Variable Interaction Between Visual Recognition and Memory in Oral Reading

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Readers were given tests containing meaningless numbers inserted between meaningful words. The inserted numbers were printed in letters or in repetitive numerals equal in length to the letter insertions (e.g., six or 666). Experiments 1 and 2 showed that oral reading took 75% longer and was more vulnerable to error when lettered insertions were to be ignored than when numerals were ignored. Experiment 3 showed that oral reading took 20% longer with inserted numerals and 100% longer with lettered letters than in a baseline condition with no insertions. These discrepancies in the speed of visually recognizing a text had no effect on memory for phrasing or meaning. Narrow confidence intervals based on pooled data of the three experiments provided strong evidence of a null effect on memory. These findings are interpreted as supporting a model of reading in which interaction between visual and memorial processes is not obligatory and may vary in form or amount according to task demands.

Suppose that the visual appearance of a printed text is changed in a way that makes the text difficult to read. Suppose also that the difficulty is evident in the speed, but not in the accuracy, with which readers recognize the printed words. The words, although identified correctly, are recognized only after long and tedious scrutiny. Would this hindrance of perceptual processing affect the reader's comprehension and memory of the meaning of the text?

If the hindrance derives from an alteration or mutilation of the visual appearance of the text, it is reasonable to suppose that some perceptual process has been slowed or degraded. Perhaps word recognition has been handicapped. The handicap may not, however, be primarily lexical; it may be a disruption of the normal pattern of eye movements or a degradation of graphemic analysis. We shall refer to these alternative possibilities with the general term visual recognition. We do so because we assume that physical distortion of a text must slow visual processing in some manner that makes it hard for the reader to locate and recognize the letters and words in which the meaning of the text resides. Our principal concern is how this aspect of reading, visual recognition, interacts with memory for meaning.

One plausible theory of reading implies that hindrance of visual recognition would also hinder memory for meaning. Perhaps as a word is identified, it is ordinarily integrated into a provisional interpretation of meaning. This interpretation is held in working memory, is updated as additional words are recognized, and eventually is stored in memory in a final form. When visual recognition of words is accurate but very slow, a provisional interpretation may be lost from working memory before all the important words have been identified. Loss of the working interpretation would cause the reader's memory for meaning to be fragmentary.

Plausible though it may seem, the foregoing theory is contradicted by a considerable body of data. In a series of studies,
Kolers (1975a, 1975b, 1976) altered the spa-
tial orientation of the letters comprising a text and thereby slowed oral reading dra-
matically. However, rather than penalizing memory, Koler's disruption of normal read-
ing actually improved performance on a sub-
sequent test of verbatim sentence recogni-
tion. To explain this surprising finding, Kolers proposed that readers remembered the operations of pattern analysis by means of which they had initially recognized the words of the text. Thus, distortion of a text slowed visual recognition by requiring novel or unusually thorough pattern analysis. By virtue of its novelty or thoroughness, the vi-
ual analysis was well remembered and, con-
sequently, the meaning of the text was also well remembered.

Kolers' views were disputed by Masson and Sala (1978). These investigators repli-
cated Kolers' finding of improvement in ver-
batim memory for sentences read under visu-
'al transformation. In addition, however, Masson and Sala showed that readers had better memory for paraphrases of trans-
formed sentences than for paraphrases of normal sentences. The finding of memorial gains for both verbatim and paraphrased sentences implies that the cognitive basis of the gain was semantic, not visual as Kolers had argued.

To explain their findings, Masson and Sala (1978) advocated an interactive model of reading. An interactive model (e.g., Rum-
ethart, 1975) assumes a continuous inter-
play between recognition of visual pattern, which may be largely data driven (Bobrow & Norman, 1975; Norman, 1976), and memory for an interpretation of meaning, which may be largely conceptually driven (Bobrow & Norman, 1975; Norman, 1976).

Interactive models may also make an assumption of compensatory interaction (Stanovich, 1980). Simply put, when the data concerning visual pattern are poor in quality, conceptually driven interpretation of meaning expands in compensation. This compensatory activity uses stored knowledge to establish plausible hypotheses concerning the meaning of a text, and thereby enlarges memory for meaning. A reader's better memory for visually transcribed than for normal sentences is explained, therefore, to be a consequence of compensatory reliance on conceptually driven interpretation of meaning. Our purpose in the present re-
search was to investigate the hypothesis of compensatory interaction and its possible obligatory nature.

