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Evidence for Auditory Temporal Distinctiveness: Modality Effects in Order and Frequency Judgments

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Two new, long-lasting phenomena involving modality of stimulus presentation are documented. In one series of experiments we investigated effects of modality of presentation on order judgments. Order judgments for auditory words were more accurate than order judgments for visual words at both the beginning and the end of lists, and the auditory advantage increased with the temporal separation of the successive items. A second series of experiments investigated effects of modality on estimates of presentation frequency. Frequency estimates of repeated auditory words exceeded frequency estimates of repeated visual words. The auditory advantage increased with frequency of presentation, and this advantage was not affected by the retention interval. These various effects were taken as support for a *temporal coding assumption*, that auditory presentation produces a more accurate encoding of time of presentation than does visual presentation.

A confluence of research has led to the rather extraordinary claim that audition is more specialized for temporal encoding than is vision. Some aspects of this claim may seem intuitive, for instance, that audition is specialized for integrating information over time, whereas vision is specialized for integrating information over spatial extents (Fraisse, 1963; O'Connor & Hermelin, 1972). Also, that discriminability of durations is dependent on modality (Allan, 1979) does not seem newsworthy. The new claim is that auditory presentation results in a more accurate code for the time of presentation of events, and that this difference is not transitory, but can be revealed in memory tasks long after the events have occurred. We will refer to this as the *temporal coding assumption*.

In this article we briefly review some of the research that has led to the temporal coding assumption. Then, we present two series of experiments that use different methods to adduce evidence consistent with the assumption. In preview, the weight of the evidence in both series of experiments supports the assumption.

The temporal coding assumption has been invoked in attempts to explain the modality effect. In brief, the modality effect is the superior recall of recently presented auditory information compared with recall of the same information presented visually. The difference between the input modalities is almost always confined to the end of the list (but see Glenberg & Swanson, 1986). Until a few years ago, the most widely supported explanation was based on a brief echoic memory that could be used to boost recall of the last few auditory events (Crowder & Morton, 1969). More recently, this account has given way in the face of demonstrations that (a) the modality effect can be long-lived (Gardiner & Gregg, 1979; Glenberg, 1984; Greene, 1985), (b) it can be found in the absence of auditory input (Nairne & Walters, 1983), and (c) it does not interact as expected with auditory noise (Crowder, 1986).

Although a number of alternatives to echoic memory accounts of the modality effect have been offered (e.g., see Campbell & Dodd, 1980; Penny, 1985), we focus on those consistent with the temporal coding assumption. Gardiner (1983) proposed that all recency effects may reflect the operation of a distinctiveness principle, that distinct encodings are easier to recall than nondistinct encodings. Furthermore, he speculated that auditory presentations are *temporally* more distinct than visual presentations. In support of this speculation, Gardiner cited research by Metcalfe, Glavanov, and Murdock (1981) indicating that auditory presentation produced better performance than visual presentation when subjects were asked to recall information in temporal order. When subjects were to recall by spatial position, the advantage was eliminated.

Other evidence is also consistent with the temporal coding assumption. For example, Glenberg and Swanson (1986) manipulated the interval between successive presentations of both auditory and visual to-be-remembered (TBR) items. If coding of time of presentation is more accurate (or more sensitive) for auditory than for visual presentations, then the manipulation should have a greater effect for the auditory items than for the visual items. In fact, increasing the interitem interval improved recall of recent auditory items and had little effect on the recall of visual items.

Modality and Order Judgments

If the temporal coding assumption is valid, we should be able to find evidence of auditory superiority in tasks that directly tap temporal (or, at least, ordinal) information. One such task requires order judgments. After presentation of a

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list of TBR items, the subject is given two of the items and is required to specify their order in the previously presented list. The temporal coding assumption leads us to expect more accurate judgments for auditory items than for visual items. The results from Experiments 1–3 are generally consistent with this expectation. After presentation of the experiments, we will develop an explanation based on the temporal coding assumption.

Experiment 1

In Experiment 1, subjects studied lists of auditory and visual TBR items. Some of the lists were followed by a free-recall test, and some were followed by an order-judgment test. The type of test was concealed from the subject until after the list was presented. The free-recall test served two purposes. First, it illustrated that the conditions of the experiment do produce a modality effect in recall. Second, as we will see, the interaction between modality and serial position is different in free-recall and order-judgment tests, providing substantial grist for the theoretical mill.

Temporal judgments could be based on features that are relatively uninteresting. For example, a subject could attempt to intentionally associate a TBR item with its ordinal position. To try to prevent these sorts of strategies, we used word pairs as TBR items, as well as an orienting task designed to focus processing on the words within a pair. If the orienting task does not completely control processing, we reasoned that subjects would be more likely to devote their time to forming associations between words within a pair, rather than between a pair and its serial position.

Method

Materials and design. Subjects studied 18 lists, each composed of six word pairs. For 9 of the lists, the words pairs were presented auditorily, and for 9, the words were presented visually. Within each modality, 3 of the lists were followed by free-recall tests, and 6 of the lists were followed by order-judgment tests. The sequence of conditions (modality and type of test) was randomized for each subject within the constraint that each successive triplet of lists contained 1 auditory list and 1 visual list followed by the order judgments and 1 list followed by free recall.

The order-judgment test consisted of three comparisons. For each comparison, the subject viewed two TBR pairs that had been presented in adjacent serial positions. The subject indicated which of the pairs had been presented most recently. The three comparisons consisted of the pairs from serial positions 1 and 2, 3 and 4, and 5 and 6. The order of the three comparisons was counterbalanced over lists.

The word pairs were constructed from a set of common, four- and five-letter, one- and two-syllable nouns. The words in a pair were not obviously related. Assignment of pairs to lists was randomized for each subject. The word pairs were recorded using an Instavox, a device that records an analog signal so that it can be randomly accessed within 400 ms. Each pair was recorded within a 1,600-ms window. For auditory presentation, an empty 1,400-ms period was appended to the window for a total presentation time of 3,000 ms. For visual presentation, the word pair was displayed on a video display for 3,000 ms.

While each pair was being presented, the subject responded to the orienting task using a three-button panel connected to the Apple II computer controlling the experiment. The subject pushed one button if the first (or left-hand) word referred to the larger object, and pushed another button if the second (or right-hand) word referred to the larger object.

Preceding each item was a 4-s interpresentation interval (IPI). This interval was filled with a two-component distractor task. The first component was evaluating an arithmetic equation (to engage working memory). Every 2 s, the subject viewed three addends and a sum. One button was pushed to indicate that the sum was correct, and a second button was pushed to indicate that the sum was incorrect. After every five problems, the range of the addends was adjusted to keep the subject at approximately 80% correct. The second component was the overt articulation of the syllable *blah* 2–4 times a second (to engage an articulatory loop). The last item of the list was followed by a 10-s retention interval filled with the distractor task.

Procedure. Subjects practiced the distractor task until it could be performed proficiently. Next, a four-pair visual practice list was presented (with the distractor task). This list was followed by free recall. A four-pair auditory practice list, followed by two order judgments, was presented next. The 18 experimental lists followed. Each list was preceded by an indication of the modality of the word pairs (but not the type of test).

