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Priming effects with phonemically similar words: The encoding-bias hypothesis reconsidered

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Recent studies have demonstrated that two-word lexical decision times may be influenced by the degree of shared phonemic and graphemic similarity between the items. Specifically, graphemically similar rhyming pairs (e.g., BRIBE-TRIBE) are responded to more rapidly than graphemically and phonemically unrelated controls (e.g., BREAK-DITCH), whereas graphemically similar nonrhyming pairs (TOUCH-COUCH) are responded to more slowly. In a series of three experiments, the present study examined the encoding-bias explanation (Meyer, Schvaneveldt, & Ruddy, 1974) of these effects by modifying or eliminating the graphemic information available. Experiment 1 found that rhyming facilitation was not eliminated by presenting the initial pair member auditorily. Experiments 2 and 3 showed the rhyming effect to be independent of graphemic similarity with equivalent facilitation for graphemically similar and dissimilar (EIGHT-MATE) rhymes. These findings were all considered contrary to the predictions of the encoding-bias model. As an alternative, a model by Forster (1976) was employed. In the Forster model, sensory representations of lexical entries are represented as entries in a separate access file organized by physical, rather than semantic, similarity. The rhyming facilitation can then be seen as the result of spreading activation between entries in this file.

In formulating explanations of reading, the notion of a mediating phonemic code has been a recurrent device allowing the comprehender of spoken speech to become a comprehender of written speech. A major source of support for this phonemic recoding process comes from a study by Meyer, Schvaneveldt, and Ruddy (1974). Using a two-item lexical decision task (in which subjects were required to determine whether either of two visually presented letter strings was a nonword), Meyer et al. demonstrated that decision times may be influenced by the degree of phonemic and graphemic similarity shared by the strings. Specifically, there was a tendency for subjects to respond more rapidly to word pairs sharing both graphemic and phonemic similarity (PITCH-DITCH) than to control pairs sharing neither dimension of similarity (BREAK-DITCH). However, when the pairs shared only graphemic similarity (TOUCH-COUCH), responses were significantly slower than appropriate controls (BREAK-COUCH). This facilitation and interference will be referred to as rhyming and nonrhyming effects, respectively.

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In explaining these results, Meyer et al. (1974) proposed the concept of encoding bias. According to their model, pair members are processed sequentially, and both require phonemic recoding prior to lexical search. When pair members are graphemically similar, the phonemic recoding of the first item may influence the manner in which the second item is recoded. That is, there is a bias to use the same grapheme-phoneme conversion rules on the second item as were employed in the recoding of the first. If the two graphemic codes do, in fact, map onto similar phonemic codes, the process can be facilitated, and lexical decisions can be made more rapidly. But, if the two graphemic codes map onto different phonemic representations, the bias will lead to an incorrect phonemic code, slowing lexical decisions.

Using the Meyer et al. (1974) model, two other predictions appear to be logically entailed. First, because encoding bias places the locus of rhyming and nonrhyming effects at the phonemic recoding stage, both should be eliminated by removing the grapheme-phoneme conversion of the first item. One sure means of accomplishing this would be to present the initial item auditorily. While it is likely that the auditory signal requires some degree of processing prior to lexical access, this processing would almost certainly not include grapheme-phoneme conversion. Hence, with auditory presentation of the first pair member and visual presentation of the second, there should be neither a rhyming nor a nonrhyming effect. Experiment 1 was designed to test this prediction.

The second prediction, also based on grapheme-phoneme encoding bias, is that rhyming facilitation should not occur when the pairs share only phonemic

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similarity. That is, since the bias is assumed to be caused by a graphemic match, a word such as LATE should be facilitated by an initial presentation of MATE but not by EIGHT (see Meyer et al., 1974, p. 318). This prediction will be the focus of Experiments 2 and 3.

EXPERIMENT 1

Method

Subjects. Forty students, drawn from introductory psychology classes, participated in the experiment as part of a course requirement. All subjects were tested individually.

Materials and Design. Stimulus materials were drawn from the Meyer et al. (1974) study. They consisted of pairs of letter strings made up of an initial item and a target item. There were 32 target items in the rhyming set and 32 in the nonrhyming set (a complete listing of the materials for all experiments is reported in the Appendix). The target items were divided into four material lists, containing eight items from each set. These four lists were combined with four subject groups using a Latin square design. Treatments in the Latin square were defined by the type of initial item presented with each target, with four treatments resulting from the combination of initial-item modality (auditory or visual) and pair type (test or control). For example, the rhyming target item DITCH in Material Set A was presented in the auditory test /pĭc/-DITCH, auditory control /bruwz/-DITCH, visual test PITCH-DITCH, and visual control BRUISE-DITCH conditions for Subject Groups 1-4, respectively. The assignment process was identical for the nonrhyming pairs except that the test item now consisted of graphemically similar, phonemically dissimilar pairs such as TOUCH-COUCH.