Our procedure is one instance of what may be called disturbance paradigm. In these paradigms, one cogni-
tive component of reading is experimentally disturbed, and an assessment is made of the effect of the disturbance on a second com-
ponent. For example, in Koler's procedure, visual recognition is disturbed by presenting a text in a spatially transfigured orientation, and memory for the text is subsequently as-
sessed. In our procedure, visual recognition was disturbed by requiring subjects to read meaningful texts 666 then 5555 (either 3333) looked 9999 like 88888 this, or six appeared five in three print nine like eight this. Forcing the reader to bypass meaningless numbers while reading a meaningful text, as in the preceding sentence, would surely slow the pace of reading. We wanted to know whether there would be a corresponding effect on the reader's memory for meaning.

Our method of disruption was suggested by Neisser's (1964) paradigm of visual search for printed characters. There is sub-
stantial evidence from this paradigm (Brady, 1971; Ingling, 1972; Sperling, Budiansky, Spivak, & Johnston, 1971; Taylor, 1978) that subjects are better able to detect a tar-
get character within a field of characters if the target and field represent different cat-
egories (e.g., letters and numerals) than if they have the same category (e.g., both let-
ters). We anticipated a similar finding in our disturbance paradigm. Readers were ex-
pected to identify the meaningful words of a text more rapidly and with fewer errors when the inserted numbers were numerals than when the numbers were spelled with letters.

Experiment 1: Reading Speed and Memory of Phrases

The first study examined how speed of oral reading and memory of phrases were affec-
ted by inserted numerals or letters and
by instructions to say or to skip the insertions.

Method

Subjects. Seventeen students enrolled in introductory psychology recollected course credit for participating in the experiment. The participants were screened to exclude the persons who had speech defects that might interfere with oral reading.

Tests. Four articles (hereafter designated A, B, C, and D) were selected from a local newspaper. The articles were judged to be of general interest but were not hard stories concerning well-known national or local events. Each article was edited to 350 words long, and all numeric items were deleted from the article. An insertion from the test, five, six, eight, nine, or none from the set 1337, 3255, 666, 8888, 9999 was placed after every word in an article except the last word. Each insertion was a monosyllabic singular digit with no morpheme that involved in any of the articles. A numeral insertion and the corresponding written insertion had the same number of words and were presented in the same way. Five insertions were random, with the restriction that (a) no digit occurred twice in a row, (b) a digit of five digits was sampled randomly from the set with replacement, followed by five similar subsequent items and (c) the sequence of letter insertions was identical to that of numeral insertions.

Memory test. Twenty target phrases, each three words long without insertions, were selected from each text. These phrases were evenly distributed throughout each text. Ten additional phrases of the same length were selected from portions of an article that had been in the original run in the newspaper but had been edited out of the version used in testing. These phrases were used for recall. Ten for each was three-word phrases from newspaper articles other than the articles for Tests A, B, C, and D. The phrases (with near and far) and target phrases (without insertions) were presented in one randomized order in a typed sheet on which subjects judged whether a phrase had appeared in the text.

Design and procedure. A test was presented to a subject on a typed page. The subject was instructed to read quickly but with clear articulation and to be prepared for any memory test concerning the content of the text. Four subjects were assigned to each of four groups. The groups and text form a 4 x 4 Latin square by which each subject read each article with the four treatments. Thus, each subject received all tests and treatments, but the text-treatment combinations differ for the four groups. The order of text-treatment combinations was randomized, and each subject received a different order of insertions at the first.

The treatments were (a) skip numerals, (b) subjects were to read orally, quickly, but with clear articulation, without saying the inserted numerals, (c) skip letters. Subjects were to read orally, quickly, but without saying the inserted letters; and (d) say numerals. The inserted numerals were to be read orally, but quickly, and not at the same time. The test procedure was as described in the text. Following each treatment, subjects were given the memory test for the text assigned to the treatment. The four treatments were given in a single session lasting about 1 hr.

Data analysis. In a Latin square in which rows (subject) and columns (treatments) were independent variables, treatment effects may be tested with an unbiased F ratio (Winer & Keough, 1955, 1957). Thus the use of split-plot statistics (Clark, 1973) may be avoided. The analysis began with a mean over subjects for each of the 16 cells in the Latin square. Variance for that the grand mean for the five A, B, C, and D tests, subject groups, and test conditions for 3 of 4 and 6 of 6 were reduced for the estimation of error.

