 60 ms in Experiment 4A and 45 ms in Experiment 4B. Otherwise the procedure was the same as that used in Experiment 3.

Results

The two versions of Experiment 4 were analyzed separately, and for each mixed-factor ANOVA, type of prime (repetition vs. unrelated) was the within-subjects factor and repetition proportion (low vs. high) was the between-subjects factor. Figures 5 and 6 show the means and priming effects for Experiments 4A and 4B, respectively. Appendix A (Table A4) lists the complete ANOVA results for each experiment.

Word targets. There was a main effect of priming in the response latency analysis of each experiment. Priming interacted with repetition proportion in both experiments as well, in the direction of larger priming effects for the high repetition proportion group than for the low repetition proportion group. There was also a main effect of priming in the error rate analysis of each experiment. A repetition proportion by priming interaction obtained in the error rate analysis only for Experiment 4A, where 60-ms primes were used.

Nonword targets. In Experiment 4A, with 60-ms primes, there was a nonword priming effect in the response latency analysis that interacted with repetition proportion. Nonword priming was reliable only in the high repetition proportion group. In Experiment 4B, with 45-ms primes, the nonword priming effect was marginal and did not interact with repetition proportion in the response latency analysis. There were no reliable effects in the error rate analyses of either experiment.

Discussion

Experiments 4A and 4B successfully replicated the priming advantage found in the high repetition proportion group relative to the low repetition proportion group for low-frequency targets in Experiment 3. This repetition proportion by priming interaction obtained whether primes were shown for 60 ms or 45 ms, suggesting that use of different

SOAs across earlier experiments did not seriously alter the pattern of results for word targets. Prime duration appears to be a much more important factor for nonword targets, as nonword priming and its interaction with repetition proportion emerged only with the 60-ms primes.

Experiment 5

The purpose of Experiment 5 was to determine whether an interaction of repetition proportion and priming could be obtained with the same set of high-frequency targets that failed to produce this interaction in Experiment 3-HF. Experiment 5A was a replication of the Experiment 3-HF using a 45-ms SOA, and Experiments 5B-5D adapted three manipulations of word/nonword discriminability from Bodner and Masson (1997). In Experiment 5B, the high-frequency word and nonword targets from Experiment 5A were presented using case-alternation, as was done in Experiment 2B. Case-alternated targets are more difficult to process, which should increase subjects' reliance on masked primes and hence provide a better chance for observing a modulated influence of repetition proportion on priming (Bodner & Masson, 1997). In Experiment 5C, the same high-frequency targets were mixed with pseudohomophone nonwords which sound like real words but are not spelled like real words (e.g., BRANE). When pseudohomophone nonwords are used, lexical decisions are more difficult because phonology cannot be used as a basis for correct responding. In Experiment 5D, the opposite tack was taken: Words and nonwords were made more dissimilar from each other by using consonant string nonwords (e.g., GLDRD). The use of consonant string nonwords makes lexical decisions easier, and hence should decrease subjects' reliance on the primes. If subjects' reliance on the primes is sufficiently attenuated repetition proportion might not interact with priming.

Method

Subjects. There were 20 subjects randomly assigned to the low repetition proportion group and to the high repetition proportion group in each of the four versions of Experiment 5, for a total of 160 subjects.

Materials and design. All four versions of Experiment 5 used the Experiment 3-HF items, with accidentally duplicated targets replaced. Experiment 5A was a replication of Experiment 3-HF. Experiment 5B was the same as Experiment 5A except targets were presented in case-alternation. Targets were presented in uppercase letters in Experiments 5C and 5D. For Experiment 5C, a set of 220 pseudohomophone nonword targets (e.g., BRANE), each with an unrelated pseudohomophone prime, were created and used as the nonwords in the same manner as in earlier experiments. The items were all 4-6 letters in length. The frequency of the base words from which pseudohomophones were formed (e.g., BRAIN) and the number of letters shared by a pseudohomophone and its base word were not controlled. For Experiment 5D, a set of 220 consonant string nonword targets (each with an unrelated consonant string prime), 4-6 letters in length, were constructed and used as the nonwords, again in the same manner as in earlier experiments.

Procedure. The procedure was the same as in earlier experiments, except the lexical decision instructions in Experiment 5C were modified so that subjects were asked to decide whether each target spelled a real English word or not. Subjects were tested using a Macintosh G3 computer and a 45 ms SOA.

Results

Each version of Experiment 5 was analyzed separately, and for each mixed-factor ANOVA, priming (repetition vs. unrelated) was the within-subjects factor and repetition proportion (low vs. high) was the between-subjects factor. The obtained means and priming effects for Experiments 5A, 5B, 5C, and 5D appear in Figures 7, 8, 9, and 10, respectively. The complete ANOVA results for each experiment appear in Appendix A (Table A5).

Word targets. The main effect of priming was significant in the response latency analysis of each experiment (only the low repetition proportion group in Experiment 5D, who saw consonant string nonwords, did not produce a reliable priming effect). Experiment 5A replicated the absence of a repetition proportion by priming interaction found in Experiment 3-HF. In contrast, in Experiments 5B, 5C, and 5D, priming interacted with repetition proportion such that priming effects were larger when the repetition proportion was high than when it was low. In the error rate analyses the only reliable effect was a main effect of priming in Experiment 5A and 5B.

Nonword targets. There were no reliable effects in any of the response latency or error rate analyses.

Discussion

Priming for uppercase high-frequency words presented with pronounceable nonwords does not appear to be influenced by repetition proportion, at least as tested here. Even when the Experiment 5A data were pooled with the Experiment 3-HF data, creating 40 subjects per repetition proportion group, the 6-ms difference in priming effect between the low and high repetition proportion groups (41 ms vs. 47 ms) remained far from significant, $F < 1$. In contrast, Experiments 5B-5D make it clear that priming for high-frequency targets can be modulated by repetition proportion under other circumstances. As hypothesized, the interaction emerged when target processing was made more difficult, either by using case-alternated targets (Experiment 5B) or pseudohomophone nonwords (Experiment 5C). Contrary to expectation, however, the interaction also emerged when these targets were made easier to process by using consonant string nonwords (Experiment

5D). Thus, the absolute difficulty of the lexical decision task does not appear to influence the extent to which repetition proportion modulates priming.

The picture that is emerging is one in which repetition proportion influences masked priming in many--but not all--circumstances. The absence of a repetition proportion by priming interaction in Experiments 3-HF and 5A seemed particularly troublesome at first, given that it obtained when the task was made either harder or easier. It was not until the experiments were categorized differently that this pattern--as well as the reason why the interaction failed to obtain in mixed lists in Experiment 2--became interpretable. The rule that was discovered is the following: The interaction obtains when targets tend to be similar in difficulty from trial to trial. In contrast, the interaction does not obtain if the ease of target processing tends vary greatly from trial to trial. If target processing is too variable, the lexical decision task becomes more challenging, and reliance on the primes seems to occur in a way that is independent of the repetition proportion. This variability explanation is now shown to account for the pattern of data found in Experiments 1-5. It is then used to make a novel prediction for the final experiment in Series 1.

Consider first the experiments where the repetition proportion and priming did not interact. In Experiment 3-HF and 5A there was a marked variability in the ease of target processing experienced from trial to trial, namely between the very high-frequency targets and the nonword targets. Similarly, in Experiments 2A and 2B, high-frequency targets and nonword targets again varied in their difficulty, but additionally, low- and high-frequency targets varied greatly from each other. The argument being put forward here is that variability in the ease of target processing from trial to trial of this order was not present in the remaining experiments, where repetition proportion and priming interacted. These are considered next.

In Experiment 1, 154 of the 200 targets were lower in frequency than the high-frequency words used in the rest of the current experiments. Hence, there would tend to be less variability in the ease of target processing from trial to trial in Experiment 1 relative to Experiments 2A and 2B. Experiments 3-LF, 4A, and 4B all used low-frequency targets, which are more difficult to process and hence vary less from the nonwords than do high-frequency targets. High-frequency targets were used in Experiments 5B-D, but critically, in each case the variability in how easy these targets were to process relative to the nonword context was minimized relative to Experiment 5A. In Experiment 5B, variability was reduced by presenting all targets in case alternation, an unfamiliar visual format. In Experiment 5C, variability was reduced by using pseudohomophone nonwords, which ruled out phonology as a basis for lexical decisions. These manipulations minimized variability in ease of processing by making word and nonword targets <u>more difficult</u> to distinguish. By contrast, in Experiment 5D, use of consonant string nonwords minimized variability in the ease of target processing experienced across trials by making word and nonword targets easier to distinguish than in Experiment 5A. High-frequency words and consonant string nonwords are very different from each other, but both types of targets are very easy to process. Hence, in Experiment 5D subjects experienced minimal variability across trials in terms of how easy it was for them to make their lexical decisions.

To summarize, in all experiments where repetition proportion did not interact with priming, there was marked variability in the ease with which targets could be processed from trial to trial, either between high-frequency targets and pronounceable nonwords (Experiments 2A, 2B, 3-HF, and 5A) or between low- and high-frequency targets (Experiments 2A and 2B). In contrast, variability of this magnitude was absent in all experiments where repetition proportion and priming interact, as the majority of word and nonword targets were either relatively difficult (Experiment 1, 3-LF, 4A, 4B, 5B and 5C) or relatively easy (Experiment 5D) to classify.

This variability notion therefore appears to do a good job of accounting for the results from Experiments 1-5. For example, reducing the trial-to-trial variability between words and nonwords in Experiments 5B-5D allowed priming of high-frequency targets to become tuned to prime validity in a way that did not occur in Experiment 3-HF and 5A. Marked variability between processing low- and high-frequency words also provides a plausible explanation for why repetition proportion and priming did not interact when these targets were presented in a mixed-list format in Experiments 2A and 2B. Perhaps the biggest shortcoming of the variability explanation is that, to this point, its account for the present data has been entirely post-hoc. However, given that the lack of interaction of repetition proportion and priming in Experiments 2A and 2B has been attributed to the presence of marked variability, a novel prediction can be made: The interaction should obtain if the probability of high trial-to-trial variability in the mixed-lists used in Experiment 2A and 2B is reduced. Experiment 6 tests this novel prediction.

Experiment 6

Experiment 6 was designed to test the claim that the absence of the repetition proportion by priming interaction in mixed-lists in Experiments 2A and 2B was due to a strong dichotomy in the ease of target processing from trial to trial. To smooth out the trial-to-trial variability that was present in those experiments, a more unimodal distribution was created by replacing a subset of the low-frequency items and a subset of the high-frequency items with a set of medium-frequency items. If the variability account is correct, reducing the probability of sharp trial-to-trial discontinuities in ease of target processing should allow the system to remain sensitive to the repetition proportion of the primes. If so, the interaction of repetition proportion and priming absent in Experiment 2A and 2B should now emerge. The influence of trial-to-trial contingency effects is well-established, having been demonstrated in tasks as wide-ranging as signal-detection (e.g., Ratcliff, Van Zandt, & McKoon, 1999), visual search and memory-scanning (e.g., Strayer & Kramer, 1994a, 1994b), lexical decision (e.g., Gordon, 1983), and priming of word reading (Zevin & Balota, 2000).

After the experiment, subjects were also asked to estimate the repetition proportion they experienced and to rate their confidence in their estimate. If the high repetition proportion group were better able to see or process the primes, for example, they might provide higher and/or more confident estimates than subjects in the low repetition proportion group.

Method

Subjects. Twenty subjects were randomly assigned to each repetition proportion group.

Materials, design, and procedure. Forty-four of the 110 low frequency and 44 of the 110 high frequency prime-target pairs used in Experiment 2 (40 critical and 4 practice per target type) were replaced with a comparable set of 88 (80 critical and 8 practice) medium frequency prime-target pairs constructed from the 30-80 frequency range (median = 45; Kucera & Francis, 1967). The assignment of blocks of items was handled as in Experiment 2, here with 5 blocks of 16 medium items and 5 blocks of 12 items for the low and high frequency sets. The rest of the design and procedure were the same as in Experiment 2, except a 45 ms SOA was used.

Before debriefing, subjects were (as usual) asked what they saw on every trial, just before the target was shown. They were then informed that on each trial, a repetition or an unrelated prime had been presented and that even though they may not have seen these primes, they were to pick a number from 0-100 to reflect the percentage of trials they thought had repetition primes. Subjects were then asked to rate their confidence in their repetition proportion estimate on a scale from 1-10 (1 = completely guessing, 5 = somewhat confident, 10 = completely confident).

Results

Subjects in the high repetition proportion group made nonsignificantly higher repetition proportion estimates than subjects in the low repetition proportion group (60% vs. 52%; $F[1, 38] = 2.20$, $MSE = 314$, $p = .15$) but were not more confident in the accuracy of their estimates (3.4 vs. 3.9; $F < 1$).

Mixed-factor ANOVAs in which frequency (low vs. medium vs. high) and priming (repetition vs. unrelated) were the within-subject factors and repetition proportion (low vs.

high) was the between-subject factor were used to analyze the data. The ANOVA results reported in Appendix A (Table A6) are represented diagramatically in Figure 11.

Word targets. The addition of a set of medium frequency targets resulted in a significant interaction of repetition proportion and priming, favouring the high repetition proportion group, in both the response latency and error rate analyses. Both analyses also revealed a main effect of priming that interacted only with word frequency such that priming increased inversely with target frequency.

Nonword targets. None of the comparable nonword effects were reliable.

Discussion

In Experiment 1, repetition proportion and priming interacted when targets were selected from a wide-range of frequencies. No such interaction was found in Experiments 2A and 2B when targets were also wide-ranging in frequency but were selected from two distinct and separated frequency bands. Large trial-to-trial discontinuities in the ease of target processing were more likely in Experiment 2 than 1, since most of the targets in Experiment 1 were lower in frequency than the very high-frequency words used in Experiment 2. Consistent with the prediction that this sort of variability interferes with a repetition proportion-based modulation of priming, a repetition proportion by priming interaction occurred in Experiment 6 when the probability of discontinuity relative to Experiment 2 was reduced by replacing some of the low- and high-frequency items with a set of medium-frequency items. How trial-to-trial variability might affect sensitivity to prime validity is considered in more detail below.

The interaction of frequency and priming found in Experiment 2A , 2B, and 3 was observed once again in Experiment 6. Prior failures to find this interaction were likely due to use of weaker manipulations (e.g., Bodner & Masson, 1997; Ferrand et al., 1994; Forster et al., 1987; Rajaram & Neely, 1992; Segui & Grainger, 1990; Sereno, 1991). The interaction in Experiment 6 shows that it is not the case that word frequency interacts with priming only when repetition proportion does not, or vice versa. Both effects can be observed in the same experiment provided a strong manipulation of frequency is used and provided that the trial-to-trial variability in ease of target processing is minimized.

