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AN ANALYSIS OF CODING IN THE
BROWN-PETERSON PARADIGM

DISSERTATION

Presented in Partial Fulfillment of the Requirements for
the Degree Doctor of Philosophy in the Graduate
School of The Ohio State University

By
Joelle Mast, B.A., M.A.

* * * * *

The Ohio State University
1979

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INTRODUCTION

The popularity in recent years of a cognitive approach to the study of learning and memory is illustrated by the abundance of articles dealing with processing stage and coding analyses. In studies attempting to determine the locus of proactive interference (Bennett, 1971, 1975; Chechile and Butler, 1975; Dillon, 1973; Dillon and Bittner, 1975; Gardiner, Craik and Birtwistle, 1972; Loftus and Patterson, 1975; Watkins and Watkins, 1975), the Brown-Peterson task has been used extensively. Excepting Dillon et al., these studies place the interference effect in the retrieval rather than in the encoding or storage stages of memory. The task itself involves a procedure whereby a short list of items is presented to the subject followed by a distractor task designed to prevent rehearsal of the items for a given period of time. At the end of this period, subjects are asked to recall the items. Performance is scored in terms of the number of items correctly recalled by the subject for any given trial.

Given that extensive work has been made of the parameters of this task, in terms of rehearsal difficulty (Bjork and Allen, 1970), length of the distractor task
(Loess, 1964), inter-trial interval (Kincaid and Wickens, 1970), etc., it is not surprising that the Brown-Peterson task has been chosen so frequently in stage analyses of proactive interference. What is surprising, given that organization and coding are topics that have been in the forefront of the literature on memory and learning for the past decade, is that very little work has looked at the way Brown-Peterson trials are coded.

Miller (1956) provided the stimulus for much of the research on coding with his paper "the magic number seven plus or minus two" in which he described the limit on immediate memorial capacity in terms of 'chunks' or units of information. Basically, he found that the constraint was on the number of chunks rather than on the amount of information the chunks contained. If information could be recoded into higher order chunks, more information could be retained. Johnson (1970, 1978) presented a hierarchical model dealing with chunking in serial learning. In this model, chunked items are represented by a code which functions like an opaque container in that decoding occurs in an all-or-none manner. The information within a code is not immediately available but must be decoded. Thus, the probability of chunk recall is affected by number of chunks rather than the size of the chunks. In a recent article (Johnson, 1978), he noted certain
stimulus characteristics of item sets that have served to define a chunk.

...chunks have been identified with item sets that: (a) form semantic groups; (b) are rehearsed together; (c) are generated by a single rule; (d) lie between some physical marker such as slashes or spaces; or (e) fall within a rhythm pattern during reading.

By nature of the paradigm alone, Brown-Peterson trials fit chunk definitions (b) and (d), and, if the materials on a trial are from a homogeneous taxonomic category, they fit (a) as well.

As Johnson notes, all of these stimulus-based definitions of chunking imply that the items making up a chunk have a certain integrality, or interdependence, in terms of performance. He suggests that the most powerful means of accounting for this integrality would be to assume that items within a chunk are coded as a unit within memory and are therefore retrieved in an all-or-none manner. This assumption, if applied to items presented on a particular Brown-Peterson trial leads to two major predictions: 1) within the limits of memory span, retrieval of the chunk should be expected to be independent of the number of items within it, and 2) any change in a chunk should lead to the changed chunk being treated as a new unit.
There is some supportive evidence for the first prediction. Wickens, Moody, and Dow (1979), using a modification of Sternberg's (1966) paradigm, have presented data showing that retrieval times, defined as the difference in RT between secondary and primary memory tasks, were the same for 2 and 4 item memory sets. The items in the memory sets all belonged to the same taxonomic category. This suggests that subjects, at least when given homogeneous sets of items, retrieve these sets as a unit. While list size has been shown to be inversely related to performance on that list when unrelated words are used in the Brown-Peterson paradigm (Fuchs and Melton, 1974), the size of prior trials seems to have no effect on subsequent performance. Shiffrin (1970a) using lists of 5 and 20 unrelated words found free recall to be independent of the length of previous lists. For example, subjects who had received a list of 20 words did as well on a subsequent list of 20 words as subjects whose first list contained only 5 words. The effects of list size have not, to my knowledge, been examined when lists are composed of semantically related items. Given a subspan list of related items, it is possible that subjects treat each trial as a unit, possibly labeling it with the category name and some recency tag. If this is the case, one would expect buildup of proactive interference to be independent of the size of the trial unit. Studies that
have utilized categorized material have completely con-
founded number of items per category with the number of
trials (Gardiner, Craik and Birtwistle, 1972; Watkins
and Watkins, 1975) and, if the number of items per trial
were to be manipulated, it is uncertain how to control
for the possible confounding factor of study time.