Subjects. Twenty subjects were students attending the summer session at the University of Wisconsin—Madison. These subjects were paid for their participation. An additional 30 subjects were recruited from students enrolled in introductory psychology courses during the regular school year. For these students, participation in the experiment was one option for fulfilling a course research requirement.

Results

The results from the free-recall test are presented in Figure 1. The data are the percentage of words (not pairs) recalled from each serial position. There was a robust modality effect in that auditory presentation exceeded visual presentation at the end of the list. These conclusions are reinforced by an analysis of variance on the number of words recalled at each position (with the significance level set at .05 for this and all subsequent analyses). There were significant main effects for modality, $F(1, 49) = 15.81$, $MS_e = 1.28$, and position, $F(5, 245) = 66.60$, $MS_e = 1.68$. Their interaction was also significant, $F(5, 245) = 13.62$, $MS_e = 1.41$, indicating that the

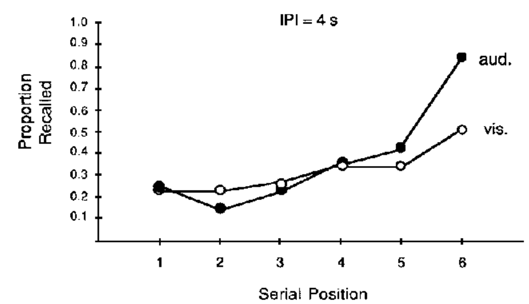

Figure 1. Experiment 1: Proportion recalled as a function of serial position and presentation modality. (IPI = interpair interval in seconds.)

auditory superiority was greater at the end of the list than at the beginning.

The percentage of correct choices on the order judgment test are in Table 1. Clearly, the task was difficult, with an average performance of 65%, whereas 50% was chance performance. The main effect of modality was not quite significant, $F(1, 49) = 4.81$, $MS_e = 1.56$, $p = .09$. The main effect of position was significant, $F(2, 98) = 6.79$, $MS_e = 1.60$, indicating a bowed serial position curve. In addition, modality and serial position interacted, $F(2, 98) = 3.37$, $MS_e = 1.25$.

Three features of these data are of interest. First, consonant with the temporal coding assumption, we have evidence for auditory superiority. Second, the auditory superiority seems to be dependent on serial position in a most unexpected way. It seems to be at the beginning and end of the sequence, but not in the middle. Note, however, that the auditory superiority at the beginning of the sequence is small and not significant in this experiment. Third, the serial position curve is bowed, in contrast to the free-recall results (Figure 1).

Clearly, before we make too much of these results, their replicability needs to be checked, and that was one purpose of Experiment 2. A second purpose was to determine if the auditory superiority is sensitive to temporal manipulations.

Experiment 2

Glenberg and Swanson (1986) found that the free-recall modality effect increased with the duration of the IPI. Consistent with the temporal coding assumption, the increase was due predominantly to changes in the level of recall of auditory items; the recall of visual items was relatively unaffected by this temporal manipulation. In Experiment 2, the length of the IPI was manipulated. For one half of the subjects, the IPI was 16 s (filled with 8 addition problems). For the other subjects, the IPI was 0 s. We expected the modality differences to be larger in the 16-s IPI condition.

Method

Subjects in the 0-s IPI condition received 96 s of the distractor task before each list. Thus, the 0-s IPI condition and the 16-s IPI condition were equated with respect to the amount of practice on the distractor task and the time between successive lists. Midway through the 18 experimental lists, the subject was given the opportunity to take a short break. In other respects, the experiment was identical to Experiment 1.

In all, 16 subjects were randomly assigned to each of the 0-s and 16-s IPI conditions. These subjects were recruited from introductory psychology classes at the University of Wisconsin—Madison.

Table 1
Proportion of Correct Order Judgments: Experiment 1

	Positions tested			
Modality	1 and 2	3 and 4	5 and 6	*M*
Auditory	.70	.57	.74	.67
Visual	.66	.60	.63	.63
Difference	.04	−.03	.11	.04

Results

The proportions recalled are in Figure 2. The modality effect is confined to the end of the list, as indicated by the significant Modality × Position interaction, $F(5, 150) = 3.99$, $MS_e = 2.04$. There were also significant main effects of modality, $F(1, 30) = 14.43$, $MS_e = 1.33$, and of position, $F(5, 150) = 56.07$, $MS_e = 1.58$. On the basis of previous research (Glenberg, 1984; Glenberg & Swanson, 1986), we had expected a larger modality effect with the longer IPI. This was not found, probably because of a ceiling effect in recall of the last auditory TBR item in the 16-s IPI condition. The relevant interaction is significant in Experiment 3.

The order judgments are in Table 2. Analysis of these data demonstrated a number of significant effects, including an interaction involving modality serial position, and IPI, $F(2, 60) = 4.08$, $MS_e = 1.00$. For ease of comprehension, further analyses were conducted on the IPI = 0-s and IPI = 16-s conditions separately. For the IPI = 0-s condition, the only significant effect was for serial position, $F(2, 30) = 9.01$, $MS_e = 0.99$. All other Fs < 1.0, ps $> .7$. In contrast, for the IPI = 16-s condition, all effects were significant; for modality, $F(1, 15) = 5.43$, $MS_e = 0.77$; for serial position, $F(2, 30) = 9.36$, $MS_e = 0.88$; and for the interaction, $F(2, 30) = 8.23$, $MS_e = 0.89$.

These data both confirm and exend the three trends discovered in Experiment 1. First, once again we have evidence for auditory superiority in the order-judgment task. Second, the auditory superiority is dependent on serial position, appearing at both the beginning and the end of the sequence, but not in the middle. Third, unlike free recall, the serial-position effect is bowed (at least for the auditory IPI = 16-s condition). Finally, and most important, all of these effects are enhanced by increasing the IPI. Note further that increasing the IPI improves performance on the auditory lists (from .65 to .72), whereas performance on the visual lists remains relatively constant (.64 to .65). Thus, as the temporal coding assumption would lead us to suspect, memory for auditory material is more sensitive to temporal manipulations than is memory for visual material.

The most interesting finding across the two experiments is the form of the Modality × Serial Position interaction in the nonzero IPI conditions; apparently, there is a small auditory advantage at the beginning of the list, a larger one at the end, and a reversal in the middle. To check on the reliability of this pattern, we collected data from an additional 8 subjects in the 16-s IPI condition. These subjects were treated exactly as were those in the main experiment, including for each subject, new random assignments of word pairs to lists and positions.

The data are in Table 3. Again, the auditory advantage is larger at the end of the list than at the beginning, and there is a reversal in the middle position. Statistically, there was a main effect of position, $F(2, 14) = 5.70$, $MS_e = 1.00$, and a significant Modality × Position interaction, $F(2, 14) = 8.60$, $MS_e = 0.60$.