The treatments formed a 2 x 3 design. However, it proved to be informative to parcellate the constituent variance into the following set of orthogonal comparisons: (a) the difference between the skip mesh and the say mesh, averaged over category of insertion; (b) the difference between letter and numeral categories within the skip conditions; (c) the difference between letter and numeral categories within the say condition.

Analyses were done separately for two dependent variables: (a) the logarithm of reading speed was measured in words of the text (not counting insertions) per second. Logarithms were used because log speed is approximately proportional to minutes log time. Consequently, results of P tests are identical for analyses of log speed and log time, an equivalence that does not obtain when raw speeds and times are utilized (Wainer, 1971). (b) Memory detection of target phrases was measured by % in completing of, near and far, words were to determine an overall rate of false alarms. The difference between near and far was not theoretically significant; it was incorporated in the design only to ensure a range of difficulty for items on the memory test. We also analyzed a criterion variation in initial detection latency, but failed to find noteworthy effects. Mean values of .000 (not significantly) from .00 to 1.00 over the subject group and columns (text) are both random variables that were earlier still in subsequent experiments. Therefore, findings unreported in this analysis were not discussed.

Results and Discussion

The findings are summarized in Table 1. As would be expected, the effect of treatment takes to a certain extent a form, a text was lengthened considerably when it was read aloud with the text. This effect is reflected in the significant increase in number of text words read per sec, FL, 61, = 3.89. The meaning of this difference in speed is ambiguous because there was almost as much to be said in the skip condition as in the say condition. In fact, for the numerals, saying approximately the same words, the mean reading speed of skipping of the reduction in speed was much smaller for letters.

There is less ambiguity concerning effects within the skip condition and within the say condition. When insertions were to be said,
it made no difference whether letters or numerals were the insertions, \( F(1, 6) = 0.6 \). In contrast, when insertions were to be skipped, numerals were skipped much faster than letters, \( F(1, 6) = 171 \). The difficulty of skipping inserted letters may be appreciated by comparing the speeds for skip letters and say letters. Not saying a letter insertion took almost as long as saying it. This pattern of results is consistent with published findings on the categorical distinction between letters and numerals in visual search (as reviewed by Taylor, 1978).

Insight into the nature of the category effect came from an analysis of intrusion errors in the skip condition. An intrusion occurred when a subject said an insertion that was supposed to be skipped. For letters, the median number of intrusions was 4, and the range was 2–6. No subject made any intrusion error for numerals. Apparently, numerals were readily segregated from the stream of processing before the process of articulation was reached. Inserted letters may have been problematic because they were difficult to segregate from segments of the text that were targeted for articulation.

A critical question was whether the pattern of results for memory was parallel to that for speed of reading. It was not. There was a modest but nonsignificant difference in memory detection (\( d' \)) between the say and skip conditions \( F(1, 6) = 3.31 \). Within the say condition, there was no memory difference between numerals and letters, \( F(1, 6) = 0.6 \). Most important was the failure to find a memorial difference between skip letters and skip numerals, \( F(1, 6) = 0.6 \). The difficulty of segregating letter insertions from the stream of articulation had no discernible effect on what the reader remembered about the phrasing of the text.

Null findings were obtained for effects of insertion type on memory even though the experiment was reasonably powerful. Experimental power was evident in the small value of the standard error of the \( d' \) means; this value was \( .13 \).

Experiment 2: Replication, With Extensions

Experiment 2 showed that inserted numbers slowed oral reading by more than 75% but had no significant effect on memory. Despite the reasonable power of Experiment 1, it is arguable that small effects could have eluded statistical significance. The experiment was replicated, therefore, with a larger sample. In addition, it is plausible that a null effect for memory of phrasing is superficial, and that a significant effect would be evident in the reader's memory of deeper meanings. Consequently, the scope of inquiry was extended by using a greater diversity of texts and by testing subjects for memory of both means and phrasing.

Method

Subjects. Forty-eight students who were free of speech defects received credit in an introductory psychology course for participating in the experiment.