If masked primes simply preactivate lexical entries, thus producing priming through a time-saving benefit, then priming effects should be equal to the prime-target SOA (Forster, 1999). The size of some of the priming effects in the low and high repetition proportion groups in Experiments 1-5 cast some doubt on this time-savings prediction. In Experiment 6, for example, the 83-ms priming effect found for low-frequency words when repetition proportion was high was reliably greater than the 45-ms SOA, $t(19) = 3.17$, $p < .01$. While it may be possible to argue that priming exceeds the SOA for low-frequency words because it sometimes takes two passes through the lexicon to locate the entry for these words (cf. Experiment 8, however), it is not clear why two passes would be required when repetition proportion is high but not when repetition proportion is low (where a 48-ms priming effect was observed).

Finally, although the repetition proportion estimates and confidence ratings in Experiment 6 were equivocal, it may prove informative to obtain such data in future experiments where masked or unmasked prime proportion manipulations are used (e.g., Milliken, Lupianez, Debner, & Abello, 1999). At this point in time there is no evidence that subjects in the low and high repetition proportion groups differ in their awareness of the primes.

Comparisons Across Experiments

The experiments in Series 1 comprise twelve sets of data involving 540 subjects. Such a large data set invites closer examination of the overall pattern of priming results collapsed across experiments. The analyses reported in the next three sections take up this invitation, and in doing so make important contributions to our understanding of when and how repetition proportion affects masked priming in the lexical decision task.

Variability and Repetition Proportion

So far, it has proved fruitful to view masked primes as episodic resources whose retrieval and use is determined by (1) the level of prime validity and (2) the level of task difficulty (Bodner & Masson, 1997; Whittlesea & Jacoby, 1990). To account for the full pattern of results reported here, it seems a third factor must be added to this list, namely (3) the level of variability from trial to trial in the ease with which targets can be processed. When variability is minimal, priming is greater when repetition proportion is high than when it is low (Experiment 1, 3-LF, 4A, 4B, 5B, 5C, 5D, 6), and when this variability is greater, priming is equivalent for low and high repetition proportion groups (Experiment 2A, 2B, 3-HF, and 5A).

This section examines the possible ways that variability could undo a repetition proportion-based modulation of masked priming. Having to frequently shift between processing "easy" and "difficult" targets could lead subjects to decrease their use of primes when repetition proportion is high, to increase their use of primes when repetition proportion is low, or both. Given previous demonstrations that reliance on masked primes increases with task difficulty (Bodner & Masson, 1997), the presence of substantial trial-to-trial variability in ease of target processing might be more likely to increase priming. If so, priming should generally be greater in experiments where repetition proportion does not influence priming ("no-interaction" experiments) than in the low repetition proportion groups in experiments where repetition proportion and priming interact ("low repetition proportion" experiments). Moreover, priming effects in no-interaction experiments and in the high repetition proportion groups in experiments where repetition proportion and priming interact ("high repetition proportion" experiments) should be at least of similar magnitude. These predictions are tested separately for high- and low- frequency targets.

For high-frequency targets, the average priming effects found in no-interaction experiments (collapsed across Experiments 2A, 2B, 3-HF, and 4A, and collapsed across repetition proportion) were compared to the average low repetition proportion and high repetition proportion priming effects in experiments where the interaction obtained (Experiments 5B, 5C, and 6). Priming effects were rather small when consonant string nonwords were used, so, working against finding the difference of interest, Experiment 5D was not included in the latter set. Even with Experiment 5D excluded, as predicted no-interaction priming was greater than low repetition proportion priming (40 ms vs. 25 ms), $F(1, 218) = 9.35$, $MSE = 1098$. This result is even more striking because the manipulations in Experiments 5B and 5C made the lexical decision task more difficult and hence, if anything, may have made the reverse pattern seem more likely (cf. Bodner & Masson, 1997; Whittlesea & Jacoby, 1990). That this did not occur implies that trial-to-trial variability can be at least as important a factor as target difficulty in determining the extent to which primes are recruited. Finally, no-interaction priming was not reliably different than high repetition proportion priming (40 ms vs. 48 ms), $F < 2.13$.

For low-frequency targets, the no-interaction average priming effects were based on Experiments 2A and 2B, and the corresponding low repetition proportion and high repetition proportion priming effects were based on Experiments 3-LF, 4A, 4B, and 6. The pattern of results was the same as with high-frequency targets. The average no-interaction priming effect was greater than low repetition proportion priming (70 ms vs. 42 ms), $F(1, 168) = 23.13$, $MSE = 1375$, but was not reliably different than high repetition proportion priming (70 ms vs. 68 ms), $F < 1$.

These results suggest that the presence of marked variability increases priming relative to low repetition proportion, but does not increase priming relative to high repetition proportion. This result would be expected if prime use is at its maximum when repetition proportion is high (i.e., in which case the presence of marked variability would not further increase priming). Alternatively, it may be possible to argue that variability produces a trade-off by making other comparisons across experiments. Trial-to-trial variability might increase prime use when repetition proportion is low, because the task is made more difficult, but it might interfere with optimum use of primes when repetition proportion is high. Unfortunately, differences in experimental design across experiments (e.g., SOA, nonword type) make it difficult to distinguish between these possibilities here.

The analyses presented here are consistent with the claim that the presence of substantial trial-to-trial variability affects prime recruitment and hence the extent to which repetition proportion affects priming. A goal of future research should be to elucidate more precisely the way in which variability and repetition proportion interact. It may be possible to design experiments in which these factors trade-off against each other to produce outcomes different than the ones observed here. For example, use of an even stronger repetition proportion manipulation may allow one to obtain the repetition proportion by interaction even in the context of substantial trial-to-trial variability. Such demonstrations would only bolster the argument that masked priming, like long-term priming and memory-retrieval more generally, is determined by a complex interaction between stimulus, context, and subject-based factors (Whittlesea, 1997).

Bias and Repetition Proportion

Readers appear to rely on a priming resource to a greater extent when that resource is frequently valid than when it is rarely valid. If this argument is correct, increased reliance on primes by the high repetition proportion group should improve performance when the prime turns out to be a repetition prime (80% of the times) and it should hurt performance when the prime turns out to be unrelated to the target (20% of the time). This pattern would imply that the influence of masked repetition primes is primarily or exclusively a form of bias effect, as has been argued for repetition priming with unmasked primes (Ratcliff & McKoon, 1997), and for proportion overlap effects in masked word identification (Allen & Jacoby, 1990; Ratcliff et al., 1989).

Figure 12 plots the mean response latencies (left panel) and error rates (right panel) on repetition and unrelated prime trials for the low and high repetition proportion groups collapsed across all the experiments that produced reliable repetition proportion by priming interactions (Experiments 1, 3-LF, 4A, 4B, 5B-D, and 6; n = 190 per repetition proportion group). In the response latency analysis, the high repetition proportion groups were indeed faster on repetition prime trials than were the low repetition proportion groups, $F(1, 378) = 4.98$, $MSE = 6,323$, but they were not reliably slower on unrelated prime trials, $F < 1$. By itself, this pattern implies that high repetition proportion produces a genuine improvement in performance. However, in the error rates, the opposite pattern was observed. Relative to the low repetition proportion groups, the high repetition proportion groups did not make fewer errors on repetition prime trials, $F < 1.34$, but they did make reliably more errors on unrelated prime trials, $F(1, 378) = 4.25$, $MSE = 21.12$.

The repetition proportion by priming interaction so clearly apparent in Figure 12 was therefore the result of a combination of reduced latencies in the repetition prime condition and elevated errors in the unrelated prime condition in the high repetition proportion groups. This bias effect is apparent only when response latencies and error rates are considered simultaneously, because of the presence of speed-accuracy trade-offs. The high repetition proportion group were not slower than the low repetition proportion group on unrelated prime trials because these trials more often led to errors. In conclusion, high prime validity appears to induce a bias toward increased reliance on retrieved prime episodes, which improves performance on repetition prime trials, but impairs performance on unrelated prime trials.

Adaptation to Repetition Proportion

The question of how rapidly subjects become sensitive to repetition proportion as trials progress is examined in this section. The growth of priming effects for word targets in the low and high repetition proportion groups was assessed by charting the size of the priming effect within each block of 100 critical trials collapsed across all the experiments where repetition proportion and priming interacted. This was done for response latencies as well as error rates; the respective data are displayed in the left and right panels of Figure 13. Two features of the response latency plot stand out: The repetition proportion by priming interaction for words appears early, and it does not change systematically across blocks. An ANOVA of mean response latencies that included critical block as a factor revealed that the interaction of repetition proportion and priming did not interact with block, $F < 1$. Unfortunately, data were not collected during the practice trials in most of the current experiments, so the interim conclusion is that the interaction emerges either in the 40 trials in the practice block or within the first 100 trials of the critical block. There were no systematic trends or speed-accuracy trade-offs in the error rates.

The rapidity and stability with which the repetition proportion by priming interaction appears in the response latencies suggests two possibilities. One is that manipulating the repetition proportion within the 40 trial practice block may have been a critical design element. This "concentrated shot" of low or high repetition proportion may have set some global level of prime use that remained constant across the 400 trial critical block. The second possibility is that use of primes is very locally determined, perhaps by the nature of the preceding trial or window of trials.

Although these possibilities remain to be systematically investigated, evidence from other studies suggests that the repetition proportion of the local context (trial N-1 or a window of recent trials) may determine the extent to which primes are recruited (e.g., Joordens, Piercey, & Mohommad, 2000; Ratcliff et al., 1999; Strayer & Kramer, 1994b). For example, rapid adaptation to a proportion manipulation occurred in a study by Ratcliff et al. (1999). Their subjects judged whether a display of asterisks had been drawn from a distribution having more vs. fewer asterisks. Across blocks, the probability that a display was drawn from one distribution or the other was switched (e.g., .8 vs. .2). The probability that subjects gave the preferred response after a switch in stimulus probability came to match that probability within 14 trials after a switch, and was well on the way within only 5 trials after a switch. In the word recognition domain, Joordens et al. (2000) found that subjects in a lexical decision task adapted within an 8 trial (4 word, 4 nonwords) mini block to a sudden change in nonword type.

Series 2: Manipulations of Relatedness Proportion

Series 1 successfully demonstrated that masked repetition priming is influenced by the proportion of repetition primes in the stimulus list. One of the major implications of this result is that it suggests that other proportion effects, particularly those found with semantic primes, may not always be the result of subjects using a conscious expectancy or semantic matching mechanism. Alternatively, however, it might simply mean that the masked repetition priming paradigm recruits different processes than are recruited in tasks involving semantic primes. The experiments in Series 2 employed masked semantic primes to allow these alternatives to be distinguished.

It is certainly the case that masked priming occurs in the lexical decision task when related rather than repeated prime-target pairs are used. Sereno (1991) found a 41-ms effect with a 60-ms SOA, de Groot and Nas (1991) found effects of 20-51 ms with a 60-ms SOA (in the within-language conditions), and Perea and Gotor (1997) found effects of 11-14 ms with a 67-ms SOA. The big question is whether prime validity can modulate the extent to which semantic primes are used to help guide target processing.

As reviewed earlier, though well established at longer SOAs, the evidence concerning whether relatedness proportion and priming interact at shorter SOAs is mixed. Interestingly, studies that have found an interaction used 240-ms SOAs and strong manipulations of relatedness proportion (de Groot, 1984, and Henik et al. 1994). In contrast, studies that have not found the interaction used either a much shorter SOA (den Heyer et al, 1983; 75-ms SOA) or a weak manipulation of relatedness proportion (Stolz & Neely, 1995; .25 vs. .50). This issue aside, no published studies have examined whether relatedness proportion and priming interact when primes are masked to prevent use of conscious strategies.

From the episodic resource-retrieval perspective, primes are more likely to be recruited when the validity of the priming resource is higher. To the extent that the semantic information is encoded into the episode for a masked prime, this resource will be more valid when relatedness proportion is .8 than when it is .2; hence an interaction is predicted. However, given previous arguments that episodes for masked primes contain mostly phonological and/or orthographic information (Bodner & Masson, 1997; Masson &

Isaak, 1999), it may be the case that the difference in prime validity on unrelated and related trials is too small to drive greater use of the resource when relatedness proportion is high.

The lexical entry-opening account was never intended to address how semantic priming occurs, although it does not predict any influence of relatedness proportion on masked priming (Forster, 1998). The expectancy and semantic matching strategies thought to account for why priming increases with relatedness proportion at longer SOAs are not hypothesized to operate when SOAs are short, let alone when primes are masked (e.g., Neely, 1991). Therefore, these conscious strategy accounts do not predict an interaction of relatedness proportion and masked semantic priming. Simply put, subjects cannot consciously use what they cannot consciously see (Forster, 1998).

Experiment 7

Experiment 7 provided an initial test of whether masked semantic priming would be affected by the proportion of related prime-target trials in the stimulus list. For this test, a low relatedness proportion group received related primes on .2 of the word trials, and a high relatedness proportion group received related primes on .8 of the word trials. For both groups, unrelated primes preceded the remaining word targets. For compatibility with earlier studies of relatedness proportion effects on semantic priming with unmasked primes, word primes preceded all nonword targets.

Method

Subjects. Fifty subjects were randomly assigned to each relatedness proportion group.

Materials and design. The materials used in Experiment 1 were actually designed for use with related primes, hence the materials in Experiment 7 were comprised of most of the same targets used in that experiment. Target frequencies ranged from 1-2216 (median = 34; Kucera & Francis, 1967), and were positively skewed as in Experiment 1 (152 of the 200 targets had frequencies less than 100). Each word target was paired with a prime with a related meaning. The nature of the relationship between prime and target varied considerably (e.g., bread-BUTTER, robin-BIRD, dusk-DAWN, plush-VELVET) and was not of interest in this study. For simplicity, the term "semantic" is used throughout. Each word target was also paired with an unrelated prime; these were assigned by repairing the semantic primes within each of the 5 blocks of 40 items used for counterbalancing. Nonwords were always preceded by word primes. Primes and targets ranged in length from 3-8 characters. For both related and unrelated pairs, the number of letters in primes and targets, the number of letters that primes and targets shared, and the number of letters that primes and targets shared in the same position were not controlled.

Procedure. The procedure was much the same as in earlier experiments. However, primes and targets were often of different lengths, so to minimize prime perceptibility a row of 10 Xs was used as a pre-mask. To make sure the lowercase primes were effectively postmasked by their uppercase targets, ampersands were presented on either side of the target so that all targets were 10 characters wide (e.g., &&VELVET&&, &&&&DAY&&&). This masking procedure in conjunction with a 45-ms SOA ensured that the primes were not easy to detect. The assignment of blocks of items was handled as in earlier experiments.

After the experiment, subjects were asked what they saw on every trial, just before the target was shown. The experiments in Series 2 were conducted before Experiment 6, so subjects were not asked to estimate the relatedness proportion or to rate their confidence in their estimate.

Results

The majority of subjects in the low and high relatedness proportion groups indicated that they were not aware that primes had been presented between the mask and target (74% vs. 68%). Only 5 of the 100 subjects reported being aware that some primes were related in meaning to their targets.