There were also 16 pairs each of word-nonword, nonword-word, and nonword-nonword strings. The nonwords were all pronounceable and were formed by substituting letters in legitimate words. As with the test pairs, half of the initial items were presented auditorily. Unlike the test pairs, all subjects were tested on the same nonword pairs, and none of the pair members was graphemically similar.

Procedure. Stimulus presentation, timing, and data collection were controlled by a PDP-11/40 computer. All visual presentations were on a videoscreen; auditory materials were prerecorded on a Teac four-channel tape deck interfaced with the computer. Timing for the auditory presentations was initiated by a 1,000-Hz tone that coincided with the onset of the stimulus item in the alternate track. Responses were made by pressing one of two microswitches mounted beneath the screen. "Word" responses were always made with the right hand.

All sessions began with a taped set of instructions describing the lexical decision task and stressing the need for speed and accuracy. Visual-visual and auditory-visual trials were presented randomly, and subjects were not preinformed as to the trial type. A test trial began with the onset of a 500-msec visual warning signal followed immediately by the initial auditory or visual stimulus. After the subject's response, this item was removed, and approximately 250 msec later, the target item appeared. For both items the subject was asked to decide whether the string was a legitimate English word. If no response was made within 2,500 msec, the string was removed and the trial counted as an error. A practice set of 20 pairs (containing both rhyming and nonrhyming trials) was used to familiarize subjects with the task.

Results

Because initial items served only to influence target decision times, they were not analyzed. Decision times for the target items were analyzed using the quasi-F′ test suggested by Clark (1973). Empty cells were filled according to a procedure outlined by Winer (1971, p. 488).

The results of Experiment 1 are shown in Table 1. Errors did not correlate with decision times for either words ($r = .004$) or nonwords ($r = .07$), belying any speed-accuracy tradeoff.

It is immediately apparent from Table 1 that target item decision times were influenced by the modality of the initial item. When targets had been preceded by an auditory presentation, "word" decisions were, on average, 116 msec longer than targets preceded by a visual item. This modality effect [$F'(1,209) = 155.5$, $p < .01$] was not found to interact with any other factors.

Of more central concern are the effects of rhyming and nonrhyming materials. While the test items were, overall, faster than the controls [$F'(1,209) = 7.58$, $p < .01$], a significant Pair Type by Rhyming-Nonrhyming interaction [$F'(1,209) = 8.52$, $p < .01$] suggested that the effect of test items was restricted to the rhyming pairs. This was confirmed by a series of orthogonal comparisons that yielded a 61-msec facilitation for rhyming pairs [$F'(1,209) = 16.22$, $p < .01$] but no effect for nonrhyming pairs. It appears the rhyming facilitation was unaffected by the modality of the initial item, whereas the nonrhyming effect was found in neither condition.

Like words, the nonword decision times were slower when preceded by an auditory presentation [$F'(1,23) =$

Table 1
Mean Decision Times and Error Rates for Target Items (Italicized) in Experiment 1

		Auditory-Visual Decision Time (msec)	Errors (%)	Visual-Visual Decision Time (msec)	Errors (%)
Words					
Rhyming Pairs	PITCH-*DITCH*	681	6.0	569	5.3
Rhyming Controls	LOAD-*DITCH*	757	5.9	615	3.1
Nonrhyming Pairs	LEMON-*DEMON*	712	2.8	605	5.6
Nonrhyming Controls	BLOW-*DEMON*	708	4.7	611	3.8
Nonwords					
Nonword-Word	PLIG-*FOIL*	690	3.8	660	8.7
Word-Nonword	MONK-*GORL*	859	7.5	757	9.7
Nonword-Nonword	BINE-*CLUW*	907	10.6	808	12.2

31.4]. (Since the nonwords were the same for all subjects, these data were analyzed using a completely within-subjects design. Hence the difference in degrees of freedom for error.) Again, this effect was not found to interact with the type of nonword employed [$F'(2,14) = 2.80$, $p > .05$]. There was a significant effect for the type of nonword presentation employed [$F'(2,35) = 52.36$]. Orthogonal comparisons reveal that nonword-word combinations were the fastest, hardly surprising since the target is a word. When the target item was a nonword, decision times were 45 msec faster when it was preceded by a word [$F'(1,35) = 11.51$].

Discussion

Three primary results emerge from Experiment 1: (1) Decision times for visually presented targets were influenced by the modality of the initial item such that targets preceded by an auditory presentation were slower than those preceded by a visual presentation. (2) Nonrhyming interference was found in neither the within- nor the across-modality conditions. (3) Rhyming facilitation was unaffected by the presentation condition, with a significant effect in both the within- and across-modality conditions.