Table 1

<table>
<thead>
<tr>
<th>Category of insertion</th>
<th>Numerals</th>
<th>Letters</th>
<th>M</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reading speed (wps)</td>
<td>2.21</td>
<td>1.24</td>
<td>1.53</td>
</tr>
<tr>
<td>Say</td>
<td>1.07</td>
<td>1.08</td>
<td>1.07</td>
</tr>
<tr>
<td>Memory phrases (d')</td>
<td>1.29</td>
<td>1.41</td>
<td>1.35</td>
</tr>
<tr>
<td>Say</td>
<td>0.78</td>
<td>1.05</td>
<td>1.11</td>
</tr>
</tbody>
</table>

Note. Means are reported in words per second (wps), but analyses were performed on log (wps). *p < .001.
Table 2

<table>
<thead>
<tr>
<th>Instruction</th>
<th>Numerals</th>
<th>Letters</th>
</tr>
</thead>
<tbody>
<tr>
<td>Skip Reading speed (wps)</td>
<td>5.22</td>
<td>1.30</td>
</tr>
<tr>
<td>Say</td>
<td>1.05</td>
<td>1.07</td>
</tr>
<tr>
<td>Memory phrases (d')</td>
<td>1.41</td>
<td>1.41 **</td>
</tr>
<tr>
<td>Skip</td>
<td>1.20</td>
<td>1.10</td>
</tr>
<tr>
<td>Say</td>
<td>1.05</td>
<td>1.15</td>
</tr>
<tr>
<td>Memory statements (d')</td>
<td>1.05</td>
<td>1.05</td>
</tr>
<tr>
<td>Say</td>
<td>.58</td>
<td>.71</td>
</tr>
</tbody>
</table>

Note. Means are reported in words per minute (wps), but analyses were performed on log (wps).

The data on intrusions, errors also replicated Experiment 1. In the skip numeral treatment, 46 subjects made no errors, and the remaining 2 subjects had 1 and 3 intrusions, respectively. In the skip letters treatment, the number of intrusions made by a subject ranged from 0 to 15, with a median of 5. Forty-four subjects made more errors in skip letters than in skip numerals, and 4 subjects had no errors in either condition. The results for memory, as measured by d', were comparable for phrases and statements and were consistent with Experiment 1. The mean d' was greater when interjections were skipped than when they were said, both for phrases (F(1, 6) = 5.41, and for statements, F(1, 6) = 16.1. (For phrases in Experiment 1, this effect had the same direction and magnitude but was statistically unreliable.) A difference between skip numerals and skip letters, which was strongly evident in reading speeds, failed to occur in detection of phrases and statements, F(1, 6) = .03 and .55, respectively. The hypothesis of no difference in memorial detection between say numerals and say letters was tenable, F(1, 6) = 2.99 for phrases and F(1, 6) = 3.99 for statements, in agreement with the finding of no difference in reading speed between these treatments.

Results and Discussion

The results are summarized in Table 2. The findings for reading speed were exactly as in Experiment 1. Subjects finished reading a text faster when insertions were skipped rather than said, F(1, 6) = 56.7. Numerals were skipped faster than letters, F(1, 6) = 388, but numerals and letters were said with equal speed, F(1, 6) = .76.

Texis. There were four groups of texts, written by four writers, E, F, G, and H. Each group consisted of (a) a 375-word article excerpted from a local newspaper, (b) a 400-word abridgment of a literary or expository passage from the Scholastic Aptitude Test (SAT), and (c) a 375-word abridgment of a difficult literary or expository passage from the American Reading Examination (GRE). Thus there were 12 texts that varied in difficulty, in subject matter, and in style. The texts were adjusted, and insertions were placed in them in the same manner as in Experiment 1.

Memory test. To test memory of phrases, 20 three- word phrases were selected from each text. Ten phrases were targets; 5 phrases were made four turns by replacing one word with a word of the same syntactic function, after a synonym; and 5 phrases were made four turns by replacing two words. Memory of meaning was tested with statements. Half of the statements were true expressions of ideas in the text and half were contrary to the text. There were seven opportunities for each paragraph, one for each SAT passage, and seven for each GRE passage. Perseverations and synonyms of the wording of the texts were used in the statements whenever possible to maximize the differences between the memorial requirements of the task of meaning and phrasing.

