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The list-strength effect and categorical frequency memory: Tests of availability

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The List-Strength Effect and Categorical Frequency Memory: Tests of Availability

by

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Abstract

The availability view of memory maintains that the retrieval of categorical frequency information is a function of recall of category exemplars. The List-Strength Effect (LSE), which is evidenced when increasing the strength of competing items in a list reduces memory for the other items, has been found to be a characteristic of recall, but not recognition, performance. The present study was designed to a) further examine the relationship between cued recall and frequency judgments of category exemplars by testing for the presence of a LSE in categorical frequency estimation; and b) to examine the role that estimation strategies may play in judgments of frequency. The results of Experiment 1 found that 1) there was modest evidence of a LSE in categorical frequency judgment, and that 2) there was an identical pattern of effects in the statistical analyses of cued-recall and frequency estimates. These results provide some evidence as to the use of recall as an estimation strategy. Experiment 2 again showed a modest LSE in category frequency judgments. However, frequency estimation did not differ significantly when the same category exemplars were repeated in the study list (same context) versus when different category exemplars were presented (different context). Experiment 2a, in which cued recall was examined for the different-context condition, did not show a LSE. Furthermore, performance in Experiment 2a was lower than category frequency estimation performance in Experiment 2. It was concluded that recall of category exemplars plays a role in category frequency estimation, but is not the principle underlying process.

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Introduction

Across a considerable range of conditions, people have been shown to have a good memory for the frequency with which an event occurs. This frequency sensitivity has been demonstrated in laboratory settings as well as ecologically valid situations which take place on a day-to-day basis. For example, people have been shown to be sensitive to the natural occurrence rates of single letters (Attneave, 1953) as well as causes of death (Lichtenstein, Slovic, Fischhoff, Layman, & Combs, 1978). In the laboratory setting, people have shown a remarkable sensitivity to how often a word is presented even when they did not know the nature of the up-coming memory test (e.g. Hintzman & Block, 1971; Hasher & Zacks, 1979, 1984; Hasher & Chromiak, 1977; Jonides & Naveh-Benjamin, 1987).

The accuracy with which people judge **classes** of episodic events has also been shown to be significant. Specifically, memory for category frequency in which higher-order, superordinate level information that has been explicitly or implicitly referenced, has been demonstrated as reliable (e.g., Alba, Chromiak, Hasher & Attig, 1980; Barsalou & Ross, 1986; Freund & Hasher, 1989; Greene, 1989; Watkins & LeCompte, 1991; Bruce, Hockley & Craik, 1991). The typical paradigm in studies involving categorical frequency estimation involves presenting subjects with a list of exemplars from different taxonomic categories such that the number of exemplars per category is varied. At final test, subjects are presented with the category names and asked to estimate how many times they saw an exemplar from that category.

Two hypotheses have been developed to account for the basis of category frequency estimates. These hypotheses are the category-counter hypothesis and the recall-estimate hypothesis.

Category-Counter Hypothesis

According to the category-counter hypothesis, frequency estimates are based on information stored at the superordinate level (Alba et al., 1980; Brooks, 1985). In effect, this hypothesis postulates that when an exemplar is seen by a subject, a depiction of its categorical superordinate is triggered in memory. A count representing the frequency of activation is kept in some manner at the superordinate level. At final test, subjects base their category frequency judgments on the number that the category-counter has automatically and continuously tabulated. Thus, frequency judgments are based on information stored at the superordinate level and not on the retrieval of the memory traces of each individual exemplar.

In this view, frequency counting is proposed to be automatic (as defined by Hasher & Zacks, 1979;1984) and as such must be evidenced in the following conditions: 1) subjects must be equally as accurate given implicit or explicit memory instructions; 2) practice, or depth of processing should not increase accuracy; and 3) tasks performed concurrently should not adversely affect frequency judgments (as long as the category exemplars receive some minimal degree of encoding).

Recall-Estimate Hypothesis

The recall-estimate hypothesis of category frequency memory is parallel to the availability hypothesis of probabilistic reasoning (Tversky & Kahneman, 1973), the multiple-trace theory of item repetition effects (Hintzman, 1976), and the exemplar theory of category representation (Hintzman, 1986). This hypothesis is in direct opposition to the category-counter hypothesis. It is proposed that each presentation of an exemplar forms a distinct memory trace and category names are used as cues in retrieving exemplar traces. At final test, the number of retrieved exemplar traces are counted and frequency judgments are based on this count. Information is not stored or retrieved at the superordinate category level.

The exemplar-retrieval hypothesis postulates that judgments of category frequency are analogous to that of cued recall. In cued recall, category names are used as retrieval cues for remembering the exemplars. Similarly, in categorical frequency estimation, the retrieved exemplars are in effect, counted. And as such, this hypothesis would predict a monotonic relationship between cued recall and categorical frequency estimation.

Recall-Estimate Evidence

Tversky and Kahneman (1973) suggested that people base frequency estimates on "availability", the ease with which instances of that particular category come to mind. They presented subjects with lists containing names. At test, subjects were asked to judge whether there were more male or female names. The results showed that the gender for which there were more famous names were not only judged to be more frequent but also, reflected better recall of those names. Since recall was shown as being akin to frequency estimation, it provided salient evidence for availability.

Lewandowsky and Smith (1983) provided further evidence in favour of the availability hypothesis. They demonstrated that repeating non-famous names increased recallability, as well as achieving a parallel increase in frequency estimates (Experiments 1 and 3). Similarly, making non-famous names more prominent by increasing presentation time, also resulted in parallel effects of the two measures (Experiment 3). Overall, Lewandowsky and Smith's results show that both recall and frequency judgment vary in the same manner to changes in the independent variable, as an availability heuristic would suggest.

In addition to casting doubt on the automaticity view of frequency memory, Williams and Durso (1986) also revealed further evidence in favour of an availability hypothesis. First they showed that correlations between frequency estimation and recall were affected by the same manipulations in a parallel manner. Specifically, judgments of category frequency were affected by instructions (explicit vs. implicit) such that subjects were more sensitive to differences in category frequency when they had been given explicit instructions to attend to category frequency. The same was true in the recall condition, where subjects recalled more items after frequency instructions than after read-only instructions. And second, by showing that manipulations in recall (e.g. varying time allowed to encode items at study and response time at final test) affected subjects' frequency estimates. In effect, varying the amount of time subjects viewed each word led to differences in the sensitivity to categorical frequency. And this pattern was mirrored in the correlations with recall. Williams and Durso, in essence, found a clear pattern such that manipulations that benefitted recall also improved frequency estimation.

Greene (1989) specifically investigated the relationship between categorical frequency estimation and cued recall. Item generation, exemplar spacing and extralist cues have all been shown to influence cued recall and as such, Greene examined whether or not categorical frequency estimation would similarly be influenced, as an availability hypothesis would suggest. The results showed that effects of generation (Experiment 1), exemplar spacing (Experiment 2), and extralist cuing (Experiment 3) affected the accuracy and magnitude of categorical frequency judgments, as well as the number of exemplars recalled. First, frequency judgments were affected by variables influencing organizational and rehearsal processes, which challenge the theory of automatic encoding of frequency estimation. Second, Experiment 3 demonstrated that extralist cues presented at test lowered the magnitude of frequency estimates as well as the number of exemplars recalled. This effect could not be due to interfering with access to the superordinate level as category names were also supplied at time of test. Because the category-counter account maintains categorical frequency estimation is solely based on information stored at the superordinate level, Greene provided salient evidence against this theory.

In effect, this literature which has demonstrated both recall and frequency estimates to vary in the same manner according to changes in the independent variable, supports a recall-estimate view. This parallel movement between recall and judgments of frequency has been achieved with strength manipulations, either by item repetition or increased presentation rate (e.g., Tversky & Kahneman, 1973;Lewandowsky & Smith, 1986; Williams & Durso, 1986). This was also true for Greene (1989) when he investigated the effects of item generation, extralist cuing and exemplar spacing. While this pattern of results does suggest that recall is an integral part of the ability to judge frequency, there is also compelling evidence which argues against this notion. Whereas in the recall-estimate view recall is considered necessary in estimating frequency, according to the category-counter hypothesis the role that recall plays is much exaggerated. Support for the category-counter view is based on several dissociations found between recall and frequency judgments.

Category-Counter Evidence

Alba et al. (1980) tested the notion that categorical frequency estimation is based solely upon higher-order, superordinate level information that is automatically activated. Subjects were presented with zero, three, six and nine exemplars from each of several high frequency categories at a 3-second rate. Subjects were sensitive to superordinate frequency regardless of whether or not: 1) subjects were expecting a frequency or recall test; 2) exemplars were randomized or blocked by category at presentation; 3) superordinate names were presented at final test at a 2-s or 10-s rate--demonstrating that subjects were able to make accurate frequency discrimination of category labels when presumably there was not sufficient time for a retrieve-and-count strategy to work. Also, it was found that categorical frequency judgments and cued recall did not behave in the same manner: As the number of exemplars from a given category increased, accuracy in recalling that information decreased, while frequency judgments increased proportionally. Thus, Alba et al. provided some evidence that exemplars implicitly activate their respective superordinate category, regardless of strategy or intentionality. These results are in direct opposition with those of Williams and Durso (1986) who found a parallel pattern of effects between cued recall and categorical frequency estimates with manipulations in intentionality, encoding time and time at retrieval.

Freund and Hasher (1989) and Barsalou and Ross (1986) also demonstrated a dissociation between cued recall and frequency estimation. Specifically, Freund and Hasher found that instructions about category membership affected frequency judgments but not recall. That is, in one condition, subjects were informed of a recall test at outset, and subsequently given a card containing all 13 category names. These subjects were further instructed by being told that rehearsing the items by category had been found to be helpful. In the not-informed condition, subjects were given no such card containing category labels following their recall test instructions. The results showed that judgments of category frequency increased more sharply in the informed than in the not-informed condition, whereas recall increased equally with frequency in both conditions.

Barsalou and Ross (1986) found a similar dissociation between recall and frequency estimates, this time with memory for property frequency. That is, when people encode words sharing a particular property such as **red** (e.g., tomato, blood, stop sign) or **hot** (e.g., sauna, fire) will they later show sensitivity to the relative number of hot and red items? The results showed that whereas cued recall increased as a function of property category frequency, frequency judgments did not.

These dissociations suggest that frequency judgments are not based on retrieval of exemplars as an availability, or recall-estimate hypothesis would predict.

More recently, Watkins and LeCompte (1991) also concluded that the role of recall in categorical frequency estimation was much exaggerated. In Experiments 1 and 2, between 6 and 24 exemplars were presented to subjects along with their category names. At test, subjects either recalled the category exemplars or gave frequency estimates. The results for both Experiments 1 and 2 showed that frequency judgements greatly exceeded the number of exemplars recalled and, reflected to a greater extent, actual category size.

Furthermore, when Watkins and LeCompte made efforts to induce a recall-estimate strategy (Experiments 3-5), the same pattern of results emerged. They did this by requiring overt recall prior to frequency estimation by using small categories (from 1-4 exemplars), and by removing the category names at study (in order to reduce the extent to which exemplars would be thought of in terms of their categories). The results of these experiments also indicated that frequency estimates more closely approximated actual category size than that of recall. These results, thus, provide strong evidence against the view that category frequency estimates are based solely on the retrieval of instances.

Bruce et al. (1991) also provided evidence as to the limitations of the availability hypothesis using cued recall by category names as a measure. To the extent that frequency judgments are a function of recall, Bruce et al. looked for: 1) positive correlations between the two measures and 2) changes in independent variables resulting in parallel movement of the two measures. The results showed that correlations were in fact moderately strong (when actual frequency was held constant) under certain conditions. In effect, strong positive correlations were found between recall and frequency estimation when: a) list instances were not categorized aloud at study; b) when cued recall preceded frequency judgments; and c) when there was a 1-week delay in the frequency and cued recall task. Under other conditions however, Bruce et al. found serious weaknesses with availability, such as when subjects categorized list members aloud during study and were given feedback, or when the cued recall task did not precede the frequency judgment task. Under those conditions, the correlation between the two measures was at best, marginal.

In terms of comparable changes between the two measures in response to experimental manipulations, Bruce et al. also revealed problems with availability. For, in most cases, this parallel movement between frequency and recall did not occur. First, variation in recall test position impaired frequency estimation, but either did not affect, or had an opposite effect on recall. In addition, the variable age also differentially affected the two measures. While frequency judgments between young and elderly subjects did not reliably differ, young subjects recalled significantly more words than elderly subjects. Overall, the results across Bruce et al.'s experiments suggest a limited role for availability when list items are subjected to powerful categorical encoding at study.

Thus, evidence for the category-counter hypothesis is in direct opposition to the evidence supporting the recall-estimate hypothesis which essentially found recall and judgments of frequency behaving similarly to various manipulations. In contrast, the literature supporting the category-counter hypothesis shows recall and frequency judgments behaving differently under different manipulations. That is, while some manipulations were shown to affect recall of study list items, they did not similarly affect estimates of frequency of those items. For example, a presentation rate of 2-seconds or 10-seconds at final test did not affect frequency judgments of those items, but did affect recall. This and other dissociations (e.g., Freund & Hasher, 1989; Watkins & LeCompte, 1991) reveal evidence of a limited role of recall in estimating frequency.

The category-counter hypothesis would suggest that recall is independent of category frequency judgments; the recall estimate theory argues that it is an integral part of frequency estimation. Empirical support for each hypothesis is mixed, as findings both consistent and problematic for each theory have been obtained.

The present study was designed to further examine the relationship between cued recall and frequency judgments of category exemplars. To this end, the list-strength effect was evaluated for both cued recall and category frequency judgments.

The List-Strength Effect

Tulving and Hastie (1972, Experiment 1) serendipitously discovered that repeating some items in a study list reduced free recall of the remaining items when the total number of different items was held constant. Ratcliff, Shiffrin, and Clark (1990) explored this effect, comparing recognition, free, and cued recall. Subjects were exposed to three lists: A pure-weak list in which exemplars were presented at a 1-second rate; a pure-strong list in which exemplars were presented at a 2-second rate; and finally a mixed list in which half of the items were presented at a 1-second rate and the other half presented at a 2-second rate. According to the list-strength hypothesis, memory for weak items in the pure-weak list should be better than weak items in a mixed list due to the other items in the mixed list being, on average, stronger. Alternatively, memory for strong items in the pure-strong list should be worse than for strong items in the mixed list because the other items in the mixed list are on average, weaker.

The list-strength effect was summarized by Ratcliff et al. by calculating a ratio of ratios, where the mixed strong-to-weak ratio should be larger than the pure strong-to-weak ratio. Table 1 shows what the list-strength effect looks like in terms of a ratio of ratios. In effect, there should be a greater difference between the strong and weak items in a mixed list, since strong items should be better remembered in a mixed list, whereas weak items should be more difficult to remember in a mixed list. Thus, while the strong-to-weak ratios should be greater than 1 in both groups, the overall mixed/pure ratio of ratios should also be greater than 1 due to the greater difference between strong and weak items in a mixed list.