There is much evidence supporting the second, more
easily tested prediction that a change in a chunk should
lead to that chunk's being treated as a new unit. Part-
to-whole transfer in free-recall studies (Bower and
Lesgold, 1969; Tulving, 1966; Wood and Clark, 1969) have
found that positive transfer is only obtained if the sub-
ject can use the same organization in the whole list as
he did in the original list. This speaks strongly for
the role organization plays in learning and memory. If
repetition alone is responsible for learning, subjects
who had previously learned part of a list should do better
not worse than a control group who learned an entirely
new list. In a serial learning task, Fritzen and Johnson
(1969), using digit sequences with either ascending or
descending runs found that most errors were made on the
first member of a run. Bower and Winzenz (1969) found
that a change in the structure of a digit series resulted
in the changed sequence being functionally equivalent to
a new unit, i.e., there was no facilitation in recall due
to item repetition. This was true when the change in structure was phonemic (i.e., 16782 being read as one sixty seven eighty two versus sixteen seven eighty two) and, more impressively, when the change was in the location of pauses (one six seven PAUSE eight two versus one six PAUSE seven eight two). Johnson and Migdoll (1971) found similar results using a paired-associate task in which the stimuli, digits 1 and 2, were paired with sequences of seven letters. When the grouping of the letter sequences was changed, the subjects learned it at the same rate as a completely new sequence, and showed significant retroactive inhibition when tested on the initial sequence.

Given the physical separation of trials in the Brown-Peterson paradigm, and the task requirement to recall the items presented on the last trial, it is possible that subjects would treat items on a particular trial as a unit. In light of what we know about clustering, this might seem especially reasonable when items are from the same taxonomic category. Thus, if indeed items on a Brown-Peterson trial are coded as a unit, one might expect effects similar to the studies mentioned above. That is, repetition would be expected to facilitate performance if and only if the entire trial unit is repeated. Repetition of items that have been regrouped should not
result in a performance increment.

The suggestion that trials or lists are functional units is certainly not new. Newton and Wickens (1956) introduced the concept of generalized response competition and Postman, Stark and Fraser (1968) proposed an analogous concept of response-set suppression as an alternative to Melton and Irwin's (1940) unlearning-spontaneous recovery theory of interference. Recently, Postman and Keppel (1977) presented data suggesting that cumulative PI was due to increases in generalized competition and declines in list differentiation. Examining retention, using free recall, they found that retention losses reflected a decrease in the number of categories recalled. Although the time parameters of the Brown-Peterson task are considerably shorter than those in Postman and Keppel's studies, both situations deal with secondary memory as initially described by James (1890). Intuitively, it seems parsimonious, given the lack of evidence to the contrary, to suggest that the processes underlying forgetting are similar in the two cases. Yet despite this, experimenters investigating interference in the Brown-Peterson paradigm with categorized materials have couched their discussions in terms of items as the units of interference. As previously noted, while most of these authors have argued that interference occurs at
retrieval, they imply that this is an item effect.

Watkins and Watkins' (1975) overloading of retrieval cues hypothesis comes close to explicitly stating this:

**Buildup of PI occurs because as successive lists within a category are presented the number of items nested under the category name increases and hence recall of a given category instance declines.** (p. 443, emphasis mine)

If the efficiency of the category name as a functional retrieval cue declines with the number of items nested under it, then the level of recall in a final test following a sequence of Brown-Peterson trials should decline with the number of other items that have been presented from the same category. (p. 447)

Loftus and Patterson (1975) posit a similar model in the form of a recency list.

**Release from proactive interference may be accounted for in either of two ways: (1) a shift in stimulus category may lead the subject to create and process a completely new recency list, or (2) the same recency list may be maintained but the information in a preshift word will invariably be sufficient to allow it to be rejected.** (p. 119, emphasis mine.)

Asymptotic performance for a particular item in the Brown-Peterson paradigm is highly dependent on the number of prior items presented. (p. 106)

While the idea of chunking items within a trial may seem reasonable from a coding standpoint, it seems rather at odds with models of retrieval that rely on strength as the critical factor determining response availability, such as Conrad (1967) and Posner and Konick (1966). Also the fact that intrusions, although low, do occur and that they tend to come from more
recent trials cannot be handled easily by current hierarchical models of coding.

Given these conflicting viewpoints, the purpose of this dissertation was to test the prediction made by a chunking theory that changing a chunk will result in that changed chunk being treated as a new unit. This was accomplished by having two major forms of repetition, triad and item. With triad repetition, an entire three word list was repeated. With item repetition, the items shown on the repeat trial had all been seen previously but had never been paired together before. A chunking view would predict that this latter type of repetition would not facilitate performance relative to a control condition in which no items were repeated.
METHOD

Subjects

The subjects were 360 students, enrolled in an introductory psychology course, who participated in the experiment to fulfill a course requirement. They were assigned randomly to one of the 8 experimental or 2 control conditions when they arrived for the experiment. There were 36 subjects in each condition.

Design

This experiment was designed to examine the effects of repetition of items compared to repetition of an entire list. To control for word effects all groups to be compared received the same words on the critical trial. The design is outlined in Table 1.