Experiment 3

Experiment 3 followed the same lines as Experiment 2 in that subjects recalled or made order judgments following

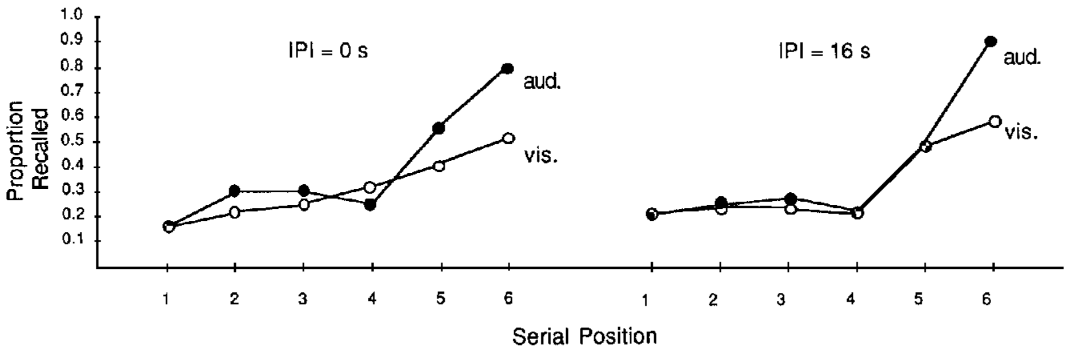

Figure 2. Experiment 2: Proportion recalled as a function of serial position and presentation modality. (IPI = interpair interval in seconds.)

visual and auditory lists presented with two different IPIs. We report this experiment for two reasons. First, in the free-recall data for Experiment 2, we did not find an increase in the modality effect with the longer IPI. In Experiment 3, we do observe this effect. Second, we included a number of parametric changes to investigate the robustness of the auditory superiority in the order-judgment task. These manipulations were that the lists were shortened from six to four serial positions, the order-judgment tests contrasted TBR items from nonadjacent serial positions, and although the TBR items were pairs, an order judgment consisted of a single word from each of the two nonadjacent serial positions. By testing all of the words we were able to double the number of observations. Finally, we included two between-subject independent variables. The first was modality of the order-judgment test. For a randomly chosen one half of the subjects, the test stimuli were presented visually (as in the previous experiments), and for the other half of the subjects, the stimuli were presented both visually and aurally. The other new independent variable was the instruction to the subjects regarding the choice of words. One half of the subjects (the first half) were instructed to indicate the word that was most recent (as in the previous experiments); the other subjects were instructed to indicate the word that was presented first.

Table 2
Proportion of Correct Order Judgments: Experiment 2

Modality	Positions tested: 1 and 2	3 and 4	5 and 6	*M*
	IPI = 0			
Auditory	.74	.65	.56	.65
Visual	.73	.63	.55	.64
Difference	.01	.02	.01	.01
	IPI = 16			
Auditory	.71	.58	.86	.72
Visual	.55	.70	.70	.65
Difference	.16	−.12	.16	.07

Note. IPI = interpair interval.

Method

Except for the changes noted next, the methodology was identical to that used in Experiment 2. Disregarding the practice lists, the subjects experienced a total of 12 four-pair lists (with pairs randomly assigned for each subject). Four of these lists (one in each of the four conditions defined by the two modalities crossed with the two IPIs) were recalled. Eight of the lists (two in each of the four conditions) were followed by the order-judgment test. The order judgments contrasted words presented in serial positions 1 and 3, or 2 and 4. During presentation of the pairs, an orienting task required a decision regarding the animacy of the stimulus words. If both words represented animate or inanimate concepts, then one button was pushed; if one word represented an animate concept and the other an inanimate concept, then a different button was pushed.

The 74 subjects were volunteers from introductory psychology classes at the University of Wisconsin—Madison.

Results

Initial analyses indicated that the two new between-subjects variables, modality of the order-judgment test and instruction on choice of word, had no effect on either recall or the order-judgment test. Thus, all reported analyses are collapsed over these variables.

The proportions recalled are illustrated in Figure 3. There is a strong modality effect that is confined to the end of the list. Furthermore, the size of the auditory advantage (in serial position 4) increased from .09 in the 0-s IPI condition to .25 in the 16-s IPI condition. These effects replicate previous work (Glenberg & Swanson, 1986).

Statistical analysis of the number of words recalled confirm conclusions suggested by the data in Figure 3. There were main effects of modality, $F(1, 72) = 25.06$, $MS_e = .052$, and position, $F(1, 3) = 98.14$, $MS_e = 0.98$. There were also significant two-way interactions involving modality and position, $F(3, 216) = 5.02$, $MS_e = 0.87$, and IPI and position, $F(3, 216) = 2.70$, $MS_e = 0.98$. Finally, the three-factor interaction was significant, $F(3, 216) = 3.23$, $MS_e = 0.87$, indicating an increase in the modality effect with an increase in the IPI. This conclusion is bolstered by the specific contrast, which indicates that the auditory advantage in serial position 4, compared with all earlier serial positions, is greater for the 16-s IPI than for the 0-s IPI, $F(1, 216) = 7.81$.

Table 3
Proportion of Correct Order Judgments From 8 Additional Subjects in the 16-Second IPI Condition

	Positions tested			
Modality	1 and 2	3 and 4	5 and 6	*M*
Auditory	.77	.54	.92	.74
Visual	.73	.79	.81	.78
Difference	.04	−.25	.11	−.04

Note. IPI = interpair interval.

The order-judgment data are in Table 4. The pattern of the means is close to what we have come to expect. Auditory presentation led to more accurate order judgments than did visual presentation, the effect is a little (but not significantly) larger at the end of the list than at the beginning, and the auditory advantage increased with increases in the IPI.

Statistically, the auditory advantage was significant, $F(1, 72) = 5.59$, $MS_e = 2.03$. Although the interaction of modality and IPI was not significant, the auditory advantage was significant for the 16-s IPI, $F(1, 36) = 5.43$, $MS_e = 1.99$, but not for the 0-s IPI.

Discussion of Experiments 1–3

Three important results emerge from the order-judgment task: an auditory superiority at the beginning and the end of the list and a reversal in the middle, a bowed serial-position curve even when free recall shows none, and the fact that increasing the IPI enhances both of the previous effects. Although these results are broadly consistent with the temporal coding assumption, a more specific explanation is called for. One such explanation comes from an extension of the Glenberg and Swanson (1986) temporal distinctiveness theory to the domain of order judgments. We begin by reviewing the temporal distinctiveness theory as applied to free recall. Comprehension of this review may be facilitated by reference to Figure 4.

A key assumption of the theory is that coding of time of presentation is more accurate for auditory events than for visual events (the temporal coding assumption). More specifically, it is assumed that for auditory events, the code for time of presentation is highly consistent with a narrow range of possible presentation times centered on the actual presentation time of the item. That is, given perfect access and decoding of this temporal code, the subject would be able to locate the time of presentation within a narrow temporal range. In contrast, for visual events, the code for the time of presentation has a low consistency with a broad range of possible presentation times.