Insert Table 1 about here

In seven experiments, Ratcliff et al. found evidence of a reliable list-strength effect in free recall, at the very most a small effect in cued recall, and an absent or negative effect in recognition. In three comparisons of the list-strength effect for free recall, the ratio of ratios varied from 1.35 to 3.22 with a mean of 1.52. All comparisons produced a significant list-strength effect. In four comparisons for cued recall, the ratio of ratios varied from 1.07 to 1.51 with a mean of 1.27. The list-strength effect was significant in only one of the four comparisons. Finally, in fourteen comparisons for recognition, Ratcliff et al. found that the ratio of ratios varied from .77 to 1.10 with a mean of .92. Ten of the 14 ratios were less than one. Ratcliff et al. were unable to identify the conditions that produced a negative list-strength effect. These results held whether strengthening was achieved by extra repetitions or study time. No one has yet determined the exact mechanism by which this phenomenon occurs.

Yonelinas, Hockley and Murdock (1992) further examined the absence of a list-strength effect in item recognition using rapid presentation rates in order to control for rehearsal borrowing between strong and weak items in mixed lists. Consistent with Ratcliff et al., under a variety of manipulations, there was no evidence to suggest a list-strength effect in recognition memory.

Experiment 1

In order to investigate the relative role that recall plays in categorical frequency judgment, Experiment 1 was designed to test for the presence of a list-strength effect in two conditions, cued-recall and frequency judgment. The role of recall in categorical frequency memory is questionable. In the list-strength effect however, the most robust effects are achieved with recall. So if the list-strength effect is found in categorical frequency judgements, it would provide strong evidence in favour of availability.

The list-strength effect has not been examined for category recall. As described above, Ratcliff et al. found a reliable list-strength effect for free recall, but only a modest, at best, effect for cued recall. However, in their cued-recall procedure, which was typical of cued-recall studies in general, and of the list-strength effect in particular, there was one to-be-remembered item for each recall cue. In the category cued-recall task in Experiment 1 of the present study, with the exception of frequency 1, there was more than one to-be-recalled item for each category recall cue. Therefore, category recall is more similar to a free-recall task than to the cued-recall task examined by Ratcliff et al. For this reason, it was predicted that a significant list-strength effect would be found for category recall. Furthermore, it is also possible that the size of the list-strength effect could increase as the number of category exemplars to be recalled increases.

In terms of frequency judgments, two opposing predictions were made, one for each theory. That is, the recall-estimate view would predict a list-strength effect in judgments of categorical frequency, thus supporting the role of recall in these estimates. Alternatively, the category-counter view predicts the absence of such an effect in categorical frequency judgments which would question the role of recall in frequency memory.

Method

Design. The design was a 2x4x2x2 factorial with test (cued recall vs. categorical frequency) as a between-subjects factor and category frequency (1,3,5,7), presentation duration (1 or 3 sec.) and list type (mixed vs. pure) as within-subjects factors. Note that test type was not a factor in the analysis of variance. The two groups were analyzed separately because the two test types had different dependent variables.

Participants. Fifty introductory Psychology students participated voluntarily for bonus credit from the participant pool at Wilfrid Laurier University. Twenty-five subjects were randomly assigned to the cued recall condition, and twenty-five to the frequency estimation condition.

Apparatus and Materials. The stimulus items were chosen from the Battig and Montague (1969) and Hunt and Hodge (1971) sets of norms. The word pool consisted of 512 words, 8 words from each of 64 categories (see Appendix A). Study lists and test lists were generated and presented on IBM compatible laboratory computers. The exemplars and category names were presented on a monochrome monitor and appeared in upper case in the centre of the screen.

<u>Procedure</u>. Each participant was individually tested and randomly assigned to either the frequency judgment or cued-recall group. Assignment of category to frequency was random in each session with the restriction that a given category was only presented in one study list in a session. In any one list, four of the categories contained one instance, four of the categories contained 3 instances, four of the categories contained 5 instances and four of the categories contained 7 instances. Thus, the study lists consisted of a total of 64 exemplars from 16 different categories. The categorical lists were presented in a blocked format; that is, the exemplars from the same category were presented sequentially in the study list.

Presentation rate (1-sec. vs. 3-sec.) was manipulated between lists in the pure list conditions and between categories in the mixed list condition. Each subject received a total of four study lists: 1 pure-fast list, 1 pure-slow list, and 2 mixed lists. In a mixed list, two of the four categories in a frequency set were presented at a 3-second rate and the other two at a 1-second rate. In the pure-fast list (pure-weak list), each exemplar was presented at a 1-second rate. In the pure-slow list (pure-strong list), each exemplar was presented at a 3-second rate. There was a .5 second blank interval between presentations of each exemplar in the study list.

The order of categories within a study list, and the order of the four lists within a given session was randomly determined. There was a different random sequence for each subject in a test group. Each subject in the frequency judgment group was yoked with a different subject in the cued-recall group and both members of a yoked pair saw the identical study lists.

Each of the four study lists was immediately followed by a test list. The test lists consisted of the 16 superordinate category names presented in random order. The order of the test cues was identical for the frequency judgment and cued-recall members of a yoked pair.

Subjects in each condition were given appropriate test instructions specifying the nature of the up-coming tests. The frequency estimation group was instructed to estimate how many times each category was represented by an exemplar; the cued-recall group was asked to write down as many exemplars from the particular category as they could remember. Subjects in both groups were shown at outset during instruction examples of what to write down during a final test. The presentation of the category labels at test was subject-paced; by pushing any key on the computer keyboard, participants were able to move onto the next test cue. Subjects initiated the presentation of each study list when they were ready to do so. A session took approximately 30 minutes to complete. There was no time limit given in either group.

Results

Recall. The mean number of exemplars recalled for each list and presentation rate condition as a function of presentation frequency are presented in Figure 1. Recall increased with the number of exemplars for both the fast and slow presentation rates in both mixed and pure list conditions.

Insert Figure 1 about here

A 4 (frequency) X 2 (presentation rate) X 2 (pure vs. mixed lists) ANOVA was performed on subject mean recall scores. All results are reported significant at the .05 level unless stated otherwise. The analysis showed strong items to be better recalled than weak items, $F(1,24)=17.02$, $MSe=0.48$. As expected, there was also a main effect of frequency, $F(3,72)=391.20$, $MSe=0.59$. There was no significant difference in overall mean recall between mixed and pure lists, $F(1,24)=.63$, $MSe=0.37$.

In terms of two-way interactions, the only one of statistical significance was that of presentation rate by frequency, $F(3,72)=6.24$, $MSe=0.31$. The presentation rate by list interaction did not approach significance, $F(1,24)=1.10$, $MSe=0.48$, nor did the frequency by list interaction, $F(3,72)=.99$, $MSe=0.31$.

Finally, there was a significant presentation rate x frequency x list interaction, $F(3,72)=4.13$, $MSe=0.26$. This interaction reflects the fact that a list strength effect was found at some, but not all presentation frequencies.

The strong/weak ratios and the ratio of ratios for each frequency are shown in Table 2. It can be seen in Table 2 that the ratio of ratios is greater than 1 for frequencies 3 and 5, indicating a list strength effect for these frequencies. However, the ratio of ratios is less than 1 for frequencies 1 and 7 indicating an absent or negative list strength effect. Averaging over frequency, the mean strong to weak ratios were 1.20 and 1.16 for mixed and pure lists, respectively. The overall mixed/pure ratio of ratios was 1.03. Thus, when cued recall is averaged over frequency, no list strength effect was found.

Insert Table 2 about here

Frequency Estimates. Figure 2 shows mean frequency estimates for each presentation rate and list type as a function of presentation frequency. The mean frequency estimates were analyzed in the same manner as the cued-recall scores. The results for the frequency estimation group paralleled those found in the recall condition. As Figure 2 illustrates, frequency estimates increased with presentation frequency and strong items in the mixed list appear to be better remembered than weak items in a mixed list.

Insert Figure 2 about here

Mean frequency estimates for strong items were greater than for weak items, $\underline{F}(1,24)=10.77$, $\underline{MSe}=0.59$. There was also a significant main effect of frequency, $\underline{F}(3,72)=191.62$, $\underline{MSe}=1.54$, and no effect of type of list, $\underline{F}(1,24)=.16$, $\underline{MSe}=0.75$. As in the recall group, the only significant two-way interaction was that between presentation rate and frequency, $\underline{F}(3,72)=2.81$, $\underline{MSe}=0.64$. The presentation rate by list interaction did not approach significance, $\underline{F}(1,24)=.48$, $\underline{MSe}=1.10$, nor did the list by frequency interaction, $\underline{F}(3,72)=.87$, $\underline{MSe}=0.50$.

Also, as for cued-recall, the presentation rate x frequency x list interaction was significant, $\underline{F}(3,72)=3.04$, $\underline{MSe}=0.48$. This interaction indicates a list-strength effect that is modified by frequency. The strong/weak ratios and the ratio of ratios are shown for each frequency in Table 3. The mixed/pure ratio of ratios were greater than 1 at frequencies 1 and 3, but less than 1 at frequencies 5 and 7. Averaging over frequency, the mean strong to weak ratios were 1.12 and 1.01 for mixed and pure lists, respectively, and the overall mixed/pure ratio of ratios was 1.11.

Insert Table 3 about here

Discussion

The List-Strength Effect

The results provide only modest support for a list-strength effect in category cued-recall, as the size of the effect varied with frequency. Also, contrary to what one might predict, the effect did not increase systematically with frequency. It is not surprising that there was no effect at frequency 1 as Ratcliff et al. (1990) did not find a reliable list strength effect for cued recall with one to-be-remembered item. However, the fact that no systematic increase in the ratio of ratios was found is inconsistent with the prediction that the list-strength effect should increase with frequency as the demands on recall increase with frequency (or, as the number of items to be recalled increase with frequency).

The results of Experiment 1 also provide limited support for a list-strength effect in categorical frequency estimation. The significance of this finding lies in the understanding of how categorical frequency judgments are actually produced. While the literature on the availability view of memory and the category-counter hypothesis have been essentially in direct opposition in terms of the role that recall plays in categorical frequency estimation (e.g. Lewandowsky & Smith, 1983; Williams & Durso, 1986; Greene, 1989; vs. Alba et al., 1980; Freund & Hasher, 1989; Barsalou & Ross, 1986; Watkins & LeCompte, 1991; Bruce et al., 1991), Experiment 1 provides evidence in favour of the recall-estimate view. That is, the finding of a modest list-strength effect in both recall and frequency estimation suggests that frequency estimation may be based, at least in part, on an availability view (Tversky & Kahneman, 1973).

This conclusion is based on previous literature in both the categorical frequency judgment and list-strength effect domains. In the frequency judgment sphere, the role of recall has been basically divided in the literature. However, studies on the list-strength phenomenon which occurs when the strengthening of some (but not all) items on a list reduces memory for the remaining items, have concurred on the fact that it is only a function of recall (Ratcliff et al., 1990; Yonelinas et al., 1992). Thus, the present finding of a limited list-strength effect in categorical frequency judgment provides some evidence in favour of the availability view of memory.

The findings of Experiment 1, however, cannot be over-emphasized as there was not a significant overall list-strength effect. Ratcliff et al. (1990) suggested that one reason that a list-strength effect could be attenuated is due to a redistribution of coding, rehearsal, or effort between items in a mixed list. In effect, this redistribution takes the form of "borrowing" time from strong items to weak items. In an effort to reduce such borrowing between strong and weak items, Ratcliff et al. blocked items in mixed lists. In the present study, it is important to note that presentation rate was blocked by category, so the size of presentation rate blocks varied within and between lists. Due to the fact that block size varied in Experiment 1 in order to manipulate frequency, rehearsal borrowing is more of a problem than in Ratcliff et al.'s experiments. Ratcliff et al. who also used multiple lists within a session, each with a different strength composition, also gave an alternate possibility as to how the list-strength effect could be compromised. They proposed that under these circumstances, subjects may have not restricted their focus to the list they just studied, but rather to the session as a whole. Therefore, any differences between pure and mixed lists could have been reduced overall.

Despite some atypicalities, the fact that the results of Experiment 1 showed the same pattern of effects in the statistical analysis of recall and frequency estimates adds support to the availability view. Specifically, the effects of presentation rate, type of list, and frequency were parallel for both types of memory tests. This finding replicates previous findings which also demonstrated parallel movement between these two measures (e.g., Lewandowsky & Smith, 1986; Williams & Durso, 1986; Greene, 1989).

Overall, the present results are suggestive, but somewhat tentative. The findings of 1) a list-strength effect qualified by frequency and 2) parallel movement between the two measures are indicative of recall playing a role in categorical frequency estimation. While Experiment 1 suggests that recall can play a role in frequency estimation, it is not clear what other strategies subjects might use.

Specifically, the factors that lead a subject to choose between one strategy over another are numerous. The question remains: Does the strategy used in estimating absolute event frequency have an impact on such estimation? There are at least two different general strategies that can be used in estimating event frequency: enumeration and nonenumeration based strategies. Enumeration occurs when individual events/items are retrieved and counted and this count serves as the basis for the frequency judgment. Nonenumeration-based strategies are those that produce relative or a more qualitative evaluation of event frequency. For example, frequency judgments could be based on an assessment of the general strength or familiarity of the category information. Therefore, interpretation of performance differences in the literature may reflect differences purely in strategy use rather than representational differences, or a combination of both. This could be one reason why findings in the literature support two contradictory hypotheses for categorical frequency estimation.

Experiment 2

Experiment 1 has provided some evidence supporting the use of recall as a strategy in estimating categorical frequency. This is not to suggest however that it is the only strategy used in the judgment of event frequency. Memory researchers have already established that there may be multiple ways in which a frequency judgment is produced (e.g., Freund & Hasher, 1989; Barsalou & Ross, 1986; Watkins & LeCompte, 1991;Bruce et al., 1991; Greene, 1989).

While Experiment 1 provided some evidence in support of recall as a strategy, there are numerous factors which may reflect performance differences in frequency judgments. In effect, performance in frequency estimation may vary with the way that the information is encoded, the strategy used, or both (Brown, in press).

There have been numerous theories developed on how frequency information is represented in episodic memory. Tversky and Kahneman (1973) based people's ability in estimating frequency on availability-- that is, the ease with which particular instances come to mind. Howell (1973) distinguished four predominant ways in which frequency information could be derived. They are: the trace strength view, the multiple traces view, the multiple processes view, and the numerical inference view. The trace strength view maintains that any identifiable event is represented by a single memory trace that grows stronger upon each occurrence. Thus, any indicator of frequency memory, be it estimation or discrimination, is exclusively a consequence of assessing the value of the current strength. The multiple trace hypothesis maintains that every experience of an event produces a separate memory trace, so that multiple representations of an event coexist and are distinguishable by some attribute that may be tagged. In this case therefore, any indicator of frequency memory would in effect, represent the **number** of stored traces for an event. Howell's multiple-process hypothesis is to some extent, a compromise in that it assumes both the strength and multiple-trace views may play a role. In essence, the multiple-process view suggest that these two hypotheses may **not** be mutually exclusive in that both strength and number of traces accumulate concurrently. Finally, Howell's numerical inference hypothesis argues that if a subject knows in advance that frequency of items is important, they will strategically count. This raises a key question in studies of event frequency, which is, at what point and under what conditions do subjects switch from overt counting strategies to some other kind of intuitive representation?