Although the main effect for modality was quite robust, a full account of its cause appears beyond the scope of this paper. Pending a more systematic investigation, any attempt to explain this modality effect would be more speculative than conclusive. Fortunately, modality was not found to interact with materials, and, whatever its cause, it appears to be independent of the processes involved in the rhyming and nonrhyming effects.

The failure to find a nonrhyming interference effect is, at least for the within-modality pairs, contrary to earlier reports. Both Meyer et al. (1974) and Shulman, Hornak, and Sanders (1978) have found nonrhyming interference with visually presented pairs. However, consideration of this discrepancy will be deferred until the general discussion, and the remaining experiments will deal exclusively with rhyming facilitation.

The presence of a rhyming effect in the auditory-visual condition indicates that facilitation is present even when the graphemic code from the initial item is unavailable. While this would seem to contradict the encoding-bias position, one could argue that auditory presentation of the initial item does not necessarily eliminate its graphemic code. There is some evidence that subjects may employ a graphemic code even when the task demands do not appear to require it. Seidenberg and Tannenhaus (in press) have found that decision times for judgments of whether two auditorily presented words rhymed were faster when the words had similar orthographies (e.g., HEAD-DEAD) than when their orthographies were different (FED-DEAD). If subjects in Experiment 1 used a graphemic code at some point in the recognition process, it may be possible to explain the auditory-visual rhyming facilitation using the encoding-bias position.

Experiment 2 was designed to test this possibility. Whereas earlier studies have compared graphemically similar rhymes (LATE-MATE), graphemically similar nonrhyming pairs (TOUCH-COUCH), and graphemically dissimilar nonrhyming pairs (TOUCH-MATE), Experiment 2 compared words that are graphemically dissimilar but phonemically similar (EIGHT-MATE). The prediction of the encoding-bias model is clear: There should be no rhyming facilitation because the absence of graphemic similarity prevents the occurrence of bias.

EXPERIMENT 2

Method

Subjects. Fifteen graduate students and faculty participated in the experiment. All subjects were run individually.

Materials and Design. Fifty words were drawn from a rhyming dictionary (Holofcener, 1960) such that a target item (e.g., MATE) could be preceded by a graphemically similar rhyme (LATE), a graphemically dissimilar rhyme (EIGHT), a nonrhyming control (VEIL), a graphemically similar nonword (JATE), or a graphemically dissimilar nonword (BAFF). The targets were divided into five groups of 10 each, which were then combined with five groups of subjects using a Latin square design. Treatments were defined by the type of initial item paired with the target.

In addition to these 50 test pairs, subjects were presented 30 additional nonword pairs consisting of 20 word-nonword and 10 nonword-nonword combinations. None of the pair members was graphemically or phonemically similar. Finally, 10 pairs of graphemically similar, phonemically dissimilar pairs (e.g., HORSE-WORSE) were included to make the task demands similar to previous studies. The word-nonword pairs were formed by recombining words and nonwords from test pairs not used on that particular group (if, e.g., a subject was presented LATE-MATE, the nonword JATE would then be used in one of the word-nonword pairs). The nonword-nonword and nonrhyming pairs were the same for all subjects.

Procedure. Presentation, timing, and data collection were performed with a PDP-8 computer. Subjects initiated a trial by pressing a foot switch, after which two vertically aligned fixation points appeared on a cathode-ray tube. After 500 msec the top point was replaced by the initial pair member, which remained on until a response was made. Following the response, the first item was removed and immediately replaced by the target item at the second fixation point. Items were presented using a different randomization for each subject; 30 practice trials were used to familiarize subjects with the task.

Results

Mean decision times and error rates are shown in Table 2. Because the three filler item groups were not counterbalanced, analyses were restricted to the five test conditions.

Decision times for these items were analyzed twice: once with subjects and once with items as a random effect. The F tests were then combined using Clark's (1973) min F′ procedure.

A significant overall treatment effect [min $F'(4,263) = 4.71$, $p < .01$] was found for decision times. Closer examination of this effect, using a series of orthogonal

Table 2
Mean Decision Times and Error Rates for Target Items (Italicized) in Experiment 2

		Decision Time (msec)	Errors (%)
Test Pairs			
Graphemically Similar Rhyme	LATE-*MATE*	490	2.0
Graphemically Dissimilar Rhyme	EIGHT-*MATE*	488	1.3
Control	VEIL-*MATE*	527	8.0
Graphemically Similar Nonword-Word	JATE-*MATE*	530	7.3
Graphemically Dissimilar Nonword-Word	BAFF-*MATE*	538	5.3
Fillers			
Word-Nonword	GRAPH-*BLATE*	645	15.2
Nonword-Nonword	COFF-*TULD*	653	13.0
Nonrhyming Pairs	HORSE-*WORSE*	467	3.3

comparisons, revealed that the two rhyming pairs were not significantly different, but both were faster than the control pair [min $F'(1,141) = 5.43$, $p < .05$]. No other differences were found between treatments.