There were five levels of the repetition factor represented by: 1) triad repetition same (TRS) which had a triad of items repeated with no change in their serial position within the triad, 2) triad repetition different (TRD) which had a triad of items from a particular trial repeated with a change in their serial position, 3) item repetition same (IRS) which, on the critical trial (4,8) received items that had been presented in the same serial
TABLE 1

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position on earlier trials but had never been grouped together before, 4) item repetition different (IRD) which had items repeated from previous trials but occupied different serial positions, and 5) a control (C) which did not receive any repetitions. There were eight trials per condition with repetitions occurring on trial 4 and/or 8. With TRS and TRD, the trial unit to be repeated was selected equally often from trials 1 through 3, and trials 5 through 7 so that average distance between an item's presentation and its repetition would be equal to that of IRS and IRD. This is shown in the Appendix. Triad position was also counterbalanced such that within each four trial block each triad appeared equally often in each trial position. By doing this, every subject in this experiment received a combination of items different from every other subject.

A second factor in this experiment was lag between presentation and repetition of the items. There were two levels of this factor. With a short lag the average distance between presentation and repetition was one trial. With a long lag the average distance between presentation and repetition was five trials. Thus, the overall design was a $2 \times 5 \times 8$ factorial with two between-factors (lag and repetition type) and one within-factor (trials).
Materials and Apparatus

Twenty-four words were drawn from each of three taxonomic categories (Battig and Montague, 1968) that had been shown to be unrelated (Collen, Wickens and Daniele, 1975) in the sense that items from one of the categories were never sorted with items from either of the other categories. Three categories were used to mitigate any possible word effect. The 24 words from each category were grouped into 8 base triads such that average word frequency per triad was balanced. The categories selected were: Fruit, with an average word frequency per triad of 6.08; Professions, with an average word frequency per triad of 41.96, and Birds, with an average word frequency per triad of 4.37. Frequencies were selected from the norms collected by Kucera and Francis (1967).

Apparatus and Procedure

Subjects were tested individually. After being seated in the experimental room, each subject received the following instructions:

This experiment is concerned with your performance on two tasks when they alternate in quick succession. The first is a verbal memory task; the second is a nonverbal counting task. Here is an outline of the experimental procedure. (Subjects at this point were handed a chart illustrating the Brown-Peterson task.) Everything will be shown on the wall in front of you. The first thing you will see is an asterisk which is simply a 'get ready' signal. It will be followed immediately by a slide containing three
words. I would like you to say these words out loud as soon as you see them. You must say them immediately because they will only be shown for two seconds. The words will be followed by a three-digit number. This is the nonverbal counting task. I would like you to count backwards by threes from this number as quickly as you can. For example, if the number 187 appears on the wall, you would say 187, 184, 181, 178, 175, etc. I will be scoring the number of correct subtractions you make within a given period, since I am interested in examining possible changes in your rate of counting across the various trials. So try to be both fast and accurate. Keep counting until a question mark appears. This is the verbal memory task. I would like you to recall the words that came before the counting task. If you remember the words in the order in which they appeared, please say them in that order. If not, simply say what you do remember. If you do not remember anything, don't say anything. The question mark will be followed by another asterisk which is a 'get ready' signal for three more words. Say these new words as soon as you see them. They will be followed by a different three-digit number. Count backwards by threes from this until another question mark appears. When you see the question mark, please recall the new words that came just before the last number you were counting back from. The experiment continues in such a manner for a series of trials. Do you have any questions?

Any questions were answered and the subject was asked to repeat the instructions briefly to determine whether the procedure was understood.

Each subject received 8 trials. Slides were projected by a Kodak Carousel projector with a Gerbrands tape timer controlling presentation rate. A trial consisted of a two second presentation of an asterisk as a ready signal, followed by a two second presentation of the word triad which the subject read aloud. The words were followed by a three-digit number that remained on the screen for 18 seconds. During this time, the subject counted
backwards by threes from the number. At the end of this interval, a question mark appeared signalling recall of the word triad. This recall period lasted eight seconds and was terminated by the presentation of an asterisk marking the start of a new trial. The entire sequence of 8 trials took 4 minutes 16 seconds. Upon completion of the 8 trials, each subject was given a final free recall task. Their instructions were:

Please write down all the words that you saw during the previous trials. You may recall them in any order you wish. However, don't guess; just write down the ones you are pretty sure you saw.

A list identification task followed final free recall. Subjects were handed a sheet of paper containing, in a random order, the triads they had actually seen. Subjects were instructed to number these triads from 1 to 8 where 1 was the first triad they had seen, 2 was the second, down to 8 which would be the last triad they had seen. Subjects were instructed to guess if they were unsure of the order.
RESULTS

Recall of the words on each Brown-Peterson trial was scored by assigning one point for each correct word, and an additional point if the words were recalled in the correct order. Because of the large number of subjects and analyses, the accepted level of significance was set at .01 in an attempt to control inflation of the alpha level.