These relations can be visualized as a consistency function, the relative consistency of a temporal code with all possible presentation times. For auditory events, the consistency function is peaked over a narrow temporal range, indicating high consistency of the code with a narrow range of possible presentation times (see Figure 4). For visual events, the consistency function extends over a broad temporal range, indicating low consistency with a wide range of possible presentation times.

Glenberg and Swanson (1986) assumed further that access and use of temporal information is controlled by temporal search sets. In the absence of more specific retrieval cues, subjects are presumed to search memory by trying to locate events that occurred at specific times, such as at the end of the list, the beginning of the list, and the middle of the list. On the basis of the work of Bellezza (1982) and results reported in Glenberg and Swanson (1986), it is assumed that the temporal search set can be constrained to a narrow interval when searching for recent events, but that a search of the more distant past requires a wider search set (one that includes a greater temporal domain). Two such search sets are illustrated in Figure 4.

The extent to which the search-set retrieval cue contacts or activates a to-be-remembered item depends on the item's *membership* in the search set. Membership in the search set is the degree to which the item's temporal code is consistent with the temporal range specified by the search set. More formally, the item's membership in the search set is the area under the item's consistency function within the range specified by the search set. Memberships are given numerically in Figure 4.

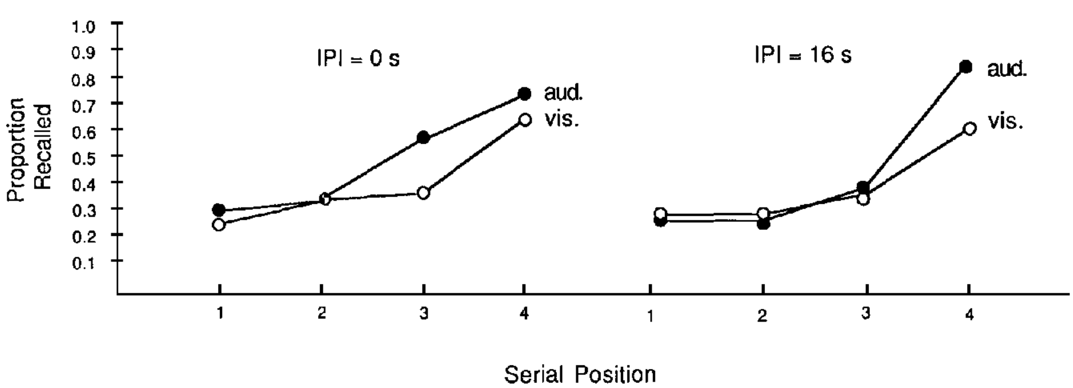

Figure 3. Experiment 3: Proportion recalled as a function of serial position and presentation modality. (IPI = interpair interval in seconds.)

Table 4
Proportion of Correct Order Judgments: Experiment 3

Modality	Positions tested		
	1 and 3	2 and 4	*M*
	IPI = 0		
Auditory	.63	.66	.65
Visual	.62	.62	.62
Difference	.01	.04	.03
	IPI = 16		
Auditory	.68	.71	.69
Visual	.63	.62	.62
Difference	.05	.09	.07

Note. IPI = interpair interval.

Finally, Glenberg and Swanson (1986) proposed a cue-overload (Watkins & Watkins, 1975) rule for how membership in a search set is translated into recall. Probability of recall of an item from a specific search set is given by the item's membership in the search set divided by the total of the memberships of all items in the search set (plus a factor that indicates noise in the search set; see Glenberg & Swanson). The rationale for this rule is that the subject has no means of specifying the appropriate to-be-remembered items except by their temporal codes. Thus, when the search set activates multiple items, they compete with one another for resources.

According to the theory, the recency effect in free recall reflects the use of a temporally constrained end-of-list search set (search sets are narrow when searching for recent information) that contacts few items. Thus, the search set is not overloaded and those items are recalled well. Because a temporally wider search set must be used to attempt recall of earlier items, the search set contacts many items (becomes overloaded) and none are recalled well.

Modality effects at the end of the list reflect the difference in the peakedness of the visual and auditory consistency functions. Because the visual consistency functions are flat and extend over a broad range of times, the end-of-list search set contacts many visual items that interfere with one another (overload the search-set cue). As illustrated in Figure 4, the approximate probability of recalling the last visual item from the end-of-list search set is given by .92 (membership of this item in the search set) divided by 1.56 (the sum of the memberships of all visual items in the search set), or 0.59.[1] Similarly, the approximate probability of recalling the last auditory item from the end-of-list search set is 2.0/2.0, or 1.00. That is, because there is but one auditory item contacted by end-of-list search set, it is temporally distinctive and recalled well.

In free recall, there is no auditory superiority at the beginning of the list (see Figures 1–3). According to the theory, this reflects the use of a temporally broad beginning-of-list search set. This broad search set contacts many auditory items and many visual items and so is overloaded for both modalities. Thus, although all auditory items have a more accurate temporal code than do visual items, at the beginning of the list this difference is masked by cue overload. Using Figure 4, the approximate recall of the first auditory item from the beginning-of-list search set is 2.00/3.19 = 0.63. The approximate recall of the first visual item from the beginning-of-list search set is 1.35/2.67 = 0.51. (These two values will be reduced, and the small difference between them all but eliminated, by the factors described in Footnote 1.)

Finally, the theory can explain the reduction of the free-recall modality effect with modest decreases in the IPI (as in Figure 3). Decreasing the IPI moves the auditory consistency functions closer together, thus increasing cue overload in the end-of-list search set and decreasing recall of the last item. Figure 4 illustrates the 16-s IPI condition. In the 0- and 4-s IPI conditions, the consistency function for the second-to-last auditory item is close enough to the end of the list to have a substantial membership in the end-of-list search set, and consequently it will reduce recall of the last auditory item. For visual items, the consistency functions are so broad that a modest change in the IPI causes little change in the overlap of the consistency functions (large changes in the IPI would eventually affect the visual recency effect; see Glenberg, Bradley, Kraus, & Renzaglia, 1983).

To apply this theory to the order-judgment task, the theory must be modified in two ways. First, note that the temporal information generated by the search-set mechanism is simply degree of membership in the search set, not a specification of the actual time at which an item was presented. Thus, although a subject may be able to compare the degree to which two items are members of a search set, that by itself does not specify the order in which the items were presented. To derive order information, the temporal location of the center of the search set must be known. For example, if the search set is anchored (centered) on the end of the list, then items with greater memberships in this search set must have been presented closer to the end of the list than items with a lesser membership in the search set. The converse applies to a search set anchored on the beginning of the list: Items with greater memberships in this search set must have been presented closer to the beginning of the list than items with a lesser membership in the search set. However, if the search set is centered elsewhere, degree of membership in the search set is not diagnostic of temporal order. On the basis of this analysis, we propose for the order-judgment task that subjects use only search sets that are anchored at the beginning and end of the list, as illustrated in Figure 4.