Mixed-factor ANOVAs in which priming (related vs. unrelated) and relatedness proportion (low vs. high) were the within- and between-subject factors were used to analyze the word data. One factor ANOVAs with relatedness proportion (low vs. high) were used to analyze the nonword data. These ANOVA results appear in Appendix A (Table A7). Figure 14 displays the relevant word data.

Word targets. Responses were a significant 20-ms faster following related primes than following unrelated primes, but this effect was not reliably greater when relatedness proportion was high (24 ms) than when it was low (16 ms). Only the main effect of priming was significant in the error rate analysis.

Nonword targets. The average response latency for nonwords was 769 ms. Their average error rate was 6.3%.

Discussion

This experiment replicated the basic finding of masked semantic priming in the lexical decision task (de Groot & Nas, 1991; Perea & Gotor, 1997; Sereno, 1991). For the episodic account, this finding implies that episodes must contain some semantic information that can facilitate target processing. These effects are smaller than those seen with related primes, supporting the claim that episodes for masked primes contain primarily orthographic and/or phonological information (e.g., Bodner & Masson, 1997; Masson & Isaak, 1999). Most importantly, even with a sample size much larger than in many of the earlier experiments, masked semantic priming was not reliably modulated by relatedness proportion. This null result supports the claim that relatedness proportion and priming do not consistently interact at short SOAs because conscious processes such as expectancy and semantic matching do not have enough time to operate (Neely, 1991; Neely & Keefe, 1989). Controlled strategies are even less likely to have been used by subjects in Experiment 7 because primes were masked and subjects were typically unaware of their presence (Forster, 1998).

Experiment 8

It is tempting to conclude--based on Experiment 7--that prime validity modulates masked repetition priming but not masked semantic priming, and hence that the findings from Experiments 1-6 with repetition primes have no real bearing on accounts of semantic priming. On the other hand, repetition and semantic priming are not just different kinds of priming, they also differ in terms of strength. Relative to a relationship of identity, the semantic relationships between primes and targets in Experiment 7 may have been too weak for use of masked primes to become tuned to the validity of this type of prime. Moreover, even if prime-target relationships were strong, 45 ms may be too little time for subjects to encode enough semantic information into the episodes for masked primes to drive an interaction between prime validity and priming.

Given that prime validity affects priming with repetition primes, a modulation of semantic priming might be observed if the relatedness proportion manipulation was strengthened. One way of accomplishing this is by replacing most of the semantic prime trials in the high relatedness proportion group with repetition primes. In Experiment 8, rather than the high relatedness proportion group receiving semantic primes on 80% of trials, these subjects received repetition primes on 60% of trials and semantic primes on 20% of trials. Thus, in Experiment 8, relatedness proportion refers to the combined proportion of related and repetition prime trials in the stimulus list.

This manipulation creates an interesting test of what exactly is being "tuned into" when prime validity is high, according to the episodic resource-retrieval framework. If the high relatedness proportion group becomes sensitive to the dominant type of prime validity, then semantic priming in the high relatedness proportion group would not benefit from the presence of repetition primes. Alternatively, the presence of repetition-primed targets might bootstrap this group's use of primes on semantic-primed trials. This latter prediction is supported by the finding that increases in prime validity create a general bias toward greater reliance on recruited primes regardless of their relation to the target (see section on "Bias and Repetition Proportion"). In contrast, explanations of proportion effects that rely on subjects using expectancy or semantic matching strategies would still not predict a difference in semantic priming between the low and high relatedness proportion groups.

The strategy of "souping up" the lexical decision task to maximize chances of finding priming effects was used successfully in a study of long-term semantic priming by Joordens and Becker (1997). In their fourth experiment, these researchers showed that the chances of finding reliable semantic priming effects across a lag of eight intervening targets was increased if the task was modified in ways that produced deeper processing of targets (e.g., by using pre-exposed pseudohomophones as the nonwords). As described below, the masked lexical decision task in Experiment 8 was modified to increase the strength of the prime validity manipulation and hence to increase the chances of finding a semantic effect at the short end of the time scale.

Method

Subjects. Twenty subjects were randomly assigned to each relatedness proportion group.

Materials, design, and procedure. This experiment was different from Experiment 7 in two ways. First, in the high relatedness proportion group, rather than assigning related primes to four of the five blocks of words (i.e., 80%), one block (20%) was assigned related primes and three (60%) were assigned repetition primes. The remaining block received unrelated primes. In the low relatedness proportion group, one of the five blocks (20%) received related primes and the rest (80%) received unrelated primes. Second, nonwords in the low and high relatedness proportion groups were assigned repetition and unrelated primes as in Experiments 1-6, rather than diluting the relatedness proportion manipulation by using all word primes as was done in Experiment 7. Both of these changes were intended to make the manipulation of prime validity more robust than it was in Experiment 7.

Results

Prime awareness was comparable to that in earlier experiments; 80% of the low relatedness proportion group and 75% of the high relatedness proportion group reported only seeing the row of Xs before targets were presented. For word targets, semantic priming was investigated using mixed-factor ANOVAs in which priming (related vs. unrelated) and relatedness proportion (low vs. high) were the within- and between-subject factors. Repetition priming (repetition vs. unrelated) for word targets in the high relatedness proportion group was assessed using repeated-measures ANOVAs. For nonword targets, repetition priming was analyzed using mixed-factor ANOVAs in which priming (repetition vs. unrelated) and repetition proportion (low vs. high) were the within-and between-subject factors. The ANOVAs for these results are presented in Appendix A (Table A8), and the means and priming effects are graphed in Figure 15.

Word targets. As in Experiment 7, there was a 20-ms main effect of semantic priming. Unlike in Experiment 7, however, use of a stronger relatedness proportion manipulation resulted in semantic priming being reliably greater when prime validity was high than when it was low (34 ms vs. 7 ms). Post-hoc tests revealed that semantic priming was significant when relatedness proportion was high, $F(1, 19) = 19.30$, $MSE = 588$, but not when relatedness proportion was low, $F < 1$. The latter result was likely due to the small sample size; the power to detect a priming effect of the same magnitude as that found in the low relatedness proportion group in Experiment 7 (16 ms) was only .11. The 71-ms repetition priming effect in the high relatedness proportion group was highly significant, and was in fact greater than the 45-ms SOA, $t(19) = 3.18$, $p < .01$. None of the error rate analyses were significant.

Nonword targets. Nonword responses were faster in the high relatedness proportion group, producing a main effect of relatedness proportion. The repetition priming effect for nonwords was significant; the interaction with repetition proportion did not reach significance. None of the error rate analyses were significant.

Discussion

Bolstering the relatedness proportion manipulation in Experiment 8 relative to Experiment 7 resulted in an interaction of overall relatedness proportion and semantic priming, an effect seldom found with SOAs less than 250-ms, and never before found with masked primes. This effect is consistent with increased recruitment of masked prime episodes when the manipulation of prime validity is sufficiently strong. The finding that prime validity effects occur with masked semantic primes under these conditions calls into question the generality of the claim that relatedness proportion by priming interactions are mediated by use of conscious strategies such as expectancy or semantic matching (e.g., Neely, 1991; Neely & Keefe, 1989).

Previous null effects of relatedness proportion at shorter SOAs may have been due to limitations in experimental design (den Heyer et al., 1983; Stolz & Neely, 1995). Stolz and Neely (1995) failed to find an interaction with a 200-ms SOA, but their relatedness proportion manipulation was weak (.25 vs. .50) relative to that used in the present experiments (.2 vs. .8). In the den Heyer et al. (1983) study, no interaction was found with a 75-ms SOA, but several aspects of their design render their conclusion questionable. First, their relatedness proportion manipulation was weaker than in the current experiment. When one calculates the relatedness proportion in their experiments in the usual manner (i.e., the proportion of word targets with related primes), the relatedness proportions were

.08, .33, and .58 in their Experiment 2, and .06, .25, and .44 in their Experiment 3 (rather than being .13, .50, and .88 as the article implies). This study also likely had weak power, because the related-prime mean response latency in the low relatedness proportion group and the unrelated prime mean response latency in the high relatedness proportion group were based on a maximum of 8 trials per subject. Finally, this study used an atypical 2:1 ratio of word to nonword targets, creating a bias toward making "word" responses, which would serve to minimize priming. Thus, prior studies of null relatedness proportion by priming interactions are not fully convincing.

Relative to Experiment 7, the relatedness proportion manipulation in Experiment 8 was strengthened two ways: by replacing most of the related primes in the high relatedness proportion group with repetition primes, and by manipulating the repetition proportion for nonwords. Thus, there are two possible reasons why the interaction of relatedness proportion and priming obtained in Experiment 8. While both changes may have contributed, some unpublished data suggest that varying the repetition proportion of nonword targets may not have mattered. An additional experiment in which the repetition proportion of nonwords was varied as in Experiment 1-6 was abandoned after nearly 20 subjects per group because it similarly failed to produce a relatedness proportion by priming interaction for word targets. Even so, it may be possible to obtain the interaction without either of these changes, for example through a more careful selection of related primes. For present purposes though, the main point of Series 2 remains, namely that masked semantic priming can be modulated by relatedness proportion.

Before leaving this section, the finding that repetition priming in the high relatedness proportion group exceeded the SOA deserves brief comment. Recall that a strong prediction of the lexical entry-opening account is that masked repetition priming should not exceed the SOA (Forster, 1999). This prediction was disconfirmed in Experiment 6, but there it could be argued that priming for low-frequency words exceeded the SOA because more than one pass through the lexicon may be necessary in order to locate the correct entry for these items. In Experiment 8 word targets ranged widely in frequency (though the distribution was positively skewed), and a regression analysis revealed that word frequency and the size of the priming effect were not correlated, $r(198) = .05$, $p = .51$. The priming-as-time-savings notion appears to be incorrect.

General Discussion

The present experiments show that under a variety of conditions, masked priming of words in the lexical decision task is influenced by the validity of the primes, operationalized as the proportion of repetition (or related) prime trials in the stimulus list. When this proportion was .80 rather than .20, more word priming was obtained: (1) when word frequency varied continuously over a wide range (Experiment 1), (2) when only low-frequency word targets were used (Experiments 3-LF, 4A, and 4B), (3) when only high-frequency word targets were used and when these targets were made more similar in terms of processing difficulty to the nonword context (Experiments 5B, 5C, and 5D), (4) when low-, medium-, and high-frequency word targets were mixed within a list (Experiment 6), and (5) with semantic primes (Experiment 8; cf. Experiment 7, however). Repetition proportion by priming interactions were obtained with 45-ms and 60-ms primes, with uppercase and case-alternated targets, and when pronounceable letter strings, pseudohomophones, or consonant strings were used nonwords. In contrast, repetition priming was not reliably affected by repetition proportion: (1) when word targets consisted of two highly distinct frequency bands (Experiments 2A and 2B) or (2) when pure lists of high-frequency word targets were distinctly easier to process than their nonword counterparts (Experiment 3-HF and 4A).

Word recognition thus appears to be highly sensitive to the nature of the prime context (i.e., prime validity), and the involvement of primes in target identification appears to depend on the similarity of targets in terms of processing difficulty. Use of the prime resource appears to not be influenced by prime validity only when targets in the stimulus list tend to vary greatly in how easy they are to respond to from one trial to the next. As pointed out in the introduction, the idea that word recognition is influenced by list context factors is not a novel one, but the present results indicate that this influence extends even to the masked repetition priming paradigm in which subjects typically report being unaware that a prime has been presented (e.g., Bodner & Masson, 1997; Forster & Davis, 1984). As described in the next section, the present results, including the finding of an interaction between word frequency and masked priming (Experiments 2A, 2B, 3, and 6), fit well with an episodic account of masked priming, even though the masked priming paradigm was originally designed to rule out the contribution of episodic processes. The implications of these results for other accounts of word recognition are then considered.

Episodic Resource Retrieval and Priming

The episodic account described in this paper posits that word recognition phenomena and memory phenomena may reflect the operation of common underlying mechanisms (e.g., Whittlesea, 1997; Whittlesea & Jacoby, 1990). Both classes of phenomena require that some processing is applied to an initial stimulus or event, and that this processing can later be used (i.e., retrospectively) to help guide the processing of a subsequent stimulus or event. Previous work has supported the idea that episodes or processing resources are constructed even for primes that are masked and presented too briefly for subjects consciously to detect (Bodner & Masson, 1997; Masson & Isaak, 1999). Such a claim is also consistent with research on the mere exposure effect, where preference judgments for novel geometric shapes can be influenced by 1-ms pre-exposures that subjects are not consciously able to encode or retrieve (e.g., Kunst-Wilson & Zajonc, 1980). The present work further proposes that the retrieval and use of priming episodes can be contingent on their validity--the probability that they contain information that will be useful for target processing.

The modulation of masked repetition and semantic priming by the validity of the prime context is consistent with an episodic account. When the prime context was more valid, priming effects were usually larger, consistent with the claim that more use was being made of the priming episodes. Moreover, word recognition appears to be highly sensitive to prime validity, as demonstrated by the early and strong influence of repetition proportion on priming (Figure 13). Also in line with this proposal was the finding that greater reliance on the prime context produced a form of bias effect. Relative to the low repetition proportion group, lexical decisions made by the high repetition proportion group were facilitated on repetition prime trials (faster response latencies), where greater reliance on the prime was an asset, and were impaired on unrelated prime trials (higher error rates), where greater reliance on the prime was a liability (Figure 12). These results link well with the bias-like influence of a similar episodic context manipulation, proportion overlap, on priming in studies of masked word identification (Allen & Jacoby, 1990; Jacoby, 1983).

The most challenging aspect of the present data for any account to explain is that repetition proportion did not consistently modulate the size of the priming effect. As described in the next section, the presence or absence of trial-to-trial discrepancies in the ease of target processing may provide a useful way of distinguishing those experiments that produce an interaction from those that do not. The episodic framework already posits that use of primes is dependent on contextual factors such as prime validity and target difficulty. Therefore, the discovery that another contextual variable--variability--might also affect the extent to which primes are recruited fits into an episodic framework quite naturally.

That masked semantic priming effects occur (Experiment 7 and 8; de Groot & Nas, 1991; Perea & Gotor, 1997; Sereno 1991), but are typically smaller than repetition priming effects supports previous claims that episodes for masked primes primarily but not exclusively contain orthographic and/or phonological information (Bodner & Masson, 1997; Masson & Isaak, 1999). That it was necessary to strengthen the high relatedness proportion manipulation with repetition primes for masked semantic priming to be affected (Experiment 8 vs. Experiment 7) is consistent with the primacy of orthographic or phonological information in driving masked priming effects. Although the interaction in Experiment 8 poses problems for semantic priming accounts that attribute such interactions to use of conscious strategies such as expectancy, an episodic account of relatedness proportion effects with visible semantic primes may well be viable (Bodner, 1997).