Short-lag Data

To examine the buildup of proactive interference, the recall scored for the first three trials were examined. See Figure 1. An analysis of variance with one between (repetition type) and one within-factor (trials) revealed a significant main effect of trials: $F(2,175) = 162.62, p < .001$, indicating significant buildup of proactive interference. The main effect of repetition type was not significant: $F(4,175) = 2.84, p > .05$, and there was no significant trial by repetition type interaction: $F(8,350) = 0.62, p > .25$. Since there were no inherent subject differences among groups and all showed equivalent performance on the first three trials, data for the critical trial 4 were analysed separately. A one-way ANOVA
Fig. 1. Mean Recall Scores for the Brown-Peterson Test Trials for the Short-Lag Repetition Groups.
showed significant group differences: $F(4, 175) = 19.75, p < .001$. Newman Keuls tests indicated that all experimental repetition types (TRS, TRD, IRS, and IRD) had significantly higher levels of recall than the control group, $p < .01$. The experimental groups did not differ among themselves, $p > .05$.

Examination of performance on trials 5, 6 and 7 revealed no significant main effect of trial: $F(2, 175) = 0.83, p > .25$. Thus, an asymptote of proactive interference had been reached. There was no significant main effect of repetition type: $F(4, 175) = 1.18, p > .25$; nor was there a significant interaction between trials and repetition type: $F(8, 350) = 1.45, p > .10$.

Trial 8 data were analysed by a one-way ANOVA which showed a highly significant difference among the various repetition types: $F(4, 175) = 7.07, p < .001$. Newman Keuls tests again revealed that all experimental groups had a significantly higher level of recall than the control, $p > .05$.

Effect of a Second Repetition

A 2 x 5 ANOVA was performed to compare the performance of the repetition groups on their first repetition trial (4) with performance on the second repetition trial (8). This analysis disclosed a significant difference
between the two trials: $F(1,175) = 4.75, p < .001$, trial 4 having a higher average level of recall than trial 8. The main effect of repetition type was, of course, highly significant: $F(4,175) = 21.59, p < .001$. Newman Keuls tests again showed all experimental conditions to be significantly different at the .01 level from the control. There were no significant differences, $p > .05$, among the experimental groups. There was no repetition type by trial interaction: $F(4, 175) = 1.438, p > .10$.

**Effect of Repetition on Subsequent Trial**

A comparison of performance on trials 3 and 5 indicated that following the repetition trial 4, groups dropped back down to their previous level of interference. There was no significant main effect of trials: $F(1,175) = 1.29, p > .25$, no significant main effect of group: $F(4,175) = 3.21, p > .01$, and no significant interaction: $F(4,175) = 1.01, p > .25$.

**Long Lag Data**

Data are illustrated in Figure 2. The scores from the first seven trials were analysed in a $5 \times 7$ ANOVA. The main effect of trials was highly significant: $F(4,175) = 107.70, p < .0001$. Repetition groups did not differ significantly: $F(4,175) = 2.037, p > .05$; nor was there a significant interaction: $F(24,1050) = 1.71$. 
Fig. 2. Mean Recall Scores for the Brown-Peterson Test Trials for the Long-lag Repetition Groups.
Given that there was no evidence of spurious subject differences among the repetition groups, data from trial 8 were analysed in a one-way ANOVA. These groups differed significantly: $F(4,175) = 14.79$, $p < .001$ and Newman Keuls tests disclosed that as with short-lag data all experimental groups differed significantly from the control, $p < .01$. In addition, TRS had a significantly greater level of recall than any of the other experimental groups.

**Lag Effect**

Comparison of short-lag performance on trial 4 with long-lag performance on trial 8 enables some assessment of the effect of lag, or number of intervening trials, upon repetition. A 2 x 5 ANOVA indicated that there was a significant effect of lag: $F(1,350) = 18.20$, $p < .001$ with recall being higher when items were repeated at a short lag than when they were repeated at a long lag. The group effect was significant: $F(4,350) = 32.62$, $p < .001$; however, the group by trials interaction was not significant: $F(4,350) = 2.16$, $p > .05$. Again, Newman Keuls tests showed that all experimental groups had significantly higher levels of recall than the control $p < .01$ but these groups did not differ significantly among themselves, $p > .01$. 
Error Data

Omissions constituted 81.1% of the errors. Overall, intrusions accounted for 18.9% of the errors, with most intrusions coming from the immediately preceding trial. The short lag experimental groups receive a repetition on the fourth trial and it is possible that the nature of this repetition might influence subjects' tendency to give intrusions. To examine this possibility, a Chi-square analysis was performed on the error data for the short-lag conditions. Significant group differences were obtained, $X^2 = 15.57$, df = 4, $p < .005$. Post-hoc contrasts revealed that TRS and the control gave significantly fewer intrusions than groups TRD, IRS, andIRD which did not differ among themselves.