The second modification is of the rule that relates membership in the search set to performance. In free recall, it seems reasonable to allow items contacted by the same search set to compete with one another for resources because the

[1] For three reasons, this calculation only approximates the predicted probability of recall of items. First, it does not consider noise in the search set. Second, it does not take into account the probability of recalling items from search sets not illustrated in Figure 4. Finally, the calculation does not reflect the probabilities of constructing various search sets. These factors are described more fully in Glenberg and Swanson (1986).

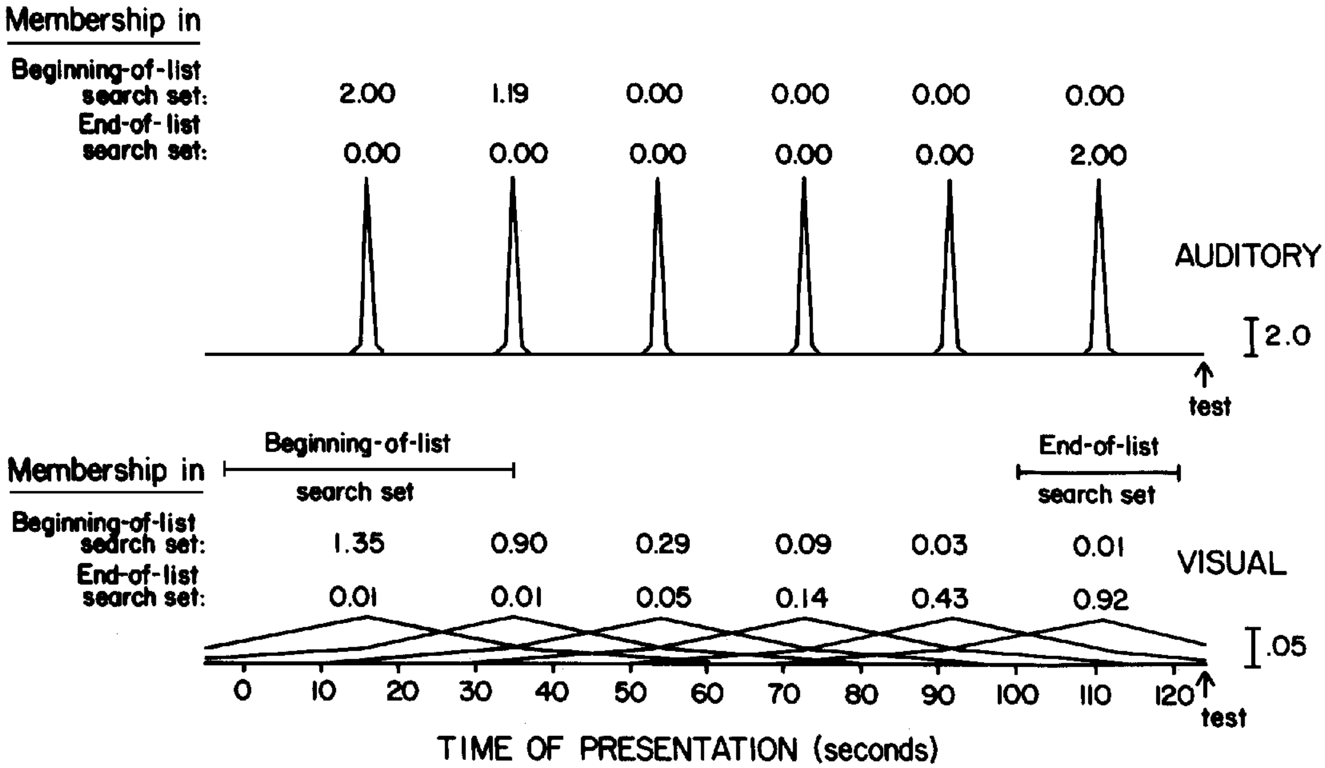

Figure 4. Application of temporal distinctiveness theory to the order-judgment task (16-s IPI [interpair interval] condition). (The six upper curves are consistency functions for the auditory items, and the six lower curves are consistency functions for the visual items. The two numerals above each curve are the memberships of the item in the beginning-of-list and end-of-list search sets. Note that the upper and lower curves are drawn to different scales; the total area under each curve is 2.0. The parameter values that generated this figure are available from the first author.)

subject has no other way of specifying or focusing the search. In the order-judgment task, however, other cues are provided. Namely, the subject is provided with the to-be-judged items. Under these conditions, we propose that subjects can focus processing on the to-be-judged items without suffering interference from other items activated by the search set. More specifically, we propose that the order judgment is a function of the difference in memberships between the two items in the anchored search set.

With these two modifications, temporal distinctiveness theory can account for the major findings from the order-judgment task. First, consider the auditory superiority at the end of the list. The end-of-list search set (the search set anchored at the end of the list) is temporally constrained and contacts few auditory items. Because the consistency functions for the auditory items are so peaked, the last auditory item will have a large membership in the search set (2.00 in Figure 4), and the penultimate auditory item will have a small membership in the search set (0.00), making for a large difference (2.00) and good performance. The end-of-list search set will contact many visual items because the consistency functions for the visual items are so flat (temporally extensive). Thus, the last and penultimate visual items will have similar memberships in the end-of-list search set (0.92 and 0.43, respectively) making for a small difference (0.49) and poor performance.

Now, consider order judgments for the beginning of the list. The beginning-of-list search set is broad (temporally extensive). The first auditory item will have a large membership in the search set (2.00). However, because the search set is so broad, the second auditory item will also have a large membership in the search set (1.19). Thus, the difference in memberships (0.81) is small (compared with the 2.00 difference at the end of the list) and order judgments will be less accurate than at the end of the list. The broad beginning-of-list search set will also contact both the first (1.85) and second (0.90) visual items, resulting in poor performance for the visual items (0.45). Thus, the modified theory predicts auditory superiority at both the beginning of the list (an auditory difference of 1.19 compared with a visual difference of 0.45) and the end of the list (2.00 compared with 0.49), but with a smaller effect at the beginning of the list—just what we found in the data.

Why is there a reversal in the middle of the list? Remember, auditory consistency functions are peaked, so they are consistent with only narrow temporal intervals. Thus, search sets anchored at either the beginning of the list or the end of the list will fail to contact the midlist auditory items (note the zero memberships of the central items in Figure 4). In this case, the subject is reduced to guessing. In fact, in the 16-s IPI condition, performance on the midlist auditory items is close to the chance level of 0.50 (see Tables 2 and 3). On the other hand, the visual consistency functions are flat and broad, so that they are consistent with a wide range of presentation times. Thus, midlist visual items can be contacted by search sets anchored at either the end of the list (difference of 0.09 in favor of the fourth item) or the beginning of the list (difference of 0.20 in favor of the third item), and performance on the visual items should be above chance. In fact, that seems to be the case for the data reported in Tables 1–3.