Moreover, the fact that repetition proportion often modulated priming of nonwords in these experiments (when 60-ms primes were used; Experiments 1, 2A, 3, and 4A) as it did for words naturally fits within the episodic account, because it is assumed that episodes are formed for both kinds of items. The occurrence of masked nonword priming suggests that familiarity following a nonword repetition prime does not always works against making a nonword response (cf. Feustel et al., 1983; Salasoo et al., 1985). The apparent speed-accuracy trade-off for nonwords in Experiment 3, however, prevents strong conclusions from being drawn, as does the absence of nonword priming when 45-ms primes were used (Experiments 4B, 5A, 5B, 5C, 5D, and 6; but Experiments 2B and 8 are exceptions). Clearly the use of priming episodes is more variable across subjects and/or conditions in the case of nonwords than in the case of words. This seems natural given that the task of processing multiple nonwords in these experiments is a relatively unusual activity and may invoke a wider variety of processing solutions than in the case of word targets.

In concluding this section, a few other ways of conceptualizing an episodic account are mentioned, particularly because this account has been couched only in rather general terms thus far. First, Bodner and Masson's (1997) episodic account implies that the construction of the prime's episode is terminated when the target is introduced, but this may not be correct. As an alternative, Masson and Isaak (1999) suggested that perhaps the masked prime is still being processed when the target is presented. If so, the processes currently being applied to the prime and those that will be employed to identify the target may be integrated on repetition prime trials, given their high degree of similarity, thus producing priming via efficient target processing (e.g., Morris, Bransford, & Franks, 1977). A related idea is that rather than an episode being retrieved, per se, priming of targets may occur through the fluent reapplication of the processing applied to the prime. The extent to which this reapplication occurs is partially stimulus bound, but would also depend on factors such as the instructions, the subject's goals, and--critically for present purposes--the prime and target contexts (e.g., Whittlesea, 1997).

Trial-to-trial Variability

From the episodic resource-retrieval account, the finding that repetition proportion did not always modulate priming led to the postulation of a third factor, in addition to prime validity and target difficulty, that influences the probability or extent to which primes are recruited. Where the interaction did not obtain, there was a relatively high probability that the targets subjects encountered from one trial to the next would vary greatly in terms of how easy they were to process. In Experiments 2A, 2B, 3-HF, and 4A, high-frequency words and nonwords differed markedly in how easy they were to process. In the Experiments 2A and 2B, high- and low-frequency words within a list were also very different in terms of how easy they were to process.

In the section on "Variability and Repetition Proportion" it was argued that discrepancies in the remaining experiments, where repetition proportion and priming interacted, were less severe or less frequent. For example, the interaction obtained in Experiment 5B-D, when three different manipulations made high-frequency words and nonwords more similar in terms of processing difficulty than they were in Experiments 3-HF and 5A. Moreover, the variability notion successfully predicted the outcome of Experiment 6. The prediction was that reducing the probability of encountering widely variable low- and high-frequency targets from trial to trial in Experiment 2A and 2B would allow the interaction--which was absent in those experiments--to emerge. This prediction was confirmed in Experiment 6 by replacing a subset of low- and high-frequency targets from Experiment 2 with a set of medium-frequency targets.

It appears that two kinds of variability in the ease of target processing across trials must be distinguished: (1) between words and nonwords, and (2) between subsets of words (e.g., low vs. high frequency). If only the first type was important, then the repetition proportion by priming interaction in Experiment 6 should not have obtained, because high-frequency words and nonwords were present in that experiment and in Experiments 2A and 2b, where the interaction was not present. The second type of variability is also needed to explain why the interaction obtained in Experiment 5B, but not in Experiment 2B. It was argued that the interaction obtained in Experiment 5B because use of case-alternated targets reduced the variability in ease of processing that existed between high-frequency and nonword targets that was present in Experiment 5A. However, case-alternated targets were also used in Experiment 2B, which should have also reduced the variability between these two types of targets, yet no interaction was observed. It must be speculated, then, that even though use of case-alternation in Experiment 2B reduced the variability between words and nonwords, it did not undo the strong variability in ease of target processing between low- and high-frequency words (to do this, a manipulation such as the one used in Experiment 6 is necessary).

From an episodic resource-retrieval account, there is some evidence to suggest that the presence of marked variability increases the likelihood of prime recruitment. Where variability was great, and the interaction of prime validity and priming did not obtain, priming effects for low- and for high-frequency words tended to be: (1) larger than in the low repetition proportion groups where the interaction obtained, and (2) about the same size as that found in the high repetition proportion groups where the interaction obtained.

This pattern implies either that the presence of marked variability cannot further boost priming in the high repetition proportion group, or that the effects of repetition proportion and variability trade-off such that variability boosts priming when the repetition proportion is low, but interferes with priming when the repetition proportion is high (see section on "Variability and Repetition Proportion"). The conditions under which repetition proportion and variability will interact clearly merit further investigation.

Implications for the Lexical Entry-Opening Account

The lexical entry-opening account of priming states that priming is produced by preactivation of the target's lexical entry by a repetition prime. The locus of priming is held to be prospective and reveals itself in the amount of time saved by not having to open the entry "from scratch" when the target word appears. The lexical account has taken three null effects as strong support: (1) the lack of proportion effects on masked priming, (2) the finding of equivalent masked priming for low- and high-frequency words, and (3) the absence of masked nonword priming (Forster, 1998; Forster & Davis, 1984). Despite continued reports of masked nonword priming (e.g., Bodner & Masson, 1997; Forster, 1985; Masson & Isaak, 1999; Sereno, 1991), Forster (1998) has maintained that masking primes eliminates the opportunity for episodic processes to influence target responses.

Taken together then, the repetition proportion by priming and frequency by priming interactions reported here, along with further examples of nonword repetition priming provide strong evidence that masked priming effects can no longer be clearly dissociated from long-term priming effects. Automatic opening of a lexical entry should occur to the same extent independently of prime validity, independently of a target word's frequency, and should not occur for nonwords which do not, by definition, have lexical entries. In addition, the lexical account's prediction that the size of the priming effect should not exceed the prime-target SOA is not always upheld (e.g., Experiments 6, 8).

Non-lexical processes can powerfully affect masked priming and thus reflect normal word recognition. It may be possible to argue that these results merely suggest that the lexical processor is far more adaptive or complicated than was previously believed. However, as acknowledged by Forster (1999, p. 14), by making concessions or adding mechanisms, the lexical entry-opening model--having already lost the grounds for distinguishing episodic and lexical effects--will also lose parsimony.

Implications for Accounts of Semantic Priming

The prime validity effects reported here also have important implications for accounts of semantic priming. The high relatedness proportion advantage in semantic priming with unmasked primes has been more commonly observed at SOAs of 500 ms or more (e.g., Bushell, 1996; den Heyer, 1985; den Heyer et al., 1983; Seidenberg et al., 1984; Stolz &

Neely, 1995; see de Groot, 1984, and Henik et al., 1994, for exceptions). This trend led Neely and colleagues to claim that interactions of relatedness proportion and semantic priming are produced by either expectancy or semantic matching (e.g., Neely, 1991; Neely & Keefe, 1989; Neely, Keefe, & Ross, 1989). Both of these processes are strategic in nature and depend on subjects being aware of the primes and having enough time to use them. Although these processes were never intended to explain masked repetition priming, they clearly would not provide a satisfactory explanation of why repetition proportion can affect masked repetition priming. More importantly, use of conscious strategies could not explain the interaction of relatedness proportion and masked semantic priming found in Experiment 8. These interactions therefore provide an important cautionary message: Finding that an effect can be produced by a conscious strategic process does not mean that it cannot also (or alternatively) be produced by an unconscious non-strategic process.

Experiment 8 suggests that use of conscious strategies may not always underlie relatedness proportion by priming interactions. However, another way that relatedness proportion might modulate priming effects was described by Stolz and Neely (1995). Their account makes use of between-level feedback in a multi-stage activation model (see Besner & Smith, 1992, for details). In this model, a semantic prime activates its entry in an orthographic lexicon, which then feeds forward to activate its entry in a semantic module. Activation spreads to related words in the semantic module, providing one locus for semantic priming. Activation from related words that have been activated in the semantic module can also feed backward to activate the entries for these related words in the orthographic lexicon, providing a second locus for semantic priming. If one assumes that this feedback occurs only when relatedness proportion is high (to conserve activation in the system), then interactions between relatedness proportion and priming are possible.

This feedback account may provide a useful way of conceptualizing prime validity effects with related primes. It can also account for prime validity effects with repetition-primed words, since feedback from a repetition prime would also activate the target's entry in the orthographic lexicon. However, nonword repetition priming, particularly when it is modulated by repetition proportion (with 60-ms primes; Experiment 1, 2A, 4A), would have to be explained a different way, as nonwords should have no representations in the orthographic or semantic module. How feedback could produce interference effects on unrelated prime trials (Figure 12) is also unclear. Moreover, feedback would have to be made dependent on the presence or absence of variability, to explain why repetition proportion by priming interactions sometimes do not obtain. Still, researchers vested in distinguishing word recognition phenomena from memory phenomena might well wish to pursue this as an alternative to the episodic resource-retrieval account.

Finally, some recent semantic priming results reported by Neely and colleagues merit further consideration, as they appear to be problematic for both prospective (e.g., Neely & Keefe, 1989) and retrospective (e.g., Ratcliff & McKoon, 1988) accounts of semantic priming. Neely, VerWys, and Kahan (1998) reported that masked repetition priming of an unmasked prime eliminates semantic priming, so that priming was found for wave-SALT-pepper but not for salt-SALT-pepper. This result is unexpected from a spreading activation account (e.g., Posner & Snyder, 1975) for example, because if anything, more activation should spread in the second case. Using the same paradigm, Neely and VerWys (1996) also found that masked repetition priming of a prime increases repetition priming, so that priming was greater for salt-SALT-salt than for wave-SALT-salt. These two studies suggest a dissociation between semantic and repetition priming (also, see Bushell, 1996). It remains to be seen whether the episodic resource-retrieval account--or the notion of variability--can help explain these intriguing results. For now, results such as these at a minimum amplify the lesson highlighted in the present work, namely that word recognition can be dramatically influenced by subtle changes in context.

Other Potential Explanations for Prime Validity Effects

It may be possible to bring two other theoretical frameworks to bear upon the present results. The first of these is the criterion model of lexical decision performance (e.g., Balota & Chumbley, 1984; Balota & Spieler, 1999; Gordon, 1983). In these models, because word and nonword distributions are somewhat separable along a familiarity/meaningfulness dimension, efficient lexical decisions might be made by appropriately placing criteria along this dimension. Targets that lie above an upper criterion are likely words, and targets that fall below a lower criterion are likely nonwords, so fast and typically accurate responses can be made to both types of items. Targets that fall between these two criteria require more analytic processing, and lead to slower lexical decisions. Priming occurs in the model by increasing familiarity, shifting the distribution for related prime trials above the distribution for unrelated prime trials, so that for words, more fast decisions will occur on the former trial type. This model can also account for frequency-blocking effects in the lexical decision task (e.g., Gordon, 1983), but not when analogous effects occur in the word naming task, where responses cannot be made based on familiarity alone (e.g., Jared, 1997; Lupker et al., 1997).

Unfortunately, the criterion model does not address how prime proportion manipulations would influence lexical decisions. Most likely, high repetition proportion could cause the criteria to shift (e.g., by moving the decision criteria closer together), but it could instead affect the placement of the distributions (e.g., by increasing the familiarity of the related prime distribution and decreasing the familiarity of the unrelated prime distribution), or it could do both. It is not clear that any of these possibilities would produce the observed outcomes. For example, the first two suggestions amount to the same thing, and would seem to imply that both related and unrelated prime trials should speed up when repetition proportion is high, which would produce a main effect of repetition proportion rather than the observed interaction with priming. How this model could account for positive nonword repetition priming is also unclear. The distribution for repetition-primed nonwords would be higher on the familiarity continuum than that for unrelated-primed nonwords, which should lead to more fast nonword responses on unrelated trials relative to repetition trials (i.e., negative priming) no matter where the criteria are placed. Whether the criterion model can explain nonword priming or how and when repetition proportion by priming interactions occur remains to be demonstrated.

A second framework that may potentially prove useful in understanding prime validity effects is the diffusion model (e.g., Ratcliff, 1978; Ratcliff et al., 1999; Strayer & Kramer, 1994a, 1994b). Performance in binary decision tasks can be modeled as a random walk process in which information accumulated over time from a starting point until one of two response boundaries is reached. These models have been used to successfully account for all aspects of reaction time data in signal-detection paradigms, for example (Ratcliff et al., 1999). They can also explain differences between task performance in blocked and mixed designs (e.g., Strayer & Kramer, 1994a, 1994b), by varying where subjects place the response boundaries relative to the starting point. However, the diffusion model has not yet been extended to account for priming effects in the lexical decision task. Priming might increase the drift rate--the rate with which evidence is accumulated for an item, for example. Less clear is how proportion by priming interactions could be modeled. It may be possible to model such effects in terms of increases in drift rate when repetition proportion is high, or movement of the response boundaries closer to the starting point, or both. Of course, this model, like any other, would also have to explain why interactions of repetition proportion occur under some conditions but not others. Trial to trial variability might be modeled as variation in the mean drift rate from trial to trial, in the starting point, or in the boundary position placement, for example. Finally, because both the diffusion model and criterion model are meant to account for performance in binary decision tasks, neither could account for validity effects with masked primes in the word reading task. Whether such effects occur is thus an important question for future research to consider.

The Status of Masked Nonword Priming

In his review of the "pros and cons" of masked priming, Forster (1998) offered alternative explanations for Bodner and Masson's (1997) nonword priming effects with case-alternated nonwords, pseudohomophone nonwords, and consonant string nonwords, and for Masson and Isaak's (1999) nonword priming effects in the word reading task. In each case, nonword priming in these situations was attributed to the operation of a process not normally used in word identification. Even if each of these alternative accounts is shown to be correct, masked nonword priming does sometimes occur when normally presented legal nonwords are used in the lexical decision task (e.g., Experiment 1, 2A, 4A, 8; Forster, 1985; Sereno, 1991), but admittedly, this is not often the case. Rather than exhaustive arguing and counter-arguing, a more fruitful strategy is to try to identify conditions under which masked nonword priming consistently occurs.

The current experiments suggest that SOA and prime validity may be key factors. Masked nonword priming was often reliable when repetition proportion was high and primes were presented for 60 ms (Experiments 1, 2A, and 4A), but it did not occur at all (Experiments 4B, 5A, 5B, 5C, 5D, 6) or was not influenced by repetition proportion (Experiments 2B, 8) when primes were shown for only 45 ms. The importance of SOA is highlighted by a dramatic contrast between the results of Experiment 5B, 5C, and 5D here and the results of Experiments 2B, 3, and 4 in Bodner and Masson (1997). Using 60-ms primes, Bodner and Masson (1997) found significant nonword priming with case-alternated nonwords, pseudohomophone nonwords, and consonant string nonwords. Using the same three manipulations but with 45-ms primes, none of the nonword priming effects in Experiments 5B, 5C, and 5D were significant. That nonword priming occurred in Experiment 4A (60 ms SOA) but not in Experiment 4B (45 ms SOA) also speaks to the importance of SOA. Although Bodner and Masson (1997) emphasized that masked nonword priming should be more likely when word/nonword discriminability is difficult, thereby reducing reliance on fluency as a basis for making decisions, the current experiments suggest that SOA and prime validity may be factors of equal or greater importance.