The error data followed a similar pattern (decision times and error rates were significantly correlated; $r = .23$). The lowest error rates were found in the two rhyming conditions, which did not differ from each other and were significantly lower than the other treatments [min $F'(1,62) = 16.24$]. No other significant differences in error rate were found.

Parenthetically, it is interesting to note that the filler items made up of graphemically similar, phonemically dissimilar word pairs yielded very low decision latencies. While it is tempting to compare these items with the test set, such contrasts should be avoided. Because there was no appropriate control group for these pairs, it is impossible to ascertain whether decision times for these targets were facilitated or slowed by the initial items.

Discussion

The most striking aspect of these data is the high degree of similarity found between decision times and error rates for the two types of rhyming pairs. No diminution of the rhyming effect was evident when the pairs were graphemically dissimilar. One possible explanation for this finding is that subjects engaged in an active anticipation strategy based on rhyme. Given an initial item, the subject might generate a number of rhyming candidates that are then compared with the target item. If there is a match between one of these candidates and the target, an immediate positive response can be made. If none of the generated words matches the target, subjects revert to the regular word recognition process. Given a "hit" on some proportion of trials, average decision latencies for rhyming targets will be reduced, perhaps enough to account for the rhyming effect.

But there are problems with using anticipation to explain the rhyming effect. The most serious concerns the time constraints imposed by the task. It must be assumed that the generation of rhyming words takes some amount of time, but it is unclear where this time is available. In Experiment 1 there was a 250-msec interval between the offset of the initial item and the onset of the target. Even by assuming that rhyme generation proceeds before the first response is made, there seems to be insufficient time to generate rhymes. In Experiment 2 the conditions were even less accommodating, since the second item onset followed immediately after the first item response.

The anticipation explanation would also appear to predict facilitation between rhyming nonword-word combinations, such as JATE-MATE, and this was not found in Experiment 2. Of course, one might modify the explanation such that the strategy is used only when the initial item is a word. But the time needed for this additional decision reduces even further the time available for generating rhymes.

Still, it could be argued that since neither Experiment 1 nor Experiment 2 contained nonword targets preceded by a rhyming word or nonword, the procedure used encouraged some type of guess based on simple rhyme. Rather than argue, post hoc, the merits and debits of this position, Experiment 3 tested the anticipation explanation directly.

EXPERIMENT 3

There has been some evidence from studies of semantic priming that suggests the facilitation found between pairs of semantically related words in a lexical decision task may be due to both facilitatory and inhibitory processes. Neely (1976) carried out a lexical decision task in which an initial priming word (which required no overt response) preceded the target item. These prime-target pairs were related (NURSE-DOCTOR), unrelated (BREAD-DOCTOR), or neutral. In the neutral pairs the first item was not a word but a row of asterisks (*****-DOCTOR). Neely found that, while responses to the related pairs were faster than responses to the other two types, responses to the unrelated pairs were actually slower than those to the neutral pairs. Apparently, when given a sufficient interval between pair members (cf. Neely, 1977), subjects anticipated the target item on the basis of information in the initial word. Hence, semantic facilitation reflected both the benefit of anticipating correctly and the cost of anticipating incorrectly.

The methodology employed by Neely (1976) can be readily extended to the present question. If rhyming facilitation is due to an active anticipation process, one should find rhyming pairs faster than a neutral control and nonrhyming pairs slower. To test this, Experiment 3 replicated the procedures and materials of Experiment 2 but added a neutral prime condition consisting of a row of asterisks paired with words and nonwords (e.g., *****-MATE). Also, to remove any information that may have been derived from the fact that only word targets occurred in rhyming pairs, half of the graphemically dissimilar word-nonword pairs (e.g., GATE-SAPH) were modified to make them graphemically similar (e.g., GATE-JATE).

Method

Subjects. Twenty-five subjects participated in the experiment. Of these, 15 were undergraduates drawn from introductory psychology classes. The remainder were graduate students in psychology.

Materials and Design. Target and initial items were identical to those used in Experiment 2. The graphemically similar nonword-word condition was replaced by the neutral prime/word condition. As earlier, the material group, subject group, and treatments were combined in a 5 by 5 Latin square. Nonword fillers were also identical to the previous experiment, except that half of the nonwords in one of the word-nonword conditions were replaced to make the pairs graphemically similar. The replaced nonwords were used as targets in the neutral prime/nonword condition. There was a total of 100 test trials preceded by 30 practice trials.

Procedure. The task used the same procedure as Experiment 2. The only difference was that subjects were asked to respond "nonword" when presented the neutral prime. While the Neely (1976) experiments required no response to the prime, it was felt that Experiment 3 should use a methodology similar to Experiments 1 and 2. Furthermore, by requiring a lexical decision to the prime, one insures that the item is indeed being read. When no overt response is required of the first item, subjects may ignore it or process it in a superficial manner, and there is some evidence (Bradshaw & Nettleton, 1974) indicating that rhyming and nonrhyming effects are not found under such circumstances. The choice of the "nonword" response for the neutral primes simply reflects an effort to make the response consistent with the task since the neutral primes were, in fact, nonwords.