Final Free Recall

Due to time constraints, three subjects in two of the conditions and two subjects in two other conditions did not take the final free recall task, therefore, corresponding subjects were eliminated from the remainder of the conditions to give an equal n of 33 per group. As the number of analyses performed was small for both final free recall and list identification tasks, the accepted alpha level was set at .05. Results are shown in Table 2.
TABLE 2

Total Recall Scores for the Final Free Recall Task as a Function of Lag and Repetition Conditions

<table>
<thead>
<tr>
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<th>Short Lag</th>
<th>Long Lag</th>
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<tr>
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<td>.404</td>
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<tr>
<td>TRD</td>
<td>.453</td>
<td>.424</td>
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<td>IRS</td>
<td>.473</td>
<td>.382</td>
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<tr>
<td>Control</td>
<td>.363</td>
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Since the repetition groups differed in the total number of items they potentially could recall, results were scored in terms of proportion of items recalled. A 2 x 5 ANOVA indicated that there was a significant main effect of the lag factor: $F(1,320) = 11.28, p < .001$. This is probably more indicative of a repetition effect than it is of lag. Short lag groups had six items repeated, whereas long lag groups only had three items repeated. There was a significant main effect of repetition group: $F(4,320) = 6.29, p < .001$. Newman Keuls tests indicated groups TRD and IRS had a higher total proportion of words recalled than the control. The interaction term was not significant: $F(4,320) = 2.04, p > .05$. Although significant repetition group differences in total proportion recalled were not large, the biggest difference was between TRD, 43.8%, and the control, 36.4%, a difference of 7.4%.

The average probability of recalling repeated items is greater than the average probability of recall for nonrepeated items, 70% compared to 35%. Since these probabilities are based on different pools of items, they are statistically independent and can be treated as separate conditions within a given individual (Myers, 1975). A 2 x 4 x 2 ANOVA having two between-factors (lag and repetition group) and 1 within-factor
(probability of recall given repeated versus nonrepeated items). Results of this analysis showed that while there was no main effect of group: $F(3, 256) = 0.3484, p > .25$, the other two factors were highly significant. The lag factor again reflects repetition: $F(1, 246) = 7.52, p < .0001$. The lag x condition interaction was also significant: $F(1, 256) = 6.76, p < .001$. This reflected the fact that the probability of recalling a repeated item is greater for the long lag groups than for the short lag: $F(1, 256) = 13.806, p < .001$ whereas the probability of recalling a nonrepeated item does not change with lag: $F(1, 256) = 0.04, p > .25$. This reflects a recency effect since the long lag groups have just received their repeated items, whereas, for the short lag groups, half of the repeated items came much earlier. When recall for the repeated items from just the final trial is examined for the short lag groups, their probability of recall given a repetition is virtually identical to that of the long lag groups, .81 compared to .80.

Clustering of the repeated items was not observed for trial 4 items for the conditions in which an entire triad was repeated. However, on trial 8 these groups did show some evidence of clustering of the last triad compared to the control group, $\chi^2 = 17.44, df = 5, p < .005$. Irwin-Fisher tests performed on the proportion of subjects clustering trial 8's repeated triad revealed that while
TRS did not differ from TRD with short lag, $Z = -0.072$, $p > .48$, TRS resulted in significantly more clustering than TRD for the long lag, $Z = 2.285$, $p < .02$. These data are shown in Table 3.

List Identification Task

Two methods were used to score this task. The first was simply the number of triads correctly identified. The second was a measure of the magnitude of error. The absolute difference between the correct serial position of a list and the position assigned was summed and a total difference score was recorded for each subject. Again, due to time constraints, six subjects from two groups and four from 5 groups did not complete this task. Consequently six or fewer, as necessary, subjects were removed from the experimental and control conditions to leave an n of 30 per group.

Figure 3 shows list identification scores as a function of lag and repetition type conditions. Main effects for both of these factors were found to be significant, when a 2 x 5 ANOVA was performed: $F_{\text{lag}}(1,290) = 5.80$, $p < .05$; $F_{\text{repetition type}}(4,290) = 3.65$, $p < .05$. The interaction of these factors was also significant: $F(4,290) = 2.89$, $p < .05$. This interaction is easily seen in Figure 3. Repetition group TRS shows better identification with a long lag than with a short, although this
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<th>Long Lag</th>
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<td>.09</td>
<td>.06</td>
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</table>
Fig. 3. Total Triads Correctly Identified for the Repetition Groups at Short- and Long-lag.
increase was not significant \( p > .05 \). All other experimental groups (TRD, IRS, and IRD) show the opposite trend, being better when lag is short. This trend was significant for groups TRD, \( F(1,290) = 6.78, p < .01 \) and IRS, \( F(1,290) = 6.04, p < .05 \) and just missed significance for group IRD, \( F(1,290) = 2.91, p < .10 \). The control groups for both short and long lag who received no repetitions had equivalent levels of list identification. Analysis of simple main effects found groups to be significantly different at long lag, \( F(4,290) = 5.83, p < .001 \), but not at short lag, \( F(4,290) = 1.99, p > .05 \). Newman Keuls tests showed group TRS to have a significantly higher level of list identification \( p < .05 \) than the other groups which did not differ among themselves \( p > .05 \).