Conclusion

Although a detailed process model based on the episodic resource-retrieval account remains to be developed, the general claim being made here is that masking and brief presentation of primes limits the contents of episodic processing resources created by the prime event, but does not eliminate their creation nor their contributions to word recognition. These contributions arise and can be modulated by factors such as list context because primes have their influence retrospectively during processing of the target, rather than via automatic preactivation of a lexical entry or via a consciously deployed strategy whose effects are exerted at the time the prime is presented. The possibility of episodic influences on masked priming has already been suggested (Forster, Booker, Schacter, &

Davis, 1990), but the current experiments indicate that the extent of this episodic influence has been significantly underestimated. As Forster (1998) pointed out, what constitutes an unwanted effect depends on one's theoretical point of view. From the perspective put forward here, the present demonstrations that masked repetition and masked semantic priming can be affected by list-wide prime validity, that masked repetition priming can interact with word frequency, and that masked nonword repetition priming can occur for normally presented targets in the lexical decision task all suggest that episodic influences are a major determinant of priming.

Footnote

1. A repetition proportion by priming interaction was also obtained in an earlier version of this experiment in which the frequencies of the unrelated primes were not matched with the frequencies of their repetition primes and targets. The experiment was otherwise identical to Experiment 1. With 20 subjects in each repetition proportion group, the interaction again reflected more priming in the high than in the low repetition proportion group (69 ms vs. 37 ms), $F(1, 38) = 11.16$, $MSE = 464$.

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Appendix

Analysis of Variance Summary Tables for Experiments 1-8

Table A1

ANOVA Results for Mean Response Latency (RL, in ms) and Error Percentage (%E) for Words and Nonwords in Experiment 1

	RL		%E	
Target and effect	MSE	F	MSE	F
Words				
repetition proportion	13,208	.67	18.0	.20
Priming	521	194.08*	4.5	16.64*
repetition proportion x Priming	521	16.58*	4.5	.82
Nonwords				
repetition proportion	17,060	2.62	44.7	.65
Priming	689	9.78*	10.0	.23
repetition proportion x Priming	689	10.90*	10.0	.39

Note: Subjects (df are 1 and 78) were the random variable. An * indicates that the effect was significant at the $p < .05$ level.

Table A2

ANOVA Results for Mean Response Latency (RL, in ms) and Error Percentage (%E) for Words and Nonwords in Experiments 2A and 2B

	RL		%E	
Exp., target, and effect	MSE	F	MSE	F
Experiment 2A				
Words				
repetition proportion	14,694	.83	30.7	5.18*
Freq.	529	662.55*	17.4	99.74*
repetition proportion x Freq.	529	5.74*	17.4	5.25*
Priming	965	124.87*	19.6	12.83*
repetition proportion x Priming	965	.70	19.6	.40
Freq. x Priming	552	17.89*	20.3	12.19*
repetition proportion x Freq. x Priming	552		.15	20.3
.86				
Nonwords				
repetition proportion	12,791	.36	28.1	.05
Priming	511	7.64*	4.4	.07
repetition proportion x Priming	511	4.17*	4.4	.22

(continued...)

Table A2 continued...

Experiment 2B				
Words				
repetition proportion	26,872	2.11	127.4	3.86
Freq.	1569	142.99*	35.2	54.43*
repetition proportion x Freq.	1569	2.79	35.2	.39
Priming	1412	73.28*	26.2	21.34*
repetition proportion x Priming	1412	.53	26.2	4.48*
Freq. x Priming	672	18.47*	21.7	.84
repetition proportion x Freq. x Priming	672	.58	21.7	.34
Nonwords				
repetition proportion	21,891	.04	85.2	.03
Priming	940	4.82*	7.4	.29
repetition proportion x Priming	940	.004	7.4	.15

Note: Subjects (df are 1 and 38) were the random variable. An * indicates that the effect was significant at the $p < .05$ level.

Table A3

ANOVA Results for Mean Response Latency (RL, in ms) and Error Percentage (%E) for Words and Nonwords in Experiment 3

	RL		%E	
Target and effect	MSE	F	MSE	F
Words				
repetition proportion	7038	.01	20.5	.42
Freq.	7038	21.33*	20.5	12.18*
repetition proportion x Freq.	7038	.11	20.5	.25
Priming	323	308.15*	8.7	19.12*
repetition proportion x Priming	323	13.57*	8.7	.001
Freq. x Priming	323	1.64	8.7	.70
repetition proportion x Freq. x Priming323			4.75*	8.7
.29				
Nonwords				
repetition proportion	13,235	1.05	48.3	.14
Freq.	13,235	9.10*	48.3	5.06*
repetition proportion x Freq.	13,235	.08	48.3	.46
Priming	531	11.08*	8.5	6.33*
repetition proportion x Priming	531	3.61	8.5	3.30
Freq. x Priming	531	9.12*	8.5	3.05
repetition proportion x Freq. x Priming531			.66	8.5
.80				

Note: Subjects (df are 1 and 76) were the random variable. An * indicates that the effect was significant at the $p < .05$ level. For nonwords, frequency refers to the frequency of the word targets in the stimulus list.

Table A4

ANOVA Results for Mean Response Latency (RL, in ms) and Error Percentage (%E) for Words and Nonwords in Experiments 4A and 4B

	RL		%E	
Exp., target, and effect	MSE	F	MSE	F
Experiment 4A (60 ms)				
Words				
repetition proportion	6257	.05	16.6	.08
Priming	355	161.10*	8.8	14.08*
repetition proportion x Priming	355	8.17*	8.8	5.21*
Nonwords				
repetition proportion	10,008	.90	14.9	.58
Priming	676	8.08*	13.6	1.37
repetition proportion x Priming	676	9.35*	13.6	.002
Experiment 4B (45 ms)				
Words				
repetition proportion	8394	.56	39.5	.46
Priming	384	192.04*	10.0	14.82*
repetition proportion x Priming	384	4.55*	10.0	2.51
Nonwords				
repetition proportion	14,490	4.24*	42.3	1.00
Priming	419	3.57	11.1	.42
repetition proportion x Priming	419	.03	11.1	1.00

Note: Subjects (df are 1 and 38 in Experiment 4A; df are 1 and 58 in Experiment 4B) were the random variable. An * indicates that the effect was significant at the $p < .05$ level.

Table A5

ANOVA Results for Mean Response Latency (RL, in ms) and Error Percentage (%E) for Words and Nonwords in Experiments 5A, 5B, 5C, and 5D

	RL		%E	
Exp., target, and effect	MSE	F	MSE	F
Experiment 5A				
Words				
repetition proportion	5,544	.13	5.1	.29
Priming	406	85.93*	2.5	4.18*
repetition proportion x Priming	406	.10	2.5	.52
Nonwords				
repetition proportion	9,415	.36	27.5	3.02
Priming	305	.36	6.8	.24
repetition proportion x Priming	305	.35	6.8	1.88
Experiment 5B				
Words				
repetition proportion	18,086	.47	16.4	.26
Priming	365	115.10*	8.3	14.36*
repetition proportion x Priming	365	8.60*	8.3	1.76
Nonwords				
repetition proportion	25,898	.07	28.0	.01
Priming	801	1.50	11.5	.43
repetition proportion x Priming	801	2.33	11.5	2.74

(continued...)

Table A4 continued...

Experiment 5C				
Words				
repetition proportion	9929	.003	9.9	.01
Priming	484	57.44*	5.7	4.06
repetition proportion x Priming	484	6.40*	5.7	.56
Nonwords				
repetition proportion	12,269	.45	94.6	.86
Priming	548	1.17	14.7	.32
repetition proportion x Priming	548	.04	14.7	.62
Experiment 5D				
Words				
repetition proportion	9652	.02	5.2	.001
Priming	478	4.38*	2.5	.01
repetition proportion x Priming	478	9.26*	2.5	1.70
Nonwords				
repetition proportion	10,136	.001	6.1	.06
Priming	331	.02	3.0	.10
repetition proportion x Priming	331	.55	3.0	2.86

Note: Subjects (df are 1 and 38) were the random variable. An * indicates that the effect was significant at the $p < .05$ level.

Table A6

ANOVA Results for Mean Response Latency (RL, in ms) and Error Percentage (%E) for Words and Nonwords in Experiment 6

	RL		%E	
Target and effect	MSE	F	MSE	F
Words				
repetition proportion	26,785	1.44	53.8	11.30*
Freq.	1033	127.78*	33.3	53.49*
repetition proportion x Freq.	1033	.76	33.3	2.56
Priming	957	149.21*	26.5	33.79*
repetition proportion x Priming	957	6.83*	26.5	5.88*
Freq. x Priming	830	10.35*	25.7	11.14*
repetition proportion x Freq. x Priming	830		.93	25.7
.59				
Nonwords				
repetition proportion	9,928	5.39*	15.1	1.14
Priming	375	1.94	5.3	.74
repetition proportion x Priming	375	.01	5.3	.03

Note: Subjects (df are 2 and 76 for effects involving Frequency; df are 1 and 38 for all other effects) were the random variable. An * indicates that the effect was significant at the $p < .05$ level.

Table A7

ANOVA Results for Mean Response Latency (RL, in ms) and Error Percentage (%E) for Words and Nonwords in Experiment 7

	RL		%E	
Target and effect	MSE	F	MSE	F
Words				
repetition proportion	7,775	.44	16.3	5.15*
Priming	400	48.31*	8.0	8.16*
repetition proportion x Priming	400	2.14	8.0	.21
Nonwords				
repetition proportion	6,697	.002	16.2	.34

Note: Subjects (df are 1 and 98) were the random variable. An * indicates that the effect was significant at the $p < .05$ level.

Table A8

ANOVA Results for Mean Response Latency (RL, in ms) and Error Percentage (%E) for Associative-Primed Words and Repetition-Primed Words and Nonwords in Experiment 8

	RL		%E	
Target and effect	MSE	F	MSE	F
Semantic-Primed Words				
repetition proportion	16,872	3.66	30.1	2.73
Priming	652	12.45*	10.2	3.60
repetition proportion x Priming	652	5.63*	10.2	.11
Repetition-Primed Words (high repetition proportion only)				
Priming	643	77.35*	14.0	4.00
Repetition-Primed Nonwords				
repetition proportion	24,939	5.61*	47.1	2.05
Priming	429	4.53*	11.6	1.46
repetition proportion x Priming	429	2.32	11.6	.22

Note: Subjects were the random variable. The df are 1 and 38, except for repetition-primed words (high repetition proportion group only), where the df are 1 and 19. An * indicates that the effect was significant at the $p < .05$ level.

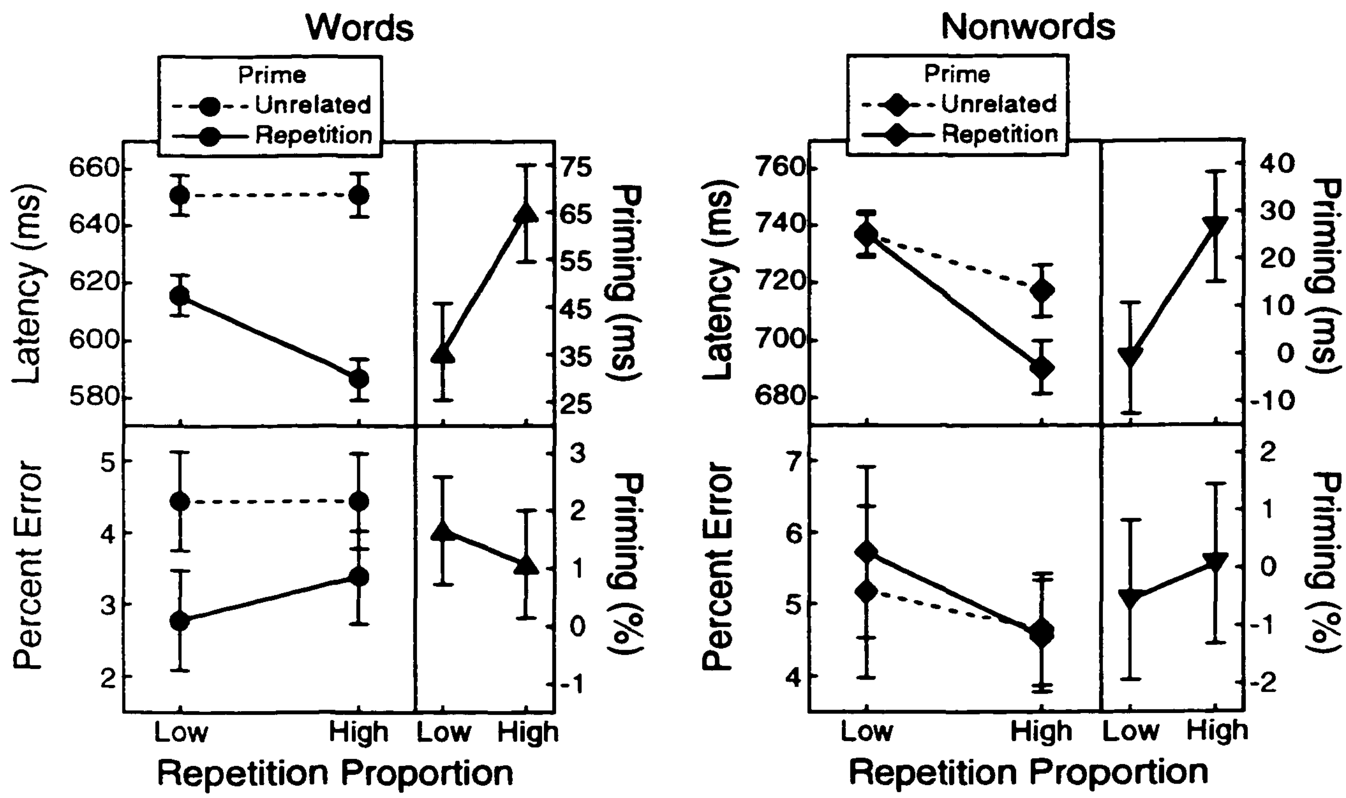

Figure 1. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 1. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