Results

Analyses were identical to those in Experiment 2. Mean decision times and error rates are shown in Table 3. Again, there was a significant treatments effect [min $F'(4,73) = 3.15$, $p < .05$], which, when analyzed by orthogonal comparison, yielded no difference between the two sets of rhyming pairs or between the unrhymed and neutral controls. A significant difference was found between the two rhyming types and the two controls [min $F'(1,70) = 6.58$, $p < .05$]. No other comparisons were significant. The pattern of errors was similar to that found for the decision latencies, although it failed to reach significance [$F(4,80) = 1.22$, $p > .05$, by subjects].

While there was no significant difference between decision times for graphemically similar and dissimilar word-nonword pairs [$F(2,40) < 1$, by subjects], there was a significant difference in error rates. Graphemically similar word-nonword pairs had more errors than either the graphemically dissimilar word-nonword pairs [min $F'(1,43) = 6.06$, $p < .05$] or the nonwords preceded by a neutral context [min $F'(1,55) = 12.33$, $p < .01$].

Discussion

The failure to find a significant difference between the neutral and unrhymed control pairs suggests that the rhyming facilitation found in Experiments 1-3 is not due to a strategy of generating and matching rhyming words to the target. Still, there remain two aspects of this experiment that may be viewed as problematic. First, the high error rate for the rhyming word-nonword pairs indicates that these pairs were more difficult than nonwords preceded by a neutral or unrhymed item. One means of explaining this finding will be presented in the following section. Parenthetically, the high error rate for these pairs was not found by either Meyer et al. (1974) or Shulman et al. (1978), and prudence dictates that any explanation of the effect be treated as tentative until the conditions that give rise to it are more clearly defined.

The second objection concerns the validity of employing a cost-benefit analysis when the word pairs have different response patterns. Although both unrhymed and neutral control pairs required the same target

Table 3
Mean Decision Times and Error Rates for Target Items (Italicized) in Experiment 3

		Decision Time (msec)	Errors (%)
Test Pairs			
Graphemically Similar Rhyme	LATE-*MATE*	538	3.6
Graphemically Dissimilar Rhyme	EIGHT-*MATE*	540	1.3
Unrhymed Control	VEIL-*MATE*	574	6.0
Neutral Context	*****-*MATE*	588	6.0
Nonword-Word	BAFF-*MATE*	608	6.8
Fillers			
Graphemically Similar Word-Nonword	MANE-*FANE*	693	24.8
Graphemically Dissimilar Word-Nonword	FAME-*RALL*	689	16.8
Neutral Context Nonword	*****-*JEAD*	700	10.8
Nonword-Nonword	COFF-*TULD*	709	16.4
Nonrhyming Pairs	HORSE-*WORSE*	526	6.0

response, the "word" response in the neutral pairs (*****-MATE) was preceded by a "nonword" response (made with the opposite hand), whereas the target response with the unrhymed controls (VEIL-MATE) was preceded by a "word" response. Perhaps the response shift required with neutral pairs inflated the target decision time, masking the cost associated with the unrhymed controls.

While this is a reasonable point, closer examination of the data suggest that the response shift did not influence target decision times. For example, because factors influencing the response stage should be independent of factors influencing earlier stages, an equivalent effect of response shift should be apparent in the nonword pairs. That is, the shift in response for the unrhymed word-nonword pairs (e.g., FAME-RALL) should slow the decision times relative to the neutral context targets (*****-JEAD), which now have the same response for both items. But inspection of these pairs yields a difference almost identical in magnitude and direction to that found when the neutral pair required a shift.

From another perspective, one could argue that the neutral pairs may actually be easier than the unrhymed controls because the asterisks can be rejected with only a superficial visual analysis (the mean decision times for neutral and word primes were 550 and 697 msec, respectively; $p < .05$ by sign test), and this may give the subject more time to prepare for the target item.

In summary, while the response requirements could have been a factor in the assessment of cost, the evidence suggests that they were not.

GENERAL DISCUSSION

According to Meyer et al. (1974), words presented in a lexical decision task are transformed into a phonemic representation prior to lexical access. Furthermore, Meyer et al. argued that the recoding of the graphemic code into a phonemic code may be biased toward the utilization of grapheme-to-phoneme transformation rules that have been recently employed on letter strings orthographically similar to the to-be-recoded string.

How do these two points fare in light of the data from Experiments 1-3? The first appears wholly compatible with these data. In fact, the demonstration of facilitation between words sharing only phonemic similarity (Experiments 2 and 3) appears to strengthen the phonemic recoding position. The second point, however, fails to predict either the facilitation found when phonemically similar auditory primes are used (Experiment 1) or that found when the graphemic codes are dissimilar (Experiments 2 and 3).