The phrase (Ph) for each text were randomly divided into two sets, to be the statements (St) in testing. In testing, the subject was given the set A for a text in one of the counterbalanced order of PSAT or PSY. Texts were given immediately after a subject finished reading each text. Subjects indicated whether they thought a phrase had occurred in the text and whether they believed a statement to be true in relation to the text.

Design, procedure, and analysis. The Latin square design was used in counterbalanced treatments, texts, and orders in the present experiment was the same as in Experiment 1. A subject was tested in two sessions, 1- 2 days apart, each lasting about 1 hr. Two treatments were given within each matched pair of texts assigned to a treatment given in a booklet. For example, (1) PSAT, and GRE texts in Group 6 were assigned to skip numerals, and for 12 subjects were given subsequently, and were followed by a different group of 12 subjects on a second treatment. Means of 38 subject-text combinations (12 subjects x 3 texts) were computed for each of the 16 cells in the Latin square. Analyses of variance were performed on these means for the independent variables of speed and memorial detection. In all other respects, the procedures were identical to those of Experiment 1.
The findings confirmed and extended those of Experiment 1. Difficulty in segregating letter insertions from the stream of articulation in oral reading affected memory for phrasing and meaning to at most a small degree. The null findings were probably not a consequence of inadequate experimental power for two reasons: First, statistically significant differences were obtained in some cases. Most notably, they were obtained in comparisons of saying versus skipping insertions. That both speed and memory exhibited the effect of poorer performance in the say condition, whereas only speed exhibited the additional effect of a difference between letters and numerals within the skip condition. Second, d' meant were very accurate, having uniformly small standard errors: .04 for phrases and .11 for statements (cf. Footnote 1). Experiment 3: Control Condition With No Insertions The goal of the third experiment was to establish a baseline against which readers' performance in the skip numeral and skip letter treatments could be compared. The baseline condition was normal oral reading of a text containing no insertions. Data on memory in the baseline condition could strengthen the conclusion, implied by Experiments 1 and 2, that the skip numerals and skip letters treatments have no noticeable effect on memory.

Method Subjects. Forty-four students who were free of speech defects received credit in an introductory psychology course for participating in the experiment. Texts. There were three groups of texts, hereafter designated 1, 2, and 3. Each group contained an edited newspaper article, SAT passage, and GRE passage, as in Experiment 2. The texts were the same as in Experiment 2, but were arranged in order to match the groups equal in memory difficulty, based on the data of Experiment 2. Design and analysis. The three treatments were skip numeral, skip letters, and no insertions. In the noinsertion treatment, neither numerals nor letters were inserted between words of the texts. The Latin square GOFs of Experiments 1 and 2 was ill-advised because a 3 x 3 square would provide only 1 d' for error. An alternative design was used, therefore, in which a subject was randomly assigned to one of the six possible arrangements for associating test text groups with three treatments. In this design, considering both subjects and test groups to be random, an unbiased P ratio for treatment effect was computed with the Treatments x Subject interaction as error (Scheffe, 1959; Wilk & Kempthorne, 1955, 1957).

Procedure. A subject received all three treatments in a single session that lasted about 1 hr. The order of treatments was randomly determined for each subject. In all other respects the procedures were identical to those of Experiment 2.

Results and Discussion The results are summarized in Table 3. There was significant variation in reading speed over the three treatments, F(2, 86) = 4.17. Although the same sequence of words was articulated in each treatment, readers were 20% faster without insertions than with inserted numerals and were 76% faster with inserted numerals than with inserted letters. Thus, the pattern of reading speeds showed a modest but significant difference between no insertions and skip numerals, and an even larger difference between skip numerals and skip letters. This pattern failed to occur for the memory measures. Neither omnibus test was significant, F(2, 86) = .02 and .21, for memory of phrases and statements, respectively, nor was any pairwise comparison between treatments significant. In short, there was no indication that substantial disturbances of reading speed had parallel effects on memory. As in the previous experiments, null findings were obtained despite small standard errors.