Finally, this modified theory explains why the modality and serial position effects increase with IPI. With short IPIs, the auditory consistency functions overlap, thus decreasing the difference in memberships in a search set and decreasing accuracy of order judgments. However, the short IPI condition does produce a benefit for the midlist auditory items: With a short IPI, midlist items are actually presented temporally close to the beginning and the end of the list, and search sets anchored at the beginning and the end of the list could contact these items. In other words, the probability that an anchored search set will contact a midlist auditory item should increase as the IPI decreases. Thus, in contrast to the first and last items, performance on the midlist auditory items should increase with decreases in the IPI. In fact, for the auditory midlist items, performance in the 16-s IPI condition is 0.56 (average from Tables 2 and 3), performance in the 4-s IPI condition is 0.57 (Table 1), and performance in the 0-s IPI condition is 0.65 (Table 2).

Recently available data from another task is also consistent with the proposed modifications of the temporal distinctiveness theory. Greene and Crowder (1988) examined modality effects in a position-judgment task. After viewing a list, subjects were given an item from the list and were asked to judge the item's input position. To apply the modified theory to this task, we need only assume that position judgments are a function of the degree of membership of the to-be-judged item in an anchored search set. That is, for search sets anchored at the beginning of the list, large membership indicates an early serial position. For search sets anchored at the end of the list, large membership indicates a late serial position. Because these end-anchored search sets will not extend far into the middle of the list, midlist position judgments should be poor and should not show a modality effect. Consistent with this analysis, Greene and Crowder found auditory superiority at both the beginning and the end of the list, but not in the middle.

Still, one of the Greene and Crowder (1988) findings needs explaining. They report (for Experiments 1 and 2) no beneficial effect of an IPI manipulation on position judgments. This contrasts with the IPI effect on order judgments for auditory items in the current Experiments 1 and 2 (see Tables 2 and 3). Because Greene and Crowder used a visual presentation in their Experiment 1, and because visual presentation is relatively unaffected by IPI manipulations, the absence of a positive IPI effect in their Experiment 1 is not surprising. However, in their Experiment 2, auditory presentation was used. The stimuli were letters from the set A, E, I, O, U, and Y that were read aloud by the subjects. The use of the same set of letters for their 120 lists may have had unanticipated consequences that could reduce or eliminate an IPI effect. According to temporal-distinctiveness theory, the judgment task requires construction of anchored search sets and the use of an item cue to focus processing on the to-be-judged items. The item cue in Greene and Crowder's experiments is overloaded by occurrence of the same items in successive lists. That is, the cue will contact traces of the item from previous lists as well as from the current list, and thus the cue may not be effective in focusing processing. In this case, the long IPI increases the retention interval for nonterminal items, resulting in a wide beginning-of-list search set, without the benefit of being able to focus on the relevant items within the search set. Under these circumstances, increasing the IPI may decrease judgment accuracy, as found by Greene and Crowder.

Modality and Frequency Judgments

In the frequency-judgment task, TBR items are presented with various frequencies, and the subject's task is to estimate those frequencies. A great deal of data is consistent with the proposal that each presentation produces a new representation (e.g., Hintzman, Nozawa, & Irmscher, 1982; Hintzman & Stern, 1978), rather than updating a single counter. Thus, the frequency judgment is based on the integration of information from all of the traces that can be contacted during the test.

Consider the following working hypothesis. Frequency judgments are based on the number of discriminably different traces (of the same item) contacted at the time of testing. Traces may be discriminably different in any of various ways. To be sure, however, traces will be discriminably different to the extent that they encode different contextual information, such as time of presentation. If temporal information is used in judging frequency, then the temporal coding assumption leads us to expect modality effects in this task. We will present the data from three experiments testing this expectancy.

Experiment 4

A common finding in the frequency-judgment literature is that frequency judgments are sensitive to the spacing of the presentations (Hintzman & Block, 1970; Rose, 1980), so that the greater the spacing the greater the judged frequency. One interpretation of this finding is that spaced presentations produce stronger or more retrievable traces. Another interpretation is that frequency judgments are based on contextual information (e.g., that specifies time of occurrence) that is affected by the spacing manipulation. Thus, spacing of presentations was manipulated in addition to modality and frequency.

Method

Design and materials. Both auditory and visual words were presented in a long list followed by a frequency-judgment test. The presentation frequencies of the tested words were 0 (tested, but not presented before), 1, 3, or 6. When a word was repeated on the list with a frequency of 3 or 6, the interval between the presentations was either massed (no other items intervening between successive presentations), short (1–3 intervening items), or long (5–15 intervening items). Thus, the three important independent variables were modality of presentation (auditory or visual), frequency of presentation (0, 1, 3, or 6), and spacing of presentations (massed, short, or long).

There were 4 exemplars of each of the 14 conditions that were represented on the list (2 Modalities × 3 Spacings × 2 Frequencies [3 or 6], plus once-presented auditory words and once-presented visual words), requiring a total of 56 different words and a list of 224 presentations. The conditions were randomly arranged within the constraint that one exemplar of each of the conditions appeared in each quarter of the list. Although only one basic list structure was constructed, the positions of auditory and visual words were counterbalanced over subjects. In addition, a 10-item buffer was appended to both the beginning and the end of the list, making a total of 244 presentations.

Presentation and timing of events was controlled by an Apple II computer. Each visual word was presented for 2 s on a video monitor, and each auditory word was presented in the first part of a 2,000-ms window using the Instavox. During the 2-s interval, the subject made an orienting response, pushing one button on a three-button panel if the word represented an active concept (defined as "capable of independent movement or change") and a different button if the word represented a passive concept. Subjects were unaware that the list presentation would be followed by a frequency-judgment test; they were led to believe that the performance on the orienting task was of primary interest.

A total of 72 words (56 critical words, 8 buffer words, and 8 zero-frequency words) was presented on the frequency-judgment test. The critical words were tested in the same order as their initial presentations, except that the zero-frequency words were interspersed throughout the test list. Each word was presented simultaneously both visually and aurally, along with a scale with the numbers 0–10 displayed on the video monitor. The subject used two buttons on the button panel to move a cursor under one of the numbers to indicate a frequency judgment. A third button was used to signal the computer to record the frequency judgment and move to the next test word. These judgments were self-paced.

All of the words were one- and two-syllable, four- and five-letter nouns. The assignment of words to conditions was randomized for each subject.

Subjects. A total of 30 students participated. These students were volunteers from introductory psychology classes at the University of Wisconsin–Madison.

Results

The results of most interest are portrayed in Figure 5. The first analysis examined the effects of frequency (1, 3, or 6) and modality. In this analysis, the spacing variable is ignored because it is undefined for items with a frequency of 1. There were main effects of modality, $F(1, 29) = 21.78$, $MS_e = 3.37$, and frequency, $F(2, 58) = 188.85$, $MS_e = 9.73$. Also, these two variables interacted, $F(2, 58) = 4.08$, $MS_e = 3.03$. These effects indicate that frequency estimates are greater for auditory events than visual events, and that the difference increases with true frequency. This interaction might have been produced by a floor effect. That is, if subjects could not remember the once-presented items, it is unlikely that there would be any modality effect. Although there was no significant difference between the modalities for the once-presented words, $F = 1.00$, there was no evidence of a floor effect. Judgments for both the auditory and the visual once-presented words were significantly greater than were judgments for the zero-frequency words (average = 0.29): For the auditory words, 30 of the 30 subjects had higher frequency estimates for the once-presented words than for the zero-frequency words, and for the visual words, 29 of the 30 subjects showed the effect. Thus, the interaction between modality and frequency is statistically reliable and not an artifact of floor effects.