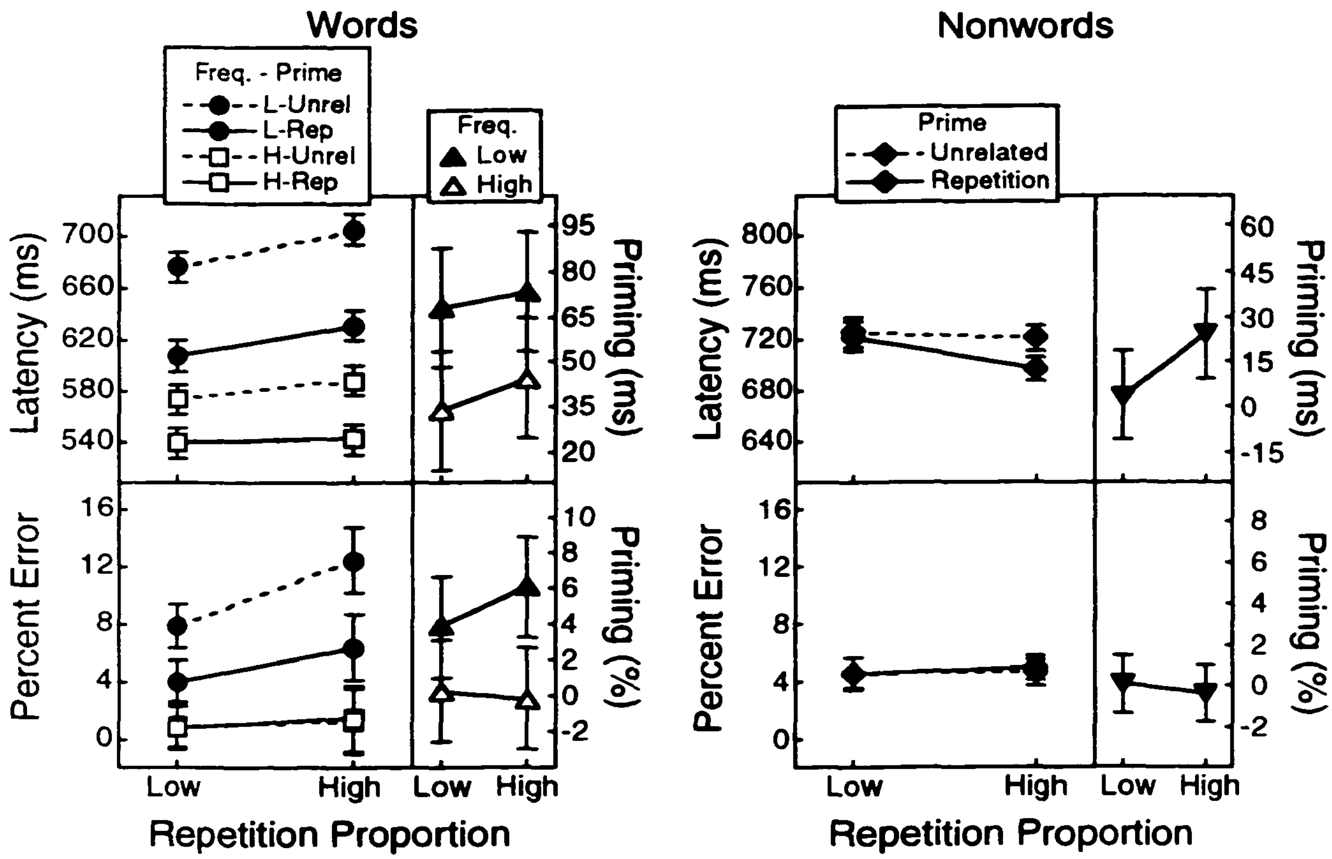

Figure 2. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 2A. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

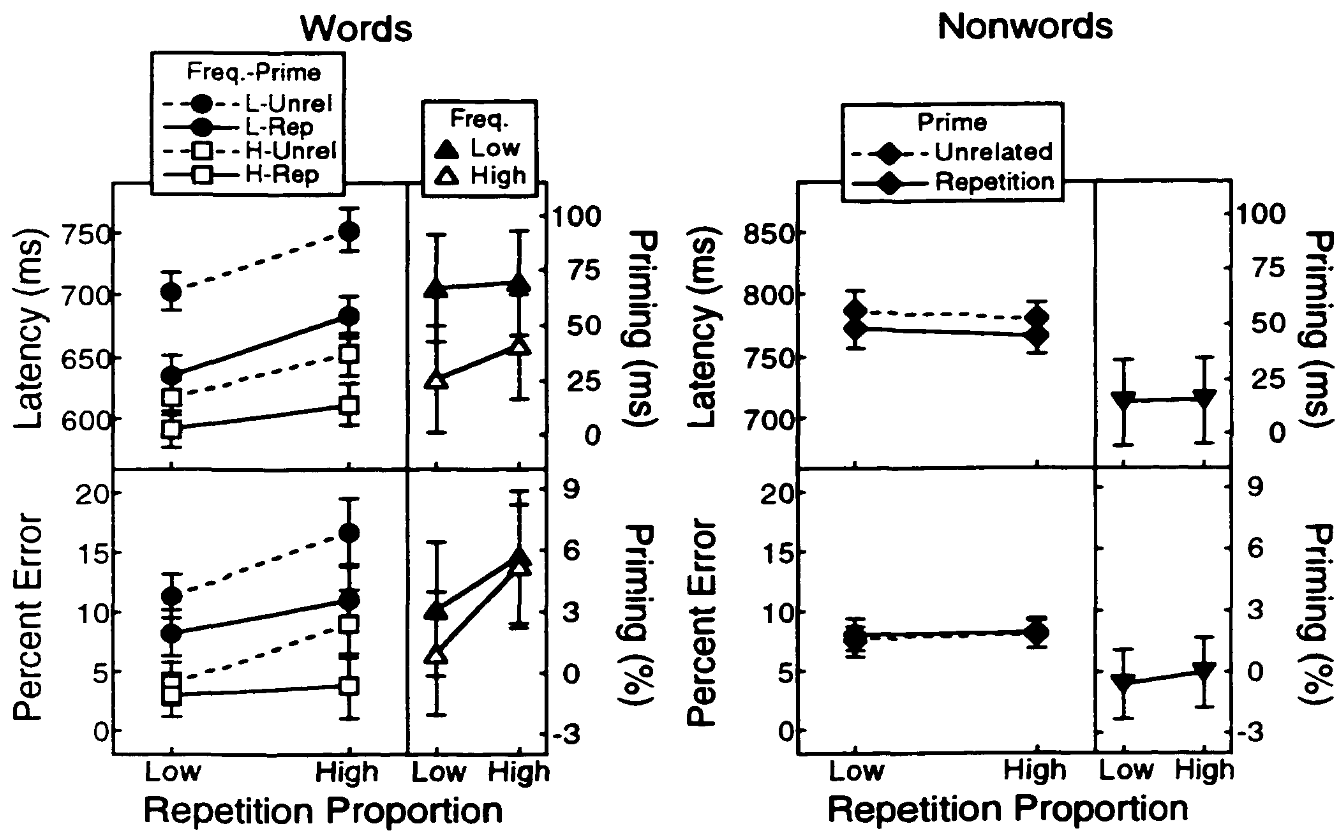

Figure 3. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 2B. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

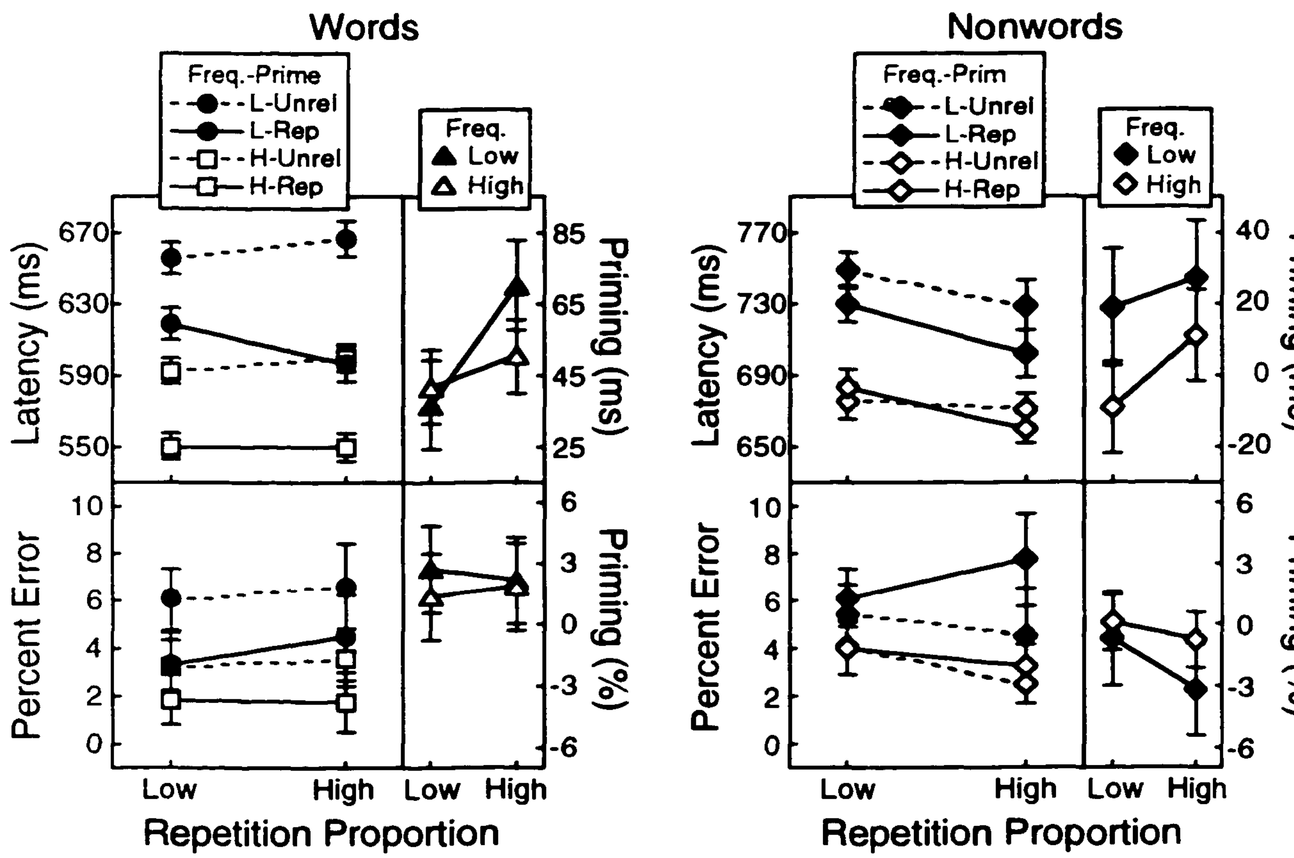

Figure 4. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 3. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero. Frequency in the case of nonwords refers to the word-frequency condition in which the nonwords were presented.

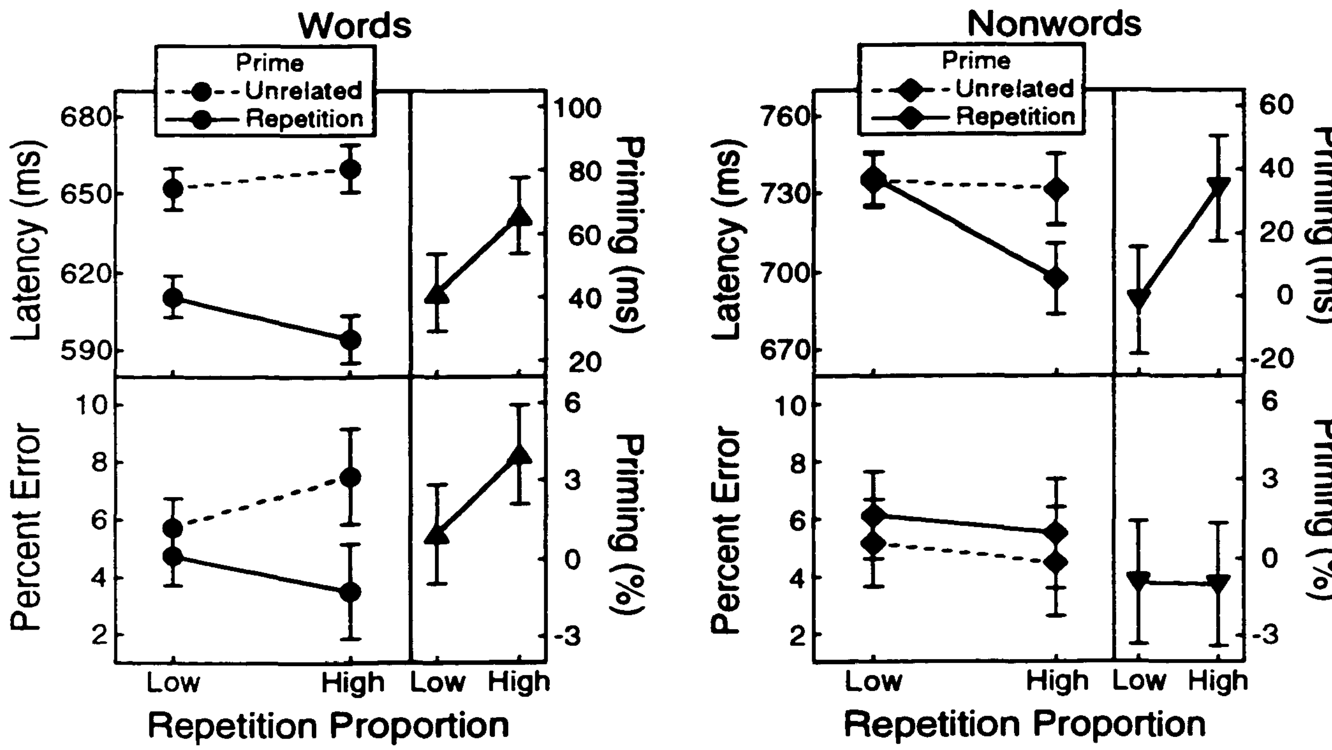

Figure 5. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 4A. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

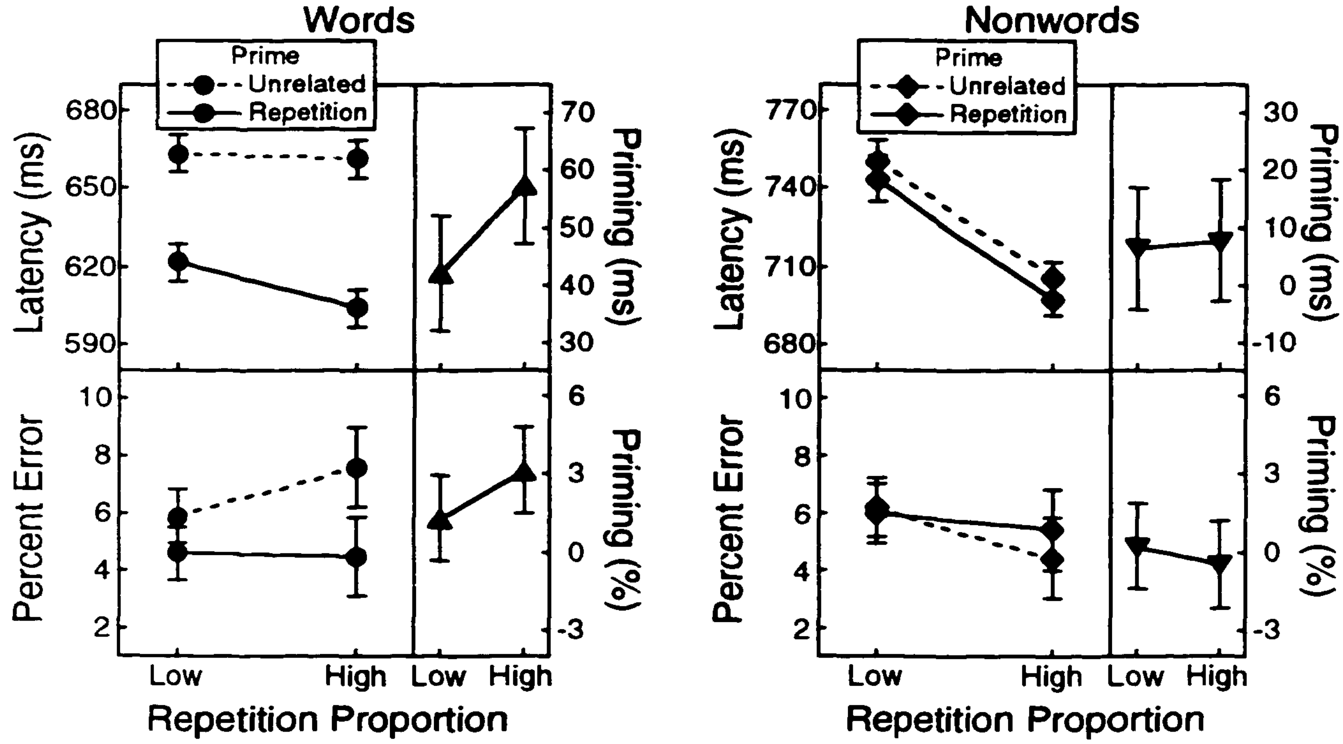

Figure 6. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 4B. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