Brown (1995) has proposed a taxonomy of distinct estimation processes in an attempt to address some of the ambiguities of people's strategy use. The main division in Brown's taxonomy is between **enumeration** and **nonenumeration** strategies. It is also useful in terms of organizing existing theoretical positions in frequency estimation.

Enumeration is the result of events or individual items being retrieved and individually counted, and this count serves as the basis for the frequency estimate. This count may be seen as being akin to recall--as the recall-estimate hypothesis would argue.

Nonenumeration processes on the other hand are **qualitative** in nature, producing a **relative** estimate of event frequency. And as such, it is assumed that these relative estimations must be transformed into numerical values for an absolute frequency judgment.

The List-Strength Effect

In Brown's (1995) investigation of the processes underlying absolute event frequency, subjects studied word pairs consisting of a category label and category exemplar (e.g., FRUIT--apple). In three experiments, subjects were presented with either a same-context or different-context study list. In the different-context condition the category label was presented with a different exemplar at each presentation (e.g., FRUIT--apple; FRUIT--pear; FRUIT--peach). In the same-context condition, the category label was presented with the same exemplar at each presentation (e.g., FRUIT--apple; FRUIT--apple; FRUIT--apple). Presentation frequencies for the target items ranged from 2 to 16.

The results across three experiments demonstrated that subjects used different strategies in estimating frequency. Verbal accounts taken in Experiment 1 revealed that different-context subjects often retrieved and counted relevant instances when they estimated event frequencies and that same-context subjects relied on nonenumeration strategies. Experiments 2 and 3 showed that response times increased sharply with presentation frequency in the different but not in the same context condition. Also in Experiments 1 and 2, it was indicated that strategy selection affected the magnitude of the frequency judgment. Specifically, Brown found that underestimation tended to occur in the different-context condition, particularly at the higher frequencies and that overestimation tended to occur in the same-context condition. Finally, in Experiment 3, it was shown that information given to subjects about the upper bound of the frequency range affected frequency judgments in the same but not in the different-context condition. In effect, telling subjects there was a bound of "x" items only affected the same-context group. Overall then, Brown demonstrated that 1) there was at least dual strategy use in estimating event frequency;and 2) that a selected strategy is related to the contents of memory.

Overall, Brown's recent evidence suggests that people enumerate when event instances are judged distinctive in nature, and use nonenumeration-based strategies when instances are deemed similar. The presence of a qualified list-strength effect in Experiment 1 indicated the use of an enumeration-based process in the judgment of categorical frequency; that process being recall. Due to the fact that Experiment 1 was a different-context paradigm, the results may be seen as being parallel to Brown's findings. On this basis, it should be possible to manipulate a number of factors that influence the way that information is encoded, stored and retrieved, and assess how these factors affect categorical frequency judgments. Taken one step further, a list-strength effect (evidence of recall and therefore, enumeration) in a different-context but not in a same-context condition, would provide salient evidence of multiple strategy use in estimating categorical frequency.

The purpose of Experiment 2, therefore, was to examine the list-strength effect for category frequency judgments in a different-context condition that would encourage a recall-based strategy and in a same-context condition that would be far less amenable to a recall-based strategy. It was hypothesized that there would be evidence of a list-strength effect in the different but not in the same-context condition.

Method

Design. The design was a 2x4x2x2 factorial with test (same-context vs. different-context) as a between-subjects factor and category frequency (2,4,8,12), presentation rate (1 or 3 sec.) and list type (mixed vs. pure) as within-subjects factors.

Participants. Forty-eight students participated voluntarily for either bonus credit from the participant pool at Wilfrid Laurier University or for payment. Twenty-four subjects were randomly assigned to the same-context condition, and twenty-four to the different-context condition.

Apparatus and Materials. The stimulus items were the same as in Experiment 1 with the additional exemplars taken from the Battig and Montague (1969), Hunt and Hodge (1971) and McEvoy and Nelson (1982) sets of norms (see Appendix B). The word pool consisted of 768 words from 64 different categories.

Procedure. The procedure of Experiment 2 was different from Experiment 1 in a number of respects in order to more closely replicate Brown's study. First, the frequencies of the categories increased from that of Experiment 1, which also meant a longer list length. The frequencies in Experiment 2 were 2,4,8, and 12. Per study list, each frequency was represented four times, equalling 16 different exemplar categories. The total list length in Experiment 2 per study list was 104 items. Also, in Experiment 2, each exemplar was presented with its respective category label on the computer screen. The category label and exemplar were placed on two lines in the centre of the computer screen. The label was placed one line above the exemplars with the label in lower case letters and exemplars in upper case letters.

There were two conditions in Experiment 2: Same-context and different-context. In the different context condition, each category label was presented with a different exemplar (e.g., City--TORONTO; City--OTTAWA; City-- LONDON). In the same-context condition, each category label was presented with the same exemplar each time (e.g., City--TORONTO; City--TORONTO; City--TORONTO). As in Experiment 1, each subject received four study lists in a session: One pure slow, one pure fast and two mixed lists. Subjects initiated the beginning of each study list sequence by pressing any key on the computer keyboard.

Each study list was followed by a test list. Subjects initiated the test list by pressing any key on the computer keyboard. Each test list consisted of the 16 different category labels presented one at a time in the centre of the computer screen. The tests were subject-paced such that upon making a decision, they typed a numerical value directly onto the computer and then pushed the enter key. Pushing the enter key advanced the display to the next test item. Subjects were also allowed to make corrections to their numerical estimates by using the backspace key before pushing the enter key. In Experiment 2, response times were also taken and were measured by the amount of time to type numbers and hit the enter key from the onset of the category test probe.

In terms of instructions, subjects were told to make their decisions both as accurately and quickly as possible, although accuracy was more important than speed. Subjects were reminded that when not sure of the correct response, to make their best guess and to move onto the next test item. They were also informed of the nature of the up-coming memory tests. Because subjects were given four separate study-test trials, explicit instructions were necessary. The average session lasted approximately 30 minutes.

Results

Mean Frequency Estimates. Figure 3 shows the mean frequency estimates for each presentation rate and list type as a function of presentation frequency for the different-context condition. It is clear in Figure 3 that frequency judgments increased with presentation frequency and that they were higher for strong items compared to weak items.

Insert Figure 3 about here

The mean frequency estimates for each presentation rate and list type as a function of presentation frequency for the same-context condition are presented in Figure 4. The results for the same-context condition paralleled those found in the different-context condition. As Figure 4 illustrates, strong items have higher mean frequency estimates than weak items. This difference between strong and weak items is considerably larger in the mixed lists which the list-strength phenomenon would predict. Thus, in mixed lists, it would appear that the strong items are remembered more and the weak items are remembered less.

Insert Figure 4 about here

A 4(frequency) x 2(list type) x 2(presentation rate) x 2(condition) ANOVA was performed on mean frequency estimates. All results were reported significant at the .05 level. The results showed estimates for strong items to be greater than estimates for weak items, $F(1,46)=18.04$, $MSe=3.81$. There was also a main effect of frequency, $F(1,46)=427.76$, $MSe=3.61$. The type of list did not approach significance, $F(1,46)=.05$, $MSe=5.60$, nor did condition, $F(1,46)=2.67$, $MSe=32.52$. The only significant two-way interaction was that of list type by presentation rate, $F(1,46)=13.08$, $MSe=2.03$, so that presentation rate (1 or 3-sec.) had differential effects on the type of list, either mixed or pure. This list type by presentation rate interaction reflects the list-strength effect.

The other two-way interactions did not approach significance:

Condition by list type, $F(1,46)=1.81$, $MSe=5.80$, condition by presentation rate, $F(1,46)=.25$, $MSe=3.88$, condition by frequency, $F(3,138)=.40$, $MSe=3.65$, list type by frequency, $F(3,138)=.49$, $MSe=2.92$, and presentation rate by frequency, $F(3,138)=1.98$, $MSe=2.67$ were all not reliable. The three way interactions of: Condition by list type by presentation rate, $F(1,46)=.24$, $MSe=2.00$, condition by list type by frequency, $F(3,138)=.94$, $MSe=2.88$, condition by presentation rate by frequency, $F(3,138)=.20$, $MSe=2.75$, and list type by presentation rate by frequency, $F(3,138)=.78$, $MSe=2.37$ were also not significant. Finally, the four-way interaction between these variables also did not approach significance, $F(3,138)=1.18$, $MSe=2.36$.

Ratio of Ratios. The list-strength effect is further illustrated in the different-context condition by overall strong to weak ratio of ratios of 1.17 and 1.02 for mixed and pure lists, respectively. The overall mixed/pure ratio of ratios was 1.16. Table 4 presents the strong/weak ratios at each frequency for the different-context condition.

In the same-context condition, a similar pattern emerged demonstrated by overall strong to weak ratios of 1.16 and 1.04 for the mixed and pure lists, respectively. In addition, the overall mixed/pure ratio of ratios was calculated at 1.11. Table 5 presents the strong/weak ratios at each frequency for the same-context condition.

Insert Tables 4 and 5 about here

Mean Response Time. The same analysis was done for response times as for the mean frequency estimates. All items reported significant were at the .05 level. Figures 5 and 6 demonstrate how response times varied with frequency. They show how there is a greater increase with frequency in the different-context condition than in the same-context condition. In effect, response latency in the same-context condition varied little as a function of frequency whereas response latency in the different-context condition tended to increase.

Insert Figures 5 and 6 about here

The analysis of variance revealed that there was a significant main effect of condition, $F(1,46)=4.14$, $MSe=36.86$, indicating a difference in response times between same and different contexts. There was also a main effect of frequency, $F(1,46)=7.43$, $MSe=5.26$ so that the latency of frequency estimates significantly increased as a function of actual item frequency. List type and presentation rate did not approach significance, with $F(1,46)=.26$, $MSe=3.12$ and $F(1,46)=2.95$, $MSe=2.29$, respectively.

There was only one significant two-way interaction, presentation rate by frequency, $F(3,138)=3.71$, $MSe=0.84$, so the rate of the items had an effect on the latency of subjects' estimates of frequency. The other two-way interactions did not approach significance: condition by list type $F(1,46)=.22$, $MSe=3.14$, condition by presentation rate, $F(1,46)=.15$, $MSe=2.27$, list type by presentation rate, $F(1,46)=2.15$, $MSe=2.85$, list type by frequency, $F(1,46)=.25$, $MSe=1.28$; and finally condition by frequency $F(1,138)=2.00$, $MSe=5.25$, which shows that there was no significant difference in response times with presentation frequency between groups.

No three-way interactions approached significance, with condition by list type by presentation rate at $F(1,46)=2.10$, $MSe=2.84$, condition by list type by frequency, $F(3,138)=.85$, $MSe=1.26$, condition by presentation rate by frequency, $F(3,138)=.49$, $MSe=0.84$, and finally, list type by presentation rate by frequency, F 3,138)=2.50, $MSe=0.89$.

The four-way interaction of condition by list type by presentation rate by frequency was significant, $F(3,138)=3.03$, $MSe=0.89$. This interaction is difficult to interpret, but indicates that the main effects of condition and frequency are qualified by type of list and presentation rate. It should be noted, however, that overall response times were quite slow and somewhat variable. The standard errors of the means presented in Figures 5 and 6 varied from 1.11 to 3.07. Thus, the response time results must be interpreted with caution. It does seem quite clear from these results, though, that response time did not increase in a pronounced or systematic fashion as a function of frequency in either the same or different context condition. Further, the pattern of response times do not show the clear differences between the same and different context conditions that Brown (1995) obtained.

Discussion

The results show some evidence in favour of a recall-estimate view of categorical frequency judgment. Whereas Brown (1995) demonstrated that different strategies affected the pattern of performance on frequency judgments depending on what context the exemplars were in, the present experiment found similar performance in both the same-context and different-context conditions. In effect, whereas Brown found evidence of selective strategy use related to the context the event frequency was presented in, the present study found evidence of common strategies across both conditions. This could mean that a) subjects used a single strategy or, b) that subjects used a mixture or combination of strategies in both conditions. If subjects used a recall strategy some, but not all of the time then one would expect a modest list-strength effect and this is what was found in Experiment 2.

The reaction time data are more difficult to interpret. Brown (1995) found that response times increased sharply with presentation frequency in the different but not in the same-context condition. From this, Brown concluded that subjects favour enumeration-based strategies when event instances are distinctive (as in the different-context condition) and alternatively, favour nonenumeration strategies when they are not. While the present experiment found an effect of condition, there was no frequency by condition interaction; reaction times did not increase with frequency significantly in the different compared to the same context condition as in Brown's study. As such, the present reaction time results do not fully support a recall strategy. However, it is possible that subjects' emphasized accuracy and may not have been very concerned with response times, which was evident in the variability of the mean response times. Instructions given to subjects did not particularly highlight the importance of speed in making their decisions. Any future replications of this aspect of the experiment should address this problem.

There are a few possible reasons as to why the present study did not replicate Brown's findings of enumeration based strategy in the different but not in the same context condition. First, Brown's study used a presentation rate of 5.5 seconds for the study lists whereas this experiment used presentation rates of 1 and 3 seconds. Perhaps Brown's 5.5-second rate allowed subjects in the different context condition more time to organize the study lists by categories which would benefit a recall strategy.

Another difference in Brown's study was the actual study list itself. There was one list only, consisting of 260 category-label exemplar pairs. The present study contained four study lists, each followed by a test list. And as such, subjects were informed at outset of the nature of the up-coming memory test. Brown alternatively, with only one study list, did not inform subjects that the up-coming memory test involved frequency estimates. Perhaps knowing in advance what kind of test it is influenced the type of strategy subjects used in remembering the information. In all likelihood Brown's subjects expected a memory/recall test and studied the lists to that end. Therefore, it is more likely that they would try to use recall at test, at least in the different-context condition. Subjects in the present experiment knew about a frequency test, and may not have categorized or organized the lists as well, or were less likely to study lists in preparation for a recall type of test.

In effect, the results of Experiment 2 did not show the two distinct strategies that Brown observed. While reaction time data did reveal a between group difference, the reliability of that data is in question. Replication with identical instructional procedures to Brown's is in order. What is clear about Experiment 2 is that it shows limitations in the generality of Brown's results.

Experiment 2a

In Experiment 2 a significant list-strength effect was found for category frequency estimation. The purpose of Experiment 2a was to further examine the nature of the list-strength effect at these frequencies, this time with a cued-recall task. All aspects of Experiment 2a were identical to those of the different-context condition of Experiment 2 with the exception of the task at test.

Theoretically, any presence of a list-strength effect should be at least as strong, if not stronger, in a cued-recall task than in a frequency judgment task. Subjects tested in Experiment 2a were presented with the same lists of study and test items in the same order as subjects tested in the different-context condition of Experiment 2.