Table 3

<table>
<thead>
<tr>
<th>Variable</th>
<th>No insertions</th>
<th>Skip numerals</th>
<th>Skip letters</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reading speed (wpm)</td>
<td>2.79</td>
<td>2.32</td>
<td>1.32</td>
</tr>
<tr>
<td>Memory phrases (d')</td>
<td>1.42</td>
<td>1.41</td>
<td>1.43</td>
</tr>
<tr>
<td>Memory statements (d')</td>
<td>1.43</td>
<td>1.47</td>
<td>1.50</td>
</tr>
</tbody>
</table>

Note. Means are reported in words per second (wpm), but analyses were performed on log (wpm). Significant differences are determined by Tukey honestly significant difference tests. *= p < .05.
General Discussion

Findings for both intrusion errors and reading speed in all three experiments showed that visual recognition was more severely disturbed in the skip letter treatment than in the skip numerals treatment. In contrast, the two treatments had no differential effect on memory, whether assessed as verbal or memory for phrases or as memory of paraphrased statements.

To demonstrate the credibility of this conclusion, we pooled data from the three experiments, calculated grand means of the difference in \( d' \) between treatments, and computed .95 confidence limits for the mean difference. Evidence for the null conclusion would be very strong if the confidence intervals included zero and had upper and lower limits that were negligibly different from zero. Table 4 shows results of this analysis. The grand means were close to zero, and the limits of the confidence intervals showed that the largest tenable departure from zero was approximately one third of a standard deviation in \( d' \) units. The evidence for a null effect was very strong for phrases and was adequate but somewhat weaker for statements.

The lack of a memorial difference between skip numerals and skip letters, together with the consistent differences between these treatments for intrusion errors and reading speed, stands in marked contrast to the findings from Koler's version of the disturbance paradigm. Recall that in Koler's paradigm, slower reading is associated with better memory (Koler, 1975a, 1975b, 1976; Mason & Sala, 1978). Is it possible to explain the contradiction between Koler's findings and ours? And what does this contradiction imply for theories of reading? There are, we think, at least three ways to resolve these questions.

The concept of time-sharing. One resolution concerns the distinction between severe and moderate disturbance. On first reading, a paragraph or an isolated sentence takes from three to six times longer to read if the letters are all upside down than if the

<table>
<thead>
<tr>
<th>Table 4</th>
<th>Grand Mean of Skip Numerals Minus Skip Letters for Memory Variables and .95 Confidence Limits of the Grand Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td>( d' )</td>
<td>Grand Mean</td>
</tr>
<tr>
<td>Memory phrases</td>
<td>(-23)</td>
</tr>
<tr>
<td>Memory statements</td>
<td>(-69)</td>
</tr>
</tbody>
</table>

Note. The grand mean is averaged over all subjects and experiments.

letters are normally oriented. A text with inserted numbers that are spelled in letters takes about twice as long to read as a normal text. Thus our procedure was different from Koler's in the severity of the disturbance imposed on visual recognition. Perhaps compensatory reliance on extensive memorial processing becomes obligatory only when the disturbance of visual recognition is extreme. When the disturbance is moderate, and perhaps when there is no disturbance, there may be a different mode of interaction. Although compensation is the mode for severe disturbance, time-sharing may be the mode for moderate disturbance.

The time-sharing hypothesis clarifies an

1. In the design of this experiment, the standard error was estimated by the conventional formula \( s/\sqrt{N} \). Where \( s \) is the standard deviation computed over individual subject's data and \( N \) is the total number of subjects.
2. Pulsing of the text was somewhat problematic in that the design of experiment 3 differed from that of experiments 1 and 2. The solution to this problem was an analysis of variance with the following factors: (a) Experiment was a fixed factor, (b) Text pair was a random factor nested within experiments. The text pairs were AB and CD in Experiment 1, EF and GH in Experiment 2, and II, JI, and JK in Experiment 3. (c) Assigned treatment was a two-level fixed factor crossed with the preceding factors. To illustrate, at one level of this factor, skip letters were assigned to A and skip numerals to B. At the other level of the factor, the assignment was reversed. (d) Last was a random factor nested within all other factors. The raw data for the analysis were differences between each subject's scores in two treatments: skip numerals minus skip letters. The grand mean of the grand mean was computed with variance estimated by the mean square for text pair pulse with the mean square for Text Pair X Assigned Treatment. This estimate was used because it's expected value is a linear combination of the variance among subjects when there are random rather than fixed treatments, thus satisfying the Text Pair X Assigned Treatment interaction to be negligible (cf. Winer, 1971, pp. 211-272).
important point. What really matters is not whether extra time is taken to read but how the additional time is used. Time may be allocated for compensatory memorial processing, in which case it is plausible that comprehension and memory might improve. However, time could also be allocated for more extensive perceptual processing or for insertion of an extra stage of processing that is additive rather than interactive in relation to other processes. In these two cases, readers may be able, within limits, to delay computing and storing the meaning of a text while waiting for completion of perceptual processing or of an inserted process. Conversely, readers may also be able to accelerate computing and storing meaning to accommodate an increased rate of perceptual processing (Fleischer, Jenkins, & Pansky, 1979). In short, the essence of time-sharing is that various component processes within the larger process of reading may be mutually regulated to achieve compatible rates.