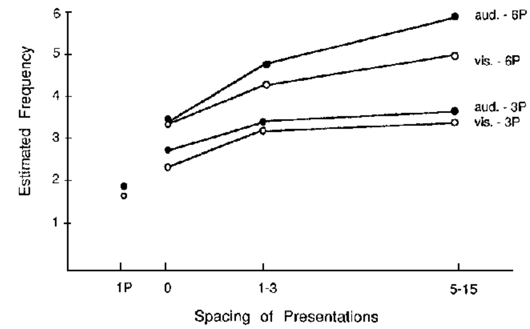

Figure 5. Experiment 4: Frequency judgments as a function of presentation spacing, modality, and frequency. (Aud. = auditory, vis. = visual, 1P = once presented, 3P = thrice presented, and 6P = six presentations.)

The second analysis included only the presentation frequencies 3 and 6, in order to examine the effect of spacing of presentations. There were main effects for modality, frequency, and spacing, $F(1, 29) = 15.06$, $MS_e = 11.99$; $F(1, 29) = 131.05$, $MS_e = 19.25$; and $F(2, 58) = 88.99$, $MS_e = 12.93$, respectively. There was a significant interaction between frequency and spacing, $F(2, 58) = 12.43$, $MS_e = 11.12$, indicating an increase in the spacing effect with increases in frequency. Overall, the interaction between modality and spacing was not significant, although it was significant in an analysis of the words presented six times, $F(2, 58) = 3.11$, $MS_e = 9.01$.

In summary, two new findings emerged. First, there was a clear effect of modality of presentation on frequency judgments: Frequency judgments were greater for auditory events than for visual events. Second, this modality effect interacted with true frequency of presentation in that the difference between the modalities increased with frequency of presentation. There was also a hint of a Modality × Spacing interaction, however it was only significant when the words had been presented six times. The finding of modality effects in the frequency-judgment task is in accord with our expectations based on the temporal coding assumption. Furthermore, the interaction of modality and frequency will help constrain our account of this effect, an account that will be introduced following the remaining experiments.

Experiment 5

Experiment 5 was conducted to replicate and extend the effects discovered in Experiment 4, and to provide another opportunity to observe a Modality × Spacing interaction.

Method

The design of Experiment 5 was very similar to that of Experiment 4, except that the presentation frequencies were 0, 1, 2, 4, and 6 (with the later three frequencies presented at spacings of massed, short [1–3], or long [5–15]). Thus, there was a total of 20 conditions represented on the presentation list. There were 4 exemplars of each of these conditions, requiring a total of 80 different words. Including the buffers, the presentation list included 316 positions. The test list consisted of the 80 critical words presented on the list and 8 zero-frequency words. As in the previous experiment, words were assigned to conditions randomly for each subject, and the positions of auditory and visual words were counterbalanced over subjects.

A second change from Experiment 4 was that each 2-s presentation was followed by a 0.75-s blank interval. The orienting response (indicating whether the word represented an active or passive concept) was to be completed in the initial 2-s interval.

A total of 31 subjects, from the same source as Experiment 4, participated in the experiment.

Results

The data (see Figure 6) were generally consistent with the results of Experiment 4. In an analysis including frequencies of 1, 2, 4, and 6 (but not the spacing variable), there was a main effect of frequency, $F(3, 30) = 218.64$, $MS_e = 6.86$. The main effect of modality was not quite significant, $F(1, 30) = 2.92$, $MS_e = 4.33$, $p = .10$, but the interaction between modality and frequency was significant, $F(3, 90) = 2.93$, $MS_e = 4.16$, indicating that the auditory advantage increased with frequency. As in Experiment 4, there was no difference between the modalities for the once-presented words, $F < 1$, even though performance was well above chance levels. For all of the subjects, the frequency estimates were higher for both visual and auditory once-presented words than for words with a frequency of zero (average = 0.19).

A number of other effects were significant in the analysis of the repeated items (with frequencies of 2, 4, or 6). There were main effects of frequency, $F(2, 60) = 154.69$, $MS_e = 17.92$, and spacing, $F(2, 60) = 136.49$, $MS_e = 17.79$. Their interaction was also significant, $F(4, 120) = 22.07$, $MS_e = 10.50$. However, the interaction between modality and spacing was not significant, $F < 1$.

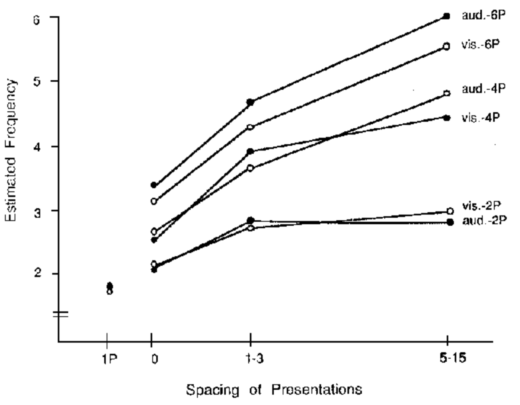

Figure 6. Experiment 5: Frequency judgments as a function of presentation spacing, modality, and frequency. (Aud. = auditory, vis. = visual, 1P = once presented, 2P = twice presented, 4P = four presentations, and 6P = six presentations.)

Experiment 6

Traditionally, modality effects are most robust after short retention intervals (Engle & Mobley, 1976; Engle & Roberts, 1982), and there is some data indicating subtle (but important) changes in the effect with long retention intervals (Balota & Duchek, 1986). In Experiments 4 and 5, because the frequency estimates were collected after all presentations, the retention interval between the last presentation of an item and its test was quite long. In Experiment 6, we manipulated the retention interval to determine if the auditory superiority in frequency judgments is sensitive to this variable.

Method

The basic methodology was similar to that used in Experiment 5, except for a few changes. Both auditory and visual words were presented with frequencies of 0 (presented only on the test), 1, 2, 4, and 6 presentations. The tests were scattered throughout the presentation sequence at three different retention intervals after the last presentation of a word. The retention interval was either short (1 intervening item), medium (5–7 intervening items) or long (10–19 intervening items). Thus, including the auditory and visual zero-frequency tests, there was a total of 26 conditions. Each of these conditions was represented by 4 exemplars. For the repeated words, the repetition lag was within the range of 3–5. As in the previous experiments, the words were randomly assigned to conditions for each subject, and the position of auditory and visual words was counterbalanced over subjects.

The 30 subjects were volunteers from the same source as in the previous experiments.

Results

The data of interest are presented in Figure 7. Except for the zero-frequency data, all of the conditions were analyzed in an analysis of variance that had three variables: modality of presentation (auditory or visual), frequency of presentation (1, 2, 4, or 6), and retention interval (short, medium, or long).

