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Variable Interaction Between Visual Recognition and Memory in Oral Reading

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Readers were given texts 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 inserted letters than in a baseline condition with no insertions. These disturbances of 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,

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Kolers (1975a, 1975b, 1976) altered the spatial orientation of the letters comprising a text and thereby slowed oral reading dramatically. However, rather than penalizing memory, Koler's disruption of normal reading actually improved performance on a subsequent test of verbatim sentence recognition. 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 visual analysis was well remembered and, consequently, the meaning of the text was also well remembered.

Koler's views were disputed by Masson and Sala (1978). These investigators replicated Koler's finding of improvement in verbatim memory for sentences read under visual transformation. In addition, however, Masson and Sala showed that readers had better memory for paraphrases of transformed 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., Rumelhart, 1977) assumes a continuous interplay 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 enriches memory for meaning. A reader's better memory for visually transformed 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 research was to investigate the hypothesis of compensatory interaction and its possible obligatory nature.

Our experimental procedure is one instance of what may be called *disturbance paradigms*. In these paradigms, one cognitive component of reading is experimentally disturbed, and an assessment is made of the effect of the disturbance on a second component. For example, in Koler's procedure, visual recognition is disturbed by presenting a text in a spatially transformed orientation, and memory for the text is subsequently assessed. In our procedure, visual recognition was disturbed by requiring subjects to read meaningful texts 666 that 5555 either 33333 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 substantial evidence from this paradigm (Brand, 1971; Ingling, 1972; Sperling, Budiansky, Spivack, & Johnson, 1971; Taylor, 1978) that subjects are better able to detect a target character within a field of characters if the target and field represent different categories (e.g., letters and numerals) than if they have the same category (e.g., both letters). We anticipated a similar finding in our disturbance paradigm. Readers were expected 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 affected by inserted numerals or letters and by instructions to say or to skip the insertions.

Method

Subjects. Sixteen students enrolled in introductory psychology received 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.

Texts. 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 lead stories concerning well-known national or local events. Each article was edited to be 350 words long, and all numbers were deleted from the article. An insertion from the set three, five, six, eight, nine or from the set 33333, 5555, 666, 88888, 9999 was placed after every word in an article except the last word. Each insertion was a monosyllabic singular digit with no homophone that occurred in any of the articles. A numeral insertion and the corresponding letter insertion had the same number of characters and were pronounced the same. The sequence of insertions was random, with the restrictions that (a) no digit occurred twice in a row; (b) a subsequence of five digits was sampled randomly from the set without replacement, followed by a similar subsequence; 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. The phrases were evenly distributed throughout a text. Ten additional phrases of the same length were selected from portions of an article that had been in the original version in the newspaper but had been edited out of the version used in testing. These phrases were near lures. Ten far lures were three-word phrases from newspaper articles other than the articles for Texts A, B, C, and D. The lures (both near and far) and target phrases (without insertions) were presented in a single randomized order on a typed sheet on which subjects judged whether a phrase had appeared in the text.

Design and procedure. A text was presented to a subject on typed pages. The subject was instructed to read quickly but with clear articulation and to be prepared for a memory test concerning the content of the text. Four subjects were assigned to each of four groups. The groups and texts formed a 4 × 4 Latin square by which the groups and texts were counterbalanced with four treatments. Thus each subject received all texts and treatments, but the text–treatment combinations differed for the four groups. The order of treatments was counterbalanced with a second Latin square orthogonal to the first.

The treatments were (a) skip numerals: Subjects were to read orally, quickly but with clear articulation, without saying the inserted numerals; (b) skip letters: Subjects were to read orally without saying the inserted letters; (c) say numerals: The inserted numerals were to be vocalized as the text was read orally; (d) say letters: The inserted letters were to be read orally with the text. Each treatment was preceded by having the subject practice the reading procedure on a brief sample passage. 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 group) and columns (text) are both random variables, treatment effects may be tested with an unbiased F ratio (Wilk & Kempthorne, 1955, 1957). Thus the use of quasi-F 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 due to the grand mean accounted for 1 *df*; texts, subject groups, and treatments each accounted for 3 *df*; and 6 *df* remained for the estimate of error.