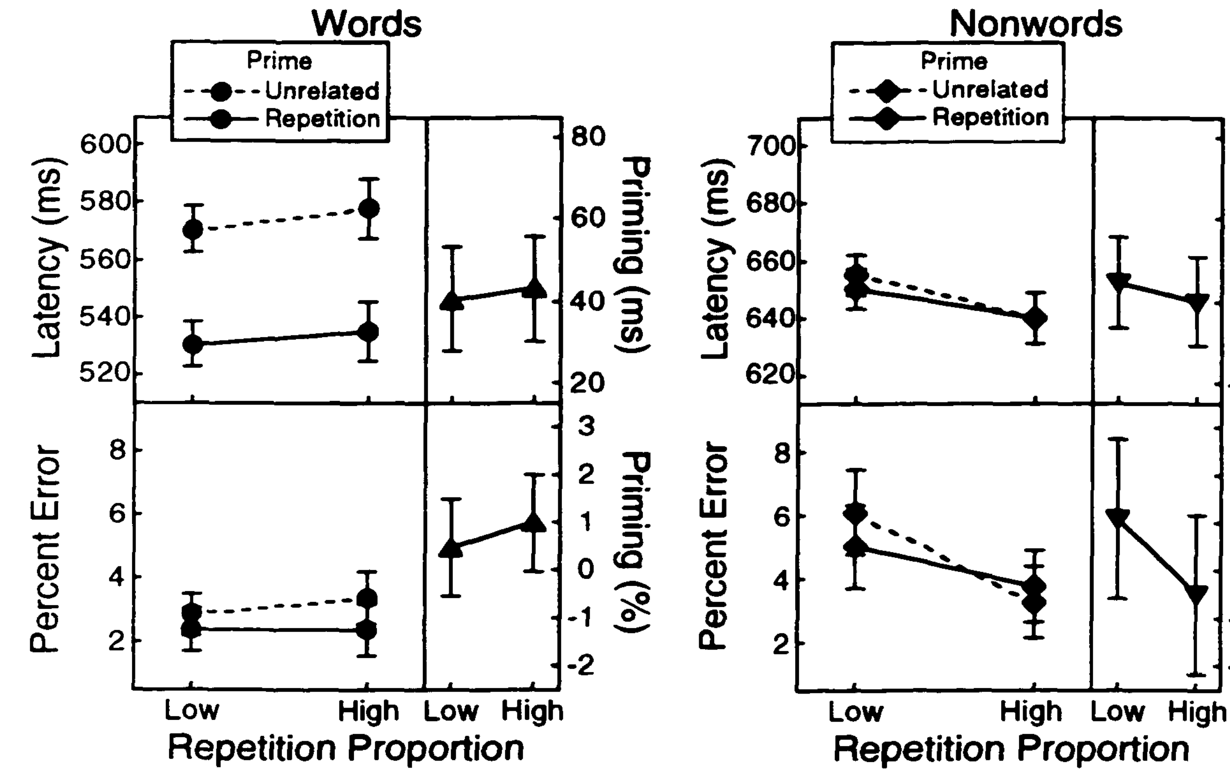

Figure 7. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 5A. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

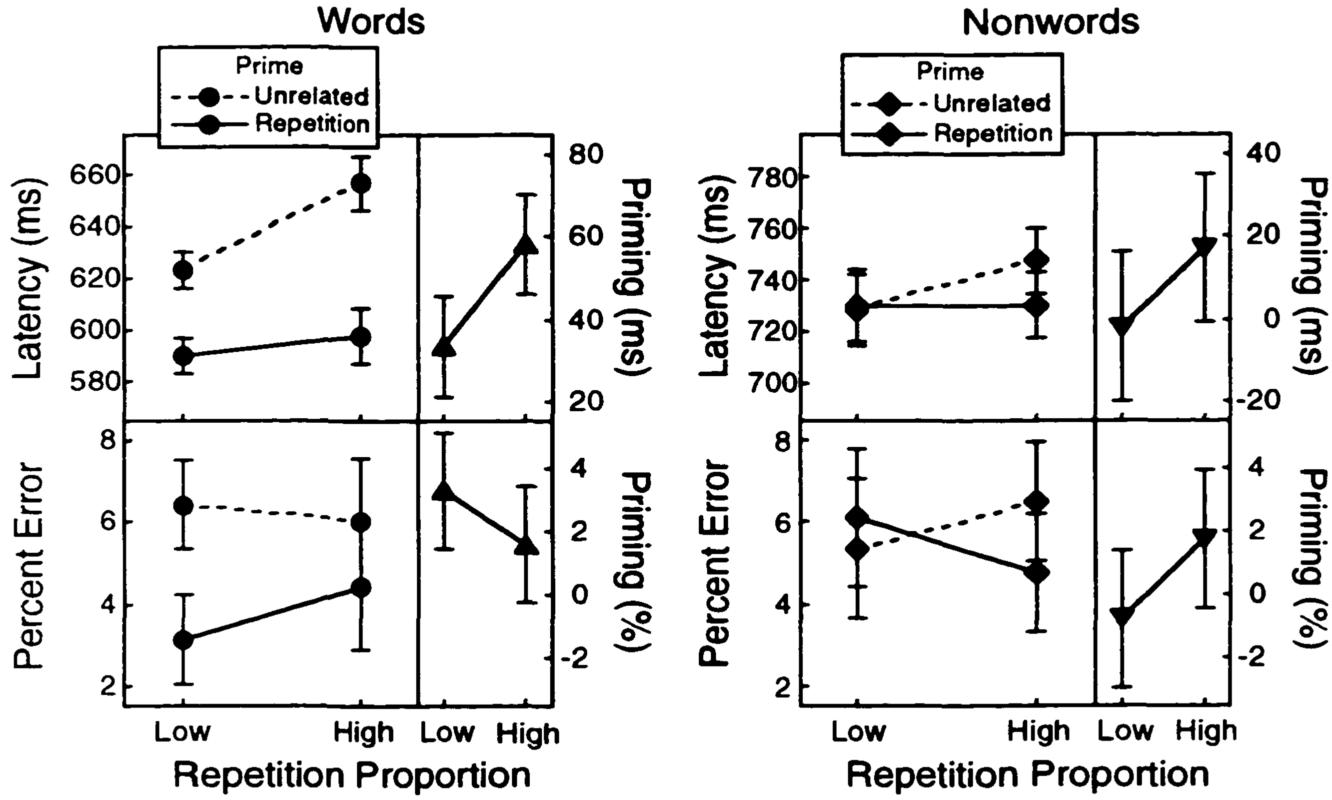

Figure 8. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 5B. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

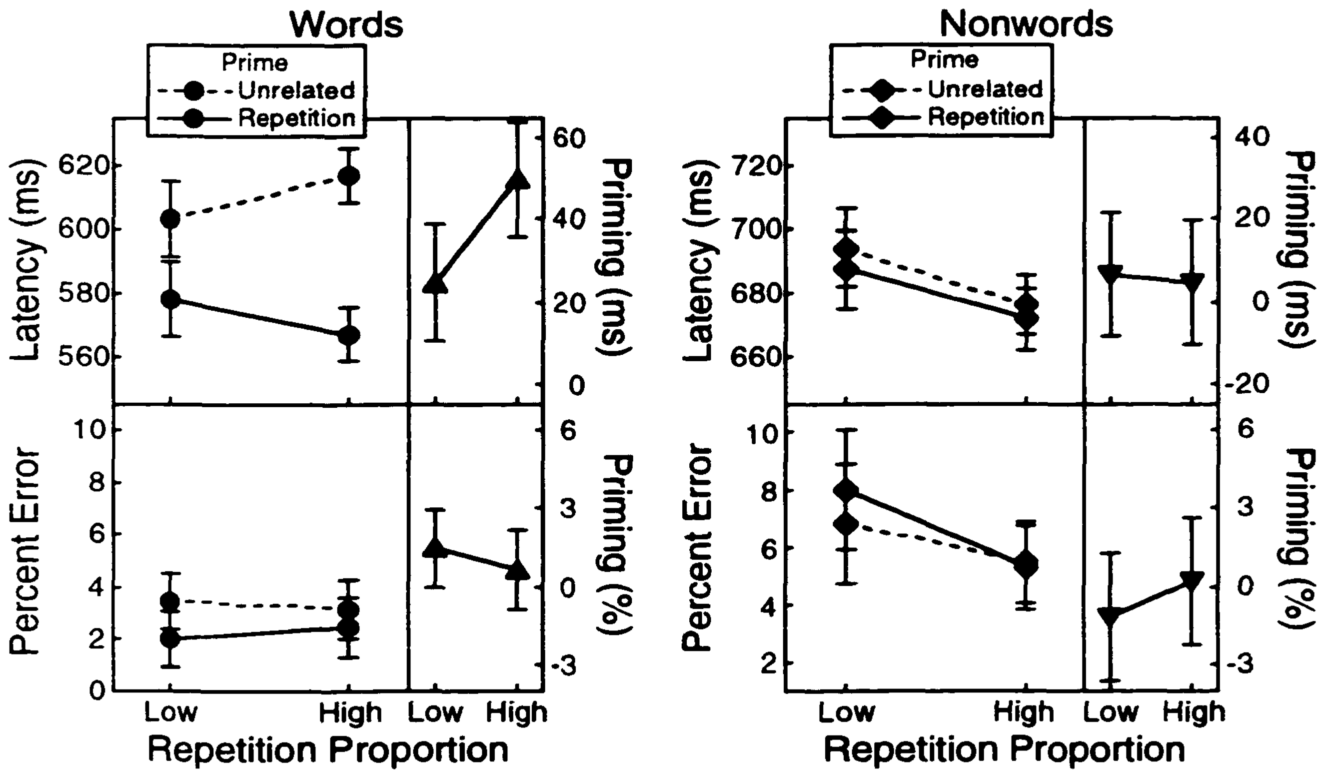

Figure 9. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 5C. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

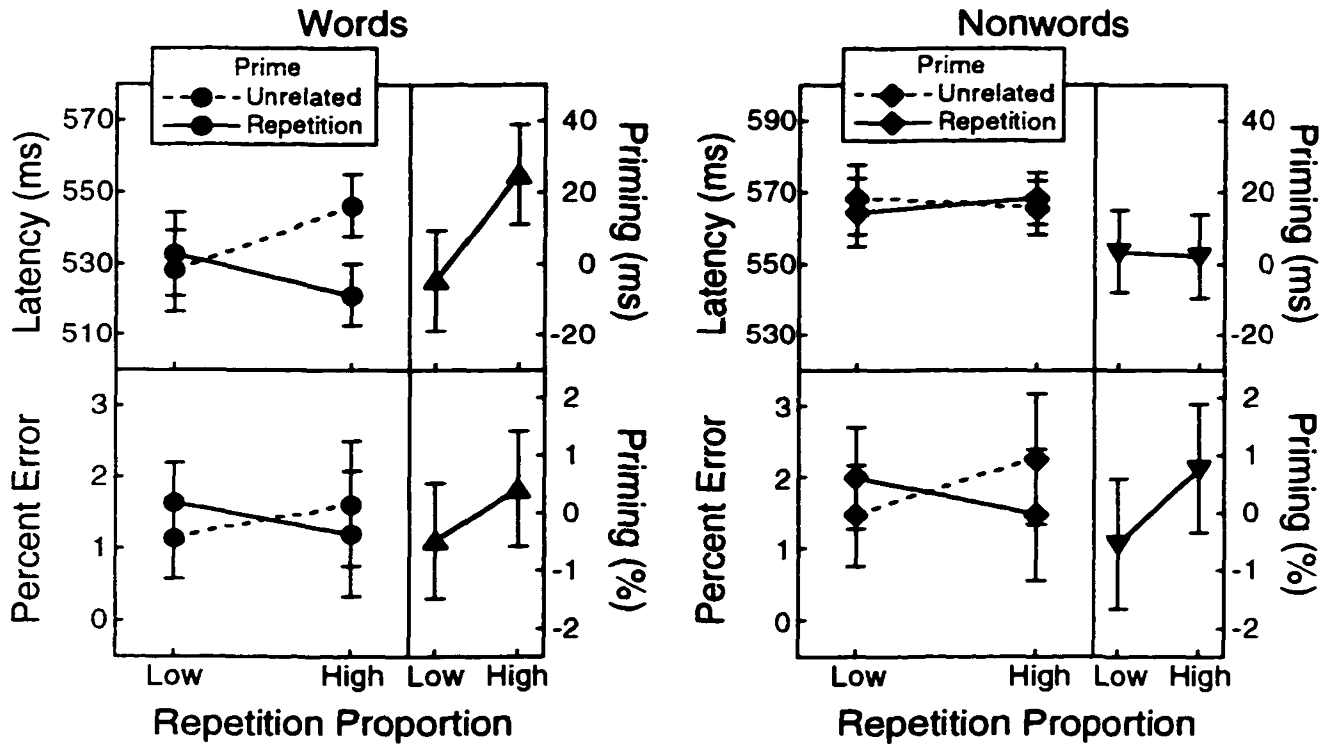

Figure 10. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 5D. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