Method

Design. A 2x4x2 totally within subjects design was used in Experiment 2a with type of list (mixed vs. pure), presentation frequency (2,4,8,12) and presentation rate (1-second vs. 3-seconds) as the factors.

Participants. Twenty-four students participated voluntarily for either course credit from the participant pool at Wilfrid Laurier University or for payment.

Apparatus and Materials. The same program was used to generate the study and test lists for Experiment 2a as was used in Experiment 2.

Procedure. The only difference in procedure in Experiment 2a was in the task itself. The subjects' task was a cued-recall test instead of a frequency test. For each category label presented at test subjects were instructed to write down the exemplars of that category that were presented on the study list. Subjects proceeded through the test at their own pace. The subjects initiated the beginning of each trial by pressing the "U" key on the computer keyboard.

Results

Mean Recall. The mean number of exemplars recalled for each list type and presentation rate as a function of presentation frequency are presented in Figure 7. Items recalled increased as a function of frequency as expected. Also evident is an effect of strength, with strong items being better recalled than weak items. Not expected was a larger difference in the pure lists between strong and weak items, as Figure 7 illustrates. The list-strength effect would predict that the difference between strong and weak items to be in the mixed lists, where on average, strong items are in a sense "stronger" and weak items are "weaker".

Insert Figure 7 about here

A 4(frequency) X 2(presentation rate) X 2(list type) ANOVA was performed on the subject mean recall scores. The analysis showed strong items to be better remembered than weak items, $F(1,23)=36.95$, $MSe=1.08$. There was a main effect of frequency, $F(1,23)=423.79$, $MSe=0.68$, and no significant difference in overall mean recall between mixed and pure lists, $F(1,23)=.07$, $MSe=0.71$.

The only significant two-way interaction was that of presentation rate by frequency, $F(3,69)=9.09$, $MSe=0.54$; the difference in the number of strong versus weak items recalled increased as a function of frequency. The presentation rate by list interaction did not approach significance, $F(1,23)=3.16$, MSe=0.39; nor did the frequency by list interaction, $F(3,69)=.40$, MSe=0.40. The presentation rate x frequency x list interaction was not significant at $F(3,69)=.10$, MSe=0.50.

Ratio of Ratios. An effect of strength was also illustrated by an average of strong to weak ratios of 1.18 and 1.30 for mixed and pure lists, respectively. The overall mixed/pure ratio of ratios however demonstrates the general absence of the list-strength effect with a value of 0.90. Table 6 presents the strong/weak ratios and the ratio of ratios across each presentation frequency.

Insert Table 6 about here

Analysis of Intrusions. One strategy that subjects could use in a category recall task would be to generate category exemplars from semantic memory and then decide whether each exemplar had been presented on the basis of episodic memory for the study list. In other words, information about category membership from semantic memory could be used to guide retrieval. Such a strategy would lead to reporting intrusions, or exemplars that were not presented on the study list. If subjects used such a strategy, it would also be expected that intrusion errors would be greater for categories where the exemplars were presented at a fast rate rather than a slow rate because episodic memory for the category exemplars presented at a fast rate would be poorer.

To test for such a possibility, mean intrusion rates were calculated by recording the average number of times subjects recalled category exemplars that were never presented in the study list. These rates are presented for each frequency and list condition in Table 7. Overall, the number of intrusions was quite low and this precluded any statistical analyses of intrusion rates between conditions. Inspection of Table 7 indicates, at most, only marginal differences in intrusion rates between categories presented at fast and slow rates, or between mixed and pure lists.

As the recall rates presented in Table 6 show, subjects on average correctly recalled approximately half of the total number of exemplars presented at each frequency. Errors were predominantly due to omission rather than intrusions or guesses. These results indicate that subjects were recalling exemplars from their episodic memory of the study list, and did not use category information from semantic memory to augment or guide retrieval to any great extent.

Insert Table 7 about here

Discussion

Overall, there was no evidence of a list-strength effect in cued-recall in Experiment 2a. Theoretically, the results of Experiment 2 should have been replicated in terms of a list-strength effect in Experiment 2a since list-strength has been shown to be a feature of recall performance. This did not occur as demonstrated in the statistical analyses and further illustrated by an overall mixed/pure ratio of ratios of .90.

Perhaps the reason a list-strength effect was not found in Experiment 2a is because the list-strength effect is small or nonexistent in any cued recall task. This conclusion would agree with the results of Ratcliff et al. and would be consistent with the cued recall results of Experiment 1. Note that in Experiment 1, there was not an overall list-strength effect for cued recall (overall ratio of ratios of 1.03). Replication is needed in order to determine if perhaps there is a limit to the list-strength phenomenon in categorical frequency memory.

General Discussion

The aim of the present study was to test the recall hypothesis for category frequency estimation by examining the list-strength effect for category frequency judgments and category cued recall. Experiments 1 and 2 showed modest list-strength effects in judgments of category frequency. However, in Experiment 2, frequency estimation did not differ significantly when the same category exemplars were repeated in the study list compared to when different category exemplars were presented in the study list. Further, Experiment 2a, which examined cued recall for the different-context condition, did not show evidence of a list-strength effect. In fact, cued recall performance in Experiment 2a was lower than frequency judgment performance in Experiment 2.

All things considered, the present findings possibly make a stronger argument against the recall-estimate hypothesis than for it. This is true for a number of reasons, starting with Experiment 1. While there was a parallel pattern of effects for cued recall and frequency estimates in Experiment 1, the actual list-strength effect was quite modest for both tests. While the overall mixed/pure ratio of ratios were greater than 1 for both types of tests (1.03 and 1.11 for the cued recall and frequency groups, respectively) the overall effects were quite weak. Similarly, Ratcliff et al.'s cued recall group (Experiment 3) showed strong items to be better remembered than weak items with strong/weak ratios of 1.86 and 1.74 for mixed and pure lists, respectively with an overall ratio of ratios of 1.07. In the present study, it was assumed that because there were multiple exemplars for each cue (with the exception of frequency 1), the cued recall task was more akin to a free recall task which, theoretically, should produce robust list-strength effects. However, perhaps the problem of only finding a modest list-strength effect is in the very fact that subjects were provided any cue at all. There remains the possibility that providing subjects even with only one cue for multiple to-be-remembered items, organized their memory for an event differently than if they were given no cue at all.

It would be interesting for future research to investigate Experiment 1 in exactly the same manner, this time using a truly free recall task providing subjects with absolutely no cue in aiding their recall. That is, simply asking subjects to remember as many words as they could at the end of each study trial, without any category cues. If the results showed robust list-strength effects then it would show that the list-strength effect is largely limited to free recall, and is not a salient feature of any cued recall test. This would also mean that comparing the list-strength effect for category frequency estimation and cued recall is not a strong test of the role of recall in category frequency estimation, because the list-strength effect is not a strong feature of cued recall. Alternatively, if the results of such an experiment showed once again modest list-strength effects, if any, then it would provide more conclusive evidence against the use of recall as a strategy; or, if in the case of a modest list-strength effect once again, recall in combination with other strategies/processes.

There are a number of reasons why Experiment 2 cannot lend a great amount of support to the recall-estimate hypothesis. The mean judgments of frequency (Tables 4 and 5) showed a very similar pattern of results for the same and different-context conditions. If recall played a critical role in these judgments, one would expect a much larger difference between these two conditions because recall of exemplars should have been much easier in the different-context condition. While Brown found this, the present study did not. Also, if recall were being used as a strategy in estimating frequency, the list-strength effect should have increased as a function of frequency, when there is more to recall and as the test becomes more similar to free recall than cued recall (see ratio of ratios in Tables 4 and 5). This did not happen in Experiment 2 as the ratio of ratios for each frequency remained relatively consistent across each frequency.

The response time data in Experiment 2 also do not provide strong support for a recall strategy as there was no interaction between frequency and condition. While there was a main effect of condition in that response times were slower in the different-context condition, they did not increase systematically with frequency. This poses a serious problem because if subjects were consistently using a recall strategy, response times should increase with frequency, especially in the different-context condition where subjects had many different exemplars to recall. In contrast, Brown found a rather sharp increase in response time with presentation frequency in the different but not in the same-context condition. He concluded that this was due to different-context subjects using enumeration based strategies and same-context subjects using non-enumeration based strategies.

However, there are at least two problems concerning the response time data in Experiment 2 which make the results difficult to interpret. First, the time that passed between the moment subjects made a decision and when they actually entered their frequency estimate, adds considerable noise to the data in Experiment 2. While Brown, in his study, demonstrated that there was not a statistical difference between these two times, it is still a factor to consider. Also another problem with the present data is the fact that instructions did not emphasize speed of responding, which could have also contributed to the variability of the response times.

The findings of Experiment 2a also pose a serious problem for the recall-estimate hypothesis for two reasons. First of all, the cued recall results did not show a list-strength effect. Second, performance was quite low. If recall was difficult in Experiment 2, then a recall strategy would not be very helpful for judgments of frequency either. Note that the recall scores were much lower in Experiment 2a than the mean frequency judgments in the different, and even the same, context conditions of Experiment 2. So, if subjects in Experiment 2 were using a recall strategy, they had to be supplementing recall with something else in order to arrive at much higher, and quite accurate, judgments of frequency.

To summarize, the reasons which provide evidence against a recall estimate hypothesis are: 1) a small or modest list-strength effect for both cued recall and frequency estimation in Experiment 1; 2) the similar pattern of results for same and different context conditions in Experiment 2; 3) the list-strength effect did not increase reliably with frequency in Experiments 1 and 2; 4) mean response times did not increase with frequency in a systematic or meaningful fashion in Experiment 2; 5) the cued recall results in

Experiment 2a did not show a list-strength effect; and 6) the low performance in cued recall in Experiment 2a. Based on these findings, it can be argued that category frequency judgments are not primarily based on recall. There was, however, some evidence for a recall component in judgments of frequency, primarily: 1) there was a list-strength effect, although modest, in judgments of category frequency in Experiment 1; 2) there was a parallel pattern of effects between frequency and cued recall in Experiment 1; and 3) there was evidence of a list-strength effect present in Experiment 2. Based on these findings, it could be argued that some subjects used recall some of the time to estimate frequency, and this resulted in the small list-strength effects which were found in Experiments 1 and 2. An alternate possibility is that subjects actually tried to recall some of the exemplars, and supplemented recall with some other information to arrive at their final frequency estimate. If judgments of frequency are only partially based on recall, one would expect only a small list-strength effect, which is what the present study found. Therefore, it is more likely that category frequency judgments are the result of complex retrieval and decision processes, recall playing some role, but it is not the primary process in such estimation.

How would this explanation account for the literature supporting the recall-estimate theory? For example, evidence that familiarity with category exemplars (Tversky & Kahneman, 1973), exemplar spacing (Greene, 1989), and exposure time (Williams &

Durso, 1986) affect categorical frequency judgments and recall in the same manner is compatible with the recall-estimate notion but does not absolutely confirm it. Watkins and LeCompte (1991) who found evidence of a dissociation between frequency judgments and recall and who concluded that a recall-estimate strategy is greatly exaggerated, argued that logically, recall and frequency judgments may respond to a variable in the same way even if the judgments are not solely moderated by recall.

There are a number of possibilities regarding future research directions with respect to the present study. First of all, as there was such disagreement with the results of Experiment 2 and Brown's findings, a further attempt to replicate Brown's (1995) results could be done following his procedure more closely. For example, perhaps the 1-second versus 3-second difference in presentation rate was not great enough. Perhaps increasing the difference between fast and slow presentation rates to 1.5 seconds versus 5.5 seconds (the latter being what Brown used in his study). Another possibility for future research is testing both cued recall and judgment of frequency within subjects. For example, having 8 lists; 2 pure and 2 mixed for the cued recall and the same for judgment of frequency tests, but do not tell subjects which test until after they have studied each list so that they do not know the test when they are studying the lists. In the present experiment, specific instructions on the type of test were given to the subjects at outset as the four tests were identical per subject. Finally, future instructions could highlight the importance of both speed and accuracy as being equally important. The present study highlighted both but emphasized accuracy over speed. This way, subjects may place more emphasis on response time. Future research could also examine possible boundaries of recall in estimating categorical frequency by manipulating factors affecting encoding, storage, and retrieval of information. It is highly likely, and consistent with the present study, that some factors, but not others, elicit the use of recall in judgments of categorical frequency. There remains a host of other factors in addition to list-strength, that need to be evaluated before any firm statements regarding strategy use may be made. Thus, while recall was found here to play some role in estimating categorical frequency in the present study, it is certainly not the rule in such estimation.

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Table 1. A simulated example of mean recall and the list-strength effect as demonstrated by strong/weak ratios and overall mixed/pure ratio of ratios.

Strength (seconds)	Mixed List	Pure List	Mixed/Pure Ratio of Ratios
3 1	1.34 1.01	1.17 1.11	
Strong/Weak Ratio	1.33	1.05	**1.27**

Table 2. Mean recall and strong/weak ratios for each frequency for the cued-recall group of Experiment 1.

Rate (seconds)	Mixed list	Pure list	Mixed/Pure ratio of ratios
(frequency 1)			
3 s	0.51	0.72	
1 s	0.55	0.57	
s/w ratio:	0.93	1.26	0.74
(frequency 3)			
3 s	2.26	2.12	
1 s	1.56	1.97	
s/w ratio:	1.45	1.08	1.34
(frequency 5)			
3 s	3.39	3.21	
1 s	2.72	3.00	
s/w ratio:	1.25	1.07	1.17
(frequency 7)			
3 s	4.49	4.50	
1 s	3.88	3.65	
s/w ratio:	1.16	1.23	0.94

Note.s/w=strong/weak.

Table 3. Mean frequency estimates and strong/weak ratios for each frequency for the frequency group of Experiment 1.

Rate (seconds)	Mixed list	Pure list	Mixed/Pure ratio of ratios
(frequency 1)			
3 s	1.67	1.47	
1 s	1.38	1.71	
s/w ratio:	1.21	0.86	1.41
(frequency 3)			
3 s	3.04	2.94	
1 s	2.71	3.17	
s/w ratio:	1.12	0.93	1.21
(frequency 5)			
3 s	4.50	4.40	
1 s	4.20	4.02	
s/w ratio:	1.07	1.09	0.98
(frequency 7)			
3 s	5.72	5.97	
1 s	5.34	5.16	
s/w ratio:	1.07	1.16	0.92

Note. s/w=strong/weak

Table 4. Mean frequency estimates and strong/weak ratios for each frequency for the different-context condition of Experiment 2.

Rate (seconds)	Mixed list	Pure list	Mixed/Pure ratio of ratios
(frequency 2)			
3 s	3.22	2.96	
1 s	2.51	3.08	
s/w ratio:	1.28	0.96	1.33
(frequency 4)			
3 s	4.72	4.40	
1 s	4.13	4.43	
s/w ratio:	1.14	0.99	1.15
(frequency 8)			
3 s	8.05	7.10	
1 s	7.09	6.65	
s/w ratio:	1.13	1.06	1.07
(frequency 12)			
3 s	10.33	9.49	
1 s	9.20	8.97	
s/w ratio:	1.12	1.05	1.07

Note.s/w=strong/weak

Table 5. Mean frequency estimates and strong/weak ratios for each frequency for the same-context condition of Experiment 2.