The treatments formed a 2 × 2 design. However, it proved to be informative to partition the treatment variance into the following set of orthogonal comparisons: (a) the difference between the skip mean and the say mean, averaged over category of insertion; (b) the difference between letter and numeral categories within the skip condition; and (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 proportional to minus log time. Consequently, results of F tests are identical for analyses of log speed and log time, an equivalence that does not obtain when raw speeds and times are analyzed (Wainer, 1977). (b) Memorial detection of target phrases was measured by d'. In computing d', near and far lures were pooled to determine an overall rate of false alarms. The difference between near and far lures 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 β, the criterion variable in signal detection theory, but failed to find noteworthy effects. Mean values of β varied nonsignificantly from .89 to 1.03 over the four conditions of Experiment 1. The range of values was smaller still in subsequent experiments. Therefore, findings concerning β will not be discussed.

Results and Discussion

The findings are summarized in Table 1. As would be expected, the time taken to read a text was lengthened considerably when insertions had to be said along with the text. This effect is reflected in the significant increase in number of text words read per sec, $F(1, 6) = 189$. The meaning of this difference in speed is ambiguous because there was half as much to be said in the skip condition as in the say condition. In fact, for numerals, saying approximately halved the mean reading speed of skipping; the reduction in speed was much smaller for letters, however.

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, $F(1, 6) = .06$. 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 2–36. 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 memorial detection (d') between the say and skip conditions $F(1, 6) = 3.31$. Within the say condition, there was no memorial difference between numerals and letters, $F(1, 6) = .66$. Most important was the failure to find a memorial difference between skip letters and skip numerals, $F(1, 6) = .46$. 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.[1]

Table 1
Means in Experiment 1 by Category and Instruction

Instruction	Numerals	Letters	*M*
	Category of insertion		
	Reading speed (wps)		
Skip	2.21 *	1.24	1.65
			*
Say	1.07	1.08	1.07
	Memory phrases (d')		
Skip	1.29	1.41	1.35
Say	1.19	1.05	1.11

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

Experiment 2: Replication, With Extensions

Experiment 1 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 meaning 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.

[1] In the design of this and the next experiment, the sampling distribution of the mean for a row (or column) of the Latin square has variance that depends on k, the number of rows (or columns). This variance is estimated by MS_e/k; the reported standard error is the square root of this estimate. This standard error depends indirectly on n, the number of subjects within a cell of the square. The within-cell mean of n subjects' individual d' values has a standard error inversely proportional to $(n)^{1/2}$. The row (or column) mean, under the design, is the mean of these within-cell means, averaged over k rows (or columns), and MS_e estimates the random variance among the within-cell means. Thus MS_e estimates s^2/n, where s^2 is the variance among individual subjects' d' values. Hence, MS_e/k estimates s^2/kn, where kn is the total number of subjects in a row (or column). Therefore, the square root of MS_e/k may be interpreted as both the standard error of (a) the marginal mean of within-cell means of d' and (b) the mean of individual subjects' d' values over all subjects in the experiment.

Texts. There were four groups of texts, hereafter designated 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 Graduate Record Examination (GRE). Thus there were 12 texts that varied extensively in subject matter, style, and difficulty. The texts were edited, 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 near lures by replacing one word with a word of the same syntactic function, often a synonym; and 5 phrases were made far lures 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 statements for each newspaper article, six for each SAT passage, and seven for each GRE passage. Paraphrases and synonyms of the wording of the texts were used in the statements wherever possible to maximize the difference between the memorial requirements of the tests of meaning and phrasing.

The phrases (P) for each text were randomly divided into two sets, as were the statements (S). In testing, the subjects were given the sets for a text in one of the counterbalanced orders PSPS or SPSP. Tests 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 used to counterbalance treatments, texts, and orders in the present experiment was the same as in Experiment 1. A subject was tested in two sessions, 1–7 days apart, each lasting about 1 hr. Two treatments were given in each session. Within a session, the three texts assigned to a treatment were given in a block For example, the news, SAT, and GRE texts in Group E were assigned to the skip numerals treatment for 12 subjects, were given successively, and were followed by a different group of texts assigned to a second treatment. Means over 36 subject–text combinations (12 subjects × 3 texts) were computed for each of the 16 cells in the Latin square. Analyses of variance were performed on these means for the dependent variables of speed and memorial detection. In all other respects, the procedures were identical to those of Experiment 1.

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, $F(1, 6) = 567$. Numerals were skipped faster than letters, $F(1, 6) = 388$, but numerals and letters were said with equal speed, $F(1, 6) = .76$.