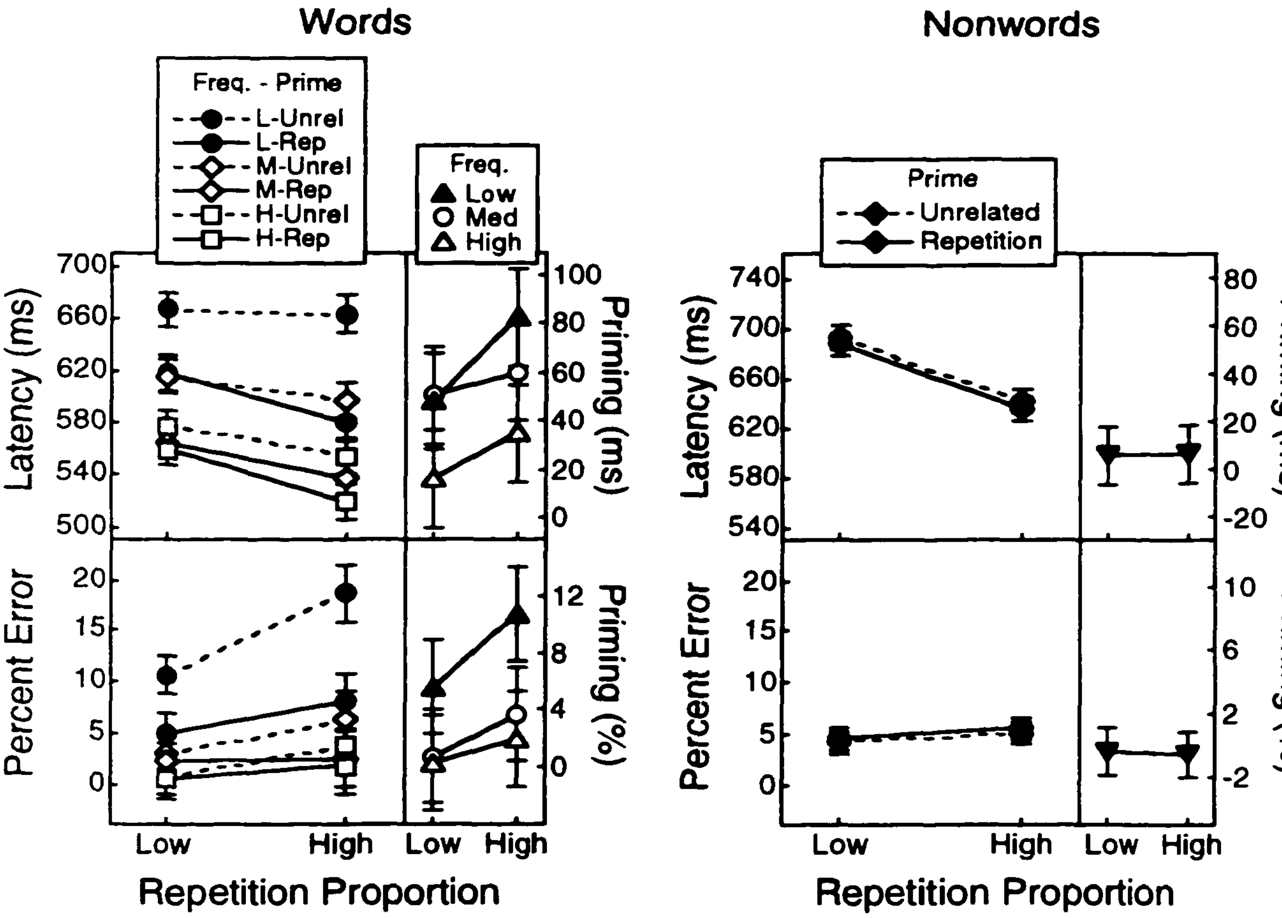

Figure 11. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 6. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

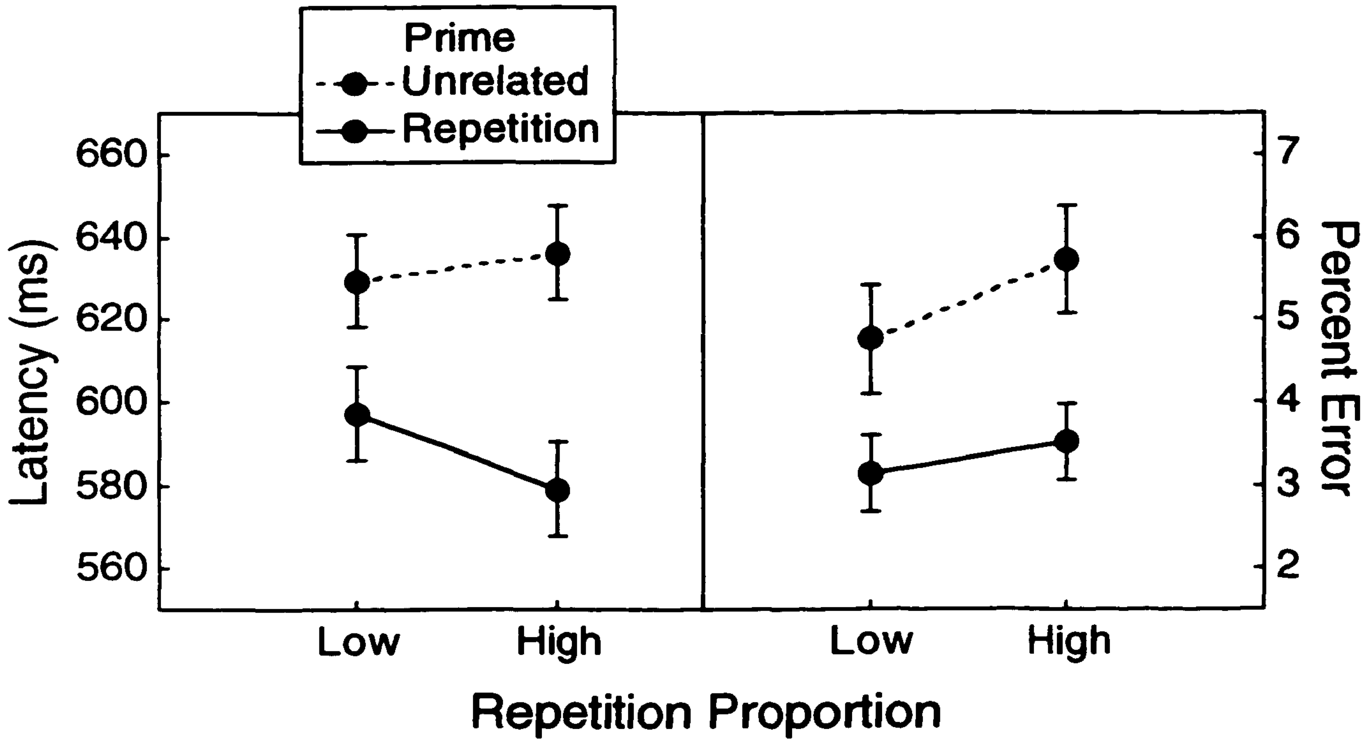

Figure 12. Mean response latency and error rates for words averaged across all experiments showing a repetition proportion by priming interaction. Error bars are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

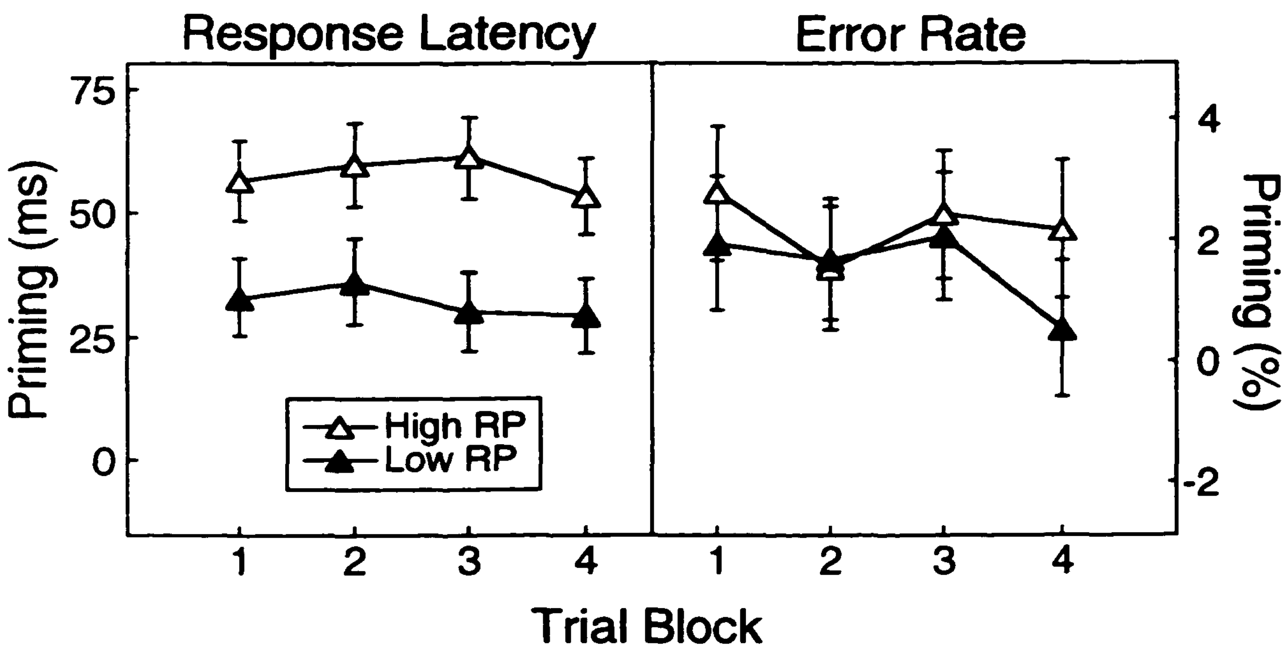

Figure 13. Mean priming for response latency and error rates for words as a function of trial block averaged across all experiments showing a repetition proportion by priming interaction. Error bars are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions within each trial block and against zero.

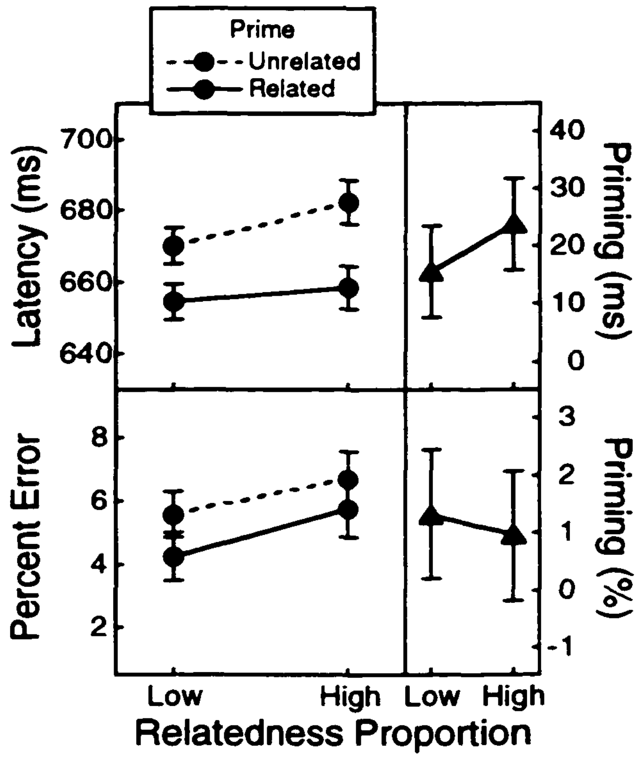

Figure 14. Mean response latency, percentage error, and corresponding priming effects for words in Experiment 7. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions. Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero.

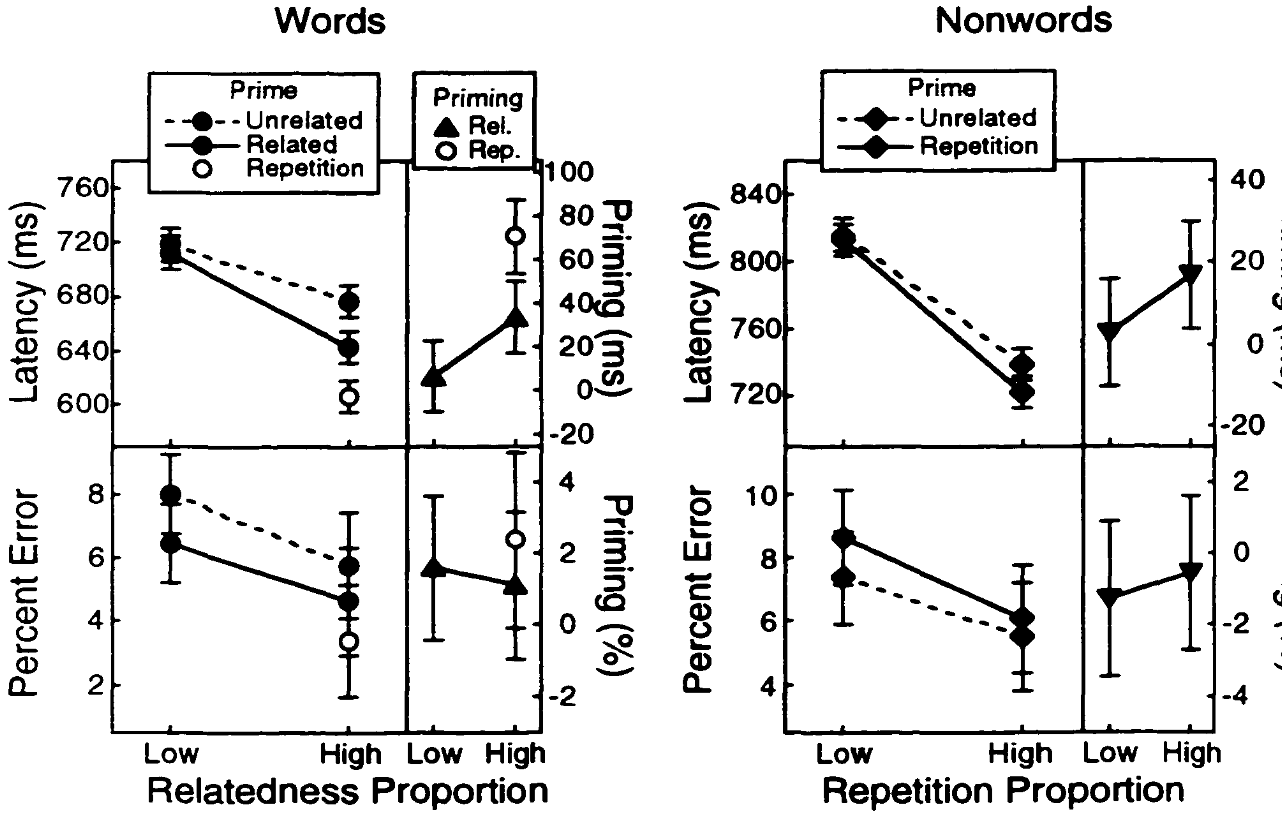

Figure 15. Mean response latency, percentage error, and corresponding priming effects for words and nonwords in Experiment 8. Error bars for latency and error are 95% within-subjects confidence intervals and are appropriate for comparing means across prime conditions (for words, these are computed separately for related and repetition primes). Error bars for priming effects are 95% between-subjects confidence intervals and are appropriate for comparing means across repetition proportion conditions and against zero (for words, these are computed separately for related and repetition primes).