Clearly, these data require that another mechanism replace or supplement the encoding-bias explanation of rhyming facilitation. Whether the same mechanism could or should explain the nonrhyming effect (or lack of it) is not clear. The goal in the remainder of the paper will be to formulate an explanation for just the rhyming effect. The reason for divorcing rhyming and nonrhyming effects is justified by more than ease of exposition. There is no reason to assume that these effects are opposite sides of the same coin. On the contrary, a case can be made for treating them as qualitatively different processes, influenced by different variables.

For example, Shulman et al. (1978) demonstrated that while the nonrhyming effect is affected by the type of nonword foils used, the rhyming effect is not. When the task required discriminating words from pronounceable nonwords (using the Meyer et al., 1974, procedure), both rhyming and nonrhyming effects were found. However, if orthographically illegal strings were used as foils, no nonrhyming effect was found; rather, facilitation was found for graphemically similar rhyming and nonrhyming pairs.

Another factor that appears to differentially influence rhyming and nonrhyming effects is the delay between presentation of the pair members. Of the five experiments demonstrating the nonrhyming effect (two by Meyer et al., 1974; three by Shulman et al., 1978), all but one (Meyer et al., Experiment 2) used simultaneous presentation of the pair members, and in this one experiment, which used zero delay between offset of the first item and onset of the second, the magnitude of the effect was substantially reduced. When the interval between the pair members was larger, as with the 250-msec delay of Experiment 1, the nonrhyming effect was absent altogether. In contrast, rhyming facilitation was not influenced by the method of presentation.

Perhaps the nonrhyming effect really is due to a bias during phonemic recoding. Thus, as Shulman et al. (1978) note, the failure to find it with orthographically illegal foils may be due to subjects' abandoning phonemic recoding in favor of an entirely visual strategy. The effect of interitem delay may reflect the short-lived nature of this bias. When words are presented simultaneously (usually vertically aligned) their orthographic similarity is highlighted, encouraging the use of similar grapheme-to-phoneme transformations. But as the pair members become separated in time and space, the bias fades rapidly, and interference decreases.

Of course, this account of the nonrhyming effect raises yet another problem. If nonrhyming interference is attributed to encoding bias, what mechanism remains to explain the facilitation between graphemically similar rhymes, graphemically similar nonrhymes (with orthographically illegal foils), graphemically dissimilar rhymes, and auditory-visual rhymes?

One clue may lie in the methodological similarity between these experiments and those used to demonstrate semantic priming. Meyer and Schvaneveldt (1971) and others have shown that a target word may be recognized more rapidly when it is preceded by a semantically similar word (NURSE-DOCTOR) than when the preceding word is unrelated (BUTTER-DOCTOR). Both semantic priming and rhyming facilitation are found when words sharing some dimension of similarity (semantic, phonemic, or orthographic) are presented simultaneously or in close succession. Also Fischler (1977) and Neely (1977) have shown that semantic priming can not be wholly attributed to an anticipation strategy. A similar conclusion was reached for rhyming facilitation in Experiment 3.

These overt similarities between rhyming effects and semantic priming effects lead naturally to the question of whether the two effects are due to similar processes. The semantic priming facilitation is assumed to be caused by a passive spread of activation between related lexical entries (Meyer & Schvaneveldt, 1971; Schvaneveldt & Meyer, 1973). But this process cannot be directly applied to rhyming facilitation without modifying some current assumptions about the structure of lexical memory. If, as semantic priming models presuppose, lexical entries are organized by semantic relatedness, in what dimension can activation spread to facilitate rhyming pairs?

A model that may allow an implementation of the priming process has been outlined by Forster (1976). In considering the demands made upon the lexical access process, Forster proposed that the lexicon must consist of a single "master file," containing all of the information normally attributed to lexical entries, which may be entered through one of three "access files." These access files are composed of the physical representation, orthographic or phonemic, of the lexical entry (the third file, used for production, is organized by semantic similarity and is not of interest in the present context), with pointers to their respective entries in the master file. Since incoming information has no meaning prior to recognition, the most efficient organization would be along a dimension of physical similarity. Separation of the access and master file allows both orthographic and phonemic access files to be organized in this manner. Lexical retrieval, then, involves preparing a coded representation of the input (which may involve phonemic recoding), searching through the phonemic and/or orthographic access files until an entry receives activation sufficient to exceed its threshold, and terminating search on the appropriate entry in the master file.

While this brief description is not meant to be an exhaustive outline of the model's properties, it is sufficient to make an important point. Since search involves activation of entries organized by physical similarity, it would be reasonable to assume that, once an entry in the access file has been activated, it takes some period of time to return to its previous level of activation. Further, this activation may spread to adjacent entries, causing them to become temporarily more sensitive to further activation. These assumptions are similar to those made by Morton (1969) in his logogen model and, as with semantic similarity, allow one to predict priming along a dimension of physical similarity. Hence, rhyming facilitation is assumed to be due to a process similar to semantic priming acting on adjacent, physically similar entries in the access files.