Rate (seconds)	Mixed list	Pure list	Mixed/Pure ratio of ratios
(frequency 2)			
3 s	3.80	4.03	
1 s	3.72	3.73	
s/w ratio:	1.02	1.08	0.94
(frequency 4)			
3 s	5.55	5.29	
1 s	4.64	5.21	
s/w ratio:	1.19	1.01	1.18
(frequency 8)			
3 s	8.42	8.23	
1 s	6.89	7.55	
s/w ratio:	1.22	1.09	1.12
(frequency 12)			
3 s	10.79	10.10	
1 s	8.96	10.18	
s/w ratio:	1.20	0.99	1.21

Note.s/w=strong/weak

Table 6. Mean recall and strong/weak ratios for each frequency for Experiment 2a.

Rate (seconds)	Mixed list	Pure list	Mixed/Pure ratio of ratios
(frequency 2)			
3 s	1.07	1.12	
1 s	1.02	0.87	
s/w ratio:	1.05	1.29	0.81
(frequency 4)			
3 s	2.21	2.15	
1 s	1.83	1.67	
s/w ratio:	1.21	1.29	0.93
(frequency 8)			
3 s	3.78	4.01	
1 s	3.13	3.04	
s/w ratio:	1.21	1.32	0.92
(frequency 12)			
3 s	5.47	5.63	
1 s	4.43	4.29	
s/w ratio:	1.23	1.31	0.94

Note.s/w=strong/weak

Table 7. Mean number of intrusions recalled for each frequency and list type for Experiment 2a.

List Type

Pres. Freq.	Fast Pure	Fast Mixed	Slow Pure	Slow Mixed	TOTAL
2	.88	.63	.71	.63	2.85
4	1.08	.79	.71	.46	3.04
8	.67	.96	.71	.75	3.09
12	.38	.42	.50	.21	1.51
TOTAL	3.01	2.80	2.63	2.05	10.49

Figure 1. Mean cued recall as a function of presentation frequency and presentation rate for mixed and pure lists for Experiment 1.

Figure 1.

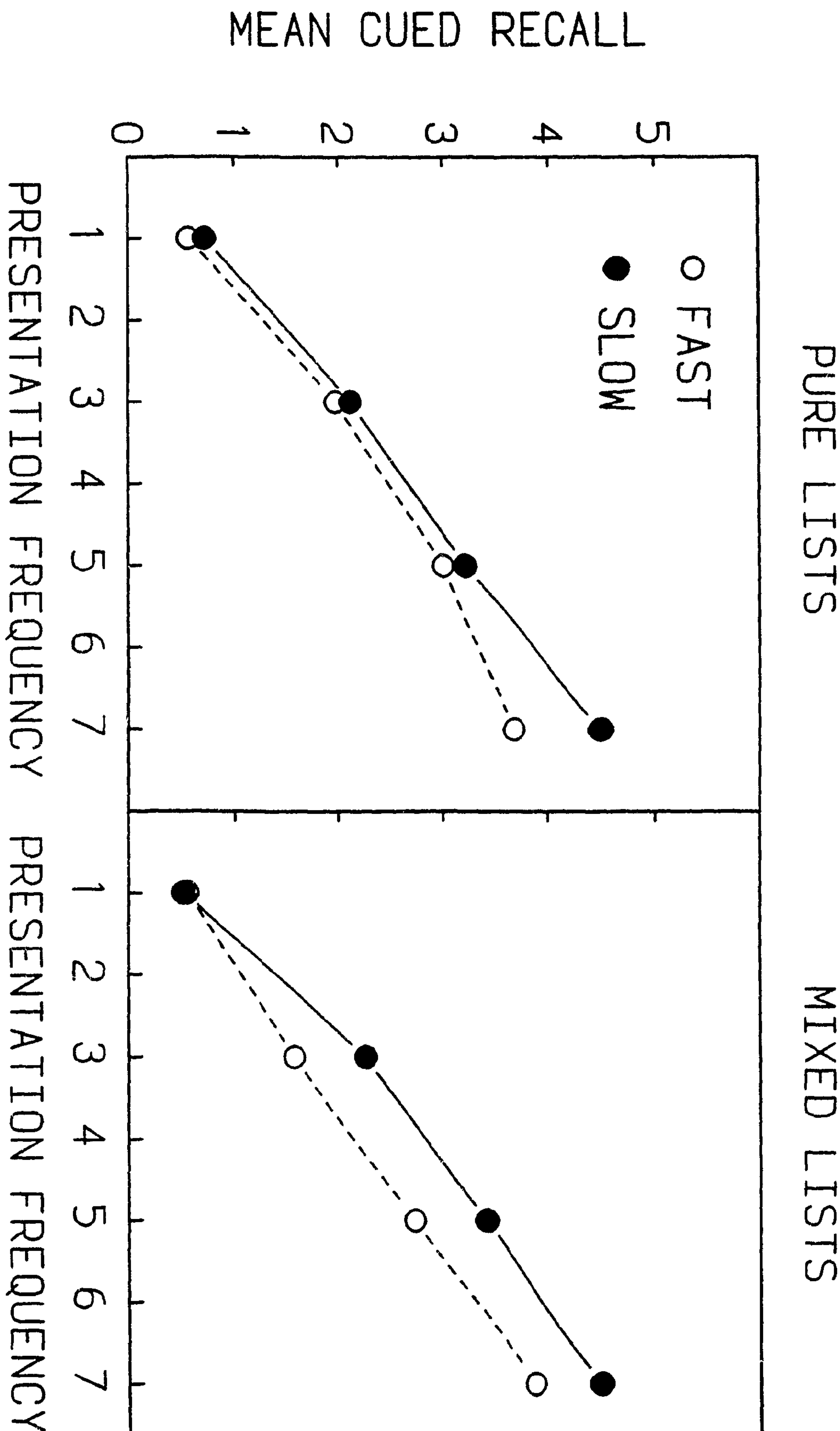

MEAN CUED RECALL
PURE LISTS
MIXED LISTS
FAST
SLOW
0
1
2
3
4
5
1
2
3
4
5
6
7
PRESENTATION FREQUENCY
PRESENTATION FREQUENCY

Figure 2. Mean frequency judgments as a function of presentation frequency and presentation rate for mixed and pure lists for Experiment 1.

Figure 2.

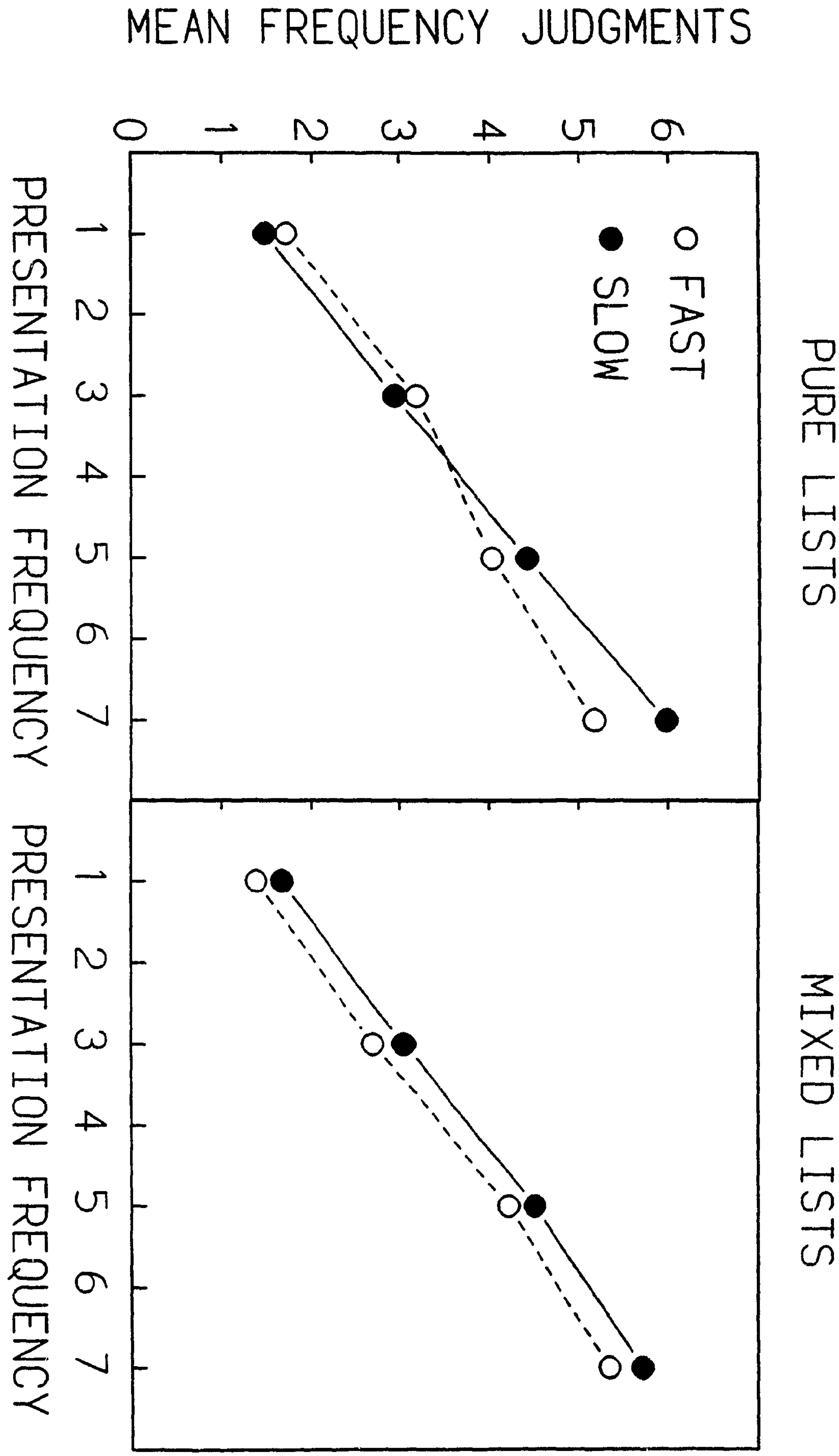
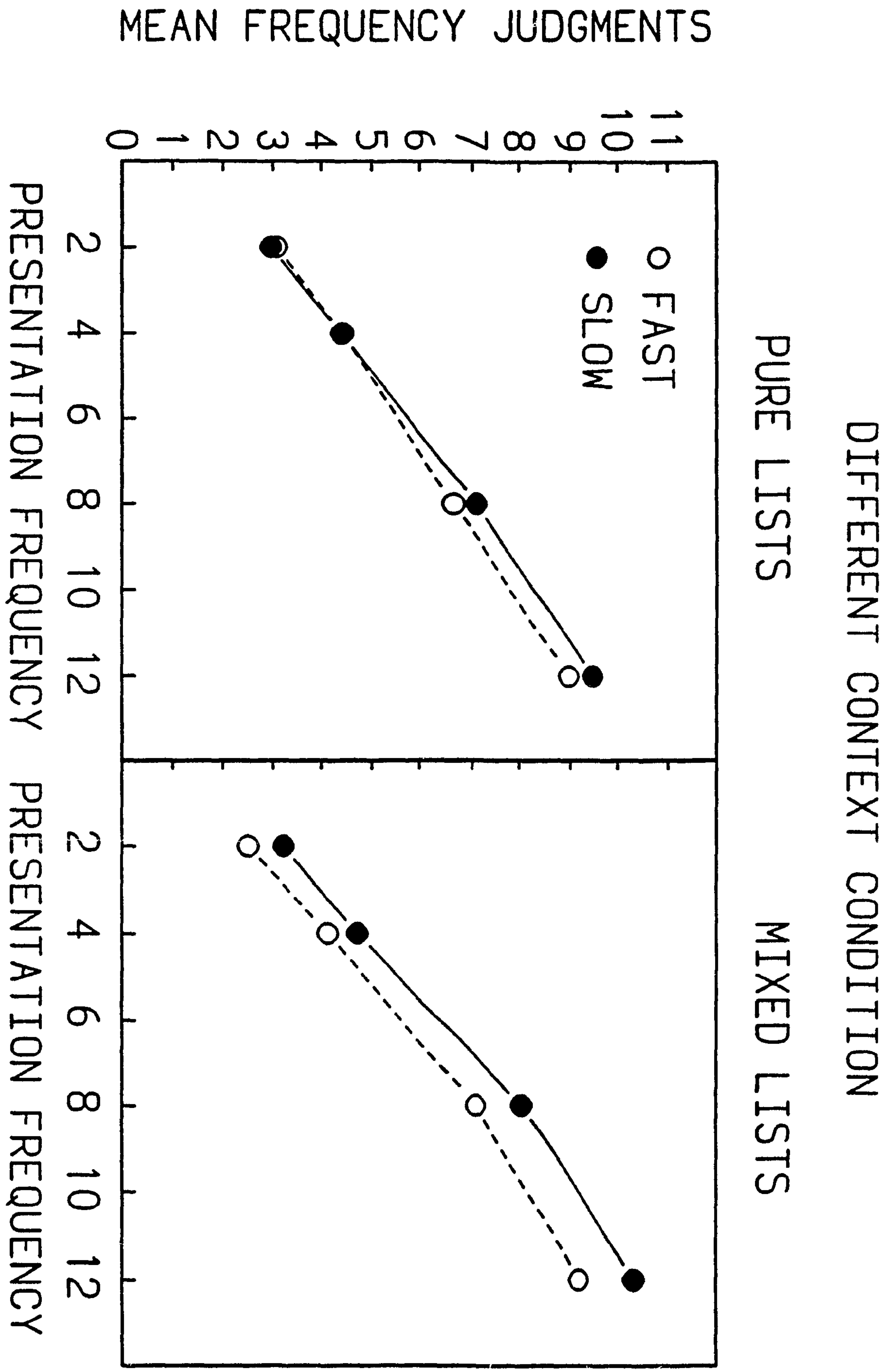

MEAN FREQUENCY JUDGMENTS
PURE LISTS
MIXED LISTS
FAST
SLOW
PRESENTATION FREQUENCY
PRESENTATION FREQUENCY
0
1
2
3
4
5
6
1
2
3
4
5
6
7

Figure 3. Mean frequency judgments as a function of presentation frequency and presentation rate for mixed and pure lists in the different-context condition for Experiment 2.

Figure 3.



DIFFERENT CONTEXT CONDITION
PURE LISTS
MIXED LISTS
MEAN FREQUENCY JUDGMENTS
FAST
SLOW
PRESENTATION FREQUENCY
PRESENTATION FREQUENCY
0 1 2 3 4 5 6 7 8 9 10 11
2 4 6 8 10 12
2 4 6 8 10 12

Figure 4. Mean frequency judgments as a function of presentation frequency and presentation rate for mixed and pure lists in the same-context condition for Experiment 2.

Figure 4.