When identification of the last triad was examined, the same pattern emerged. See Figure 4. Chi-square analyses revealed no significant group differences for the short lag groups, \( \chi^2 = 1.406, df = 4, p > .90 \) but highly significant differences for the long lag groups, \( \chi^2 = 48.28, df = 4, p < .005 \).

Much the same pattern of results is evident with the error scores. See Figure 5. Analysis of these data revealed a significant main effect of lag: \( F(1,290) = 8.05, p < .001 \); and a significant interaction between lag and repetition type: \( F(4,290) = 5.19, p < .001 \). Again
Fig. 4. Identification of the Final Triad as a Function of Lag and Repetition Group.
Fig. 5. Triad Identification Error Scores as a Function of Lag and Repetition Group.
TRS exhibits fewer errors at a long lag than at a short lag $F(1,290) = 6.15$, $p<.05$ while all other groups show the opposite trend. This increase in errors at a long lag is significant for groups: TRD, $F(1,290) = 11.07$, $p<.01$, and IRD, $F(1,290) = 7.78$, $p<.01$ but misses significance for IRS and the control, both $p's >.05$. Simple main effects analyses again revealed that groups did not differ at short lag, $F(4,290) = 0.62$, $p>.25$ but did differ at long lag $F(4,290) = 6.13$, $p<.001$. Newman Keuls tests again showed group TRS to be significantly different $p<.05$ from the other groups which did not differ significantly among themselves.
SUMMARY

The results of the Brown-Peterson task replicate the typical finding of rapid buildup of proactive interference when the materials presented across trials are from the same taxonomic category (Wickens, 1970). The finding that exact repetition of a triad TRS leads to a dramatic increase in recall corroborates the results of several other studies using both words and CCCs as the trial materials (Bjork and Allen, 1970, Cermak, 1968; Radtke and Grove, 1977; Wickens and Gittis, 1974).

The result that following such a repetition, recall falls to its previous level replicates previous findings using nonsense syllables (Cermak, 1968) and related words (Radtke and Grove, 1977). What is of primary interest is, of course, the performance of the repetition groups relative to a control on the critical fourth and/or eighth trial. For the short-lag data, all experimental groups show a clear facilitation with repetition and none of them differ significantly among themselves. Thus, the prediction of a strict hierarchical coding model that changing a chunk would lead to that changed chunk being treated as a new unit is disconfirmed.
since the IRS and IRD groups clearly benefit from repetition.

For this short-lag data, the second repetition led to levels of recall that were lower than those obtained with the first repetition. It is quite likely that this reflects the fact that PI has not yet asymptoted on the fourth trial, hence the baseline might be higher. Certainly, having had seven prior trials from a single category provides more potential interference than having had three prior trials.

Data from the long-lag condition are consistent with data from the short-lag. Proactive interference builds up rapidly and consistently across groups. Again, all repetition groups show significant improvement following repetition relative to a control that has completely new items. What is extremely interesting is that a difference does emerge among the experimental groups. TRS is significantly better than the others, TRD, IRS, and IRD. A trend in this direction is evident in the short-lag data but it is not significant.

The finding that there is more facilitation when repetitions occur at a short-lag than at a long lag is consistent with the overall results of a study by Radtke and Grove (1977) in which they repeated an entire triad after three, five or seven intervening trials. However,
they failed to find a lag effect for the first item. This possibly may be due to a ceiling effect since the recall level of their repeated first trial item averaged 94%. With regard to interpreting this finding of a lag effect in the present experiment, a point of caution must be noted. To control for trial of first presentation, one necessarily confounds trial number with lag. This might be an important confound if PI is not at an asymptote by the fourth trial when the short-lag repetition occurs.

Final free recall data showed the probability of recalling a repeated item to be much higher than the probability of recalling a non-repeated item. One might argue that part of this may be due to a recency effect in that two-thirds of the repetitions come from the last trial. However, comparison of the probability of final free recall given a repetition with the probability of recall for the appropriate control triads reveals that a large difference in favor of repetition is still present even when list position is equated: 43.7% compared to the control's 36.3%. This finding is also consistent with the results of other studies showing repetition facilitates final free recall (Fiske, 1977).
DISCUSSION AND CONCLUSIONS

The present study was designed to test a prediction made by a particular model of information coding. In this model, exemplified by Johnson's (1970, 1978) coding model, items within a chunk are coded as a unit within memory and are retrieved in an all-or-none manner. According to Johnson, a chunk could be a set of items that are semantically related, are rehearsed together, or are physically grouped. In this model, it is assumed that the container for item sets is opaque. That is, subjects retrieve the item sets via the code without direct access to the items themselves. This assumption that item sets are retrieved into working memory without knowledge of their contents is not uncommon. Such an assumption is made by Shiffrin in his 1970 model of retrieval from long term storage. Coding theorists (Bower and Winzenz, 1969; Johnson, 1978; Estes, 1972) maintain that the order of items is determined at the level of their common code, rather than being directly associated with individual items. It follows from such a model that if any change is made in the items within a chunk, the old code is suppressed and a new code will have to be made.
Thus, to benefit from repetition, the organization of items must remain the same. Changes in organization of items either between or within chunks would necessitate new codes. Hence, even though items may be repeated, subjects would treat them like new items.