Consider this model in the context of the present data. If it is assumed that the task demands encourage subjects to employ a phonemic recoding process, then in Experiment 1 both auditory and visual input would employ the phonemic access file. Hence, regardless of initial item modality, adjacent entries in the access file (i.e., rhyming words) have their activation levels briefly raised. This increased activation makes these items more sensitive to further input, facilitating rhyming words without influencing decision times for unrhymed targets.

This would also explain the high error rate found with rhyming word-nonword pairs such as MANE-FANE. Since the initial item in these pairs is a word, it will access an entry in the phonemic access file, raising activation levels of rhyming words. Because of this increase in activation, these word entries have a higher likelihood of exceeding their threshold when the subject is presented with a target nonword sharing many of the same graphemic or phonemic features. This would result in an increased false alarm rate for these nonword targets, exactly what was found in Experiment 3. However, if these same pairs are reversed, forming rhyming nonword-word pairs, no facilitation is found for the target words. This, too, is consistent with the model, because there is a much lower likelihood that the initial nonword will cause any of the similar entries to exceed threshold. Thus, there is a lower likelihood of residual activation facilitating target word recognition.

These explanations all presuppose that subjects are, in fact, employing a mediating phonemic code. If one changed the task to encourage use of the orthographic access file, phonemic priming should be replaced by orthographic priming. This is, of course, exactly what Shulman et al. (1978) found when they employed orthographically illegal foils. Word pairs that shared similar orthographies primed each other even when their phonemic codes were dissimilar.

One might legitimately question whether these data are sufficient to justify the structural and processing assumptions being put forward. If the model were solely constructed to explain the results of Experiments 1-3, the answer might very well be no. But such is not the case, since Forster's (1976) original model was derived without regard to the current questions. There is also at least one other finding that can be explained using the concept of priming within access files. Scarborough, Cortese, and Scarborough (1977) have demonstrated that lexical decision times decrease when a word is repeated in the presentation list. Although they attribute this effect to lexical activation, this interpretation is inconsistent with studies by Hillinger (1978) and Kirsner and Smith (1974). Both found the repetition effect to be diminished when the repetitions are in different modalities. If the facilitation were due only to lexical activation, it should not be influenced by modality. However, if one assumes facilitation reflects activation at both the master and access files, the decreased effect across modalities might be due to the absence of activation effects in the access files.

This modality effect is also important because it tends to strengthen Forster's (1976) claim that access files are separate from the master file. Combining semantic, phonemic, and orthographic information into one entry, as in a logogen model, would make it difficult to predict the modality difference, since input from all three sources can cause a logogen to reach threshold. Once activated, the modality information is no longer available.

Finally, the model serves a useful function in generating other questions. If repetition effects are due to processes acting on both the master and access files, what might be the locus of other word recognition phenomena? Is, say, semantic priming wholly attributable to spreading activation between master files? If so, one would expect it to be the same for both within- and across-modality presentations. In fact, Swinney, Onifer, Prather, and Hirshkowitz (1979) have data that suggest that the amount of semantic priming facilitation is not influenced by modality. What about word frequency effects? Since Scarborough et al. (1977) found that word frequency interacted with repetition, perhaps part of the word frequency effect is due to processes acting on the access files. It is interesting to note that McCusker, Holley-Wilcox, and Hillinger (Note 1) have found the magnitude of frequency effects to be influenced by input modality.

It seems the primary reasons these questions have not been posed earlier is that most lexical decision studies seldom use anything other than visual input. As more work is done comparing the interaction of the auditory and visual modalities, we will be better able to ascertain whether the assumptions made in this model are justified.

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APPENDIX

MATERIALS FOR EXPERIMENT 1

Graphemically and Phonemically Similar Pairs

PITCH-DITCH, LOAD-TOAD, MIGHT-TIGHT, POINT-JOINT, PLEA-FLEA, TRIM-GRIM, YELLOW-FELLOW, NATIONAL-RATIONAL, GRACE-TRACE, GUILT-BUILT, TILT-WILT, CLOT-BLOT, MARK-DARK, BORN-WORN, HOUSE-MOUSE, SET-WET, TICKLE-PICKLE, BRUISE-CRUISE, FILE-TILE, SOFT-LOFT, NUMB-DUMB, COIL-BOIL, CREAM-DREAM, BLAME-FLAME, MUCH-SUCH, MADE-WADE, DISH-WISH, SENT-WENT, YIELD-FIELD, BARGE-LARGE, FAIL-SAIL, FOND-POND.