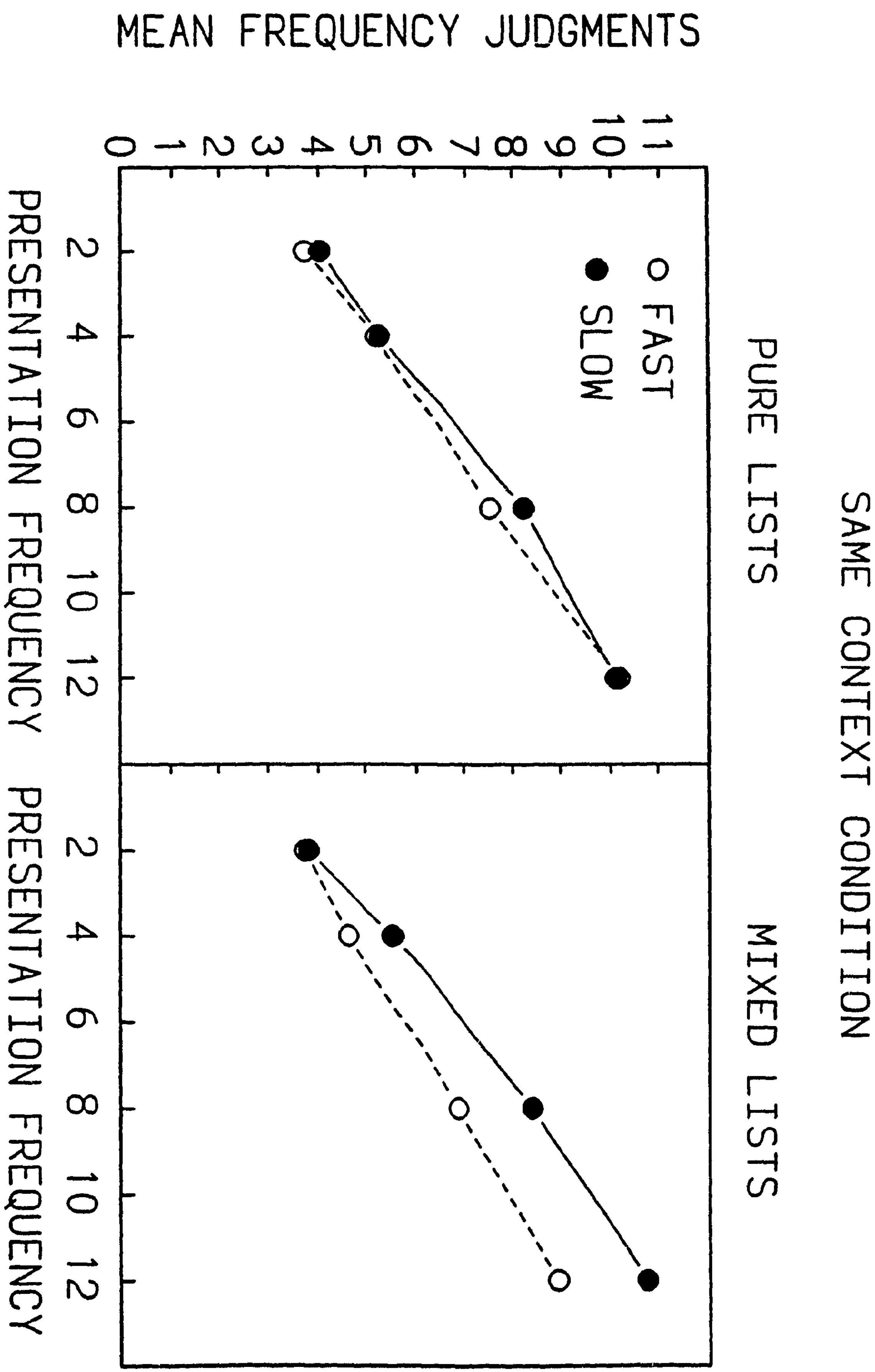

SAME CONTEXT CONDITION
PURE LISTS
MIXED LISTS
FAST
SLOW
MEAN FREQUENCY JUDGMENTS
PRESENTATION FREQUENCY
PRESENTATION FREQUENCY
0
1
2
3
4
5
6
7
8
9
10
11
2
4
6
8
10
12

Figure 5. Mean response time as a function of presentation frequency and presentation rate for mixed and pure lists in the different-context condition for Experiment 2.

Figure 5.

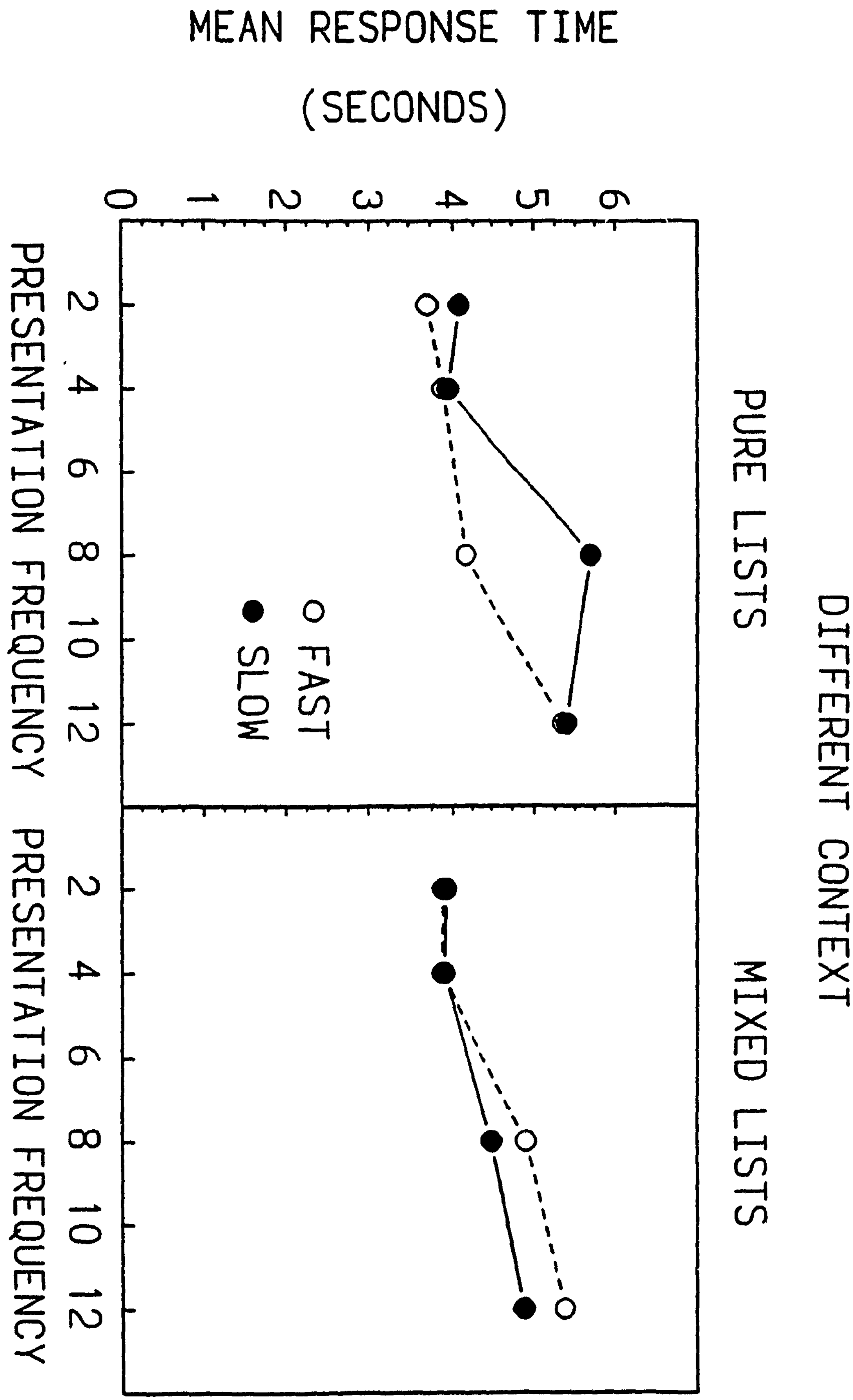

MEAN RESPONSE TIME
(SECONDS)
DIFFERENT CONTEXT
PURE LISTS
MIXED LISTS
0
1
2
3
4
5
6
2
4
6
8
10
12
PRESENTATION FREQUENCY
FAST
SLOW

Figure 6. Mean response time as a function of presentation frequency and presentation rate for mixed and pure lists in the same-context condition for Experiment 2.

Figure 6.

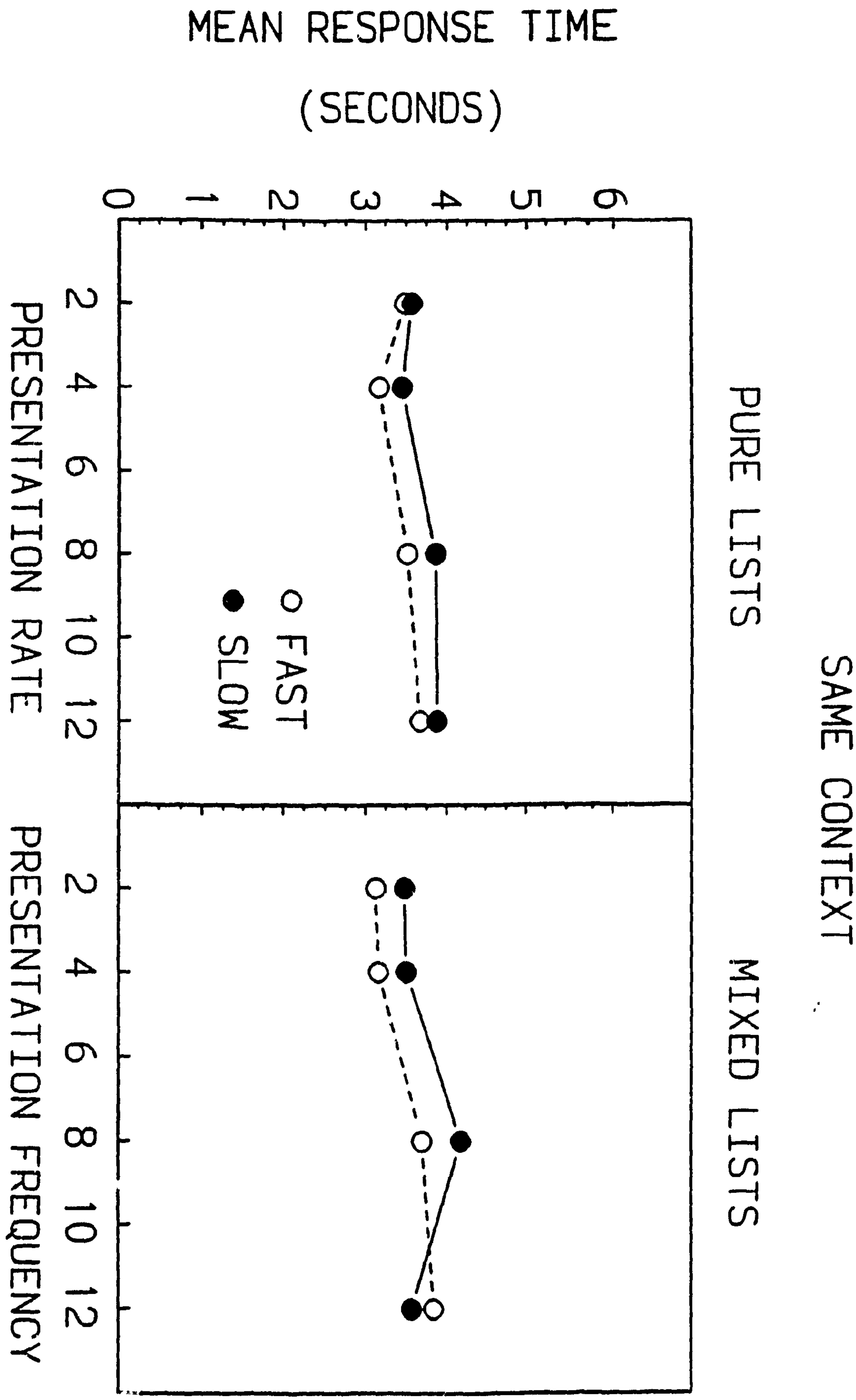
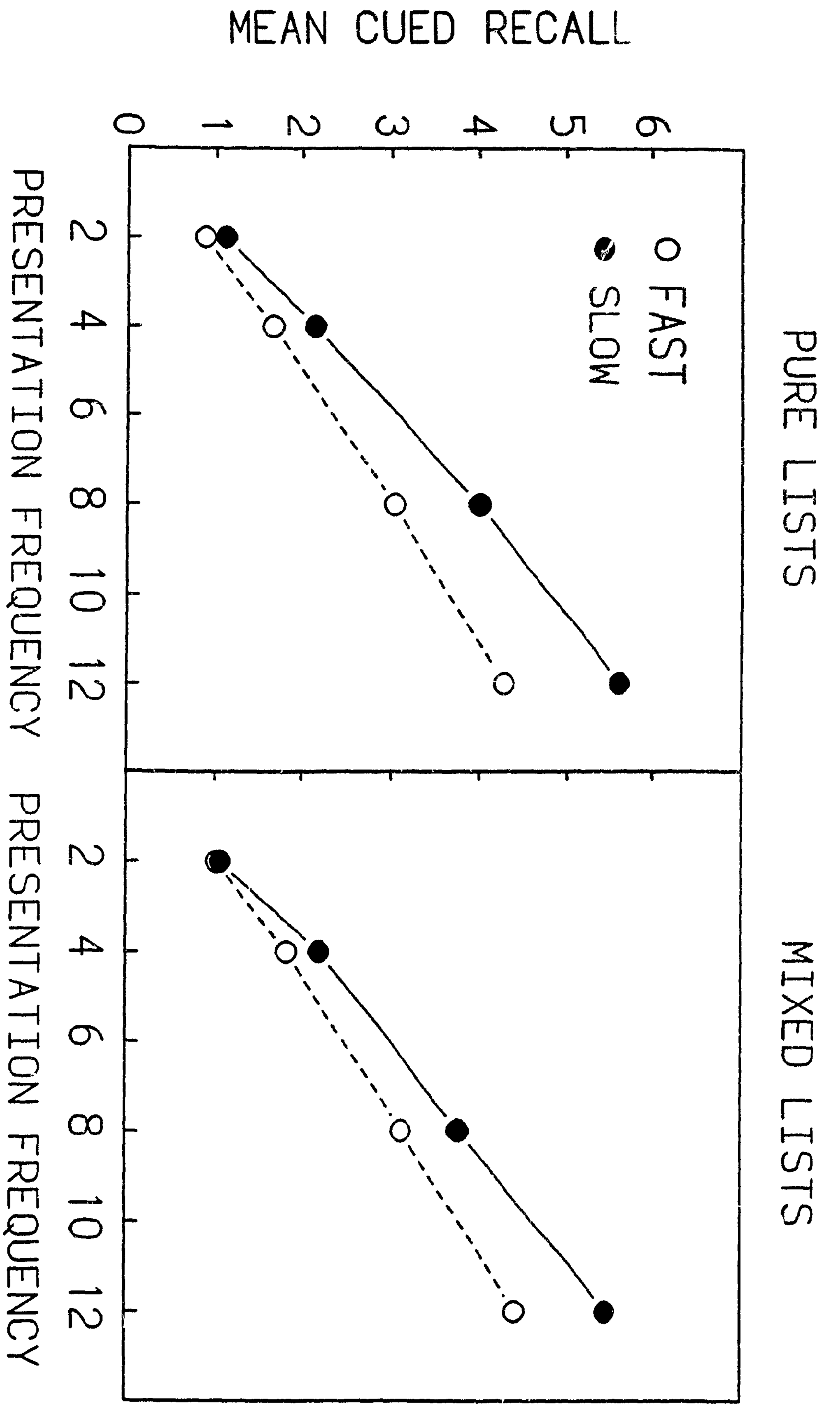

MEAN RESPONSE TIME
(SECONDS)
0
1
2
3
4
5
6
SAME CONTEXT
PURE LISTS
MIXED LISTS
FAST
SLOW
2
4
6
8
10
12
PRESENTATION RATE
PRESENTATION FREQUENCY

Figure 7. Mean cued recall as a function of presentation frequency and presentation rate for mixed and pure lists for Experiment 2a.