When time-sharing occurs, the informational output of visual processing is approximately constant. Similarly, the output of comprehension and memory is roughly constant. What varies is not the amount of output but the time required to achieve that amount. The time-sharing hypothesis describes the available data accurately. It fails, however, to specify why time-sharing occurs in our paradigm but does not in Kierls paradigm.

The distinction between data-driven and conceptually driven processing. A second explanation concentrates on the way in which visual recognition is disturbed, rather than on the severity of the disturbance. It may be significant that Kierls paradigm, in which words are spelled with rotated or inverted letters, disturbs recognition of the very words that convey the meaning of the text. In our paradigm, however, the words that convey meaning are readily recognizable. What is disturbing to the reader in our paradigm is the presence of meaningless words among the meaningful. Moreover, the meaningful words can be distinguished from the meaningless insertions by processes that rely on visual information alone.

In our paradigm, no reliance on semantic context is necessary. Reliance on semantic constraints imposed by the neighboring text is apparently helpful, however, for identifying words in Kierls paradigm. Perhaps insertion of meaningless numbers is a disruption that may be resolved by data-driven processes, whereas transformation of the spatial orientation of letters is a disruption best resolved by conceptually driven processes. Thus the form of disruption may determine the manner of processing. In turn, the manner of processing may determine the quality of memory for meaning, better memory occurring when conceptually driven processing is more extensive.

The strength of this account is that it explains both our findings and those from Kierls paradigm. A weakness, however, is that it comes uncomfortably close to being a post hoc description rather than an explanation. Lacking are test theoretical postulates for predicting what kinds of visual disruptions force conceptually driven processing to occur and why they force it to occur.

The distinction between controlled and automatic processing. A third possible explanation concerns how the relevant words of a text are recognized. In Kierls paradigm, they must be recognized in an unfamiliar orientation. To identify oddly oriented words may require controlled processing (Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). In our paradigm, the relevant words appear as they normally do. They may, therefore, be recognized automatically (Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). Perhaps memory for meaning improves when controlled processing must be invoked because more attention is given to the wording of the text and more meaning is therefore gleaned from the words. In contrast, perhaps memory for meaning is unaffected by a disruption of the reading process that allows word recognition to remain automatic.

This final account has the virtue of explaining all of the available data while providing a criterion that predicts whether a disruption will affect memory. A weakness of this account, however, is that it is indif-

ferent to the distinction, in our reading, between letters and numerals as insertions. Letter insertions took much longer to discard and produced more errors in oral reading.
than did the inserted numerals. It is plausible, therefore, that letter insertions had to be segregated from the relevant text by content-controlled processing. Perhaps it is only when content-controlled processing is given to the content worlds themselves that memory improves. Such a hypothesis can explain the results but obviously needs to be studied further in future research.

Whatever the implications of our data for theories of reading, there are several empirical results to be reckoned with. We conclude, therefore, by summarizing the important findings.

First, interaction between speed of visual recognition and accuracy of memory is not obligatory. Rather it appears to be variable. Either the timing or the manner of interaction varies according to task demands.

Second, extraneous activity is sometimes a handicap in reading, but not always. Extraneous vocalizations in the key condition in Experiments 1 and 2 produced a modest but reliable decrement in memory for wording and meaning, as one might expect. However, the extraneous activity of having to bypass inserted numbers in the skip condition produced no decrement in memory, as was shown most clearly in Experiment 3.

Third, the categorical distinction between letters and numerals is important for the time required to segregate an insertion from the text. As in visual search, targets (words of the text) were found more rapidly when the constituent field had elements from a different category (inserted numerals) rather than from the same category (letter insertions). Most likely, effects similar to those we observed for numerals would also occur for other categorically distinctive insertions, such as words in italics, uppercase letters, or non-Roman letters.

References


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