Graphemically Similar, Phonemically Dissimilar Pairs

LEMON-DEMON, BLOW-PLOW, HOME-SOME, HAVE-CAVE, CATCH-WATCH, HORSE-WORSE, BOMB-TOMB, DULL-PULL, LOWER-TOWER, GIVES-WIVES, FEAR-WEAR, YOUTH-SOUTH, PUT-NUT, TOUR-SOUR, MINT-PINT, DOLL-TOLL, DROVE-PROVE, CASH-WASH, DIVER-LIVER, BAKED-NAKED, BEARD-HEARD, BONE-GONE, HANGER-RANGER, FLOWN-CLOWN, NATURE-MATURE, DAUGHTER-LAUGHTER, NASTY-HASTY, CLOVE-GLOVE, FEW-SEW, HONOR-DONOR, SOUR-FOUR, GATHER-FATHER. (Control pairs for these items were derived by interchanging members for one half of the pairs, e.g., PITCH-TOAD and LEMON-PLOW.)

Nonword-Word Pairs

PLIG-FOIL, ROIN-SLIDE, SIPE-GRIP, PURG-BUNK, GROP-PIPE, RELP-CHASE, SLUP-CAPE, KIPE-SLIP, MULP-SOAP, FOTCH-LOCK, GREL-PLUG, COSE-SWET, NINK-MEAT, SERL-SOLD, CLOGE-FOOT, SPOSH-THIGH.

Word-Nonword Pairs

MONK-GORL, KNIT-PARG, ROAST-BULGE, THUMB-FLOM, COMB-NAISE, DODGE-FRINK, CLUE-WOLT, GHOST-DURP, SMILE-PISH, BOLT-HANER, GRIEF-PLOS, PEPPER-MEAB, SQUARE-NOST, BLADE-JIRY, RIVAL-GILF, TANK-FLOME.

Nonword-Nonword Pairs

BINE-CLUW, PATA-NIST, BIMT-COSH, POIST-PANK, MUDGE-RABE, HASD-BRONT, WARSE-FOID, PANK-TARCH, SONT-BELP, DALL-PROT, HULB-CORL, CRINK-SULE, GRALD-SPIME, TOSH-HING, CAIL-LAVER, TROBE-TUGHT.

MATERIALS FOR EXPERIMENTS 2 and 3

Target	Rhyme Similar	Rhyme Dissimilar	Nonword	Target	Rhyme Similar	Rhyme Dissimilar	Nonword
MATE	LATE	EIGHT	SATE	CRIES	FLIES	GUYS	GIES
LACKS	TACKS	AXE	GACKS	POKE	COKE	FOLK	LOKE
MADE	WADE	SUEDE	SADE	GOAL	COAL	BOWL	VOAL
STAFF	CHAFF	GRAPH	BAFF	MOLE	HOLE	SOUL	YOLE
TALE	SALE	VEIL	LALE	LOAN	MOAN	TONE	DOAN
CANE	MANE	VEIN	TANE	POOL	COOL	RULE	SOOL
BEAR	PEAR	HAIR	CEAR	MOON	COON	TUNE	FOON
WALL	CALL	MAUL	RALL	ROOT	LOOT	FRUIT	KOOT
TAME	FAME	MAIM	HAME	COPE	HOPE	SOAP	WOPE
DANCE	TRANCE	PLANTS	FANCE	MORE	BORE	FOUR	VORE
FATE	HATE	BAIT	JATE	HOSE	ROSE	SEWS	COSE
BEARD	GEARED	WEIRD	DEARD	MOAT	GLOAT	NOTE	JOAT
BEAT	MEAT	SUITE	GEAT	NOISE	POISE	TOYS	LOISE
HEAD	DEAD	SAID	JEAD	GOOSE	MOOSE	JUICE	WOOSE
FIRST	THIRST	WORST	KIRST	NUDE	RUDE	LEWD	CUDE
STEW	SPEW	SHOE	REW	ROUGH	TOUGH	CUFF	SOUGH
ZOO	WOO	TRUE	VOO	PUKE	DUKE	SPOOK	MUKE
THIEF	GRIEF	SHEAF	BIEF	DUMB	NUMB	SOME	LUMB
LIAR	BRIAR	BUYER	SIAR	DONE	NONE	SPUN	FONE
LIGHT	MIGHT	QUITE	DIGHT	HOUR	SOUR	TOWER	BOUR
TILE	FILE	AISLE	KILE	FIELD	YIELD	SEALED	KIELD
WINE	SINE	SIGN	BINE	CURE	PURE	TOUR	MURE
BIRD	THIRD	HEARD	FIRD	BOOR	POOR	LURE	FOOR
FIRE	HIRE	CHOIR	NIRE	NEWS	HEWS	FUSE	BEWS
PERK	CLERK	QUIRK	DERK	CREAM	DREAM	SEEM	MEAM

Note—See text for a description of how the items were combined for Experiments 2 and 3.

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