Figure 7.


MEAN CUED RECALL
PURE LISTS
MIXED LISTS
O FAST
● SLOW
0
1
2
3
4
5
6
2
4
6
8
10
12
PRESENTATION FREQUENCY
PRESENTATION FREQUENCY

Appendix A

1 GEMS
2 DIAMOND
3 RUBY
4 EMERALD
5 SAPPHIRE
6 PEARL
7 OPAL
8 JADE
9 TOPAZ
10 UNITS OF TIME
11 HOUR
12 MINUTE
13 SECOND
14 YEAR
15 DAY
16 CENTURY
17 MONTH
18 DECADE
19 RELATIVES
20 AUNT
21 UNCLE
22 FATHER
23 MOTHER
24 BROTHER
25 SISTER
26 COUSIN
27 GRANDMOTHER
28 METALS
29 IRON
30 COPPER
31 STEEL
32 GOLD
33 ALUMINUM
34 SILVER
35 TIN
36 BRASS
37 READING MATERIALS
38 MAGAZINE
39 BOOK
40 NEWSPAPER
41 PAMPHLET
42 NOVEL
43 TEXTBOOK
44 JOURNAL
45 ARTICLE
46 FOUR-FOOTED ANIMALS
47 DOG
48 CAT
49 HORSE
50 COW
51 LION
52 TIGER
53 ELEPHANT
54 PIG
55 TYPES OF CLOTH
56 COTTON
57 WOOL
58 SILK
59 RAYON
60 NYLON

61	VELVET
62	LINEN
63	SATIN
64	COLOURS
65	BLUE
66	RED
67	GREEN
68	YELLOW
69	ORANGE
70	BLACK
71	PURPLE
72	WHITE
73	UTENCILS
74	KNIFE
75	SPOON
76	FORK
77	PAN
78	POT
79	SPATULA
80	STOVE
81	MIXER
82	RELIGIOUS BUILDINGS
83	CHURCH
84	SYNAGOGUE
85	TEMPLE
86	CHAPEL
87	CATHEDRAL
88	MOSQUE
89	SHRINE
90	MONASTERY
91	PARTS OF SPEECH
92	NOUN
93	ADJECTIVE
94	PRONOUN
95	VERB
96	ADVERB
97	CONJUNCTION
98	PREPOSITION
99	INTERJECTION
100	TYPES OF FURNITURE
101	CHAIR
102	TABLE
103	BED
104	SOFA
105	DESK
106	LAMP
107	DRESSER
108	TELEVISION
109	BODY PARTS
110	LEGS
111	ARMS
112	HEAD
113	EYE
114	FOOT
115	NOSE
116	FINGER
117	EAR
118	FRUITS
119	APPLE
120	ORANGE

121	PEAR
122	BANANA
123	PEACH
124	GRAPE
125	CHERRY
126	PLUM
127	WEAPONS
128	GUN
129	RIFLE
130	BOMB
131	CLUB
132	SWORD
133	PISTOL
134	BAYONET
135	WHIP
136	ELECTIVE OFFICES
137	PRESIDENT
138	VICE-PRESIDENT
139	SENATOR
140	MAYOR
141	TREASURER
142	SECRETARY
143	GOVERNOR
144	CONGRESSMAN
145	DWELLINGS
146	HOUSE
147	APARTMENT
148	TENT
149	CAVE
150	HUT
151	HOTEL
152	TRAILER
153	DORMITORY
154	ALCOHOLIC DRINKS
155	BEER
156	WHISKEY
157	GIN
158	WINE
159	VODKA
160	BOURBON
161	SCOTCH
162	RUM
163	COUNTRIES
164	FRANCE
165	RUSSIA
166	ENGLAND
167	GERMANY
168	CANADA
169	ITALY
170	SPAIN
171	CHINA
172	CRIMES
173	MURDER
174	RAPE
175	ROBBERY
176	ASSAULT
177	ARSON
178	KIDNAPPING
179	LARCENY
180	FORGERY

181	TOOLS
182	HAMMER
183	SAW
184	NAILS
185	SCREWDRIVER
186	CHISEL
187	RULER
188	WRENCH
189	PLIERS
190	CLERGYMEN
191	PRIEST
192	MINISTER
193	RABBI
194	POPE
195	BISHOP
196	PASTOR
197	REVEREND
198	MONK
199	SPICES
200	SALT
201	PEPPER
202	SUGAR
203	GARLIC
204	VANILLA
205	CINNAMON
206	CLOVES
207	PAPRIKA
208	FUELS
209	OIL
210	GAS
211	COAL
212	WOOD
213	KEROSINE
214	PETROLIUM
215	PROPANE
216	BUTANE
217	PROFESSIONS
218	DOCTOR
219	LAWYER
220	TEACHER
221	DENTIST
222	ENGINEER
223	PROFESSOR
224	CARPENTER
225	SALESMAN
226	EARTH FORMATIONS
227	MOUNTAIN
228	HILL
229	VALLEY
230	RIVER
231	ROCK
232	LAKE
233	CANYON
234	CLIFF
235	SPORTS
236	FOOTBALL
237	BASEBALL
238	BASKETBALL
239	TENNIS
240	SWIMMING

241 SOCCER
242 GOLF
243 HOCKEY
244 WEATHER
245 HURRICANE
246 TORNADO
247 RAIN
248 SNOW
249 HAIL
250 SLEET
251 STORM
252 WIND
253 CLOTHING
254 SHIRT
255 SOCKS
256 PANTS
257 SHOES
258 BLOUSE
259 SKIRT
260 COAT
261 DRESS
262 BUILDING PARTS
263 WINDOW
264 DOOR
265 ROOF
266 WALL
267 FLOOR
268 CEILING
269 ROOM
270 BASEMENT
271 CHEMICALS
272 OXYGEN
273 HYDROGEN
274 NITROGEN
275 SODIUM
276 IRON
277 POTASSIUM
278 CARBON
279 SULFUR
280 MUSICAL INSTRUMENTS
281 PIANO
282 DRUM
283 TRUMPET
284 VIOLIN
285 CLARINET
286 FLUTE
287 GUITAR
288 SAXOPHONE
289 MONEY
290 DOLLARS
291 NICKELS
292 DIMES
293 QUARTERS
294 PENNIES
295 FRANCS
296 PESOS
297 LIRA
298 KINDS OF MUSIC
299 JAZZ
300 CLASSICAL

301	ROCK
302	POPULAR
303	FOLK
304	SYMPHONY
305	OPERA
306	BLUES
307	BIRDS
308	ROBIN
309	SPARROW
310	CARDINAL
311	BLUEJAY
312	EAGLE
313	CROW
314	CANARY
315	PARRAKEET
316	BEVERAGES
317	MILK
318	COKE
319	WATER
320	COFFEE
321	TEA
322	LEMONADE
323	JUICE
324	SPRITE
325	VEHICLES
326	CAR
327	BUS
328	AIRPLANE
329	TRAIN
330	TRUCK
331	BICYCLE
332	MOTORCYCLE
333	BOAT
334	SCIENCES
335	CHEMISTRY
336	PHYSICS
337	PSYCHOLOGY
338	BIOLOGY
339	ZOOLOGY
340	BOTANY
341	ASTRONOMY
342	MATHEMATICS
343	TOYS
344	DOLL
345	BALL
346	DOLLHOUSE
347	YO-YO
348	TEDDYBEAR
349	JACKS
350	PUZZLE
351	RATTLE
352	DANCES
353	WALTZ
354	TWIST
355	FOX-TROT
356	CHA-CHA
357	JITTERBUG
358	RUMBA
359	TANGO
360	POLKA

361	VEGETABLES
362	CARROT
363	PEA
364	CORN
365	BEAN
366	POTATO
367	TOMATO
368	LETTUCE
369	SPINACH
370	FOOTGEAR
371	SHOES
372	BOOTS
373	SANDALS
374	SOCKS
375	LOAFERS
376	MOCCASINS
377	SNOWSHOES
378	SNEAKERS
379	INSECTS
380	FLY
381	ANT
382	BEE
383	MOSQUITO
384	SPIDER
385	BEETLE
386	ROACH
387	WASP
388	GIRL NAMES
389	MARY
390	SUSAN
391	ANNE
392	JANE
393	JUDY
394	CAROL
395	BARBARA
396	KATHY
397	BOY NAMES
398	JOHN
399	BOB
400	BILL
401	JIM
402	TOM
403	JOE
404	DICK
405	MIKE
406	FLOWERS
407	TULIP
408	CARNATION
409	ORCHID
410	CHRYSANTHEMUM
411	PANSY
412	GARDENIA
413	DAFFODIL
414	DANDELION
415	DISEASES
416	CANCER
417	TUBERCULOSIS
418	MEASLES
419	LEUKEMIA
420	POLIO

421	MUMPS
422	SYPHILIS
423	MALARIA
424	TREES
425	OAK
426	MAPLE
427	PINE
428	ELM
429	BIRCH
430	DOGWOOD
431	SPRUCE
432	REDWOOD
433	SHIPS
434	SAILBOAT
435	DESTROYER
436	SUBMARINE
437	ROWBOAT
438	YACHT
439	CANOE
440	FREIGHTER
441	TUGBOAT
442	KINDS OF FISH
443	TROUT
444	BASS
445	SHARK
446	HERRING
447	CATFISH
448	PERCH
449	SALMON
450	TUNA
451	CITIES
452	TORONTO
453	CHICAGO
454	WASHINGTON
455	LONDON
456	PARIS
457	VANCOUVER
458	SASKATOON
459	DETROIT
460	EMOTIONS
461	FEAR
462	LOVE
463	HATE
464	ANGER
465	HAPPINESS
466	JOY
467	SADNESS
468	SORROW
469	AUTOMOBILES
470	FORD
471	CHEVROLET
472	PONTIAC
473	BUICK
474	CADILLAC
475	PLYMOUTH
476	OLDSMOBILE
477	DODGE
478	HOLIDAYS
479	CHRISTMAS
480	EASTER

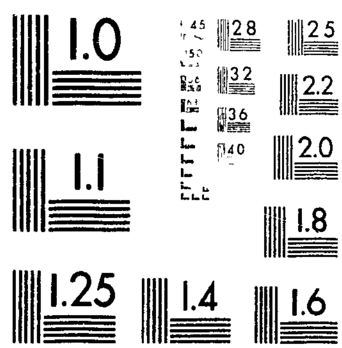
481 CANADA DAY
482 THANKSGIVING
483 LABOUR DAY
484 NEW YEAR'S
485 SUMMER
486 MARCH BREAK
487 RELIGIONS
488 CATHOLIC
489 JEWISH
490 BAPTIST
491 METHODIST
492 PROTESTANT
493 BUDDHIST
494 HINDU
495 PRESBYTERIAN
496 SENSES
497 SMELL
498 TOUCH
499 TASTE
500 SIGHT
501 HEARING
502 BALANCE
503 SOUND
504 FEELING
505 MONTHS
506 JUNE
507 APRIL
508 JANUARY
509 JULY
510 MARCH
511 MAY
512 FEBRUARY
513 AUGUST
514 MEATS
515 STEAK
516 PORK
517 BEEF
518 CHICKEN
519 LAMB
520 HAM
521 FISH
522 HAMBURGER
523 LANGUAGES
524 FRENCH
525 SPANISH
526 ENGLISH
527 GERMAN
528 RUSSIAN
529 ITALIAN
530 LATIN
531 GREEK
532 DRUGS
533 PENICILLIN
534 ASPIRIN
535 MORPHINE
536 CODEINE
537 DEXEDRINE
538 HEROIN
539 MARIJUANA
540 OPIUM

541 COSMETICS
542 LIPSTICK
543 POWDER
544 MASCARA
545 ROUGE
546 EYELINER
547 EYESHADOW
548 FOUNDATION
549 BLUSH
550 PLANETS
551 MARS
552 VENUS
553 JUPITER
554 EARTH
555 SATURN
556 MERCURY
557 NEPTUNE
558 URANUS
559 GODS
560 ZEUS
561 APOLLO
562 HERCULES
563 ODYSSEUS
564 APHRODITE
565 ULYSSES
566 CUPID
567 ATLAS
568 REPTILES
569 SNAKE
570 LIZARD
571 ALLIGATOR
572 CROCODILE
573 FROG
574 TURTLE
575 DINOSAUR
576 RATTLESNAKE

Appendix B

2 of/de 2

PM-1 3½"x4" PHOTOGRAPHIC MICROCOPY TARGET
NBS 1010a ANSI/ISO #2 EQUIVALENT



PRECISION℠ RESOLUTION TARGETS

1 Gems
2 DIAMOND
3 RUBY
4 EMERALD
5 SAPPHIRE
6 PEARL
7 OPAL
8 JADE
9 TOPAZ
10 AMETHYST
11 ONYX
12 GARNET
13 TURQUOISE
14 Time Units
15 HOUR
16 MINUTE
17 SECOND
18 YEAR
19 DAY
20 CENTURY
21 MONTH
22 DECADE
23 WEEK
24 MILLISECOND
25 EON
26 ERA
27 Relatives
28 AUNT
29 UNCLE
30 FATHER
31 MOTHER
32 BROTHER
33 SISTER
34 COUSIN
35 GRANDMOTHER
36 GRANDFATHER
37 NEPHEW
38 NIECE
39 HUSBAND
40 Metals
41 IRON
42 COPPER
43 STEEL
44 GOLD
45 ALUMINUM
46 SILVER
47 TIN
48 BRASS
49 BRONZE
50 LEAD
51 PLATINUM
52 URANIUM
53 Reading Material
54 MAGAZINE
55 BOOK
56 NEWSPAPER
57 PAMPHLET
58 NOVEL
59 TEXTBOOK
60 JOURNAL