The auditory advantage was significant, $F(1, 29) = 64.84$, $MS_e = 7.36$. Also, modality interacted with frequency, $F(3, 87) = 13.8$, $MS_e = 5.71$. Note that the size of the auditory advantage increased systematically from a frequency of 1 to a frequency of 6. As in the previous experiment, estimated frequency of both auditory and visual once-presented items was greater than the estimated frequency of the zero-frequency items (average = 0.10) for all of the subjects.

The new information from Experiment 7 is that the Modality × Frequency interaction is found at all retention intervals. The three-way interaction had an $F < 1$. Thus, we can be fairly confident that the auditory advantage in frequency judgments is not dependent on testing after inordinately long or short retention intervals.

Three other effects were significant. There were significant main effects of frequency of presentation, $F(3, 87) = 444.41$, $MS_e = 21.21$, and of retention interval, $F(2, 58) = 9.32$, $MS_e = 5.01$. Note that frequency estimates tended to increase slightly with the retention interval. Also, retention interval interacted with frequency of presentation, $F(6, 174) = 3.27$, $MS_e = 5.55$, apparently because the increase in frequency

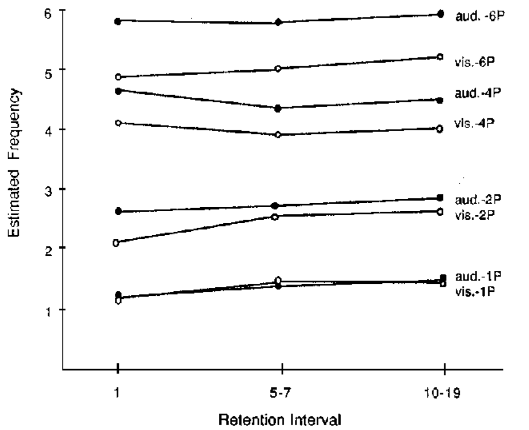

Figure 7. Experiment 6: Frequency judgments as a function of retention interval between the last presentation and the judgment, presentation modality, and presentation frequency. (Aud. = auditory, vis. = visual, 1P = once presented, 2P = twice presented, 4P = four presentations, and 6P = six presentations.)

estimates with retention interval was absent at a frequency of 4. Other than to note that Hintzman and Block (1970) did not find a significant effect of retention interval on frequency estimates, we have no comment on the latter two effects.

Discussion of Experiments 4–6

We began by supposing that frequency judgments reflect the accumulation of evidence from discriminably different traces of the same item, and that one way in which traces can be discriminably different is if their codes specify occurrences at different times. If coding of time of presentation plays a role in frequency judgments, then the temporal coding assumption leads us to suspect a modality effect. Just such an effect was found in Experiments 4 and 6, and it was marginally significant in Experiment 5. In all three experiments, the size of the auditory advantage increased with true frequency.

It would be premature to propose a specific process model for the frequency-judgment task, because the temporal distinctiveness theory has not been developed to accommodate effects of repetition such as encoding variability (Glenberg & Smith, 1981) and study-phase retrieval (Cuddy & Jacoby, 1982). Nonetheless, the outlines of such a model can be suggested. Suppose that the copy cue on the frequency-judgment test is used as a memory probe, and that it generates a feeling of familiarity (e.g., Clark & Shiffrin, 1987). An item with a familiarity value just above a low threshold is judged as having occurred once. Because temporal information plays little or no role in the processes specified so far, we would expect little or no modality effect for items presented with a low frequency.

Now, however, consider an item whose familiarity is well above the threshold. From the subject's point of view, there is clear evidence that the item was presented, but the task is to generate a frequency estimate in addition to a recognition response. If presentation frequency were the only variable that contributes to familiarity, then the frequency estimate would be simply a function of familiarity. However, subjects are probably aware that factors such as recency of presentation also contribute to subjective familiarity, and thus a more analytic process is required for accurate judgments. We propose that it is this analytic process that makes use of temporal information gleaned from temporal search sets.

An item with high familiarity induces the subject to create multiple temporal search sets to determine the number of previous presentations of the item. The frequency judgment is based on the number of search sets in which a trace of the to-be-judged item has a membership that exceeds a threshold. This threshold is needed because consistency functions (the consistency of an item's coded time of presentation with possible presentation times) are continuous; that is, each trace has a nonzero membership in all search sets (see Glenberg & Swanson, 1986). Thus, to count an item as having occurred in a given temporal interval (specified by the search set), the membership must exceed a minimum threshold. Once the threshold mechanism is in place, it produces the modality effect. Remember, the temporal code for a visual event has low consistency with various presentation times. Thus, the membership of visual items in temporal search sets will occasionally be too low to exceed the threshold, and some traces of the visual event will not be counted.

In one respect, this explanation of the frequency-judgment data is similar to our proposal regarding the order-judgment data (Experiments 1–3). Because search is guided by the to-be-judged item, as well as the temporal search set, there is no need to invoke the cue-overload rule. That is, other items with memberships in the search set do not interfere with judgments about the cued item. The explanation for the frequency judgments differs from that for the order judgments in that for frequency judgments the search sets are not anchored. Remember, for the order-judgment task, search sets needed to be anchored to derive order information. For the frequency-judgment task, order information is not relevant, so search sets need not be anchored.

At least one puzzle remains, however. The data from Experiments 4–6 indicate that there is little or no modality effect on frequency judgments for the once-presented items. Our explanation for this finding is that a frequency judgment of one is based on a familiarity mechanism that is insensitive to temporal information—in effect, this type of frequency judgment is nothing but a recognition judgment. However, Conway and Gathercole (1987) reported three recognition memory experiments in which once-presented auditory items were much better recognized than once-presented visual items. Why there should be strong modality effects in the recognition memory task, but none for frequency judgments of once-presented items, is a puzzle that remains to be solved.

Conclusions

These experiments establish two new modality-linked phenomena. The first is that modality of presentation affects order judgments. The second is that modality of presentation affects frequency estimates.

These new phenomena are different from modality effects in recall in two ways. First, these new phenomena occur after both short retention intervals and long retention intervals and at the beginning and the end of lists. The modality effect in recall is generally confined to the end of the list. Second, in the frequency-estimation task, the auditory advantage is found only after multiple presentations. In recall (and in order judgments), a single presentation is sufficient.

These differences are sufficient to rule out simple, single-factor interpretations of the modality effect. For example, explanations based on temporary storage advantages cannot account for the data. Similarly, explanations that propose that auditory items are, in some sense, simply stronger than visual items, are foiled. Instead, because the modality effect is critically dependent on the type of test, we must consider the contribution of retrieval. That, of course, is just what we have tried to do by introducing the temporal search set idea.

Whether or not the temporal distinctiveness theory provides the correct explanation for these various modality effects, it is clear that the results provide strong support for the temporal coding assumption: Time of presentation is coded more accurately for auditory events than for visual events. This focus on time of presentation reflects the basic nature of representations in episodic memory. As important as what occurred, is when it occurred.

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