As discussed in the introduction, studies of serial learning using material low in meaningfulness have supported such a model. It was therefore considered important to test the generality of an opaque code model to other tasks as well as other materials. The Brown-Peterson task has been widely used to examine the nature of coding in memory and much is known about the physical and semantic variables that affect performance on this task. Therefore, it was chosen as the paradigm to examine whether the model described above is applicable to the coding of highly meaningful (semantically related) material in a non-serial learning task.

The pattern of results obtained in this study highlights the obvious, but frequently overlooked, caveat against simplistic interpretations of memorial processes. Neither a strict theory of opaque codes nor a theory based on item representation alone can adequately handle the data. Results from the Brown-Peterson task clearly failed to support the predictions of an opaque memory code in that changing the nature of a chunk did not cause the
chunk to be treated as an entirely new unit. If this had been the case, then the performance of groups IRS and IRD, on the repetition as well as the other trials, would be equivalent to that of the control. Instead, both of these groups showed clear facilitation when presented repeated items. This is not to say that results in any way violate the notion that items within a particular Brown-Peterson trial are chunked, nor that position information is unimportant. In the Brown-Peterson task, as well as the final free recall and list identification tasks, there was clear evidence that subjects use order as well as item information. Changing the order information tended to lessen the facilitating effect of repetition.

Certainly, the relative roles order and item information play in memory is not a new issue. It is at the nub of serial learning. In fact, the way coding theories as opposed to associationistic theories account for order and item information is a fundamental difference between the two. That is, early associationistic theories accounted for order information in terms of interitem associations, whereas most hierarchical coding models assume that items are not directly connected, but instead are chunked together by a common memorial code which handles order by tagging the codes for order (Johnson,
or by sequentially reverberating circuits (Estes, 1972).

The question of whether items are retrieved in chunks is relevant to the topic of information availability. A system in which sets of information are retrieved in a unitary fashion is an efficient system. It obviates the need to search memory one item at a time and thereby enables the subject to have whole chunks in his working memory to decode. Empirical support for this concept comes from a variety of sources. A major source mentioned above comes from studies of serial learning (e.g., Bower and Winzenz, 1969; Johnson and Migdoll, 1971). Using a modification of Sternberg's (1966) reaction time task, Wickens, Moody and Dow (1979) have found strong evidence for retrieval of entire memory sets, rather than individual items.

In the Brown-Peterson task, the fact that subjects are frequently able to give an item's position even when it is the only one they recall, i.e., saying "blank, DENTIST, blank", also suggests that order is not totally dependent upon interitem associations. How then are the findings that a change in order decreases the repetition effect (for Brown-Peterson long lag, FFR, and list identification) reconciled with the finding that repetition of an item facilitates recall performance over a control item
even when its order in terms of neighboring words and/or position is changed?

While a theory in which information is stored in opaque codes is hard put to explain this facilitation, the results do not rule out the possibility that subjects retrieve sets of items into working memory. The effect of repetition of items could be accounted for by increased response availability for production. This would place the beneficial effect of repetition in the decoding or unpacking stage. Obviously, this is a topic for additional research, as the present study does not address this issue.

To understand the differences between the results of the present study and the studies supporting an extreme coding model, one must consider the nature of the tasks used in the present study. It has been demonstrated that subjects will trade-off item for position information, if the experimental task requires it (Hastie, 1975). Although subjects in the Brown-Peterson task are told to recall items in the order presented, the task is not a serial learning task, strictly speaking, as subjects are told to recall words even if they cannot remember the order and they are given credit for words recalled regardless of order. In line with this, Murdock and vom Saal (1967) note that, in a Brown-Peterson task,
while performance is better with semantically related materials than with unrelated, the related items show more transpositions.

Given that the very nature of the Brown-Peterson paradigm requires subjects to recall items from a particular trial together, it is not at all surprising that they pick up some position context, as well as item information. However, the type of materials used might also affect the degree to which subjects rely on such position or contextual information. This brings up another difference between the present investigation and studies supporting coding theory. The latter have typically used materials that are not meaningful in any broad sense, such as number or letter series. The present study used words from the same taxonomic category. It is possible that with such materials the category label elicited by their relatedness overrides contextual information as a retrieval cue for a particular trial's items. If this is the case, then using unrelated items should increase subjects' use of inherent contextual organization. Several investigators (Gardiner, Klee, Redman, Ball, 1976; Reutner, 1969) have presented evidence consistent with this. Their work has found that when consonant trigrams are presented across a series of Brown-Peterson trials, changing the color of the typeset facilitates recall. However, when
words are presented, changing the color of the typeset has no effect. Obviously, whether using items low in semantic relatedness would decrease the facilitatory effect of repetition for IRS, IRD, and TRD remains an empirical question.