61 ARTICLE
62 LETTER
63 ENCYCLOPEDIA
64 PERIODICAL
65 POEM
66 Four-Footed Animals
67 DOG
68 CAT
69 HORSE
70 COW
71 LION
72 TIGER
73 ELEPHANT
74 PIG
75 BEAR
76 MOUSE
77 RAT
78 DEER
79 Types of Cloth
80 COTTON
81 WOOL
82 SILK
83 RAYON
84 NYLON
85 VELVET
86 LINEN
87 SATIN
88 POLYESTER
89 FLANNEL
90 BURLAP
91 DENIM
92 Colours
93 BLUE
94 RED
95 GREEN
96 YELLOW
97 ORANGE
98 BLACK
99 PURPLE
100 WHITE
101 BROWN
102 MAROON
103 GRAY
104 PINK
105 Appliances
106 REFRIGERATOR
107 STOVE
108 DISHWASHER
109 WASHER
110 OVEN
111 RANGE
112 DRYER
113 FREEZER
114 TOASTER
115 BLENDER
116 MIXER
117 VACUUM
118 Math Operations
119 ADDITION
120 DIVISION

121	MULTIPLICATION
122	SUBTRACTION
123	EQUATION
124	ALGEBRA
125	FORMULA
126	DERIVATIVE
127	EXPONENT
128	PERCENTAGE
129	LIMIT
130	QUADRATIC
131	Speech Parts
132	NOUN
133	ADJECTIVE
134	PRONOUN
135	VERB
136	ADVERB
137	CONJUNCTION
138	PREPOSITION
139	INTERJECTION
140	PARTICIPLE
141	SUBJECT
142	SENTENCE
143	VOWEL
144	Furniture
145	CHAIR
146	TABLE
147	BED
148	SOFA
149	DESK
150	LAMP
151	DRESSER
152	TELEVISION
153	STOOL
154	BUREAU
155	BOOKCASE
156	CABINET
157	Body Parts
158	LEGS
159	ARMS
160	HEAD
161	EYE
162	FOOT
163	NOSE
164	FINGER
165	EAR
166	HAND
167	TOE
168	MOUTH
169	STOMACH
170	Fruits
171	APPLE
172	ORANGE
173	PEAR
174	BANANA
175	PEACH
176	GRAPE
177	CHERRY
178	PLUM
179	GRAPEFRUIT
180	LEMON

181 TANGERINE
182 APRICOT
183 Weapons
184 GUN
185 RIFLE
186 BOMB
187 CLUB
188 SWORD
189 PISTOL
190 BAYONET
191 WHIP
192 SPEAR
193 AXE
194 HATCHET
195 MACHETE
196 Elective Offices
197 PRESIDENT
198 VICE-PRESIDENT
199 SENATOR
200 MAYOR
201 TREASURER
202 SECRETARY
203 GOVERNOR
204 CONGRESSMAN
205 COUNCILMAN
206 CHAIRMAN
207 JUDGE
208 ALDERMAN
209 Dwellings
210 HOUSE
211 APARTMENT
212 TENT
213 CAVE
214 HUT
215 HOTEL
216 TRAILER
217 DORMITORY
218 MANSION
219 COTTAGE
220 CABIN
221 CASTLE
222 Alcoholic Drinks
223 BEER
224 WHISKEY
225 GIN
226 WINE
227 VODKA
228 BOURBON
229 SCOTCH
230 RUM
231 BRANDY
232 RYE
233 CHAMPAGNE
234 VERMOUTH
235 Countries
236 FRANCE
237 RUSSIA
238 ENGLAND
239 GERMANY
240 CANADA

241 ITALY
242 SPAIN
243 CHINA
244 MEXICO
245 SWEDEN
246 BRAZIL
247 THAILAND
248 Crimes
249 MURDER
250 RAPE
251 ROBBERY
252 ASSAULT
253 ARSON
254 KIDNAPPING
255 LARCENY
256 FORGERY
257 FRAUD
258 PERJURY
259 MANSLAUGHTER
260 BLACKMAIL
261 Tools
262 HAMMER
263 SAW
264 NAILS
265 SCREWDRIVER
266 CHISEL
267 RULER
268 WRENCH
269 PLIERS
270 DRILL
271 SCREWS
272 PLANE
273 SAWHORSE
274 Clergymen
275 PRIEST
276 MINISTER
277 RABBI
278 POPE
279 BISHOP
280 PASTOR
281 REVEREND
282 MONK
283 NUN
284 PREACHER
285 ARCHBISHOP
286 DEACON
287 Flavourings
288 SALT
289 PEPPER
290 SUGAR
291 GARLIC
292 VANILLA
293 CINNAMON
294 CLOVES
295 PAPRIKA
296 OREGANO
297 NUTMEG
298 THYME
299 PARSLEY
300 Fuels

301 OIL
302 GAS
303 COAL
304 WOOD
305 KEROSINE
306 PETROLIUM
307 PROPANE
308 BUTANE
309 GASOLINE
310 PETROL
311 DIESEL
312 STEAM
313 Professions
314 DOCTOR
315 LAWYER
316 TEACHER
317 DENTIST
318 ENGINEER
319 PROFESSOR
320 CARPENTER
321 SALESMAN
322 NURSE
323 PSYCHOLOGIST
324 PLUMBER
325 ACCOUNTANT
326 Earth Formations
327 MOUNTAIN
328 HILL
329 VALLEY
330 RIVER
331 ROCK
332 LAKE
333 CANYON
334 CLIFF
335 OCEAN
336 VOLCANO
337 STREAM
338 SEA
339 Sports
340 FOOTBALL
341 BASEBALL
342 BASKETBALL
343 TENNIS
344 SWIMMING
345 LACROSSE
346 BADMINTON
347 BOWLING
348 WRESTLING
349 SOCCER
350 GOLF
351 HOCKEY
352 Weather
353 HURRICANE
354 TORNADO
355 RAIN
356 SNOW
357 HAIL
358 SLEET
359 STORM
360 WIND

361	CYCLONE
362	SUNSHINE
363	LIGHTNING
364	THUNDER
365	Clothing
366	SHIRT
367	SOCKS
368	PANTS
369	SHOES
370	BLOUSE
371	SKIRT
372	COAT
373	DRESS
374	BRA
375	TOP
376	SWEATER
377	JACKET
378	Fixtures
379	SINK
380	TOILET
381	FAUCET
382	BATHTUB
383	LIGHT
384	MIRROR
385	WINDOW
386	SHOWERHEAD
387	TILE
388	SOAPDISH
389	SHOWER
390	TUBMAT
391	Chemicals
392	OXYGEN
393	HYDROGEN
394	NITROGEN
395	SODIUM
396	POTASSIUM
397	CARBON
398	SULFUR
399	ZINC
400	MERCURY
401	ARGON
402	CALCIUM
403	CHLORINE
404	Musical Instruments
405	PIANO
406	DRUM
407	TRUMPET
408	VIOLIN
409	CLARINET
410	FLUTE
411	GUITAR
412	SAXOPHONE
413	TROMBONE
414	TUBA
415	HARP
416	CELLO
417	Money
418	DOLLARS
419	NICKELS
420	DIMES

421	QUARTERS
422	PENNIES
423	FRANCS
424	PESOS
425	LIRA
426	YEN
427	RUBLE
428	SHILLING
429	CENTS
430	Music
431	JAZZ
432	CLASSICAL
433	ROCK'N
434	POPULAR
435	FOLK
436	SYMPHONY
437	OPERA
438	BLUES
439	COUNTRY
440	ALTERNATIVE
441	RAP
442	PUNK
443	Birds
444	ROBIN
445	SPARROW
446	CARDINAL
447	BLUEJAY
448	EAGLE
449	CROW
450	CANARY
451	PARRAKEET
452	HAWK
453	BLACKBIRD
454	WREN
455	PARROT
456	Beverages
457	MILK
458	COKE
459	WATER
460	COFFEE
461	TEA
462	LEMONADE
463	JUICE
464	SPRITE
465	PEPSI
466	COCOA
467	ROOTBEER
468	SODA
469	Vehicles
470	CAR
471	BUS
472	AIRPLANE
473	TRAIN
474	TRUCK
475	BICYCLE
476	MOTORCYCLE
477	BOAT
478	SCOOTER
479	WAGON
480	TRICYCLE

481	TRACTOR
482	Sciences
483	CHEMISTRY
484	PHYSICS
485	PSYCHOLOGY
486	BIOLOGY
487	ZOOLOGY
488	BOTANY
489	ASTRONOMY
490	MATHEMATICS
491	GEOLOGY
492	MICROBIOLOGY
493	MEDICINE
494	ENGINEERING
495	Type of Nuts
496	WALNUT
497	PEANUT
498	PECAN
499	CASHEW
500	ALMOND
501	BRAZILNUT
502	MACADAMIA
503	PISTACHIO
504	ACORN
505	CHESTNUT
506	COCONUT
507	HAZELNUT
508	Dances
509	WALTZ
510	TWIST
511	FOX-TROT
512	CHA-CHA
513	JITTERBUG
514	RUMBA
515	TANGO
516	POLKA
517	HUSTLE
518	SQUARE
519	DISCO
520	BALLET
521	Vegetables
522	CARROT
523	PEA
524	CORN
525	BEAN
526	POTATO
527	TOMATO
528	LETTUCE
529	SPINACH
530	ASPARAGUS
531	BROCCOLI
532	CELERY
533	CABBAGE
534	Footgear
535	SHOES
536	BOOTS
537	SANDALS
538	LOAFERS
539	MOCCASINS
540	SNOWSHOES

541 SNEAKERS
542 HEELS
543 FLATS
544 GALOSHES
545 PUMPS
546 CLEATS
547 Insects
548 FLY
549 ANT
550 BEE
551 MOSQUITO
552 SPIDER
553 BEETLE
554 ROACH
555 WASP
556 GRASSHOPPER
557 LADYBUG
558 GNAT
559 MOTH
560 Girl Names
561 MARY
562 SUSAN
563 ANNE
564 JUDY
565 JANE
566 CAROL
567 BARBARA
568 KATHY
569 LINDA
570 KRISTINE
571 NANCY
572 CARMEN
573 Boy Names
574 JOHN
575 BOB
576 BILL
577 JIM
578 TOM
579 JOE
580 DICK
581 MIKE
582 GEORGE
583 STEVE
584 MARK
585 HARRY
586 Flowers
587 TULIP
588 CARNATION
589 ORCHID
590 CHRYSANTHEMUM
591 PANSY
592 GARDENIA
593 DAFFODIL
594 DANDELION
595 ROSE
596 IRIS
597 LILAC
598 GERANIUM
599 Diseases
600 CANCER

601	TUBERCULOSIS
602	MEASLES
603	LEUKEMIA
604	POLIO
605	SMALLPOX
606	MONONUCLEOSIS
607	PNEUMONIA
608	LEPROSY
609	MUMPS
610	SYPHILIS
611	MALARIA
612	Trees
613	OAK
614	MAPLE
615	PINE
616	ELM
617	BIRCH
618	DOGWOOD
619	SPRUCE
620	REDWOOD
621	FIR
622	HICKORY
623	SYCAMORE
624	ASH
625	Ships
626	SAILBOAT
627	DESTROYER
628	SUBMARINE
629	ROWBOAT
630	YACHT
631	CANOE
632	FREIGHTER
633	TUGBOAT
634	STEAMSHIP
635	SCHOONER
636	LINER
637	MOTORBOAT
638	Fish
639	TROUT
640	BASS
641	SHARK
642	HERRING
643	CATFISH
644	PERCH
645	SALMON
646	TUNA
647	GOLDFISH
648	SWORDFISH
649	SUNFISH
650	WHALE
651	Cities
652	TORONTO
653	CHICAGO
654	WASHINGTON
655	LONDON
656	PARIS
657	VANCOUVER
658	SASKATOON
659	DETROIT
660	WINNIPEG

661 FREDERICTON
662 MIAMI
663 CALGARY
664 Emotions
665 FEAR
666 LOVE
667 HATE
668 ANGER
669 HAPPINESS
670 JOY
671 SADNESS
672 SORROW
673 DEPRESSION
674 JEALOUSY
675 EXCITEMENT
676 BITTERNESS
677 Automobiles
678 FORD
679 CHEVROLET
680 PONTIAC
681 BUICK
682 CADILLAC
683 PLYMOUTH
684 OLDSMOBILE
685 DODGE
686 HONDA
687 JEEP
688 CHRYSLER
689 VOLKSWAGEN
690 Dogs
691 COLLIE
692 POODLE
693 BEAGLE
694 SPANIEL
695 DOBERMAN
696 HUSKY
697 TERRIER
698 BULLDOG
699 SHEPHERD
700 SIBERIAN
701 SHEEPDOG
702 DALMATIAN
703 Parts of Buildings
704 DOOR
705 ROOF
706 WALL
707 FLOOR
708 CEILING
709 ROOM
710 BASEMENT
711 BRICK
712 HALL
713 STAIR
714 ELEVATOR
715 CHIMNEY
716 Religions
717 CATHOLIC
718 JEWISH
719 BAPTIST
720 METHODIST

721	PROTESTANT
722	BUDDHIST
723	HINDU
724	PRESBYTERIAN
725	MOSLEM
726	EPISCOPAL
727	LUTHERAN
728	JUDAISM
729	Months
730	JUNE
731	APRIL
732	SEPTEMBER
733	JANUARY
734	OCTOBER
735	JULY
736	NOVEMBER
737	MARCH
738	DECEMBER
739	MAY
740	FEBRUARY
741	AUGUST
742	Meats
743	STEAK
744	PORK
745	BEEF
746	CHICKEN
747	LAMB
748	HAM
749	FISH
750	HAMBURGER
751	SIRLOIN
752	ROAST
753	TURKEY
754	BOLOGNA
755	Languages
756	FRENCH
757	SPANISH
758	ENGLISH
759	GERMAN
760	RUSSIAN
761	ITALIAN
762	LATIN
763	GREEK
764	JAPANESE
765	MALTESE
766	UKRAINIAN
767	POLISH
768	Drugs
769	PENICILLIN
770	ASPIRIN
771	MORPHINE
772	CODEINE
773	DEXEDRINE
774	HEROIN
775	MARIJUANA
776	OPIUM
777	LSD
778	COCAINE
779	BENZEDRINE
780	NOVACAINE

781	Cosmetics
782	LIPSTICK
783	POWDER
784	MASCARA
785	ROUGE
786	EYELINER
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790	POWDER
791	POLISH
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793	CREAM
794	Composers
795	BEETHOVEN
796	BACH
797	MOZART
798	BRAHMS
799	TCHAIKOVSKY
800	CHOPIN
801	SCHUBERT
802	HAYDN
803	MANCINI
804	GERSHWIN
805	STRAUSS
806	HAMMERSTEIN
807	Mythical Beings
808	ZEUS
809	APOLLO
810	HERCULES
811	ODYSSEUS
812	APHRODITE
813	ULYSSES
814	CUPID
815	ATLAS
816	UNICORN
817	CYCLOPS
818	MEDUSA
819	PEGASUS
820	States
821	GEORGIA
822	FLORIDA
823	ALABAMA
824	CALIFORNIA
825	TENNESSEE
826	TEXAS
827	VIRGINIA
828	MISSISSIPPI
829	KENTUCKY
830	MAINE
831	ILLINOIS
832	MARYLAND