Table 2
Means in Experiment 2 by Category and Instruction

	Category of insertion			
Instruction	Numerals		Letters	*M*
	Reading speed (wps)			
Skip	2.22	**	1.30	1.66
				**
Say	1.10		1.05	1.07
	Memory phrases (d')			
Skip	1.41		1.42	1.41
				**
Say	1.20		1.10	1.15
	Memory statements (d')			
Skip	1.18		1.05	1.11
				*
Say	.58		.85	.71

Note. Means are reported in words per second (wps), but analyses were performed on log (wps).
* $p < .01$. ** $p < .001$.

The data on intrusion errors also replicated Experiment 1. In the skip numerals 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 insertions were skipped than when they were said, both for phrases $F(1, 6) = 54.1$, 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, $Fs(1, 6) = .03$ and $.55$, respectively. The hypothesis of no difference in memorial detection between say numerals and say letters was tenable, $F(1, 6) = 2.89$ for phrases and $F(1, 6) = 3.99$ for statements, in agreement with the finding of no difference in reading speed between these treatments.

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. Thus 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' means 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 numerals and skip letters 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 notable 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 I, J, and K. 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 regrouped to make the groups equal in memorial difficulty, based on the data of Experiment 2.

Design and analysis. The three treatments were skip numerals, skip letters, and no insertions. In the no-insertions treatment, neither numerals nor letters were inserted between words of the text. The Latin square design of Experiments 1 and 2 was ill-advised because a 3 × 3 square would provide merely 2 *df* for error. An alternative design was used, therefore, in which a subject was randomly assigned to one of the six possible arrangements for associating three text groups with three treatments. In this design, considering both subjects and text groups to be random, an unbiased F ratio for treatment effect is computed with the Treatment × Subject interaction as error (Scheffé, 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 speeds over the three treatments, $F(2, 86) = 417$. 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 .01, 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 effect on memory. As in the previous experiments, null findings were obtained despite small standard errors

Table 3
Means in Experiment 3 by Variable and Condition

Variable	No insertions		Skip numerals		Skip letters
Reading speed (wps)	2.79	*	2.32	*	1.32
Memory phrases (d')	1.42		1.41		1.43
Memory statements (d')	1.43		1.47		1.50

Note. Means are reported in words per second (wps), but analyses were performed on log (wps). Significant differences are determined by Tukey honestly significant difference tests.
* $p < .001$.

of the d' means: .05 for phrases and .10 for statements.[2]

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 letters treatment than in the skip numerals treatment. In contrast, the two treatments had no differential effect on memory, whether assessed as verbatim 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.[3] 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 Kolers's version of the disturbance paradigm. Recall that in Kolers's paradigm, slower reading is associated with better memory (Kolers, 1975a, 1975b, 1976; Masson & 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 timesharing. 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 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 Kolers'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

Table 4
Grand Means of Skip Numerals Minus Skip Letters for Memory Variables and .95 Confidence Limits of the Grand Means

d'	Grand M	Confidence limits
Memory phrases	−.03	−.12 to +.05
Memory statements	.09	−.16 to +.33

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

[2] In the design of this experiment, the standard error is estimated by the conventional formula $s/(N)^{1/2}$, where s is the standard deviation computed over individual subject's d' values and N is the total number of subjects.

[3] Pooling of the data was somewhat problematic because 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 experiment. The text pairs were AB and CD in Experiment 1, EF and GH in Experiment 2, and IJ, IK, 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 was assigned to A and skip numerals to B; at the other level of the factor, the assignment was reversed. (d) Subject 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 standard error of the grand mean was computed with variance estimated by the mean square for text pair pooled with the mean square for Text Pair × Assigned Treatment. This estimate was used because its expected value is a linear combination of the variance among random subjects and among random text pairs, assuming the Text Pair × Assigned Treatment interaction to be negligible (cf. Winer, 1971, pp. 371–375).

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, & Pany, 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 Kolers's 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 Kolers's 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 meaning*less* words among the meaning*ful*. 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 Kolers's 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 Kolers's paradigm. A weakness, however, is that it comes uncomfortably close to being a post hoc description rather than an explanation. Lacking are firm 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 Kolers's 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 indifferent to the distinction, in our research, 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 controlled processing. Perhaps it is only when controlled processing is given to the content words 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 say 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 contextual 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.

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