In the final free recall task, the beneficial effect of repetition was again apparent. In all groups, the probability of recall given a repetition was greater than the probability given a single presentation, and there was no interaction between type of repetition and these probabilities. Clustering of items that had been presented and repeated together is something a coding theorist might expect. Yet, there was virtually no evidence for chunking according to triad in the groups that had a triad repeated, except on trial 8. For repeated trial 8 items, the probability of clustering was higher for TRS and TRD compared to the control.

A pattern evident in the clustering of the eighth trial items illustrates an interesting and statistically significant trend that was apparent in all three tasks. That is, for the long lag groups, TRS is significantly different not only from the control but also from the other repetition groups, TRD, IRS and IRD. In the Brown-Peterson task, for long lag groups, TRS showed significantly more facilitation given a repetition than TRD, IRS
or IRD. In the final free recall task, for long lag, the proportion of subjects clustering the repeated triad was significantly greater for TRS than for TRD, .42 compared to .17. At long lag in the list identification task, TRS showed a level of recognition higher than any of the other groups and had significantly fewer errors. This was true not only for the overall score, but also for identification of the position of the final triad. The TRS group is the only condition in which an exact repetition occurs. In all of the other groups, there is some change in position context. This again suggests that subjects are indeed using order, as well as item, information. However, for the short lag groups, there was no statistically significant evidence for a difference between TRS and the other groups in any of the tasks.

There are several factors which might account for this difference between short and long lag conditions. In the Brown-Peterson task, it is possible that PI is not at an asymptote by trial 4. Failure to find differences between TRS and the other groups might be due to a ceiling effect. With regard to clustering in the final free recall task, there are several points to bear in mind. Murdock (1976) discusses studies that suggest in the Brown-Peterson paradigm, as well as in other memory tasks, order information is lost prior to item information.
Given this, it is not surprising that significant clustering occurred only for the last triad presented. This, however, does not explain why, on the last trial, the long lag TRS group differed from the TRD but, on that same last trial, short lag TRS and TRD did not differ statistically. There are two things to consider about the prior experience of short and long lag groups that might shed light on this. First, while the beneficial effects of repetition are independent of whether the repeated items had been recalled on the initial presentation trial (Cermak, 1968; Nield, 1976; Radtke and Grove, 1977), there is evidence that prior recall does increase the probability an item will be recalled in a FFR task (Radtke and Grove, 1977). Short lag groups received their first presentation of the trial 8 items when PI was at an asymptote and the probability of recall was low; whereas, the long lag groups received their first presentation in the first three list positions. Second, lag here is confounded with number of prior repetitions. Short lag groups have already received a repetition, whereas long lag groups have not. Finally, the long lag groups have 4 to 7 intervening trials between an item's first presentation and repetition. The short lag groups only have 0 to 2 intervening trials.

Considering the list identification data, long lag TRS is again significantly different from the other long
lag groups. While the IRS, IRD and TRD groups show better identification of a triad's list position at short compared to long lag; TRS shows a trend in the opposite direction. The reasons for this effect are not clear. One may be that with long lag the TRS group gets exactly the same triad in the primacy and in the recency positions of the trial sequence. In this case, the repeated triad can serve possibly to anchor the ends of the list position sequence.

It should be kept in mind that list identification is essentially a recognition test of position information. Tulving (Watkins and Tulving, 1975) has presented a large amount of data showing that recognition is impaired when context is changed. Group TRS is the only group that did not experience a distortion in position context, (i.e., order with respect to surrounding items and/or position) with repetition. If one accepts Murdock's contention that order information is lost with time, it makes sense that the IRS, IRD and TRD groups should show a decrement with increasing lag. The present study was not designed to investigate lag; hence, much of the above discussion is speculative. Certainly, the topic merits further investigation.

On the whole, it seems that when order information is required for good performance, such as with serial learning tasks, then changing the position of an item,
either within or between a chunk, will mitigate the benefit of repetition. When item information is the critical determinant of performance, as in a Brown-Peterson paradigm, such position changes have much less effect, and repetition (perhaps by increasing a response's availability) leads to a clear increment in performance. As already discussed, the results of the present study are inconsistent with a coding theory in which items are only accessible through some higher order code. It is likely that the nature of both the materials and of the task determines the degree to which subjects use contextual information for retrieval. When materials already have a highly structured representation in memory, contextual information may be less important to the episodic organization of those materials for a memory task, than when less meaningful materials are used. Given the complexity of the memorial system, it is not surprising that a single explanation will not handle retention performance of all types of information under all task conditions.
APPENDIX

TABLE 4

Procedure for Counterbalancing the Trial Position of Repeated Items and Trials

<table>
<thead>
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<th>Trial 1</th>
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<th>Trial 1</th>
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Letters refer to different words within a particular category. Triads are counterbalanced for trial position within four trial blocks. The outline above illustrates the method used to counterbalance the initial trial position of items and triads that are repeated on trial 4. Trials 4-8 were counterbalanced in an identical manner for the short-lag group.
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